

SCIENTISM IN PSYCHOLOGY

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While the temper of the logico-empirical tradition has been, on the whole, sound, it may have taken a few wrong turns, as far as the behavioral and social sciences are concerned. These mistakes may be compensated for by personalistic, organismic and phenomenological emphases which, I am convinced, are not opposed to the positivistic tradition but rather complement it. It is with this explanation that I should like to plunge into my subject.

THE SCIENTIFIC METHOD

Contributions. Let us mention at the outset some of the major contributions of the logico-empirical tradition, the scientific method, in the behavioral sciences. To mention its contributions in the material sciences would be superfluous. Among the former might be mentioned: (1) the universalization of experiences which give genuine meaning to the phrase, "the brotherhood of man;" (2) the reduction of interracial, intergroup and interfaith strife through the increasing dissipation of bigotry and superstition; (3) an increase in the scientific attitude and in the employment of scientific method, with a consequent promotion of objectivity and detachment in the study of both man and nature; (4) an enhancement of human consciousness so that the relations among the elements of our experience can now be grasped in numerous ways which were alien to the animistic and anthropomorphic mentality of the past.

Shortcomings. The shortcomings of science, insofar as the study of human behavior is concerned, are generally held to be the following: (1) neglect of the causative role of value, meaning, and the effort of the individual to maintain himself as a system; (2) relative neglect of the role played by the inner, mental life, and a preference for a physicalistic, external, input-output point of view; (3) an indifference on the part of scientists *qua* scientists to the lag between scientific discovery and its human utilization; (4) the promotion of a dangerous, ethical relativism through a superficial, philosophic approach to individual and group ethics, a charge levied against scientists in general and social scientists in particular; and (5) an uncritical insistence that there are no other ways of relating to experience, apart from that resident in the scientific method and in the scientific attitude.

Neither the list of contributions nor the list of shortcomings is complete. The latter, however, is sufficient to indicate certain attitudes which give rise to the shortcomings. These attitudes proceed from a mothering matrix of *scientism*, and I should like to turn to a discussion of scientism and in particular the attitudes which it is prone to generate.

Scientism defined. Broadly speaking, scientism in psychology is a point of view which assumes that the methods and assumptions which have been so successful in the physical sciences will, with some modification, be equally successful in the behavioral sciences. The advocate of scientism prefers to treat the human organism strictly from an external point of view, as some sort of an input-output system in which it is important to have a firm knowledge and measure of all inputs and outputs, while remaining indifferent to the nature of the system itself. The radical empiricism which such a position represents has its origin frequently in some of the less critical theses of physicalism as it appeared in the early writings of the logical positivists. Scientism will tend to abjure such concepts as "consciousness," "mental states" and "organismic purposes," preferring to study human behavior in terms only of manipulations (test-operations) and externally observable data (test-results). Furthermore it takes as an article of faith that *only the methods of science*, particularly its physicalistic presuppositions, are sufficient to solve all human problems. In the following the attitudes produced by scientism are discussed.

MODES OF SCIENTISM

Metromania. Though the recognition of the importance of number does not begin with Pythagoras, the *systematic emphasis* on the notion that number lies at the heart of the universe, can be said to have begun with him. This emphasis we shall refer to, with due poetic license, as the *Pythagorean Complex* (PC). Having discovered the extent to which the laws of harmonics were expressible in mathematical terms, the Pythagoreans then extrapolated from these discoveries and took as an article of faith that similar mathematical regularities would be found to underlie all phenomena. With the rise of modern science, following Newton's amazingly successful theories, this faith has been strongly reinforced. As a result, various brands of uncritical positivism have arisen in our time which take at least two tenets as articles of faith for intellectual inquiry, namely, the observational methods of the natural sciences and the operations of measurement.

They believe that these together will suffice to reveal truth in all her glory. This faith has been increasingly borrowed by workers in the behavioral and social sciences over the last half century or more and, apart from the fact that it is nursed as carefully as though it were the heir to a throne, has admittedly inspired a large amount of research, much of which has proven to be of real significance.

However, in their eagerness to give expression to the PC, some research workers are prone to overlook the fact that when positive results are found statistically, the task of explaining these first begins. The wooden application of statistical procedures and statistical design to data from the behavioral and social sciences represents an undesirable and uncritical form of the PC, since it fails to recognize that we, ourselves, have imposed the statistical procedures on the data and that the statistical relationships found do not imply that the data have a lawfully expressible inner harmony of their own in terms of some underlying causal nexus. This type of situation represents, of course, what we mean by *spurious correlation*, and every research worker would do well to read Simon's (8) discussion of this topic. Where we do have reason to believe that there is some natural mechanism underlying our data, the statistical relationships, themselves, do not necessarily help us to obtain any understanding of how the data came to be causally generated.

Perhaps the most scathing attack to date on the modern form of the PC has come from Sorokin (11). If one discounts certain excesses of his argument, one can safely say that he has given *some of the follies* generated by the PC the sound lambasting which they deserve. For the more infelicitous forms of research generated by the PC, Sorokin has coined the pungent phrases "quantophrenia" and "metromania."

Sorokin's major criticisms can be stated briefly. Addiction to measurement without any genuine understanding of what is being measured is a particularly pernicious form of the PC. Uncritical use of measurement for the sake of measurement can, indeed, assume ridiculous and often unrealistic forms in the behavioral and social domains. We are likely to overlook significant causes, the psychological and social mechanisms, if we are only interested in statistical relationships and statistical order. Meanings and values underlying the behavior which we have brought into statistical focus, are likely to be forgotten. There is also the more intrinsic criticism of "metromania," namely, that it is our responsibility to acquire a *genuine understanding of the theory of measurement*, apart from the cookbook applications of research design, so that we can justify the appropriateness of our procedures for the texture of reality in which we happen to be temporarily interested.

Modeling after the physical sciences. A second manner in which scientism prefers to express itself, is by the use of the mathematical

model in psychology. This entails both assets and liabilities. Its chief asset, though not its only one, of course, must be its predictive value. Any mathematical model which has proved to have *some* predictive value in the domain for which it was intended, has shown itself to have *some* merit, even if its inability to make further predictions forces us to discard it for a better one. However, it is in the abuses of the mathematical model that its liabilities rest, and when a critic applies the adjective "scientistic" to a model-builder in the behavioral or social sciences, he generally refers to one or more of these abuses. I should like to mention just a few of these here.

It must be recognized that a decision as to which relevant variables probably account for a given set of phenomena involves an interpretation of the texture of reality. This means that many sets of variables may have equal, predictive value and that an insistence on the *reality* of the interpreting variables chosen, is dogmatic and should not be pressed. If the model-builder does so, a school or cult is inevitably born.

It must also be recognized that models of individual human behavior neglect the role of meaning and value in the determination of that behavior and are therefore likely to have only modest success, if any. Efforts to get around this difficulty, by attempting to translate meaning and value determinants into observable motor and verbal input-output correlates, as Skinner does, will probably achieve only limited success. This will happen not so much because this operation is, of course, illegitimate in that it is philosophically questionable, but rather because there are too many internal dispositions of value and meaning which, because of their affective concomitants, cannot adequately be given reportable or observable correlates.

Wiener (13) has pointed out some singular weaknesses of model-building in the behavioral sciences, which are doubly cogent because they come from a distinguished mathematician and model-builder himself. He would argue against the possibility of a mathematical social psychology, for instance, on the grounds that a social model should be homeostatic in nature and that since privileged groups in large, modern social and economic complexes, prevent a free flow of information, a homeostatic model is impossible. He also feels that the scientific observation of communities will tend to alter their originally characteristic behavior, particularly if their members become aware of the subsequent descriptions and the predictions based upon these. Description and prediction of aggregate behavior are not likely to be

well-realized because "it is in the social sciences that the coupling between the observed phenomenon and the observer is hardest to minimize" (13, p. 190), i.e., a group's knowledge of our predictions may act so as to unsettle them.

An even more cogent difficulty has been pointed out by von Neumann (12). Model-building in learning theory and in all those cases where the model-builder assumes that behavior is mediated by the central nervous system (CNS), implicitly assumes that the language (processes) of the mediating CNS is, itself, mathematical and logical in nature. This gives rise to the expectation that a mathematical model will be *isomorphic* to CNS activity and, derivatively, its molar correlates. However, this assumption is factually invalid. In von Neumann's own words:

We have now accumulated sufficient evidence to see that whatever language the central nervous system is using, it is characterized by less logical and arithmetical depth than what we are normally used to . . . When we talk mathematics, we may be discussing a *secondary* language, built on the *primary* language truly used by the central nervous system. Thus the outward forms of *our* mathematics are not absolutely relevant from the point of view of evaluating what the mathematical or logical language *truly* used by the central system is. . . . Whatever the system is, it cannot fail to differ considerably from what we consciously and explicitly consider as mathematics (12, pp. 81-82).

While we cannot afford to neglect statistical and mathematical models in the social and behavioral sciences, we should, as Wiener says, "neither build exaggerated expectations of their possibilities. There is much which we must leave, whether we like it or not, to the un-'scientific,' narrative method of the professional historian" (13, p. 191).

Factophilia. In true Hegelian fashion every error of the intellect is followed, sooner or later, by its opposite error. There is a brand of psychologist who, in addition to sharing the scientific faith of the devotees of the PC, is reluctant to use theory at all for fear it will stray too far from the facts. The intellectual and doctrinal roots of this attitude have been discussed by Brunswik (3) in what he refers to as "hostility to theory and to central inference." Macdonald (5), a distinguished writer, has clearly recognized the existence of what may be designated "factophilia," to borrow a leaf from Sorokin, and has attempted to trace the social determinants in American life of this American worship of directionless facts unleavened by integrating theory. This propensity is found in much professional work in psychology. To recognize its existence one does not, of course, have to be "factophobic," but one does have to realize that the amassing of

facts without the power to integrate them can be a form of intellectual stasis.

In psychology, Bergmann (2) recognizes the historical origin of factophilia as a desperate effort to reject all forms of rationalism. A radical empiricist like Skinner, in Bergmann's view, in his effort to avoid the embarrassment of theory, throws out the baby with the bath. Bergmann says:

Radical empiricists harbor a profound suspicion against "abstract" things. To them, all such things smack of rationalism and are "fictions" at best. In the philosophy of science the suspicion becomes a bias against all highly defined concepts. Nor does it stop there. Typically, the basic terms of a theory are, as we saw, highly defined. Thus theory itself becomes suspect. The most influential representative of this tendency was, perhaps, Ernst Mach. Its most vigorous advocate among contemporary psychologists is Skinner. He thinks that the "abstract" concepts of behavior theory, no matter how "objectively" and "operationally" defined they may be (I am again merely parading these two words), portend a covert revival of "mentalism" or "antibehaviorism." I know of only one way to explain so strange an aberration. Skinner's is the radical empiricist's suspicion of "abstract" terms. They are the stalking horses of rationalism, and rationalism is the champion of "mind." Hence, psychological "theory" is inherently "mentalist" and, therefore, not good "behaviorism." Queer. Yet it is also delightful to discover all these subterranean connections, to formulate, if I may so put it, the unconscious grammar of the unanalyzed metaphysics which so many philosophically inclined scientists carry around with them without knowing it. *Plus ça change, plus c'est la même chose* (2, p. 54).

So prominent a psychiatrist as Meierloo (7) sees the social roots of factophilia in our schools, which he describes as fact-factories. Meierloo defines two kinds of intellectuals in academic life, the *quantellec-tuals* who aim solely and strictly for quantity of knowledge, and the *quintellectuals* for whom intellect is a quality of personal integrity through which intellectual integration is achieved, and disparate collections of facts are suffused with meaning, value and perspective. Perhaps it is our quantellec-tuals who are most disposed to the virus of scientism.

Perhaps factophilia and an increasing, uncritical and professional worship of measurement and mathematical models, may, at times, have its roots in a personal sense of inadequacy in the face of criticism. As Adler put it, "Authors afraid of losing ground or of being assailed by criticism attach importance only to those facts that are capable of receiving physical confirmation in laboratories and that can be recorded and reduced to figures" (1, p. 198). Elsewhere he said, "The primitive orientation in the world which corresponds to the antithetical assertions of Aristotle, as well as to the Pythagorean tables of opposites, originates in the feeling of insecurity" (1, p. 229). An urge to achieve facts or rigor, to the exclusion of other considerations, may in

fact be a kind of uncatalogued, occupational maladjustment. The only way to a correct assessment in this connection, would be to develop professional standards for discriminating the important from the meaningless in both factual research and in the use of quantitative procedures. The development of such standards is still a consummation devoutly to be sought.

COUNTER-APPROACHES TO SCIENTISM

Classifiable today as predominantly free from the virus of scientism, are those psychologists whose work might reasonably fall under one of the following rubrics: personalism, phenomenology, gestalt psychology, individual psychology, orthopsychology, field theory and existentialist psychology. The holistic and *verstehende* ancestry of most of these can be traced to the work and ideas of such figures as Husserl, Stern, Adler, Dilthey and Brentano. There is a theoretical commonalty among representatives of the anti-scientistic front. A few of the concepts which are of central importance to most, if not all, the points of view just mentioned are the following.

The creative self. This is a concept which asserts: (1) There are stable, central processes which serve to integrate behavior and give it purposeful direction over time, and which furnish the human organism with a capacity for creative activity by enabling it to de-randomize experience through infusing meaning and value into that experience. (2) These processes are able to introduce novelty into the external field of the subject, thereby determining those conditions to which their own growth shall be subject. (3) These processes create purposes, through rearranging immediate experience or the memories of it into new, *possible* combinations, and then devise means to fulfill such goals. (4) The observable sensory-motor correlates of these processes are not to be equated with them and can give, even by constructive inference, only a very inadequate picture of these processes themselves.

Expressiveness. Each personality has a uniqueness whose flavor is sensed in ordinary life and is roughly indicated by such concepts as self-actualization, style of life, master motive, etc. This uniqueness develops in response to the effort to achieve masterfulness or superiority in some dimension, or to enhance awareness and sensitivity to the meaning and value of experience. This effort is open-ended both as to direction and content. It manifests itself to some degree through the subject's pattern of values and interests and can to some extent be

detected by the methods of depth psychology and by expressive movement.

Altruism. Only a very limited aspect of the human psyche is knowable in terms of opportunistic functions. This conviction assumes that social interest (1) is one of the more fundamental aspects of man's nature and that man, unless warped by a set of socially pathological values, derives satisfaction in helping to fulfill the needs of other personalities. It assumes that man is capable of identifying the self and the other more intensely than is commonly realized. Increasing factual evidence along these lines has been gathered by Sorokin (9, 10).

The holistic emphasis. The conceptual position involved here assumes with Lecky (4) that the primary task of the psychologist is not the execution of more and more experiments, or the continuous accumulation of more and more facts, but instead the development of a conceptual scheme enabling him to see a significant and orderly relationship within the present facts. Such schemes cannot escape the requirements of interpretation and constructive inference. Unless the holistic urge is employed to try to grasp the unity of the human organism, research will increasingly fragment the whole man, provide less and less predictability in a molar context, and move further and further away from the required synthesis of current findings. Furthermore such holistic schemes will have to continue to be open-ended so that changing interpretations can encompass more and more findings and approximate more and more closely the explicitly assumed unity of the human subject.

The understanding approach (Verstehende Psychologie). Unless we make concerted efforts to understand behavior in terms of the motives and goals of the behavior, we are sure to wind up in a series of blind alleys. Long-circuiting behavior is purposeful. This is surely one of our most human characteristics. Symbolic behavior associated with long-range preservation of the self-system is never opportunistic. Infrahuman organisms are incapable of such behavior. It is to be understood only through an accurate picture of persistent motives and conceived values. Only by asking how we, ourselves, would behave in a given context if our motives were those of an observed subject for whom we had a historical sample of past behavior, are we likely to comprehend the inner dynamics of that behavior. No radical empiricism can furnish that understanding, and no amount of well-controlled operant conditioning can substitute for that understanding, in the scientific enterprise.

The frame of reference. The facts of human behavior are not independent things which we can depend upon always to be true. A datum of distinctly human, that is, symbolic behavior, has truth only in relation to the value-schema and spontaneity of the individual subject. Symbolic behavior is then a projection in time of such a schema. The explanation of this observed molar behavior or projection cannot be adequate unless connected to the frame of reference which represents this schema. This frame of reference is the aggregate of these stable values and symbols by which the creative self functions, and the observable behavior is the instrument for mediating this aggregate. The significance of this frame of reference is lost when the individual is treated as an input-output system, regulated by a black box, to whose internal structure and working arrangements one can remain relatively indifferent.

These are only a few of the concepts which, I believe, will yield for a science of behavior a greater adequacy than can be obtained by simply restricting our approach to a physicalistic, input-output bias. A fuller discussion of similar considerations will be found in the orthopsychological work of Maslow (6), although a good deal of revision is also being furnished by others. If the present discussion will encourage the adoption of a more critical attitude towards the encroachments of scientism, it shall have paid off.

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