

## RATIONAL-EMPIRICAL CRITERIA OF SCIENCE AND THE PROBLEM OF PREDICTION

G. BAYMOND STONE, University of Oklahoma, Norman

The temper of our times is uncertainty. The quest of our times is for security. This state of affairs is not unknown historically, but the contemporary scene, at least in this country, has one significant difference from the historical past: whereas in previous times the quest for security in the face of insecurity manifested itself in wholesale returns to religion, the modern reaction—with some formal underwriting — has been to turn in significant proportions to the sciences of human behavior for a solution. The magnitude of the demand caught psychology unaware, and the unpreparedness of the profession resulted in an hiatus between demand and supply. The profession ballooned much faster than the science could progress (13). An unhappy result has been that the science of psychology has been far oversold.

It is the thesis of this paper that the hiatus between demand and supply is closing and that the time is ripe to proceed with the caution of science. That psychology has done all that has been asked of it is more in the realm of wish-fulfillment than *fait accompli*. The hustle and bustle of activity in clinical psychology in the last five years has not been without some unfortunate concomitants. The pressure of demand has resulted in some loose method and spurious productivity. There is a danger that the professional balloon might burst from the hot air of excessive promises, and the field itself is becoming self-conscious, and, as it were, conscience-stricken. The leaders (10, 12 14) are calling for more adequate methods and less of the helter-skelter techniques which frequently lead to deviations from the proper and tested methods of scientific production. There is an apparent desire to survive as a science rather than an esoteric and unreliable art. The profession is becoming organized again on the basis of new standards.

It is time to slough off the penumbra of false method and specious half-truths that have infiltrated into contemporary psychology and concentrate on the methodical and sure procedures of scientific inquiry, slow and painful as they may be to the panacean enthusiast. It is time we separate the trained scientist from the educated charlatan. It is time for caution against continuing excesses. It is time for a reaffirmation of what Percy Bridgman (3) has termed "intelligent" method.

So much for an introduction. Now for a bill of particulars.

**APPEAL TO AUTHORITY.** First of all, there is a tendency within psychology to substitute an appeal to authority as a measure of scientific validation for the established and proper appeal to the empirically tested fact. But there is no substitute for the empirically tested fact. There is no authority that can long stand in front of the most stubborn thing in all of science: a controverting fact. However, it is in the areas of relative absence of facts that the authoritarian criterion is used. It is said that in such areas it is better to follow the brilliant opinions of one man than to do nothing at all. This is a specious half-truth. It results in a procedure which is extravagant, time-wasting, emotional, and, in the ultimate, inadequate and unproductive. This has been a hard lesson for psychologists to learn. The procedure is easily recognizable and usually follows a sequential pattern such as this:

- Phase 1. The psychologist either studies under or reads the writings of someone with whom he agrees. In the absence of any criterion better than this, he labels the authority "brilliant."
- Phase 2. If the psychologist has not before studied under the authority, he now goes and does so for about two weeks.
- Phase 3. The psychologist comes back to his home grounds with something of a "Father" or "Master" complex and interprets everything he

sees or does in terms of whether or not it agrees with the opinions of his Master. This is passed off as research. This appeal not only fails to be factual, it fails to pay cognizance to the very methods by which facts are obtained.

Phase 4. There is a period of varying duration in which this technique of verifiability dominates the method to the exclusion of the possible or even, and this is the crux of the matter, the *destrability* of factual confirmation.

Phase 5. Finally, the ultimate stage, the psychologist disagrees a little bit with the Master, — and lo, we have a new and more satisfactory authority.

Some exaggeration has been taken for purposes of illustration. (How much?)

**ANTI-ANALYSIS ATTITUDE.** A second area of misapprehension might be described as the hangover from the anti-analysis debauch. The literal meaning of anti-analysis is anti-control, and it usually takes the forms of anti-laboratory, anti-comparative, and anti-statistical biases. This viewpoint is, therefore, essentially anti-scientific, and it very thinly veils a philosophy of despair and a consequent methodological anarchism. Again specious reasoning is employed to camouflage an unproductive negativism.

The very essence of the empirical component of scientific method is control, systematic variation and measurement (4). These are experimental procedures and as such are analytic. They are not necessarily laboratory procedures, nor do they necessarily involve the ultimate in precision with measurements always in refined and extensive units; yet as procedures they are analytic. Analysis is not only the customary method of science; it is the absolutely essential method to produce advances in knowledge. Without it not even intelligent communication is possible. There can be no argument against analysis *per se*, but there can be and have been arguments against that type of analysis which believes that the only proper result is to find restricted and static units which have an independent entity existence in their own right, and which by combinations and concatenations shall account for all characteristics of a synthetic whole (2). This type of analysis involves several improper assumptions and has been adequately criticized in the past—for example, by John Dewey (9) in his paper on the reflex-arc concept in psychology, and by Harvey Carr (7) who called it a naive methodological monism, or "quest for constants."

However, the same reasons which have made this "atomistic" type of analysis invalid also act as reasons to require the existence of controls in method. An essentially changing subject matter by its very nature cannot be studied without controls. Control, in this sense, does not mean rigidity or lack of variation; a process can control a process. Without control it is never possible to verify the effect of a stimulus variable because its effect would be confounded by what the changing subject matter might be doing in spite of the stimulus variable. This is a very simple matter and it is the first step in experimental method. Without it, no amount of endeavor can produce data with scientific meaning. But to someone who has interpreted anti-atomism as anti-control even this first step is unavailable, and so, therefore, is scientific production.

The purpose of control, or analysis, is to identify variables and then either to eliminate them, or keep them constant, or counterbalance them, or measure their effect separately from the independent variable of experimentation (16). Not to do so confounds the data. Without the proper design there is no amount of data collection which can have scientific meaning. We frequently hear of the need to "explore" relationships and to do so in an "unstructured" or "field" or "dynamic" situation. As frequently as not this phrasing is employed to hide the absence of controls. There is a tendency to believe that an unfettered collection of data or protocols will, if continued long enough, somehow end in establishing meaningful generalizations. But

data taken under a lack of control or design are confounded. No meaningful generalizations can result. There may be numbers, there may be protocols, and there may be statistical manipulations. When typed these things may fill notebook after notebook and library after library. But they are categorically worthless. There is no need to underemphasize this point. These data are irrevocably and irretrievably without meaning. There is no statistic which can make confounded data have any greater use than to fill a waste-basket.

There is a design in human experimentation which illustrates this point at a denotative level. It involves the use of a pre-test, the introduction of an independent variable, and then a post-test. To attempt to draw a conclusion from a quantitative comparison of the results of the pre and post tests and attribute the conclusion to the independent variable is to do the simplest and most fundamentally wrong thing that is possible. If there is no control group or comparison condition which has the same design without that independent variable, no comparisons can be made or conclusions drawn. Data can be obtained, — yes. It may be put on 8½ x 11 sheets of paper and piled as high as the Empire State building. Frequency distributions may be tabulated, standard errors computed, and critical ratios obtained to show that the chances are less than one in a trillion trillion that the pre and post test difference could be accounted for by chance. But this is not to say that this difference was due to the independent variable.

There is no need for understatement here. This uncontrolled design has been used. Results from its use have been reported as "research." This is a desecration of the name of science and a travesty of its method. There is no reason to be proud. Ignorance of the need for control is unintelligent. An anti-control attitude is inexcusable.

**PREDICTION AS A CRITERION OF THEORY.** There is a third area in which psychologists can be accused of loose usage and unfortunate misconceptions. It involves a tendency upon the part of some to believe that the requirement of theory in the method of science (5, 8) is in and of itself an adequate justification for the existence of any rational interpretative framework. But theory is a technical procedure in the logic of science and there are positive criteria that are available to evaluate the worth of any theory that may be proposed. There is a rather vague appreciation of this in some quarters and theories are apt to be defended "because they work." But the word "work" as used in this connection is itself a technical term and a close examination of how some theories "work" reveals an absence of the necessary evidential criteria for the term to apply. There is one sense in which all theories "work;" i. e., that they can predict with some success. Not even the pragmatists, however, are willing to accept this criterion without qualification. There is no theory that would lack acceptance and dignity if this were the only criterion of its worth. It and it alone would make great theorists of St. Paul, Father Divine, Madame Zita, Mary Baker Eddy, Aimes Semple McPherson, the race-track tout, and the penny weighing machines on Main Street.

In clinical psychology it is a commonplace that any therapy works, and as all or nearly all therapies are based upon theoretical frameworks, any of the latter also work. If this were the only criterion of the acceptability of a scientific theory it would certainly not be necessary to require four or five years of the highest type of formal education to learn it. It is the type of reasoning that keeps ignorant superstition alive. Presumably our theories should be better than that else we would be hard put to defend our graduate instruction to say nothing of certification and licensure proceedings.

All theories involve premises or postulates and it is true that scientific method does not require that all of these premises be capable of direct empirical verification (1, 11, 15). Rational procedures of deduction from the premises, however, do have a requirement which represents the crux of the matter under discussion. Premises must be stated in such a way that deductions from them allow for the operations of both confirmation and failure of confirmation

(6, 8). Only under these conditions does testability exist. Only predictions from this type of postulate set test a theory. Only a theory which works under these conditions works in a scientific sense. A proof is not a proof if there was no possibility of disproof. This is the original meaning of the phrase

that an exception proves the rule.

Consider the premise that "all events occur as the will of God," or that "all men are mortal," or that behavior is the result of either a life instinct or a death instinct working together, or that a particular therapy can help those who can help themselves. How would or could theories with any one of these postulates be disproved? Where is the deduction which would test? But they *do* predict. They "work" in that sense. But so does superstition work.

Consider the theory that juke-boxes are worked by a little dynamic spirit inside who pockets the money and then plays the record. A prediction can be made that if you put in money a record will be played. When you perform the empirical operations the theory may well be confirmed. But there is the question of *valid* confirmation. This requires the operations of a test where the crucial aspect is not the prediction of success so much as the possibility of the prediction of failure. To name the little dynamic spirit a "hypothetical construct" in no way dignifies the theory which subsumes within its postulate everything that empirical procedures can produce. It will catch a datum no matter which way it may fall. It does not predict in a scientific sense. Yet it may produce a spurious sense of satisfaction to the theorist who believes that predictions are being confirmed. A great deal of emotion may be marshaled to defend this procedure which gathers counterfeit wool in place of the proper currency of science: testable fact.

**ANARCHISM.** A final point in the negative bill of particulars — the least understandable and most outrageous one of all. There is a current and, let us hope, only local belief that because science recognizes there is no absolute knowledge and all knowledge is relative, that — here it comes — anybody can define science in any way he wants to. This is a *non sequitur* so horrendous that the writer could not have believed it to come from a presumably trained source. What can one say about such a syllogism? It is not so much the expression of abysmal ignorance that is serious, it is the motivation which must lie behind it that is frightening. It is possible to do something about ignorance, but what can be done about the *desire* to be anti-scientific?

This leads to the final problem of this paper. For the writer whose research strongly suggests that the saying of "wrong" is not enough to eliminate forms of behavior, the proper question he may ask himself is: what good can come from this negative specification? The theory in learning has it that alternative forms of correct behavior need to be present and available before negative instruction can do any good. But this theory assumes a motivation to be correct and correctness is always defined by the experimenter, not by the subject. If there is no such motivation to be correct by the experimenter's definition, there is nothing the latter can do if he cannot change the subject's motivation.

There is another theory, this time from clinical psychology, that says positive corrective action is not possible until there has been a release of negative expression to clear the air. For myself, the air has been cleared.

#### LITERATURE CITED

1. BERGMANN, G. and SPENCE, K. W. 1941. Operationism and theory in psychology. *Psychol. Rev.* 48: 1-14.
2. BORING, E. G. 1929. A history of experimental psychology. New York: Appleton-Century-Crofts.
3. BRIDGMAN, P. W. 1949. Science, materialism and the human spirit. *The Wiley Bulletin.* 32: No. 3.
4. CAMPBELL, N. R. 1920. *Physics, the elements.* Cambridge: Harvard University Press.

5. **CANTRELL, H., et. al.** 1949. Psychology and scientific research. *Science*. 110: 461-464; 491-497; 517-522.
  6. **CARNAP, R.** 1936-37. Testability and meaning. *Phil. Sci.* 3: 419-471; 4: 1-40.
  7. **CARR, H.** 1933. The quest for constants. *Psychol. Rev.* 40: 514-532.
  8. **COHEN, M. R. and NAGEL, E.** 1934. An introduction to logic and scientific method. New York: Harcourt Brace.
  9. **DEWEY, J.** 1896. The reflex-arc concept in psychology. *Psychol. Rev.* 3: 357-370.
  10. **HILGARD, E. R.** 1949. Human motives and the concept of the self. *Amer. Psychol.* 4: 374-383.
  11. **HULL, C. L.** 1943. Principles of behavior. New York: Appleton-Century-Crofts.
  12. **MARQUIS, D. G.** 1948. Research planning at the frontiers of science. *Amer. Psychol.* 3: 430-438.
  13. **PEATMAN, J. G.** 1949. Policy and plans of APA. IV. *Amer. Psychol.* 4: 486-489.
  14. **SHAFFER, L. F.** 1947. The problem of psychotherapy. *Amer. Psychol.* 2: 459-467.
  15. **SPENCE, K. W.** 1944. The nature of theory construction in contemporary psychology. *Psychol. Rev.* 51: 47-68.
  16. **UNDERWOOD, B. J.** 1949. Experimental psychology. New York: Appleton-Century-Crofts.
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