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Editorial Statement: Theory and Method and Their Basis in Psychological Investigation

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It is ironic that after 100 years of existence the social sciences and particularly psychology are still muddled in an experimental and theoretical quagmire concerning what to study and how to study it. That psychologists are arguing over subject matter is evident in the existence of well over 1000 related publications, each with an editorial policy hypothetically distinct and separate from the other. Nevertheless, every manuscript submitted for publication must be substantiated by some form of methodological backing; methodological alternatives are cited to explain experiments gone awry as well as experiments *not* gone awry. Results can be easily attributed to inconsistencies in the testing procedures or artifacts in the experimental design. Yet at the bottom of all this wrangling lies the suspicion that these polemics are not mere superfluity, but that psychology's difficulty in coming-of-age as a recognized science is inevitably in the nature of its subject matter. Regardless of the orthodoxy of one's methodological approach, the existence of "mind" has become psychology's nemesis, and invariably makes its presence felt, if only to be dismissed as auxiliary to the investigation at hand.

In Support of Research: Sound Theory of Proper Methodology?

As any active researcher knows, publishing conditions within psychology are dilemmatic. Although the number of academic journals is obviously on the rise, the body of knowledge itself remains largely unorganized and dissociated. While the number of manuscripts continues to increase, scientific credence in psychology does not seem to be growing with them. The quest for credibility has become a retreat: journals become more narrow in their scopes and more precise and inflexible in their editorial policies. Editors do not screen potential articles on their total content and objectives, but often on the particular methodology supporting an idea. Often the criteria for publication rest within the methods of support themselves, with minimal attention paid to the

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meaning and implications of the theory being tested. By overemphasizing methodologies — often to the exclusion of sound theorizing — teachers, researchers, and editors have turned instrumentalism into the sine qua non of psychology. This trend, which began with the call for a stricter operationalism, has evolved to such an extreme that the operations used to translate theory into method have gained the stature of models themselves. In the case of factor analysis, for example, researchers may fail to realize that the rotation and extraction of factors is dependent upon what "goes into" the correlation matrix, and the items selected for the matrix are directly theory-relevant (Gold, Note 1).[1]

Science represents the pursuit of objective knowledge gleaned from observation (Neale and Liebert, 1973). Hypotheses are explanations used to account for consistencies in observation, and methodologies determine whether observations are reliable. Observations without methodological certification should not pass through the final scientific screening process. Informally, however, scientific progress does not work in such an ordered fashion: Scientific advancement depends upon the quick, untested exchange of ideas and hypotheses (Horrobin, 1975). Frequently, scientists generate ideas without apparent contextual order; these insights are the forerunners of the formal hypotheses and are crucial to the expansion of scientific thought. Such insights comprise the matrix of ongoing discovery, though they are seldom taken into account except in retrospect.

The relationship between ideas, theories, and their applications is an issue of validity (i.e., the truth of the theory). It is a scientific cliché that one's methodology cannot totally validate an axiom; consequently, methodology pertains more to the issue of reliability (i.e., are the results consistent over time) than validity. By assuring ourselves that our results are reliable, we can then take the next step toward securing validity. In actuality, however, methodological rigor can often create a false, premature sense of validity. This occurs when (1) an unsound or careless theory is lent experimental support via a particular design, (2) post hoc analyses uncover results consistent with theoretical expectations, and (3) certain experiments deemed methodologically "tight" are published over theoretical dictums not supported by such elaborate methodology. Whether experimentally reliable behavior is indicative of theoretical truth remains, more often than not, for further verification. Because journals typically publish reports disconfirming the null hypothesis, many interesting, untested null hypotheses go unexplored. Scientific conservatism dictates that we examine *differences* between variables before

[1] Clusters within the matrix can virtually always be obtained (rotation usually stops when the eigenvalues account for less variance than a particular test item's standard score), and even when the amount of variance accounted for by the factors is not substantive, experimenters have no reservations about labeling factors *as if* the truth or falsity of their theory were determined. An experimenter can easily fulfill his or her prophecies in the process of labeling factors, yet plead ignorant of the dialectic existant between theory (factors) and method (rotation of the matrix).

we test *similarities:* Two variables can be more easily proven different than they can be proven consonant. Both convergent and discriminant validity are necessary in the development of a theory; nevertheless, with the emphasis upon the "positive" result as an indicator of cause and effect, discriminant and convergent validity have not been equally respected.[2] Accentuating the positive result further heightens our false hopes by giving us confidence in the internal consistency of experimental procedures while saying little about the applicability of the experiment (Lana, 1969).

Ultimately, overemphasis upon methodology and operationism may result in the application of methodological jargon that bears no relationship whatsoever to the ontology of the subject (e.g., Hull 1943).[3] Hypothetical constructs serve no longer as mere tools for explanation, but supplant the systems they are devised to explain. Under the influence of such evolution, models have become technological surrogates, appealing in their component parts but ritualistically pedantic in their capacity to validate. As such instrumentalism increases, disciplines become more technologically defined; a separatism results, influencing the structure and definition of paradigms. Paradigmatic agreement becomes rare; moreover, this disparity legitimizes and even encourages the trend toward greater specialization.

Internal and External Validity

Any sound and proper experiment must satisfy two criteria — internal and external validity (also called experimental and theoretical validity). The relationship between internal and external validity is paradoxical. In order, for instance, to satisfy the criteria of internal validity (i.e., is the experimental treatment responsible for the obtained results), the experimenter must establish experimental control. Such control keeps plausible, rival hypotheses which threaten to account for one's findings to a minimum. Good experimental procedure requires that we guard against the contaminating influences of experimenter bias, subject maturation, differential selection of subjects, instrument decay, measurement influences, statistical regression, and differential loss of subjects due to treatment condition (Campbell and Stanley, 1966). This keying-in on control is a consequence of narrow operationalism: Such control insures that the

[2] Convergent validity is based on the idea that two measures of the same thing should correlate highly; discriminant validity assumes the lack of correlation between two variables purported to measure different processes.

[3] Hull (1943), for instance, determined that to predict a particular response from an organism, one would have to ascertain the following: stimulus energy, neural result of the stimulus, neural interaction arising from the impact of simultaneous stimulus components, habit strength, efferent impulse leading to action, occurrence of reinforcement, evocation potential of stimuli on the same stimulus continuum as the original, generalized habit strength, drive strength, work involved in an evoked reaction, reactive inhibition, conditioned inhibition, effective reaction potential, oscillation, momentary effective reaction potential, and reaction threshold.

theoretical variable gets translated into the independent variable, and that the independent variable is the cause of the differences between experimental and control groups. By holding all variables other than the independent variable constant, one's treatment achieves confidence.

Yet the emphasis upon control can preclude the experiment's external validity (i.e., can the results be generalized to situations outside the testing environment, and to the population the sample approximates). Because the variables under investigation naturally occur in conjunction with other variables — not as isolated moments of stimulus-response sequences — the external validity may suffer an undertermined artificiality which results from equating experimental and control groups on all variables other than the independent: The greater the emphasis upon experimental control as a means to assess the influence of one's treatment, the greater the isolation of the subject from other relevant clusters of simultaneously occurring variables. To this extent then, does one's experimental situation fail to mime the environment it strives to represent.

Undue emphasis on control may not only mislead an experimenter into believing that an erroneous theory has been proved, but restrict the proper realm of research. Fundamentally, experimental treatment levels should be picked randomly (from an array of sequentially-related treatment levels); in reality, treatment levels are deliberately chosen with the probability of obtaining successful results firmly in mind. An experimenter chooses the treatment level on the assumption that certain consequences will follow. If these consequences don't occur (an issue of internal validity), most experimenters reorganize their experimental procedures (seek better control or choose different treatment levels), instead of examining the theoretical ramifications of the null result. If expected results are obtained, the experimenter concludes that the results are applicable to larger populations, often without testing other treatment levels.[4]

But psychology's maladies do not end with its inability to sanction results by means of theoretical validity. The validity of a theory should grow or diminish with the substantiation or disintegration of previous, related theories. Even so, it has become an uncommon practice in the social sciences to cross-check one's theories and results with related disciplines. For example, within psychology itself, cognitive dissonance theory (a substratum of cognitive psychology) uses neither the same terminology nor schemata as instrumental conditioning theory (a substratum of the psychology of learning) — though both theories may apply to similar behavioral processes and like experimental issues. It is not surprising that displacement of one theory by another rarely occurs

[4] Often, scientific progress depends upon the utilization of "randomly-chosen events." Such was the case in the discovery of oxygen (Kuhn, 1962): it was only through Priestley's reluctance to continue mixing the unknown gas with standard volumes of nitric oxide that oxygen was finally discovered.

when in fact the theories purport to explain different processes derived from different premises. Similarly, we should not raise our eyebrows to the realization that hypothetical constructs bear little resemblance to the ontological precepts they test and explain, nor that they possess any theoretical reality apart from forming a logical basis for further formulation of models. Contemporary models of human experience attain an alarming degree of heuristic independence: Any attempt to uncover finer understanding of mediating processes, or to realize semantic frameworks, would be a violation of the model's parameters.

Because subjects do construct meanings out of experimental manipulations, psychology must recognize the existence of phenomenal translations that occur between treatment and result. The inability of experimenters to speak sensibly about interpretations of treatment-effects may not restrict their use of the scientific method, but will obviously limit what advancement their method can offer psychology. Skinner's (1950) query whether theories of psychology are necessary is a notable case in point. If one accepts the ethics of his system, one can seldom find fault in the internal consistency of his experiments — his methods and results are impeccable within the confines of his system. However, if one's explanations of human behavior bear no a priori relationship to the processes they seek to clarify, such models will be difficult to test, and impossible to prove, because their final validity depends upon real, ontologically ascertainable processes, and not upon referentially untestable semantics.

Even if the *crucial experiment* were a possibility, the results of such an experiment could always be reexamined with rival hypotheses and post hoc methodology.[5] Such constant doctoring can lead to fractionation of the discipline: Carried to its logical outcome, a sectarian network of subdisciplines neither sustained by similar vocabularies nor refined by interdisciplinary communication would result (Turner, 1971). This splintering further promotes methodological dissension because the members of each subdiscipline chauvinistically defend their method as *the* true scientific prototype. Competition prevailing in place of cooperation leads to complete separatism. Because no two disciplines are founded on the same set of assumptions, methodological techniques become more finely tuned — each technique offering substance to a foundationless psychological science.

The substitution of methodologizing for the process of theorizing may be partially the fault of competitive training in graduate schools, where students are required to publish research reports before they have become cognizant of the theories involved in their research. In this respect, they imitate their mentors faithfully, as Kuhn (1962) maintains,

> Though scientists talk easily and well about the particular individual hypotheses that

[5] Of course, post hoc statistics are not considered completely "proper" because after-the-fact tests can abuse the laws of chance and therefore violate the basic assumptions of the a priori tests.

> underlie a concrete piece of current research, they are little better than laymen at characterizing the established bases of their field, its legitimate problems and methods. If they have learned such abstractions at all, they show it mainly through their ability to do successful research. That ability can, however, be understood without recourse to hypothetical rules of the game. (p. 47)

If psychologists are really going to study human behavior, they must eventually offer hypotheses concerning the unseen sets of variables that transpire between onset of a stimulus and onset of a response. Such hypothesizing sheds light on the possible verification of processes assumed to exist; it offers the promise of cognitive reification, and it eliminates the unrealistic chasm between the subject and the operations implicit within the individual that have served as assumptions from the beginning.

The Politics of Experimentation

Not only are some methods of scientific experimentation granted greater credence than others, some individuals are granted greater scientific freedom than others: Typically, journals only publish theoretical papers from authors with recognized, outstanding credentials (Horrobin, 1975). As Horrobin (1975) states: "The rule . . . is that ideas can be presented or criticized only by those with a record of experimental work in a field" (p. 1). Although it is realistic to assume that some individuals are more capable researchers, and others better theoreticians, the policies of academic journals do not allow for such a plausibility. If journals continue to proselytize methodology alone and defy laws of ratiocination, the most we can hope for is the buildup of a super ficial body of knowledge, which ignores the very distinctions making psychology different from both reductionist biology and theoretical philosophy.

The editors of *The Journal of Mind and Behavior* believe that existent publications are unable to provide a forum for ideas concerning human behavior that will lead to optimal development of scientific understanding. We wish to revitalize the premise that empirical method rests upon an implicit basis of thought and motivation. When Galileo designed experiments to demonstrate that the velocity of a falling body is independent of its mass, he constructed a physical situation which approximated an ideal: frictionless interaction without air resistance. His constructs were idealizations differing from the reality of physical situations outside his experimental domain, yet belonging to that same reality insofar as his experimental abstractions formed systems, and their behavior was approached by real systems asymptotically. In this sense Galileo was the prototypical scientist, meeting the demands for internal and external validity. Looking back on Galileo, it is perhaps difficult for us to imagine ourselves without the intellectual heritage to abstract from a myriad of perceptions those which lead to an understandable description of physical systems; it is our hope that one day we may have the same reflections with regard to psychology.

In conceiving this journal the editors wish to rely upon the paradigm of empirical science as a fundamental basis of scientific knowledge rather than be a slave to it. The editors recognize the need to propagate ideas

and speculations as well as the need to form empirical situations for testing them. It is of prime importance to draw ideas from empirical results; only under such conditions can ideas interact in a generative process. For laws to be offered as explanations of events, the laws themselves must be examinable.

Although the physical world has cooperated with the empirical method, mental events hold themselves aloof: not because they do not form part of the human experience — to state that would be absurd. All scientific observation is, of course, an experience of mind as is speculation and hypothesis. Empirically, we have not been able to find constructs abstracted from the reality of the universe of mental events which will form a system to explain the order of mental occurrences themselves.[6] These, of course, are part of the human experience.

The Journal of Mind and Behavior will review manuscripts from diverse scientific approaches. The editors believe in a working reciprocity between theory and method as well as a unity among the sciences; the editors are receptive to both traditional and non-traditional approaches to the various disciplines. Although it is often difficult to distinguish editorial policy from individuals comprising an editorial review board (or the decision-making processes used by these individuals), we hope to maintain the process as an open system — both affecting and being affected by the flow of articles.

[6] We include in the class of constructs abstracted from the universe of mental events *all* behavioristic, physiological constructs.

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