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The False Promise of Falsification

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The principle of falsification is discussed in light of the fact that it deals exclusively with methodological considerations. It is up to the researcher to decide which of several theories should be put to the test of falsification. Unfortunately, current prejudices against telic description in psychology permit journal editors to reject sound empirical evidence based on (a) confusions between theory and method, and (b) misunderstandings of the role of falsification as a methodological rule of procedure. Since there must always be the possibility of an alternative theory accounting for any observed pattern of evidential data, it is inappropriate to dismiss a theoretical explanation based on the claim that another theory can "account for" the findings observed in an experiment. This common practice in the editorial review of psychological researches has made it impossible to falsify telic theory. Recommendations are given for rectifying this unobjective and essentially repressive tendency in the future.

There is what amounts to a tradition in psychology which holds that, even though we may wear our theories lightly, we always take the weight of empirical evidence seriously. This tradition encourages us to look suspiciously at the "armchair" theorist, a label which is applied to psychologists who supposedly accept the plausibilities of (biased) reason as a standard of proof rather than expending the effort to gather properly scientific evidence through experimentation. The "objective" psychological theorist is willing to submit his or her ideas to empirical test, from which a decision can then be rendered on the soundness of a line of development, the making of a case, and so on. Granting this empirical tradition, it is natural to believe that though psychology suffers certain drawbacks in the realm of theoretical flexibility, it at least avoids the pitfalls of having doctrinaire theories being supported more by the weight of unyielding convention than by the weight of changing scientific evidence.

The present paper challenges this view of psychology as a science open to alternative theoretical descriptions so long as the rigorous test of empirical falsification is met. Thanks to some oversights in the total research effort, psychology has fostered a selective disregard for certain forms of hard evidence in favor of protecting what is already accepted as "the" scientific style of theoretical explanation. Indeed, it is herein contended that the weight of theoretical convention is so great in modern psychology that methodological evidence is regularly *ignored* in the name of scientific

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rigor. In order to provide some background for this paradoxical charge, we will first turn to a consideration of the principle of falsification.

Principle of Falsification

Although other philosophers of science have alluded to something of the sort, the principle of falsification is usually attributed to Karl Popper (1959), who noted that "*It must be possible for an empirical scientific system to be refuted by experience*" (p. 41). A scientifically phrased hypothesis must be put in such a way that the observed events to follow either support it or refute it without equivocation. Thus, Popper asserts that the statement "It will rain or not rain here tomorrow" is *not* an empirical hypothesis because it cannot be refuted, whereas the statement "It will rain here tomorrow" *is* empirical because it can be negated through observation — i.e., it can be *falsified.*

The principle of falsification, therefore, refers to the logic of scientific validation, and specifically to the fact that the *course* of proving anything to be true or false follows an "If-then" sequence. An experimental scientist essentially reasons as follows: "*If* my theory is correct, *then* the events which I have arrayed in a certain way via my independent-variable and control-variable(s) manipulations will result in 'that' and 'only that' outcome as observed in the dependent-variable measurements to follow." The stipulation of what does or does not vary over the course of an experiment is unequivocal; therefore, the theory under test is definitely put to risk. Things might *not* go as predicted, and thanks to the singularity of the predictions we can judge whether or not the theory has empirical validity.

As any logician will testify, there is an insurmountable problem with this If-then course of scientific investigation. Donald K. Adams (1937) once pointed this fact out to Clark Hull (1937) in the context of Hull's hypothetico-deductive technique, noting that it necessarily "affirmed the consequent" of an If-then line of reasoning. If we were to express this logical error syllogistically it would go as follows: The major premise, which unites an antecedent and a consequent meaning into a further meaning-relation, would state "If a human being appears, he/she will be a mortal being." Affirming the antecedent term through what is called the minor premise, we would state "This identity standing before me is a human being." It follows necessarily: "Therefore, this identity is a mortal being." However, had we affirmed the consequent term of our major premise and said "This identity standing before me is a mortal being," we could *not* conclude "Therefore, this identity is a human being." There are many sub-human beings which are nevertheless mortals.

Returning to the logic of empirical investigation, when the scientist says "*If* my theory is true, *then* my empirical data will array as predicted" this is akin to a major premise. The research experiment to follow is akin to the minor premise, and if the scientist is lucky, he or she can affirm at some point: "My observed data arrayed as my theory predicted." Does this mean the scientist can conclude with certainty (necessity) that "My theory is true?" Obviously not, because he or she has affirmed the consequent of an If-then line of reasoning. Of course, science proceeds in spite of this built-in proviso that for any observed fact pattern there are alternative theories that could possibly account for the empirical facts.

It is due to the error of affirming-the-consequent that we used to insist that all experimental hypotheses be reframed in the null form. Because all an empirical study can do is falsify, it is pointless to speak about anything except the *rejection* of some assertion concerning what will or will not be observed. We can never or at least should never accept the null hypothesis — i.e., affirm the meaning contained therein. The same goes for *any* hypothesis about observed events. When we believe we have learned something in an experiment, we can only hold to this belief out of a framework of information which has nothing directly to do with the evidence *per se*. Evidence and the framework in which it is embedded are different aspects of the total scientific effort.

There is a fundamental difference here between *theory* and *method* (Rychlak, 1968, p. 76). Theories are meaningful relations between terms or "constructs" as we usually call them, but the question of whether or not to believe in the meanings framed by a theory is of a totally different order. We base our belief in a theory on some *method* of bringing evidence to bear. The first test is always a kind of coherence test of truth in which things hang together plausibly or not, and it may be termed *procedural evidence* (Rychlak, 1968, p. 75) because it allows us to "proceed" with our theoretical line of development. We see procedural evidence in operation in the tests of mathematical consistency, self-evidence, and more generally, common sense. Philosophical proofs and even so-called "theoretical proofs" come down to procedural evidence tests of meaning relations within congeries of even broader meaning relations, all of which signify an internal consistency to the reasoner. In the 17th century, with the advent of the scientific method, a form of evidence was propounded which called for an empirical demonstration based on preliminary controls and the prediction of an outcome. This requirement of a correspondence between an hypothesis and the array of events to follow is usually called *validating evidence* today (Rychlak, 1968, p. 77).

Thus, thanks to our common-sense understanding of what we must do in order to be scientifically correct, the principle of falsification is accepted by us based on procedural evidence. We do not have to go out and conduct an empirical study to see the merits of such a principle because it makes "intrinsic" sense from the outset. Even so, as a prescriptive recommendation the principle of falsification refers to the domain of validation. It is, if we now begin to use the theory-method distinction, more a methodological than a theoretical rule of thumb. Its aim is to promote objectivity in empirical experimentation. Unfortunately, as we now hope to demonstrate, in psychology the principle of falsification has been short-circuited thanks to serious misunderstandings about the causes at play in experimentation, and the affirming-the-consequent insight has been used so arbitrarily as to completely invalidate its *practical* utility for the development of sound theories.

Problems With the Falsification Principle

When we are sensitive to the theory-method bifurcation, we realize that a scientific method *must* be theory free. There is a theory of knowledge lying behind scientific methodology, of course, but the context of proof within which falsification is said to operate should in no way foreclose on the validation of *any* theory put to it. So long as the theory can be framed in a testable manner, by being translated into the so-called "variables" of experimental method, then the falsification principle is expected to "work." In the later 16th and early 17th centuries, when people like William Gilbert and Sir Francis Bacon were putting down the ground rules of scientific method, a major emphasis was given to the efficient cause in the production of evidence. That is, a scientist was admonished to manipulate events and show a literal change taking place in the events to follow in the efficient-cause sense of an impetus, thrust, alteration of a substance (which involved material-cause changes as well), and so on. As Bacon said: "The secret workings of nature do not reveal themselves to one who simply contemplates the natural flow of events. It is when man interferes with nature, vexes nature, tries to make her do what he wants, not what she wants, that he begins to understand how she works and may hope to learn how to control her" (Farrington, 1949, p. 109). In time, this "interference with nature" was pictured as taking place through manipulation of the independent variable (IV) of experimentation, while observing the efficient-cause "effects" on the dependent variable (DV). We have already called this type of evidence "validating," to distinguish it from the more contemplative, "procedural" evidence.

The precise terms "independent and dependent variables" can be traced to the 19th century mathematician, Dirichlet, who coined them in elaborating on Leibniz' concept of the *function* (Eves, 1953, p. 371). Dirichlet stipulated that a variable y is a function of x when some numerical value assigned "at will" to x automatically (determinately) assigns a value to y. The x variable is "independent" because its value is arbitrary, and the y variable is "dependent" because its value is fixed by the mathematical ratio of relationship with x. Note that this tandem of independent variable-dependent variable, or IV-DV, is *not* based on efficient causality. This "functional relationship" is solely a formal-cause designation, so that whatever determination there is between variables is through the *intrinsic pattern* defined by the mathematical assumptions. Of course, such relations can be used to measure the observed changes in variables as measured empirically, some of which may be under the direct manipulation of the experimenter and others of which may be tracked in their natural setting. Psychology has considered the direct manipulation of experimental variables to be more "basic" scientific investigation than those studies in which the IV and DV are framed in the natural setting, outside of direct (efficient-cause) control.

And yet, the fact remains that all the scientist *ever* perceives is a relational tie of IV to DV, which describes what is sometimes called a *law*. As Ernst Mach was to argue for physics at the turn of this century, *all* observed regularities of an IV-DV nature are more akin to correlational relations — that is, formal causes — than to the efficient causes of an antecedent event manipulating, thrusting, or triggering a consequent event to move along over the passage of time (Bradley, 1971). Mach rejected the naive realism of the Newtonians in favor of what he called his phenomenological physics. It is not widely recalled today that when B.F. Skinner (1931) critically attacked the primitive S-R conceptions of reflex psychology he pointedly drew from Mach's writings and was careful to speak of the "observed *correlation* of stimulus and response" (p. 439; italics added). John Watson did not think of the stimulus and response as a correlational (formal-causal) relationship, but as the two ends of an efficient-cause sequence.

If we are careful to think of the IV-DV relationship as a formal-cause patterning — whether we call this correlational, mathematical, or logical — then there is a basis for keeping our methods separate and distinct from our theories. That is, we can frame our observations and measure them objectively in the IV-DV format, and yet separately consider it our duty *also* to frame a theoretical account of why this patterning is to be observed. Why does the Machian phenomenal pattern occur? We might then explain these observed regularities of human behavior in stimulus-response (S-R) terms, or, suggest that a subject behaves in the patterned way thanks to predications "for the sake of which" he or she *intends* to behave. The S-R theory would draw from the efficient cause, but the intentional theory would employ the meaning of a final cause. Final-cause theories are teleologies, and though space requirements do not permit consideration of the whole question in the present paper, it can be said that those physical sciences appearing on the historical scene before psychology had dismissed the final cause meaning as unsatisfactory for the description of material reality (see Rychlak, 1977, p. 39).

Of course, except for medicine, these earlier sciences did not profess to speak for the description of human behavior. Even Bacon, who was adamant in his rejection of final causation for physical-science description, included final causes in his broader conceptions of knowledge when the human condition was under consideration. One might have hoped that in the birth of a new science like psychology a vigorous wing of the discipline might have explored this crucial question regarding human nature. Is the person an agent in his or her behavior, or not? Those who would say "yes" in response to this question would have to go beyond description of behavior in an exclusively efficient-cause fashion. Agency is related to telic description, which means that the person is seen to influence behavior in opposition to or at variance with past experience or current environmental manipulation *at least some of the time*. The person is on the "independent" side of the IV-DV tandem in a teleology, directing events to follow "at will." Unfortunately, the question of agency in psychological science was never to be properly analyzed, thanks to the fact that the formal-cause nature of the IV-DV relationship was never really appreciated. Whenever a "rigorous" psychologist of this century garnered empirical evidence in the experimental context, he or she assumed that it was an efficient-cause tandem which was under consideration.

How could this arise? Because our leading psychologists, beginning with Watson, and on through people like Tolman, Weiss, Stevens, Spence, Hull, and yes, even Skinner who might have retained his Machian lessons a bit better, consistently equated the S-R efficient-cause sequence with the IV-DV methodological sequence (see Rychlak, 1977, pp. 170-172). To cite just one example, Bergmann and Spence (1941) framed the goals of our discipline as follows: "Like every other science, psychology conceives its problem as one of establishing the interrelations within a set of variables, most characteristically between response variables on the one hand and a manifold of environmental variables on the other" (pp. 9-10). To show how ingrained this tendency to mix theory talk (response) with method talk (variable) has become, the reader may look at page 578 of the widely used psychological dictionary by English and English (1958), where we are informed that an independent variable is either a stimulus or an organismic variable, and the dependent variable in psychology "is *always* the response" (italics added).

If we as experimenters believe that just because a relationship emerges between the IV and DV measurements this must necessarily be an "S-R relationship" or an "S-R law" rather than an "IV-DV law" we have effectively silenced the falsification principle. Why? Because it then becomes impossible *in principle* to invalidate S-R theory! Even when the IV-DV relationship does not come out as predicted but achieves significance in some unexpected way psychologists are likely to see this as further proof that S-R regularities are "out there," taking place in the hard reality and just waiting to be tracked. We state "S-R," but of course the precise theoretical language shifts back and forth between modern cybernetic lingo of the input-feedback-output and our more historical phraseology of stimulus-mediation-response. The important thing here is to recognize that an efficient-cause *theory* is pressed upon a *method* in which formal causation is *also* at play, making it impossible for falsification to test any but an efficient-cause theory.

This confound of IV-DV with S-R can be devastating to the teleologist who wishes to put a non-efficient-cause theory to the test of falsification. For example, assume that a final-cause construct like *telosponsivity* — i.e., behavior "for the sake of" predication rather than "in response to" stimulation (Rychlak, 1977, p. 283) — is translated into an experimental design and tested according to the IV-DV manipulations of validating evidence. It may even be cross-validated several times. Yet, the hapless teleologist is likely to find that the editorial reviewers of our most rigorously prestigeful journals frequently remain unimpressed and unmoved by the evidence. Seeing *their* efficient-cause (S-R) theory in the teleologist's (IV-DV) findings, these reviewers dismiss the telic theory prompting the experimental test as fanciful and unnecessary elaborations of the "observed facts."

This direct substitution of the S-R efficient cause for the IV-DV efficient cause regularly takes place in journals which profess to be "basic" in research orientation, such as those relating to experimental psychology, verbal learning, or cognitive psychology. If an effort is made to head off such mechanistic distortions of the findings by introducing the arguments now under consideration, the teleologist is likely to be lectured by the editor — in the letter of rejection — who suggests that such "philosophical" discussions have no place in "basic" research journals. The upshot is that there is no way in which to break into the protective encampment of these "basic" psychological scientists, ringed as it is by a purely *theoretical* style of explanation which the teleologist does not happen to favor but which he or she cannot call into question even when empirical evidence is involved.

In the journals which specialize in clinical, social, or personality psychology a somewhat different but related maneuver takes place when the reviewer confronts non-efficient-cause theory. This maneuver trades on the affirming-the-consequent fallacy that all sciences must operate under. It was brought home to the writer recently when a journal submission was returned with literally *no* criticism made of the basic experimental design or the manner in which data were scored and analyzed. What were the reasons given for the rejection of the study? Well, one reviewer thought that the "response style" literature could have accounted for the findings (which were framed around telic description, of course). The second reviewer cited "cognitive consistency" as the theoretical explanation of choice. And, as if for good measure, the editor personally tossed in the likelihood that "saliency" was the best way in which to frame the *unchallenged* IV-DV (methodological) findings.

Now, there is no *a priori* reason for doubting that response style, cognitive consistency, or saliency formulations might "account for" the experimental findings in some fashion or other (though the reviewers failed to spell anything out). Let us assume for the sake of argument that these alternative theories could have accounted for the findings. Is this an acceptable reason for rejecting the research report? We have already noted that, according to the principle of falsification and the affirming-the-consequent of an If-then line of logical reasoning, there must *always* be the possibility of an alternative theory accounting for any fact pattern. As Ernst Mach once said of physical explanations: "Different ideas can express the facts with the *same* exactness in the domain accessible to observation. The *facts* must hence be carefully distinguished from the *intellectual* constructs the formation of which they suggested" (cited in Bradley, 1971, p. 83). In other words, for any observed IV-DV fact pattern there are *in principle* "*N*" potential alternative explanations of what is taking place.

What the reviewers of this submission did not understand is that all any scientist can falsify is his or her *own* theory in submitting it to a given experimental test. The reviewers' responsibility was to confront the *theory under submission* at its own level and to decide "Did the facts meet the predictions made with proper scientific safeguards?" If we are to base rejections on the fact that there is a possible or even actual alternative explanation of the findings cited, then we might as well close down our journals because this is a universal truism, applicable to every submission and for all time. It is, of course, perfectly acceptable to challenge the theory under submission and to show, for example, that there is an anomaly in the findings which can be accounted for by an alternative theory. If serious enough, this could surely provide grounds for rejection. But there was no such anomaly mentioned. There was no reason given at all why the reviewers preferred their theory to the one under submission, except that they were unfamiliar with the latter and by implication lacked the time to review the book and research publications cited in the write-up.

If journal reviewers are to prescribe what theory a psychologist is to use, then there should be more — much more — journal space given over to the *reasons* selected for taking one approach or another. This is clearly a theoretical consideration, unamenable to empirical resolution but of the utmost importance — more important than any study imagineable! Thus, for example, an exchange of this sort would point up that theories of response style, cognitive consistency, and saliency are all based upon non-telic, mediation conceptions of behavior *moving over time*. But what if the telic theory under test is based *not* on mediation conceptions, *not* on principles of frequency and contiguity, but rather on predication conceptions and a principle of tautology in what is considered a logical form of learning (Rychlak, 1977, p. 209)? How can a teleologist remain true to his or her own thoughts and then go on to translate these ideas into some variant theory which must necessarily rob them of their core meaning? This is the problem posed by such, if not arrogant then at least supremely confident, decisions of those who believe themselves to be standing for scientific method against the "talky talk" of teleological phraseology.

And it is right here that one begins to realize that psychology's heralded commitment to method is *in fact* a covertly protectionistic commitment to *one* style of theorizing about behavior, and *one style only*. If one's theory does not readily tautologize with the IV-DV sequence, a linear, sequential, demonstrative conceptualization of events moving via efficient causality over time, then there is no possibility of putting it to the test of falsification as currently practiced. Psychology has, as Burtt (1955) once said of the Newtonian, transformed "a method into a metaphysics" (p. 243). The problem today is not "armchair" theory but "lab stool" theory, framed as it is in terms of the methodological apparatus (research design) rather than in terms of the theoretical conceptions which may have prompted construction or design of the apparatus in the first place. We have even adopted an imprecise way of speaking about our empirical work, referring to the study of "variables" at every turn. This is true enough if we mean that once we have framed what behavior means to us theoretically we now translate our ideas into *methodological* variables for the testing of our hunches. But, if we mean that there are literal "variables" interlacing "out there" in reality — whether of a personal or situational variety — and that these somehow interact and summate to efficiently cause behavior we are merely rekindling the Newtonian flame. Unfortunately, the falsification principle is still held aloft as talisman for the eventual resolution of our basically philosophical problem in psychology — holding out what is undoubtedly a *false promise* for the future.

Some Hopes for the Future

If the principle of falsification is not working, if we cannot even design a study to test the alleged inequities that now exist in the development of new ideas in psychology, then what *can* we do to make our science more objective? Whatever we do, it will not be something which has been suggested by research findings, nor can we design an empirical study at present which might tell us how to rectify the situation. In other words, whether we like it or not, we face here a philosophical problem in theory construction, and it will be corrected only through a more sophisticated understanding of the philosophy of our science in the future.

One of the first things we can do is to draw a clearer distinction in psychology between what are our theories (Mach's intellectual constructs) and what is our method of validating these theories. When we do this we appreciate more fully that the principle of falsification makes reference *solely* to the methodological context. The principle of falsification can *never* select the "right" theory for us to put to test. This principle has nothing *at all* to do with theoretical explanation! It is concerned with how theoretical explanations are framed for testing — but never with how the theoretician reasons to account for the expected course of empirical observation. Two theories could be equally falsifiable; yet one might be neglected out of simple prejudice and, therefore, never given the stature of a "scientific" theory. To achieve a more open confrontation of theories, we must take as much interest in theory construction as we now take in methodological validation. We have to study the theoretical and even "metatheoretical" assumptions made in the framing of all hypotheses put to test in an experiment.

If we are reviewers of a paper submitted for publication, and the language is strange, it is not sufficient for us to dismiss this language in favor of our more familiar terminology. At the very least, we should make an effort to understand why the theoretician submitting data to test is framing things in the manner chosen. This may call for additional communications between the person submitting and the person reviewing the paper in question. Too often, a research submission is given a "reading" by an editor, and decisions rendered are based on a half-baked understanding of why the research is being conducted and even less understanding of what went on in the procedure. It is clear in such a case that what the reviewer wants to do from the outset is find a reason for rejection, so that we have a kind of "game" taking place in which anything at all is thrown up as a serious objection to publication. Often those studies which *are* accepted for publication have no implications for what a lay person would consider *basic* human issues — such as the nature of mind, identity, free will, and so on. Yet these superficial or artificial experiments are precisely the ones likely to be called basic research. This interpretation rests on the Lockean assumption that there are underlying, simple laws which can be coaxed out and studied as a basis to everything else, including the subtleties of human nature. In other words, knowledge is expected to *summate* from the basic to the complex so that no matter how trite an empirical regularity may be, if it has reliability, then we can expect it to "add up" someday to knowledge when put together with other empirically observed regularities or "laws" (as these methodological findings are usually called).

No one really believes any more that experimental findings are going to coalesce into uniform knowledge — we used to call this a "higher-level law" — but our traditions force us to behave as if this were still true. We focus on the experimental procedure at the expense of the theory being investigated, which is often never even mentioned but only assumed in a research report. This is why the methodological context begins to serve as a theoretical frame of reference *as well as* a strictly evidential context. The "truest" theory becomes the one which is closest to a rephrase of what is observed in the IV-DV regularity, with assorted embellishments to make the underlying efficient causality palatable to the masses.

As another step in the direction of liberalizing psychological explanation, the writer would favor a kind of "running contact" between a researcher and the journal in which publication is being sought. This would entail an exchange of letters with the editorial staff, and also some possible *contractual* arrangements for the future; e.g., the researcher might agree to conduct a further experiment, and the reviewer would guarantee publication, assuming certain results were forthcoming. At the very least, such exchanges would undoubtedly slip over into philosophical issues concerning what can or cannot be tested, what the independent variables really measure, and so on. Some of these exchanges might be published even if further research is not conducted. That is, if a journal were to publish an original submission *plus* the exchange of several letters between a reviewer who rejected this submission on theoretical grounds and the psychologist who submitted the paper and felt put upon due to theoretical bias, it could well prove more instructive and widely read than most present publications. We would have empirical data to contemplate, but we would *also* be drawn into the philosophy of science. There is the practical problem of anonymity, but the editor's contribution could be kept anonymous if he or she would prefer it that way.

This published exchange would give research — or at least certain forms of research — an openly polemical quality, and some of us would find this distasteful. It would appear to be "mixing science and politics," but the parallel is not accurate. Politics accepts the rule of vested interest regardless of an impartial standard against which to judge what is being asserted. In science, we do have an impartial standard in experimental validation. As scientists, we put our philosophical assumptions "on the line." We design a study, call the shots, and then step back to see what happens with our fingers "out of the evidential pie." Politicians are paid to keep their fingers in as many pies as possible. But what we learn when we follow the rules of scientific logic is that there are many *different* ways of slicing the same pie.

To say that research journals deal exclusively in hard data, and that the underlying assumptions of a philosophical nature have no place in such journals is to undermine the possible development of sophisticated theory in psychology. There ought to be more of this kind of examination going on. Indeed, once each year an experimental journal should invite critical reviews of the work reported in its publications by psychologists who do *not* favor its preferred theoretical orientation. These, essentially philosophical, critiques would relate *solely* to the research papers published in the journal, offering (if possible) alternative interpretations of the research line, or, challenging its instructiveness in general, suggesting certain key studies if the line is to be more instructive, and so on. It goes without saying that such critiques could be rebutted by the editors or readers of the journal in question.

The effects of such an exchange could only prove salutary. It would cultivate a sense of "how do we account for facts?" and "what kind of facts are we manufacturing and why?" rather than the currently naive — and I believe harmful — attitude of "what are *the* facts?" Our younger generation of psychologists would begin to see that experimentation is always concerned with a relationship between theoretical predilection in assumptive understanding and the resultant course of observation in controlled circumstances. It is meaningless to speak of "the" facts in other than a strictly methodological fashion — which comes down to "the findings in the present data collection." When we begin to generalize beyond the methodological context then we must acknowledge the role of a theoretical frame in our interpretation of all such factual observations.

Why do we not have at least one of our research journals in psychology dedicated to the answering of certain basic questions about the human condition? If we had a journal willing to accept studies which falsify telic theory, as well as mechanistic theory, then we might see a continuing exchange between those who would entertain agency in human behavior and those who do not. We could have some purely theoretical exchanges, and hopefully iron out what kind of a research line would be acceptable to both sides. This would never result in the designing of a crucial experiment, of course, because we shall always have the affirming-the-consequent fallacy with us. Crucial experiments are impossible. But we might at least then hammer out a research approach acceptable to both sides. The writer personally holds that the remarkable influence of awareness in conditioning research is better evidence for agency than for mechanism. Yet the vast majority of research psychologists go along in blithe disregard of what seems clearly to be a Kuhnian (1970) anomaly in their mechanistic theories as if it were a mere puzzle. Brewer (1974) can say as the title of his excellent review of awareness in conditioning that "There is no convincing evidence for operant or classical conditioning in adult humans," and everyone yawns and turns the page.

Nobody seriously thinks that an efficient-cause view of reality has been falsified by this remarkable survey of the experimental facts to date. If Brewer had begun from a telic view of things and written a paper entitled "There are no scientifically valid data which falsify agency in the behavior of adult humans," a number of psychologists today would be far more agitated, claiming no doubt that some kind of theoretical foul had been committed in the interpretation of the experimental findings. No one sees a threat to the interpretation of what conditioning means at present because the validity of the efficient-cause theory on which conditioning theory rests is tautologized with the IV-DV sequence of experimentation. And who can deny that IV-DV facts *exist*? As we have noted, every experiment we do establishes something of the sort "right before our eyes." With this belief firmly in mind, psychologists expect the current flap over the role of awareness in conditioning to blow itself out, or probably to be rectified by some as yet undetermined breakthrough in experimentation. In either case, the outcome is that psychologists cannot be permitted to falsify telic theory. It is all in how one looks at things. And, how we look at things is what philosophy is all about.

It is only through philosophical analysis — or, variantly expressed, through theory-construction analysis — that we psychologists can clarify and correct the paradoxical illusion of objectivity which now threatens our science and keeps it from becoming that unique contribution to the family of the sciences that it most surely can be. Validation is essential to psychology, but we have the responsibility to understand as never before the assumptions made in the hypotheses put to test via the experimental method. The very core of human nature is at stake, for the conclusions we draw as "facts" from our experiments cannot help but influence what people will think about and expect of themselves as they move into the future.

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