©1991 The Institute of Mind and Behavior, Inc.
The Journal of Mind and Behavior
Winter 1991, Volume 12, Number 1
Pages 135–150
ISSN 0271-0137

Some Theoretical and Methodological Questions Concerning Harcum's Proposed Resolution of the Free Will Issue

Joseph F. Rychlak

Loyola University of Chicago

Questions of both a theoretical and methodological nature are raised concerning Harcum's interesting paper on the resolution of the free will issue. The theoretical questions deal with the meaning of "free" as the supposed capricious disregard of environmental circumstances, the theoretical perspective from which agency is construed, the sort of causation that is involved, the choice of a predication model rather than a mediation model, and the role of opposition in framing alternatives. Methodological questions raised center on the role of the experimental instruction, manipulation of the independent variable, and the reliance on randomness or error variance in the validation of free will conceptions. It is concluded that Harcum's findings are consistent with human agency, but that his theoretical account requires some rethinking.

I am pleased to have the opportunity to comment on Harcum's paper (1991, this issue), because it reflects both the theoretical and methodological problems faced by the psychologist interested in teleology. Harcum is doing much to clarify and advance the field of teleological behavioral study. I am especially gratified that he is prepared to extend his theoretical formulations to empirical test. We have not had too many such efforts in psychology, a fact that I have witnessed first hand as an Associate Editor of *The Journal of Mind and Behavior*, where we specifically invite colleagues to do empirical research on teleological topics (see inside back cover of this issue). Such submissions have been remarkably low, suggesting that either empiricists have no interest in this topic or they are at a loss as how to proceed.

The latter attitude is probably the major stumbling block, since there is no real consensus as to just what human volition, agency, or "free will" involves, which in turn adversely affects its empirical standing. Indeed, Har-

Requests for reprints should be sent to Joseph F. Rychlak, Ph.D., Department of Psychology, Loyola University of Chicago, 6525 N. Sheridan Road, Chicago, Illinois 60626.

cum's paper was not accepted for our routine editorial assessment. Despite this fact, the Editor felt that there was an opportunity here for some of the problems in conceptualization and empirical validation to be aired. I concur wholeheartedly, and seek now to raise questions and criticisms with the Harcum contribution. Even as I do so, it should be understood that Harcum and I are both researchers in human teleology. I am not out to undermine this area of study, but to strengthen and advance it. My comments will be divided into two sections: theoretical and methodological observations.

Theoretical Observations

Probably my greatest dissatisfaction with Harcum's approach to the study of "free will" is that he accepts an interpretation of this agential capacity that is used by mechanistic critics. He tells us in many different ways that a truly free will would be capricious, random, independent of the environment, and hence so unpredictable that a science of human behavior would be impossible to develop. His emphasis here is entirely on the "free" and disregards the meaning of "will" in the free will concept. To my way of thinking, willful actions are those which *initiate* an alternative course of behavior vis a vis the environment rather than merely *respond* to the environment unidirectionally. But Harcum has will a response to the environment, contending that "there is a part of a person which has a capacity or will to respond to environmental conditions such that ordinarily it cannot be empirically distinguished as separate and different from environmentally controlled factors in the control of individual behavior" (p. 95).

Note that his stance as a theoretician here is extraspective, or third-person. He is talking, or I think he is talking, about a part of the person as will "over there." The person "over there" makes a number of responses to environmental stimuli (or whatever), and some of these are willful in nature. Unfortunately, we cannot tell these willful responses from all of the other responses that people make. The "basic question" he puts to himself is whether or not these responses are "created or originated by an internal operation, or are they just the manifestation of internalized control by the external environment through reward contingencies and other known mechanisms of learning theory. . . ?" The latter mechanisms involve so-called ancestorial factors, i.e., "internalized results of past experiences" such as supposedly occur in a reinforcement history.

Harcum seems to think that ancestorial factors are always at play, people are always responding today thanks to the shapings of yesterday, but since these determinants are so remote it is proper to think of today's determinants in voluntaristic terms. He postulates a "responsive human will" that can both react appropriately to environmental constraints but can also "originate

stimulus-independent behavior." Precisely how this latter origination occurs is not spelled out for us, but he seeks to demonstrate its occurrence empirically as a substitute for the theoretical deficiency. He designs his experiment on seat selection because he believes that ancestorial factors are not so clearly evident in this activity, and therefore we can expect to see willful response taking place. The upshot is, he is out to prove that "the behavior of the will is not necessarily random with respect to external referents, as would be the case with a free will, but rather that will responds in a lawful manner to environmental conditions" (p. 96). It is in this lawful responsivity of the will that Harcum can find it plausible to conclude: "Telic behaviors are . . . predictable" (p. 96). Harcum believes that the empirical data he presents us with can act as "a paradigm or model, rather than as proof of either determinism or voluntarism" (p. 97).

Here are the theoretical questions I raise:

(1) *Is the meaning of "free" in the free will phrase really that of a capricious disregard of environmental circumstances?*

Harcum accepts a straw-man definition of "truly" free will as somehow unrelated to reality, as oriented to some totally unrealistic inner world that makes no sense, cannot be predicted, indeed, is probably random. But the legal and religious systems of humanity, which are based on the assumption of free will, surely do *not* conceptualize human agency in this manner. The point of free will theorizing for centuries has been that the individual as an agent is responsible for his or her actions – deliberate, planned, intended actions, not random and/or accidental actions. This is what "will" means. Under legal scrutiny, accidental actions are seen to mitigate the crime. Here we have the distinction between murder (intentional) and manslaughter (via carelessness, etc.) in the taking of a life. In the religious sphere, willful "thoughts" sans overt action can be judged sinful, as overt injuries done to others through misguided intentions can be readily forgiven.

Although it is surely within the purview of a free will conception to include the capricious and random behavior, since in the final analysis the decisions of a freely willing organism can be shown to be arbitrary, the fact is that rarely do people express their free will capacities in this manner. The subject in Experiment II who took the experimenter's seat behaved in this fashion. It surprised me that none of the students in this latter experiment had the creativity to sit on the floor. But, were we to talk with this person, analyze the grounds "for the sake of which" he or she sat in the experimenter's seat, we would doubtless find a rationale, a "reason" for doing so. I would suggest that it is in this grounding or predicating rationale that we must look for the origins of free will.

(2) *Is it possible to theorize about free will behaviors in an extraspective fashion?*

Though I have commended Harcum's empiricism, there is a sort of trap

that I think the empiricist falls into that works against a proper grasp of human teleology. I am referring here to the sort of thing that Jones and Nisbett (1971) found taking place between observers of an event and the actors within that event. Actors in a situation believe that it is the circumstances which they understand to be framing the situation that determine their behavior, whereas observers of these same actors are likely to attribute the cause of the latter's behavior to what we have been calling "ancestorial factors" like previously formed habits or personality shapings. This divergence in causal attribution can be reversed through the use of videotapes by having an observer look at things from the viewpoint of the actor, and vice versa.

What we have here is the theoretical clash of an extraspective, third-person theory of an observer with the introspective, first-person theory of an actor. If we think of free will in extraspective terms, about "that" person behaving "over there" we are likely to give special weight to ancestorial factors (past experience, previous shapings) whereas if we look "with" the person, view the world from his or her perspective, we are more likely to see that it is in the framing or predicating of the environmental circumstances that a line of behavior is really being determined.

Probably the most common criticism that a teleogist receives from a mechanist is "You surely can't deny that a person's previous experience influences his or her behavior in the present." What is not made clear in this assertion is that the ancestorial influences being referred to are construed by the questioner in extraspective fashion, as if things always happened "to" the person, whose framing grasp of this experience was irrelevant "early on." But what if people do not "respond" in the unidirectional manner assumed by those who stress ancestorial influences as unidirectional determinants of today's behavior? What if people "take a position on" life, predicate the circumstances they face *from the outset* of life? In other words, what if the ancestorial factors were *also* predicated by the person who brought "experience" into existence through a form of causation that is not appreciated in the mechanistic account but is central to the teleological account. In this case, the ancestorial factors would themselves be both influential today and freely predicated (construed, assumed, etc.) by the person who relies on them today.

(3) *Can we understand free will behavior as an aspect of efficient causation, or do we need to introduce final causation into our account?*

I believe that Harcum dooms his teleological theory from the outset when he accepts the causal terminology of traditional mechanism in speaking of will as a response. The stimulus-response terminology of psychology's history is based on Aristotle's (1952) notion of *efficient* causation (p. 271) or the billiard-ball form of influence in which antecedents cue or impel consequents without intention or choice. The other causes delineated by Aristotle are, of course, the *material, formal,* and *final.* I cannot go into the reasons for the adoption

of efficient causation as "basic" in Newtonian science (see Rychlak, 1988, for a complete analysis), but it is most certainly true that in today's scientific endeavors we no longer consider this form of causation as the primary determinant in nature (see Prigogine and Stengers, 1984; Zukav, 1979). Today, the fundamental cause emerging in all branches of science is the formal cause, interpreted now in the sense of a pattern, an organized context, or an ordered sequence of events that may include contradictory determinants occurring simultaneously.

Another important development is that today's scientist considers himself or herself to be a "participator" in the kind of "facts" that are discovered empirically. Scientists now take an introspective perspective, recognizing that their assumptive frameworks influence not only the kinds of studies that they do, but the very data that issue from such study. This activity can be framed in final-cause terms such as "that [reason, paradigm, assumption, etc.], for the sake of which" (Aristotle, 1952, p. 270) a line of theorizing is carried on, investigated, proven to be "true," and so on. The "that" in the final cause phrasing is always a formal cause. We could not speak of final causaton if there were no formal cause patternings of meaning on the basis of which to initiate a course of behavior. The Wittgensteinian (1968) distinction between "reasons" and "causes" is really a distinction between "formal/final" and "efficient/material" causes respectively. The Aristotelian usage is no longer popular, but the causal meanings survive and I find them more specific and clarifying than the Wittgensteinian distinction.

Now, it seems to me that such modern scientific behavior is not well rendered in efficient-cause terms like stimulus-response or input-output. There is something taking place here, a framing "that" (formal cause pattern as a "reason") for the sake of which the scientist chooses – manifests free will! – to understand his or her ongoing scientific behavior. If we take the introspective perspective, then viewing the person as a predicator rather than a responder begins to make sense. I think that we teleologists can never capture what we are out to study if we accept "behavior" as limited to efficient causation – as solely "responsivity." We need both the introspective perspective and the formal-final cause account to make our case clear. This is why I have felt it necessary to coin a final-cause concept of the *telosponse* to oppose to the concept of *response* (Rychlak, 1988, p. 283). To telospond is to behave "for the sake of" a predication rather than "in response to" an antecedent stimulus. To predicate is to frame experience rather than take-in from experience in a mediational sense.

When Harcum borrows from Pollio (1981) to the effect that "the environment produces a repertoire of possible responses, from which a viable will selects the specific response [to be enacted]" he comes close to my telosponsive usage. But there is also a vital difference. He and Pollio (whom I con-

sider an excellent model for research teleologists to emulate![1] would apparently have the environment frame or create the responses to be made, and the will would then mediate between and select from among these predetermined action potentials. I would suggest that what we call "will" is dependent upon a predicational process, one that not only selects from among pre-framed possibilities, but actually frames these possibilities in the first place.

(4) *Is the willful aspect of behavior situated within a mediational or a predicational process?*

Harcum tells us that the "ancestorial factors are more remote" than behavioral manifestations taking place in the present. His principle of "remote antecedents," based on an efficient-cause line of influence, suggests that the more remote in time a causal influence is from the present circumstance, and the greater the dissimilarity between earlier and later situations, the less likely we are to be able to explain what is presently taking place in non-voluntaristic terms. Thus, if we pass a person's hand over a flame, the painful cry resulting is determined and has nothing to do with free will. But if this person names a vocational choice, the antecedent influences here are so remote from today's behavior that it is "counterproductive" to look for (efficient-) causal determinants "beyond the telic free will of the person."

It strikes me that this is not a convincing explanation of free will, because Harcum is suggesting that *in principle* there could well be remote causal influences unidirectionally determining the person's behavioral course from out of the past. We simply cannot identify the precise efficient causes of yesterday which impel the responses of today. So, in a circumstance like this, let's just call the unknown a "responsive will" and then we have accounted for free will.

Harcum's theoretical problem, as I see it, is that he is relying on a mediational model of behavior – which happens to be the model of choice for an extraspective theorist as Jones and Nisbett (1971) demonstrated. In a *mediation model, something that is taken in or input comes indirectly to play a role in a process that was not initially a part of this process.* The process under description in mediation is not conceived as the immediate creator of what is to be active within it (e.g., "information"), but rather as the conveyor of that which it takes in "as given" by the environment and proceeds thereafter on the basis of. If the teleologist grants that the environment provides a repertoire of responses, and the will selects from among these possibilities, then

[1]It should be noted that I wrote this statement before we knew that Dr. Pollio would be the other commentator on Dr. Harcum's paper. Initially, Dr. William Stephenson had been recommended by Dr. Harcum to comment on his work. Tragically, Dr. Stephenson was taken ill and subsequently passed away before completing the project. Dr. Harcum recommended Dr. Pollio, who kindly agreed to contribute what has turned out to be a most lively and interesting commentary.

it is but a mere step to suggest that the will's options are themselves based upon "past inputs" from the environment, shaping the will's selection today. Harcum reflects his acceptance of mediational explanation in his discussion of the research by Howard and Conway (1986) and Slife (1987), both of whom he felt were subject to the criticism that ancestorial factors were at play as mediators to determine the outcome of their studies. I will return to this issue when I discuss methodological issues, below.

Predication involves the cognitive act of affirming, denying, or qualifying broader patterns of meaning in relation to narrower or targeted patterns of meaning. A predicational process is not simply mechanically conveying signs, from input to output, but involves actually creating symbolical expressions by using a known meaning as the predicating context within which to situate another meaning, qualify its participation in the context, or negate its relevance for the context entirely. Once a broader meaning engulfs a narrower meaning, the extension from the precedent (wider) meaning to the targeted (narrower) range of meaning is immediate! Once we align "George is reliable," engulfing the former (target meaning) by the latter (precedent meaning), the meaning-creation is immediate. The same would hold in the negation "George is not reliable" (unreliable). Such immediacy is best referred to as a *sequacious* (logically necessary) meaning extension. Precedent-sequacious meaning extensions are therefore outside of time, relying on the logical patterning of meanings in predication. Premises encompass predications, and telosponsivity is such a premising, predicational process.

When we view the person introspectively, as a telosponding organism, we can frame Harcum's Executive as more than simply a "processor of input." Such mediational theorizing can be supplanted by the Executive as the framer or "meaning endower" of such "input." If predication lends meaning to what is processed cognitively, then input is never input without such a logical affirmation taking place. This framing is the willful act. Willful action cannot be limited to choices among alternatives already shaped and given "whole hog" to the person. Willful action must be seen to generate such alternatives as an intrinisic aspect of the cognitive process.

(5) *Can we describe a free will process as exclusively unidirectional or non-oppositional?*

Harcum's acceptance of a "truly" or "completely" free will as capricious, unpredictable action has an element of truth to it, but his theory does not enable him to understand how this open-endedness in human cognition demands in turn that a fixed presumption, a "set" predication be framed by the reasoning human being. That is, if human beings had a predicational process in which "anything goes" as a grounding assumption, and were forced to make a decision for each and every one of their actions, the psychological responsibility would be unbearable. What to do today, what to eat, where to walk, what

to wear, who to talk with – and never a repetition of what has been decided on these questions previously! Who could put up with this? It is not easy being saddled with a free will. The child looks to the parent not because the child is a machine needing shaping. The child models the behaviors of parents and siblings because, after all, there has to be "some way" in which to behave, to lend order to an existence that is open to multiple alternatives. So why not look to others to find grounds "for the sake of which" we might, should, ought to behave? As we mature, we therefore take on the assumptions and values of others because we must lend rational order to a course of life that is threatening or dissatisfying because of the alternatives open to us as we confront it daily. And many of life's alternatives are nestled within oppositionality.

It was George Kelly (1955) more than any other theorist who understood the fundamentally oppositional nature of human cognition. According to the Kellyian insight, the repertoire of behaviors that Harcum and Pollio postulate would not be a collection of singularities. For every behavior "X" there would be implied some "non-X" which would represent a contrary condition, a contradiction, contrast, or negation of the initial action. Selections would not only be made between behaviors "X, Y, or Z," but even more crucially, "within" the behavioral possibility of doing the opposite of what is "input" from the environment as "non-X, non-Y, and non-Z," or some theme-and-variation of the latter which is not even included in the initial possibilities provided by the environment! Unlike machines, human beings can learn, recall, and reason oppositionally (for the first "learning curve" reported in the psychological literature on oppositionality in human memory, see Rychlak, Barnard, Williams, and Wolman, 1989).

Here is where I would look for human free will, as framing alternatives within a sea of opposite possibilities, rather than in the remote antecedents of a unidirectional line of influence that supposedly determines things without the intrinsic possibility of contrariety, contradiction, contrast, or negation taking place at *any point along the way*. If we look at the person introspectively, realize that he or she behaves "for the sake of" predicated meanings (formal and final causation), and that it is always possible to see the contrary, contradiction, contrast, or negation of the meanings under predication, then we have both elucidated what it means to telospond and provided a rationale for free will. Free will is the popular way of referring to the fact that predicating organisms can transcend the biological and/or social promptings of experience and behave "in the present" according to alternatives suggested by various combinations of opposite possibilities. Usually, of course, people behave habitually since it removes the demand of having to make decisions at every turn. Just because people fall into routine habits does not contradict the fact that they are capable of generating freely willed alternatives. Indeed, as I noted

above, the very openness of experience forces the person to seek a rational pattern of behavior lest he or she suffer from the continuing effort of having to decide repeatedly on every minor event in life. This takes us to Harcum's experiments and a consideration of certain methodological problems facing the teleologist.

Methodological Observations

When we come to the methodological side of the ledger, in which we are called upon to provide empirical evidence for our theoretical beliefs, the teleologist faces a number of problems. Before turning to the three questions I find gnawing at telic researchers, again and again, I would like to comment briefly on Harcum's interpretation of complementarity. It was Bohr (1934) who first suggested a principle of complementarity to – if not resolve – then at least to set aside the puzzling question of the ultimate nature of light: whether wave-like or particle-like. The essential point of complementarity is that these two predicating accounts of light are *mutually exclusive.* When one experimental strategy is used to measure light the other is not. It strikes me that Harcum's principle of complementarity is not of this stripe. He seems to be trying to show that "free choice" and "mechanical habit" are interactive factors going on in the experiment *at the same time.* To my knowledge, there is no comparable "wave" and "particle" interaction in the experiments of physics. Perhaps I missed something. I now turn to my questions of a methodological nature.

(1) *What is the role of the experimental instruction, and must it foreclose on a telic formulation of the observed findings?*

The logic of experimentation dictates that a willing subject must be instructed in what to do. In having to give experimental instructions, the experimenter who professes to be testing a teleological conception like free will is frequently placed in the untenable position of having to meet the following objection from a critic: "You cannot be testing free will because your very experimental instruction 'determines' the course of behavior you are studying." I was sorry to see that Harcum so readily accepted this line of criticism, prompted no doubt by his belief that efficient-causal influences from "long ago" are always "here" to influence the present situation unidirectionally.

Thus, in discussing Howard and Conway (1986), Harcum (1991) summarizes their findings as follows: "Because the subjects tended to comply with the instructions [to eat peanuts], showing greater frequency of instructed behaviors on instructed days, the researchers concluded that a case had been made for volitional research" (p. 96). He then throws cold water on this conclusion by referring to ancestorial influences. Actually, in some of their experimental conditions, Howard and Conway turned the decision over to the subject

as to whether to eat or not eat peanuts on any particular day, allowing them to rearrange the expected experimental program at will or whim. The only way to find out what took place in the peanut-eating schedule was to ask the subject afterwards concerning his or her intentions. But even such freedom to select an alternative can be laid at the foot of the "experimental manipulation" presented by the experimental instructions.

Since all experiments on human beings require that we instruct them in some way, and since an experimental manipulation is presumed to be efficiently causal in nature, it would seem to be impossible to test a telic theory. Harcum's hope in selecting the seat-choosing task was that it could not be said to be tightly tied to ancestorial factors. But I am afraid that our mechanistically-oriented colleagues would not find his argument convincing. He did, after all, ask one group of subjects to take a seat (Experiment I) and then told a second group that he was interested in what seat they would be taking (Experiment II). There were significant differences in what took place across these two data collections, hence according to the canons of validation he has proven that his experimental instructions had a "manipulative effect" on the subjects' behavior. Those are the observed facts. His telic theoretical rendering of these facts would doubtless be seen by the mechanists as less parsimonious than their own preferred account of response generalization, discriminative stimuli, and the like.

I think that we teleologists need to point out that our mechanistic colleagues have turned the scientific method into a biased, non-objective procedure in which only one form of theorizing can be validated. I have argued for some time now that mechanistic psychologists confound their efficient cause theory (stimulus-response, input-output, etc.) with the evidential requirements of scientific methodology to efficiently cause certain prearranged conditions to be related to predicted outcomes. This latter independent-dependent variable tandem, which can readily be seen as fundamentally a logical, formal-cause sequence, has been reified into the tracing of "lawful" behaviors, supposedly moving across the passage of time in *solely* an efficient-cause manner (Rychlak, 1981, pp. 57–60; pp. 172–174). It is from this tradition of a confounded mechanistic theory with the need to manipulate variables mechanically in validating theory that we derive our present predicament. What is the teleologist to do?

I think our best answer is to rely upon the principle of falsification (Popper, 1959). This principle is not perfect. As Bohr's form of complementarity suggests, it is always possible *in principle* to design and prove the truth value of a concept from more than one predicating theoretical assumption. Falsification cannot tell us which theory accounting for a phenomenon is "the" correct theory for all time, or even give us the choice between competing theories in the present when they both accrue validating evidence – as in the experiments on light (Rychlak, 1980). The technical reason for this state of af-

fairs is that in science we must of necessity commit the logical fallacy of "affirming the consequent" in our "if-then" line of reasoning from theory "to" experimental test (ibid.). But this open-endedness in evidential support of a theory is what makes research in science so great. We can question anything, even the most hallowed beliefs of the scientific community, so long as we follow it up with empirical tests of our claims. But now, in framing an experiment, according to the principle of falsification it must be possible to *refute* the theoretical statement of hypothesis put forward (Popper, 1959, p. 41). In other words, if we say "All experimental instructions are ipso facto efficient-cause manipulations of the experimental subjects' behavior," it must be possible to show how we can *refute* this statement. If this statement cannot be refuted, then it lacks scientific status.

By taking this approach, we teleologists can stop the mechanistic criticism based on experimental instruction in its tracks. Mechanists cannot falsify their assertion that the experimental instructions are the sole determinant of a subject's behavior. This assertion involves the projection of a theoretical claim into a methodological process. And the theory under projection here is itself unfalsifiable. As a result, this "instructions über alles" argument is a non-scientific assertion. We experimental teleologists have as much right to the exercise of empirical tests as mechanists do. Here is where Harcum is doing ground-breaking work, putting his thinking forward in the arena of empirical test. I am finding fault with him in my commentary, but this does not mean I am unappreciative of his scientifically responsible efforts. We teleologists will learn what to do together, but only if more of us take this additional and much needed step of moving from simply philosophically criticizing the mechanistic theories of our day to actually finding empirical evidence in support of a telic presumption in human behavior.

I am personally bored with the teleologically oriented colleague who constantly harrangues the mechanists for their shortcomings but never rolls up his or her sleeves to conduct empirical research. Instead of adapting current scientific precepts to the problems of human agency, such a colleague wants to undermine the scientific grounds employed in psychology – hoping, I guess, that some alternative method of proof will emerge more friendly to telic description. But scientific method per se can never dictate theoretical usage. To do so would invalidate its objective status. There is ample room in the "control and prediction" strategies of traditional scientific method to accommodate human agency. We who hold to telic theories of behavior must simply get our positions stated more clearly, and show our critics precisely why our formulations are being supported in data that we gather in ways that are rigorous and – at least sometimes – reproducible.

(2) *Is the independent variable solely under the influence of the experimenter, or does the subject play a role in this manipulation?*

This question not only relates to question (1), but in many ways serves as

a background consideration to the entire issue of experimental manipulations. The experimenter's empirical stance is necessarily extraspective, since the target of an experimental design is to be observed taking place "over there." The theory being put to test need not be framed extraspectively, of course. Even so, the *theory of knowledge* on which scientific validation rests is drawn from physical science, in which there was no need for an introspective theoretical slant to be taken concerning the targeted items under investigation. We do not ask the point of view or intention of a planet as it winds its way around the sun each day. Both the theories and the method of physical science could therefore profit from an extraspective slant on the targeted data. But, as I noted above, in the modern age a role for introspective theoretical formulations has emerged in the fact that the scientist now appreciates his or her role as "participator" in the ongoing process of scientific investigation. This refers not only to the theory under promulgation, but also to the sorts of empirical strategies that are designed by the scientist to test his or her theory of the physical universe.

Well, in like fashion, we psychologists have been finding a participative role for the subject of our experiments. The paucity of understanding achieved by psychology in systematically ignoring what a subject presumes (premises, predicates, etc.) regarding the experimental task is legion. My favorite example here is the startling effects of a subjects' task predication in either a classical or an operant conditioning format (Brewer, 1974). Whatever conditioning "is," it isn't what we initially thought it was, for the subject's awareness of what might be going on, and willingness to go along with what might be going on, is crucial to the attainment of the "conditioning effect." I think the evidence is clearly on the side of a telic formulation here, for if the subject is not cognizant of what the experimenter is driving at in the experimental procedure, and/or if he/she is not willing to cooperate with what is going on, we do *not* find "conditioning" taking place. It continues to amaze me how readily the mechanistic colleague dismisses this glaring finding.

I think we are learning through such results that our subjects have always been a contributing factor in the so-called manipulation of the independent variable. They do not always know the reason why we are doing a study. Harcum found a remarkably low level of subject awareness in his studies. Of course, we do not know if he conducted a sophisticated post-experimental interview concerning subject awareness. I suspect that there were all sorts of hunches that his subjects had, and these predicating meanings "determined" what took place through their effects as an independent variable *not* as a dependent variable. In fact, I think that some of the richest potential he had for study occurred in what he referred to as "incidental observations." Here subjects gave the grounding reasons "for the sake of which" they behaved. These reasons are potent "independent variables" in their own right. If there

were some way to conduct this sort of research, in which subjects could be studied framing their predicate assumptions from the outset, I think we might learn a lot more about human teleology.

I would suggest to Harcum that if he wants to think "interactively" in data gathering he should think about the interaction between the experimenter's construal of the independent variable and the subject's construal of the independent variable. I think there is a lot to learn here, even by re-doing classical research designs with this contrasting influence in mind. And, best of all, such an influence is readily conceptualized in agential terms. Doubtless the greatest "prediction" is achieved when the subject's formulation of the independent variable matches the experimenter's, combined with a willingness on the subject's part to comply with what is being indicated. This study of two independent "participators" would be welcomed in *The Journal of Mind and Behavior.*

(3) *Can a free will concept be successfully validated by an appeal to the error variance of a sample distribution?*

The kind of free will theory I have been pressing for in question (2) is diametrically opposed to the interpretation of free will made by Harcum in his experiment. I am suggesting that a freely willing person is continually looking for grounds "for the sake of which" to behave. If the person can indeed affirm the grounds (form the predication) "for the sake of which" he or she will behave, particularly in contradiction to biological or environmental determinants, then I take this as evidence for free will or human agency. Now, in a formulation like this the person cannot be said to be behaving according to "chance." As I noted above, Harcum equates "free" with "random," and therefore when he designs his experiment the evidence for freedom is to be found in the error variance, in the chance distribution of events that lack a framing, structuring predication. How can we teleologists ever hope to make an impact on our colleagues if we claim support for our theory based upon a random finding?

Should we now claim that all those experiments on human beings which have been done to this point in time, but which did *not* reflect significant differences across experimental conditions, were actually providing supportive evidence for free will in the experimental subjects involved? Are we to base our claims on such negative findings? Mechanists are only too willing to place the teleologist in this untenable position. Some years ago, Truax (1966) tried to "test" the agential theory of Carl Rogers in the same vein. Truax devised a scoring system for the Rogerian interviewing procedure, to see if the therapist was actually "differentially reinforcing" the client in contradiction to Roger's non-directive claims. It never occurred to Truax to see if clients were also "differentially reinforcing" therapists to an equal or greater extent in the non-directive verbal interaction. In framing his test of Rogerian theory, Truax

arrayed 27 different correlations between "blind" ratings of therapist and client statements.

Truax never predicted which of the 27 correlations had to reach significance and which did not, nor did he look at the specific meaning of the 11 correlations that did reach the .05 level or better in terms of Rogerian theoretical claims. He just presumed that if "any" correlation were found this meant that the Rogerian therapist was exerting some kind of influence, and hence could not be said to permit clients free agency in their therapeutic interactions. Once again, agency was put on the side of unpredictability or negative findings (randomness, chance, etc.). The possibility that agency might be reflected in a *certain pattern* of significant findings is not even considered here. I have gone into detail on these correlational findings in another context (see Rychlak, 1981, pp. 434–436). Suffice to say that none of them reflected serious detractions from a Rogerian theory of human agency. The point I am making here is that, by confounding the IV-DV experimental sequence with S-R theory, Truax could confidently suggest that if a significant relationship were noted in the former, this meant that there was evidence for the (mechanistic) functioning of the latter. Such reasoning has the teleologist hoping for negative findings, relying upon the error variance for empirical support. Mechanism takes the positive findings and teleology the negative.

Obviously, this kind of "testing" of the telic position cannot be taken seriously. Agency or free will does *not* mean randomness. The legal and religious system of humanity would not employ a free-will conception of human behavior if this meant that when free will is exercised, a random outcome is the result. Free will is important to religion and the law because it holds that human beings can be expected to behave in a self-determined, responsible, controlled fashion – *not* in an uncontrolled, irresponsible, random fashion. It seems to me that what Harcum has proven empirically is that people do not behave randomly, even in something so routine as choosing a seat in an auditorium. There are common preferences as well, such as sitting in the center of the auditorium or near the aisle. People do not ordinarily like to sit in the outlying seats, particularly down front, and so on. Another finding emerging in the comparison of the two experiments is that, when a person is told that the experimenter is interested in his or her choice of seat, the person is more likely to take a habitual seat than when no such experimenter interest is indicated.

I suspect that if we were to have interviewed each subject in detail we could have found that the experimental instructions across the two data collections altered the meaning of the experiment for the subjects involved. I suspect that the 27 subjects in Experiment II who went to their habitual seat would have given us some grounds (i.e., a plausible "reason") for their selection in light of this experimental instruction. If I were a subject, and someone was

interested in my seat selection, I would most probably take the "usual" one and be prepared to say "why," to give my predicating grounds for the sake of which I typically behave. On this score, in a movie theater I like a seat down front because there are fewer people around me to act as distractions (talking, munching popcorn, going to the lavatory, etc.). But in a lecture hall, I invariably select a seat near the back because I want to "beat the crowd" out of the hall at the end of the hour.

One might wonder whether I began initially to have reasons for where I wanted to sit in various situations, or whether initially I had the mind set to take literally "any seat," in random fashion, only to be shaped into performing my habitual pattern by the knocks of hard experience sans choice and intention. The position taken here depends upon one's theory of learning. I do not see how it is possible to account for human agency based on a learning theory that is not itself intrinsically agential in conception. Thus, in my case, if the reasons I now give were learned over time through a series of experiences in theaters and lecture halls, the case for teleology is lost entirely when we subsume a telic line of behavior by a mechanistic learning theory. Almost all theories of learning are intrinsically mechanistic. What we need is a learning theory that is intrinsically teleological – which is why, of course, I have been propounding my logical learning theory (1988).

We should also not overlook the fact that telic behavior can enter "after the fact," as when people make up excuses for "why" they acted in a certain way or made a certain decision. Human beings must be understood to predicate both their past and their future experiences. Life predications are not formed just once, directing the future course of behavior, then lost in a puff of smoke. Life is continually under a predicating meaning, and such meanings shift so that our pasts change in significance as we grow older. But, if as I have been contending, a person – before or after the fact – can *oppose* himself/herself to the biological or environmental prompting/circumstance (see point [5] of the theoretical observations, above), and frame a meaningful course of action from that point onward, I would consider this a freely willed course of behavior. So, even if the first time I entered a room and sat down "by whim" in a seat, then found I did not like the angle on the screen or the lectern, and then shifted to another seat, and another, until I had found a general area in which I liked to sit, this would still be in line with teleology so long as the source of this learning is situated in my agency and not in the so-called "external stimuli" directing my behavioral "responses" without intention (i.e., final causation). This latter development would reflect a mediational model and what we need is a predication model of learning (see point [4] above, under theoretical observations).

In other words, a person's "past experience" is not *all* ancestorial efficient-causation! I may have been pushed about by ancestorial influences and whims,

but over time I as a predicating organism framed this influence and my whims to signify certain meanings "for the sake of which" I then behaved. So my intentions today are built upon intentions framed yesterday, both before and after the fact in a succession of my behavioral experiences. There is also no need to claim that all of my intentions have been framed verbally. We human beings can have "right brain," pictorial intentions as well. And, of course, there may also be a realm of unadmitted (unconscious) intentions to consider in human actions. It is in the process of fixing the grounds ("reasons" etc.) for the sake of which we behave that we are to find free will, which ever looks for the recurring patterns and not the randomness of life. Is the person free to fix (alter, modify, change, etc.) the grounds for the sake of which he or she will be determined? That is the key question in the free will controversy. My answer is "yes," and Harcum's data nicely support this theoretical formulation even though his theory seems to me off the mark.

References

Aristotle. (1952). *Physics.* In R.M. Hutchins (Ed.), *Great books of the western world* (pp. 257–355). Chicago: Encyclopedia Britannica.

Bohr, N. (1934). *Atomic theory and the description of nature.* Cambridge: The University Press.

Brewer, W.F. (1974). There is no convincing evidence for operant or classical conditioning in adult humans. In W.B. Weimer and D.W. Palermo (Eds.), *Cognition and the symbolic processes.* Hillsdale, New Jersey: Lawrence Erlbaum Associates.

Harcum, E.R. (1991). Behavioral paradigm for a psychological resolution of the free will issue. *The Journal of Mind and Behavior, 12,* 93–114.

Howard, G.S., and Conway, C.G. (1986). Can there be an empirical science of volitional action? *American Psychologist, 41,* 1241–1251.

Jones, E.E., and Nisbett, R.E. (1971). *The actor and the observer: Divergent perceptions of the causes of behavior.* Morristown, New Jersey: General Learning Press.

Kelly, G.A. (1955). *The psychology of personal constructs* (2 volumes). New York: W.W. Norton.

Pollio, H.R. (1981). *Behavior and existence.* Monterey, California: Brooks/Cole.

Popper, K.R. (1959). *The logic of scientific discovery.* New York: Basic Books.

Prigogine, I., and Stengers, I. (1984). *Order out of chaos: Man's new dialogue with nature.* New York: Bantam Books.

Rychlak, J.F. (1980). The false promise of falsification. *The Journal of Mind and Behavior, 1,* 183–195.

Rychlak, J.F. (1981). *A philosophy of science for personality theory* (2nd edition). Malabar, Florida: Robert E. Krieger Pub. Co.

Rychlak, J.F. (1988). *The psychology of rigorous humanism* (second edition). New York: New York University Press.

Rychlak, J.F., Barnard, S., Williams, R.N., and Wolman, N. (1989). The recognition and cognitive utilization of oppositionality. *Journal of Psycholinguistic Research, 18,* 181–199.

Slife, B.D. (1987). Telic and mechanistic explanations of mind and meaningfulness: An empirical illustration. *Journal of Personality, 55,* 445–466.

Truax, C.B. (1966). Reinforcement and nonreinforcement in Rogerian psychotherapy. *Journal of Abnormal Psychology, 71,* 1–9.

Wittgenstein, L. (1968). *Philosophical investigations* [third edition, G.E.M. Anscomb, Trans.]. New York: Macmillan Book Co.

Zukav, G. (1979). *The dancing wu li masters: An overview of the new physics.* New York: Bantam Books.