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A Research Strategy for Studying Telic Human Behavior

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Numerous writers have recently called for reform in psychological theorizing and research methodology designed to appreciate the teleological, active agent capacities of humans. This paper presents three studies that probe individual's abilities to volitionally control their eating behavior. These investigations suggest one way that researchers might consider the operation of telic powers in human action. Rather than seeing teleological explanations as rivals to the more traditional causal explanations favored in psychological research, this paper elaborates a position that sees human volition as a causal force embedded in (and influenced by) the traditional causal influences studied in psychological research. Finally, the theoretical and methodological refinements suggested here and elsewhere are seen against the backdrop of a philosophy of science that sees change as a more gradual, evolutionary process, rather than the Kuhnian, revolutionary process.

New Ideas in Psychology recently devoted a subsection to Joseph Rychlak's challenge, "Can Psychology be Objective about Free Will?". Several important issues were raised in the ensuing dialogue that should be highlighted. First, Rychlak (1983a) elaborated upon several points he had articulated elsewhere (Rychlak, 1976, 1977, 1981), namely: the theory-method confound (closely related to the philosophical notion of the underdetermination of theory by evidence); the prevalent aversion in psychological research to telic theories as explanation of empirical findings; the preference for demonstrative rather than dialectical views of humans; and consequently, the failure by scientific psychology to be able to shed light on concepts such as free will, volition, consciousness, and so forth.

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Rychlak proposes a science of psychology that takes the research methods traditionally employed in psychology and conducts studies which will *be interpreted* as suggesting the telic capacities of humans (or final cause; or "that for the sake of which" something exists, is happening, etc.). Such a human science will prove more satisfying, he believes, than our current version which views humans as mechanistically-determined via material (a substance conception which captures what something is "made up of") and efficient (an impetus motion explaining how things: came about; are assembled; or now are moved?) causal influences. This belief is based upon Rychlak's view of the telic nature of humans; and he believes this view possesses the potential for developing into a viable scientific account of human action. Obviously, he has issued a huge promissory note—but his account is not without assets, as his research on logical learning theory (Rychlak, 1977) represents an important first step in his agenda for altering the interpretation of the meaning of research findings in psychology.

The present paper has several ambitions. First, we hope to properly appreciate the importance of Rychlak's critiques, and the ways in which his own research reflects alternatives to the standard position in psychological research. Second, we believe psychology's theory-method problem results, in part, from psychologists' misconceptions regarding what types of evidence can represent an appropriate warrant for knowledge of humans. Third, in suggesting that a subject's correct self-prediction and/or self-control of his or her behavior represents an appropriate warrant for understanding telic phenomena, we hope to recommend a method that strongly (although not totally) suggests the telic capacities of humans. Fourth, three studies that demonstrate the effect of human volition (as against material or efficient causal explanations) in a particular behavior will be presented. Fifth, we will discuss how such empirical demonstrations not only support several of Rychlak's claims but also show how current methodology in psychology can be improved to appreciate the telic capacities of humans, and thereby ameliorate some of the debilitating effects of the theory-method problem in psychological research.

Rychlak centers the problem of the theory-method confound in psychology's fixation with material and efficient cause theories. While he would like to radically revise psychological theory, he finds no fault with method as currently practiced: "There is nothing in my arguments that need challenge the traditional research method to which all psychologists conform. I have felt considerable repression in psychology for over a generation now. But my point is: the repression is *not* due to the scientific method" (Rychlak, 1983b, p. 258). While we agree that the current hegemony of material and efficient causal explanations must be broken, we also believe that certain modifications in currently accepted research designs would facilitate transformation toward

a science capable of appreciating the telic nature of human beings[1]. Like Rychlak, we demand that these methodological refinements adhere to the strictest standards of evidential credibility, as we believe a telic science of human action can be as methodologically rigorous as our current empirical efforts. But why should any change in scientific method be made?

Rychlak points out that the findings of many psychological investigations can be interpreted either (for example) in efficient cause or final cause terms. The evidence does not demand either type of explanation, not can either type (usually) be ruled out. How, then, does an experimenter decide which type of explanation to entertain? Cronbach (1982) expands our conception of the "validity" of a study by including consideration of how the scientist chooses to interpret the findings.

> Validity depends not only on the data collection and analysis *but also on the way a conclusion is stated and communicated.* Validity is subjective rather than objective: the plausibility of the conclusion is what counts. And plausibility, to twist a cliché, lies in the ear of the beholder. (Cronbach, 1982, p. 108) [italics added]

While telic accounts of experimental results resonate to Rychlak's ears, most psychological researchers find efficient or material accounts more plausible. Undoubtedly, part of this overwhelming preference is due to the force of habit. Analogously, Rychlak points out that the structural similarity—between the methodological practice of having independent variables produce differences in dependent variables, and the theoretical view of efficient causes producing effects—has a seductive influence on researchers. Together these influences render many psychologists ill-prepared to *hear* telic explanations as plausible accounts of evidence. And if research findings are indeed interpretable by both efficient and final cause accounts, why should researchers entertain explanations of those data that sound less plausible to them? We have here all the elements of a standoff. Can science be of any help in arbitrating such disagreement?

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[1]At various points in his writings Rychlak deals with a broad array of concepts under his notion of method, from some overarching issues of scientific rationality (e.g., method represents the means for enlisting empirical support for theoretical propositions) to rather circumscribed issues of research design and technique. We are in complete agreement with Rychlak's views on method as they relate to the logic of scientific rationality. The contribution to be offered herein concerns itself with issues of method on a molecular level, that is, in the formation of human action. But scientific studies do not "test" mechanistic or telic theories *per se*. That is, the underdetermination of theory by evidence thesis precludes any empirical demonstration either "proving or disproving" agenic or mechanistic theories. However, this fact does not ameliorate the position that certain empirical demonstrations suggest mechanistic influences on human action rather clearly. The reason for suggesting molecular-level changes in method is offered in the hope of soon achieving empirical demonstrations that highlight and appreciate the telic nature of human action.

Numerous writers (e.g., Gergen, 1982; Howard, 1984a; Toulmin, 1981; Tyler, 1983) have asserted that the science of psychology has been heavily influenced by the methodologies that proved successful for the physical sciences. But since the physical sciences desired to understand behavior that was seemingly non-telic in nature (e.g., the movement of planets; chemical reactions, etc), scientific explanations became identified with material, efficient, and formal cause explanations. To anthropomorphize one's subject matter was viewed with distain by scientists. Psychology accepted this antipathy for final cause explanations, and modeled its research strategies after those of the physical sciences. Specifically, like other sciences, psychology viewed its goal as gaining an understanding of its subject matter (namely, human beings). Further, psychologists agreed that the proper warrant for their belief that they were achieving successively better explanations rested primarily in achieving greater experimental prediction and/or control (although these two desiderata are not the only criteria, as internal coherence, simplicity, external consistency, fertility, unifying power, and so forth are also valued [Howard, 1985; Kuhn, 1977; McMullin, 1983]).

The type of prediction and/or control found in the physical sciences was prediction and/or control *by the scientist*—not the subject matter. Chemicals, planets, and falling objects do not predict or control their behavior better; it is the scientist who becomes better able to accomplish these important tasks. As mentioned earlier, volitional actions appear not to be in the nature of inanimate objects, and hence scientists should find volitional interpretations of that behavior unsatisfactory. However, it might be argued that volitional action is quite possible with human beings, but that such capacities cannot be properly appreciated empirically solely with the traditional criteria for scientific knowledge—prediction and/or control by the scientist.

In an earlier paper (Howard, 1984b) it was argued that self-prediction and/or self-control might be entertained as appropriate warrants for a scientific understanding of telic capacities. The studies presented below demonstrate how self-control may be employed as a criterion that suggests volitional human behavior in a particular domain. In the first study, evidence for subjects' ability to control their eating behavior will be interpreted as suggesting telic capacity for volitional control. However, an efficient cause explanation of the results is still possible (because, methodologically speaking, instructions are confounded with volitional choice). In all other respects, the study represents an example of the best of experimental rigor in psychology. In order to render the efficient cause end-run (claiming the experimental instruction efficiently caused subjects' eating) more implausible, the second study self-consciously seeks to disentangle telic self-control from the experimental instructions. In so doing, the two studies might represent evidence for telic capacities of humans for which all material and efficient cause explanations are implausible.

Is it necessary to employ self-control in order to show evidence of telic action? While Rychlak's work demonstrates that it is not *necessary* to do so, we believe that such a move will prove very helpful in achieving a satisfactory research-based understanding of telic human action. Perhaps an analogy might be instructive. One might be able to build perfectly adequate houses (a theoretical account of human action via material and efficient causes only) by employing materials such as lumber, bricks and mortar (using traditional research designs). However, if one wished to build a modern skyscraper (a theoretical account including final causality along with material and efficient causes), they would be ill-advised to use lumber, motar and bricks solely. By incorporating other building materials such as glass and steel (research designs specifically developed to appreciate telic capacities) the architectural achievements in modern buildings can be achieved. Note that while it is possible to construct multi-story buildings with lumber, bricks and mortar, one might be ill-advised to use them to the exclusion of other helpful building materials. But it should be noted that the findings of this series of studies are completely consistent with Rychlak's findings, in spite of the fact that the similar conclusion is reached via a different methodological route. This convergence of findings represents an important form of confirmation of Rychlak's program of research.

The Genesis of Scientific Change

The reader will sense that we have now embarked upon an analysis of how change occurs in scientific disciplines. As a philosophy of science, logical positivism proved inadequate to illuminate how change occurred in scientific communities. Thomas Kuhn (1962, 1977) showed how paradigm shifts could be employed as a conceptual device to understand how profound shifts in the thinking of scientific communities might occur. But recent thought (Laudan, 1984) suggests that Kuhn's picture might depict scientific change as being overly abrupt and discontinuous in nature. Certainly, Kuhn's use of concepts such as "incommensurability between paradigms," "scientific revolution," "conversion experience," "irreversible Gestalt-shift," and the like, leads to the view that paradigm shifts are abrupt, and influence every level of scientific rationality. As Kuhn himself says in *The Structure of Scientific Revolutions*, "In learning a paradigm the scientist acquires theory, methods, and standards together, usually in an inextricable mix" (Kuhn, 1962, p. 108). Laudan notes that "Paradigm change, on this account, clearly represents a break of great magnitude. To trade in one paradigm for another is to involve oneself in changes at *each* of the three levels—we give up one ontology for another, one methodology for another, and one set of cognitive goals for another. Moreover, according to Kuhn, this change is *simultaneous* rather than *sequential*" (Laudan, 1984, p. 101).

Rychlak's (1983a) and Howard's (1984b) recommendations for change in psychological research are perplexing, given the Kuhnian view of simultaneous shifts at all levels of scientific rationality, when paradigm shifts occur. Rychlak, for example, calls for a change in the types of theoretical explanations psychologists entertain while keeping the aims and goals of psychological research and its methodologies intact. On Kuhn's view, would not such a piecemeal approach result in paradigmatic confusion? Laudan furnishes a key to this puzzle by suggesting that paradigm shifts take place over long periods of time, rather than abruptly.

In his reticulational model of scientific change, Laudan (1984) proposes that changes in any of three levels of scientific activity (the axiological [or the aims and goals of science]; the methodological; or the factual [which includes both theories and evidence]) are possible at any point in time. Such changes are usually not accompanied by suggested modifications at other levels, as Kuhn's analysis would suggest. Rather, Rychlak (1977, 1983a) merely recommends changes in the type of theory considered, while Howard (1984b) only suggests a methodological alteration. On Laudan's view, such changes, if they prove successful, might produce changes at other levels also, which could in time lead to the massive conceptual changes associated with paradigm shifts. However, we hasten to add that while many such changes are suggested, few are successful, and fewer still lead to changes at other levels. But it is precisely this trial and error, piecemeal approach that constitutes the fuel for scientific change and the backbone of scientific rationality. Toulmin (1972) sees the rationality of science as being embedded in the manner in which sciences change and evolve, rather than remaining stagnant: "a man demonstrates his rationality, not by a commitment to fixed ideas, stereotyped procedures, or immutable concepts, but by the manner in which, and the occasions on which he changes those ideas, procedures, and concepts" (p. x). Finally, the damage done by not considering suggestions for scientific change is highlighted by Albert Einstein who claimed

> Concepts which have proved useful for ordering things easily assume so great an authority over us that we easily forget their terrestrial origin and accept them as unalterable facts. They then become labeled as "conceptual necessities," "a priori situations," etc. The road of scientific progress is frequently blocked for long periods by such errors. It is therefore not just an idle game to exercise our ability to analyze familiar concepts, and to demonstrate the conditions under which their justification and usefulness depend. (cited by Holton, 1973, p. 5)

Three Studies of Volition

The following studies assess subjects' telic ability to exert volitional control over their peanut eating behavior. The studies deal with the relative contribution of volition (the decision to eat peanuts on some days and not on

others), and one external, efficient cause factor (whether or not subjects receive a reminder), on the amount of peanuts they consume. Peanut eating was chosen because it provides a noncontroversial dependent measure, and because eating peanuts is an activity subjects tend to enjoy, but which they should be able to control. This empirical demonstration is meant to suggest a model for using self-control as a warrant for inferring volitional control. By providing actual examples of how volitional effects might be ascertained, the discussion of *whether* science can probe volitional action might be shifted to a consideration of *how* empirical studies of this sort can contribute to our understanding of volitional action.

Study 1

Method

Subjects. Thirty-nine volunteer undergraduate students served as subjects in the study. Subjects received experimental credit for participating in the study.

Materials and procedures. Each subject was furnished daily with a filled 16 ounce jar of peanuts and given one of two sets of instructions: eat as many peanuts as you wish; or try not to eat any peanuts at all (this represents the manipulation [in a methodological sense] of volition). Every morning of the study the amount of peanuts left in the jar from the day before was weighed with a Hanson Dietetic scale, and the jar was refilled and returned to the subject along with that day's instruction. On half of the days of the study, a written reminder of whether this was an "eat" or a "not eat" day was left in students' rooms in the late afternoon (this represents the manipulation of reminder). The study ran for twenty consecutive school days (weekends were excluded). The order of presentation of the four conditions (eat/reminder; eat/no reminder; don't eat/reminder; don't eat/no reminder) was counterbalanced both within and across subjects. There was a minimal incidence (1%) of contaminated data (e.g., friends inadvertently eating some peanuts). In such cases, the same condition was rerun the next day. Consequently, complete data were obtained on all subjects.

Results

The design was a 2 × 2 factorial wherein both factors (volition; reminder) were within-subject factors. Mean weight (in grams) of peanuts eaten in each of the four conditions was: 98.20 g for try to eat/reminder; 87.30 g for try to eat/no reminder; 1.88 g for try not to eat/reminder; and 4.47 g for try not to eat/no reminder. A two-way repeated measures ANOVA revealed a signifi-

cant main effect for volition (F [1,38] = 42.67; $p < .0001$). There was neither a significant main effect for reminder (F [1,38] = 1.02; $p > .31$) nor for the volition by reminder interaction (F [1,38] = 2.13; $p > .15$). Strength of relationship measures (Partial Eta Squared [Maxwell, Camp, and Arvey, 1981]) were computed for each effect. The effect size for volition was .53; the corresponding figure for the reminder was .03; and the effect size for the interaction was .05.

Discussion

Great care should be taken in interpreting these results. We believe that this study represents a rigorous empirical demonstration of how one might consider the relative infuence of a final cause factor (volition) and an efficient cause factor (reminder) on a person's eating behavior. Viewed from this perspective, the results strongly suggest a huge volitional component in this domain. However, as suggested above, some psychologists might counter that the differences between the "eat" and "not eat" conditions are due to subjects' need to conform to the instructions of the experimenter. This interpretation represents an efficient cause end-run designed to undercut the volitional interpretation, offered herein, and is quite similar to the reaction Rychlak (1977, 1988b) describes to his research on final causal influences.

The purpose of the second study was to untangle (as much as possible) these conflicting interpretations (volitional control versus conformity to the experimenter's commands) of the eat-not eat differences.

Study 2

Method

Subjects. Ten volunteer undergraduate students served as subjects in this study. Subjects received experimental credit for participating in the study.

Materials and procedures. Materials and procedures were identical to the first study with the few exceptions described below. The volition factor (eat-not eat) was crossed with a meta-volitional factor (namely, follow today's instructions versus do the opposite of today's instructions). The meta-volition factor was operationally specified in the following manner. Each morning upon receiving their filled jar of peanuts, each subject decided whether he or she would follow or do the opposite of that day's instructions. The subject recorded this decision, but did not let the experimenter know the results of his or her decision. The experimenter instructed the subject as to whether it was an "eat" or "not eat" day (the order of presentation of these instructions was counterbalanced both within and between subjects). Subjects were instructed

to keep the number of "follow instructions" versus "do the opposite" conditions about equal. Therefore, on a particular day, if a subject decided to "do the opposite" and the experimenter said "not to eat the peanuts," the subject should view it as an "eat" day. There were no reminders given in this study. The study ran for 24 consecutive school days. There was a minimal amount of contaminated data (1.2%) which was discarded. Each subject ended his or her participation in the study when at least four observations in each of the four cells were reached.

Results

The design of the study was a 2 × 2 factorial design wherein both factors (volition; meta-volition) were within-subject factors. Mean weight (in grams) of peanuts eaten in each of the four conditions was: 135.70 g for try to eat/follow instructions; 10.45 for try to eat/do the opposite; 3.40 g for try not to eat/follow instructions; and 120.20 g for try not to eat/do the opposite. A two-way repeated measures ANOVA revealed a significant interaction of volition by meta-volition ($F\ [1,9] = 16.91$; $p < .005$). There were no significant main effects for volition ($F\ [1,9] = .60$; $p = .45$) or for meta-volition ($F\ [1,9] = .08$; $p = .78$). Strength of relationship measures (Partial Eta Squared) were computed for each effect. The effect size for the volition by meta-volition interaction was .65. The effect size for volition was .06 while the effect size for meta-volition was .01.

As expected, these results suggest that subjects could easily choose to disobey the experimental instructions to "eat" or "not eat" when they chose to do so (that is, on days when they decided to do the opposite of what the experimenter instructed). While a die-hard efficient cause proponent could still maintain that in disobeying a particular instruction the subject was really conforming to the meta-instruction of the study, such a move appears, to us, to be grasping at straws. Further, the results of two additional studies (Howard, 1989; Howard and Conway, 1986) test the plausability of the conformity interpretation in still different ways, and find the data suggest that the effect of volition is not due to conformity to the experimental demands. The Howard (1987) study is particularly important in this regard. Here the question of who causes a subject's behavior (the subject him/herself or the experimenter through the experimental instructions) was approached by *collapsing the distinction between the subject and the experimenter.* Thus, the same person was both experimenter and subject in the study. Enormous volitional control of alcohol consumption was evident in the study. But if—as both the experimenter and subject—he/she was merely conforming to the experimental instructions, then he/she was conforming to his/her own commands—but this is precisely the character of volition! The data presented thus far

in this paper seem to be most appropriately understood as suggesting telic, volitional power over peanut eating. What the two demonstrations accomplish is to point out a technique for teasing our influences in human behavior that are most plausibly understood (as Cronbach suggested above) as telic, volitional effects.

Study 3

One of the threads that has run through the humanism-behaviorism debate over the past few decades involves the question of whether human behavior is primarily volitional (or telic) in nature, or whether human behavior is best understood as mechanistically determined via biological influences and/or environmental factors. Radical behaviorists, such as Skinner, have put forward strong theoretical positions (which, in turn, have spawned important research programs) that emphasize environmental causal explanations, which often disparage telic explanations as unscientific. Humanists, on the other hand, have been credited with offering a strong critique of the mechanistic and deterministic excesses of behavioristic psychology. However, humanists can be faulted for failing to provide viable research programs as alternatives to behaviorist programs. Perhaps the "either/or" character of the debate has been partially responsible for our difficulties thus far in integrating telic influences with biological and environmental factors in our empirical efforts. The philosopher, Larry Wright, highlights the either/or character of the problem in the following way.

> Teleological accounts of behavior and causal accounts of behavior are *rivals:* teleological explanations *contrast with* causal ones; one sort will *rule out* the other. Did you jump, or were you pushed? Did you do it, or did it happen to you? It simply could not be both; nothing could be clearer. Furthermore, the argument continues, the forward orientation of teleology—the feature primarily responsible for this contrast—is hostile to the whole causal perspective. Attempting to assimilate cause and teleology must therefore rest on a fundamental misunderstanding. (Wright, 1976, p. 26)

Wright then demonstrates why this either/or view of teleological-nonteleological explanations is misguided, thus leaving the door open for a model of human action that sees biological and environmental influences as conditions that partially direct and mold telic human action.

Personal Agency and Human Action

We will now outline a model of human action that presents a robust, forward-oriented focus on conditions and consequences, as primary characteristics of teleological explanations of human action. The central explanatory mechanism in this model is called personal agency. However, this central

mechanism could just as easily been referred to as ego (Freud, 1923/1969), consciousness (Sperry, 1982), active agency (Harré and Secord, 1972), self-determination (Deci, 1980), personal causation (de Charms, 1968), mind (Rychlak, 1977), or self (Rogers, 1961). Unlike most other "humanistic" models, this approach has broad heuristic value for research efforts. As with all scientific theories, its ultimate success rests with its empirical adequacy, external consistency, internal coherence, fertility, and unifying power (Howard, 1985; Kuhn, 1977; McMullin, 1983).

Building theoretical models is an important part of any science. But commiting oneself to a model is not an unequivocal boon, as Bernstein (1976) notes.

> Intellectual orientations lend weight to a sense of what are the important issues, the fruitful lines of research to pursue, the proper way of putting the issues. The most important and interesting challenges to any dominant orientation are those which force us to question the implicit and explicit emphases, that make us self-conscious not only to what is included in the foreground, *but excluded or relegated to the background as unimportant, illegitimate, or impractical.* (p. 41) [emphasis added]

The issue at stake here involves the relationship between a scientist's model of human beings, and how that scientist will interpret the results of his or her studies. In contradistinction to the belief of most psychologists today, we believe that it is not the past or the present that is primarily responsible for human behavior, but rather it is the future, as imagined by that individual, that is crucial in how we form our actions. Schematically, our model of the wellsprings of human action is depicted in Figure 1.

In Figure 1 (first presented in Howard, 1986) the rectangle represents a particular person's behavior. The circles depict constructs (or entities) thought to exert causal influence upon the person's behavior. In the proposed model, personal agency is located at the center of human behavior. As can be seen from the diagram, biological, social, environmental, and psychodynamic factors can exert their influences independently of personal agency (the shaded areas). But from this perspective, nonvolitional potential influences (such as environment, biology, etc.) of human behavior sometimes (perhaps often!) achieve their effects in the formation of human action through personal agency (the striped areas). This conceptual move stands the conventional wisdom in psychological research on its head. For example, let us assure you that there is a very close relationship between the amount of coffee one of the authors drinks at the office and how much writing he gets done. Any biologically-oriented psychologist would immediately begin to talk about the effects that the stimulant, caffeine, has on the author's activity level, and how this facilitates his writing. That analysis is not totally incorrect. It simply misses the most interesting aspect of the phenomenon. Let us assure you that when

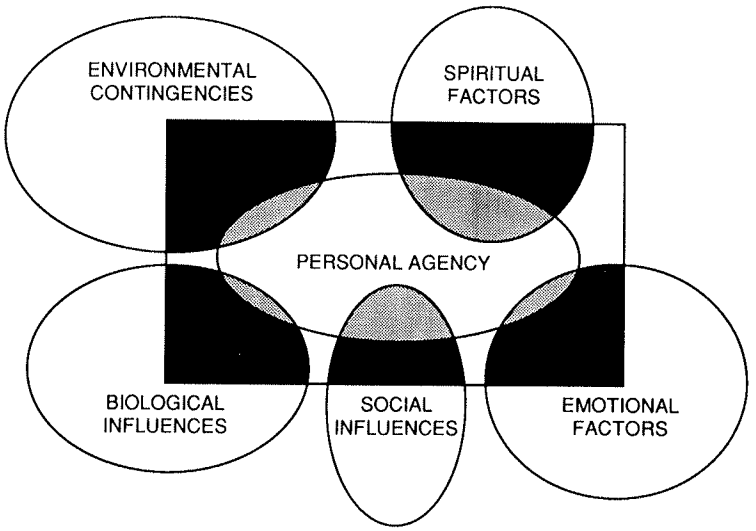

Figure 1. Casual factors in human action.

the author decides to write, he chooses to drink a good bit of coffee in order to facilitate the desired action.

Or consider an example involving environmental contingencies. One can usually get a lot more writing done when he/she leaves the office door closed, than when it is open. Environmentally-oriented psychologists in noting this relationship might maintain that the person is under the control of this environmental condition. But an agenticist instead focuses upon the person choosing to alter the environment (by closing the door, or turning off the stereo, or whatever) in order to meet certain goals.

As one can readily see, this revised model places personal agency at the center of human action as the dominant "cause" in explaining an individual's behavior. Thus the agent's hopes for the future, his or her plans, goals, intentions, dreams, and purposes, assume a prominence in the genesis of human action. Conversely, in this model the causes traditionally investigated in psychological research (e.g., reinforcement contingencies, physiological-biochemical factors, etc.) recede to a position of being viewed as enabling conditions which either enhance or detract from the likelihood that an agent will achieve his or her intentions.

One way in whch this perspective might be operationalized in empirical psychological investigations has been demonstrated in Studies 1 and 2 above. In Studies 1 and 2, subjects were largely able to uncorrelate their behavior from any possible causal influence (save the conformity to the experimenter's

instructions interpretation; which was considered specifically in Study 2). We are inclined to view such demonstrations as evidence for the force of personal agency (volition, will) in the formation of the behavior in question. To the degree that subjects are unable to modify their own behavior, we suspect that volitional control is minimal in those cases. But how will psychology's accumulated research-based knowledge of human behavior "look" from this personal agency perspective?

The final study will attempt to consider the role of two important psychological constructs as moderating influences in subjects' volitional control of their peanut eating. The two constructs considered are feedback (or knowledge of results) and incentive. Rather than viewing feedback and incentive as the causes of subjects' eating behavior, Study 3 will view the two constructs as conditions that might either enhance (or detract from) subjects' ability to finely tune the volitional control of their eating.

Method

Subjects. Forty-seven undergraduates, enrolled in introductory psychology classes, were offered four extra credits for participating in the study.

Procedures. Subjects were randomly assigned to either the feedback or no feedback conditions. Each subject was furnished daily with a 16 ounce jar of peanuts and given one of four sets of instructions: eat no peanuts during the next day; eat 50 g of peanuts during the next day; eat 100 g of peanuts during the next day; or eat 200 g of peanuts during the next day. Subjects were also shown representative samples of the three non-zero amounts, so that they would be better able to visualize the amount they were supposed to eat that day. The order of presentation of the instructions was counterbalanced both within and across subjects.

Every evening during dinner, the amount of peanuts left over in the jar from that day was weighed on an Ohaus Triple Beam Balance scale, and the jar was refilled and returned to the subject, along with the instructions for the next 24 hour period. In addition, subjects in the feedback group were given feedback as to how closely the amount of peanuts they had consumed approximated the amount they had been instructed to eat. For example, suppose a student was in the condition "Eat 50 grams of peanuts," but had eaten 72 g of peanuts that day. The student would be over the specified amount by 22 g, and would be informed of the difference. In addition, the subject would be shown what approximately 22 g of peanuts looked like. Students in the no feedback group were given no such feedback, but merely had their jars of peanuts weighed, refilled, and returned with a new set of instructions. The above procedure continued every day for twelve consecutive school days

(weekends were excluded). These twelve measurements (or three time blocks) were used to establish a baseline for both groups.

During the fourth time block of the study, both the feedback and no feedback groups were randomly divided into halves, and half of each group was offered the added incentive of ten dollars if they stayed especially close (relative to their performance in baseline) to consuming the specified amount of peanuts. At the end of the fourth time block, this incentive group was returned to the normal baseline condition. During the fifth time block, the second half of each group was offered the added incentive of ten dollars for the increased accuracy in regulating their peanut intake during that week.

Subjects were repeatedly admonished not to speak to anyone else involved in the study, to minimize the chances that subjects would prematurely discover that they might receive bonus points later in the study. At the end of the study, 44 out of 47 subjects received the ten dollar bonus since they were able to better control the precise amount of their peanut consumption in the incentive condition than in the baseline phase.

Results

There are two dependent measures in the study. The first one is the number of grams of peanuts actually eaten each day; the second measure is the accuracy score, or the absolute value of the discrepancy between what the subject was told to eat and what he or she actually did eat (accuracy score). During baseline subjects consumed an average of: 10.64 g in the 0 g condition; 71.96 g in the 50 g condition; 101.59 g in the 100 g condition; and 164.32 g in the 200 g condition. A two-way ANOVA (Condition × Feedback) on weight of peanuts consumed during baseline was conducted. There was a significant increase in the amount of peanuts consumed across conditions ($F\,[3,43] = 170.69$; $p < .001$), suggesting that subjects could, at this most global level of analysis, volitionally modulate their peanut consumption. However, the main effect for feedback and the feedback by condition interaction failed to reach significance.

The more important questions in this study involved the effects of feedback and incentive on the change in accuracy scores from baseline to incentive (i.e., from time block three to time block four). A two-way (Feedback × Incentive) ANOVA was conducted over data from the 5 g, 100 g, and 200 g conditions only (since it was impossible to keep feedback of results from the "no feedback" subjects in the 0 g condition). There was a significant improvement in accuracy ($F\,[1,45] = 6.43$; $p < .05$) for subjects who received the incentive in time block four relative to their control group counterparts. However, the main effect of feedback and the feedback by incentive interactions were nonsignificant.

Discussion

Subjects demonstrated some degree of volitional control during the baseline phase of this study, but their control was far from perfect. When offered a monetary incentive to further improve their control, subjects were able to improve their accuracy (by about 23%) relative to subjects who received no such incentive to improve their accuracy at that point in time. Thus, it appears that subjects employed the incentive to achieve a finer degree of volitional control of their eating.

It was a bit surprising that feedback failed to improve subjects' accuracy scores. Since subjects were present and involved in the weighing of the peanuts each day, it is posible that no feedback group subjects might have obtained some indirect or anecdotal knowledge of their results from time to time. To the extent this might have occurred, it would weaken the test of the effects of feedback in the present study.

The three studies collectively present a strong case for the role of agenic self-determination in the genesis of human action. Assignment by the experimenter of large numbers of days to either "try to −" or "try not to−" conditions creates two groups of days that are (in all likelihood) equal to one another in all respects. When we note large differences on the dependent measure between these two groups of days, all possible explanations for mean differences between the two conditions, save two, are rendered implausible. The two possible explanations are: (a) that these mean differences reflect the agent's power of self-determination (or volition) in this particular instance; and (b) that subjects were compelled to obey the experimenter's instructions and could not do otherwise. There is now substantial evidence that suggests the implausibility of this latter explanation. Thus, the present three studies (plus Howard, 1989; Howard and Conway, 1986; Lazarick, Fishbein, Loiello, and Howard, 1988) strongly suggest the importance of agenic self-determination in human action. Agenic self-determination is assumed by action control theorists as diverse as Carver and Scheier (1981), Rychlak (1977), Kuhl (1985), and Bandura (1986). All such models of self-regulatory processes should view these demonstrations of self-determination as strong support for the soundness of the presupposition of human agency implicit in their models.

General Discussion

The philosopher of science, Imre Lakatos (1978), viewed research programs as extended sequences of developing, interrelated bodies of theories that could conceivably last for centuries as viable experimental endeavors. Examples of research programs are: The Ptolemian view of astronomy; the Newtonian approach to mechanics; the Darwinian evolutionary perspective; and the

mechanistic/deterministic approach to research that is currently practiced in many areas of psychology. Lakatos says that important characteristics of any research program are its positive and negative heuristics. The negative heuristic says: hands off—don't meddle here (this represents the hard core of the theory—something assumed to be true, and never doubted). The positive heuristic says: here is a set of problem areas ranked in order of importance—worry only about questions at the top of the list (Lakatos, 1978). One might view several of the recent critiques of research in psychology (e.g., Gergen, 1982, Giorgi, 1970; Harré and Secord, 1972; Rychlak, 1977, 1983a) that pose active agent alternatives to the standard approach to research in psychology as the beginning of a new program.

Conversely, one might choose to defend the more restricted claim, that these efforts seek only to modify the more restrictive aspects of the negative heuristic of the received view. From this perspective, the work on volition reported herein demonstrates the relative impact of final cause influences (the effect of an agent's volitional control) as well as efficient cause factors (such as reminders and incentives) on a particular type of behavior. Rychlak has shown that the standard position in psychology holds (wrongly) that properly scientific explanations should not invoke final cause factors. By breaking the theory-method confound, and interpreting his results via final cause explanations, Rychlak defies that aspect of the negative heuristic. The work on volition, reported herein, alters the same injunction in a slightly different, though fully compatible, manner.

But are we certain that the currently dominant research program is in need of reformulation or even replacement? Lakatos claims that a research program is not seriously modified or abandoned if it is both theoretically and empirically progressive; otherwise it is degenerating and should be replaced if a viable alternative is available. The crucial question then is whether the current mechanistic/deterministic approach to research in psychology, that seeks to uncover material and/or efficient causal determinants of human action, is progressive or degenerating?

To our minds, Gergen (1982) goes a long way toward answering that question in the following statement.

> Observers of the science frequently comment on what they take to be a deep and pervasive discontent with the outcomes of traditional research pursuits (cf. Sarason, 1981). With increasing outspokenness, investigators of high visibility and lengthy research experience have begun to raise sobering questions concerning the promise of traditional science. Meehl's (1978) critique of traditional hypothesis testing along with the Popperian view of science, Bruner's (1976) view of psychology as in its "winter of discontent", Cronbach's (1975) lament over the cumulativeness of experimental findings, Sarbin's (1977) argument for a contextualist orientation to understanding human action, Neisser's (1976) misgivings about the predictive capability of cognitive research, Bronfenbrenner's (1977) concern over the ecological irrelevance of much developmental research, Argyris' (1975,

> 1980) elucidation of the manipulative and misleading implications of traditional empirical research, Riegel's (1972) attack on the ahistoric character of traditional developmental psychology, Sarason's (1981) examination of the social and ideological roots of psychological theory, Fiske's (1978) dismay with the meager progress of personality research, Mahoney's (1976) assessment of the damaging effects of the professional reward system, and recent protrayals of research on learning and memory as both ideologically and historically bound (Kvale, 1977; Meacham, 1977; Schwartz, Lacey, and Schuldenfrei, 1978) are all indicative of a major evolution in thinking. It seems fair to say that such generalized ferment has not taken place in psychology since the advent of radical behaviorism in the 1920s. Such citations are only representative of a much broader population of critical self-appraisals within recent psychology. (p. 190)

Gergen proceeds to list an additional seventy references of recent articles that seriously question the fruitfulness of pursuing our traditional approaches to psychological research. Such evidence suggests a degenerating research program (à la Lakatos) in psychology. If Gergen's interpretation is valid, psychology might strive to reorient its approaches to studying humans (such as the approaches suggested in this article) in an effort to undertake a more progressive program of research.

Remember that this article began with Rychlak's (1983) question of whether psychology could be objective about free will. Have the above studies brought us any closer to a science that can study free will effectively? We maintain that demonstrating human volitional control—or the causal efficacy of personal agency—was a precondition for arguing for a free will conception of human behavior. But human action generally represents acts of bounded-will rather than totally free will. We suspect that human beings exert telic, final cause influence within a nexus of material, efficient, and formal cause influences of the world in which we dwell. Many voices from outside of psychology have been calling for us to make precisely the types of change in theorizing and methodology described herein. The philosopher of science, Stephen Toulmin (1981), offers encouragement in the following manner:

> If we are to heal the wounds created by the Cartesian split, and reintegrate humanity with nature, it will follow as a result that human actions, too, are performed *within* the world of natural processes and the older philosophical barriers separating rationality from causality will have to be dismantled along with all the others. A world of nature into which humanity has been reintegrated will no longer be an impersonal, mechanistic world. Rather, it will be a world within which the human reason itself is a causally efficacious agency, within which—as the ancients recognized but the philosophers of the seventeenth century denied—we have the elbowroom that we require to exercise the autonomy that is the chief mark of our humanity. (p. 35)

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