
Internal Versus External Control of Reinforcement

A Case History of a Variable

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ABSTRACT: *In this article the construct of generalized expectancies for internal versus external control of reinforcement is used as a model to present a brief on the importance of broad theory and training in theory construction and evaluation in psychology. Specifically, the extraordinary heuristic value of this construct is attributed to four characteristics: the nature of the definition, the careful imbedding of the construct in a theoretical context, the use of a broad behavior theory in construction of a measure of individual differences, and the programmatic nature of the original research as well as the format of the early publications. Reevaluation of the characteristic criteria for publication of research studies and for the evaluation of promotion or advancement for psychologists involved in research activities is also discussed.*

Internal versus external control of reinforcement, often referred to as locus of control, is currently one of the most studied variables in psychology and the other social sciences. Frequent studies have been done in fields as diverse as political science and public health, and a test of individual differences in locus of control has been translated into many different languages. Current research continues at approximately the same high rate as it did 20 years ago. Perhaps even more surprising has been the relative success of a test of individual differences in predicting behavior for many different populations in a way that made good sense, rather than providing after-the-fact explanations that strained credulity.

Briefly, internal versus external control refers to the degree to which persons expect that a reinforcement or an outcome of their behavior is contingent on their own behavior or personal characteristics versus the degree to which persons expect that the reinforcement or outcome is a function of chance, luck, or fate, is under the control of powerful others, or is simply unpredictable. Such expectancies may generalize along a gradient based on the degree of semantic similarity of the situational cues (Rotter, 1966).

Numerous reviews of internal-external control research and applications (Lefcourt, 1976, 1981; Phares, 1976) have been published, and last year Bonnie Strickland's (1989) APA presidential address brought much of

this literature up to date. I do not intend another review. But the enormous and somewhat surprising popularity of this variable is, in itself, interesting, and I will try in this article to speculate on the reasons for the widespread interest in locus of control.

Naturally, I rejected luck as an explanation, but at first, I thought (Rotter, 1982b) that the social upheaval of the times might be responsible. The Vietnam War, Watergate, the inner-city riots, and political assassinations were disturbing in themselves, but were also concerning many people—including social scientists—because of the perceived lack of control over their own lives. Such factors probably played some role in the interest in locus of control, as did the interest in the F Scale, a measure of implicit antidemocratic or potentially fascist attitude (Adorno, Frenkel-Brunswick, Levinson, & Sanford, 1950), following World War II. However, on further reflection I felt that there have frequently been such periods of social upheaval in the past and that the explanation for the many research studies published was probably due more to scientifically technical reasons.

In the remainder of this article, I will discuss four propositions that may account for the heuristic value of internal-external control, propositions that I believe are particularly relevant to the field of personality theory and personality measurement, but also to the study of psychology as a whole.

The Importance of Precise Definition

The first proposition I would suggest is that the heuristic value of a construct is partially dependent on the precision of its definition. One might wonder why so many social scientists in the last 30 years have been involved in studying a concept that could not be regarded as anything new. Surely in prerecorded history humans were concerned with ideas of causality, and early recorded history is replete with stories and myths describing events controlled by capricious gods and fate, as well as the results of one's own behavior. When social scientists emerged, they also dealt with interpretations of causality, beliefs in luck, fate, notions of alienation (Seeman, 1959), and celestial intervention. An interest in the perception of causality has been present for a long time. What was needed was a

concept that was clear enough to lead to acceptable measurement.

A good definition, especially of a cognitive or subjective variable, must be stated in language that is careful and precise and leads to a common understanding. It needs to be illustrated with many behavioral examples of its consequences if its presence or absence is not directly observable. It should be stated in such a way that the operations for its measurement are not only clear but are widely accepted as logical and reasonable. Terms such as *self-actualization*, *coping*, *intrinsic* versus *extrinsic motivation*, *unconditional love*, and *cognitive psychology* create interest and enthusiasm, but each scientist reads his or her own meaning into the concept, and operations for measurement either do not exist or vary so much that the resulting research produces a series of contradictory or nonreplicable studies. In 1945, Herbert Feigl provided a set of criteria (p. 258) for a good operational definition that would still provide dividends to psychology, if every graduate student in psychology would learn and understand them.

Even with precise definitions, it is usually necessary to distinguish between the construct being defined and other constructs used in the past or present with which it can be confused, as well as making clear the connections to other constructs so that previously collected data can be interpreted and built on.

The Importance of Imbedding a Construct in a Broader Theory

The second proposition is that the heuristic value of a construct is considerably enhanced if it is imbedded in a broader theory of behavior. Although the majority of studies of locus of control have dealt with applied problems, it is important to recognize that the concept originated both from theoretical and clinical concerns, with social learning theory organizing our thinking in both cases.

More specifically, in several of our studies involving increments and decrements of expectancies following both negative and positive outcomes, a large number of our subjects were not raising their expectancies after success or lowering them after failure, and we began to see a pattern of difference in situations in which the subject believed that success was dependent on one's own skill versus those situations in which it was clear that the experimenter was manipulating success and failure inde-

pendently of the subject's behavior. My ideas about these findings crystalized in discussing a psychotherapy client with Jerry Phares who I was supervising. As supervisor, I was trying to understand and interpret the client's behavior from a social learning point of view. This client, whom we had persuaded to try out some new behaviors that met with success, persistently explained away the successes as a matter of luck and not likely to happen again. He appeared, in most situations, to feel that what happened to him was entirely beyond his control. This led to our hypothesis that not only did learning take place differently in chance versus skill situations, but it took place differently among individuals in situations that might be considered ambiguous or novel or that had elements of both chance and skill.

Although the implications for human learning theory were of prime importance in the development of this construct, it was a measure of *relatively* stable, cross-situational, individual differences that captured the interest of many and accounted for at least 90% of the publications that followed.

Two things about this puzzled me. One of these was that although many social scientists were eager to use the test of individual differences, they clearly did not understand the theory in which it was imbedded. Specifically, they regarded these individual differences as fixed traits, or types. The notion of a generalized expectancy, of course, involves the learning theory principle of generalization and a gradient of generalization. Such a gradient implies both generality and specificity, but nevertheless, numerous articles were written and published challenging the notion of generality because some specificity could be demonstrated (Rotter, 1975). The theory does not specify independent traits, faculties, or types, but numerous psychologists have taken a 23-item test, subjected it to an orthogonal factor analysis, and concluded mistakenly that the concept had no generality because some specificity could be demonstrated. Generality-specificity is a matter of degree, not kind.

The second thing that puzzled me was the reaction to the studies of performance. Many of the original studies of internal-external control were done comparing groups on their performance on skill and chance tasks. Sometimes the tasks were different and sometimes the same ambiguous task was described differently to groups of subjects. The substance of these studies was that changes in expectancies and, consequently, both acquisition and performance were very different for chance versus skill tasks, not merely in degree but in kind. For example, in a number of replicated studies, we discovered that the well-known finding that 50% reinforcement is more resistant to extinction than 100% reinforcement was true *only* in chance or experimenter-controlled tasks. In skill tasks in which the subject believes that reinforcement is contingent on his or her own skill, the opposite is true (Blackman, 1962; Holden & Rotter, 1962; James & Rotter, 1958; Rotter, Liverant, & Crowne, 1961). One hundred percent reinforcement is more resistant to extinction than 50% reinforcement.

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Award-based manuscripts appearing in the *American Psychologist* are scholarly articles based in part on earlier award addresses presented at the APA convention. In keeping with the policy of recognizing these distinguished contributors to the field, these submissions are given special consideration in the editorial selection process.

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The size of the increments and decrements following success and failure also differs in chance and skill tasks, as does the generalization of expectancies from one task to another (Bennion, 1961; Blackman, 1962; Phares, 1957; Rotter et al., 1961). What was surprising to me was that these findings, replicated with a variety of tasks, were not greeted by a chorus of huzzahs by learning theorists and did not start up a flurry of research as did the publication of a measure of individual differences.

Part of the reason for this, I believe, was that learning theory, once the heart of psychology, was being abandoned as more and more experimental psychologists were turning to information theory, cognitive psychology, the study of memory, and artificial intelligence.

I believe this was partly the result of pressure, from students and others, to make their work relevant to humans, but the emphasis in the learning theories of the time was on models of learning based on animals who had no language to develop concepts and consequently generalized over many fewer dimensions and, presumably, did not engage in complex thought processes. The pendulum has swung away from learning theories, but it will swing back when psychologists are willing to face the more complex problems of understanding acquisition and performance in humans. It may well be that the present trend toward something vaguely called cognitive theory based on computer models rather than complex humans, who infrequently behave rationally, will eventually suffer the same fate as did the learning theories based on animal models.

What these last two concerns mean to me is that many psychologists are inadequately trained in theory—not the memorizing of some principles or hypotheses that go by the name of theory, but in understanding the characteristics of good theory and bad theory, principles of theory construction and the use of theory to tackle applied problems.

Measurement Principles Should Be Derived From Psychological Theory

The third proposition is that the predictive value of a test is likely to be increased if the principles of measurement are derived from the same theory as the constructs to be measured. The response to test items or stimuli is indeed behavior and, as such, follows psychological laws as well as any other behavior. Statistical methods are useful tools but do not substitute for a theory of behavior. For example, many personality tests are constructed using statistical techniques developed to measure independent faculties of the mind, although the authors themselves would deny being faculty psychologists. Correlation among relative stable personality characteristics is the rule not the exception, and it is probable that the use of orthogonal factor analysis to build personality tests is usually misconceived.

The Internal-External (I-E) Scale (Rotter, 1966) was constructed not only by keeping in mind a theoretical variable and its hypothesized characteristics but also some ideas of the kinds of behaviors and situations we were

interested in studying and the subjects or populations we intended to use. In social learning theory (Rotter, 1954, 1982a), a basic assumption is that the unit of investigation for the study of personality is the interaction of the individual and his or her meaningful environment. Behavior in different situations will be different, although there may be a gradient of generalization from one situation to another.

In studying locus of control, because we were dealing with a broad construct intended to study behavior in a variety of situations, we wanted to sample many different situations without making the total score more dependent on one kind of situation (such as school achievement) than on another (such as political involvement). We did not try for a high alpha (that is, the accepted measure of internal consistency) because we assumed that the correlations among different behavioral referents for the concept were positive but low. One can get very high alphas by asking the same question over and over again, but the predictive limitations of such a test are obvious. Not all, indeed, not any, so-called stable characteristics are totally cross-situational and a test with an alpha in the high .90s is suspect in that it may well be measuring response style more than providing a *representative* sample of the behaviors and situations one wishes to predict or it may merely consist of redundant items.

Along these same lines, it seems foolish to follow the often used procedure of doing an item selection on the basis of some single criterion obtained in one specific situation on one highly homogeneous sample of subjects, when one wants the test to predict behavior in several different situations for different subjects. In constructing the I-E Scale we made several attempts at using Likert formats, but these early measures were found to be too highly correlated with the Marlowe-Crowne Social Desirability Scale (Crowne & Marlowe, 1964). Consistent with social learning theory then, this instrument treats social desirability not as error to be totally eliminated but as a motive affecting choice behavior in test-taking as well as other behaviors. We, therefore, moved to a forced-choice format to bring this correlation down to a reasonable level. I must note here that a zero correlation with a measure of a need for social approval is only desirable if one's theory or experience dictates that the need for social approval is in fact uncorrelated with the variable one is studying—an unlikely probability with personality characteristics, which always tend to be correlated with many other variables.

To improve our item selection, we did indeed use not one but two very different criteria and two populations. One was criterion performance or change in expectancies on ambiguous tasks using college students (Rotter et al., 1961), and the other involved information-seeking behavior on the part of patients, mostly noncollege-educated adults, in a tubercular ward (Seeman & Evans, 1961). If an item predicted either criterion significantly and correlated with the other items in the scale significantly higher than it did with the Marlowe-Crowne measure, it was retained. In all, five years of research and

five forms of the test were used before we attempted to publish the scale (James, 1957; Phares, 1955; Rotter, Seeman, & Liverant, 1962). I believe the implications are that much hard, thoughtful, work has to go into devising a useful measure of a personality variable. One has to have a theory of behavior and, consequently, of test-taking behavior, as well as some notions of the theoretical properties of the variable being studied in order to devise a construct valid measure.

I suppose I could put these ideas in the form of a question, "How many psychologists does it take to construct a new test?" One of my colleagues, Dorothy Hochreich (personal communication, April 1989), suggested the answer of "at least four . . . one to think up and define the variable, one to collect the data, one to do the statistics and a fourth to keep the other three from publishing prematurely."

The Dissemination of Knowledge

The fourth proposition is that the research monograph is an ideal form of publication for the dissemination of knowledge. At this time, *Current Contents*, a journal that records citations, reports that since 1966 when I first published the monograph on internal-external control, there have been at least 4,700 citations to that monograph in the psychological and social science literature, a number far in excess of any other article in the psychological and social sciences for the same period of time. It was also one of the last monographs to be published by the American Psychological Association (APA). (I am not sure whether this was a chance or causal relationship.) However, APA publications appear to favor short articles presumably because of the pressure of numerous submitted manuscripts.

It is my belief that the rapid spread of interest in locus of control would have been markedly truncated without this kind of publication. In fact, the first full description of the construct appeared in 1962 as a chapter in a two-volume book entitled *Decisions, Values and Groups*, edited by N. F. Washburne and written by myself, Melvin Seeman, and Shephard Liverant. In this format, it did not seem to reach many social scientists, and later appearing articles rarely cited it. The monograph (Rotter, 1966) reported the history of related constructs, a careful, elaborate definition, a description of the construct's theoretical relation to other variables in social learning theory, a summary of studies of chance and skill task performance, methods of test construction for the test of individual differences, a variety of norms for different populations, and a summary of results of over 20 validity studies involving very different kinds of criteria and studies of antecedents and long-term predictions. Much of the data presented had not been previously published. The monograph included statements of limitations for the measure of individual differences not only for different subject populations but also for testing conditions and behavioral predictions. I do not believe that a dozen or more separate articles scattered through different journals

over a period of at least eight years would have reached as many people or created nearly as much interest.

If psychology is to advance in understanding human behavior, it needs to emphasize programmatic research (whether theoretical or applied) in which theory and empirical findings are combined. It needs also to build on past research. The tendency to dismiss the past, encouraged by the pressure to keep publications brief, does not speed up the accumulation of stable knowledge. One form of this is particularly pernicious, that is, the tendency to use new terms for old concepts and thereby ignore all the research theory previously accumulated. Kirsch (1985, 1986) has illustrated the value of such analyses in his discussion of antecedents to the concept of "self-efficacy," as have Zuroff and Rotter (1985) in their history of the expectancy construct.

I am aware that the stumbling block to achieving such an atmosphere for publication is the practice of many employers, including our "best" universities, of merely counting publications instead of evaluating them for their creative, heuristic, or scientific merit. Promotions, tenure, and merit raises are determined by such counts of publications. Longitudinal research in human development and psychopathology has suffered for the same reason because such research usually requires several years before the results are available for publication.

I have seen several instances in which an author has two publications when none would have been preferable. The first article reported some finding, and the second reported that the finding disappeared when a new control or different population was used. Most articles I see in the journals are unimpressive because they lack sufficient controls, the theory or significance of the findings is either not clear or not present, or there is no tie-in with the large numbers of studies that have preceded it. Indeed, if the number of publications in scientific journals were a measure of how advanced a scientific field was, then psychology might be at the forefront of all the sciences. Unfortunately, that does not seem to be the case.

Changing publication policies is easier than changing the criteria for recognition of merit. Somewhere, I believe, a start needs to be made. Although the importance, long-term significance, or heuristic value of a research project may be hard to evaluate at the time of its publication, it is not as hard to evaluate the thoroughness, the concern for scientific objectivity, and the presence or absence of replication. It may be time to stop cluttering the journals with hit-and-run studies that are properly designed but add little to improving theory or have no serious practical applications.

I am aware that these ideas are not new. Many psychologists have written about one or the other of them, but we all feel that change is in the hands of powerful others and there is little or nothing we can do about it. But, perhaps there is, and the efforts of many can still bring about change.

I would hope that a strategy of gradual change could be worked out by the efforts of such groups as the APA and American Psychological Society (APS) Board of Di-

rectors, the APA Science Directorate, the APA Publications Board, the Council of Chairpersons of Psychology Departments, and the groups of directors of psychology training programs.

Summary

It is probably clear by now that I have used generalized expectancies of internal versus external control of reinforcement as a model for a brief on the importance of broad theories in psychology. Such theories may have suffered a decline with the abandonment of molecular learning theories. Training in understanding the nature of theories and in the evaluation of theoretical constructs and their operations has also declined. As a result, I believe genuine progress in psychology suffers. Much of the literature filling our journals will soon be forgotten and will give way to new fads and enthusiasms that will be little better than the ones they replace.

Perhaps no one has said this better than E. R. Guthrie in his 1946 presidential address to the American Psychological Association:

Facts may accumulate without theory; but they will prove to be unstable and of little profit in the end. Theories may flourish if their basis lies not in scientific fact but in opinions and interpretations acceptable only to the members of a limited faction; but they will be bad theories. Schools flourish only when theories are not carried back to public facts. Unless psychologists maintain an interest in general theory the fields of psychology will increasingly become independent collections of undigested information. (pp. 19–20)

I can only add to Guthrie's prophetic warning that if the theories are going to add to our understanding of humans, they must be rooted in observables but also must take into account the complexity of an animal who is highly dependent on a rich language, who often behaves irrationally, and whose complex thought processes, motivations, and emotional states seriously affect behavior.

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