

Is Social Psychology Really Different?

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Abstract. Gergen (1976), outlines a number of problems that make it difficult to apply general social psychological theories, or to assess their validity unequivocally. These difficulties are not unique to social psychology, however. The application of general scientific principles has never been a simple matter, not even in the well-established physical sciences. Moreover, there are formidable difficulties in assessing general theoretical propositions in every field of inquiry, since empirical procedures will inevitably depend on assumptions about local field conditions, the adequacy of measurement techniques, and the like. As a consequence, if results are inconsistent with theoretical expectations, there will always be some uncertainty as to where the problem lies. Social psychologists should not assume that their difficulties are totally unlike those encountered in other fields of scientific inquiry. The problems raised by Gergen do not, consequently, rule out the possible development and application of general social psychological theories.

Gergen's most recent essay on the "history issue" (1976) emphasizes a somewhat different point of view than the one that was central in his initial effort (Gergen, 1973). His earlier piece included a substantial discussion of certain differences between social psychology and the "hard" sciences that made it seem unlikely (in Gergen's view), that social psychologists would ever find stable laws. Particular emphasis was given to the argument that people are often motivated to maintain their unpredictability and might thus alter their behavior when apprised of some general pattern that had been detected by previous researchers (the enlightenment effect). The prescriptive (evaluative) character of many psychological theories was also felt to be important, since evaluative theories are likely to induce behavioral changes in response to the "advice" that they so freely offer.²

In his more recent piece Gergen has modulated these emphases, for he tells us that ". . . the precise extent to which enlightenment and evaluative loading influence the behavior of those familiar with current social psychology is . . . an empirical issue." Nonetheless, he continues as a forceful critic of "general theory," although he now appears to admit that such theories might be possible if one allowed for theoretical propositions of a rather abstract character, such as exchange theory. He is quick to add, however, that general theories of this sort do not, by themselves, allow us to make concrete predictions regarding social behavior, for they are invariably affected by unstable contextual variables. For example, social exchange theory is "virtually without predictive value," unless we can determine the utility of different outcomes to the individual(s) that we are observing; and unfortunately, these utilities (pay-off values) may be quite unstable. Hence concrete predictions will necessitate a detailed up-to-the-minute analysis

of the values that are prevalent among the people we intend to study.

General Theory. In this reading of his essay, Gergen seems to be arguing that despite the continuing operation of a stable set of general laws (e.g., exchange theory), social behavior may fluctuate widely, since the situational variables (e.g., pay-off values) that enter into our theoretical formulation may change from time to time and from place to place. This conclusion seems quite unobjectionable; virtually all social psychologists would agree that under changing conditions, the behaviors implied by some unchanging general theory are likely to vary substantially. For example, Schlenker approvingly quotes Homans (1967, p. 56): ". . . a relatively few general propositions hold good of human behavior, from which under a variety of given conditions . . . a great variety of different forms of concrete behavior follows." In a similar vein, Manis (1975) suggested that "despite changes in the observed phenotypic relationships between a given set of variables, diverse findings may nonetheless be consistent with a single underlying genotypic law as it affects social behavior in a variety of times and places."

I am struck by Gergen's underlying assumption here that general theory is useless, if by itself, it is of little value in predicting complex social behaviors. And yet, this criticism would seem to be unfair and unrealistic; we would not, for example, consider it reasonable to ask a meteorologist to predict tomorrow's weather from a general forecasting system, while denying him vital information concerning air pressure, winds, neighboring weather patterns, etc., that would presumably serve as important variables in the application of his model.

The relationship between any theoretical proposition and the concrete observations that it implies is invariably complex, since the application of a theory normally involves several ancillary assumptions. Thus, in criticizing traditional psychological research (as contrasted with computer simulations), Reitman has argued (1965): ". . . we cannot get a test of theory in and of itself. We must settle for a test of the theory taken together with all the assumptions about manipulations, measures, and conditions that couple the theory by means of operational definitions to the real world."

An illuminating paper by Forsyth (1976) reminds us that this difficulty has previously been recognized in other scientific fields. In The Aim and Structure of Physical Theory, Duhem (1906) contended that a physicist could not unambiguously test any isolated hypothesis, but was always forced to assess an entire group of hypotheses. As a consequence, if an experimenter obtained a set of results that disagreed with theoretical expectations, there would inevitably be some uncertainty as to just where the problem lay. Gergen's example of exchange theory seems quite appropriate here; an investigator might be forced to consider whether an unexpected set of results derived from the invalidity of the exchange proposition, from an error in estimating the effective pay-offs in the situation, or from some other faulty assumption that had tacitly been accepted.

In his discussion of general theory, Gergen seems to dwell briefly (and pessimistically) on this point. He tells us, for example, that "psychologists have found it virtually impossible to conduct critical tests. . . to link general statements unambiguously to observables." He then goes on to conclude that "such problems are inherent in the attempt to validate any theory of broad generality" (emphasis added).

is last quote is unfortunately somewhat ambiguous, since the words "any theory of broad generality" may refer to (1) any social-psychological theory of broad generality, or (2) any theory of broad generality, either in the physical or the social sciences. Duhem's thesis would argue for the second of these interpretations, and Duhem would doubtless even further in contending that there are logical difficulties in devising a clearcut single ("yes or no") test for any scientific hypothesis, regardless of its breadth or narrowness (see Forsyth, 1976, for re on the difficulties involved in establishing any "crucial experiment").

Since social behavior is always substantially influenced by contemporaneous forces that may change from time to time (and hence must be monitored carefully), Gergen is doubtful about the value of general theory. Indeed, he is concerned that such theories are predictively impotent, and are almost always applied post hoc. . . after we have observed the behavior that we wish to explain. General theories can, however, be applied in other, more useful ways.

In his previous essay (1973), Gergen outlined the case for theory as a "sensitizer" that might help us to comprehend the social patterns we observe. This type of application constitutes only a modest gain, however, if indeed it must always proceed after the fact. But there are other possibilities. General theory may serve a useful sensitizing function by stimulating us to consider (predict) the future circumstances in which a particular pattern should emerge. Moreover, if we can control some of the independent variables that are operative in our theoretical system, we may be able to apply the theory in the attainment of some useful practical goal.

For example, consider the general proposition that through intermittent reinforcement it may be possible to sustain a learned behavior pattern over a very long period of time, without expending much in the way of incentives. This law might conceivably have some application in classroom setting, if we can empirically determine the events that serve as effective reinforcers for a particular group of children. The existence of the general law may thus stimulate us to explore various events that may potentially have reinforcing properties because we believe (on the basis of our general theory) that when we discover an effective reinforcer, we will be able to apply it intermittently to maintain certain behaviors that we consider socially desirable. Note in this example that, in accordance with Gergen's concerns, knowledge of the general theory would be insufficient by itself; our faith in the general model might, however, guide us to engage in a type of "local engineering" research that we might not have considered in its absence (i.e., assessing various events to determine their reinforcing properties). The obvious hope in all this is the possibility that the general model, in combination with an appropriately chosen (i.e., "locally effective") pay-off might ultimately yield a practical classroom procedure.

Gergen seems to feel that the limitations outlined in the preceding paragraphs render general theories relatively useless, since they cannot be applied to any concrete situation without a detailed understanding of the circumstances that prevail there. The application of general scientific laws has never been a simple matter, however, not even in the well established physical sciences. The technical problems that engineers routinely encounter in the development phase of a project should remind us of the difficulties that concrete applications often entail, even in

fields that have access to a substantial set of stable, generalized, laws. And I do not see any basis for believing that abstract psychological principles can (or will) be applied without a comparable expenditure of time and effort. Gergen's stress on accurate information concerning the local (then and there) forces that are operative in any particular setting thus seems quite appropriate, both in social psychology and in other scientific fields as well.

Let us conclude this section by agreeing with Gergen that there are indeed difficulties in deriving unambiguous tests and practical applications from any theoretical proposition. Despite similar problems, however, some fields of scientific inquiry have succeeded in developing abstract, reasonably stable sets of laws that serve as a sort of intellectual ideal for many social psychologists, and that sometimes appear to have desirable practical applications. This does not guarantee a similar success for our own field. Nonetheless, the various arguments that Gergen has assembled in his present essay do not convince me that our problems are unique. Hence I do not see any compelling reason why social psychologists should give up the search for stable theoretical laws as an unrealistic, hopeless, quest, while other investigators continue the search (with occasional reports of success), despite the acknowledged difficulties of the task.

Underlying processes. In an earlier piece (Manis, 1975), I suggested that the processes that controlled social behavior might be relatively stable, although they might operate on an endless variety of social contents, to yield the instabilities that we often observe as we carry out our studies in varying times and places. Gergen (1976) contends, however, that these stable underlying regularities (processes) may be largely fictitious: "Such regularities are inevitably hypothetical constructions, artificial templates employed in making sense of an otherwise inchoate flow of particulars. Thus, when we speak of 'processes underlying social behavior' we are dealing primarily with the skill of the investigator in formulating a hypothetical structure suitable for encapsulating a series of selected observations."

In contrast to Gergen's unbelieving, critical tone, I take this passage to present a fairly accurate description of the scientific enterprise in some of its proudest moments. For the development of a major scientific theory quite commonly involves the construction (invention) of some hypothetical but unseen mechanism or process that lends coherence to an otherwise chaotic set of particulars. Consider, for example, Mendel's hypothetical construct, the hereditary factor (or gene, as we now know it), which was not directly observable in Mendel's time, but constituted something of a fictional entity, an "as-if" mechanism that was useful in explaining, predicting, and unifying a variety of observations regarding heredity; subsequent advances in genetics have, of course, established a more palpable body of information regarding the gene. Similarly, Darwin's theory of natural selection may plausibly be regarded as an invention, an imaginative hypothesis concerning an unseen general process that seemed to offer a plausible explanation for the myriad forms of life that Darwin observed in his travels on the ship, Beagle. While the distinction between content and process is not so clear as one might like, I do not see why social psychologists should refrain from the attempt to develop a hypothetical framework of underlying processes, to "make sense" of the varied social patterns that have

been reported. This strategy seems to have been useful in other fields of inquiry, and may prove helpful in our field as well (if it has not already done so).

Durables. Near the end of his essay, Gergen raises the difficult question: "Where are the durables?" He then cites a number of "classic" findings (e.g., the relationship between attitude and learning, the sleeper effect, etc.) that have recently proved difficult to replicate. How are we to interpret this?

(1) One possibility that must be considered in such cases focuses on the validity of the original reports. In discussing some of these cases, Greenwald (1975) implies that the initial research reports may have reflected Type I errors in which the null hypothesis was erroneously rejected. Moreover, he suggests that the initial studies in these fields may have led subsequent investigators who could not obtain the established findings to regard their own work as inadequate in some way, and hence unworthy of journal publication. The well known editorial bias against "negative" results may also have contributed to the seeming replicability of these effects in years gone by.

(2) A second interpretation of these non-replicated effects follows the "if-then" strategy that Gergen (1976) finds so repellent. Here the theorist might accept the initial findings as valid, along with the more recent negative results, but would attempt to discover some moderator variable (or group of variables) whose presence was critical for demonstrating the phenomenon in question. This is, as Gergen notes, a difficult strategy to execute convincingly, especially if the critical changes affecting our results derive from shifting public values and attitudes, since in such a case it might be difficult (but not necessarily impossible) to determine which change(s) were truly vital.

(3) Gergen opts for a third alternative that is in some ways rather similar to #2 (above). He believes, for example, that while it was probably true at one time that people found it difficult to learn and retain information that challenged their own beliefs, for reasons that cannot be specified in any detail, historical-cultural changes have led to the erosion of this earlier generalization (law?). This historical interpretation can also be expressed as an "if-then" proposition: If we are concerned about the behavior of people in the years before 1958, then it is true that attitudes were related to learning speed (people readily retained information that they agreed with); if we are not concerned with this period, and instead focus on the years since 1958, then we must conclude that there has in recent years been no relationship between attitude and learning.

The main difference then between alternatives #2 and #3 concerns the level of analysis that we apply to our moderator variable. Alternative #2 is based on the hope that in many cases it may be possible to use theoretical terms (e.g., prevalent values, social structure, coping styles, etc.) to characterize the situations in which a given effect is likely to be observed. Alternative #3 sets a more modest goal, and is content to characterize the moderator in exceedingly global terms (e.g., contemporary social forces) without attempting to discover which factors have been truly critical.

Alternatives #2 and #3 may be contrasted in another way. The traditional if-then strategy (#2) is based on the hope (or assumption) that in many problems of interest, the critical moderator variables will op-

erate with reasonable stability. For example, if a moderator variable (M) is an important factor in determining whether attitudes (A) do or do not affect learning (our dependent variable), then we should find a stable MxA interaction in different studies, conducted in a variety of social settings. However, suppose the M x A interaction is radically affected by many other variables that prove to be quite changeable in these differing environments. Then we would have a situation where variable M could not be characterized as an effective moderator; that is, the M x A interaction (moderator x attitude) would not yield a stable result, but would be continually interacting with many other variables in the system. We might thus have one or more higher-order interactions (e.g., MxA x X x Y x Z) in which the moderators of the attitude effect operate so complexly that we would conclude, for all practical purposes, that we could not succinctly explain why it was that the attitude-learning effect was clearly present at some times and places, but not at others. In brief, we would have to accept alternative #3, which would assert that an interacting complex of changing social-historical-economic-ecological-etc. forces (unspecified) seem to be important in determining whether attitudes do or do not affect learning. We would then presumably follow Gergen's suggestion and accept the fact that our research could do no more than establish the prevalent patterns at a given time and place, recognizing that these effects were likely to prove ephemeral.³

Now it seems reasonably clear that there are many areas of social-psychological interest in which this sort of instability is bound to be true. Indeed, my personal guess is that our social psychology journals are filled with provocative research reports which often aspire to universality, but which are, alas, primarily a reflection of "local norms." I think that we all suspect this to be true, but hope (and sometimes believe) that our own work will be dealt with less rudely by changing historical circumstances.

It occurs to me, moreover, that the instability of many results may derive from the very ambitious and detailed level at which we have often pitched our work. Hendrick's contribution to this symposium (1976) quotes Scriven (1964) to the effect that astronomy would be unlikely to have advanced very far if astronomers had set their goal as one that involved "predicting the behavior of planets to within a micron or an inch." In such a case, apparently, a number of variables that are normally ignored would be of substantial importance, and the resulting "openness" of the system would make it impossible to generate accurate predictions. Is it conceivable that some of the difficulties we encounter in trying to locate stable behavior patterns derive from the level of specificity that we have bravely attempted to deal with in our attempts to develop "general" theories?"

Consider, for example, the recent difficulties that have been reported in attempting to relate behavior to attitudes and personality traits (e.g., Wicker, 1971; Mischel, 1968). Most of this work has been carried out with what Fishbein and Ajzen (1974) termed single act criteria (i.e., engaging in a particular act, such as church-going, thought to reflect some underlying attitude or trait). These studies have typically reported discouragingly weak and often nonsignificant relationships between the questionnaire measures and the behavioral item that was selected for study. Results of this sort may, however, be interpreted as a reflection of the difficulties we encounter when we attempt

to predict behavior at a highly specific level. By contrast, Fishbein and Ajzen's research indicates that by broadening the focus of our inquiry through the use of multiple act criteria, we may be far more successful in relating attitudes and behaviors. Note that in this case we forego the goal of predicting which of our respondents will engage in a particular behavior (e.g., who will donate money to a religious institution?), and attempt instead to predict overall behavior patterns (e.g., which respondents will display the greatest "volume" of pro-religious behavior when we examine a large pool of relevant actions, and treat each act as equivalent with respect to our "behavior scale?"). This is, of course, the same strategy that has been followed with some success in validating tests of academic aptitude. If, in place of the multiple act (multi-item) criteria that have been routinely used in this work, we had attempted to validate aptitude tests against individual achievement items (e.g., who was the third president of the United States?), we would doubtless find it difficult to show any appreciable, stable relationship between aptitude test scores and classroom performance.

Interactions. Many social phenomena appear to be affected by interactions involving two or more independent variables. This has proven to be a difficult problem, since the relationship between an independent variable (A) and some behavior of interest will often vary as we change (1) the character of our respondents, (2) the setting that we choose to study, or (3) certain unspecified cultural forces that may make our attempted replications inexact "copies" of the original studies that attracted our attention (e.g., our present subjects will have been exposed to a somewhat altered set of social values in their day-to-day lives). The difficulty here is that the impact produced by variable A may prove quite inconstant as our "background" variables (B, C, D, etc.) assume different values; hence it may be difficult to make any general statement about the effects of A (even if A appears to have a powerful impact in some "local" circumstances).

Cronbach (1975) presents us with many examples of this sort, ranging from inconstancies in the relationship between aptitudes and classroom performance to inconstancies in other, relatively "non-social" fields (see below). Is it possible to maintain a belief in general laws, in the face of interactions like this? Cronbach is somewhat doubtful and comes out for a position much like Gergen's.

Interaction effects are, of course, a clear sign that many of our independent variables do not affect behavior in an unchanging, "general," fashion. However, there still remains the possibility that at some more abstract level an unchanging general process underlies the variability that we observe. As Cronbach observes, Darwin's theory of natural selection provides a classic example of this, for it enables the biologist to explain certain empirical inconstancies that may be noted, as he varies the time and place of his investigations. For example, while one species (S_1) may have been more prevalent than another species (S_2) at one point in time, the forces of natural selection may now have reversed this, so that today's investigator may find a greater prevalence of species S_2 . This interaction between (1) the two populations being compared, S_1 and S_2 , and (2) the year that our hypothetical studies were conducted might nonetheless be fully consistent with Darwin's theory, and hence we would not conclude that observations like this necessarily constituted a severe challenge to the biologists' hope that

they might reasonably continue in their quest for broad explanatory principles. Analogously, it is conceivable (but far from certain) that many of the disturbing interactions that have been reported in the psychological literature may ultimately prove to be more readily intelligible when viewed through the lens of some underlying abstract process.

Cronbach's article describes interactive effects in several fields that are quite distant from social psychology. For example, he outlines a study in which the sleep-inducing effect produced when a standard dose of hexobarbital was injected into a laboratory mouse was altered substantially (sleep time was cut by more than 50%), depending upon whether the bedding in the animal's cage was derived from hard or soft wood. Softwood shavings (like red cedar) seemed to step up the activity of certain enzymes that metabolize hexobarbital, and hence enabled the animals to recover from the drug relatively quickly. I suspect that this example would not lead most psychopharmacologists to reject general theories as irrelevant, however, since it mainly testifies to the importance of metabolism, an underlying general process that satisfactorily explained the inconstant effects produced by the hexobarbital.

The diminished effectiveness of DDT in recent years provides yet another example of empirical inconstancy, but again, it is an example that can plausibly be fitted into a general biological model. In this case it is assumed that the selective pressures that have been operative since the introduction of DDT may have led to a systematic change in today's mosquito population making them more resistant, on the average; that is, it is assumed that survival and subsequent reproduction would be most likely among those mosquitoes which, through random genetic variation, were relatively hardy in the face of this man-made hazard.

Examples of this sort, in biological fields quite distant from social psychology, have led me to wonder if perhaps the empirical inconstancies (interactions) that are so commonly reported in psychological research might be common in the more mature nonbiological sciences as well. Hasty consultation with an elementary physics book and with a number of colleagues in the hard sciences provided substantial support for this conjecture. For example, the gravitational acceleration of a freely falling body varies somewhat from one place to another, depending on such things as latitude, elevation, and the density of the subsurface material where we conduct our experiment. Thus, in any given locality, the distance that an object will fall in a particular time interval may be described by the equation $d = 1/2 g t^2$, where g is the acceleration due to gravity at that locale, and t is time interval in seconds. Note, however, that if g varies somewhat from place to place (as it does), when we plot d as a function of t^2 , we will obtain an array of rising straight lines, all of which will start at the origin and then fan out, since some lines will have a steeper slope than others (the slope will depend on g , the rate of gravitational acceleration at that point on the earth's surface). Thus, if our object was dropped in London, g would have a value of 981.188 cm/sec²; hence, it would accelerate more rapidly and fall further in a given time interval than the same object dropped in Honolulu (where the g is 978.966 cm/sec²). If we were to subject such data to an analysis of variance in which the distance that the object fell constituted our dependent variable, and the independent variables were (a) the various locales that served as test sites, and (b) the length of time that the object had fallen before we determined the dis-

tance it had traversed, we would obtain a highly significant interaction; i.e., the observed distances could not be explained by an additive combination of our independent variables. If these empirical data represented all that we knew about falling objects, we would have to admit that (1) while distance was a direct function of time in every locale, (2) the distance that was traversed in any given time interval was inconstant and varied (for unknown reasons), depending upon the location of our experiment. This example reminds me of Cronbach's data, which show that classroom performance normally goes up as aptitude is increased (just as distance is directly related to time); the slope of this aptitude-performance function varies substantially from one classroom to another, however, leaving us in our present state of partial puzzlement.

This line of argument does not mean that psychology will inevitably be able to account for its inconstancies (unexplained interactions) with the same sort of confident general theories that we sometimes hear from our colleagues in the hard sciences. It does, however, suggest that interactive effects need not be interpreted as a sign that we must restrict ourselves to purely local phenomena and give up any hopes we may have had for broad theoretical understanding.

Where does all this leave us? It seems to me that Gergen's observations do not eliminate any reasonable hopes for general social-psychological theories; however, there is no guarantee that we will ultimately succeed. His comments forcefully remind us that general theories by themselves are likely to be insufficient when it comes to predicting or controlling concrete social phenomena, whether in the laboratory or in the real world. Things might be much simpler if the world was so consistently structured that we could, in fact, account for all the particulars that we observe by reference to just a few general propositions. Things seem to be more complicated than this, however. While general principles seem to be operative in many areas of inquiry, they are almost invariably affected by "local conditions," which must be properly evaluated before we can apply these abstract theories to aid our understanding (in our "pure" research), or to help us cope with practical problems (in our applied efforts). In these respects, social psychology shares many of the problems that have previously been encountered in other fields of inquiry.

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Footnotes

- ¹Requests for reprints should be sent to Melvin Manis, Research Center for Group Dynamics, Institute for Social Research, University of Michigan, Ann Arbor, Michigan 48109. All statements are those of the author and do not necessarily reflect the opinions or policy of the Veterans Administration. Dorwin Cartwright, Howard Schuman and William Uttal provided helpful comments in response to an earlier draft of this paper.
- ²Lewin's classic paper (1935) on Aristotelian and Galileian modes of thought emphasizes the fact that Aristotle's physics included many concepts that would currently be regarded as preeminently evaluative. For example, the "highest" forms of motion were thought to be circular and rectilinear, and would presumably occur only in the heavenly movements of the stars. By contrast, modern physics is exact and mathematical; abstract functional relations have now replaced the evaluative explanations of the past. Lewin felt that psychology was still replete with many value concepts, although he felt that the field was then in a state of transition. Consideration of our current state of knowledge suggests that this transition is far from complete.
- ³If, as Gergen asserts, social-psychological patterns are changing, and the changes are fairly rapid, the would-be researcher faces a very difficult task; many complex scientific issues might remain unresolved because the phenomena in question had changed substantially before any real understanding was achieved.