

## **Gergen's Reappraisal of Experimentation in Social Psychology: A Critique**

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Gergen (1973) was an early critic of the findings of social-psychological experimentation for their lack of stability over time. That early statement of his views was criticized, in turn, by some who claimed that Gergen had failed to distinguish between temporally unstable facts about social behavior and the basic processes underlying that behavior which remain constant over time. In a recent restatement of his views, Gergen (1978) has responded to those critics, and has extended and further refined his position. The present paper points out flaws in Gergen's recent arguments, especially in those which appear to have been intended to answer his critics, and suggests that those critics appear to have been correct in pointing out limitations of Gergen's position. In conclusion, a more moderate view than Gergen's of the external validity problem in social-psychological experimentation is advocated.

In his article, "Experimentation in social psychology: A reappraisal," Gergen (1978) makes some valuable contributions to our understanding of the limitations of experimental research in psychology and of the theories on which they bear. He addresses a wide range of issues, but the present discussion will be limited to one of his major foci, historical fluctuation. To summarize very briefly, Gergen argues that the relationship between an experimentally manipulated variable and the subjects' behavior will vary with the particular subjects, time, and place. As a result, it is impossible to establish a universal link (psychological law) either between the independent and dependent variables, or between either of them and the intervening processes upon which their relationship depends. And it is impossible to choose among the competing theoretical explanations on the basis of experimental findings. According to Gergen, any hypothesis can and will be valid for some persons at some time.

These issues were raised previously by Gergen (1973) in an article which was widely publicized and debated. His "reappraisal" seems to be, in part at least, a response to critics such as Schlenker (1974) and Manis (1975). In an apparent attempt to provide more support for his position, Gergen has carried the argument further. He has extended the notion that the results of an experiment cannot be generalized with confidence

beyond the context in which the experiment was performed, in two ways. In both, it appears that he has exceeded the limits of reason.

In the first case, Gergen claims that alterations in the relationships between variables can occur "over very brief periods of time" (1978, p. 520). As a case in point, he considers the relationship between similarity and attraction. He first suggests that the relationship might be mediated by an enhancement of self-esteem produced by an encounter with a similar other. Then he argues that this basis of attraction may be altered not only in the course of long-term cultural changes, but that it is also vulnerable to "situational cues or constraints" (1978, p. 521). Gergen goes on to specify a hypothetical example of the latter: "...if the individual participates in an experiment where *self-esteem* is threatened, the bond between similarity and esteem may become relevant; in a situation threatening one with *boredom*, stimulus needs might become more salient" (1978, p. 521, italics added). Gergen appears to be suggesting that because two experiments testing the similarity-attraction hypothesis might differ in so far as one experiment threatens subjects while the other bores them, they could produce conflicting results. This issue is often considered under the heading of external validity. In those terms, the findings would be said to be limited to (i.e., could not be generalized beyond) their own experimental context.

Gergen points out correctly that because the effects of contextual variables can never be eliminated completely, the generality of experimental findings is always limited. But he is wrong to conclude that experimental findings are chaotic or useless. In fact, it is often through the appearance of such discrepancies that the effects of previously unrecognized causal factors are detected. A comparison of the procedures of discrepant experiments may reveal differences that are possible causes of such confounds. Follow-up-experiments can then include those possible causal factors as independent variables and thus may supply evidence, in the form of significant interactions between those variables and the original independent variable, which extends our knowledge of the phenomenon. It has been argued by some that a lack of standardization of experimental procedures is desirable because it can lead us to uncover such qualifying interactions, or, should no interactions be found, demonstrate a broader generalizability of our experimental findings across such contextual variables.

Gergen's second extension of his earlier position is, like the first, more extreme than his original argument. Not only do empirical findings vary from time to time, culture to culture, and so on, Gergen now argues, but the fundamental processes underlying behavior are no more likely to be "transhistorically valid", to use his words. Gergen attempts to defend this assertion by showing that not only are relationships between independent

and dependent variables limited to particular contexts, but so are the mediating relationships that underlie them. He attempts to support his position by citing examples, but they are far from convincing.

It may be true, as Gergen argues, that the similarity of political attitudes could be demonstrated to induce attraction more easily during periods like the sixties when political attitudes were highly salient cues in person perception, than in the politically quiescent fifties or seventies. But the fluctuation in findings would not, in itself, discredit the possibility of a persistent link between similarity and attraction. There is a vast difference between a change in the kind of similarity which induces attraction and a change in the similarity-attraction relationship itself. To argue otherwise would be tantamount to claiming that there is no general tendency for the probability of a response to be influenced by its consequents because the behavior of a three-year old is modified by the subsequent delivery of M&M's but the behavior of a Wall Street banker is not.

Perhaps even more illuminating is Gergen's treatment of another of his examples, gift-giving. In it, his failure to make the distinction (see Manis, 1975) between the contents of social behavior and the basic processes which underlie them becomes even more apparent. Giving a gift to someone might induce attraction in the context of American culture, Gergen points out, but the same act might discourage attraction in Japan, where gifts have very different implications for the status of the recipient. In this case, Gergen fails to recognize that the effects of gift-giving are merely an example of the many social phenomena which follow the more general rule that people are attracted to those who provide them with valuable rewards. The claimed discrepancy between our culture and the Japanese culture would not invalidate the general rule. It would be merely another case of the reinforcement value of a particular stimulus varying from one context to the other. Does the American's response to a North African host's offer of sheep's eyes as an appetizer before dinner indicate that gifts do not induce attraction even in the members of our own culture?

Admittedly, the two points raised above address only a small part of Gergen's treatise. But their implications are very important. If one accepts Gergen's arguments, then one is forced to his conclusion that experimentation in social psychology, and elsewhere, is of little value in developing adequate theories of behavior. But if the above criticisms of those arguments prevail, so that alterations in empirical relationships are trusted to be more understandable than Gergen claims, and the basic processes underlying behavior may remain largely unaffected by those alterations, then it is possible to retain the more moderate view that experimental findings need to be qualified according to the generality of the subjects, settings, and operations employed in producing them. In

this latter view, the magnitude of the external validity problem varies with each individual experiment. The problem does not necessarily render experimental findings indeterminate or useless.

An instructive example, and one which indicates the breadth of the problem itself, is provided by research on the operant control of visceral responses. Early studies showed that curarized rats reduced their heart rate about 20% when reinforced by direct stimulation of the brain or avoidance of electric shock. These findings had important theoretical and practical consequences. They led to both a reconceptualization of the relationship between visceral and somatomotor processes and to such applications to human problems as biofeedback treatment for hypertension. In the meantime, however, investigators were having great difficulty replicating the original findings. According to one representation of the historical alteration in the findings (Miller and Dworkin, 1974), there was a steady decline in the magnitude of the observed effect during the five years following the first experiments.

An intensive search for the cause of this historical alteration was undertaken by some of the investigators involved, but it met with little success. Hypotheses were advanced about changes in the procedures used to maintain curarized animals by artificial respiration, changes in the design of the respirators themselves, variations in the manufacture of the curare compound used, and changes in the conditions under which the rats were bred and raised which might have altered their level of emotionality (see Obrist, Black, Brener, and DiCara, 1974). None of those hypotheses was supported by convincing evidence.

Ironically, it now appears that curarization, the likely root cause of the difficulties in reproducing the original heart rate conditioning findings, failed to serve the intended purpose of ruling out the involvement of somatomotor processes in heart rate changes. But the crucial point for the present argument is that the instability of these findings has not prevented them from contributing greatly to our knowledge of visceral processes. This is because the variation in the magnitude of the conditioning effect did not reflect any change in the underlying mechanism. The fact that particular experiments in which the effect was demonstrated could not be repeated does not mean that their findings were artifactual in the conventional sense. That fact does not deny the role of reinforcement in the control of autonomic responses. That is why it has been possible to use these findings as a base for further progress in both theory and application, even as they were changing.

The experience of those investigating the voluntary control of visceral responses is not unique or even unusual. A wide variety of inconsistencies in experimental findings which result in failures to replicate, differences between laboratories, and the like are common in all scientific disciplines. In his analysis of social-psychological experimentation,

Gergen emphasized the role of cultural change as a cause of such inconsistencies, especially as that change is brought about by the dissemination of social scientists' research findings to prospective research subjects. That seems an unlikely explanation for the inconsistencies in the heart rate conditioning findings, though the more general label of changes in the research context would apply. But neither the distribution of alterations in the effects of experimental manipulations nor the nature of their causes is the real issue here. The main point of contention is the implication of those inconsistencies.

Responding to criticisms of his contention that experimental tests of hypotheses are rendered meaningless by their lack of transhistorical validity, Gergen struggles to convince us that change can be rapid and unpredictable and that underlying processes are as variable as observed relationships between independent and dependent variables. Critics like Schlenker (1974) and Manis (1975) had pointed out that Gergen's extreme conclusions required such extreme premises, and Gergen attempted to provide them. But his arguments are not convincing. Gergen deserves credit for being among the first to stress that external validity is a serious problem for experimentation, perhaps in social psychology even more than elsewhere. But experimental findings are not chaotic and useless. They are a necessary part of the continuous growth of knowledge. Research findings, and the theories which depend on them, do their work in stimulating further research and theory (perhaps even by their lack of consistency), and then give way to their successors. But even if their half-lives are very short, they serve as necessary links between the past and the future. Gergen's arguments are similarly valuable, despite their limitations, for the debate which they have sparked over these important issues.

It is interesting to note, in conclusion, that the debate over these issues has been somewhat out of character for an empirical discipline like social psychology. Neither rhetoric, argument by example, nor the weight of heavy prose seems a likely path to a final settlement. A recent article by Dipboye and Flanagan (1979) suggests an alternate approach. It is possible to specify precisely the range of subject characteristics, settings, specific measures of criterion behaviors, and even time, across which any empirical relationship has been found to hold. Such an evaluation of external validity has obvious advantages over the exchange of claims and counterclaims. Perhaps it will set a new direction for the future consideration of these questions.

### References

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