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Gergen Versus the Mainstream: Are Hypotheses in Social Psychology Subject to Empirical Test?

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K. J. Gergen's (1982) argument that hypotheses in social psychology are not empirical propositions is critically examined and shown to be erroneous. Nevertheless, this article demonstrates that, without necessarily appearing obvious, some hypotheses can be derived from propositions that are like tautologies and that their confirmation as such is of little interest. An analysis of hypotheses in recent articles in the *Journal of Experimental Social Psychology* and the *Journal of Personality and Social Psychology* suggests that hypotheses derivable from propositions very much like tautologies may not be infrequent. Implications are considered for what kinds of social psychology experiments are of value to perform.

Most of the criticisms of social psychology experiments have seemed to call not for radical change but only for modifications and improvements of existing approaches (for an example see E. Aronson, Brewer, & Carlsmith, 1985; Greenberg & Folger, 1988; Jones, 1985). Thus, in response to ethical problems, researchers have attempted to minimize stressful treatments and the use of deception and to emphasize debriefing. Experimenter effects can be avoided by arranging for those conducting a study to be unaware of the experimental condition of the subjects. Ecological validity—when at issue—may be enhanced by making laboratory settings more realistic or by moving into the field. Efforts can be and are being made to be sensitive to the meaning of the experimental situation for the subject and to avoid obvious hypotheses that represent common sense or "bubba-psychology"—what one's grandmother knows already.

There has, however, also been a concerted attack, spearheaded by Gergen (1982, 1985a, 1986b, 1991), against the very idea that social psychologists should undertake experimentation to test hypotheses at all. Gergen has claimed that social psychological hypotheses are not empirical propositions but merely reflect linguistic conventions. Although this claim has garnered a lively following in the social constructionist movement, the claim seems to us, as to mainstream social psychologists, erroneous. Yet we believe that something very close to Gergen's belief is true for some, though not all, hypotheses. In this article we argue that although the empirical demonstration of some hypotheses provides useful evidence for their validity, the demonstration of certain others—which need not be obvious ones—has little value.

In the sections that follow, we shall take a closer look at Gergen's (1982, 1985a, 1986b, 1991) arguments and attempt to sort out the respects in which we feel he is correct from the respects in which we feel he is wrong. We believe and attempt to show that some hypotheses in social psychology can be derived from propositions that are very much like tautologies and that their confirmation as such serves little purpose. Furthermore, we present an analysis of hypotheses in runs of recent articles in the *Journal of Personality and Social Psychology (JPSP)* and the *Journal of Experimental Social Psychology (JESP)*, which suggests that hypotheses derivable from propositions much like tautologies may occur more than occasionally in mainstream social psychology.

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Gergen's Challenge

One of Gergen's (1973, 1982) best known arguments, significant as it is, does not actually deny the possibility of putting social psychological hypotheses to empirical test. This is the argument (Gergen, 1973, 1982) that the phenomena and relationships of primary concern to social psychologists are inherently unstable. "There would seem to be few patterns of human action, regardless of their durability to date, that are not subject to significant alteration" (Gergen, 1982, p. 34). If this is true, it certainly militates against the possibility of general laws in social psychology analogous to those of the hard sciences. However, it would not imply the impossibility of empirical tests of social psychological hypotheses. The generality of the hypotheses put to test would just need to be restricted. Perhaps one could, as McGuire (1985, 1989) has urged, seek to identify moderator variables affecting when relationships do and do not hold. There would be nothing against confirming a hypothesis for a limited population at a specific time.

Gergen's (1982) more radical assertion, however, is that "the sociobehavioral sciences are essentially nonempirical" (p. 79). The validity of hypotheses about human behavior cannot be assessed by empirical means (Gergen, 1982, p. 74); confirmation or falsification by experimental data is a chimerical goal (Gergen, 1982, p. 73). Most mainstream social psychologists seem to find Gergen's claims (or the related claims of Smedslund, 1985, and Ossorio and Davis, 1968) sufficiently outlandish that they do not really take him seriously. They may even say to themselves, "With enemies like these, who needs friends?"

The upshot seems to be a cleavage into mainstream and radical camps with little communication between them. This makes it too easy to ignore Gergen's severe malaise with experimentation entirely if one has not enrolled in his radical wing.

Why does Gergen believe that observation cannot support psychological hypotheses? What appears to be responsible is his conception of the relationship, or rather lack of relationship, between psychological description and observable events. Human conduct is described in terms not of spatiotemporal parameters, which keep changing, but of meanings and intended ends—psychological states and dispositions of the behaving person (Gergen, 1982, p. 80f; 1985b, p. 118; 1986a, p. 151; 1987, p. 118). Most social psychologists regard these as inferred from observation, but according to Gergen (1985b, p. 114) there is no valid basis for such inferences. The meaning and motivation of any observable behavior are matters of interpretation, and interpretation is indeterminate. An indefinite number of interpretations are always possible, and we cannot establish one of them as correct. Thus, agreement with the sentence "There is a direct connection between how hard I study and the grades I get" may reflect internal locus of control, or, as Gergen, Hepburn, and Comer Fisher's (1986) subjects demonstrated, such agreement may reflect any number of other possible traits. For example, one of their subjects linked it with shyness by saying, "Such a rationale excuses the shy person from too much socializing and allows him to secrete himself in his room" (p. 1263).

Alternative interpretations cannot be ruled out by further observations, because the further observations themselves will in turn confront the dilemma of indeterminacy of interpretation (Gergen, 1986a, p. 148; 1989, p. 470). For example, if a subject presses a lever that presumptively delivers shocks to another subject, this might represent aggression or might reflect obedience or curiosity or a sense of moral duty, or it might be accidental. Suppose we then ask the subject what he or she was trying to do. Even if the subject says that he or she wanted to harm the other subject, one cannot accept this as the subject's intention without assuming—for which one has no empirical warrant—that the subject is aware of his or her actual motivation and is not prevaricating. One is locked in an infinite interpretive regress.

Convinced that the ascription of psychological states and dispositions cannot be empirically grounded, Gergen (Gergen & Gergen, 1991) concluded that such ascription depends not on empirical evidence but on linguistic convention:

> Whether or not one uses the term "jealousy" does not depend on what is "actually the case," but on local conventions of naming or indexing patterns of events. By the same token, the "determinants" or "antecedents" of jealousy are not read from nature, nor derived logically from observation. (pp. 80–81)

It is part of the very meaning of jealousy that it is caused by seeing the object of one's attraction as attracted to another. This is not an empirical truth of psychology but a nonempirical linguistic formula that follows from conventions of word usage.

Where Gergen Is Wrong

This attack on their basic beliefs and practices has not, it seems to us, received adequate attention from mainstream social psychologists. A great deal of debate did occur, especially in the 1970s, over Gergen's argument against general laws in social psychology (see Jackson, 1988; Jones, 1985, for an example). However, although Gergen's more radical argument that social psychological hypotheses cannot be empirically tested has been deplored (e.g., Stroebe & Kruglanski, 1989), mainstream social psychologists do not seem to have really confronted it—perhaps because of the seemingly obvious fact that experiments were testing such hypotheses. We believe that confronting it will help elucidate when experimentation is and is not useful.

How, then, may Gergen be answered? Mainstream social psychologists would agree that psychological states and processes cannot be directly observed. They would also agree that one can never be certain of psychological interpretations of behavior. The construct validity of operational definitions is never guaranteed. They believe, however, that some interpretations are more likely than others, especially when supported by converging operations, conceptual replication, or the like. When a subject presses a lever that ostensibly delivers shocks to another subject, it may be far from certain that he or she intends to harm this other subject. If the subject also asserts that this was his or her intention, or it happens that on the experimenter's declaration that the experiment is over, the subject proceeds to punch the other subject in the nose, then, all else being equal, it seems likely that harm was intended. Whereas a subject's agreeing that there is a direct connection between how hard he or she studies and his or her grades may reflect shyness or a host of other traits, if the subject also agrees with such statements as learning depends more on the student's studying and less on the teacher's instruction and disagrees with such statements as failure is more likely to be a consequence of lack of ability than of lack of effort, then, all else being equal, it seems likely that this subject believes he or she is personally responsible for his or her academic achievement.

In Gergen's view, however, even assertions of the likelihood of particular psychological processes or dispositions, given particular observations, are not empirically justified. That delivering shocks is likely intended to harm the recipient does not follow from subjects' assertions to that effect unless one assumes that they (probably) know their intentions and are not lying. Nor can this be inferred from the delivery of punches after the shocks unless one assumes that (probably) both the shocks and the punches resulted from intentions in the first place and the punches, too, were intended to harm. A subject's likely belief in his or her personal responsibility for academic achievement can be inferred from his or her pattern of agreements and disagreements only if it is assumed that the subject (probably) has beliefs about such matters as whether there is a direct connection between how hard he or she studies and his or her grades, knows what those beliefs are, and is reporting them truthfully. Because intentions, knowledge, beliefs, and so forth are not themselves observable, so Gergen's argument goes, none of these assumptions can be verified.

Gergen is correct that there are always alternative possible interpretations of any observations and that there is no way to tell even which are likely without making further assumptions. Observation of behavior is alone insufficient to justify psychological interpretations. Without the assumption of other knowl-

edge, one cannot show that a given interpretation is better than others.

Gergen is wrong, however, that this means psychological interpretations cannot be empirically justified. Interpretations often can be warranted on the basis of propositions that, involving interpretations themselves, are not directly verifiable but that most social psychologists would be willing to assume as background knowledge. For example, most social psychologists would, we believe, be willing to assume that people are unlikely to distort the truth (either to others or themselves) unless they expect to gain from doing so, that in most circumstances people do not expect to gain by representing themselves as intending a usually disapproved outcome, and that harming another is usually disapproved. Assuming these propositions, it follows that, barring something unusual about the situation or the subjects such that they would expect to gain from distorting the truth, claiming an intention to harm another is not likely to be false.

Gergen would have to say that propositions like these that can ground the psychological interpretation of observations are themselves empirically unwarrantable—that they merely reflect linguistic conventions. Yet (albeit implicitly) they play a crucial role in the prediction and explanation of observation. Note, for example, the difficulty of accounting for how often our predictions are correct when we accept what people say as true if we could not assume them to be unlikely to lie in the absence of expected gain from doing so. Or consider how our accuracy would decline in predicting parents' reactions to seeing their child punch another if we could not assume that harming another is usually disapproved.

That predictive and explanatory power are good reasons for acceptance of propositions that cannot be directly verified is quite generally recognized. Even Thomas Kuhn, often called on by Gergen (1982, 1985c, 1991) as well as by others in support of claims that science is not objective, agreed with this and denied that he ever thought otherwise (see Kuhn, 1970, p. 198f; 1977, p. 321f). Acceptance of propositions, on the basis of their contribution to prediction and explanation, that cannot be directly verified has been essential in the achievements of the natural sciences—for which Gergen (Gergen & Semin, 1990) admitted that social constructionists "have failed as yet to render compelling accounts" (p. 16). There seems no reason why psychologists should not also be able to accept, on the grounds of their explanatory and predictive power, propositions that are not directly verifiable. Why should direct verifiability be required in social psychology, if physics gets along so well without it?

The Problem That Remains

Contrary to Gergen's claims, then, psychological interpretation can be empirically grounded, and social psychological propositions need not merely reflect linguistic conventions. Psychological interpretations, though always fallible, can be justified on the basis of observation, given assumptions that there is good reason to accept. As we demonstrate, however, the problems of psychological interpretation to which Gergen called attention do mean that some social psychological hypotheses are not actually subject to empirical test. In the face of potential evidence against them, the interpretation of the evidence would always be questioned rather than the hypotheses themselves. Although they may appear testable, the hypotheses are immune to empirical disconfirmation. (For an excellent discussion of propositions of this kind, see the philosopher Putnam's classic 1962 article.)

Consider, for example, the proposition that, ceteris paribus, if interest in a target is enhanced, attention to it is likely to increase. No observation would lead one to accept that interest in a target increased, whereas attention to it did not, although nothing else had changed and there were no special circumstances. If this ever seemed to happen, one would look for errors of interpretation: Interest was not really enhanced, the assessment of attention was inaccurate, something else had changed, or some special circumstance was in play, such as, for example, that the increased interest was a young man's interest in a young woman from whom he wished to hide his interest.

That enhancing interest in a target is likely to increase attention to it is not, we submit, a hypothesis subject to empirical test. Evidence that the operational indicators are not related as predicted would always be taken to imply that they lacked construct validity, all other things had not been rendered equal, or special circumstances prevailed. Imagine the following experiment: Undergraduates are told they will play a competitive game against another student in which they will have a chance to win a substantial amount of money. Before playing, each subject is to watch a video screen simultaneously displaying in its four quadrants a silent film clip of the head and shoulders of their future competitor and three other students talking with someone off-screen. Informed that they will be asked some questions later about the clips, the subjects are told the quadrant in which their competitor will appear. Suppose the duration of eye gaze directed at the critical quadrant turns out to be significantly longer than that directed at each of the other quadrants—a result that might but need not occur. The investigator claims to have confirmed the hypothesis that enhancing interest in a target is likely to increase attention to it.

This hypothesis—with the ceteris paribus clause implicit in all hypotheses—could not be disconfirmed no matter what the results. The absence of significant differences could of course readily be accounted for in terms of an insufficiently strong manipulation of interest or an insufficiently sensitive measure of attention. However, even significant results in the opposite direction would not be taken as a disconfirmation. Suppose duration of eye gaze directed at the critical quadrant was significantly shorter, not longer, than that directed at the other quadrants. This evidence would not lead one to doubt the hypothesis but to conclude that there must have been something wrong with the experiment. Perhaps, for example, the subjects did not want to show their interest and thus were trying to disguise it or suspected that the real purpose was to deceive them into ignoring the other quadrants.

We do not believe that any other experiment's results would lead one to doubt the hypothesis, either. That the operational variables have the meanings intended, that all else has been kept equal, and that no special circumstances were in play would, given the inevitable uncertainties of psychological interpretation, always be more questionable than that, if all else is equal, enhancing interest in a target is likely to increase attention to it. This proposition appears to be so well entrenched in the system

of assumptions implicit in social psychological thinking that no observations would suffice to make one deny it.

Propositions of this kind that look testable but are so firmly entrenched that, like tautologies, they cannot be disconfirmed we term *near-tautologies.* Although social psychologists may not always agree that a particular proposition is near-tautological, we believe that there are propositions on which most will agree. Such propositions will be "obvious" ones, but the problem goes beyond that of obviousness or common sense. Although obvious or commonsense hypotheses may sometimes be found to be wrong (see Kelley, 1991), near-tautological hypotheses cannot be.

Certain hypotheses, then (near-tautological ones), are not actually subject to empirical test, and their confirmation per se will serve little purpose. Furthermore, they are not the only hypotheses rendered problematic by this analysis. Hypotheses that are not near-tautologies may nevertheless be derivable from near-tautologies. As we demonstrate, derivable hypotheses, which are less likely to appear obvious, also will be of little interest to confirm as such.

Let us develop this argument by considering an experiment in which the hypothesis may be less obvious than most. J. M. Aronson and Jones (1992) were interested in the attribution of ability, particularly intelligence, to students by teachers or tutors. They proposed that such attribution would be affected both by the performance pattern of the student—whether performance across problems improved or showed a decrement—and by the goal of the teacher—whether the teacher wanted the student to succeed on specific problems at hand ("facilitators") or to learn how to solve problems of that type more generally so that the student could succeed independently on similar problems in the future ("instructors"). Prior research had suggested that subjects observing students trying to solve a series of problems show a robust primacy effect in attributions of intelligence; that is, they attribute higher intelligence to students with more successes earlier rather than later in the series (number of successes held constant). J. M. Aronson and Jones's major hypothesis was that the primacy effect would be weaker for instructors than for facilitators.

In the experiment they conducted, J. M. Aronson and Jones (1992) asked female undergraduates to serve as tutors through an intercom for a second (actually fictitious) subject in another room, who would be their student. The tutors were told that they were to present a series of anagrams for their student to try to solve within 30 s and to provide clues for some of the anagrams by disclosing the position of two of the anagram's letters. One group of tutors (instructors) was encouraged to present clues that would implicitly suggest strategies that would help the student become better at solving such anagrams in general and were told that there would be payment for each anagram the student solved afterward in a second series with no assistance. Another group of tutors (facilitators) was encouraged to present clues that would be particularly useful for the given anagrams and were told that there would be payment for each anagram the student solved when the tutor had provided a clue.

The tutor heard the "student's responses" through the intercom. With the total number of anagrams correctly solved held constant, some tutors heard more correct solutions earlier in the series (decreasing success pattern), whereas others heard more correct solutions later in the series (increasing success pattern). They were then asked several questions, particularly to rate their student's intelligence. As predicted, the primacy effect was significantly weaker for the instructors than for the facilitators.

J. M. Aronson and Jones's (1992) hypothesis may be far from obvious; one would not expect many grandmothers to come up with it. Nonetheless, it seems possible to derive it from near-tautologies, if one assumes that there is not another effect of being an instructor versus a facilitator that may counterbalance the hypothesized effect. Just as hypotheses are understood as having an implicit ceteris paribus clause, so should the propositions in the derivation be understood. (For example, that higher intelligence tends to be attributed to students to whom more learning is attributed [see Proposition 7] need not hold if other things, such as initial knowledge, are not equal.) Some of what are called near-tautologies might be viewed as true tautologies; this would not affect the derivation. The derivation follows:

Near-tautology:
1. A student's pattern of increasing success is potential evidence that the student is learning.

Near-tautology:
2. Individuals who more strongly want something to occur tend to be more aware of potential evidence of its occurrence.

By definition:
3. Instructors more strongly want students to learn than do facilitators.

Therefore, from Propositions 1, 2, and 3,
4. Instructors will tend to be more aware of students' patterns of increasing success than will facilitators.

Near-tautology:
5. Individuals will be more likely to attribute learning to a student when they are more aware of potential evidence of learning.

Therefore, from Propositions 1, 4, and 5,
6. Instructors will tend to attribute more learning to students with patterns of increasing success than will facilitators.

Near-tautology:
7. Higher intelligence tends to be attributed to students to whom more learning is attributed.

Therefore, from Propositions 6 and 7,
8. Instructors will tend to attribute higher intelligence to students with patterns of increasing success than will facilitators.

Therefore, from Proposition 8,
9. The primacy effect—attributing higher intelligence to students whose pattern of success is decreasing rather than increasing—will tend to be weaker for instructors than for facilitators.

If we are correct that J. M. Aronson and Jones's (1992) hypothesis can be derived from near-tautologies assuming the absence of a counterbalancing effect, then their experiment does little for this hypothesis. Once the derivability is recognized, one would already be so confident of the hypothesis unless counterbalancing seems a possibility, that any negative results would lead one to question not the hypothesis but the experiment. However, if counterbalancing effects do seem a possibility, then one would need to consider what they might be and to determine whether they are in fact able to cancel the hypothesized effect. Just documenting that the hypothesized effect occurs does not accomplish much, because one can deduce from near-tautologies that it will occur unless canceled by a counterbalancing effect.

A possible counterbalancing effect can in fact be envisioned in the J. M. Aronson and Jones (1992) case, and the authors mentioned it: Instructors might, as suggested by Kelley's (1971) discounting principle, be more likely than facilitators to attribute evidence of students' learning to instructional efforts and less likely to attribute it to students' intelligence. Yet Aronson and Jones's experiment does not argue against the possibility of this effect canceling the hypothesized effect. It only tells us that the hypothesized effect was not canceled under a particular set of operational circumstances that were not designed to see whether it could be. Had the instructors been able to expect that their efforts would make more of a difference to their students' learning useful strategies and thus getting better at solving anagrams in general, the findings might have been reversed.

Thus, a hypothesis, whether obvious or not, may be derivable from near-tautologies in the absence of a counterbalancing effect, and then its confirmation per se will be of little interest. If counterbalancing seems possible, then one needs to see whether it can occur. However, if one can assume the absence of a counterbalancing effect, then the hypothesis follows from near-tautologies.

Analyses of Hypotheses in Runs of Journal Articles

That a hypothesis may follow from near-tautologies in the absence of a counterbalancing effect might not be so serious a problem if it was only an infrequent occurrence. We believe, however, that enough of the hypotheses in current mainstream social psychology experiments may be derivable that the problem deserves attention. To explore the issue further, we attempted derivations of hypotheses in runs of recent articles in the two top mainstream social psychology journals, *JPSP* and *JESP*. Using the top mainstream journals seemed desirable to give a sense of what might be encountered in the best judged instances of work in the field. Using runs of articles, that is, all articles within a given time period were considered, seemed desirable to give a sense of what might be found on reading the contents of entire journal issues. Finally, using recent articles seemed desirable because practices in current work appeared to be of greater interest than practices in older work.

There is no algorithm for determining whether a hypothesis follows from near-tautologies in the absence of a counterbalancing effect. Our procedure essentially was to look for near-tautological background knowledge that might lead one to expect the hypothesis to be true. Some of the derivations were easy to arrive at; others were difficult. We could not always be sure that a hypothesis would be derivable until we had succeeded in deriving it. Once a derivation was carried out, however, it seemed reasonably clear. This is not to deny that there may sometimes be disagreement as to whether a particular proposition is near-tautological.

Our first run comprised all 14 articles in three successive issues of *JESP* (September and November 1991 and January 1992)—the current issues when we began work on this study. Our second run comprised all 14 articles in the *Attitudes and Social Cognition* and the *Interpersonal Relations and Group Processes* sections of two successive issues of *JPSP* (August and September 1992)—the current issues after we had a first draft of the main body of this article. We consider each run in turn.

JESP Articles

Of the 14 *JESP* articles, 8 had hypotheses (in 7 cases the central hypothesis, in 1 case one of two central hypotheses) that appeared to be derivable from near-tautologies in the absence of a counterbalancing effect. The 8 hypotheses and their derivations follow. Again, all propositions are to be understood as having an implicit ceteris paribus clause, and some of what we call near-tautologies might be considered true tautologies.

Houston, Sherman, and Baker (1991)

The hypothesis was as follows:

> Satisfaction [with a choice an individual has made between two items will] depend upon an interaction between the valence of the unique features of the alternatives and which alternative (the accepted or the rejected) the individual is focused upon. For unique good pairs [good features different, bad features alike], satisfaction [will] be greater when the focus is on the accepted rather than the rejected alternative. For unique bad pairs [bad features different, good features alike], satisfaction [will] be greater when the focus is on the rejected rather than the accepted alternative. (p. 421)

Our derivation was as follows:

> Near-tautology:
> 1. When considering a choice an individual has made between two items, the features that differ between items will be more salient than the features that are alike.
>
> Therefore,
> 2. When the pairs are unique good pairs (pairs in which the items differ in good features and not in bad), good features will be more salient than bad features; when the pairs are unique bad pairs (pairs in which the items differ in bad features and not in good), bad features will be more salient than good features.
>
> Near-tautology:
> 3. When good features are more salient than bad features, satisfaction will be greater when the focus is on the accepted rather than the rejected alternative (one would rather gain good features than lose them); when bad features are more salient than good features, satisfaction will be greater when the focus is on the rejected rather than the accepted alternative (one would rather lose bad features than gain them).

Wilder and Shapiro (1991)

The hypothesis was as follows:

> Cues that reinforce ingroup identity are likely to facilitate the use of related outgroup stereotypes. (p. 451)

Our derivation was as follows:

> Near-tautologies:
> 1. Cues that reinforce in-group identity heighten the likelihood of categorization into in-group and out-group.
>
> 2. Categorization into in-group and out-group is likely to facilitate the use of related out-group stereotypes.

Flink and Park (1991)

The hypothesis was as follows:

> When judges are rating traits of target persons, dependence on

those target persons for outcomes, known as "outcome dependency," is likely to "increase consensus." (p. 457)

Our derivation was as follows:

Near-tautologies:

1. Dependence on a target person for outcomes is likely to increase interest in the target's traits.

2. Increased interest in a target's traits is likely to increase attention to trait-relevant information.

3. Increased attention to trait-relevant information is likely to increase accuracy of trait ratings.

4. Increased accuracy of trait ratings is likely to increase consensus.

Erber (1991)

The hypothesis was as follows:

When a person can be described by both a positive and a negative trait, a perceiver's mood [will] influence which trait category is accessed for subsequent inferences about the person's behavior. [If the perceiver is in a positive mood], the person [will] be perceived as more likely to engage in behaviors implied by the positive trait category. Similar predictions can be made about the effects of negative mood. (p. 480)

Our derivation was as follows:

Near-tautology:

1. When someone is in a positive mood, he or she is more likely to think of positive than of negative things.

Therefore,

2. When someone is in a positive mood, he or she is more likely to think of positive than of negative traits of another person.

Near-tautology:

3. When someone thinks of another's positive rather than negative traits, that other will be perceived as more likely to engage in behaviors implied by the positive traits.

The same reasoning applies to negative moods.

McGill (1991)

The hypothesis was as follows:

People will [tend to] provide more complex explanations [explanations of an event in a target episode based on a greater number of features or causal factors] the greater the number of [dissimilar] causal backgrounds [contexts against which the target episode is considered where the event did not occur, that is, negative instances] presented. (p. 544)

(Although "dissimilar" is not specified in the statement of the hypothesis, it is clear from the rest of the article that it is intended.)

Our derivation was as follows:

Near-tautologies:

1. The greater the number of dissimilar causal backgrounds presented, the greater the number likely to be considered.

2. The greater the number of dissimilar causal backgrounds considered, the greater the number of causal factors likely to be necessary to distinguish the target episode from all of these backgrounds (they are negative instances that differ from one another).

3. People will tend to provide explanations of an event in a target episode on the basis of a greater number of causal factors, the greater the number of causal factors necessary to distinguish the target episode from all of the contexts against which it is considered where the event did not occur.

Lord, Desforges, Ramsey, Trezza, and Lepper (1991)

The hypothesis was as follows:

Individuals who are relatively expert at dealing with [a] category display less of [the] typicality effect than do relative nonexperts. (p. 550)

That is, it will be less true of experts than of nonexperts that "general category attitudes are . . . more likely to guide behavior toward typical than toward atypical social category members" (p. 550). (Presumably, experts and nonexperts both are able to recognize typicality, whereas experts know more than nonexperts about the members of the category.)

Our derivation was as follows:

Near-tautologies:

1. An individual's general attitude toward a social category is more likely to guide behavior toward a member of that category, the more the individual attributes characteristics to the member that are the basis for the individual's general category attitude.

2. Individuals who are experts are more likely than nonexperts to realize that typical category members may lack, and atypical members may possess, the characteristics that are the basis for the individual's general category attitude.

Therefore, from Proposition 2,

3. Whether a category member is typical or not is less likely to affect whether experts than nonexperts attribute characteristics to the member that are the basis for the general category attitude.

Therefore, from Propositions 1 and 3,

4. Whether a category member is typical or not is less likely to affect the extent to which the general category attitude of experts than of nonexperts governs their behavior toward the member.

Allison, McQueen, and Schaerfl (1992)

The hypothesis was as follows:

Group members will be more likely to use the prominent social decision making rule of "divide equally" when they share resources that are partitioned into equal units [and the partitioned resources can be easily divided into the number of equal portions required for each member to receive one] than when they share nonpartitioned resources. (p. 23)

(Although such easy division is not specified in the statement of the hypothesis, it is clear from the rest of the article that it is intended.)

Our derivation was as follows:

Near-tautologies:

1. People will be more likely to use a rule when that rule requires less effort than when it requires more effort.

2. When resources are partitioned in such a way that they can be easily divided into the number of equal portions required for each member to receive one, the rule of "divide equally" requires less effort than when resources are nonpartitioned.

Schaller (1992)

The hypothesis was as follows:

> [In making inferences about individuals from their behavior, people will be more likely] to aggregate across situations rather than to take into account situational constraints [if they have only sparse data than if they have large data sets]. (p. 68)

(For example, people might know [sparse data set] that Mary won 2 out of 8 of her games in League X and won 2 out of 2 games in League Y, whereas Veronica won 0 out of 2 of her games in League X and won 6 out of 8 games in League Y. Or people might know [large data set] that Mary won 20 out of 80 games in League X and won 20 out of 20 games in League Y, whereas Veronica won 0 out of 20 games in League X and 60 out of 80 games in League Y. According to the hypothesis, with the sparse data set people are more likely than with the large data set to aggregate games and ignore league and thus to infer that Veronica is the better player.)

Our derivation was as follows:

> Near-tautologies:
> 1. People are less likely to take situational constraints into account when their effect is less clearly shown.
>
> 2. The effect of situational constraints on behavior is less clearly shown by sparse data sets than by large ones.

JPSP Articles

Of the 14 *JPSP* articles, 4 have hypotheses (in 3 cases the central hypothesis, in 1 case one of two central hypotheses) that appear to be derivable from near-tautologies in the absence of a counterbalancing effect. Thus, 4 of 14 *JPSP* articles, as well as 8 of 14 *JESP* articles, had derivable major hypotheses. The 4 *JPSP* hypotheses and their derivations follow. Once more, all propositions are to be understood as having an implicit ceteris paribus clause, and some of what we call near-tautologies might be considered true tautologies.

Roskos-Ewoldsen and Fazio (1992)

The hypothesis was as follows:

> Objects toward which individuals hold attitudes that are highly accessible . . . are more likely to attract attention when presented in a visual display than objects involving less accessible attitudes. (p. 198) [Objects toward which individuals hold attitudes that are highly accessible are understood to be objects for which] the association between the . . . object and the evaluation of the object is sufficiently strong that the evaluation is capable of being activated automatically from memory on mere observation of the . . . object. (p. 199)

Our derivation was as follows:

> Near-tautologies:
> 1. Objects for which the association between the object and its evaluation is sufficiently strong that the evaluation is capable of being activated automatically from memory on mere observation of the object are likely to be objects that matter more to individuals than objects for which the association is less strong.
>
> 2. When objects matter more to individuals, the objects are more likely to be noticed than when they matter less.

Dunning and Cohen (1992)

The hypothesis was as follows:

> When making judgments of others, high-performing Ss [who are aware of their own scores] tend . . . to rate target performances [scores] less favorably than . . . low-performing Ss. (p. 341)

(Although the authors did not state that the subjects are to rate scores rather than actual performances and to be aware of their own scores, the experiments were clearly designed so that this would be the case.)

Our derivation was as follows:

> Near-tautologies:
> 1. A person will tend to rate a score more favorably, the better it is relative to another score of which the person is aware.
>
> 2. A given score will be better relative to a second score when that second score is low than when it is high (where high is better than low).
>
> Therefore, from Propositions 1 and 2,
> 3. A person will tend to rate a score more favorably when another score of which the person is aware is low than when it is high.
>
> Therefore, from Proposition 3,
> 4. A person will tend to rate a score (which can but need not be another person's) less favorably when another score of which the person is aware (which can but need not be the person's own) is high than when it is low.

Ford and Stangor (1992)

The hypothesis was as follows:

> Group stereotypes, as assessed by strength of association, are more likely to be based on attribute dimensions for which the difference between group means is large than on dimensions for which the mean difference is small (p. 358) [and to be based] on attribute dimensions for which within-group variability is low than on ones for which within-group variability is higher. (p. 364)

Our derivation was as follows:

> Near-tautologies:
> 1. A stronger association will tend to be formed between an attribute and a group when the attribute is more characteristic of the group.
>
> 2. An attribute will be more characteristic of a group when the attribute differentiates this group more clearly from other groups.
>
> 3. An attribute will differentiate a group more clearly from other groups when the mean of this group differs more on that attribute dimension from the means of other groups and when within-group variability on that attribute dimension is low rather than high.
>
> Therefore,
> 4. A stronger association will tend to be formed between an attribute and a group on those attribute dimensions where the mean of the group differs more from the means of other groups and where within-group variability is low rather than high.

Stasser and Stewart (1992)

The hypothesis was as follows:

> Groups [will] be less prone to overlooking unshared information if

they believe that their task has a demonstrably correct answer. (p. 426)

Our derivation was as follows:

Near-tautologies:

1. If groups believe that their task has a demonstrably correct answer, they will be likely to believe that sufficient information exists to demonstrate the correct answer.

2. Groups will be more likely to look for sufficient information to demonstrate a correct answer if they believe that such information exists.

3. Groups will be less likely to overlook information (shared or unshared) if they are looking for sufficient information to demonstrate the correct answer.

Discussion and Conclusion

Central hypotheses in 12 of the 28 articles in our analysis thus appear to be derivable from near-tautologies in the absence of a counterbalancing effect, which suggests that the problem of derivability warrants serious attention. We have argued that hypotheses of this kind are not useful to confirm as such. Because one can deduce from near-tautologies that the hypothesized effect will occur unless canceled by a counterbalancing effect, an experiment that simply documents the hypothesized effect serves little purpose.

This does not mean that experiments confirming hypotheses that are derivable from near-tautologies cannot be of value. They can be useful if the particular conditions of the experiment have a function beyond just serving as an operationalization of the derivable hypothesis. One reason the occurrence of a hypothesized effect under the particular conditions of an experiment might be interesting is because the independent variable is a minimal manipulation (see Prentice & Miller, 1992). Wilder and Shapiro's (1991) experiments, for example, showed that the mere presence of two fellow students from the same university ("cues that reinforce ingroup identity")—a minimal manipulation—can be sufficient to facilitate a student's use of stereotypes of students from a different university. One of Erber's (1991) experiments showed that merely holding a rolled-up paper towel tightly between one's teeth, which caused facial expressions simulating those typically associated with anger and was intended to put subjects in a negative mood—another minimal manipulation—can lead subjects to perceive another person as more likely to engage in behaviors implied by negative rather than positive traits.

These are interesting findings. However, they are not of interest as confirmations of the hypotheses that the experiments were designed to test and are claimed to support. The hypotheses, as shown in our derivations of them, follow from near-tautologies in the absence of counterbalancing effects, and experiments could only increase confidence in them if possible counterbalancing effects were shown unable to cancel the hypothesized effects. Rather, the findings are of interest because they demonstrate the efficacy of manipulations that might not have been expected to suffice.

Experiments may be explicitly designed for the purpose of such demonstrations. As one example, Arkes, Boehm, and Xu (1991), in one of the articles in our analysis that did not prove to have derivable hypotheses, described experiments that had the express purpose of exploring boundary conditions for the validity effect—the increase in a statement's judged validity that has been found to result from its repetition. One of their experiments, for instance, showed that the effect could be produced even if, rather than repeating the statement whose validity was to be judged, a different statement only slightly related to that one was presented instead. It could not have been known beforehand that the validity effect would be obtainable with this minimal a manipulation.

Another reason the occurrence of an effect under the particular conditions of an experiment can be interesting is because the dependent variable is one that is difficult to influence (see again Prentice & Miller, 1992). Mackie, Allison, Worth, and Asuncion (1992), in another example from our analysis, had the nonderivable hypothesis that a certain kind of information about the performances of individuals belonging to a given group could counter stereotypical judgments not only of the individuals in question but also of the group to which they belonged. Finding that it could, they succeeded in demonstrating stereotype reduction, an effect that one might expect to be difficult to obtain.

Some of social psychology's most impressive experiments were impressive precisely because they showed that certain effects could be obtained on very resistant dependent variables. Asch's (1952) conformity experiments, for example, were designed to test whether group pressure could lead individuals to make incorrect judgments of clear perceptual stimuli, a resistant dependent variable. His results were so striking not because they showed that group pressure tends to produce conformity—a relation that can be derived from near-tautologies in the absence of counterbalancing effects—but because they showed that conformity from group pressure can occur even when exhibiting conformity requires the subject to make assertions that are obviously incorrect. Another example is the experiment Milgram (1974) designed to test whether obedience to the authority of an experimenter can extend to the performance of acts that apparently are harmful and dangerous to another, a resistant dependent variable. His results were so arresting not because they supported the hypothesis that authority tends to be obeyed (again derivable) but because they showed the extreme lengths to which such obedience can go.

One set of reasons an experiment may be of interest, regardless of whether it deals with a relationship derivable from near-tautologies, is because it demonstrates an effect under particular conditions in which this effect might not have been expected, for example, a minimal manipulation, a resistant dependent variable, or other circumstances that make it unlikely. Such an experiment informs one that the range of conditions under which the effect can be obtained is greater than one might have thought.

Another type of reason an experiment may be of interest, regardless of whether it deals with a derivable relationship, is because it shows that the range of conditions under which an effect can be obtained includes conditions of practical significance. For example, it would seem to be derivable that "persistence in the face of failure is enhanced when attributions for failure are changed from low ability to lack of effort" (Weiner, 1985, p. 567). Ceteris paribus, that one is less likely to persist when one believes one has less chance of success and that one will tend to

believe that one has less chance of success when one attributes one's prior failure to one's low ability more than to one's lack of effort both appear to be near-tautologies. Merely documenting empirically the relationship Weiner described thus seems of little interest. However, it is of direct practical significance to study whether mentally retarded individuals with motivational problems can be led to greater persistence in the face of failure by attributional training (see Zoeller, 1980).

An experiment need not, of course, deal with derivable relationships at all. We see no way, for example, to derive from near-tautologies a relationship between the ability to judge another person's feelings and the degree of covariation over time between that person's physiological states and those of the judge. That such a relationship exists was the hypothesis of Levenson and Ruef (1992) in one of the articles in our analyses. As another example from our study, we see no way to derive that, controlling for actual differences in memory accuracy with age, children's reports about having been touched would be judged less credible than adults' reports. Leippe, Manion, and Romanczyk's (1992) evidence of this not only supports an apparently nonderivable relationship but also seems significant for the practical issue of response to child testimony in judicial proceedings regarding abuse.

Experiments, then, are of value even when they deal with relationships that can be derived from near-tautologies, if they demonstrate effects under conditions in which the effects might not have been expected or under conditions of practical significance. In addition, experiments need not deal with derivable relationships at all. However, an experiment that demonstrates a derivable relationship under conditions for which the only function is to provide a means of operationalizing that relationship will serve little purpose.

In sum, Gergen (1982) is wrong that the ambiguities of psychological interpretation mean that social psychological propositions are not empirical and can never be supported by experiments. These ambiguities do mean, however, that relationships between some psychological constructs may be sufficiently entrenched in the system of assumptions implicit in social psychological thinking as to be impossible to disconfirm, that is, may constitute near-tautologies, and the experimental demonstration of near-tautologies, or of relationships that follow from near-tautologies in the absence of counterbalancing effects, is of little interest. Social psychologists, we believe, need to become more aware of the system of assumptions implicit in their thinking and to discontinue the practice of designing experiments to demonstrate what are in fact near-tautologies or relationships derivable from them. We hope that the kinds of derivations illustrated in this article will help toward that end.

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