Journal of Personality and Social Psychology
1995, Vol. 69, No. 4, 611-618

0022-3514/95/$3.00

"What Kinds of Social Psychology Experiments Are of Value to Perform?" Comment on Wallach and Wallach (1994)

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L. Wallach and M. A. Wallach (1994) argued that many hypotheses tested by social psychologists are either "near-tautologies" or derivable from "near-tautologies" and thus are of little interest. The authors of this article applaud their concern but find that their conclusions are based on flawed analyses and arguments. Their conceptualization of "near-tautology" is problematic. Their analysis is based on a misconceived notion of falsifiability and is inattentive to the social context within which scientific knowledge is accumulated. These problems undermine their efforts to offer a careful analysis of social psychological hypotheses. Most alarmingly, their flawed arguments imply a dangerously narrow prescription as to "what kinds of social psychology experiments are of value to perform."

> Science is an essentially anarchic enterprise: Theoretical anarchism is more humanitarian and more likely to encourage progress than its law-and-order alternatives. (Feyerabend, 1975, p. 17)

In the spring of 1919, a group of scientists journeyed to the West African island of Principe to observe a solar eclipse. After years waiting for this celestial event to occur, the physics community prepared to take measurements of distant galactic lights as they passed the sun, and so to test empirically Albert Einstein's theory of general relativity. If supported, the landmark finding would challenge 250 years of Newtonian physics and powerfully alter the way scientists and laypeople alike viewed their universe. Einstein was not part of the group in Principe. He was not overly concerned about the results of this test. Indeed, in a letter to a colleague years earlier, Einstein wrote, "Now I am fully satisfied, and I no longer doubt the correctness of the whole system, whether the observation of the eclipse succeeds or not. The sense of the thing is too evident" (Clark, 1971, p. 175). On learning that the eclipse results supported his hypotheses, he was unmoved and noted simply, "But I knew that the theory is correct." When further pressed about how he would have reacted had the results shown otherwise, he responded, "Then I would have been sorry for the dear Lord—the theory *is* correct" (Clark, 1971, p. 230).

It is with this anecdote in mind that we reflect on the opinions expressed by Wallach and Wallach (1994). Wallach and Wallach discussed the nature of social psychological hypotheses and the manner in which psychologists respond to the empirical results of experiments designed to test those hypotheses. In doing so, they offered some provocative opinions about what makes a hypothesis interesting, important, or worthwhile to test. They argued that, in social psychology, "hypotheses derivable from propositions very much like tautologies may not be infrequent" and that "their confirmation as such is of little interest" (Abstract; see also p. 241). In support of this argument, they reviewed 28 articles published in leading social psychology journals and explained in detail how 12 of these articles tested hypotheses that appeared to be derivable from "near-tautologies."

Wallach and Wallach's (1994) analysis implies that a large number of studies reported in our major journals test hypotheses that are so obvious or trivial that it is simply not worth demonstrating them. This is a sobering conclusion. Before accepting that conclusion, however, careful critical consideration is warranted. We believe that there are serious missteps in the logic underlying the argument and in the manner in which Wallach and Wallach demonstrated support for it. These considerations suggest that their conclusions are misguided.[1]

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We are indebted to Kenneth Gergen and Tony Greenwald for their perceptive comments on an earlier version of this article.

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[1] The focus on "near-tautologies" is not the whole of Wallach and Wallach's article. They also critically discussed Gergen's (1982, 1988) thesis that social psychology is essentially nonempirical. We generally agree with their appraisal of "Gergen's challenge;" our concerns are specific to Wallach and Wallach's focus on "near-tautological" hypotheses.

How Does One Identify Hypotheses as "Near-Tautologies"?

The problems begin with Wallach and Wallach's (1994) conceptualization of "near-tautological" hypotheses. This concept is apparently descendent from Gergen's (1988) thesis that the intelligibility of any psychological proposition "is largely a by-product of tautology" (p. 45). It become clear, however, that unlike true tautologies, "near-tautologies" are defined not by their own logical properties but by the subjective judgments of an observer. In attempting to define the concept of "near-tautology," Wallach and Wallach wrote:

> Propositions ... 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. (p. 236)

They also wrote of "near-tautological" hypotheses that, "In the face of potential evidence against them, the interpretation of the evidence would always be questioned rather than the hypotheses themselves" (p. 235).

To illustrate this assertion, Wallach and Wallach (1994) offered the example of a scientist testing the hypothesis that interest increases attention. In the face of disconfirming evidence, they argued, the scientist is likely to doubt the particulars of the experimental methods and apparatus but not doubt the veracity of the hypothesis. The hypothesis, they claimed, is not subject to empirical test because it "could not be disconfirmed no matter what the results" (p. 235).

This assertion is remarkable, and it is wrong in its assumptions about the formal logic of disconfirmation and about the informal logic of scientists.

The Logic and Psycho-Logic of Falsification

"Except for operationism," wrote Hull (1988, p. 342), "no other philosophical doctrine has been so thoroughly misunderstood or caused more damage in science than falsifiability." Indeed, it appears that Wallach and Wallach's argument falls prey to a number of misunderstandings.

First and most generally, Wallach and Wallach (1994) appear to use falsifiability as the single sine qua non of scientific value. Falsifiability does occupy a justifiably important role in the evaluation of hypotheses as scientific. Few (if any) contemporary philosophers of science, however, judge the value of hypotheses solely on the basis of falsifiability or allow falsification to occupy the singular governing role in the accumulation of scientific knowledge (cf. Hull, 1988; Lakatos, 1970; Laudan, 1977; Popper, 1972).

More specific problems emerge in Wallach and Wallach's (1994) perspective on the process of falsification. At the very least, Wallach and Wallach's argument misses the valuable distinction between *empirical disconfirmation* and *conceptual disconfirmation* (e.g., Greenwald & Ronis, 1981). They wrote: "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" (p. 235). Not so. If the evidence is inconsistent with the predictions of the hypothesis, the hypothesis has been empirically disconfirmed. It remains to the judgment of the scientist to determine whether the hypothesis has been *conceptually* disconfirmed.

What judgment might the scientist make? Wallach and Wallach (1994) offered a simple response to a matter of considerable complexity. Any empirical observation tests a network of hypotheses. At the very least, the empirical observation tests not only a conceptual model but also a measurement model. Recognizing that there are these inevitable ambiguities, philosophers of science (e.g., Duhem, 1954; Neurath, 1935; Quine, 1953) have argued that one cannot decide rationally which of the several underlying models or hypotheses should be abandoned in the face of an anomalous result. It is not more rational or "right" to abandon the conceptual hypothesis than to question the instruments or means through which the observations were obtained. Scientists wrestle with this conundrum whenever they are faced with an empirical disconfirmation, and philosophers of science have offered numerous perspectives on how the dilemma ought to be resolved. Contemporary perspectives suggest that an empirical disconfirmation raises questions about conceptual hypotheses as well as measurement models. Laudan (1977, p. 44) wrote that "whenever a complex of theories generates an anomaly, that anomaly counts against *each* element within the complex." Similarly, Hull (1988, p. 281) noted that

> Scientists need not abandon their most fundamental views in the face of a single apparent counterinstance, but they cannot totally ignore data either. ... If through time enough phenomena turn out to be sufficiently recalcitrant, [a scientist] might well be led to abandon the theory altogether.

Wallach and Wallach's perspective overlooks the cumulative nature of scientific investigation and the crucial impact of aggregate evidence on falsification.

Their perspective also overlooks the communal nature of scientific investigation. Falsification is based not solely on the judgment of a single scientist examining empirical evidence but on the often-differing judgments of all scientists who are aware of that evidence. Individual scientists may be protective of their hypotheses and cling to them in the face of repeated empirical failures to support those hypotheses, but the scientific community at large will rarely be so single-minded. As Hull (1988, pp. 2–3) noted, "The objectivity that matters so much in science is not primarily a characteristic of individual scientists, but of scientific communities."

Finally, Wallach and Wallach's (1994) statements blur the crucial distinction between the judgments of scientists and the logical bases of the hypotheses that scientists test. It is not true that, merely because scientists fail to consider a hypothesis wrong, the hypothesis is logically nondisconfirmable. As Hull (1988) reminded us, "Nearly all the views that we tend to dismiss as not being 'scientific' because they are not 'falsifiable' are actually quite falsifiable. The problem is that their proponents are not interested" (p. 281).[2] Wallach and Wallach erred in equating the subjective philosophical and psychological bases of scientific decision making with logical weakness in the hypotheses being tested.

By failing to consider these important issues, Wallach and Wallach's (1994) perspective on falsifiability is confusing. Their arguments imply (perhaps unintentionally) two perplexingly different and extreme indictments of social psychologists. On the one hand, Wallach and Wallach indicated that there is little value in submitting highly plausible and believable hypotheses to empirical disconfirmation in the first place. That is, if the hypothesis follows logically from a set of previous observations, then the hypothesis might simply be accepted as true in the absence of direct empirical support. On the other hand, if such hypotheses are tested, Wallach and Wallach appear to indict social psychologists for continuing to believe these hypotheses in the face of empirical disconfirmation. This implies that, when empirically disconfirmed, highly plausible and believable hypotheses should be rejected as quickly and easily as implausible and tenuous hypotheses. That is, a single disconfirmation of a highly plausible ("near-tautological") hypothesis should be accorded the same evidentiary value as a single disconfirmation of a highly implausible or unlikely hypothesis.

Neither perspective makes good sense for science in general, and certainly not for a progressive science of social psychology. Social psychology is necessarily Bayesian in practice—the prior probability of a conceptual hypothesis receiving empirical support or disconfirmation importantly influences our interpretation of the empirical outcome. Yet the prior probability of the hypothesis cannot be assumed to be 1.0. Scientists' confidence must inevitably play some role in the acceptance or rejection of a hypothesis, but that confidence cannot preempt scientific investigation into the hypothesis.[3]

The Social Construction of "Truth"

If it is impossible on purely logical grounds to claim that any given hypothesis cannot be disconfirmed, determination of "near-tautology" necessarily rests on subjective judgments as to how obvious or firmly entrenched a hypothesis might be. A tremendous amount of thinking in philosophy, sociology, and psychology reveals that the extent to which a particular proposition appears to be "obvious" may reveal less about the proposition than it does about the perceiver.

It would be vain to attempt to summarize briefly the breadth and depth of inquiry into the subjective nature of knowledge and truth. This inquiry has a rich history in philosophy (e.g., Husserl, 1962; Kierkegaard, 1846/1968) and in science (e.g., Bar-Tal & Kruglanski, 1988; Polanyi, 1958; Stark, 1977; Weimer, 1979). Gergen (1988), for instance, suggested that we view knowledge "not as a possession of individual minds . . . but as an artifact of social communities" (p. 45). Indeed, it appears that Wallach and Wallach's (1994) uneasiness with social psychological hypotheses stems in part from the recognition of the subjective element in the acquisition of knowledge; however, whereas they suggest that this element results in "near-tautological" propositions, we argue that it renders the concept of "near-tautology" meaningless.

What appears to be "obvious" or "commonsense" depends to a great degree on numerous psychological factors and processes that influence the appraisal of all hypotheses. These psychological influences include chronic or situation-specific motives (Kruglanski, 1989; see especially chapter 10), context and construct accessibility (Higgins & Stangor, 1988), and hindsight bias (Hawkins & Hastie, 1990). Perhaps most fundamentally, apparent "obviousness" must be dependent on one's familiarity with the relevant phenomenon. That is, what can be defined as a "near-tautology" depends on background knowledge and depth of experience with the state of research and theory in a particular domain.

One of us is reminded of his embarrassment, early in graduate school, following his blithe assertion that "the mind is in the brain." Unfortunately, his audience for this claim was a graduate class in philosophy, which promptly (and with more glee than he was comfortable with) informed him of the folly of his belief. What appeared to him to be a basic, fundamental, even trivial assumption turned out to be highly questionable and worthy of significant attention.

Experiences of this sort are common when people from different disciplines or subdisciplines interact. These experiences illustrate Polanyi's (1958) account of scientific progress, which argues that much of the business of science (both in theorizing and in designing theoretical tests) remains tacit and dependent on personal knowledge. The carrying out of science relies on understanding that is often unspoken and not directly tested or demonstrated. From this perspective, the definition of a "near-tautology" depends on the state of knowledge of the perceiver and thus is not inherent in the theoretical claim itself. One person's "near-tautology" can be another's obviously false claim.

Thus, there are no logical or objective grounds for defining

[2] Whereas psychologists have tended to be critical of this sort of hypothesis protectionism (e.g., Greenwald, Pratkanis, Leippe, & Baumgardner, 1986; Mahoney, 1976; Tetlock & Levi, 1982; Tweney, Doherty, & Mynatt, 1981), observers who ascribe to more sociological perspectives have been more charitable (e.g., Hull, 1988; Mitroff, 1974). Hull (1988, p. 377), for instance, advanced the position that "Although this pigheadedness often damages the careers of individual scientists, it is beneficial for the manifest goal of science."

[3] It might be illustrative to consider the analogy between a "near-tautological" hypothesis and a computer program. According to Wallach and Wallach, "near-tautological" hypotheses follow logically from established truths or accepted premises. Similarly, a well-written computer program consists of a series of logically connected and seemingly straightforward input statements. Does that mean that the program will necessarily yield the output it was painstakingly programmed to produce? No. Computer scientists recognize that any program that is complex enough to be interesting will almost certainly surprise its programmers. The single best way to find out what a program will do is to run it and see; no logical input-scanning procedure can efficiently predict the outcome. An analogous process is necessary to assess the veracity of scientific hypotheses—especially hypotheses relevant to systems as complex as that of the human psyche in its social environment. No matter how confident a theorist might be in a particular hypothesis, that hypothesis still demands empirical testing. (We thank Tony Greenwald for bringing this analogy to our attention.)

what a "near-tautology" might be. Wallach and Wallach (1994) readily admitted this. In preparing their critical analysis of the articles published in the journals, they wrote:

> 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. (p. 237)

The subjectivity of this exercise might be somewhat less troubling had Wallach and Wallach (1994) offered some evidence of interrater reliability or broader consensus for their judgments. Unfortunately, no such reassurance is offered. In the end, it appears that the determination of "near-tautological" hypotheses was largely the product of Wallach and Wallach's personal background knowledge and expectations. They admit that "there may sometimes be disagreement as to whether a particular proposition is near-tautological" (p. 237). Given the variability in scientists' knowledge, we suggest that disagreement is inevitable (and is probably a good thing).

In summary, we suggest that the concept of "near-tautology" is meaningless. When judged against the criterion of inherent falsifiability, virtually no hypothesis can be deemed "near-tautological." When judged against the criterion of obviousness, virtually all hypotheses might be viewed as "near-tautological"—depending on who is doing the looking and the context within which that viewing occurs. Furthermore, these issues are not specific to social psychology but apply to all natural, physical, and social sciences alike.

By overlooking these important considerations, and by assigning the subjective and pejorative label of "near-tautologies" to social psychological hypotheses, Wallach and Wallach (1994) proceeded to mislead their readers in at least two ways. First, they mischaracterized a number of actual hypotheses. Second, and most troublingly, they offered potentially dangerous prescriptions as to "what kinds of social psychology experiments are of value to perform" (p. 233, Abstract).

Incautious Analyses of Specific Hypotheses

Given the uncertain and highly subjective criteria for classification as "near-tautology," Wallach and Wallach's (1994) analysis of specific social psychological hypotheses is suspect. The propositions that they characterized as "near-tautologies" are empirical questions. Some of these hypotheses are, in fact, exemplars of the sort of disputable assertions that cannot easily be answered simply on the basis of a single test.

For instance, in their analysis of Flink and Park's (1991) article, Wallach and Wallach (1994) identified as a "near-tautology" the proposition that "Increased attention to trait-relevant information is likely to increase accuracy of trait ratings." In fact, as self-evident as that statement appears to be, it is not necessarily true. There are a number of conditions under which increased attention to trait-relevant information may not result in increased accuracy of trait ratings—such as when people have prior expectations, motives, or goals (Kruglanski, 1989; Kunda, 1990; Pyszczynski & Greenberg, 1987). In fact, there is evidence indicating that particular forms of outcome dependency—*the specific manipulation* used by Flink and Park (1991)—can lead to biases and *in*accuracies in trait inference (Klein & Kunda, 1992).

Similarly, concerning an article by Ford and Stangor (1992), Wallach and Wallach (1994) argued that traits strongly differentiating social groups from each other will always be more likely to be used spontaneously to describe these groups (compared with traits that are less differentiating) and that this hypothesis is a "near-tautology." Again, however, this is not necessarily the case; the effect is context dependent. Research by Ford (1993) demonstrated that people who are motivated to view their own group in a positive light are not influenced by the extent to which a trait differentiates their in-group from an out-group, but are influenced primarily by whether that trait is favorable or unfavorable to their in-group. Again, Wallach and Wallach dismissed as "near-tautological" a hypothesis that—for important psychological reasons—is sometimes wrong.

Furthermore, in a number of instances, the propositions offered by Wallach and Wallach (1994) fail to reflect accurately the *psychology* underlying the hypotheses offered by the authors of the articles they analyze. If one does not render the hypothesis accurately, some misrepresentation of the logic underlying the hypothesis inevitably follows.

Consider Wallach and Wallach's (1994) deconstruction of the hypothesis offered by Schaller (1992). Although they accurately paraphrased Schaller's hypothesis, they erred when decomposing it into the two "near-tautological" propositions that (a) "People are less likely to take situational constraints into account when their effect is less clearly shown," and (b) "The effect of situational constraints on behavior is less clearly shown by sparse data sets than by large ones" (p. 239). The second proposition is problematic in two ways. First, it is not tautological: No prior research results support it, nor is it intuitively apparent that it is even true. Second, the proposition fails to convey the psychology implied by Schaller's hypothesis, which accorded important psychological roles to people's appreciation for the law of large numbers and to their desire to aggregate across constraining situations in order to "acquire" sufficient data to make confident judgments. Wallach and Wallach's deconstruction of Schaller's hypothesis does not, by itself, communicate the hypothesized psychological consequences of encountering small sets of data.[4]

These examples highlight the point, discussed above, that any supportable hypothesis can be deconstructed into a series of propositions that appear "near-tautological" to someone. (Further supporting this contention, we had no trouble in transforming several of Wallach and Wallach's (1994) examples of nontautological hypotheses [p. 241] into sets of underlying propositions that seemed "obvious" to at least one of us.) More important, these examples illustrate the point that the necessar-

[4] In fact, that second "near-tautological" proposition identified by Wallach and Wallach might legitimately be offered, with some amplification, as an alternative account of the psychological process underlying Schaller's (1992) results concerning the relation between sample size and judgment. (Although a full consideration of Schaller's data renders this alternative hypothesis unlikely, it is perhaps not entirely ruled out.) Far from identifying a "near-tautological" component of Schaller's hypothesis, Wallach and Wallach may have identified a plausible alternative hypothesis.

ily subjective exercise of classifying hypotheses as "near-tautological" is potentially misleading. In attempting to reduce a hypothesis to a simple set of seemingly obvious propositions, it is all too easy to misunderstand and misrepresent the psychology underlying the actual hypothesis.

What is most troubling, however, is this: By engaging in such an exercise, it is all too easy to dismiss potentially important, controversial, and generative scientific ideas as unworthy of scientific investigation.

The Scientific Value of Seemingly "Near-Tautological" Hypotheses

In discussing what they consider to be "near-tautological" hypotheses, Wallach and Wallach (1994) were harsh in their judgments, not only of scientists who test such hypotheses but also of the scientific value of these hypotheses. They repeatedly claimed that confirmations of these hypotheses are of little interest or serve little value to the science of social psychology. Wallach and Wallach offered no objective defense of this opinion, and it is an opinion worth considering more closely. Are apparently "near-tautological" hypotheses uninteresting? Do they lead to bad—or worse, dull—research? Quite the contrary: Many hypotheses that could certainly be labeled "near-tautological" have been of considerable interest to social psychologists, and by that definition have demonstrated value to the science.

Consider a set of hypotheses from a well-known classic contribution in social psychology: Latané and Darley's (1970) *The Unresponsive Bystander: Why Doesn't He Help?* We quote a series of their hypotheses:

> The process a person must go through if he is to intervene in an emergency requires first that the individual notice the event; second that he define the situation as an emergency; third that he decide that he himself is responsible for taking action; and finally, that he choose a particular course of action to take. (p. 43)

It is difficult on purely logical grounds to dispute the hypotheses that before one will help, a person (a) must notice the situation requiring help and (b) must interpret the situation in such a way that help would be useful. From Wallach and Wallach's (1994) perspective, these hypotheses appear to be "near-tautologies." Conceivably, it would be of little value to conduct experiments designed to confirm those relationships, but the record suggests otherwise. The studies reported by Latané and Darley (1970) have been and continue to be widely cited and well known. More important, their work inspired several generations of further research into altruism and helping behavior. Would this have happened in the absence of attempts to empirically test the "near-tautological" hypotheses? Almost certainly not.

Wallach and Wallach (1994) also underestimated the value of hypotheses that are logically derived from tautologies or "near-tautologies." They wrote:

> 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. (p. 236)

In this statement, Wallach and Wallach (1994) not only dismiss a set of hypotheses, but they also repudiate an important syllogistic process through which hypotheses are developed. Wallach and Wallach's thesis suggests that hypotheses derived by syllogistic logic are of little scientific value—that a syllogistic argument should be considered embarrassing and trivial. Perhaps Wallach and Wallach place a high value only on leaps of faith or inspired acts of speculation; if so, we suspect they may be in a minority. Many scientists admire closely reasoned logical arguments. In fact, many scientists would argue that hypotheses derived through syllogistic reasoning are crucial to the incremental advancement of scientific knowledge, because they highlight connections between previously unconnected phenomena and contribute to a more integrated set of knowledge. Wallach and Wallach did finally concede that there may be some value to drawing previously unnoticed connections (p. 240), but they maintained that such hypotheses are valuable primarily as applications of previously existing knowledge, not as interesting hypotheses in their own right.

Again, however, it is easy to identify hypotheses that have been derived from "near-tautologies" that have proved to be of considerable interest and value within the field of social psychology. Consider, for example, the hypothesis tested by Hamilton and Gifford (1976). These researchers proposed that stereotypic judgments of minority groups can result from the tendency to overestimate the frequency with which two distinctive stimuli occur together. This novel hypothesis was syllogistically derived from a set of relations previously established by definition or research—relations that Wallach and Wallach (1994) might call "near-tautologies":

> 1. Members of minority groups are statistically infrequent.
> 2. Non-normative behaviors are statistically infrequent.
> 3. Statistically infrequent events are distinctive.
> 4. People are more attentive to distinctive stimuli.
> 5. Therefore, from Propositions 1 through 4, people are likely to be especially attentive to events in which members of minority groups engage in non-normative behavior.
> 6. Heightened attention to distinctive stimuli results in greater encoding of that information.
> 7. Greater encoding results in enhanced recall for distinctive stimuli.
> 8. Therefore, from Propositions 1 through 7, compared with events involving members of majority groups or involving normative behaviors, people have exaggerated recall for events in which members of minority groups engage in non-normative behavior.
> 9. Judgments about the frequency for stimuli are influenced by recall for the stimuli.
> 10. Evaluations of groups are influenced by judgments about the frequency with which groups engage in particular types of behaviors (e.g., normative vs. non-normative).
> 11. Therefore, minority groups are, in general, likely to be evaluated more negatively than majority groups.

Each of these propositions is likely to qualify as tautological or "near-tautological" to some set of psychologists (although because the definition of "near-tautology" is dependent on prior knowledge, it is impossible to assert that the propositions would be judged consensually to be "near-tautologies"). As a result, the confirmation of Hamilton and Gifford's (1976) hypothesis would be judged by Wallach and Wallach (1994) to be "of little interest." This has not been the case; Hamilton and Gifford's (1976) article has become something of a classic within the recent literature on stereotyping and prejudice. The article continues to be widely cited (well over 100 citations in the *Social Sciences Citation Index* since 1980), the methodological paradigm they developed continues to be used in the study of stereotype development, and the phenomenon they identified continues to receive considerable empirical scrutiny. In fact, a substantial amount of recent research on this phenomenon has challenged the original, "near-tautological" line of reasoning (e.g., Fiedler, 1991; McGarty, Haslam, Turner, & Oakes, 1993; Smith, 1991). Any objective measure of interest or value appears to contradict the thesis offered by Wallach and Wallach.

The Danger of Rigid Prescription

"Planning inquiry cannot be the subject of prescriptions," observed Cronbach (1986, p. 103). More disconcerting than any of Wallach and Wallach's (1994) specific judgments are the implicit prescriptions that accompany their value judgments. Several prescriptions as to "what kinds of social psychology experiments are of value to perform" seem particularly worrisome.

Wallach and Wallach (1994) implied that insights resulting from syllogistic reasoning are of dubious scientific interest or value. The implicit prescription is that social psychologists ought to study only surprising or counterintuitive phenomena but not to test hypotheses derived carefully from past observations. The prescription overlooks the subjectivity involved in judging some phenomena to be surprising, counterintuitive, or unexpected, and it is potentially dangerous in directing scientists away from the linear logic of scientific discovery. Counterintuitive hypotheses and unexpected findings play an important role in the development of scientific theory, but so do results that support carefully reasoned hypotheses.

Wallach and Wallach's (1994) article also implies that it is better to conduct "crucial" tests between countervailing predictions than to test hypothesized relationships between variables. This prescription ignores the pitfalls and drawbacks associated with such "crucial" experiments (Greenwald, 1975; Greenwald et al., 1986). Moreover, in passing judgment on tests of single hypotheses, it implicitly commands researchers to create potentially awkward alternatives to a clear hypothesis. As an intellectual exercise, this might be valuable, but it also increases the likelihood that scientific manuscripts (and published articles) would be littered with the unnecessary debris of "alternative" hypotheses constructed solely to create the illusion of a countervailing prediction.

Third, and most generally, Wallach and Wallach's (1994) arguments offer a particularly narrow perspective as to what makes a hypothesis interesting or valuable. In singling out falsifiability as the fundamental arbiter of scientific value, they ignore many other defining characteristics of valuable theories and hypotheses. These include ability of the theory or hypothesis to explain phenomena, to solve problems, and to facilitate the discovery of new knowledge (Lakatos, 1970; Laudan, 1977). Moreover, in drawing their conclusions, Wallach and Wallach overlooked one of the considerations that prompted their inquiry: Social psychology hypotheses—like hypotheses in all sciences—are developed and evaluated within a social environment. The value of any hypothesis is importantly determined not only by the features of the hypothesis but also by the characteristics of the scientific community (or communities) within which that hypothesis exists. If scientific knowledge accumulates through a process of epistemological evolution, as many philosophers and scientists have suggested (e.g., Campbell, 1974; Hull, 1988; Popper, 1972), then the engine of scientific progress is fueled by diversity of ideas and methods. Any prescription that places artificial boundaries on the means through which hypotheses are generated is likely to threaten this natural diversity and to undermine progress in science.

To offer a contribution to science and society in general, social psychologists must generate good hypotheses—hypotheses that are testable, disconfirmable, and interesting. One of the hardest things to teach budding social psychologists is how to generate valuable hypotheses, although a number of useful suggestions have been published over the years (Beveridge, 1950; McGuire, 1973, 1983; Nisbett, 1990). The worrisome judgment offered by Wallach and Wallach (1994) is that certain very productive strategies of hypothesis development are not good—or at least are not as good as others. The implicit prescription is that social psychologists should follow a more narrow set of strategies for generating good hypotheses. Unless more compelling arguments can be offered to support this prescription, it may best serve our field to ignore it. The danger of adhering to a narrower set of strategies for hypothesis generation is that we might limit the variety of hypotheses that will be generated, limit the scope of our field of study, and therefore limit the extent to which our field generates interesting and important results.

Social Psychology and Science

Let us return to Einstein and his apparent greater willingness to question "the dear Lord" than to question his theory. Was Einstein supremely arrogant? Not according to his biographers (e.g., Clark, 1971). Instead, he knew he had constructed a wonderfully logical theory—ironically, as Newton had done before him—and the pieces just had to fall into place. Were his hypotheses "near-tautological"? By the criteria offered by Wallach and Wallach (1994), yes. Were these hypotheses problematic, uninteresting, and unworthy of empirical test, as Wallach and Wallach's perspective implies? We think not.

The anecdote illuminates a number of the issues we have discussed. First, it highlights the value of maintaining confidence in conceptual hypotheses even in the face of disconfirmation. Had peripheral assumptions regarding light measurement been inaccurate, or had weather conditions attenuated the precision of the viewing instruments, the eclipse observations might indeed have revealed anomalous results. Given the choice between dismissing the theory and questioning the particular data, it is to the benefit of science that Einstein would choose the latter. Second, the intellectual controversy raised by Einstein's hypotheses points to the difficulty of defining any hypothesis as "near-tautological." The hypotheses were "near-tautological" to Einstein and his supporters, implausible to some, and ridiculous to yet others. Third, it is clear that Einstein's tight syllogistic reasoning was a strength, not a weakness, of his hypothesizing. In the absence of carefully derived logical underpinning, it is unlikely that Einstein's theory would have had the same intellectual impact. Given the skeptical demeanor of science, truly revolutionary scientific ideas are likely to fail quickly unless they are carefully documented to follow logically from well-entrenched propositions. Finally, this anecdote reveals the value of submitting even confidently held, "near-tautological" hypotheses to empirical test. Einstein's unshakable belief in the correctness of his theory in no way meant that experimental tests of the theory served no purpose. Indeed, his hypotheses were sufficiently compelling to motivate many researchers to spend much money and travel long distances to perform such experiments, and these experiments have served science well. Although Einstein might never have doubted his theory, empirical support was necessary so that his confidence could propagate across the broader scientific community.

It is clear that the issues raised by Wallach and Wallach (1994), and explored here, are not limited to social psychology but are played out across the breadth of scientific disciplines. Hypotheses of the sort that Wallach and Wallach dismiss as derivable from "near-tautologies" have served a valuable purpose not only in the sciences of social psychology and physics but also in other sciences. It is illuminating to note that the theory of evolution by natural selection has been repeatedly criticized for being tautological and nonfalsifiable (e.g., Peters, 1976). Convincing rebuttals (Caplan, 1977; Sober, 1984; Stebbins, 1977) have noted that these criticism arise from the "confusion between tautology and a set of logical consequences of accepted premises" (Cockburn, 1991, p. 20). The theory of evolution by natural selection is the latter and, like the theory of general relativity, has proven to be of some enduring scientific interest.

Coda

In the late 1960s, biologist Robert MacArthur submitted a manuscript to the *American Naturalist.* While the manuscript was under review, MacArthur discovered that it contained a mathematical mistake that, once corrected, completely reversed the conclusion. MacArthur rewrote the article, adjusted the conclusion, and resubmitted the corrected manuscript to the editor. Meanwhile, the editor had received a review of the erroneous draft. The reviewer had not noticed the error but had recommended rejection anyway on the grounds that the conclusion—now known to be entirely wrong—was simply too obvious to be of interest. Given the nature of the critique, the editor felt compelled to accept the corrected draft for publication.

In addition to illustrating again the fallibility of "near-tautological" hypotheses, this anecdote (from Fretwell, 1975) reminds us that science is a social process that often proceeds as a conversation. Medewar (1982) conceived of scientific reasoning as "a dialogue between two voices, the one imaginative and the other critical" (p. 46). Hull (1988) wrote that "Science is a conversation with nature, but it is also a conversation with other scientists" (p. 7). There is an implicit division of labor in the scientific process: Individual scientists may feel free to follow their imaginative muse, no matter what form it might take, and remain confident that their fellow scientists will be all too happy to serve as the voice of criticism. The danger in perspectives like that offered by Wallach and Wallach (1994) is that they suggest that each individual scientist must embody all the attributes of the scientific process. If scientists take perspectives like this to heart, it will be all too easy for the voice of the imaginative muse to be muffled by a preemptive chorus of criticism. There is nothing more damaging to creativity or productivity than a preemptively harsh and proscriptive internal editor. Thus, we submit that there can be no single authoritative answer to the question "What kinds of social psychology experiments are of value to perform?" In the long run, social psychology (not to mention the sciences in general and society at large) is likely to benefit from an unrestrictive, eclectic, even anarchic approach to the generation of ideas and hypotheses.

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Received December 12, 1994
Revision received May 17, 1995
Accepted May 20, 1995 ■