Under What Conditions Does Theory Obstruct Research Progress?

Anthony G. Greenwald
Michael R. Leippe

Anthony R. Pratkanis
Michael H. Baumgardner

New address, effective August 1, 1986
Anthony G. Greenwald
Department of Psychology, NI-25
University of Washington
Seattle, WA 98195
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Anthony G. Greenwald
Ohio State University

Michael R. Leippe
Adelphi University

Anthony R. Pratkanis
Carnegie-Mellon University

Michael H. Baumgardner
Burke Marketing Services
Cincinnati, Ohio

Researchers display confirmation bias when they persevere by revising procedures until obtaining a theory-predicted result. This strategy produces findings that are overgeneralized in avoidable ways, and this in turn hinders successful applications. (The 40-year history of an attitude-change phenomenon, the sleeper effect, stands as a case in point.) Confirmation bias is an expectable product of theory-centered research strategies, including both the puzzle-solving activity of T. S. Kuhn's "normal science" and, more surprisingly, K. R. Popper's recommended method of falsification seeking. The alternative strategies of condition seeking (identifying limiting conditions for a known finding) and design (discovering conditions that can produce a previously unobtained result) are result centered; they are directed at producing specified patterns of data rather than at the logically impossible goals of establishing either the truth or falsity of a theory. Result-centered methods are by no means atheoretical. Rather, they oblige resourcefulness in using existing theory and can stimulate novel development of theory.

Imagine looking at a projected photographic image that is so badly focused that identification is impossible. The picture is gradually focused until it is just slightly blurred, at which point you are asked to guess what it is. Without the gradual focusing, you might identify the slightly blurred picture about 75% of the time. However, with prior exposure to the more blurred image, you can correctly identify it only about 25% of the time. Interpreting this finding, both Wyatt and Campbell (1951) and Bruner and Potter (1964) suggested that subjects' preliminary hypotheses, formed on the basis of early, poor data, interfered with effective interpretation of later, better data.

The Wyatt–Campbell and Bruner–Potter findings provide striking illustrations of the pervasive phenomenon of confirmation bias—the tendency for judgments based on new data to be overly consistent with preliminary hypotheses. The range of demonstrations of confirmation bias includes primary effects in impression formation (Asch, 1946; Luchins, 1957) and persuasion (Lund, 1925; Miller & Campbell, 1959; delayed discovery of simple problem solutions (Luchins, 1942; Mynatt, Doherty, & Tweney, 1977; Wason, 1960); expectancy biasing of pupil achievement (Rosenthal & Jacobson, 1968); perseverance of belief in discredited hypotheses (Anderson, 1983; Nisbett & Ross, 1980; Ross, Lepper, & Hubbard, 1975); and selective retrieval of information that confirms one's hypotheses (Snyder & Uranowitz, 1978), one's opinions (Pratkanis, 1984; Ross, McFarland, & Fletcher, 1981), or one's self-concept (Mischel, Ebbesen, & Zeiss, 1976; Swann & Read, 1981). In addition, confirmation bias is manifest in the many effects that can be described in terms of Merton's (1948) concept of self-fulfilling prophecy (Darley & Fazio, 1980; Festinger, Riecken, & Schachter, 1956; Snyder & Swann, 1978). As is indicated by the variety of these illustrations, confirmation bias is a very general phenomenon: One's preliminary hypotheses have a decided advantage in the judgment process.

In contrast with this picture of a widespread human trait of confirmation bias, consider the familiar stereotype of the scientist as an impartial observer whose hypotheses stand or fall according to the blind justice of objective data. This stereotype may have a kernel of truth, but the evidence indicates that scientists, like other humans, frequently operate in confirmation-biased fashion (e.g., Armstrong, 1982; Brush, 1974; Feynman, 1985; Gould, 1981; Kuhn, 1970; Lakatos, 1976; Mahoney, 1977; Mitroff, 1974; Orne, 1969; Popper, 1934/1959; Rosenthal, 1966; Westfall, 1973). The scientist's confirmation bias is the main subject of the present article, in which we (a) document the obstructive potential of researchers' confirmation biases, (b) examine the psychological and social functions that (nevertheless) maintain those biases, and (c) identify research strategies that can remedy these problems of confirmation bias in science.

After a preface note on theory, we give an overview of the history of an empirical phenomenon, the sleeper effect. The authors' involvement in research on the sleeper effect motivated the methodological analysis that follows; that analysis, in turn, provides the basis for research strategy recommendations that are presented and analyzed in the remaining sections.
cure than of one accompanied by an accepting cue or no cue).
 Also, Cook made the startling observation that there really had never been any satisfactory evidence for the (absolute, or proper) sleeper effect; the widely cited Yale studies had demonstrated only the relative sleeper effect. (See Capon & Hulbert, 1973, for a similar assessment of the early sleeper effect literature.)

On encountering Cook’s observations about the inadequacy of the sleeper effect evidence, Gillig and Greenwald (1974) converted their investigation into an attempt to find the (absolute) sleeper effect. The Gillig and Greenwald findings, based on seven separate experimental tests, produced clear evidence for the relative sleeper effect but none for the sleeper effect proper.

Recent History of the Sleeper Effect (1978–1984)

Use of new methods to test the sleeper effect. Perhaps Gillig and Greenwald’s experiments had not found a sleeper effect because their procedures—even though more powerful than those of previous sleeper-effect experiments—were nevertheless not powerful enough. That observation led to some efforts at technical development. Ronis, Baumgardner, Leippe, Cacioppo, and Greenwald (1977; see also Baumgardner, Leippe, Ronis, & Greenwald, 1983) reported an initial series of experiments using a laboratory computer to construct a message-dense environment that made it possible to observe decay of persuasive messages’ impacts within a single laboratory session. Initial applications of this method to the search for the sleeper effect, conducted in 1977, were unequivocally negative. In four replications, the relative sleeper effect was repeatedly found, but there was clearly no absolute sleeper effect. This initial research is reported in more detail as part of a report that includes studies conducted at Ohio State University over a 7-year period (Pratkanis, Greenwald, Leippe, & Baumgardner, 1985).

1978: A reliable sleeper effect reported. Concurrently with the research being done at Ohio State University, Cook and his colleagues had also undertaken a search for the sleeper effect. Their search was based on technical improvements and conceptual analysis that were first described in Cook’s 1971 paper. Cook’s analysis specified the following requirements for a sleeper effect (quoted from Gruder et al., 1978):

(a) a persuasive message [must have] substantial initial impact on attitudes; (b) this change is totally inhibited by a discounting cue; (c) the cue and message are dissociated over time; and (d) the cue and message are dissociated quickly enough so that the message by itself still has some impact when dissociation occurs. (p. 1074)

Gruder et al.’s (1978) study obtained a sleeper effect and, importantly, replicated it. They concluded that when the above conditions are implemented, one obtains a sleeper effect.

However, it remained disturbing that some of the studies that appeared to have satisfied Cook’s conditions for a test of the sleeper effect had not produced the effect. In particular, it was not clear why the seven tests by Gillig and Greenwald or the four initial tests by Pratkanis et al. (1985) did not find a sleeper effect. In trying to solve this puzzle, the present authors’ attention was drawn to an aspect of Gruder et al.’s procedure that differed from most previous sleeper effect studies: Gruder et al. had presented their discounting cue (the information that led subjects to distrust the message) after subjects had read the message. That might have been a critical aspect of their procedure. Accordingly, the present authors conducted further studies with the message-dense procedure, varying whether the discounting cue information was presented before or after the message.

Empirical resolution: Timing of discounting cue is critical. Figure 1 synthesizes into one graph the results of 16 experiments with the message-dense procedure, conducted between 1977 and 1984, that included 26 discounting-cue treatments. (These studies are reported by Pratkanis et al., 1985.) The experiments summarized in Figure 1 tested several procedural variations for their possible impact on the sleeper effect. Only one variation made a difference: whether the discounting cue information came before or after the persuasive message. Of course, there were many differences between the multimessage, one-session, computer-controlled procedure of the studies in Figure 1 and the single-message, two-session procedure with which Gruder et al. (1978) had produced a sleeper effect. Accordingly, conclusive evidence for the importance of timing of source information in producing the sleeper effect might be obtained by replicating Gruder et al.’s procedure with an added condition in which the discounting cue precedes the message. Pratkanis et al. (1985) conducted such a replication, adding to Gruder et al.’s design the critical condition in which the discounting cue preceded the persuasive message. Gratifyingly, Pratkanis et al. replicated Gruder et al.’s sleeper

Figure 1. Summary figure for 16 sleeper-effect experiments (Pratkanis et al., 1985). (The units of the abscissa are activities such as reading messages or responding to opinion items on other topics, which intervened between presentation of a message and assessment of its effect on opinion. The time interval for 10 such units is typically less than 10 min. Each experiment included several or all of the delay intervals between 0 and 10 units. The plotted data are average linear regression slopes, weighted by the number of observations in each experiment in which the condition was used.)
It is possible for the disconfirmation dilemma to be resolved repeatedly by preserving faith in the guiding theory. When the researcher’s faith in the theory cannot be shaken by disconfirming data, it is inappropriate to describe the research strategy as theory testing. Rather, the strategy is effectively one of theory confirming.

A theory-confirming researcher perseveres by modifying procedures until prediction-supporting results are obtained. Particularly if several false starts have occurred, the resulting confirmation may well depend on conditions introduced while modifying procedures in response to initial disconfirmations. However, because no systematic empirical comparison of the evolved (confirming) procedures with earlier (disconfirming) ones has been attempted, the researcher is unlikely to detect the confirmation’s dependence on the evolved details of procedure. Although the conclusions from such research need to be qualified by reference to the tried-and-abandoned procedures, those conclusions are often stated only in the more general terms of the guiding theory. Such conclusions constitute avoidable overgeneralizations.

Examples of (Avoidably?) Overgeneralized Conclusions

Published reports rarely provide the information needed to determine whether a researcher was engaged in theory confirming rather than theory testing. However, in Greenwald’s (1975) survey, many researcher–respondents did report practices that correspond to the theory-confirming approach, such as (a) persevering by modifying procedures when an initial hypothesis test produces a disconfirming result and (b) being reluctant to prepare reports of disconfirming findings for publication. The histories of many research problems show periods during which it appears (at least in the light of hindsight) that researchers labored to produce avoidably overgeneralized conclusions. The following cases, starting with the sleeper effect, come from diverse areas of research in order to make clear that no subdiscipline of psychology has a monopoly on the theory-confirming strategy. (Nor is there any reason to believe that theory-confirming research and avoidably overgeneralized conclusions are confined to psychology; see Brush, 1974, and Feynman, 1985, pp. 340 ff., for examples from physics.)

The sleeper effect. The differential decay interpretation of the sleeper effect could have surfaced as early as 1959, when Miller and Campbell used a similar principle to analyze persistence functions for the impact of a sequence of two opposing persuasive communications. (See Ronis, 1980, for a similar analysis.) As suggested previously, the 25-year delay in reaching the present understanding can be credited to researchers’ collective fixation on attempts to test the dissociation interpretation of discounting cue effects.

The insufficient justification effect. It was predicted from cognitive dissonance theory (Festinger, 1957) that one should like a course of action more if one has chosen to follow it with a small (and seemingly insufficient) inducement rather than a large (and certainly adequate) inducement. After the report of a confirming result by Festinger and Carlsmith (1959), it took about 13 years of very active research controversy (see, e.g., Janis & Gilmore, 1965; J. M. Nunn, 1975; Rosenberg, 1965) to establish severe limiting conditions for this insufficient justification effect (Calder, Ross, & Insko, 1973; Collins & Hoyt, 1972). The 13 years between discovery and establishment of limiting conditions included more concentrated research effort and journal space than have been commanded by any other social psychological phenomenon, before or since. In retrospect, that research was of great value in establishing a new and influential account of the effects of incentives on human motivation and cognition. Nevertheless, there was a high ratio of wasted to effective effort on the insufficient justification problem as (a) researchers formed into several theoretical camps (dissonance, learning theory, self-perception, and impression management); (b) each camp published own-theory-confirming results; (c) many findings remained unpublished and uninterpreted because they did not easily fit with any of these theoretical positions; and (d) consequently the social psychology community was delayed in discovering the limiting conditions of published findings.

The law of effect. Thorndike (e.g., 1933) was the first to claim support for the theory—the law of effect—that rewards have an automatic effect of strengthening the stimulus–response relations on which they are contingent. For several decades the law of effect was widely treated as the basic principle of the psychology

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2 This situation was discussed by Greenwald (1975) as a case of "prejudice against the null hypothesis."

3 It is tempting to credit researchers’ reluctance to prepare reports of disconfirming findings to journal editors’ prejudices against publishing such reports. However, it seems that authors rarely provide the occasion for those prejudices to be exercised.
Should/Can Anything Be Done?

Is the Social System of Research Adequately Self-Correcting?

Perhaps one need not be concerned about researchers’ biases toward confirming their preferred theories. After all, scientific disciplines are social systems; their participants can be expected to have complementary and, therefore, mutually compensating theoretical biases (see Campbell, 1986, for a discussion of this topic).

Unfortunately, however, theoretical competition cannot be counted on to produce empirical resolution. Consider, as an example of a topic for which there is much theoretical controversy but no empirical resolution, the topic of psychic (extrasensory) phenomena. Most laboratory researchers on psychic phenomena are advocates. Not surprisingly, then, the great majority of empirical publications on psychic phenomena report confirming results. More generally, fringe topics—those that are marginal to accepted, established domains—are researched almost exclusively by advocates who are grinding theoretical axes (e.g., speed reading, lie detection, biorhythms, and subliminal influence). Additionally, there is an abundance of nontraditional procedures that are claimed to have therapeutic effect (e.g., proposed techniques for weight control, anxiety reduction, pain relief, or amelioration of drug abuse) for which virtually all research is done by the technique’s small group of proponents.

Even within established, accepted domains of research, greater rewards of research support, career advancement, and public recognition tend to fall to the researcher who investigates and confirms a new prediction than to the one who pursues a more familiar idea (see discussions by Armstrong, 1982; Fishman & Neigher, 1982; Greenwald, 1975). Editors may similarly be more ready to allocate journal space to results that confirm novel predictions than to studies that do the more mundane mapping-up of the trail left by these novel developments. This pattern of incentives to the researcher would be justified if results that supported novel theoretical predictions were generally informative. However, if the novel result has been obtained within the context of a theory-confirming strategy, its conclusion has been effectively foreordained and is, therefore, not very informative.

Sad Fate of Suggested Remedies

Previous critics, in the process of making observations about researchers’ strategies, have noted that it is difficult to take such published research at face value (e.g., Mehl, 1967). Further, they have suggested various remedies, which include (a) using a more stringent alpha criterion than the customary .05, to combat the effective “alpha inflation” that is incurred by repeated serial testing of the same prediction (Selvin & Stuart, 1966; Sterling, 1959); (b) standardly reporting measures of magnitude of effect, because null-hypothesis rejection can be obtained with trivially small effects when enough observations are made (Hays, 1981); (c) basing editorial decisions on reports of just methods (not results) in order to avoid bias against publishing null results (Walster & Cleary, 1970); and (d) developing statistical procedures for drawing conclusions in form other than rejection of null hypotheses—specifically, using interval estimation or Bayesian techniques (Bakan, 1966; Edwards, Lindman, & Savage, 1963; Grant, 1962; Greenwald, 1975). Although one can find occasional demonstration uses of these procedures, it is remarkable that the suggested remedial techniques are not much used (even by their advocates!).

The nonacceptance of remedial measures indicates that existing practices, faulty though they may be, have a powerful appeal. The present analysis identifies that appeal as their theory-confirming function. The remedies suggested by previous critics may remain unused precisely because they uniformly reduce prospects for obtaining results that confirm researchers’ theoretical predictions. (Never mind that it is the least justifiable theory confirmations that the alternative procedures should selectively avoid!)

This is a harsh verdict. To state it bluntly: Researchers’ dispositions to confirm hypotheses support their use of methods that are demonstrably prone to misinterpretation and, because of that, obstruct scientific progress.

What Is the Appeal of Confirmation-Biased Methods?

Confirmation bias delays finding support for beliefs other than those presently held. Why have confirmation-biased methods not been supplanted, long ago, by more objective methods? We suggest three answers. Although these answers are speculative, they have the virtue of suggesting ways to overcome the problems of confirmation bias.

First, confirmation bias can be a useful heuristic. That is, even though they lack objectivity, confirmation-biased methods may often work well (see Klayman & Ha, 1984). Confirmation bias
condition-seeking question is "Under what conditions does a communication accompanied by a discounting cue produce delayed persuasive impact?" The condition-seeking researcher deliberately seeks to produce a qualified conclusion (such as "A persuasive message accompanied by a discounting cue produces delayed impact if the cue is presented after an effective message, but not if it is presented before the message").

Increasing precision of conclusions. Sustained use of the condition-seeking method generates a progression of research questions that, if pursued empirically, yield increasingly precise conclusions. In abstract, the sequence is as follows:

Question 1: Under what conditions does R occur?
Answer 1: Under Condition C1 (and not in the absence of C1).
Question 2: Under what conditions does C1 produce R?
Answer 2: Under Condition C2 (and not in the absence of C2).
Question 3: Under what conditions does (C1 + C2) produce R? (etc.)

In summary, when a research question is asked in condition-seeking form, the answer must specify conditions under which a given result is not obtained, as well as specifying conditions under which it is obtained. The difference between unqualified and qualified conclusions corresponds to that between statistical main and interaction effects. The condition-seeking method can therefore be understood as an interaction-effect-seeking method.

Design Approach

In contrast with the method of condition-seeking, which discovers conditions on which an existing finding depends, the design approach aims to specify conditions that can produce a presently unobtainable result. The design approach is an attempt to engineer a desired result.

As with the method of condition seeking, research questions based on the design approach can be phrased to start "Under what conditions . . . ?" One use of the design approach is to discover conditions that can produce results that are of known practical value—for example, "Under what conditions do people who are inebriated decide that they should not drive?" or "Under what conditions do adolescents resist pressure from peers to smoke cigarettes?"

Another use of the design approach is to generate results that constitute significant reversals of familiar theory-based findings—for example, "Under what conditions does viewing televised violence reduce subsequent tendencies for the viewer to act aggressively?" or "Under what conditions does a child's liking for an activity increase directly with the magnitude of incentive to engage in it?" Because it is, in effect, well known how not to obtain the sought result, the outcome of an experiment using the design strategy need not be in the form of an interaction effect.

The condition-seeking and design strategies form a complementary pair. The method of condition seeking aims to discover conditions on which an already obtainable finding depends, whereas the design approach seeks conditions on which a presently unobtainable finding depends. The condition-seeking method reduces the range of generality of a finding, whereas the design approach increases (from nii) the range of generality of a finding.

Is the Condition-Seeking Strategy Already in Use?

Some readers of a preliminary version of this article suggested that the advocated condition-seeking strategy, even if not explicitly identified by this (or any other) label, is already a widely used strategy. This comment occurred in two forms: (a) that there is a long-standing tradition among researchers whereby predicted statistical interaction effects are considered to be a particularly valuable form of result and (b) that condition seeking occurs effectively as the collective product of theoretical competition in a research community, even if it is not the goal of individual researchers or teams. Although we agree with both of these observations, we cannot agree that contemporary research practice is therefore free of the ills of confirmation bias.

Interaction-effect-seeking in practice. There appears to be at least a limited informal tradition favoring interaction-effect research designs, as indicated by responses to a query sent to several innovators in social psychological research method. At the same time, explicit discussion of the importance of interaction effects is almost nonexistent in psychological methods texts. The following statement by Mills (1969) was the most detailed that we could find in an examination of prominent psychological methodology texts of the past three decades (cf. Carlsmith, Ellsworth, & Aronson, 1976, p. 248; McGuire, 1969, p. 140).

Perhaps the most valuable kinds of experimental studies in social psychology are ones that find a statistical interaction between two independent variables in their effect on a dependent variable. To show that a phenomenon varies as a function of certain conditions usually helps a great deal to narrow down the possible explanations for the phenomenon. (p. 437)

The lack of general use of condition-seeking methods is indicated both by the discussion of the problem of avoidable overgeneralizations, earlier in this article, and by McGuire's (1973, 1983) criticisms of researchers' inattention to conditions on which their findings depend. (McGuire's views are considered in more detail below.) In sum, there appears to have been an informal discovery (perhaps independently among several researchers) of the value of a condition-seeking strategy. However, this informal discovery has not been accompanied by any strong or widespread advocacy of the method.

Examples of collective condition seeking. It is possible to find many examples of condition seeking emerging as the informal, collective product of competitive theory-centered individual research efforts. As previously noted, controversy over the insufficient justification effect was resolved by the progressive, collective discovery of qualifying conditions (see further discussion, below). Another problem that occupied several theoretically competing groups of social psychologists was the risky shift effect, the finding that group decisions were typically more risky than those made by individuals (Stoner, 1961; see the discussion of scientific progress in the context of this problem by Cartwright, 1973). The risky shift controversy was eventually resolved by the discovery of conditions that produced the opposite effect, a conservative shift (Brown, 1965; Myers & Lamm, 1975). A problem

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8 Personal communications from Elliot Aronson (May 17, 1984), Jack W. Brehm (January 15, 1985), Edward E. Jones (January 28, 1985), and Judson Mills (January 14, 1985).
term-maintaining cognitive change (cf. Greenwald, 1980; Schlenker, 1982; Tesser & Campbell, 1983). Statement 2 is, for example, powerful enough to suggest a fifth qualification of the insufficient justification effect: It should not occur among persons whose self-concept includes traits that are consistent with making disbelieved statements. (Nevertheless, further progress at condition seeking should lead toward an interpretation even more powerful than Statement 2.)

McGuire’s Analysis of the Relation of Theory to Research

Our conception of the relationship of research to theory has been preceded and heavily influenced by William J. McGuire’s (1973, 1983) writings. This influence can be seen most readily through some quotations from McGuire’s (1983) description of the methodological approach that he calls contextualism. (These quotations are from McGuire’s section entitled “Corrupting Effects of the ‘Hypothesis-Testing’ Method.”)

It can be taken for granted that some set of circumstances can be found to confirm any expressible relationship, provided that the researcher has sufficient stubbornness, stage management skills, resources, and stamina sooner or later to find or construct a situational context in which the predicted relationship reliably emerges (pp. 13-16).

A researcher ingenious enough and persistent enough will at last design an experiment that comes out “right” and it is only this sanitizing experiment that becomes the sole portion of the research program revealed in the manuscript submitted to the dissertation committee or the journal. (p. 15)

The more valuable information obtainable through empirical confrontation emerges from the pattern of contexts in which the predicted relationship obtains as contrasted with those in which the contrary relationship or none at all obtains. (p. 16)

Contextualism maintains that a theory (like knowledge on any other level) is an oversimplified and distorted representation of any situation. A hypothesis or its contradictory is each adequately true in a few appropriate contexts and each is dangerously false in many others. (Contextualism regards) empirical confrontation, not as a test to determine whether the hypothesis is true or not, but rather as a continuing discovery process to disclose the hypothesis’s full meaning by revealing its hidden assumptions and so specifying in which contexts its misrepresentations are tolerable and in which seriously misleading. (pp. 7-8)

We agree very fundamentally with the premises of McGuire’s analysis. Further, our suggested condition-seeking and design strategies overlap with some of the research-strategy suggestions McGuire makes (1983, pp. 26-27). We exceed McGuire’s recommendations primarily in concluding that theory testing should often be displaced from its status as a central goal of research.

Conclusion: The Role of Theory in Research

Science in the Middle Ground Between Truth and Falsity

Contemporary philosophical analyses of scientific method demand a skeptical regard toward theory: No theory can be proven true by empirical data. And, just as it is impossible to prove a theory true, so also is it impossible to prove one false. Any theory can be patched, by ad hoc addition of assumptions, to fit with existing data. The joint impossibility of either proving or disproving theories might appear to justify dispensing with theories entirely. However, such a skepticism is difficult to maintain. The ability to benefit from experience is greatly facilitated by converting experience into conceptual knowledge (theory) that can serve as a basis for action, even if that knowledge rests on an uncertain epistemological foundation.

Fortunately, there is an immense middle ground between the philosophically unjustifiable positions of regarding theories as empirically provable or disprovable. And it is the work of science to evaluate theories within this middle ground. Fragile and misdirected although they are, theories are the essential containers of scientific knowledge and the necessary vehicles of scientific progress. As suggested by the metaphors of containers and vehicles, criteria such as storage capacity and speed of progress—criteria that are appraisable without having to speak of proof and disproof, or of truth and falsity—are most appropriate for evaluating theories. The work of science may best be regarded as approving and disapproving of theories, rather than as proving and disproving them.

Evolution of Theory in Response to Empirical Goals

Perhaps the greatest virtue of researchers is their perseverance, their willingness to continue data collection until a sought pattern materializes. When this perseverance is applied in the context of a theory-centered method, the ill-defined (because logically impossible) activity of theory testing is often converted into the better defined activity of producing the pattern of results predicted by the theory. In order to produce a theory-predicted pattern, stress is put on procedure. The consequence is that the persevering researcher finds a procedure that will produce the desired relationship. But when, as is often the case, the finished research product provides no contrast between the theory-predicted pattern and its absence, there may be no solid gain in understanding of the conditions on which the empirical pattern depends. More specifically, the researcher may not be able to describe, in a published report, the procedures that were critical to obtaining the reported prediction-confirming finding. Such a finding is difficult to replicate, and the theory that predicted it is difficult to apply.

By contrast, the condition-seeking and design strategies do not allow the often-recommended trait of perseverance to corrupt (as McGuire puts it) the research process. By explicitly seeking empirical interaction-effect patterns, these strategies put stress on theory, along with procedure. This stress obliges theory to increase its precision and thereby to become increasingly useful.

The observations just made can be recast in terms of the functioning of scientific research within an evolving system of knowledge. Alternative research strategies can be understood in terms of the manner in which they allow theories to act as evolutionary variations and the pressures that they apply to select among those variations. It would certainly be efficient for such selection pressures to operate directly on theories. However, it is an article of faith among scientists (and perhaps a chief defining characteristic of scientific activity) that theory is to be evaluated primarily by the indirect means of empirical test. In an empirical test, the conceptual elements of a theory are given operational form, and
A Reaction to Greenwald, Pratkanis, Leippe, and Baumgardner (1986):
Under What Conditions Does Research Obstruct Theory Progress?

Jeff Greenberg
University of Arizona

Tom Pyszczynski
University of Colorado, Colorado Springs

Sheldon Solomon
Skidmore College

Lynne Steinberg
Indiana University

Greenwald, Pratkanis, Leippe, and Baumgardner (1986) argued that a theory-testing research orientation contributes to a confirmation bias that impedes the progress of research. To eliminate this confirmation bias, they proposed two complementary result-centered approaches: the method of condition seeking and the design approach. We argue that Greenwald et al. confused the relation between theory and research and that the result-centered strategies they proposed would in no way minimize the bias. We also suggest that result-centered research can impede the progress of psychology because it retards theoretical, methodological, and technological advancement, and encourages increasingly narrow and trivial research endeavors. We conclude by discussing ways to minimize these problems.

Even scholars of audacious spirit and fine instinct can be obstructed in the interpretation of facts by philosophical prejudices. The prejudice—which has by no means died out in the meantime—consists in the faith that facts by themselves can and should yield scientific knowledge without free conceptual construction.

Albert Einstein
(Clark, 1971, p. 63)

Greenwald, Pratkanis, Leippe, and Baumgardner (1986) recently proposed that the progress of empirical research is obstructed by a theory-centered research strategy. Greenwald et al. argued that investigators who use this strategy are likely to become ego-involved advocates of the theories they are testing and that, consequently, they often ignore data that are inconsistent with their theories and persevere with research directed toward confirming their theories by modifying procedures until results that fit theoretical predictions finally occur. They suggested that this confirmation bias can be circumvented by replacing theory testing as the routine research orientation with two result-centered research strategies, the method of condition seeking and the design approach. The method of condition seeking is an explicit attempt to determine the specific conditions under which a particular known finding can and cannot be obtained. In contrast, the goal of the design approach is to discover conditions under which a previously unobtainable result can be produced.

We agree with Greenwald et al. that researchers are often biased toward confirming their preferred theoretical positions. However, we do not believe that the confirmation bias results from a theory-testing research orientation per se, and we consider Greenwald et al.'s proposed cures to be worse than the disease itself. More specifically, we propose that (a) Greenwald et al. are confused about the relation between theory and research; (b) the methods they propose are currently used and in no way eliminate the confirmation bias; and (c) a result-centered approach is likely to encourage increasingly narrow and trivial research endeavors and discourage the development of more useful methods and theories for understanding human behavior.

Before proceeding, we would like to note a few limitations of this article. First, because of space limitations, it does not acknowledge the work of a variety of authors in psychology and other disciplines that addresses these issues. Second, although we believe our concerns are pertinent to all areas of psychology, our examples are drawn from social psychology. Finally, our arguments, and Greenwald et al.'s as well, primarily apply to basic as opposed to applied science.

The Appropriate Relation Between Theory and Research

Despite occasional declarations of the value of theory, the title of Greenwald et al.'s article ("Under What Conditions Does Theory Obstruct Research Progress?") as well as its tone and contents seem to imply that the goal of scientific inquiry is research progress and that theory is, at best, an essential aid to research progress and, at worst, an impediment to research progress. This viewpoint reverses the appropriate relation between theory and research. The primary goal of basic scientific inquiry is to provide an understanding of the phenomena under consideration. Theories are the basis of such understanding in that they are explanations for how or why particular phenomena occur, and for how they are related to other phenomena. Therefore, rather than piling up "facts" about the nature of reality, the primary goal of basic science is to develop increasingly use-
ful theories regarding the phenomena of interest (cf. Sechrest, 1977, 1986). Research should therefore be a tool for facilitating the progress of theory, not, as Greenwald et al. repeatedly implied, the other way around. As Sechrest (1986) similarly observed, “research is a method for testing theoretical propositions . . . to do . . . work simply for its own sake is a futile exercise in non-productivity” (p. 318). If applied to the early twentieth century work on genetics, Greenwald et al.’s reasoning would have led one to advise genetics researchers to abandon their theoretical notions because they were distorting their study of fruit flies.

The Role of Overgeneralization in the Advancement of Science

Greenwald et al.’s underestimation of the importance of theory in science is also reflected in their excessive concern that theories foster overgeneralization. With any but the most well-worn theories, generalizations are bound to go far beyond what existing empirical findings can unequivocally justify. That is why further research is needed. Without theory, particular findings could not be generalized at all. All one could say is that a particular effect occurred as assessed by particular measures under particular conditions with particular subjects in a particular lab at a particular point in time. Any theoretical interpretation of a particular finding is therefore necessarily an overgeneralization—and it should be. By framing a theory as broadly as possible and extensively testing its diverse implications, its full power, as well as the unanticipated qualifications and limits on its domain, are eventually discovered. Although Greenwald et al. did acknowledge the inevitability of overgeneralizations, they failed to recognize the extent to which they are both useful and correctable within the context of theory testing science.

Of course, as Greenwald et al. argued, overgeneralizations can lead to inappropriate applications to the real world. However, scientists usually express appropriate caution about application of theory until sufficient theory-testing research has been conducted; the problem is that individuals who must deal with real-world problems often ignore such cautionary statements and attempt application prematurely. Greenwald et al. used the desegregation efforts in the United States, which have not been especially effective (Stephan, 1978), as an example of the problem of overgeneralization. It is an extremely poor example, however, because the social scientists who contributed to the movement toward desegregation specified, on a theoretical basis, the conditions necessary for effective desegregation, none of which were subsequently met (e.g., Allport, 1954). This example is one in which the problem of not waiting for theory to be evaluated is apparent; additionally, it illustrates that overgeneralizations sometimes occur not because of problems with the theory but because of inappropriate applications in which theoretically specified conditions are ignored.

Theories Do Specify Limiting Conditions

Greenwald et al. argued that whereas a theory-testing approach promotes the question “Does a particular effect occur?” a condition-seeking approach would lead to the question, “Under what conditions does a particular effect occur?” There are two problems with this assertion. First, until a particular effect is initially shown to occur, it makes little sense to investigate the specific conditions under which it will and will not occur. Second, theories most often do specify conditions under which effects should and should not occur. Even Festinger’s (1957) original cognitive dissonance theory led to the testing of the hypothesis that attitudes toward a boring task would become more favorable if subjects were led to tell someone that it was interesting when the incentive for doing so was small but not when it was large. This example demonstrates that the simplest level of limiting conditions does not necessarily involve an interaction. Furthermore, many psychological theories do specify complex sets of conditions under which particular effects should and should not occur.

The Relation Between Theory and Research According to Greenwald et al.: An Ambiguous Position

Toward the end of their article, Greenwald et al. made statements that seem highly compatible with our position and inconsistent with their own. For example, on the last page of an article arguing for the replacement of a theory-testing approach with a result-centered approach, they stated that:

For result-centered methods (such as condition-seeking and design), again research procedures provide the variations on which selection operates directly. The selection criterion is agreement versus disagreement of an obtained data pattern with one specified in advance. If a procedural variant that has been suggested by a theory does not produce the desired pattern, it is replaced, and, importantly, the theory that suggested it is mildly discredited. The survival of a theory is a function of its ability to generate effective procedures (p. 227).

We cannot discern any meaningful difference between what is described in the foregoing quote and the typical theory-testing approach. A theory generates a prediction which suggests procedures that should yield a certain pattern of results. If the pattern is not obtained, the theory gains no support and may be mildly discredited if the correspondence between the concepts and the operational procedures is above suspicion (an unusual occurrence in psychology). If the pattern is obtained, some support for the theory is claimed. The survival of a theory is a function of its ability to generate predictions that are supported by the results of operational procedures. Greenwald et al.’s position seems to suggest that theories should guide the choice of procedural variants to obtain particular results; if so, whether the desired results are or are not obtained will certainly have implications for assessment of the theory. Indeed, it would be of little use to obtain certain results if they did not have implications for assessment of some theoretical idea.

Consider Greenwald et al.’s specific methods. In condition seeking, the question asked is “Under what conditions does a particular effect occur?” The best way to proceed from there is to use theory to generate a potential answer. The purpose of the research would then be to test the validity of that theoretical answer. Similarly, the design question, “Under what conditions does a previously unobtainable result occur?”, is best addressed by the theoretically specified conditions. The experiment that would then be designed would test the theoretical idea that those particular conditions lead to the previously unobtainable result. These theory-guided approaches, which Greenwald et al. seemed to advocate, clearly involve theory testing. The only
alternative approach would be to search for the effect under various arbitrarily selected conditions. Although Greenwald et al. did not explicitly advocate such an atheoretical approach, it is the only real alternative to theory testing; therefore, we believe that their general advocacy of a result-centered focus in which the importance of theory is reduced and theory testing is not recognized as the purpose of basic research will encourage empirical research that is detached from theoretical concerns; consequent problems resulting from such research will be considered later.

Evaluating Greenwald et al.'s Result-Centered Approach

Although Greenwald et al.'s position on the relation between theory and research is ambiguous, it is clear that their solutions to the problem of the confirmation bias will not eliminate the problem, and may lead to additional problems of their own.

Why Result-Centered Research Strategies Cannot Help

Greenwald et al. argued that the confirmation bias is a liability of the theory-testing approach. In contrast, we propose that the confirmation bias is in no way uniquely tied to the formal testing of theories and that it is likely to arise in research regardless of whether one employs the traditional approach criticized by Greenwald et al. or the result-centered methods they suggested as a remedy. The difference between the two approaches is primarily a matter of the centrality of the conceptual framework used to guide one's choice of variables to investigate. With a condition-seeking method, there are a virtually infinite number of possible limiting conditions for any given effect. Rather than randomly choosing variables to investigate, the researcher selects conditions that for a specific reason she or he believes will influence the effect. With a theory-testing approach, these conditions are explicitly derived from a theory or a combination of theories concerning the phenomenon of interest. Because of their concern with assessing theory, theory-testing researchers are likely to pay considerable attention to the theoretical basis of their research and will therefore be likely to work from a clearly developed and articulated theory; in contrast, researchers concerned with condition seeking rather than theory assessment will be more likely to use vague, intuitive theoretical ideas to derive conditions to assess. With either approach, the research is inevitably guided by some theoretical understanding of the problem at hand.

Regardless of the basis of the researcher's choice of mediating conditions to investigate, she or he is therefore likely to expect a particular pattern of results (usually an interaction). In addition, the expected interaction is the only result that would provide a clear contribution with a good chance for publication; a failure to replicate the prior effect could be attributed to a variety of factors (including inadequate research methods), and a main effect would simply fail to limit the generality of the original theoretical statement. The expected interaction effect would thus be most informative and most beneficial to the researcher's career; it is likely, then, that the researcher would have a strong desire to find that interaction.

To the extent that the confirmation bias results from researchers' expectations and desires for particular conclusions, this analysis strongly suggests that the method of condition seeking in no way discourages the bias. If the researcher is searching for limiting conditions, she or he will be biased toward confirming a refined version of the theory rather than the original version; however, the bias will be just as strong, as long as the researcher believes in the refinement, and is motivated to publish to obtain promotions, raises, and prestige.

For similar reasons, the design approach does little better in avoiding the confirmation bias. The most efficient and common way to produce a previously unobtainable result is to derive these conditions from a theory. An example of this phenomenon used by Greenwald et al. is the research on the jigsaw classroom (Aronson, Stephan, Sikes, Blaney, & Snapp, 1978). Clearly, the purpose of this research was not to test a theory, but to apply one. In applied research, as with Greenwald et al.'s design approach, the appropriate goal is often to achieve a desired result. However, in attempting to improve minority achievement and interethnic harmony in the classroom, Aronson et al. applied theoretical principles derived from the work of Gordon W. Allport, Muzaffer Sherif, and others. Therefore, even in this example of the design approach, the researchers were likely to have strong theory-based expectations and, possibly, allegiances to particular theories as well. Thus, the conditions likely to lead to a confirmation bias quite clearly exist even when the design approach is used.

Up to this point, we have argued that all research, whether guided by intuitive or formal theory, is subject to a confirmation bias on the part of the researchers. Perhaps, the only solution, then, would be truly atheoretical research, something that Greenwald et al. explicitly disavowed. Unfortunately, even atheoretical research with a practical goal would not necessarily escape the confirmation bias. Even if one assumed that Aronson et al. (1978) had no theory-based concerns, it is quite likely that they expected and were motivated to find positive effects of their jigsaw classroom technique (for altruistic reasons, self-serving reasons, or both).

The confirmation bias would not be eliminated by the procedures recommended by Greenwald et al., or by any other procedures, as long as creatures with beliefs, expectations, and desires conduct research. As a variety of theorists and researchers have noted, human beings have pervasive propensities to apply schemas, expectations and hypothesis-testing strategies in their interactions with the world and these propensities are likely to produce confirmation biases (e.g., Chapman & Chapman, 1969; Fiske & Taylor, 1984; Snyder, 1984). In addition, human beings are motivated to obtain a variety of goals, including money and esteem; therefore, as long as certain findings are particularly likely to lead to these outcomes, there will be bias toward obtaining those certain findings. Unless we find a different species to conduct research, the best we can hope for is to minimize the impact of the confirmation bias on the progress of the discipline.

Additional Problems With Result-Centered Research

Aside from the confirmation bias, which is a problem with all research, result-centered research is likely to contribute to a number of other problems as well. It is important to briefly consider these additional problems because, regardless of appearances to the contrary, there is reason to believe that a great
deal of basic science research conducted in psychology is, in fact, result-centered. 1

1. Result-centered research retards theory progress because it discourages focus on maximizing theoretical clarity and internal validity and encourages post hoc assessment of theories.

If researchers were not guided by the goal of theory testing, they would be insufficiently concerned with theory development and articulation and with designs that maximize internal validity. If one were simply trying to produce a previously unobtainable result or discover conditions under which an effect will and will not occur, there would be little reason to worry about the precise theoretical base of one's research, or, if the expected result were found, with whether or not the procedures one chose would be consistent with a single theoretical explanation. Thus, in comparison to the goal of obtaining results, the goal of theory assessment is much more likely to encourage researchers to clearly specify their theoretical base and to avoid confounding variables. Quite simply, if one's purpose were simply to create conditions that alter an effect or to produce a new effect, then the theoretical understanding of how such results were obtained would not be sufficiently important to the researcher.

There is another perhaps more basic problem inherent in Greenwald et al.'s methods. To progress toward better theories, theories must somehow be assessed; if this is not done a priori, by assessment of predictive validity via explicit theory-testing research, then it must be done by assessment of the post hoc explanatory capability of known theories—or the induction of new ones. This would bias the assessment process in favor of theories that are sufficiently vague, complex, or flexible to account for most any research finding, thereby leading to what most psychologists would view as a serious regression of the discipline back to the days of development and acceptance of untestable theories and purely descriptive models.

2. Result-centered research seeks results rather than answers to important questions.

Greenwald et al.'s method of condition seeking begins with a previously established research finding and proceeds to ask under what conditions this finding will and will not occur. This method limits the questions addressed by research in a number of ways. First, initial evidence for a variety of important phenomena would never even be sought. Consider, for example, the research on self-awareness theory. The theory specifies a number of very interesting effects of self-awareness; motivation to test this theory led to the innovative use of a mirror to heighten self-awareness. In the absence of an interest in testing theory, it seems unlikely that anyone would have investigated the effects of the presence of a mirror on attitude-behavior consistency. There are innumerable similar examples in which the theory-testing approach has led to important research that would never have been initiated by a result-centered approach.

Second, a result-centered approach encourages derivative research that searches for significant effects without consideration of what effects would or would not enhance our understanding of a particular phenomenon. Concern with testing theories guides researchers toward potentially informative rather than uninformative research. When not guided by theory-testing concerns, research will often be conducted in a fashion that promotes consideration of increasingly trivial questions that become detached from the concerns that initially generated the research. The point is that questions that ask whether or not a particular theoretical idea is valid are most likely to advance our understanding of the phenomena of central importance to the discipline. If the purpose of research were obtaining results rather than testing theory, such questions would not often be asked or answered.

3. The result-centered method of condition seeking restricts theoretical concern to explaining research findings rather than the full range of phenomena that comprise the appropriate domain of the discipline.

The condition-seeking approach is focused on laboratory-produced experimental findings of current concern. Researchers are supposed to investigate the conditions under which the effect does and does not occur. On the basis of such investigations, someone is apparently supposed to induce a refined or new theoretical framework that can account for the results obtained, although research should not be explicitly directed toward testing the validity of this framework (see Greenwald et al., pp. 217, 225, 226). One major problem with this approach is that the data base from which theories are induced would consist primarily of research elaborating upon experimental laboratory phenomena. 2 Therefore, the theories that would be developed would be directed toward explaining research findings, rather than the real-world phenomena that are presumably of primary interest and importance to the discipline. Consequently, the result-centered approach would encourage the evolution and assessment of theories that are constrained by existing questions, currently established findings and methodological limitations. Such theories would not adequately explain or even consider a variety of issues that are of obvious relevance and importance to the discipline. A result-centered approach would thus encourage theories and the various disciplines themselves to become increasingly narrow and trivial and, ultimately, profoundly detached from the real-world phenomena that they should be directed toward understanding.

4. The result-centered method of condition seeking retards the development of new methodological and technological advances because it relies too heavily on existing research paradigms.

According to the method of condition seeking, research should be directed toward determining under what circumstances previously obtained effects can and cannot be produced. This would encourage researchers to continue using the methodologies and technologies that have been used in the past.

1 For example, editors sometimes request that authors reconceptualize the supposed theoretical basis of research submitted for publication and, without prompting, authors often present post hoc explanations of findings as if they were a priori. In addition, because interactions are less susceptible to alternative explanations than main effects, it is likely that researchers will be motivated to find interaction effects. Finally, there is a long tradition of studies in psychology that demonstrate interesting phenomena rather than test new theories. Indeed, the sleeper effect was originally studied not because it supported an interesting theory, but because it was an interesting phenomenon.

2 We are not arguing that research findings are always inappropriate bases for theory induction but that bodies of research involving a great deal of control and intervention by the researchers generally yield findings of very limited use for induction. In contrast, systematic descriptive research (e.g., the work of Charles Darwin) may be an excellent supplement to casual observation and introspection as a basis for theory induction.
to produce the effects of interest, because the effects must be replicated for the sacred interaction to occur, and thus, for evidence regarding limiting conditions to be obtained. Consequently, the methodological context would either explicitly or implicitly circumscribe the domain of theoretical explanation, leading to theories specifically designed to apply to current methodological and technological contexts.

In contrast, as Einstein proposed for the physical sciences (Clark, 1971) and as Sechrest (1977, 1986) has argued for psychology, new theories induced from real-world phenomena often pose questions and generate hypotheses that cannot be addressed by current methods and techniques. The goal of testing new theories therefore often inspires the development of methodological and technological advances that allow for the study of phenomena that have not previously been considered in research settings.

Summary

Under what conditions does research obstruct theory progress? It does so when (a) researchers are more concerned with promoting their own careers than with assessing theoretical ideas, (b) research is directed toward producing results rather than advancing understanding through theory testing, (c) theories are assessed by research findings in a post hoc rather than an a priori fashion, (d) existing research is the primary determinant of what are considered to be the important questions for a field, (e) laboratory research is treated as the sole or primary basis for theory generation, and (f) currently available research methods and technologies dictate the form and content of new theories.

Conclusion: Can Anything Be Done to Minimize These Problems?

The problems of confirmatory bias and insufficient theoretical focus occur largely because researchers are thinking, motivated creatures with expectations and desires that affect their behavior. Therefore, we believe that there are no easy solutions to these problems. We would, however, like to make a few suggestions that may reduce the damage that they cause.

As Greenwald et al. noted, the problem of ignoring null results contributes substantially to the confirmation bias. However, it is not clear how to avoid this problem because null findings are not generally informative; there are usually too many possible explanations for why a predicted effect did not occur. In addition, it is not particularly interesting to find that a new theory has failed to gain empirical support. On the other hand, when well-accepted theories continually fail to be supported, or when initial findings supporting a theory are not found in a carefully designed direct replication, publication outlets can be receptive. Fortunately, some aspects of the current publication system help minimize the problem of individual confirmatory biases. To publish work in prestigious journals, one must submit the work for peer review; this can be viewed as an adversarial system in which the authors advocate their work and the reviewers attempt to find sufficient weaknesses to help the editor justify rejection of the manuscript. Viewed this way, authors are encouraged to take a confirmatory stance and promote their own work. In order to publish their work, however, authors generally have to convince others who do not share their biases; this should limit the extent to which authors’ confirmation biases lead to the publication of unworthy work.

After a manuscript is published, a similar adversarial process occurs over a longer period of time. Ultimately, a given theory that is proposed in the literature will gain sufficient support in subsequent research to be accepted, will be refuted by sufficient contrary evidence, will be refined in light of partially supportive evidence, or will be ignored if little further relevant research, supportive or disconfirming, is conducted. In the long run, theories that are supported by spurious or biased results will fall by the wayside, and theories that are overly general will be refined.

Although a given researcher will never be able to avoid confirmatory biases, the community of researchers can thus minimize their overall impact on the field. This will only work, however, if there is considerable diversity of beliefs, motives, and theoretical allegiances, and tolerance for such diversity, in the scientific community. Greenwald et al. noted that for fringe topics, such as parapsychology, "very similarly directed biases are shared by many of the researchers. We agree, but we do not consider this to be a major problem precisely because these are considered fringe topics and are treated with appropriate skepticism by the rest of the field. What does concern us, however, are conditions under which too many researchers within a major subdiscipline within psychology, or within an entire field, become too homogeneous in the direction of their confirmatory biases. Such shared confirmatory biases, as manifested by homogeneity in graduate training, editorial practices, and funding priorities, lead to a state of affairs in which the content of a discipline is constrained by current fads and fashions. The psychoanalytic and behaviorist movements may be examples of this phenomenon from the past; the social cognition movement within social psychology may be an example in the present (cf. Neisser, 1980). This state of affairs can lead to an intolerance of other interests, theoretical orientations, methods, and findings. Under these conditions the adversarial controls on confirmatory biases break down, and consequently, the progress of the field toward understanding is seriously impeded.

We are not sure how to combat the problem of widespread shared confirmatory biases, except to advocate staunch tolerance for diversity, resistance to conformity, and skepticism toward any major movement within a field. We do, however, have a more specific suggestion that may minimize some of the problems we have discussed: greater theory development and dissemination without the requirement of immediate, direct empirical support. We believe that an overemphasis on empirical work contributes to the confirmation bias, impedes the development of useful theories, and encourages increasing detachment of the activities of a discipline from its appropriate subject matter.

\[3\] As our advocacy of theory testing suggests, the type of theories that we believe should be encouraged are those that are potentially testable. It seems to us that many of the "theories" and "models" that are proposed in contemporary psychology are largely descriptive rather than explanatory. These conceptual frameworks do not generate clear predictions and can usually account for any finding posthoc. Therefore, there is no potential basis for empirically assessing their validity and limitations; consequently, they are not likely to contribute towards an understanding of phenomena of interest.
THEORETICAL NOTES


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Editor Named for APA's Clinician's Research Digest

The Publications and Communications Board has named George Stricker of Adelphi University's Gordon F. Derner Institute of Advanced Psychological Studies Editor of Clinician's Research Digest (CRD), which is being published by the American Psychological Association as of July 1988.

The six-page newsletter reports on research related to approaches to treatment modalities, including any systematic empirical study, as well as some coverage of child and gerontological issues. Although therapy is the main focus, key assessment and diagnostic questions as well as forensic issues are covered. CRD is an easy-to-read, fact-based, findings-oriented digest of research that summarizes for practitioners relevant material from the science base of practice. Complete citations are included so that clinicians interested in more information can request the full article from the author.

Although all published material will originate with the Editor, readers of CRD are invited to refer to Stricker any references or reprints of articles they find valuable.