



## A Note on the Armstrong/Mitroff Debate

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*Recently, Armstrong and Mitroff have joined an important debate over the methods of science. We are afraid that because of the tongue-in-cheek fashion in which it was done that two important issues were intertwined and may not be fully appreciated. The issues involved the practice of science versus the methods and theory versus data.*

As one of the newer areas of scientific enquiry, management theory is one of the more active in undergoing critical self-searching, questioning the legitimacy of methods and of approach in the development of potentially fruitful research programs. One such issue has been raised again in the *Academy of Management Review* in an exchange between Armstrong (1980) and Mitroff (1980). Perhaps because of the brevity of these two papers, there is a danger that two quite separate, but equally important, questions may be confounded. One is that of the legitimacy of subjectivity versus objectivity, or of emotional commitment versus rational evaluation, in pursuing a research program. The second is that of the appropriate response when empirical data do not confirm a proposed hypothesis. This issue is important not only for scholars but for practicing managers because they also become committed to managerial techniques (e.g., MBO, job enrichment, leadership training) in spite of data questioning the success of these techniques.

Mitroff's (1974) examination of how scientists behave with respect to the methods of science has led him to suggest a set of norms counter to those commonly espoused and advocated as good scientific practice. Examples of these counternorms would include: (a) emotional commitment, as opposed to emotional neutrality, to one's ideas and theories; (b) particularism versus universalism (that is, accepting or rejecting ideas/claims based on who made them); and (c) organized dogmatism versus organized scepticism (or clinging to one's ideas in the face of evidence to the contrary). Further, Mitroff suggests that these counternorms serve a useful adversarial role in advancing knowledge. Citing Westfall (1973), he states:

The history of science shows repeatedly that science [is] advanced by

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persons who passionately believed in their pet hypotheses literally to the point of ignoring data that went against them.

Furthermore, he goes on,

the system of science can therefore not only afford to have a few individuals whose degree of commitment to their ideas is so strong that they may truly be incapable of giving up their positions, but the system may actually need such individuals because they stimulate others to prove them wrong and to pursue new ideas. (Mitroff, 1980, p. 514)

Mitroff's contention that advocacy and emotional commitment have important roles to play in the advancement of science has not met with universal acceptance. Armstrong (1979, 1980) argues that emotional commitment can have dysfunctional side effects, among them that of editorial censorship. Armstrong's position would seem to be that in designing and carrying out a research program, the role model should not be what successful scientists do in practice; but rather, in some ideal sense, what they should do. Thus, in his attack on Mitroff's position, Armstrong contends that ignoring data when we do not like the results not only is not in keeping with generally accepted methods of science, but certainly should not be accorded a position of acceptability nor advocated. Armstrong is not alone in this position; others also have taken stands against being fixated on theories, methods, or points of view (Dunnette, 1966).

Unfortunately, while raising some quite legitimate concerns on the potential dangers inherent in excessive enthusiasm for a particular hypothesis or research paradigm, Armstrong at the same time gives at least the appearance of arguing for the primacy of data over theory. The point of this note is to suggest that these two issues are separable, and that commitment to a hypothesis in the face of apparently adverse evidence can have an objective legitimacy quite apart from any element of subjective advocacy.

### **Logics-in-Use Versus Reconstructed Logic**

The first issue raised in the Mitroff/Armstrong debate revolves around how scientists do behave versus how they should behave. This parallels what Kaplan (1964) has referred to as "logic-in-use" versus "reconstructed logic." According to Kaplan, scientists have a more or less logical cognitive style which they employ in solving problems. Some of them formulate this approach explicitly, while for others it remains intuitive. The latter Kaplan calls the logic-in-use; the former, the reconstructed logic.

As Kaplan notes, logic-in-use is embedded in a matrix of an alogic-in-use, or even an illogic-in-use. Reconstructed logic, on the other hand, idealizes the logic of science in showing us how it would appear if it were extracted and refined to utmost purity. Armstrong appears to be upset that logic-in-use is not for everyone the same as the reconstructed logic we commonly associate with the methods of science. This concern can be justified if it can be shown that only reconstructed logic is useful in the development of scientific theories.

A study of the way scientific enquiry is conducted and the consequences that result should not be limited by the Platonic notion that the proper way to analyze and understand something is to refer it to its most ideal form (that is, its form abstracted from any concrete embodiment). The test for logics-in-use lies in the success or failure of the consequences in meeting the goals of science. It is here that Armstrong appears to be most upset with Mitroff's position, for he contends that the consequences of the logics-in-use of Mitroff's scientists are by-and-large detrimental. But, we ask, detrimental to whom or to what? Beneficial to whom or to what? It seems to us that a distinction should be made between the individual, the scientific community, and the theories under consideration.

Table 1 displays some of the consequences of employing logics-in-use (which, we argue, follows from Mitroff's position) versus using reconstructed logic (which Armstrong advocates). As Table 1 shows, neither the individual scientists nor the ideas they champion may benefit as much by the reconstructed logic Armstrong advocates as by the logics-in-use Mitroff describes. One merely needs note who in the academic community is recognized by his peers, invited to present papers and review the work of others, has work published, and is tenured and promoted. Most often it is a person who is strongly associated with a particular theory or approach. It sometimes does not seem to matter if the ideas are correct, incorrect, or even generally accepted.

On the other hand, the scientific community is likely to be the biggest beneficiary of the reconstructed logic if we assume that an explicit logic is the greater guarantor of correctness or extendability.

The second issue revolves around the logic of proof. By that we mean the reasons for accepting or rejecting a hypothesis. In a more general sense, the true issue is: What are the implications of each of these issues for the development and extension of theory. Here the costs and benefits are less clear. Certainly, if the scientific community were to insist on scientists making their underlying logics-in-use explicit there would seem to be less opportunity for intellectual dishonesty and fraud, but perhaps at the cost of an increased scientific conservatism. By contrast, the logics-in-use position would encourage a more lively interchange of ideas and opinions; but is forced, in the final analysis, to rely on an unprovable conviction that something equivalent to Adam Smith's "invisible hand" of economic markets will also guide the forum of scientific exchange. In fact, the arguments on both sides have much in common with those advanced for more, or less, regulation of economic markets.

### **The Classical Approach**

The classical approach to hypothesis formulation and verification consists of three basic steps (Bailey, 1978). In Stage 1, concepts are defined and relationships are proposed. Stage 2 consists of devising ways to measure the concepts empirically and writing testable hypotheses. Finally, in Stage 3, data are gathered and analyzed, and inferences drawn.

As an example, suppose we are interested in the effects of job satisfaction on job performance; and, having been exposed to two-factor theory and having heard the adage that happy workers are productive workers, write a proposition

Table 1  
Consequences of Logics-in-Use Versus Reconstructed Logic

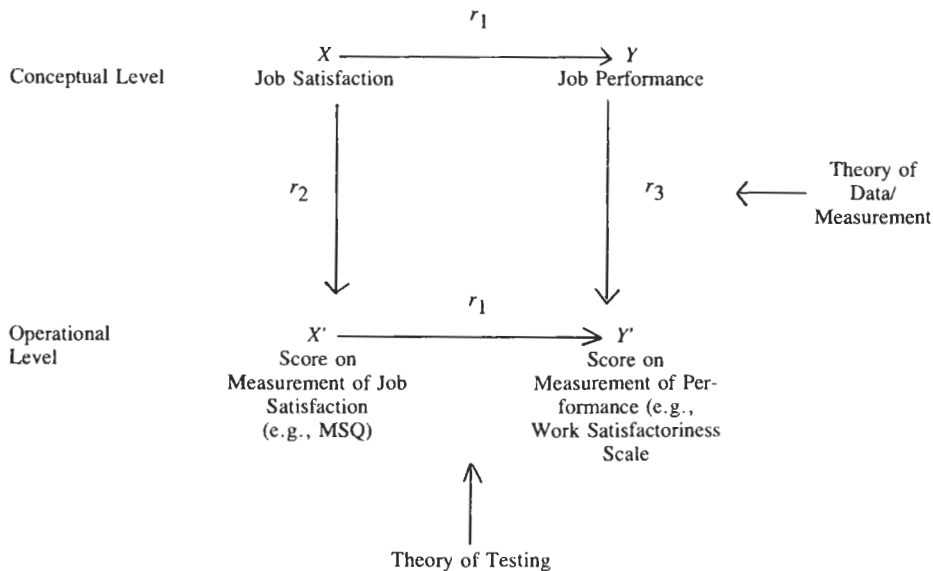
	Consequences		Theories
	Individual	Scientific Community	
Logics-in-use			
Emotional Commitment	Becomes the "expert" chosen to participate in symposia, present papers	Logics-in-use may be superior to reconstructed logic (e.g., deduction vs. induction)	Get full hearing, all nuances examined
Particularism	Gains recognition, becomes gatekeeper	Clash of ideas by partisans (assumptions/methods hidden)	Not abandoned prematurely
Dogmatism	Easier to meet requirements for academic tenure and promotion		Not abandoned when incorrect
Solitariness			Favored over interpretative theory i.e., data
Reconstructed Logic			
Objective	Learns from peers, avoids others' errors	Efficient use of resources	Partial hearing
Universalism	Doesn't re-invent the wheel	Selfcorrective	Data (i.e., interpretative theory) favored over explanatory theory
Scepticism	Others borrow ideas, publish first	Clash of ideas by emotionally neutral individuals (assumptions/methods open)	Abandoned more easily when incorrect
Openness	More competition for recognition		Abandoned prematurely when partially correct

(Stage 1) that the higher the level of job satisfaction, the higher the level of job performance. In Stage 2, after first having defined our constructs, we begin by specifying measures for both job satisfaction and job performance. Then we write a hypothesis linking these measures. Our hypothesis is that scores on the job satisfaction scale are positively related to scores on the job performance scale. Finally, we are ready to gather data to test our hypothesis and draw some conclusions (Stage 3).

The relationships between the conceptual and operational levels are diagrammed in Figure 1. The empirical measures of the theoretical concepts  $X$  (job

Figure 1

The Classical Approach to Hypothesis  
Construction and Verification



satisfaction) and  $Y$  (job performance) are designated by symbols  $X'$  and  $Y'$  respectively. On the conceptual level,  $r_1$  represents the hypothesized causal relationship between job satisfaction and job performance. The primes simply represent the same relationship at the empirical level. Only the value  $r_1'$  can be computed. However, if we assume that  $X'$  and  $Y'$  are accurate measures of  $X$  and  $Y$  respectively, then  $r_1 = r_1'$ . These assumed relationships between the conceptual and empirical levels are generally referred to as epistemic relationships or correlations.

Failure to verify a hypothesis may be due to: (a) inadequate sampling, (b) measurement error (this occurs when the epistemic correlations are less than 1.0), (c) an inadequate test, (d) an incorrect hypothesis, or (e) a combination of the above. Mitroff observes that scientists are likely to conclude that failure to verify a hypothesis is more likely to be due to measurement or sampling error than an incorrect hypothesis. Armstrong (apparently in the tradition of early

Popper, 1959, with his emphasis on the importance of falsification) appears to favor the likelihood that, in the absence of compelling reasons to the contrary, the null hypothesis is incorrect. In fact, Armstrong (1979) argues for the simultaneous consideration of multiple hypotheses, but still seems to imply that it is objectively necessary to affirm that hypothesis which is most consistent with the empirical evidence.

It is interesting to note that one way of demonstrating the construct validity of our measures, i.e.,  $r_1 = r_1'$ , is to show that they support the theory they are meant to test. As Cronbach and Meehl (1955) note, "We do not first 'prove' the theory, and then validate the test, nor conversely" (p. 69). This points out the dilemma concerning whether or not it is ever possible to separate theories from their tests. This dilemma manifests itself in two ways. The first manifestation concerns the theory-laden nature of observation. This is clearly seen when we attribute (a) different properties to numbers (e.g., nominal, ordinal, interval, or ratio) or (b) a meaning to dimensions derived from factor analysis. In Figure 1 we refer to this as the theory of data or measurement (cf. Guilford, 1954; Nunnally, 1978).

The second manifestation of the dilemma concerns the theories underlying how relationships between constructs are, or should be, observed. We refer to this as the "theory of testing" (see Figure 1). For example, in management theory contingency notions currently enjoy great favor. The question arises as to how these contingencies might be observed in our data. It is becoming quite popular today to use moderated hierarchical regression analysis to test these notions. However, as Busemeyer and Jones (1983) point out, there are many hidden assumptions concerning the monotonicity and reliability of the data that underlie the use of this statistical technique.

Thus, as we will discuss below, it is not always clear whether we are testing the theory or testing the data. If the two cannot be objectively separated, as is suggested by Churchman's (1971) Kantian inquiry system, then the question of whether to throw out the theory, the data, or both becomes problematical when they are in apparent disagreement. For, as Johnson (1981) points out: "pieces of data are not facts. 'Facts' are patterns of data that relate causes to effects and problems to solutions.... 'Meaning' is something human beings give to events—it comes from without, rather than residing within" (p. 13).

### Issues in the Philosophy of Science

Adherence to a strict falsificationist model is at variance with Duhem's (1906/1954) early argument that because of all of the many unstated assumptions underlying any empirical observation, no such observation can clearly falsify a theoretical hypothesis. Duhem points out that it may be the auxiliary assumptions underlying the observation, rather than the theoretical hypothesis, that is falsified by an inconsistent observation. As Kuhn (1970) has noted, "If any and every failure to fit were ground for theory rejection, all theories ought to be rejected at all times" (p. 146).

Lakatos (1968), in response to this difficulty, has suggested that logical consistency ought to be the criterion of scientific assessment, rather than either

the verificationist (inductive) or falsificationist (deductive) models. He makes the point clear in the following passage:

The problem is then *not* when we should stick to a 'theory' in the face of 'known facts' and when the other way round. The problem is *not* what to do when 'theories' clash with 'facts.' Such a 'clash' is only suggested by the mono-theoretical 'deductive model.' Whether a proposition is a 'fact' or a 'theory' depends on your methodological decision. 'Empirical basis' is a mono-theoretical notion; it is *relative* to some mono-theoretical deductive structure. In the pluralistic model the clash is between two high-level theories: an *interpretive theory* to provide the facts and an *explanatory theory* to explain them... The problem is not whether a refutation is real or not. The problem is how to repair an inconsistency between the 'explanatory theory' under test and the—explicit or hidden—'interpretive' theories; or if you wish, *the problem is which theory to consider as the interpretive one which provides the 'hard' facts and which the explanatory one which 'tentatively' explains them.* Thus experiments do not overthrow theories as [early] Popper has it, but only increase the problem-fever of the body of science. *No theory forbids some state of affairs specifiable in advance; it is not that we propose a theory and Nature may shout NO. Rather, we propose a maze of theories and Nature may shout INCONSISTENT.* (Lakatos, 1968, pp. 161-162).

There are several points worth noting. First, according to Lakatos, Nature's 'No' from a mono-theoretical bias "takes the form of an assented potential falsifer ... [which] we claim Nature has uttered and which is the negation of our theory" (p. 162). However, from a pluralistic perspective, Nature's inconsistent "takes the form of a 'factual' statement couched in the light of one of the theories involved, which we claim Nature has uttered and which, if added to our proposed theories, yields an inconsistent system" (p. 162). From this perspective, the wording of inferences to be drawn in hypothesis testing depends on whether one favors an inductive, deductive, or logical-consistency model of hypothesis verification. In keeping with the argument set forth by Lakatos, it is suggested that the third set of inferences (consistent/inconsistent) is to be preferred (see Table 2).

Second, if we accept the Popperian notion that science is revolution in permanence then, in the social sciences, Nature must be viewed as temporal and changing. Yesterday's consistency may be today's inconsistency, and vice versa.

Third, (in management theory at least) it may be necessary, if not desirable, to substitute the current collective judgment of the scientific community for the Nature in Lakatos' position. To quote an old baseball umpire:

Some say they call them as they are  
Some say they call them as they see 'em  
But I say they ain't nothing until I call 'em.

(Anonymous; cited in Simon, 1976, p. 29)

Finally, the question arises as to how one resolves the dilemma when data and

Table 2

## Inferences Drawn from Hypothesis Testing

	Data and Hypothesis in Agreement: Conclude	Data and Hypothesis in Disagreement: Conclude
Verification (Inductive) Model	Accept hypothesis	Do not accept hypothesis
Falsificationist (Deductive) Model	Do not reject hypothesis	Reject hypothesis
Logical Consistency Model	Data and hypothesis consistent	Data and hypothesis inconsistent
	or	or
	Interpretative and explanatory theories consistent	Interpretative and explanatory theories inconsistent

theory are inconsistent (or, if you will, among competing, possibly false, theories). Lakatos (1968) notes that Popper suggests that

a theory is better than its rival (a) if it has more empirical context, that is, if it forbids more 'observable' states of affairs, and (b) if some of this excess context is corroborated, that is, if the theory produces novel facts. (p. 163)

Lakatos, in addressing the dilemma caused by inconsistent theories, suggests that

if we have two conflicting theories, one explanatory and one interpretative, and we do not know which is which—that is, we do not know which should prevail as the interpretative theory providing the facts—we have to try to replace first one, then the other, then possibly both, and opt for the new set-up which represents the most progressive problem-shift, with the biggest increase in corroborated content. (p. 165)

As Lakatos notes, growth in science is not the linear sequence that early Popper would have in which theories are followed by eliminating refutations which in turn lead to better theories. Growth can occur without refutations. What is required is that sufficiently many and sufficiently different theories are offered. It is these streams of theories, or scientific programs, that clash (i.e., provide the Nature of social science research) as they encroach on each other's territory.

Eventually, one program tradition wins in that we accept it over its rival for the time being, so long as it represents a consistently progressive theoretical shift coupled with an intermittently progressive empirical shift.

As Kuhn (1970) has pointed out, "the act of judgment that leads scientists to reject a previous accepted theory is always based upon more than a comparison of that theory with the world. The decision to reject one paradigm is always simultaneously the decision to accept another" (p. 77).



Returning to our earlier example, rejection/replacement of the adage that a happy worker is a productive worker requires not only inconsistent data whose underlying theories have been corroborated; but, in addition, another paradigm (e.g., expectancy theory) with corroborated content not explained by the rival program.

Finally, Lakatos observes (as do Mitroff's scientists) that without the dogmatic attitude of sticking to a theory stream of research as long as possible, we could never find out what is in the theory/program nor have an opportunity to assess its strengths and/or weaknesses. Armstrong's concerns for the potential dangers inherent in scientific advocacy, while undoubtedly well-founded, cannot be resolved simply by uncritically giving a dominant position to the empirical evidence in the name of objectivity.

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# ***I'm Sorry That Science Is a Complex Phenomenon and It Doesn't Work by Simpleminded Rules***

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The authors of this note have located well the source of my extreme displeasure with Armstrong's caricature of my views regarding science. In virtually all my publications, I stressed that it was *not* an either/or choice between (a) an idealized set of norms or (b) a less than ideal set of norms-in-use. Rather, it was a case that both sets of norms operated simultaneously; and further, that one could not understand science on any basis, normative or descriptive, without seeing it as the dynamic interplay between two very sharply conflicting sets of norms.

What really incited my anger about Armstrong's position is that he never saw the extreme passion and emotionalism in his own defense of non-emotionalism. Unlike the author of the current note, he never analyzed both the positive and the negative consequences of both sets of norms.

If I overstated the advantages of the nonconventional norms-in-use, it was only because they have been systematically overlooked, perhaps even suppressed, by those such as Armstrong who are overly zealous of having science be a superrational belief system.

I have constantly stressed in my every publication on the matter that science could not exist if either set of norms operated without the counter-restraining existence of the other. This does not mean that in some contexts one set will not operate more or be more appropriate than the other. As to how this happens in all contexts, we still have no general theory. Even more to the point, we will have no general theory of science unless we study all its aspects, unsavory as well as savory. I would really like science to be as objective as its proponents would like it to be. But to achieve that we will have to study what science is really like, not keep on clinging to a set of boy-scout norms.

No one denies, least of all me, that science needs a set of ideal norms. But this trite observation begs the \$64 x 10<sup>9</sup> questions: "Whose set of ideal norms?" and "How do norms-in-use based on actual practice shape new ideal norms, and vice versa?"

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# **The Importance of Objectivity and Falsification in Management Science**

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In general, I thought that the Boal and Willis "Note on the Armstrong/Mitroff Debate" provided an interesting and fair discussion.<sup>1</sup> The summary of the consequences of the subjective versus objective approaches (Table 1 in their paper) was helpful. It clearly outlined the dilemma faced by scientists: "Should I strive for personal gain or for scientific contributions?" It also described what is likely to happen to the theories generated from the subjective and objective approaches. For example, the authors claimed that the subjective approach will yield a fuller hearing for a theory.

Given my preference for empirical evidence, I was disappointed that Boal and Willis had little evidence to report. Fortunately, recent research has been done on the above topics. This research supports some of Boal and Willis's conclusions, but it falsifies their conclusion that the subjective approach will provide a fuller hearing for theories.

The evidence seems consistent with Boal and Willis's summary of the conflict between the advancement of scientists and scientific advancement. My summary of the empirical evidence on this conflict led to the "Author's Formula" (Armstrong, 1982a, p. 197). This states that scientists who are interested in career advancement should: (a) not select an important problem, (b) not challenge existing beliefs, (c) not obtain surprising results, (d) not use simple methods, (e) not provide full disclosure, and (f) not write clearly. These rules for scientists conflict with the aims of science. Unfortunately, many scientists use these rules and profit from them. Those who break the rules are often dealt with harshly by the scientific community.

## **Objectivity versus Subjectivity**

While many arguments have been presented on the value of subjectivity in science, little evidence exists to favor such an approach. My original review of

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<sup>1</sup>One exception is that the statement on page 204 suggesting concern for objectivity (reconstructed logic) can be justified if it can be shown that *only* (emphasis added) reconstructed logic is useful in the development of scientific theories. I believe this statement to be fallacious.

the empirical evidence (Armstrong, 1979) led me to conclude that procedures used by many scientists are too subjective.

Cotton (1982) suggests that it is important to distinguish between the conduct of the research work and the reporting of this research. He favors objectivity in reporting.<sup>2</sup> In my review of the empirical evidence on the communication of scientific research (Armstrong, 1982b), I concluded that objectivity was lacking here also. For example, acceptance of papers is influenced by the institutional affiliation of the author (Peters & Ceci, 1982). Thus, I do not agree with Boal and Willis that the subjective approach will help theories to get a full hearing. My conclusion is that some theories will get a full hearing: namely, those theories by well-known scientists that confirm popular beliefs. Theories that challenge these beliefs will have more difficulty, especially if by unknown scientists.

Sound research, conducted in an objective manner, often does yield conclusions. In my opinion, evidence on which of several approaches is most useful provides the best way to communicate the results. Furthermore, I see much benefit in scientists becoming enthusiastic about their conclusions and trying to communicate them to large audiences.

The concern over shortcomings in objectivity has led to changes in scientific journals. These include formalizing the editorial policies related to objectivity (e.g., calls for papers that test a number of competing and reasonable hypotheses), preparing structured reviewing forms, asking authors to nominate a list of potential referees, and blind reviewing. These appear to be useful steps.

### Falsification

Science requires a combination of confirmation and falsification. Boal and Willis provide a good listing of reasons why falsification might be misleading, though they might also have included errors and cheating. Still, I believe that there is too little emphasis on falsification in management science.

Studies that falsify beliefs are apparently regarded as more useful by scientists. First, falsification studies apparently seem more important to their authors: Greenwald (1975) found that about half of those researchers who falsified the null hypothesis submitted a report for publication, while only 6% of those who failed to reject the null hypothesis attempted to publish. Reviewers also prefer falsification: The bogus paper in the study by Atkinson, Furlong, and Wampold (1982) was more likely to be accepted for publication when the rejection of the null hypothesis was statistically significant. Finally, other scientists also seem to regard falsification as more important: Christensen-Szalanski and Beach (in press) found that studies showing human judgment to be faulty were cited six times more often than studies showing good performance by judges. Studies confirming current judgmental procedures do not call for changes. Those that show current judgmental procedures to be wrong should receive more attention, not only for further testing, but also to determine better ways to make judgments.

Unfortunately, falsification is often misused in order to protect existing

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<sup>2</sup>Cotton favored subjectivity (advocacy) in conducting research, but he presented no evidence on this.

beliefs. For example, a scientist can create the illusion of falsification by using a widely held existing belief as the proposed hypothesis, with a trivial alternative as the null hypothesis. The trappings of science, such as mathematics and obscure writing, are then used to enhance the proposed hypothesis. The defeat of the null hypothesis is used to support the existing belief. My guess is that such a study will mislead scientists by adding to their confidence in this belief.

The scientist must keep in mind that falsification of important beliefs is dangerous work. Mahoney's (1977) experimental study shows that other academicians will claim that the quality of the research is poor. (If you don't believe experimental studies, it can be learned by experience: Just try to publish such a study.) Manwell (1979) reports that he nearly lost his position as a tenured professor by challenging existing dogma. Threats were made to Peters and Ceci as a result of their quasi-experimental study suggesting that existing journal review procedures are faulty (Sieber, 1983).

Some journals are trying to encourage the publication of papers that falsify existing important beliefs. One approach is to reserve space for the publication (without review) of highly controversial studies. Another approach is to publish controversial studies along with commentary (Harnad, 1979). Still another approach is to publish the referees' reports (with the agreement of the referees) for controversial papers. Finally, publication prospects for controversial papers might be improved by asking referees to evaluate the study when the results are withheld. (This procedure is used, when requested, by the *Journal of Forecasting*.) All of these steps seem to be useful in dealing with studies falsifying important beliefs.

### Conclusions

Arguments on the best way to do research should be subjected to empirical research. Much has already been learned from recent research. My conclusion from this research is that those who call for more subjectivity in scientific research or reporting are looking at a major shortcoming in scientific practice and are proclaiming it to be a virtue.

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