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Discovery and communication of important marketing findings: evidence and proposals

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Discovery and Communication of Important Marketing Findings: Evidence and Proposals

J. Scott Armstrong, University of Pennsylvania

Abstract

My review of empirical research on scientific publication led to the following conclusions. Three criteria are useful for identifying whether findings are important: replication, validity, and usefulness. A fourth criterion, surprise, applies in some situations. Based on these criteria, important findings resulting from academic research in marketing seem to be rare. To a large extent, this rarity is due to a reward system that is built around subjective peer review. Rather than using peer review as a secret screening process, using an open process likely will improve papers and inform readers. Researchers, journals, business schools, funding agencies, and professional organizations can all contribute to improving the process. For example, researchers should do directed research on papers that contribute to principles. Journals should invite papers that contribute to principles. Business school administrators should reward researchers who make important findings. Funding agencies should base decisions on researchers' prior success in making important findings, and professional organizations should maintain web sites that describe what is known about principles and what research is needed on principles.

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Discovery and Communication of Important Marketing Findings: Evidence and Proposals

Introduction

A colleague told me that when his son asked him what he did, he answered, "I am a management scientist." "What do scientists do?" his son asked. "They discover things," my colleague said. "What have you discovered?" The rest of the conversation, my colleague said, was short. Similarly, when I have raised this issue about important discoveries in marketing with my colleagues, the initial reaction is that I am making a small joke. When I persist, it has been difficult to get a long answer. When I provide a list of leading marketing scientists and ask colleagues to tell me about important findings by each of these scholars, again the answers have been short.

Progress of science in marketing concerns some scholars. Anderson (1994) criticizes the ability of marketing science to deliver solutions to business problems. Bloom (1987), who reviewed the quality of research on marketing, the AMA Task Force on Marketing (1988), and Wells (1993), who assessed consumer research, also concluded that progress is slow. Similar concerns are expressed in other areas of management science; Boland et al. (2001) lists 12 sources lamenting the problem.

When important discoveries are made in chemistry, psychology, medicine, or engineering, mass media often report on them. Inasmuch as marketing is a central part of our lives, one would expect the mass media to show some interest in findings related to marketing. In October 2000, I conducted a computer search of the *NY Times* and the *Wall Street Journal* from 1980 to 2000, using the terms "marketing and academic." I found no studies reporting on findings by marketing professors. With a more careful search, one would be likely to find something, but the point is that it is not easy.

Marketing practitioners and researchers looking for useful findings are faced with a large number of academic papers. Although these papers have passed through a rigorous screening process, few seem to contain important findings.

Most conclusions in this paper come from my review of empirical research on scientific publication. The review incorporates earlier literature reviews, such as a meta-analysis on peer review that found 68 empirical papers (Armstrong 1997). I generalize from these studies, most of which are from outside the field of marketing, and I indicate if my conclusions are not based on empirical evidence.

First I provide suggestions on how to decide whether a finding is important. Given the apparent scarcity of important findings, I consider barriers to discovery and communication. Proposals are then made to increase the number of important papers in marketing science.

Criteria for Importance

For findings to be important, they should be replicated, valid, and useful. Findings that are also surprising are especially important. These criteria, borrowed from commonly accepted ideas about research, are unlikely to be controversial. They are discussed below along with some related research.

Replicated

A replicable study is one that others can repeat, using the same procedures, and obtain similar results. According to Rosenthal and Rosnow (1984, p. 9) "Replicability is almost universally accepted as the most important criterion of genuine scientific knowledge." Leone and Schultz (1980) claim that replication is the key to generalizations in marketing. Unfortunately, researchers often think that they have assessed replicability when they perform tests of statistical significance. Oakes (1986) surveyed 70 experienced academic psychologists and found that 60 percent believed that an outcome that is significant at the .015 level has a 0.99 probability of being statistically significant if the study were replicated. This is not so, as Oakes showed. Kerr, Tolliver and Petree (1977, p.138, 140) surveyed 429 editors and review board members of 19 management and social science journals; they found a bias against publishing papers with replications even if the replications were done competently. Similar results were obtained by Rowney and Zenisek (1980) for psychology journals and by Neuliep and Crandall (1990) for social science journals.

Hubbard and Armstrong (1994), using a probability sample, examined half of the papers published in three marketing journals from 1974 to 1989. There were no direct replications and few replications with extensions (Table 1). (Ehrenberg's work stands out as a major exception. See a summary of his work at http://msc.city.unisa.edu.au/msc/JEMS/ehrenberg.html). The proportion of papers describing replications decreased from the 1970s to the 1980s for all three journals: for example, for the *Journal of Marketing Research*, the percentage went from 2.8 to 1.1 Although the optimum number of replications is difficult to say, to obtain a benchmark, the study examined a 25 percent sample of papers from the *American Economic Review* from 1965 through 1989 and found that 21 percent of the 698 empirical studies were extensions.

Table 1
Marketing Extensions as a Percentage of Empirical Studies
(number of empirical studies)

	1974-1979	1980-1989
Journal of Marketing	4.2 (95)	2.7 (112)
Journal of Consumer Research	2.5 (79)	2.2 (183)
Journal of Marketing Research	2.8 (176)	1.1 (190)

The importance of replications with extensions is shown by the fact that nearly half of them fail to provide full support for the original study. Hubbard and Vetter (1996) examined the conclusions reached in 33 published extensions in marketing; only 21 percent of the extensions provided full support (see Table 2). They conducted similar analyses for accounting, economics, finance, and management, and the results were similar except in management, where half the studies provided full support and only 14 percent conflicted. These results overstate the negative, because those who did the replications were often concerned only with whether the results in the replication were also statistically significant (almost all original studies reported statistically significant results). A more appropriate approach would be to examine whether the directions of the results were the same. Nevertheless, Hubbard and Vetter's results indicate that replications are needed to test findings and to better define the conditions under which they apply.

Table 2
Outcomes of Replications with Extensions in Marketing
(33 studies)

	Percentage
Conflicting results	46
Partial support	33
Full support	21

Valid

To be valid, a test must assess what it purports to test. The test can be judged to have face, construct, or predictive validity. Of these qualities, face validity provides the weakest assessment. Even if done with care, such as using independent evaluations by a diverse group of experts, face validity tends to reject new findings as shown by Mahoney's (1977) experiment on peer review.

Construct validity depends on whether different approaches yield similar conclusions. For example, in our study on the effects of market-share objectives on profits (Armstrong and Collopy 1996), we summarized prior research consisting of nearly 30 prior empirical studies, ran 23 laboratory experiments on 43 occasions, and analyzed 54 years of field data for 20 companies. The results from all approaches showed that market-share objectives harm profits.

Scandura and Williams (2000), in their content analysis of 732 papers from three management journals, found that most studies did not examine construct validity. Furthermore, the percent that did so went from 49 in the 1985-87 period to 25 in 1995-1997.

Predictive validity is the ability of a theory or method to make accurate forecasts. Some researchers argue that predictive validity is the most important test of a theory. In an attempt to demonstrate how findings from academic research can produce theories that have predictive validity, I examined research on consumer behavior (Armstrong 1991). This seemed like an ideal area for such a demonstration because papers in this area typically begin with extensive descriptions of prior findings and the results are almost always consistent with the authors' hypotheses. Short descriptions for each of 20 studies from the *Journal of Consumer Research*

(JCR) were provided to academics specializing in consumer behavior. They were asked to predict the direction of the results for the 105 hypotheses tested in these studies. It was expected that their knowledge of findings in the area would enable them to make accurate predictions of the outcomes. To provide a benchmark, 16 marketing practitioners did the same task; they were expected to make reasonably accurate predictions based on their experience. Finally, I asked 43 high school students, as we expected that some results could be predicted based on common sense. The academics were no more accurate than high-school students and none of the groups did better than chance (Table 3).

To find out whether my results were surprising, I described the study in a questionnaire sent to the members of the editorial board of the *JCR* and asked about their expectations for the results of my study. Forty-three board members responded. Their expectations differed substantially with respect to the predictive accuracy achieved by academics, practitioners, and high-school students. The board members also varied greatly among one another in their expectations.

Table 3
Predictive Validity for Hypotheses on Consumer Behavior

	Percentage of correct predictions (# of predictions)	**Percentage of correct predictions expected by JCR Board**
Academics	51.3 (360)	80
Practitioners	58.2 (270)	65
High-school students	56.6 (1,106)	55

I am not aware of any studies that examine the frequency of tests of validity in the marketing literature. My impression is that validity tests are rarely used.

Useful

To be regarded as useful, findings should suggest changes that would eventually (or possibly) have substantial benefits. The benefits might accrue directly to practitioners, consumers, or policy makers. Alternatively, the value might accrue indirectly through researchers.

Usefulness is not common in academic research. Miner (1984) examined 32 well-regarded theories in organizational behavior and concluded that only four had been shown to be useful.

Few papers in marketing journals would fall into the category of having findings that are useful. Examine the abstracts of papers from these journals. One must look hard to find studies that seem useful, especially, I believe, in the more prestigious journals.

To assess whether researchers in marketing provide useful findings, one might examine basic textbooks. In many professional fields, such as engineering, chemistry, medicine, and law,

one would expect to find a summary of the most useful findings in the introductory textbooks. Armstrong and Schultz (1993) examined nine introductory marketing textbooks published between 1927 and 1988. Four of their doctoral students found 566 normative statements related to making marketing decisions; the results were that

- none of the statements was supported by empirical research, and
- the four raters agreed on only 20 statements as being meaningful.

Twenty marketing professors then examined the 20 meaningful statements and judged that:

- none were surprising, and
- nine were nearly as correct when their wording was reversed.

Here is an example of one of the statements that scored best in this study: "A careful analysis of the causes of sales returns should be carried out by the retailer with a view to overcoming them." In short, we found no useful findings. This search was confined to basic textbooks; it might be that research content is perceived to harm the potential sales of such books. However, some specialized textbooks contain important findings, for example, Rossiter and Percy (1997) and Nagle and Holden (2002).

Do researchers in marketing consider usefulness when they decide on research topics? Some claim that their work on topics that interest them is likely to lead to something useful. When I have asked researchers why their problems are important, their answers are typically vague. After spending over 40 years as a practitioner, student, researcher, reviewer, and editor, I cannot recall a single instance of a marketing study that originally appeared to be useless that later turned out to have value. I have invited readers to send me examples of such papers.

Surprising

Surprising findings differ from current practice or current beliefs. A finding can be surprising to practitioners, consumers, or researchers. Surprising findings may be innovative or new, although not always. When the problem is important, surprising findings are likely to be controversial or unpopular. When it meets the other three criteria (replicability, validity, and usefulness), a surprising finding is often viewed as especially important.

Findings need not be surprising to be important. Indeed, replication studies can make important contributions by supporting findings, especially early in the life of a theory. Studies can also assess the strength of a relationship and the conditions that affect the relationship. For example, while it is well known that an increase in price will typically lead to a reduction in units purchased, studies on price elasticity are useful for decision makers (e.g., see Tellis 1988). Researchers have also identified conditions under which the relationship is the opposite of the normal relationship (the scarcity studies; see the meta-analysis by Lynn 1991).

In a mail survey, Armstrong and Hubbard (1991) asked editors of 20 leading psychology journals, "To the best of your memory, during the last two years of your tenure as an editor of an APA journal, did your journal publish one or more papers that were considered to be both controversial and empirical? (That is, papers that presented empirical evidence contradicting prevailing wisdom.)" Sixteen editors responded. Seven of them could remember none ("unfortunately," according to one editor). Four said "yes" and indicated that there was one such paper. Three said that there was at least one, and two said there were several. The journals published few papers with controversial findings partly because the editors received few such papers. For example, six editors said they did not receive a single such paper. Furthermore, when such papers were received, reviewers judged them harshly. Only one of the editors could recall a controversial empirical paper that its reviewers unanimously recommended for publication. However,, that editor told us that he had invited the submission of this paper and had selected reviewers he expected would view the paper favorably. Thus, for a sample representing 32 journal-years, we were able to locate only one paper with controversial results that was unanimously recommended for publication by the reviewers. I expect that this situation also exists in marketing journals.

Lee (1980) examined the management techniques described in textbooks and journals. He concluded that they typically rested on commonly held beliefs.

Identifying surprising findings is not easy. Slovic and Fischhoff (1977), in three experiments, found that when people evaluate the results of scientific experiments, they tend to believe that they knew all along what the results would be. Gordon, Kleiman and Hanie (1978) asked non-psychologists to predict the outcomes of 61 published studies, based on descriptions of the studies. On average, they correctly predicted three out of four findings. Mischel (1981) found that ten-year old children could predict the outcomes of 12 of 17 classic studies in psychology. Thus, many classic studies did not have surprising findings. Do some of the most highly respected studies merely affirm the obvious?

Gage (1991) reviews receptions to "obvious" findings and reports on follow-ups to Mischel in unpublished doctoral theses, by Baratz in social research in 1983 and by Wong in education in 1987. The findings supported those by Mischel. People tend to regard even contradictory research results as obvious.

Examples of Important Findings

Using the above criteria, I have asked marketing professors to identify important findings in marketing. This question is difficult to handle. In addition to the complexity of judging importance, respondents had to decide what topics marketing includes (for example, whether marketing is relevant to public policy issues). Based on their suggestions and on my own judgment, I provide examples of important findings:

- Deregulation increases net benefits for consumers.

- Formal planning generally improves corporate profits.
- Research-based design procedures reduce total survey error.
- Judgmental bootstrapping produces more accurate forecasts than unaided judgment.
- Relatively simple forecasting methods are as accurate as complex methods.
- Perceptions of sunk costs mislead consumers and marketing managers.
- Most marketing managers will commit what they think are socially irresponsible acts if they are led to believe that doing so is part of their job.
- Structured judgmental procedures are likely to improve the selection of marketing personnel.
- Conjoint analysis is likely to improve product design.
- Quasi-contracts (contracts are specified at the time of sale and buyers are reminded after a mishap about the promised payout, and are then asked whether the contract is acceptable) can reduce product liability costs. People typically decide not to sue in such cases.
- Perceptions of fairness affect the amount that people are willing to pay for a product.
- When someone possesses an object, its perceived value to that person increases.

In short, given the four specified criteria, important findings do exist. That said, it has not been easy to identify them.

Barriers to Important Findings

Judging from the empirical evidence on scientific publication, many of the barriers to the discovery and communication of important findings can be traced to the reward system for scientists. Of course, the history of science is filled with cases of scientists who made major discoveries in spite of the reward system. Barber (1961) and Campanario (1998a) describe examples of major scientific achievements that were resisted by their peers.

The current reward system is based heavily on peer review. Journals rely on peer review in accepting papers and educational institutions evaluate faculty based on publications and additional peer review.

Certainly peer review is valuable. It leads to improvements in papers. This paper, for example, was greatly improved by peer review. As an editor, I thought that researchers were foolish to submit papers without first obtaining review from peers, yet it was obvious that some did so. In a survey by MacNealy, Speck, and Clements (1994), 80 percent of the 96 authors responding said that they found the reviewers' suggested revisions to be "reasonable." In Bradley's (1981), survey of 361 statisticians and psychologists, 72percent thought, "The net effect of refereeing upon the quality of the article was to improve it." Fletcher and Fletcher (1997) report on an experiment comparing medical research manuscripts as originally received with the same manuscripts after revision based on reviewing and editing. Researchers (who were blind to the treatment) rated the revised papers as superior on 33 of 34 elements of quality. However, peer review is misused. Gans and Shepard (1994) surveyed eminent economists and found that they believed that journals were unreceptive to their most important papers. Some of them thought that journals provided a poor way of communicating important findings. Horrobin (1990) described important advances in medical science and showed that the peer review system ignored the basic objectives of medicine, which are to cure, relieve, and comfort. As a result, there have been extensive delays in publication of important medical findings.

In the remainder of this section, I review the problems with peer review. This sets the stage for proposals to improve the system.

Process is flawed

Researchers, sometimes working in teams, spend hundreds of hours working on specialized topics, often collecting empirical evidence and applying formal analytical techniques. They write papers and usually benefit from pre-submission peer reviews. They strive to follow standards for scientific work, and sign their names to their work. Their reputations depend to some extent on the quality of their papers.

In contrast, papers are reviewed by people who are working in related areas but generally not on that same problem. So the reviewers typically have less experience with the problem than do the authors, although in some aspects of the research, such as methodology, reviewers may have more expertise. Typically, those who are less capable judge work done by the best researchers.

Peer reviews are typically based on unaided judgment. They are the reviewers' opinions about the findings or the way the study was done.

Reviewers generally work without extrinsic rewards. Their names are not revealed, so their reputations do not depend on there doing high quality reviews. On average, reviewers report spending between two and six hours in reviewing a paper (Jauch and Wall 1989; King, McDonald, and Roderer 1981; Lock and Smith 1990; Yankauer 1990).

Reviewers seldom use systematic procedures, and they do not contribute analyses. As an author and editor since the 1960s, I have encountered only one occasion in which a reviewer tried to replicate an aspect of a study. Reviewers are not held accountable for following proper

scientific procedures. In short, they match their unaided opinions against the scientific work of the authors.

Authors are often critical of the quality of the reviews they receive. Bradley (1981) surveyed authors about their experiences on the "last compulsorily revised article published in a refereed journal." When asked whether the changes advocated by the referees were based on "whim, bias, or personal preference," only 23 percent said none were, while 31 percent said the changes improved important aspects of their papers. Forty percent of the respondents said that the referees had not read their papers carefully.

Editors decide whether to publish a paper based primarily on two or three reviews. Authors may appeal the decision, and some journals have formal procedures for appeal, but editors apparently do not often change their decisions. The editors of the *American Sociological Review* agreed with the authors on only 13 percent of the decisions that were appealed (Simon, Bakanic and McPhail 1986). I feel fortunate: when I ranked my own published papers on importance, none of the top 20 was reviewed favorably. My career has depended upon finding editors who did not follow reviewers' recommendations.

Unreliable

Reviewers' recommendations often differ from one another. In the behavioral sciences, the reliability of recommendations for a sample of over 3,000 papers was low at r = .2 (Cicchetti 1991). Many authors have probably received contradictory reviews. For example, here are reviews for one of my papers: Referee #1: " . . . The paper is not deemed scientific enough to merit publication." Referee #2: " . . This follows in the best tradition of science that encourages debate through replication."

Uninformative to readers

Peer review results in either acceptance or rejection of a paper. Readers get no information as to which of the accepted papers are most important or as to what are the perceived shortcomings of the papers. The design of the peer review system restricts the market for ideas.

Weak evidence on quality

Gottfredson (1978), using from one to three experts per paper, found that their ratings of the "quality" of 387 published psychology papers had a weak relationship to the number of times they were cited over an eight-year period.

Although peer review improves papers, errors can be found in nearly all published papers. Consider such a simple measure of quality as the accuracy of references. Evans, Nadjari, and Burchell (1990) studied three medical journals and found a 48 percent error rate in the references. They stated, "a detailed analysis of quotation errors raises doubt in many cases that

the original reference was read by the authors." Linsky (1975) found that 33 percent of the references in papers on survey research were incorrect. Eichorn and Yankauer (1987) found that 31percent of the references in public health journals contained errors, and 3percent of these were such that they could not locate the source items. Faunce and Job (2001) found a 32percent error rate in five experimental psychology journals published in 1999. Most important, Eichorn and Yankauer (1987) found that the authors' descriptions of previous studies differed from the original authors' interpretations for 30 percent of the citations, with half of these descriptions being unrelated to the authors' contentions. Harzing (2002) provides 12 guidelines for good academic referencing. She then shows how these guidelines are routinely violated and that the effects can lead to incorrect conclusions

Wolin (1962) replicated seven studies in psychology and found gross miscalculations in three. Corrections would have led to substantial changes in the conclusions.

Murray (1988) evaluated the statistical procedures in 28 papers that had been published in the *British Journal of Surgery* and found that four papers should have been rejected, seven needed major revisions, and 11 needed minor changes. Thus, only 21 percent of the papers passed this post-publication peer review that considered statistical procedures alone.

Stewart and Feder (1987) provide further evidence of errors in published papers. They studied the 18 papers John Darsee published between 1978 and 1981, before he admitted to scientific fraud. The papers had been reviewed and published in major biomedical journals. Stewart and Feder found errors in 16 of the 18 papers. On average, they found 12 errors per paper, some minor but many major. For example, according to Darsee, the father in one family had his first child at age eight and the next at age nine.

Reviewers' ability to identify even such major transgressions as plagiarism seems weak. Epstein (1990) conducted an experiment in which he sent out two modifications of a previously published paper that were intentionally flawed methodologically. He submitted them for publication to journals in social work, sociology, psychology, counseling, and medicine. Reviewers from only two of the 110 journals to which he sent the paper noted that it had been published previously. This finding occurred despite frequent prior citations to the paper. Although the study's control group had been omitted from the paper, few reviewers mentioned this as a problem. Epstein concluded that only six of the 33 reviews received were competently done.

Biased against replications

Peer reviewers tend to focus on quality and originality. A survey of journal reviewers in 43 disciplines showed that "originality" was the first-ranked consideration in reviewing papers (Juhasz et al. 1975). Replications fail on this criterion, of course.

Ignores usefulness

In a survey by Lindsey (1978, pp. 18-21), editors of journals in psychology, social work, and psychology rated usefulness ("the value of an article's findings to affairs of everyday social life") tenth out of 12 criteria. Similar results were obtained in a survey by Beyer (1978): "applicability to practical or applied problems" ranked last for the ten criteria presented to editors of physics, chemistry and sociology journals, and it was next to last for political science. Would results be similar in marketing?

Rejects surprising findings

> "The human understanding when it has once adopted an opinion draws all things else to support and agree with it. And though there be a greater number and weight of instances to be found on the other side, yet these it either neglects and despises, or else by some distinction sets aside and rejects, in order that by this great and pernicious predetermination the authority of its former conclusion may remain inviolate." Bacon, Francis (1620), "*The New Organon and Related Writings*." (Later edition 1960, Liberal Art Press, New York.)

As seen in the quote from Francis Bacon, the rejection of new findings has long been recognized. In a survey of 163 accounting faculty authors, Borkowski and Welsh (2000) found that 73percent believed that confirmatory biased existed, while the rest were uncertain.

Kuhn (1962), in an impressionistic review of the history of science, used the term "paradigm shift" to refer to findings that meet all of our four criteria. He suggests that researchers resist important findings that are surprising. Experimental studies in psychology support Kuhn's conclusion. Goodstein and Brazis (1970) split 282 psychologists into two groups and asked each to review one of two abstracts that were identical except for the results. The psychologists rated those in which the results were in accord with their own beliefs as better designed and said that they were more suitable for publication. Mahoney (1977), in an experiment involving 75 unsuspecting reviewers, found that the reviewers given a bogus paper with controversial findings rejected it, while those given an identical paper that supported conventional beliefs accepted it. They based the rejection on poor methodology. Experiments by Abramowitz, Gomes, Abramowitz (1975) and Epstein (1990) yielded similar results.

In laboratory experiments, Koehler (1993) found that, when given results on a controversial issue, graduate students and practicing scientists rated the quality of a research report higher if it agreed with their prior beliefs. In a nonexperimental study, Smart (1964) found that 30 percent of studies in doctoral dissertations in psychology failed to support a dominant hypothesis, but this dropped to 20 percent of papers presented at the annual American Psychological Conference, and to 10 percent of papers published in journals.

Careful study is not required to see that reviewers resist surprising findings. I have documented such resistance to my own papers (Armstrong 1996). For example, our paper showing that the use of the Boston Consulting Group (BCG) matrix is harmful to profits was

originally submitted to a journal in mid-1989. The reviewers often differed with one another. When two reviewers stated, "I cannot imagine obtaining the results reported here," and the "conclusion . . . seems to lack a certain face validity," an editor summarized these reviews by stating that the results were not controversial because "the BCG portfolio matrix has been widely criticized in the popular and academic literature over the past 10 years." One reviewer said, "The author shows good knowledge of the relevant literature." Another said, "The author's review of the literature is scant" (but did not cite any missing research studies). As to whether firms use the BCG matrix, one reviewer said, "The author makes no attempt to explain why so many firms appear to be using an approach which has so little to recommend it." Another said, "I do not think that many managers rely on the BCG approach." One reviewer said "I cannot help but think that the undergrads cannot rise to the occasion and that the task might have been too demanding for them," while another reviewer said that the task was "too simple."

The reviewers presented no evidence and did not otherwise support their criticisms. For example, one said, "The task was unfair to the BCG matrix," but did not suggest what type of test would have been fair. A reviewer said that the BCG was "the weakest of the portfolio matrices," but did not say how he had arrived at this conclusion.

Over its 3-year review period, the paper went through seven rounds of reviews with 14 referees at four journals. It had many supporters, but some referees recommended rejection each time. Despite continued improvements in the study, the reviews did not become more favorable. In the end, an editor agreed with our rebuttal and accepted the paper despite negative reviews (Armstrong and Brodie 1994). Wensley (1994) published a comment.

Is it possible that we are wrong about the dangers of the BCG matrix? Of course. But the proper grounds for refutation should be in replications and extensions, not whether referees agree with the findings.

Communication of Findings

Although the percentage of published research that is useful is low, I believe that much useful research is published. However, research findings seem to be ignored.

For example, Gage (1991) described a study to determine whether experts were aware of key principles in education. Twelve findings were selected from a handbook of research on teaching as the basis for a questionnaire to groups with different levels of expertise: undergraduate engineering students, undergraduate psychology students, teacher trainees, and experienced teachers. Teachers were no more accurate than the other groups when asked to select the true findings.

Dakin and Armstrong (1989) examined the impact of empirical findings on the beliefs of 21 personnel consultants who specialized on selection. Their ratings on the validity of 11 selection criteria were uncorrelated with well-established findings from hundreds of studies published over half a century

Cierpicki, et al. (2000) examined the opinions of 15 senior marketing managers and market researchers. They asked them what factors contribute to the success of new products. Of the 15 guidelines the respondents provided, three were tautologies, six had some empirical support, and six were contradicted by empirical studies.

Helgesen (1994) conducted a survey of 40 respondents from ten advertising agencies in Norway on the use of academic research on advertising. He concluded that academic research on advertising was "distant and largely unknown."

No attempt is made here to provide an exhaustive summary of findings on the topic. What I have found seems to reinforce what people commonly believe. That is, important findings in the management and social sciences are often ignored.

What Can Individual Researchers Do?

The responsibility for important research depends primarily on individual researchers. If they produce nothing of value, all else is meaningless. One issue is whether they set out to do something important.

No lack of important problems exists. Here are some important (but perhaps dangerous) questions for research in marketing. No doubt readers could add others. Few marketing academics have addressed these problems.

- Under what conditions is regulation helpful to consumers?
- Under what conditions can the government satisfy consumer needs more effectively than can the private sector?
- When is it beneficial to force people to use certain products (e.g., education)?
- When does increased consumption of goods and services beyond the average level increase happiness?
- Are there any conditions under which mission statements improve or harm the profitability of firms?
- Why do marketing managers use procedures that are expected to have detrimental effects (e.g., BCG matrix, focus groups)?
- How can companies defend themselves against false allegations (e.g., cancer is caused by breast implants)?

How can one strive for important findings? In my opinion, the primary goal of a management scientist is to do research that will contribute to better decision making. One can search for topics by thinking about principles: What actions should be taken under the specified

conditions? The researcher should ask how the study might contribute to developing evidence about principles.

Prior to doing a study, researchers might also try the “press release test”: Assuming that your study was as successful as it could be, write a press release telling why the findings are important. If a researcher cannot write an interesting press release, he should consider abandoning the project.

To obtain important findings, researchers might use the method of multiple competing hypotheses. In particular, they should demonstrate that their proposed methods or concepts lead to better decisions or to better forecasts than current approaches. Chamberlin (1890) claimed that sciences in which researchers test competing hypotheses develop more rapidly than those that do not. Evidence supports this view, as summarized in a review of 14 empirical studies by Armstrong, Brodie, and Parsons (2001). Furthermore, according to their survey of 38 marketing professors, the use of multiple hypotheses was generally regarded as a desirable research strategy. On the other hand, Armstrong (1980b) found evidence that advocacy of a single hypothesis might be a good strategy for the advancement of *scientists*. For example, in a study of prestigious space scientists, Mitroff (1972) concluded that they became prestigious because they were advocates for their favored hypothesis and they tried to suppress competing viewpoints.

Leone and Schultz (1980) concluded that few universal generalizations could be drawn from research in marketing; nearly all findings depend on conditions. Sutton and Rafaeli (1988), for example, found that smiling by convenience store clerks was associated with low rather than high sales. It turned out that when store are busy, the clerks are under pressure to perform and thus have less time to smile. This finding does not mean, however, that clerks should not smile during slow times. Unfortunately, researchers in marketing seldom include conditions in their hypotheses. Armstrong, Brodie, and Parsons (2001), in their study of 1,700 empirical papers from six major marketing journals, found that only about 12 percent of the studies included conditions along with the hypotheses.

The research cited above suggests that researchers often do not follow procedures that are likely to advance marketing science. Instead, they seem motivated to advance their careers. To be successful in the current system, one should follow the "author’s formula." I developed this set of guidelines from a review of the empirical research (Armstrong 1982). A more extensive examination of 68 empirical studies added support (Armstrong 1997). The author's formula suggests that to get their work published, researchers *should not:*

- examine important problems,
- challenge existing beliefs among scientists,
- obtain surprising results,
- use simple methods,
- provide full disclosure, or

- write clearly.

Knowing that reviewers will believe that most findings are obvious, researchers who obtain surprising findings can ask experts to predict the outcomes of their studies. I have asked experts to make predictions with some of my papers, such as noted above in Table 3. Lazarsfeld (1949) in a paper dealing with his famous study, *The American Soldier*, began with list of six obvious findings from his book such as "As long as the fighting continued, men were more eager to return to the States than they were after the Germans surrendered [because during the fighting, soldiers were in danger of getting killed, but after the surrender there was no such danger]." After suckering the reader, Lazarsfeld reported that all of these obvious findings were false.

Researchers with important findings should violate the author's formula. For example, they should write intelligibly, particularly in abstracts. However, this advice creates a problem for the advancement of scientists because obscure writing confers more prestige on the author. This conclusion is shown in a study that asked academics to evaluate passages from journal articles (Armstrong 1980a). When the passengers were poorly written, the academics had a higher opinion of the author. The same study showed that journals that were less intelligible were Regarded as more prestigious than journals that were more intelligible. The implication is that researchers who have nothing important to report should continue to write in an incomprehensible manner. Metoyer-Duran (1993), in a related study of the journal, *College & Research Libraries*, found that reading ease scores were slightly better for 119 rejected manuscripts than for 82 published papers.

Researchers' responsibility does not end with publication. They should also try to circulate news of their findings, assuming they are worthwhile. Few people are likely to become aware of the findings if readership is restricted to those who use the journal. One way to make the findings available is to put them on a web site, along with a management summary. For example, I have put nearly all of my papers in full text on jscottarmstrong.com; when using key words for research areas, such as "judgmental bootstrapping," "forecasting", "combining forecasts", "escalation bias", or "replication in marketing" on a Google search, my papers are often listed as the first item (or near it).

What Can Journals Do?

Here are some specific recommendations that would help journal editors to publish important papers. In most cases, these recommendations would mean establishing explicit policies and using a set of procedures to ensure that the policies are implemented. Following these policies requires a strong editor who has been done important research. Franke, Edlund and Oster (1990), analyzed 17 management journals over a 12-year period and concluded that journals were most successful when their editors were successful researchers.

Offer to publish all papers:

This recommendation is most important. Print only those sections of papers that cover what the authors discovered, how they discovered it, and why it is important. Allocate space according to the importance of the findings as judged by the editor. Data and a full disclosure of the methods should be posted on the internet, thus allowing for an increase in the number of published papers. In effect, all papers could be published.

This proposal might sound unusual to those in the social science. However in the hard sciences, most papers are published, and serious researchers do not seem to be concerned about whether their paper will be published (Cicchetti 1997; Martinko, Campbell, and Douglas 2000).

Journal editors might attach ratings to the papers as signals to readers. They could have a range of ratings say from, "recommended by review board with great enthusiasm," to "papers without merit." Reviews would be published along with the papers, with brief comments in the printed version and full text online. Authors whose papers receive poor reviews could withdraw them.

Educational institutions will eventually consider the number of publications to be of little importance in making hiring and promotion decisions. Instead, they should focus on whether the findings were important, whether peer reviews were favorable, and how much attention the paper attracted. I expect that the number of useless publications would decrease if researchers received no credit for simply publishing and if their work were subject to open peer review.

Do not ask reviewers for publication recommendations:

Journal editors should not ask reviewers for publication recommendations. Editors should make these decisions. Although many editors are convinced that they apply their own judgment, Munley, Sharkin and Gelso (1988) and Marsh and Ball (1989) found that editors' decisions to publish are highly correlated with reviewers' recommendations. Bakanic, McPhail and Simon (1990), in a review of papers submitted to the *American Sociological Review* from 1977 to 1982, found that "a single recommendation to reject often resulted in a rejection." Rothwell and Martyn (2000) found that, although reviewers' recommendations were unrelated to one another for journals in clinical neuroscience, editors were much more likely to accept papers on which two reviewers agreed. Weller (1990), in a study of medical journals, found that when two or more reviewers were in agreement, the editors followed their recommendations 85 percent of the time. Cicchetti (1997), in studies of reviews for the *Journal of Applied Psychology* found that if two reviewers recommended "accept," the paper was accepted 88 percent of the time; in contrast, if the recommendations were one to accept and one to reject, the paper was accepted 14 percent of the time. Cicchetti also provides similar evidence for other journals. For example, for a medical journal, Thorax, of 107 papers recommended for publication by both reviewers, all were published; for 107 papers where both recommended rejection, none were published. Given that important papers are almost always rejected by at least one reviewer, leading journals would be unlikely to publish important papers.

While waiting for journals to act on this advice, I invite reviewers to refrain from making publication recommendations to editors. When I have asked reviewers to do this in my role as editor, my impression has been that they have come back with more substantive suggestions for improving papers.

Invite researchers to submit papers:

When you invite researchers to submit papers, the implied contract is that you will publish the paper, although you may request revisions. In a survey of editors of 28 education journals, Rodman and Mancini (1977) found that 89 percent of them published "inside track submissions." The successful *Journal of Economic Perspectives* publishes only invited papers.

Laband and Piette (1994), in a study of 28 top economics journals, found higher citations for papers published by authors who had obvious connections to the editors. Similarly, in a study of 15 accounting journals, Smith and Laband (1995) found that citations for articles for which they could identify an author-editor connection were more than triple those for articles for which they saw no connection. In the five years after publication, 63 percent of the papers by authors with no editorial connections were never cited, whereas only 31 percent of those with a connection were not cited. Campanario (1996) found a positive relationship between the percentage of papers by authors connected to a journal and the journal's citation impact factor.

I suggest that editors of journals invite papers on principles that they judge to be important but where little is known. For example, I have translated this advice into a list of needed research in the area of forecasting (see the researchers' page at forecastingprinciples.com). If they agree to study the problem and can provide a decent letter of intent, the papers would be accepted subject to revision.

Invite researchers to suggest reviewers:

Authors are likely to know who would be able to provide useful reviews. The *Journal of Consumer Research* has a well-stated policy in this regard, and it appears in the first paragraph in their section on the review process:

> "If authors believe there are individuals who would be particularly appropriate as reviewers or other individuals who, while knowledgeable on the topic of research, would not be appropriate, we welcome that input. While authors' suggestions will be given consideration, assurance cannot be provided that suggestions will be followed."

Solicit open peer review:

Editors could solicit open (signed) peer reviews and add them to the reviews on the journal's web site. Authors could publish responses to the reviews on the site. In addition,

authors could make corrections to the paper with hyperlinks for the original text. As shown by Friedman (1990), the current system is not well suited for making corrections after publication, nor is it easy to track corrections.

Assess importance:

Journals should state that their primary goal is to publish papers with important findings. Reviewers should be asked to evaluate the importance of the findings. For guidance, editors can use follow-up surveys of readers to assess what types of findings were useful.

As part of importance, editors might want to assess how surprising the results might be for each study. Invite authors to provide a thumbnail description of the study along with a listing of possible outcomes. This has been done at the *International Journal of Forecasting*, but few have taken advantage of this offer.

Use structured rating sheets:

If journals insist on getting recommendations from reviewers, the use of a structured rating sheet can help to improve the process. Cicchetti (1997) reviews evidence from four studies showing that structured rating sheets produce substantial improvements in the reliability of recommendations.

Structured rating sheets can contribute also to validity. For example, if the journal is intent on publishing papers that challenge existing dogma, they could ask for ratings on this dimension. One way this could be done is to provide short "results-free" descriptions of studies on controversial topics and to ask the reviewers to predict the outcomes prior to reviewing the complete paper.

Structured rating sheets can also be used to assess the importance of a paper's contribution. That is, what improvements does the paper provide over current practice, or over other recommended procedures? This calls for testing multiple hypotheses, a procedure often recommended but rarely used in marketing. Armstrong, Brodie, and Parsons (2001) examined 1,700 empirical papers from six major marketing journals published between 1984- 1999. Only 13 percent used the method of competing hypotheses, and when they did, they typically used closely related hypotheses rather than embracing substantially different viewpoints; only three percent of these studies compared hypotheses that differed substantially. The current published policy of the *Journal of Marketing Research* is exemplary in this regard: " . . methodological articles must go beyond a simple presentation of a new method. The new method should be compared with alternative approaches for attacking a problem. The article should indicate the circumstances under which the new method is superior and why it is superior. Authors are encouraged to include empirical illustrations comparing a new method with current methods." Unfortunately, this policy does not cover other empirical papers, such as those in which authors claim to have discovered a new phenomena or a new principle. Furthermore, to my knowledge, no procedure exists for enforcing this guideline.

Structured rating sheets might serve as useful checklists for reviewers even if they were not asked to make publication recommendations.

Provide full disclosure for papers published:

To identify the conditions under which the findings hold and to aid replications, journals should require authors to include full disclosure about the methods, data, and source of funding. Historically, however, published studies fail to provide sufficient information. As a result, those seeking to replicate studies must often contact authors. Madden, Franz and Mittlestaedt (1979) surveyed authors of 60 papers in proceedings from the annual conferences of the AMA and the Association for Consumer Research from 1975 through 1977; half of the authors failed to provide any of the information that would be needed to do a replication. Reid et al. (1982) requested information from the authors of 99 empirical papers published in the 1978 and 1979 issues of the *Journal of Consumer Research*, the *Journal of Marketing,* the *Journal of Marketing Research,* the *Journal of Advertising,* and the *Journal of Advertising Research*. They received materials from only half of the authors.

The requirement that authors identify their sources of funding is based on research showing that the funding source influences the reported findings. Cho and Bero (1996) and Davidson (1986) found substantial biases in the funding of research on medical treatments. Campanario (1998b) reviewed evidence on this from the biomedical field.

Dedicate space to studies with controversial findings:

A separate editor should control this space. Such space has been provided for me as a contributing editor of *Interfaces* since 1982, and I have published some controversial papers. Unfortunately, I seldom receive submissions.

Dedicate space to replications:

The first item in the *Journal of Marketing's* editorial policy refers to replications and extensions. I hope that their well-stated guideline leads to additional replications. It should help for journals to have replication editors and to set aside space for replications. After the *Quarterly Journal of Business and Economics* adopted a policy of encouraging replications and extensions, their percentage of such papers went from zero during 1978 to 1984 (assessed by Hubbard and Armstrong 1994) to 21 percent during 1984 to 1989 (assessed by Fuess 1996).

Discourage reporting of statistical significance:

Although it typically has little relationship to whether the findings are important, statistical significance plays a strong role in publication decisions as shown by studies in

management, psychology, and medicine (Begg and Berlin 1988; Greenwald 1975; Hubbard and Armstrong 1992; Salsburg 1985; Sterling, Rosenbaum, and Weinkam 1995). Cohen (1994) summarizes the case against statistical significance for psychologists and McCloskey and Ziliak (1996) do so for economists. Confidence intervals are more appropriate than significance tests for assessing the uncertainty associated with a finding.

Atkinson, at al. (1982) conducted an experiment to determine whether reviewers place too much emphasis on statistical significance. They prepared three versions of a bogus manuscript in which identical findings differed by the level of statistical significance. The reviewers recommended rejection of the paper with nonsignificant findings three times as often as the ones with significant findings. They generally claimed that they based their decisions to reject on the design of the study even though the design was the same for all versions.

Using significance tests in publication decisions leads to a bias in what is published. As Sterling (1959) noted, when journals reject studies containing nonsignificant results, researchers may continue to study that issue until, by chance, someone obtains a significant result. This problem still exists according to assessments by Sterling, Rosenbaum and Weinkam (1995). If journal editors discourage the use of statistical significance, researchers will find more appropriate measures of uncertainty.

Eliminate "fairness" as a consideration:

Sherrell, Hair and Griffin (1989), in their survey of 328 marketing academics, obtained results showing that some of the above recommendations would be regarded as unfair and unethical. They object to an editor selecting "reviewers that have a strong bias (pro or con the manuscript's content area or methodology) in order to ensure acceptance or rejection," preempting "the normal review process by accepting or rejecting the manuscript without a formal review," accepting or rejecting "a manuscript contrary to all reviewers' recommendations," and actively soliciting "manuscripts for special issues on interesting new topics . . . without sending them through the review process." Similar findings were obtained in a survey of those who published in management journals (Street, Bozeman and Whitfield 1998). Based on empirical research on scientific publishing, such attitudes are likely to reduce the number of important papers published.

Editors should not use blind reviews (i.e., where authors names and affiliations are not revealed to the reviewers) unless an author requests them. While reviewers exhibit bias based on a researcher's position (as shown in the experiment by Peters and Ceci 1982), a researcher's prior accomplishments provide useful information in determining the quality of a study.

What Can Business Schools Do?

Business schools should encourage publication if they want to be well recognized. Business schools whose faculty members do not publish have low prestige. Armstrong and Sperry (1994) showed that published research helps to explain differences in prestige even

among the most prestigious schools. They used scholarship ratings (an equally weighted index based on the number of publications, citations, and peer ratings) obtained from Kirkpatrick and Locke (1992). They used prestige ratings obtained from academic deans, recruiters at firms, and prospective MBA candidates. The relationships between scholarship and prestige were strong (Table 4). Armstrong and Sperry compared research impact with the overall prestige, basing prestige on an equal-weights index from deans, recruiters, and candidates. For example, the top three schools with respect to research impact had an average prestige ranking of 2.3, while the bottom three had a prestige ranking of about 22.

Table 4
Research Impact versus Prestige of U.S. Business Schools, 1985-1991
(adapted from Armstrong and Sperry 1994)

School	Research Impact	Prestige
Stanford	1	1
Pennsylvania (Wharton)	2	2
MIT (Sloan)	3	4
Columbia	4	13
Carnegie Mellon (GSIA)	5	15
Rochester (Simon)	6	26
Chicago	7	7
Cornell (Johnson)	8	10
Northwestern	9	3
UCLA (Anderson)	10	8
Maryland	11	24
Duke (Fuqua)	12	9
Pittsburgh (Katz)	13	28
Dartmouth (Tuck)	14	6
Michigan	15.5	14
Purdue (Krannert)	15.5	19
Harvard	17.5	5
NYU (Stern)	17.5	17
Texas (Austin)	19	18
Wisconsin	20	20
North Carolina	21	12
Minnesota (Carlson)	22	27
University of Washington	23	29
Texas A&M	24	31
Illinois, Urbana	25	25
SUNY Buffalo	26	30
Penn State (Smeal)	27	23
Indiana	28	16
Ohio State	29	21
Washington, St. Louis	30	22
Syracuse	31	32
Virginia (Darden)	32	11

Similar findings were obtained by Trieschmann et al, (2000). They showed a strong relationship between the number of pages published in 20 leading management journals from 1986 through 1998, and the MBA rankings from the *U.S. News and World Report* for 1995-1999. Of the ten most prolific schools, only three were not in the top ten in the MBA rankings.

A school's emphasis on research impact benefits its students. I analyzed data on the net present value of salaries for the five years after their graduation for 17 of the schools in Table 4 (Armstrong 1995). This showed a strong correlation with the research impact score. This might not be solely due to the better reputation that derives from research; Abrami, Leventhal and Perry (1982) found that learning was higher in courses taught by those engaged in research.

Currently, the reward system does not seem to promote important research. Judging from Singh and Bush's (1998) survey of 281 marketing professors at institutions that grant Ph.D. degrees, most respondents agreed with the following statement: "The extrinsic rewards for research and publication do not appear to be commensurate with the effort . . ."

The reliance on peer review is likely to lead academics to a focus on networking rather than performance. Luthans, Hodgetts and Rosenkrantz (1988) conducted an empirical study of 457 managers from many organizations. They found little relationship between success (pay and promotions) and effectiveness (getting the job done efficiently). Those who were most successful spent most of their time networking, while those who were most effective spent little time networking. Are academic institutions exempt from such findings? Singh and Bush (1998) report a comment made by one of their respondents: "I have 52 publications at {… } University, but am paid less than faculty with three publications in the same period, and substantially less than new hires."

When schools use objective criteria, they typically use indirect measures, such as number of publications. This measure may be a useful in evaluating those who never publish; indeed, many academics never publish. However, counting the number of publication can be detrimental, giving academics an incentive to increase the number of their publications at the expense of quality.

In an effort to be fair, some schools count only papers that appear in the most prestigious journals. This may narrow a researcher's interests. In addition, it creates a backlog at these journals. According to Van Fleet, McWilliams and Siegel (2000), this practice of developing lists of acceptable journals was confined to only 14 percent of the 252 responding schools, and these schools were not known for producing important research.

One useful approach to reducing the emphasis on quantity has been adopted by some business schools. This approach asks faculty members who are seeking promotion to list their three best papers and to explain why they are important.

Many researchers on scientific publication regard citation rates as the best single objective measure of quality. They reflect what captures the interest of other researchers. Citations are typically favorable, sometimes neutral, and seldom negative (Spiegel-Rosing 1977). That said, citations are not direct measures of importance. When I look at my own work, for

example, the papers that I regard as my most important are not the most frequently cited. In particular, the papers that challenge existing beliefs do not have high citation counts.

Instead of looking at indirect measures, schools should be concerned with whether researchers obtain important findings. They could ask faculty members to describe in writing what important findings they have made in the past five years. These descriptions should be intelligible to those in other fields and to practitioners. I am aware of two schools that asked faculty members to describe their findings; they met with resistance from faculty members. Also, when I included descriptions of my findings in the materials for my own promotion, I was told that some members of the personnel committee found this to be unacceptable behavior for an academic.

Business schools should try to ensure that faculty members have the time, resources, and freedom to do research. Gordon and Marquis (1966) found that to make important findings, researchers need freedom to study topics. Presumably, this means that those who are most effective in making important discoveries should have light teaching loads. Hancock et al. (1992) found that of faculty members who publish, those who spent more time with students published less than those who spent less time. Fox and Milbourne (1999), in a study of 150 economists in Australia, found that a ten percent increase in the number of teaching hours reduced research output by about 20 percent. In a meta-analysis, Feldman (1987) found that time spent on research was positively related to research productivity. He also found that faculty members who engage in research do not receive lower teacher ratings than those who do not (nor higher, which may be a more interesting way of looking at this finding). Based on eight studies, he also found that time spent on teaching and teaching-related activities was not related to teacher ratings. Hattie and Marsh (1996) conducted a meta-analysis on the relationship between teaching and research, where teaching was based on student evaluations for 80 percent of the studies, peer assessments of teaching on 19 percent, and self-assessments on one percent. The 58 published studies showed that the relationship was zero.

It is not risky for prestigious business schools to emphasize research rather than teaching. Armstrong and Sperry (1994), in a study of 32 prestigious MBA programs, found that graduates' reported satisfaction with the schools' teaching was unrelated to the schools' prestige. Furthermore, there is little evidence that the traditional teaching methods are effective in aiding learning. For example, Hunt, Chonko, and Wood (1986) obtained survey responses from a representative sample of 1,076 members of the American Marketing Association. Those with marketing majors at either the undergraduate or MBA level did not report more extrinsic success (salary or position attained) or intrinsic success (seven measures of satisfaction) than those with other majors. Furthermore, those with marketing majors had lower positions. Those with higher grades at school typically did worse on pay and title and they reported less satisfaction with their work. Similar results have been obtained in other disciplines as well. (I summarize nine studies in "Teacher versus Learner Responsibility in Management Education," under selected papers at jscottarmstrong.com.)

What Can Funding Agencies Do?

Adam Smith contended that Scottish universities were more creative than English universities because they did not receive public funding (Kealey 1996, p.10-11). Milton Friedman (1992) suggested that those who favored government funding should have the burden of proof to show that such expenditures are useful. He believed, for example, that the National Science Foundation (NSF) funding for research "has done harm to the progress of science." In a review of the history of science and of relevant empirical evidence, Kealey (1996) reached a similar conclusion. Sherwin and Isenson (1967), in assessing the benefits of basic research funding by the Department of Defense, estimated a return on investment of only 0.3 percent.

Here again, the problem lies partly in peer review. In a major study of NSF funding that covered 1,200 proposals submitted in ten different fields, Cole and Cole (1972), using citation counts, concluded that peer review of proposals was not predictive of good research.

Peer reviewers favor existing beliefs. In a survey of 1980 applicants for National Cancer Institute funding, 61 percent of the respondents agreed that reviewers were "reluctant to support unorthodox or high-risk research" (Chubin and Hackett 1990, p.66).

Abrams (1991) examined NSF proposals in ecology. He found that past performance was predictive of future research. As a result, he suggested that grant money be provided to those who have done good research. This strategy is used by some funding organizations. For example, the MacArthur foundation awards "unrestricted fellowships to talented individuals who have shown extraordinary originality and dedication in their creative pursuits, and a marked capacity for self-direction."

What Can Professional Organizations Do?

Professional organizations, as independent third parties, could help to encourage research and to communicate findings. For example, they might try to influence the reward system by rating the effectiveness of researchers and schools in producing important findings. Such ratings might attract much interest. Kirkpatrick and Locke (1992), for example, rated faculty members, departments and business schools for research impact (Table 4) and their ratings achieved high visibility, even before being published. Schools that did well according to these criteria helped to publicize the study. Professional organizations could rate schools on the basis of important findings.

Professional organizations could communicate important findings so that they would be easily available to practitioners and researchers. They could summarize the findings as principles and post them on a web site. Rossiter (2001) suggests that the findings be organized as concepts, structural frameworks, strategic principles, and research principles. The web site should allow for open peer review so that the principles could be challenged, updated, and refined.

I have been developing such a site for forecasting principles (forecastingprinciples.com). The goal is to influence the ways that academics do research and the ways that practitioners

forecast. The site gets a reasonable number of visits; as of 2002, there were 1,200 visits per week and the rate was growing. I suspect that this rate represents more attention than is paid to the leading journals in the field, the *International Journal of Forecasting* and the *Journal of Forecasting*.

Conclusions

The number of important findings in marketing seems modest. Few researchers produce findings that meet the criteria of being replicated, valid, and useful. Of those that do, few have surprising findings.

Peer review poses a barrier to generating, publishing, and applying important findings, especially when the findings are also surprising. Given technological changes, it is no longer necessary to use peer review to censor work. Instead, peer review can be used to improve research papers and to provide useful signals to readers.

To increase the number of important findings and make them accessible to researchers and practitioners, researchers should conduct research that can contribute to principles, and describe the findings so that they are understandable to potential users. Journals should encourage research on principles and make peer review open and continuous. Business schools should reward faculty who make important findings, a policy that should benefit the schools as well as to their students. Funding should be provided for research on principles. Finally, professional organizations could summarize knowledge about principles and identify research needs. Given the low costs of transmitting information via the Internet, I expect that many of these suggestions will be followed. But when?

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