January 1982

Research on Scientific Journals: Implications for Editors and Authors

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Postprint version. Published in *Journal of Forecasting*, Volume 1, Issue 1, 1982, pages 83-104.
Publisher URL: http://dx.doi.org/10.1002/for.3980010109

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Research on Scientific Journals: Implications for Editors and Authors

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Abstract

A review of editorial policies of leading journals and of research relevant to scientific journals revealed conflicts between 'science' and 'scientists." Owing to these conflicts, papers are often weak on objectivity and replicability. Furthermore, papers often fall short on importance, competence, intelligibility, or efficiency. Suggestions were made for editorial policies such as: (1) structured guidelines for referees, (2) open peer review, (3) blind reviews, and (4) full disclosure of data and method. Of major importance, an author's "Note to Referees" (describing the hypotheses and design, but not the results) was suggested to improve the objectivity of the ratings of importance and competence. Also, recommendations are made to authors for improving contributions to science (such as the use of multiple hypotheses) and for promoting their careers (such as using complex methods and obtuse writing).

The major purpose of the paper is to suggest improved policies for scientific journals. Many of these policies are already used by journals. This paper is also directed to authors. While the goals of authors and editors often agree, on some points they conflict. Recognition of the conflicts may help authors to achieve better their goals, whether they relate to career advancement or to scientific contributions.

Scientific journals provide a foundation for scientific progress. They aid in the communication of knowledge and provide an historical record. In Levitan's (1979) survey of biomedical scientists, 85 percent ranked 'the journal' as the most frequently used information source for keeping up to date.

Since 1960, the number of journals has increased by about 2 percent per year, a bit slower than the growth in the number of scientists (King *et al.,* 1978; Garvey and Griffith, 1970). This growth has occurred despite claims by scientists that the overall quality of papers is poor. For example, in a convenience sample of 15 physicists, all thought that the vast majority of papers in the journals that they read were of poor quality or little significance (Mulkay and Williams, 1971).

This paper reviews the current state of knowledge about scientific journals. It draws upon the editorial policies of many journals. Particularly useful in this respect were the guidelines prepared in 1976 for the editor of the *Journal of Personality and Social Psychology* (Greenwald, 1976), and the survey of authors and readers of the American Psychological Association journals (Brackbill and Korten, 1970).

I reviewed empirical research on scientific papers. Most of this research was from the social sciences, but some was from the natural sciences. Experts from a variety of fields also assisted me.[1] The

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[1] These included Stephen Brush (history of natural sciences), James Cameron (publishing), Robert Carbone (operations research), Robert Ferber (survey research), Robert Fildes (operations research), Baruch Fischhoff (psychology), Nigel Gilbert (sociology of science), Robin Hogarth (management), David Horrobin (medicine), Ed Lusk (operations research), Albert Madansky (statistics), Spyros Makridakis (forecasting), and Bill Remus
[continued on next page]

paper is organized according to six criteria. The first two, objectivity and replicability, distinguish scientific from other non-fiction papers. The other four criteria, importance, competence, intelligibility, and efficiency are common to all non-fiction publications. These criteria are applied to papers and to the reviewing process associated with papers.

Objectivity

Objectivity can be defined as the extent to which a study is independent of the researcher's bias. Various steps can be taken to improve the objectivity of a paper and of the way it is reviewed.

Objectivity has been considered a primary criterion for scientific work. Violations of this criterion can cause great concern among scientists. For example, the Lysenko case in Russia represented a gross violation of objectivity. Lysenko was the only scientist able to demonstrate certain findings in biology. The state supported Lysenko: researchers who proposed alternative viewpoints were dealt with harshly. Scientists around the world protested this violation of objectivity in science (Medvedev, 1969; Rich, 1974).

The goal of objectivity is one that is sought but seldom achieved because the bias of the researcher is always present. The fact that it is seldom achieved has been cited by Mitroff (1972) as an argument that objectivity is an unrealistic goal. Rather, he claims, scientists should recognize their biases openly, then be advocates for their favored hypothesis. He concluded that scientists became famous not by being objective, but by being advocates. This appears to be true. Advocacy is a good strategy for career advancement. However, I believe that it is bad advice for making scientific contributions (Armstrong, 1980a).

Proponents of advocacy believe that the “marketplace for ideas” provides an efficient way to separate the good ideas from the bad. Under this system, scientists write papers to advocate their viewpoint. These papers are examined by secret peer review, then they are published and subjected to further review by readers. But numerous case histories illustrate the failure of the marketplace to provide an objective and efficient approach to science. For example, scientists tried to deny an opportunity for Immanuel Velikovsky to present his findings because they were in conflict with established scientific viewpoints in many scientific disciplines in the 1950s. These scientists boycotted the Macmillan publishing company, which had published Velikovsky's Worlds *in Collision* (DeGrazia, 1966). Velikovsky's viewpoints are still censored in many areas of science.

Another example of failure in the marketplace of ideas is provided by the Hawthorne factory experiments. Authors of these studies were committed to explaining their results as being due to actions by management. They concluded that management's concern for employees led the employees to work harder. Belief in this “Hawthorne Effect” has persisted for half a century despite evidence that it yielded a poor explanation in these studies. A better hypothesis was that the workers were more productive when they made decisions about their work (Blumberg, 1968; Franke and Kaul 1978). But this hypothesis is seen as contradicting the dominant hypothesis that *management* controls productivity.

Limitations of journal space affect the marketplace for ideas. On the positive side, numerous journals exist. Thus, it is likely that a paper will appear someplace. Although *Science* publishes only 20 percent of the papers submitted, a follow-up of rejected papers showed that nearly all were published in other journals (Abelson, 1980). Nevertheless, the more prestigious journals in the field have a greater impact and here the competition for space is strong. Competition for space is especially strong in the social sciences. For example. 70 percent of economics and psychology papers are rejected, compared with about 30 percent in chemistry, biology, and physics (Zuckerman and Merton, 1971).

Space is allocated primarily by the decisions of referees. The referees are presumed to be objective, Is this a fair assumption? Recent research suggests that referees are seriously biased by knowledge of who wrote the article and by whether the conclusions in the paper conform with their beliefs.

(production). These experts have had extensive experience as editors, referees, and authors. In addition to identifying relevant research, they provided comments on early drafts.

Bias against Authors

Peters and Ceci (1982) resubmitted 12 papers to the same prestigious psychology journals that had published them within the previous three years. Fictitious names were used for the authors and instituitions; the original authors had been from prestigious universities. (These journals did not use blind reviewing.) Cosmetic changes were made to disguise the papers, but the content was not changed. Three papers were detected. Of the remaining nine papers, eight were rejected. The stated reasons for rejection all referred to "methodological shortcomings." None of the referees rejected a paper because it "added nothing new." Many explanations were considered for these results; the most likely one was that the referees were biased against authors from unknown institutions.

In a large-scale study of papers submitted to physics journals, Gordon (1980) found a strong bias in that referees from major universities rated papers more highly if the authors were also from major universities. This bias was not present for referees from minor universities.

Bias against Disconfirming Evidence

Goodstein and Brazis (1970) mailed abstracts of an empirical study on astrology to members of the American Psychological Association. About half of the 282 respondents received an abstract confirming common scientific beliefs and the ether half received an identical abstract but with disconfirming results. The study that supported existing beliefs among referees was rated as better designed and having more siguificimce for future research.

Mahoney (1977) submitted a paper to 75 referees from the *Journal of Applied Behavior Analysis*. Some received results that confirmed the dominant hypothesis held by scientists in that field. Other referees received the identical paper except that the results were reversed so they disconfirmed the dominant hypothesis. The referees were more likely to reject the study with disconfirming evidence and to accept the one with confirming evidence. They rated the study in disconfirming evidence poorer on "relevance and methodology," despite the fact that these were identical in both versions. Apparently referees had different standards for articles where the results conflicted with their own beliefs.

Implications for Journals Regarding Objectivity

Bias against authors can be reduced by blind reviewing (where the referee is not given the name of the authors or institutions). Some journals use blind reviewing. but most do not. For example, in the late 1960s, only 25 percent of economics journals (Coe and Weinstock, 1967) and less than 20 percent of Crane's (1967) sample of 50 social and physical sciences journals kept the author's identity secret from the referee. Although some studies have indicated no need for blind reviewing (e.g., the experiment by Mahoney, Kazdin, and Kenigsberg, 1978, and the survey by Kerr, Tolliver and Petree, 1977), other studies have shown a need (Peters and Ceci, 1982). No studies have shown blind reviewing to be harmful. Given that its cost is negligible, the case in favor of blind reviewing is strong.

In a convenience sample survey of full professors, respondents thought that the author or institution would not be apparent to the referees for about two-thirds of the papers (Armstrong, 1982). Furthermore, in Peters and Ceci (1982), 92 percent of the referees and editors were unable to identify the author or institution of a paper that had already been published in that journal. Thus, blind reviewing should be relevant for most papers.

Introduction of blind reviewing should not be difficult. In my survey of full professors (Armstrong, 1982), 14 of the 19 respondents for this question were favorable to the use of blind reviewing for the prestigious journals in their field.

Numerous policies can be used to reduce bias against papers that disconfirm existing scientific beliefs. 1 describe six policies here; a "note to referees," referees nominated by authors, two-sided recommendations by referees, unrefereed contributions, open peer review, and preferences given to studies using multiple hypotheses.

1. *The "Note to Referees"*: Authors of controversial papers could be encouraged to submit a "Note to Referees." This would describe the hypotheses and the design of the study, but not the results. Such a procedure would help the referee to make an objective evaluation of the importance of the study and the methodology. After responding to this "Note to Referees," the referee would open a sealed package containing the complete paper and would proceed with the review.

2. *Referees Nominated by Authors:* Authors should be encouraged to suggest possible referees. The authors are likely to know which referees are experts in their area. More importantly, they may be able to identify qualified referees who would not reject the study simply because it conflicted with their own viewpoint. (This policy is used by *Science)* Lacking this, an effort should at least be made to select referees with different viewpoints on the problem.

3. *Two-sided Recommendations by Referees*: Referees would be asked to provide arguments why the paper should be published. Then they would provide arguments as to why the papers should not be published. Koriat, Lichtenstein, and Fischhoff (1980) found that an explicit two-sided argument helped people to assess their confidence in the validity of an argument. This might improve the chances for papers that disconfirm current wisdom.

4. *Unrefereed Contributions*: A paper might be important, yet so controversial that it cannot receive an unbiased review. Publication in an unrefereed "Contributions" section could provide a channel for such papers. The section would be clearly labeled as "unrefereed." (Such a procedure has been used occasionally by *Management Science*.) This solution is of modest value because of space limitations. The paper would have to be highly controversial to be published. (These papers might receive a technical review to assist the author.)

   A "Clearinghouse" would establish another channel for authors with deviant viewpoints. This would list the title, name and address of the author, and a short abstract (e.g., l00 words or less). It could mention relevant working papers that might later be submitted, papers that would not be submitted, or papers that had been rejected. (The reason for listing would not be mentioned to avoid bias against papers that were rejected.) To be listed, the author would need to convince the editors that it is relevant to the readers of the journal. The "Clearinghouse" might be useful in cases of dispute between authors and the journal.

   The "Clearinghouse" also would help authors to obtain feedback and to stake an early claim for their work.

5. *Open Peer Review:* Many experts claim that referees will be more open and objective in their criticism if their identity is kept secret. Almost all journals keep the referee's identity secret.

   Other experts have suggested open peer review. The proposed advantages are appealing. For example, referees should be more highly motivated to do a competent and fair review if they may have to defend their views to the authors and if they are identified with the published papers.

A modified position would be that referees could decide whether they would allow their identity to be revealed. My small-scale survey of full professors found that 45 percent would be willing to have their names revealed to the authors and to the readers (Armstrong, 1982).

Referees could be offered an opportunity to publish their reviews for important papers. This policy, analogous to the current procedure used for book reviews, would encourage referees to accept papers on important issues.

The availability of referees' reviews would provide useful information to scientists. Few readers can devote the attention to a paper that is given to it by referees. Journals should publish the names and addresses of the referees for each paper so readers can write for reviews.

Open peer review would improve communications. Referees and authors could discuss difficult issues to find ways to improve a paper.

After publication, open peer review could be continued by "letters to the editors" and by publishing replies. For important and controversial papers, the editors could publish opposing viewpoints. *The Behavioral and Brain Sciences* uses this as standard procedure (Harnad, 1979). Other journals have used it for selected issues, such as the symposium on the value of econometric forecasting methods by the *Journal* of *Business* (Madansky, 1978).

6. *Multiple Hypotheses:* Journals could ask referees to give preference to studies that examine a set of reasonable hypotheses. This set should include hypotheses representing existing beliefs. (The rationale for this recommendation is presented in the next section.)

Implications for Authors Regarding Objectivity

Scientific contributions

In order for a paper be judged on scientific merits, researchers should submit their papers to journals that provide blind reviews, when feasible to do so.

Authors have a major responsibility for ensuring that the papers they submit meet the standards for objectivity. Elsewhere (Armstrong, 1979), 1 have argued that researchers should use the method of multiple hypotheses to help ensure objectivity. These multiple hypotheses would reflect existing scientific beliefs as well as new hypotheses.

Researchers using the method of multiple hypotheses would be less likely to develop pride of ownership for a particular hypothesis. Referees would be less concerned whether the study refutes the prevailing wisdom and more concerned with how well the evidence allowed one to select from among the leading hypotheses.

Although the method of multiple hypotheses was first proposed in 1890 by Chamberlin (reprinted in Chamberlin, 1965), it is still not common in the social sciences. For example, in my analysis of papers published in *Management Science* from 1955 to 1976, 64 percent of the empirical papers advocated a dominant hypothesis, 14 percent had no hypothesis, and only 22 percent used the method of multiple hypotheses (Armstrong, 1979).

Career advancement

Unknown authors or authors from low status institutions should submit their papers to journals that use blind reviewing. However, if you are well-known or from a prestigious institution, your chances are better at a journal that does not have blind reviewing.

The method of multiple hypothesis is not likely to be a good one for a scientist's career. It adds to the time required to do the research. It also increases the length of the paper, which reduces the likelihood of publication. Thus, a conflict exists between what is good for the scientist and what is good for science.

Career advancement will be enhanced by becoming an advocate for a dominant hypothesis that does not challenge current beliefs. Build upon previous beliefs and make small changes.

The research also suggests that the author select an alternative hypothesis that clearly will be defeated by the proposed hypothesis, even if such an hypothesis is implausible. Such behavior is rewarded by journals. Greenwald (1975), using a survey of reviewers for a journal, found a bias favoring papers that rejected the null hypothesis. Kerr, Tolliver and Peirce (1977), in a survey of reviewers for journals in management and social science, found similar biases; referees said they were less likely to accept a paper with statistically insignificant findings even when the theory tested was of interest to the field. Sterling (1959) also found evidence of the preference for a dominant hypothesis: His review of research in four commonly read psychology journals found that 97 percent of the papers confirmed the dominant hypothesis (at $p < 0.05$). Bozarth and Roberts (1972) replicated Sterling's study with a sample of published papers from three more journals in psychology and found 94 percent confirmed (at $p < 0.05$). Smart (1964) also found a tendency for journals to confirm the dominant hypothesis: In his examination of studies from psychology, confirming results were reported by 70 percent of doctoral dissertations, but by 80 percent of papers at the annual American Psychological Association meetings, and by 90 percent of the papers in journals.

Replicability

Replicability means that other researchers should be able to reproduce the key elements of the study. In addition, replicability also refers to the duplication of some key elements with variations in other elements. The purpose of replication is to determine whether the same results can be achieved by other researchers.

Apparently, replication is not common in the social sciences. Bozarth and Roberts (1972), in their survey of 1334 papers published in three psychology journals, found that less than 1 percent were replications of previously published journal articles. Reid, Soley and Wimmer (1981), in an examination of 501 papers on advertising research, found that less than 1 percent were direct replications and only 5 percent were extensions.

One function of a scientific journal is to ensure that information is available for replication. This is typically expensive. However, the ability to replicate is vital to ensure that the results were not due to chance, mistakes, or cheating. In Reid, Soley and Wimmer (1981), 12 of the 30 replications yielded results that conflicted with the original study.

Chance alone would be sufficient to provide a supply of papers with significant results. This provides one argument for replication. Of course, significant results would not be expected every time that the study is replicated (as noted by Hunt, 1975). The key question is "what is the significance of the results given the cumulative evidence?"

Mistakes are not unusual. Wolin (1962) replicated seven studies in psychology and found gross miscalculations in three. Corrections in these studies led to substantial changes in the conclusions. Interestingly, mistakes generally favor the researcher's hypothesis (Rosenthal, 1976).

Cheating also occurs. Publication of a fictitious study was reported by DuShane et al. (1961). On occasion, cheating has involved famous scientists, well-known studies, and prestigious institutions. These include Cyril Burt's alleged creation of fictitious data in studies on the IQs of identical twins (Wade, 1976; Dorfman, 1978; Gillie, 1979) and a Sloan-Kettering study on cancer treatment (Culliton, 1974). Trafford (1981) reports on scandals in medical research at Yale, Boston University, and Massachusetts General Hospital. Mahoney (1979, footnotes 91-95) provides additional references on deceptions and hoaxes in science.

Implications for Journals Regarding Replicability

Journals should ensure that interested researchers can obtain adequate information on the method. Often, this can be accomplished in the paper itself. Sometimes, however, it will require supporting documents. A copy of these documents should be provided to the editors, although interested readers could be encouraged to contact the author; publishing the author's address would help.

Similarly, access should be provided to the data. If the raw data cannot be included in the paper and if they are not easily available from published sources, copies should be provided to the editors. If this is too costly, summaries and samples of the data could be provided. This requirement is a serious one. Wolin (1962) tried to obtain data from 37 authors of articles in psychology journals. Of the 37, 32 replied, but 21 said that the data had been inadvertently lost or destroyed. Two authors provided data on the condition that they have control over subsequent publications. Two authors replied after such a long delay that it was not possible to replicate them. In other words, Wolin was able to replicate less than 20 percent of the published studies.

Journals can, and sometimes do, impose the condition that the data and method become part of the public domain. Credit would be given to the supplier of the data, but the supplier would have no control over subsequent publications. This requirement is controversial because:

1. It interferes with the strategy of "fragmented publication" whereby authors chop the study into small pieces to publish in various journals over a period of years. They would not want others to compete with them by using their data.

2. Authors may want to use the method or data for commercial gain.

3. The data may be confidential.

4. A substantial effort might be required to prepare the data for use by others.

Good scientific journals should discourage fragmented publications. The second argument is irrelevant as journals have no reason to promote commercial gain for an author. The third argument might be dealt with to some extent by disguising the data. The additional effort remains a serious drawback to full disclosure.

Journals can encourage replications, especially initial replications of papers that obtained surprising results. This contrasts with the aims of many journals: In the Kerr, Tolliver, and Petree (1977) survey of management and social science journals, 71 percent of the editors reported a reduced likelihood of accepting a competent paper that was "a direct replication of an original study recently published in your journal (which) adds no new dimension to theory."

Implications for Authors Regarding Replicability

Scientific contributions

Authors should provide full disclosure of method and data in the paper and its supporting documents. This requires additional time but is time well spent for *important* papers because authors are likely to receive requests for further information. Furthermore, they should be willing to assist others in replications of the study.

Career advancement

This can be examined from two viewpoints. First, should authors assist others in replicating their study? Second, should an author replicate studies done by others?

An author of a competent and important paper would benefit from having others replicate the study. But if a paper does not fall into this category, the added time and cost may not be justified. If replication fails, it could be damaging to the original author. Furthermore, other researchers may take important extensions using data or methods developed by the original author, thus depriving the author of an opportunity.

As to the second question, do scientists become famous for replications? This seems unlikely. Researchers who replicated Watson's famous study on conditioning of a baby were unable to obtain the same results. Their studies were generally ignored (Harris, 1979; Samelson, 1980), while Watson’s fame grew.

Rather than making direct replications, researchers could alter previous designs. They then present it as an important extension or even as an original study. Such a strategy should be beneficial to the author. It may also be beneficial to science.

Importance

Importance has many dimensions. Among these:

a) Is the *problem* important?

b) Do the *results* add significantly to what is known?

c) Will the paper affect *decision-making* or *future research?*

An important paper would do well on all dimensions.

Seldom does the value system in research institutions encourage scientists to work on important problems. In fact, might the opposite be true? Francis Gallon, for example, encountered resistance when he chose to work on an important problem in the 1870s: What is the value of prayer? (Brush, 1974). Barber (1961) describes resistance that famous scientists met when they worked on important problems, and Colman (1979) provides a brief review on the delay in publication encountered by important papers.

Journals do not seem to give preference to important problems. My own experience is shared by many and was summarized by an unknown biochemist who claimed that “the merit and importance of any one of his publications were directly proportional to the difficulty encountered in getting the thing into print” (Jones, 1974).

Editors of journals in psychology, social work, and sociology rated importance (“the value of an article’s findings to affairs of everyday social life”) tenth out of a list of 12 criteria in Lindsey's (1981, pp. 18-21) survey. Similar results were obtained in the survey by Beyer (1978): “applicability to practical or applied problems” ranked a distant last for the ten criteria presented to editors of physics, chemistry, and sociology journals, and it was next to last for political science.

Papers with surprising results are especially important for adding significantly to what is known. Typically, they are surprising because they defeat the leading hypothesis. For example, Galton's studies led to the conclusion that prayer was not effective. Such disconfirming results could be due to a failure in the research methods or they could actually refute the leading hypothesis.

Established scientists can be expected to claim the former; in other words, the theory is sound but the research was not competent. An example would be the reaction to McCord's (1978) study which concluded that social work was ineffective: Opponents claimed that her research methods were not adequate. In any event, surprising results are worthy of publication, even if only to define better the conditions under which the leading hypothesis is valid.

Papers important to decision-making appear to have lower status than those in which the applications are not obvious. In their attempts to gain status, journals frequently depart from problems that might contribute to decision-making.

Peer ratings of psychology papers do not correlate well with the number of citations that the papers eventually receive (Gottfredson, 1978; Gordon, 1977). This suggests that existing policies are not adequate for identifying papers that will be important for further research.

Implications for Journals Regarding Importance

Presumably editors want to publish important papers. On the other hand, they are concerned that the journal might look foolish. Thus, the screening of papers for relevance, which typically rejects a large percentage of papers, provides a potential barrier to important papers. A procedure whereby more than one editor could process papers might increase the likelihood that an important paper will be reviewed.

Referees' rating sheets could ask for explicit ratings of importance for the problem, the results, and their possible use in further research or in decision-making. Preference would be given to those studies rated as more important.

To obtain a better assessment of the importance of the problem, referees could rate importance before reading the paper. This could be done by allowing the author to explain the problem in the "Note to Referees." In this way, the referees' ratings would not be influenced by their biases about "proper results." An important paper is one that would be important no matter what the results.

Another way to assess the importance of results is to ask referees to predict the results by using the "Note to Referees." According to the study by Slovic and Fischhoff (1977), researchers seldom report that they are surprised by research results; they manage to rationalize whatever results they see. In that study, researchers reported that they were not surprised when given outcome X, and an equivalent sample of researchers reported that they were not surprised when presented with not-X. Prediction of results might help to overcome this.

After reading a paper, referees might be asked if they can imagine any reasonable basis to expect that the results could have turned out otherwise (Greenwald, 1976). If not, the results would be of little importance.

Importance to decision-making can be assessed by asking the author to report on applications. If this is not possible, the author could provide an example of how the findings *might* be used. Importance to further research can be measured after publication by the use of citations (excluding self-citations). These are reported in the *Permuterm Citation Index* published by the Institute for Scientific Information in Philadelphia. Citations arc almost always done in a positive way (e.g., less than one percent of the citations in *Science* Studies were negative according to Spiegel-Rosing, 1977). The major disadvantage of journal citations is the substantial time lag: In psychology, the median age for citations in a journal is seven years (Garvey and Griffith, 1970). One measure of importance that has been discussed in the natural sciences is the use of a paper by other academic disciplines (e.g., Moravcsik, 1974). By this reasoning, citations among the "invisible college" (usually about 100 researchers working in one area) is not as good a measure of importance as citations from different disciplines. Journals can assess this in the long run from *Journal Citation Reports* (Institute for Scientific Information). In the short run, they can also ask a referee from another academic discipline to rate the importance of the paper.

Implications for Authors Regarding Importance

Scientific contributions

Researchers should select important problems. In their papers they should describe why the problem is important, identify how their results add to what was known previously, and show how their conclusions did (or would) affect decision-making or future research.

Career advancement

It is risky for authors to work on important problems, especially early in their careers. Such problems require more time and longer papers. Also, surprising results on important problems are likely to meet with resistance.

Competence

A necessary condition for scientific work is that the research be competent. Competence involves a number of issues:

a) Are the research methods appropriate? (Especially convincing here is the use of multiple methods or multiple measures.)

b) Does the paper properly use prior research? (This cumulative effect is a key to progress.)

c) Is the analysis free of errors?

Competent research can reduce the likelihood of bias by researchers. Terpstra (1981) showed this to be a likely source of bias because positive findings were found more often in studies using poor methodology.

Responsibility for competence lies primarily with the researchers. They should use alternative approaches to test their hypotheses. Also, they should seek peer review prior to submitting a paper to a journal. Informal peer review can be effective. Garvey and Griffith (1970) found that about half of the authors who published in major psychology journals had circulated preprints; of these, over 60 percent received feedback that prompted them to revise their manuscripts.

Are referees capable of judging competence of research methods? Historical accounts make one suspicious (e.g., see McCutchen, 1976). Further doubt was cast on the competency of referees in the study by Mahoney and Kimper (1976). Four logical arguments and four simple experiments were presented to 77 physicists, biologists, sociologists, and psychologists. Fewer than 8 percent were able to identify logically relevant experiments and fewer than 10 percent were able to specify the two experiments that had the potential of proving an hypothesis to be false.

The study by Peters and Ceci (1982) also raises doubts about the ability of referees to judge papers. They rejected eight out of nine papers that had been published previously. Their reasons had to do with poor methodology.

Further concern over the ability of referees to identify competent work was obtained from studies of the agreement among referees. While Zuckerman and Merton (1971) found high agreement among referees for papers in physics, Orr and Kassab (quoted in Zuckerman and Merton) found only modest agreement among referees in biomedicine, as did Smigel and Ross (1970) in sociology. Additional studies were reviewed by Peters and Ceci (1982, pp. 3-4), who concluded that agreement among referees is poor.

Implications for Journals Regarding Competence

Editors should encourage authors to obtain prior peer review. Science does this by asking the authors to list in a cover letter those colleagues who have reviewed the paper. Evidence of additional peer review, such as presentation of the paper at conferences and publication in proceedings, can be requested.

Editors can also provide a structured guide for referees as a way to improve the agreement among referees. Such a guide can be used to emphasize relevant criteria, such as the importance of a paper. It may also be useful in reducing the bias due to irrelevant factors.

Referees could be asked to identify defects in research methods. A checklist, such as that used by Terpstra (1981). might be useful. It seems best to view competency only as a negative factor that could disqualify a study.

The focus should be on the competency of the paper, not the competency of the author. Blind reviews should be of value here.

Ratings of competency are biased if the results of the study are known. To deal with this problem, the journal could ask referees to judge the competency of the method first by using only the description of the study in the author's “Note to Referees.”

Referees can help to detect errors in logic or in computations. For empirical papers, this requires a careful technical review. This practice, followed by many of the better journals, can help the author.

The literature review is one area that can be judged reliably. Does the paper include all relevant prior research? (Computer searches may prove useful here.) Mistakes on citations seem common. For example, I found one study (Linsky, 1975) in which 33 percent of the 57 references to journal articles were incorrect. In view of such problems, the *Journal of Economic Literature* checks all references. Spot checks by editors and referees provide another way to control for such errors.

Sometimes previous research is not correctly interpreted. Harris (1979), in his report on the work of the psychologist J. B. Watson, found that numerous journals and textbooks had misreported the Little Albert study over the years (including misreporting by Watson himself). Harris concluded that “no detail of the original study had escaped misrepresentation in the telling and retelling of this study.”

It is unreasonable to assume that published papers are free of errors. Here again, open peer review would be valuable. Some shortcomings may have been identified by the referees. Readers could contact the journal or the referee for a copy of the review. Continuing peer review can be aided by giving high priority to a “corrections section” and the journal's index should trace these errors back to the original paper. A “Letters to the Editors” section provides a channel for rapid disclosure of errors.

Implications for Authors Regarding Competence

Scientific contributions

Research methods should be chosen not for their elegance but for their usefulness in solving the problem.

Authors should conduct an adequate search of the literature. Include all relevant citations, but be selective. The references in the final copy should be checked against the original sources. Authors should seek extensive peer review before submitting a paper to a journal. This also aids career advancement because it increases the number of readers, the likelihood of acceptance, and the quality of the contribution.

Authors should report errors in their papers as soon as they become apparent after publication.

Career advancement

Authors can try to impress the reviewer with their competence by using complex and difficult procedures. This may divert the reviewers' attention from the problem, which may not require a complex approach. Siegfried (1970), in a humorous attack on this strategy, provided a “rigorous” mathematical approach to demonstrate that 1 + 1 = 2.

Established researchers can focus on their competence by citing their own work, although excessive self-citations might be detrimental. The use of a small number of self-citations increased the likelihood of a favorable review by referees in the study by Mahoney, Kazdin and Kenigsberg (1978).

Finally, you should cite authors who are approved by the audience and avoid those disapproved. For example, on more than one occasion I have been advised not to cite work by Immanuel Velikovsky. Approved citations, on the other hand, improve the “illusion of competency” for the paper.

Intelligibility

The primary purpose of journals is to communicate knowledge. Another purpose is to confer prestige to the authors. These goals may conflict. In my study of journal papers (Armstrong, 1980b), academics rated the competency of the author higher when the writing was less intelligible. A possible explanation would be:

1. Profound thought is difficult to understand.
2. My thought is difficult to understand.
3. Therefore, my thought is profound.

My study was an extension of the “Dr. Fox” study. Dr. Fox gave a one-hour lecture followed by a 30-minute question and answer period. The lecture was comprised of double talk, meaningless words, contradictory statements, irrelevant humor, and references to unrelated topics. The talk was senseless. Dr. Fox, an actor, was able to present this lecture to three audiences with a total of 55 academic and non-academic people. None of the listeners realized the lecture was pure nonsense. In fact, Dr. Fox received good ratings for this lecture (Naftulin, Ware and Donnelly, 1973).

Readability is important for scientific journals because English is a second language for many readers. Readability is also important if a journal expects to have an impact beyond its own area of specialization. If a journal is intent upon the application of knowledge, papers should be intelligible and interesting to practitioners. It is important that the complete report in the scientific journal be clearly written because summaries of journal articles in popular magazines or in textbooks often omit details that are valuable for making applications.

Implications for Journals Regarding Intelligibility

Many journals say that clearly written papers are likely to be published. But it is not clear that contributors believe this claim.

To emphasize the need for clear writing, some journals ask referees to rate the clarity of the writing. A stronger step would be *to measure* readability. Although readability can be impaired in many ways (complicated logic, unnecessary mathematics, lack of structure, omission of key details), an index based on word length and sentence length will measure some of the complexity. Many readability indices are available and they yield similar results (Powers, Summer and Kearl, 1958). The Gunning fog index is one that is simple to compute, widely used, and easily interpreted. The formula is:

$$G = 0.4\ (S + W)$$

where S is the average sentence length, W is the percentage of words with three or more syllables (not counting common prefixes and suffixes), and G is the "fog index." The fog index approximates the grade level of education needed to understand the writing (e.g., 14 would be appropriate for people who completed two years of college).

Authors should be informed that the journal will use a readability index. A specific goal can be set: For example, "the Gunning index for papers published in the journal should be less than 16." The readability index would be published on the first page of the paper.

Referees could be asked to suggest improvements in readability. Suggestions on writing style, such as those presented by Strunk and White (1978), are helpful (e.g., use subheadings, use active rather than passive voice). The use of first person rather than third helps to identify the source of the action and it helps to distinguish facts from opinions. Exhibits can be used to illustrate complex logic. Data can be simplified; this avoids implications of false precision. Graphs can be used to summarize data.

Consider using referees also from other disciplines and referees for whom English is not their native language. These referees should be able to identify passages and terms that are difficult to understand.

In addition to rating the readability of the paper, referees could suggest improvements. Especially useful here is that the author receive a marked-up copy of the original so that changes can be made.

Copy editing should be provided by the journal to suggest improvements in the writing style of the accepted version. This implies the need for the author to receive a copy edited version, then page proofs. This should be a requirement for scientific journals.

Implications for Authors Regarding Intelligibility

Scientific contributions

Researchers who have a significant contribution can afford to write clearly. This is especially so if the researcher hopes to have an impact on decision-making or research. Furthermore, if "contributions to related fields" is used as a measure of importance, clear writing is vital. For important papers then, there is little conflict between career advancement and scientific contributions.

Career advancement

Good arguments exist for writing papers that are difficult to understand. It takes less time and it may increase the likelihood of acceptance. The writing could also be passive and use the third person to create the illusion of objectivity; Gilbert (1976) says that this gives the appearance that the researcher was not actively involved in the study. This advice on writing is especially important if the author has little to say. Unfortaunately, this advice is detrimental to scientific contributions.

Efficiency

Efficiency means that journal space is used wisely and that time is saved for readers and authors. This implies a need for:

1. articles that are as short as possible, and

2. a short time lag from submission to publication.

Although the papers should be short, they should also be self-contained. In many fields, authors split a study into many papers (Broad, 1981). This is been referred to as writing the "Least Publishable Unit." To obtain sufficient details on a study, the reader is forced to use different journals. Furthermore, parts of the description of the study are repeated in numerous papers, thus using space inefficiently.

Clearly many papers could be shortened without a loss in content. Ackoff (1967), in a pilot study, found that reader comprehension suffered little when eight articles were edited to one-third of their original length.

Benefits from reducing the length of papers can be substantial. Assume that an author is intent on writing a successful paper to be published in an academic journal with a circulation of 10,000. If it is an important paper, it might be used in courses and reprinted in books. Citations can lead to additional readers. Over its life, let's say that 4,000 people read this paper. How much time would the author be justified to spend on rewriting the article to make it shorter and easier to read? By saving 15 minutes for the average reader (reducing reading time from, say, 50 to 35 minutes), about 1,000 hours would be saved. For the total system (ignoring present worth calculations and equating the author's wage rate with that of the readers), the author could spend up to 1,000 hours to make the changes. Realistically, such rewriting would take a small fraction of this time, perhaps no more than 50 hours. Thus, each hour spent by the author could save 20 hours for readers.

This does not mean that journals should impose page limits. Long papers are often necessary. This can occur for review papers, large scale studies that need to be self contained, studies using the method of multiple hypotheses or multiple methods or multiple measures, papers breaking new ground, and highly important studies.

Respondents to the survey by Levitan (1979) claimed that time lag for publication was not a key factor in the author's choice of a journal. Arguing against this is the perceived need by junior faculty to produce quick results. Publications are worth money, especially the first ones by an author (Tuckman and Leahey. 1975). Another argument for rapid publication is that it helps an author to stake a claim. Finally, readers gain from a short time lag for important papers.

Implications for Journals Regarding Efficiency

No page limit should be set by the journal. But authors should be advised to make their papers as short as possible. Referees could be asked to estimate the extent to which a paper could be shortened and to make specific suggestions on what to eliminate.

To "stake a claim" for the author, a list of "papers accepted for publication" could be published. This is done by many journals. The address of the author would be provided so people could write for pre-publication copies. Authors would be expected to supply single copies. This notification should reduce the likelihood of plagiarism. Although plagiarism does occur, as, it seems, in the dramatic case of Alsabti (Broad, 1980), it is rare and likely to be detected eventually.

Many journals set time deadlines for referees in order to reduce reviewing time. More effective, it seems, is to send an honorarium (say $25) with the paper. Funds for the referees' honorariums could be obtained by using a submission fee (perhaps $75). This practice is used (with apparent success according to their editors) by the *Journal of Political Economy* and by the *Journal of Financial Economics.*

Implications for Authors Regarding Efficiency

Pascal, in 1657, said "I have made this letter longer than usual only because I have not had time to make it shorter." Take the time. Time spent writing shorter articles should contribute both to career advancement (more likely publication) and to scientific contributions (better use of journal space and an increased likelihood that your paper will be read). Supporting documents, available to interested readers (and also filed with the editors), can reduce the length of the published paper.

Conclusions

A summary of the policy recommendations is provided in Table 1 (next page). Some of these policies were drawn from the research. Of particular interest is the "Note to Referees," which describes the study without the results. This might improve the objectivity of the review and the ratings of importance and competency. Provision of this option should be inexpensive because only a small percentage of authors would need to use it.

The dotted boxes in 'Table 1 suggest where conflicts exist between the author's goals of career advancement and making scientific contributions. For example, science benefits if the author uses multiple hypotheses including the current beliefs, but the author benefits from selecting a dominant hypothesis that does not challenge current beliefs. The "Note to Referees" provides one way by which authors could reduce the conflicts.

Some readers may feel that the suggestions here for career advancement are extreme. However, they provide a description of many papers published in the social sciences. Frequently, authors select unimportant problems, advocate a dominant hypothesis, avoid challenges to current beliefs, provide less than full disclosure, use complex methods, or write in an obtuse manner. It is not by accident that intelligent and successful scientists produce such work.

The policies in Table I, except for the last column, are ideals to strive towards. The aim is to set ambitious standards to guide editors, referees, and authors. But the policies may not be appropriate in all cases; for example, many of the policies pertain only to empirical papers. Violations of the policies should *not* exclude a paper for consideration by a journal.

In general, the purpose of the proposed editorial policies is to provide a favorable setting for scientific contributions – to help the "unreasonable man."

> *"The reasonable man adapts himself to the world; the unreasonable one persists in trying to adapt the world to himself.*
>
> *Therefore all progress depends on the unreasonable man."*
>
> George Bernard Shaw

Table I. Major policy recommendations for editors and authors

Criteria	Editors	Authors: Scientific Contributions		Authors: Career Advancement
Objectivity	Blind reviewing	Submit to journal with blind reviews	↔	Submit to journal with blind reviews if author known or from high status institution
	Favor multiple hypotheses			
	Authors to nominate referees			
	"Note to Referees"			
	Two-sided recommendations by referees	Use multiple hypotheses	↔	Advocate a dominant hypothesis
	Unrefereed contributions	Challenge current beliefs	↔	Do not challenge current beliefs
	Open peer review			
Replicability	Full disclosure of method/data (use supporting documents)	Provide full disclosure	↔	Avoid full disclosure
	Public access to method/data			
	Encourage replications			Reuse previous design
Importance	Use "Note to Referees" to:			
	Rate importance	Select important problems	↔	Avoid important problems
	Predict results			
	Obtain evidence on applications	Describe why paper is important		Avoid surprising results
	Examine citation rate	Describe use of recommendations		
	Referees from other disciplines			
Competence	Evidence of prior peer review	Use most appropriate method	↔	Use complex methods
	Structured guide for referees			
	Adequate competency?	Include all literature relevant	↔	Cite approved works only
	Blind reviews			
	"Note to Referees" used to rate competence	Obtain prior peer review		Use self-citations
	Technical review	Check references against original source		
	Complete/accurate literature review	Report errors after publication		
	Open peer review			
Intelligibility	Ask referees to:	Write clearly	↔	Write obtusely for unimportant papers
	Rate readability			Write clearly for important papers
	Suggest improvements			
	Publish readability index			
	Referees with varied backgrounds			
	Copy editing by the publisher			
Efficiency	No page limit	Make paper as short as possible		Make paper as short as possible
	Rate length vs. contribution	Provide supporting documents		
	Publish "Papers Accepted" list			
	Referees to receive:			
	Time deadlines			
	Honorariums			

Acknowledgement

This paper was written while the author was a visiting professor at IMEDE in Lausanne, Switzerland.

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Editor's Note

The preceding paper explains the process and evidence used to develop policies for the *Journal of Forecasting.* Many, though not all, of the policies have been adopted for the *Journal*. The policies are reflected in the "Guidelines for Authors" published on the back cover of this issue and in the "Guidelines for Referees," in the appendix of this paper. (A complete Editorial manual is available from the editors.)

We feel that the *Journal of Forecasting* policies are ambitious. We propose these policies as a way to obtain better papers and to help authors to increase the likelihood of publication. However, violations of these guidelines will not eliminate a paper as a candidate for publication. We would not be so bold as to suggest these policies define the best procedures for all authors, associate editors, or referees. The policies should be an aid, not a restriction.

Appendix

GUIDELINES FOR REFEREES

NOTE TO REFEREES

The author(s) of this paper have submitted a 'note to referees'. In these unusual cases, the editors have promised to find referees who will follow a special reviewing procedure. This procedure is as follows:
Do *not* open the envelope containing the manuscript until you have completed the ratings in items 1 to 3. To answer these questions, read only the 'note to referees' which is attached.

---

1. How important is the topic . . .
   (a) to practitioners?

( )	( )	( )	( )	( )
1	2	3	4	5
of no importance				extremely important

   (b) to researchers?

( )	( )	( )	( )	( )
1	2	3	4	5
of no importance				extremely important

2. Are the research methods appropriate?
   ____ Yes
   ____ No (explain)
3. Predict the outcome of the study.
   When you complete these three items, open the envelope containing the manuscript and review it by following the structured guide (which is mandatory for this special review process).

REFEREE'S RATING SHEET

[CHECK HERE IF 'NOTE TO REFEREES' WAS PROVIDED □]

Title of Paper______________________________

1. How important is the topic . . .
   (a) to practitioners?

( )	( )	( )	( )	( )
1	2	3	4	5
of no importance				extremely important

(b) to other researchers?

( )	( )	( )	( )	( )
1	2	3	4	5
of no importance				extremely important

2. Does the paper describe applications or illustrate potential applications?
   ____ Describes applications
   ____ Illustrates potential applications
   ____ No applications described
3. How easy is the paper to understand?

( )	( )	( )	( )	( )
1	2	3	4	5
Very hard to understand		Average for academic journals		Very easy to understand

4. Suggest general ways to improve readability (beyond your comments in the margins). Example: 'Put mathematics in an appendix')
5. Is the title descriptive and short?
   ____ Yes
   ____ No (suggest new title)
6. Is the abstract complete and correct?
   ____ Yes
   ____ No (suggest changes)
7. Can the paper be reduced in length?
   ____ Yes (by what percentage? ____ %)
   If yes, please provide suggestions on how to reduce length of the manuscript:
   ____ No
8. Was the review of prior literature adequate?
   ____ Yes
   ____ No (list some relevant papers that should be added)
9. Were the references correct?
   ____ Yes
   ____ No (show errors here or on manuscript)
10. Does the paper follow the 'Guidelines to Authors'?
    ____ Yes
    ____ No (suggest changes)
11. Please classify the paper (check *all* that apply):
    (a) *Empirical* (check only one item in section (a))
    ____ (1) Exploratory
    ____ (2) Single dominant hypothesis
    ____ (3) Multiple hypotheses (two or more reasonable hypotheses)
    This category is of particular interest to the *JoF*.
    (b) *Descriptive*
    ____ (4) Describes methods
    ____ (5) Describes data
    ____ (6) Theory (mathematical or verbal)
    ____ (7) Case study

(c) *Other*

____ (8) Review

____ (9) Does not fit above classification. Please describe:

Questions 12 to 18 relate primarily to *empirical papers*. If not relevant, go to question 19.

12. How surprising did you find the results to be?

( )	( )	( )	( )	( )	( )
1	2	3	4	5	not
not at all surprising				very surprising	applicable

13. Did the design of the study help to ensure objectivity?

____ Yes ____ Not applicable

____ Not sure

____ No

14. Were the research methods appropriate?

____ Yes

____ No (explain)

15. Was sufficient information on the *method* and the *data* provided to allow for replication by others:

____ (a) Definitely ( ) Not applicable

____ (b) Not sure (explain)

____ (c) No (explain) ________

16. Was supporting information provided (if not available in previously published sources)?

____ Yes

____ No ( ) Not applicable

If yes, was it sufficient? ____ Yes ____ No

If 'no' or 'not sufficient', explain what is needed

17. Can you imagine any reasonable basis to expect that the results could have turned out otherwise?

____ Yes

____ No

(If 'no', the paper would be regarded as of little importance.)

18. Does this paper make a significant contribution beyond what the author has published in (submitted to) other journals? [To be answered by the Associate Editor.]

____ Yes

____ No

EVALUATION

Before making your recommendations, please review your evaluations for questions 1, 2, 3, 11a(3), 12, 15 and 17. Then answer *both* questions 19 and 20.

19. Summarize reasons *in favour of* publishing the paper in the *JoF*.
20. Summarize reasons *against* publishing the paper in the *JoF*.
21. What is your overall opinion?

____ (a) publish as is

____ (b) publish with minor revision

____ (c) publish with major revision

____ (d) reject

22. List alternative journals that would be relevant for this article (if you selected alternative (d) in question 19).
23. Please provide a listing of mistakes in the margin of the manuscript. (If not obvious from notes in the margins, describe how errors could be corrected on a separate page or in your letter to the Associate Editor.)