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## Peer Review for Journals as it Stands Today—Part 2

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*This two-part article reviews the current literature on journal peer review. Research on this subject has grown during the 1980s and 1990s and has increased our awareness of both the myths and facts about peer review. Part 1 summarizes research findings on the participants in the system (the appointment mechanisms of editors and referees, and reviewer tasks and qualifications) and systemic problems of reliability, accuracy, and bias. Part 2 describes current research on how fraud, favoritism, and self-interest may affect the review system and on such policy issues as interference of particularistic criteria; connections among editors, authors, and referees; and double-blind review. Although the literature indicates that peer review has many problems, the author concludes that it is difficult to imagine how science could advance without such a key quality control mechanism.*

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## ***Peer Review for Journals as It Stands Today—Part 2***

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*In the first part of this article, which appeared in the March 1998 issue of *Science Communication* (Volume 19, Number 3), I reviewed research findings about the participants in the peer review system and various problems related to reliability, accuracy, and bias. The second part summarizes current research findings about fraud, favoritism, and self-interest in peer review, beginning with what happens when manuscripts are rejected. In addition, the review examines what is known (and not known) about other important topics and policy issues, about how particularistic criteria may affect the review process, and about the role played by professional connections among editors,*

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*Author's Note:* The author thanks Jerry Keller for his help in the writing of the article and the *Science Communication* referees for their comments. Address correspondence to Juan Miguel Campanario, Grupo de Investigación en Aprendizaje de las Ciencias, Departamento de Física, Universidad de Alcalá, 28871 Alcalá de Henares, Madrid, Spain; phone: 34-91-8854926; fax: 34-91-8854942; e-mail: fscampanario@alcala.es.

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authors, and referees. I close with consideration of the research on double-blind review.

### ***Reconsideration of Rejected Manuscripts***

What happens when authors get rejection letters? A common myth is that a rejection decision is always irrevocable, perhaps because most journals do not have formal appeal mechanisms or describe in their guidelines how an author might attempt to reverse a negative decision. *The Lancet* was the first journal to appoint an ombudsman, whose task is "to record and, where necessary, to investigate episodes of alleged editorial maladministration" (Horton 1996, 6). However, in psychology, for example, only three of thirty editors surveyed by Hartley (1988) responded positively to the question: "Do you have a formal appeals procedure to resolve possible disputes between editors and authors?" Another eighteen editors stated that they rely on informal procedures to deal with above-mentioned conflicts. Actually, by means of these informal procedures, and contrary to all odds, some authors manage to convince editors to reverse their decisions concerning the rejection. On occasion, editors must deal with resubmissions, even though authors have been told in strong language that a manuscript is of no publishing value (Neal 1994). When authors of rejected manuscripts complain to editors, this action can terminate with a reiteration of the original verdict, with a new refereeing process, or even with a request to the authors to revise and resubmit the manuscript.

Simon, Bakanic, and McPhail (1986) followed the fate of rejected manuscripts as well as authors' reactions when a complaint was lodged to the editor of the *American Sociological Review*. Sixty percent of the time, the editor rerouted the paper to new reviewers; in 22 percent of the cases, the original decision was reiterated without a new refereeing process. Thirteen percent of authors who complained were fortunate enough to have the decision reversed, and their manuscripts were accepted for publication. Authors with doctoral degrees from Ivy League universities were overrepresented among those who took their complaints to the editorial boards. Interestingly, 27 percent of authors who complained had served as referees for the journal during the previous year, while these referees accounted for 16 percent of submissions. Hence, referees were more likely to complain about other referees' decisions. This finding may indicate that they knew the journal and the system.

From a survey of two groups of sixteen and eighty-six medical journals, Weller (1991) concluded that 62 percent of editors in the first group had submitted rejected papers to a new round of reviewing in favor of the authors

who complained about the final verdict. Editors in the second group did that only 14 percent of the time, and they contacted authors to explain the verdict 69 percent of the time. Alfred Soffer (1987), editor of *Chest*, found that about 50 percent of rejected authors who engaged in a dialogue with reviewers had their manuscripts accepted for publication.

Most leading journals assume that authors of rejected manuscripts will attempt to have them published at any cost, and therefore they offer counsel about where to submit a rejected manuscript (e.g., see the latest guidelines to authors of *Science* and *Nature*; also see Eichorn and Van den Boss 1985; Hernon, Smith, and Croxson 1993, table 4; Stossel 1985).

### ***Fraud, Fantasy, and Mistakes***

Referees often do not discover main errors in fraudulent papers. According to Stewart and Feder (1987), there were twelve errors per paper in eighteen manuscripts written by John Darsee and published in major biomedical journals. Instances of scandals involving plagiarized papers or papers with fabricated data are publicized on an almost weekly basis in the news sections of leading journals such as *Science* and *Nature*, and the inability of the peer review system to detect and prevent fraud seems to be proven beyond doubt. Consider, for example, the controversy involving a paper charged of being fraudulent, which was published in the February 1994 issue of the German journal *Angewandte Chemie*. According to the submitted article, a static magnetic field can induce chemical reactions in favor of chemical species over the other complementary chemical entity or "enantiomers." After clearing the usual peer review process, the paper was published, but it was retracted when evidence against the results began to accumulate. According to one prestigious chemist, the results were too good to be true (Clery and Bradley 1994); however, referees had not detected the mistake.

In the behavioral sciences, some classic experiments have proved to be classic blunders because referees did not discover gross mistakes in manuscripts published by eminent psychologists (García 1981). Armstrong (1996) cites an experiment done by J. B. Watson on conditioning of behavior, which was based on data from a "sample" of one baby. Descriptions of the experiment kept appearing in textbooks without reference to the failed replications. One controversial economics study of social security contained a major programming error; its correction led to results that were the opposite of those originally published (Pressman 1994, n. 5).

According to Gingold (1973), the cumulative number of publications dealing with anomalous water ("polywater") reached 500. Some theoretical

models were even devised to explain the structure of such a polymer, which eventually proved to be a mixture of silica, water, and other substances (Benguigui 1993). Experiments showing evidence for a heavy neutrino in 1985 began a series of other experiments and the publication of many articles over the course of eight years. Most researchers believe that there is no such heavy neutrino (Morrison 1993).

Robin and Burke (1987) have listed many instances of what they have coined "medical fantasies," which were published in reputed, peer-reviewed journals. Among these are that bronchial asthma is caused by worms, that Alzheimer's disease is ameliorated by intrathecal cholinergic drugs, or that cerebral ischemia could be treated with barbiturates. Other instances of errors that passed peer review are documented by Franke and Kaul (1978), Gorn (1982), Shapiro (1985), and Wrege and Perroni (1974).

Other reputable journals have published numerological fantasies. *Nature* published a short note relating the fundamental constants in physics to the number  $\pi$ , and many years ago, the *Journal of Physical Chemistry* published an article on the relations between fundamental physical constants by a scientist named J. E. Mills. Unfortunately, the relationship was dependent on the units in which the constant were expressed. Numerological fantasies on protein synthesis that originated in inaccurate experimental data have also been published in peer-reviewed journals (Klotz 1995).

Even Nobel Prize winners have committed mistakes in print. A well-documented example is Pauling and Corey's published model of a DNA molecule containing three helixes, which was later proven to be wrong (Olby 1994, chap. 20). Fibiger's work on the propagation of malignant tumors was awarded the Nobel Prize but turned out to be altogether mistaken (Zuckerman 1977, 38). Luce (1989) considers mathematical learning theory as another example of false beginnings. Another instance is the often used formula for estimating "heritability," which was wrong because of a mistake in its derivation. This mistake was finally discovered after sixty years of uninterrupted application.

## **Reviewer Bias**

The editors of the *New England Journal of Medicine* state that rejection is motivated by "the paper's lack of originality, its dubious scientific precision, style or an appeal to readers" (Kassirer 1992, 1238). But research shows that other factors, such as bias, negligence, and favoritism, may also play a role. The results of various theoretical simulations seem to support the hypothesis that type II errors (recommending not to publish papers that

should have been published) are inherent to the peer review system. For example, Stinchcombe and Ofshe (1969), using a theoretical model of the editorial review process, estimated that about half of the interesting papers submitted to a journal eventually will be rejected.

Furthermore, referees may believe that for their reviews to “count,” they must find something wrong in a manuscript. In consequence, referees often give in to fault finding (Finke 1990, 669). According to L. R. Pondi, former editor of *Administrative Sciences Quarterly*, “[O]ur present corps of reviewers have been trained and conditioned in a persecution mentality—through poor treatment of their papers by other reviewers” (quoted in Eysenck and Eysenck 1992, 394). This viewpoint is supported by the findings of Fiske and Fogg (1990), who studied the reports elaborated by referees in nine American Psychological Association (APA) journals and discovered that reviewers often focused on weaknesses in the papers and that positive features of papers were described in broad and imprecise statements. Boice et al. (1985) also found criticism to be more frequent than praise during their study of refereeing practices in the *Journal of Applied Behavior Analysis*. And Bakanic, McPhail, and Simon (1989) showed that no manuscripts in a sample of those submitted to *American Sociological Review* from 1977 to 1981 received unequivocally favorable reviews. The mean number of negative versus positive comments was 15.5 and 3.2 for rejected manuscripts and 15.3 and 4.2 for accepted ones. Confidential comments also tended to be negative, and if by chance they were positive, such comments were brief and stated in general terms (Bakanic, McPhail, and Simon 1989, 644). Most reviewers probably know that praise—while pleasant to the ego—will not help the author.

In addition, even conscientious reviewers may succumb to dogmatism, narrow-mindedness, and subconscious bias (Glenn 1976, 181). For example, B. F. Skinner once revealed that he initially refused to examine the arguments of his critics (Mahoney 1979, 352), and the famous economist John Maynard Keynes is known for the dubious honor of rejecting many innovative papers while he was the editor of *Economic Journal* (Gans and Shepherd 1994).

García (1981, 149) has charged editorial consultants with being “neophobic,” and Horrobin (1974, 218) believes that “there is an objective evidence that some referees, and even some highly respected individuals in top academic positions, are at best ignorant and careless and at worst, deliberately obstructive,” documenting his assertion with instances of unfair or wrong referee reports.

There is evidence that referees score papers according to whether the results agree or conflict with their own beliefs. Goodstein and Brazis (1970) demonstrated that referees were more severe with papers on astrological

predictions of vocational choice when the results confirmed such relationship. Another experimental study asked thirty-three first authors of research papers to review part of a fictitious paper on electric stimulation of nerves (Ernst and Resch 1994). Their results show that these referees were clearly influenced by their own preconceptions, such that referees who might be expected to agree with a paper's findings tended to judge it less harshly than referees who might be expected to disagree. When Mahoney (1977) asked seventy-five journal reviewers to referee hypothetical manuscripts that described identical experimental procedures but were varied according to positive, negative, mixed, or no results, he found that referees were biased strongly against manuscripts that reported results contrary to their theoretical perspective. Similarly, Abramowitz, Gomes, and Abramowitz (1975) also found that referees and editors tend to reject findings that contradict their own beliefs. The researchers constructed two versions of an empirical report on the psychological well-being of two types of student behavior: political activism and noninvolvement. They discovered that referees' political ideologies strongly influenced their recommendations for publication of the reports. Under some circumstances, referees were inclined to justify their decisions on methodological bases although they were actually biased by their beliefs. Such findings should not seem strange, however, because scientists often become "ego involved" in their theories and they may be willing to persevere indefinitely, despite evidence against such theories. As Boring (1964, 682) said, "[A] theory which was built up its author's image of himself has become part of him. To abandon it would be suicidal, or at least an act of self-mutilation."

Paucity of publishing space is also a reason (to a certain degree) for many type II errors. In addition, referees are rarely supplied with a precise operational definition of what characteristics and qualities must be present to accept a manuscript for publication (Bornstein 1991, 447). Although researchers may agree on the "normative" criteria to be applied in judging a paper's scholarly worthiness, they may disagree on the application of these criteria to given manuscripts (Cone 1991). In the absence of formal or rigid guidelines, it should not be surprising that referees resort to "heuristics" (Harcum and Rosen 1993, 5). As one of these heuristics, Beyer (1978, 82) noted, reviewers often "are looking for something in a submitted paper which would have justified its rejection, and given the low consensus in the social sciences over many issues, they usually accomplish it." These heuristics can also crystallize in bias against negative results or against replication.



## ***Reviewer Negligence***

Another source of errors during the reviewing process is the sometimes careless evaluation of manuscripts submitted for publication. Harcum and Rosen (1993, 6) complained that editors sometimes do not read papers with due attention. Similar opinion was expressed by Crandall (1991), former editor of the *Journal of Social Behavior and Personality*. Hughes (1979) described how editorial meetings are notoriously casual and unbusiness-like; no notes are taken, and there is no record on decision making. One study found editorial correspondence to be so incomplete that the researchers could not calculate the number of manuscripts submitted and rejected throughout the given period (Hernon, Smith, and Croxen 1993). According to Glenn (1976), editorial decisions in sociology are often based on hasty, careless, biased, and/or incompetent refereeing, and as early as 1974, Ingelfinger complained that many reviewers are too busy to give a manuscript required attention. More than twenty years later, Parmley (1995), editor in chief of the *Journal of the American College of Cardiology*, complained in a similar fashion and noted that peer review as a whole is conducted less rigorously now as it was before. Other authors believe that highly original and innovative advances in a given field are rejected due to the referees' whimsical stance and often consider them "uninformed, quixotic or simply irresponsible" (Finke 1990, 669). Actually, the most common complaint (54 percent) from authors of rejected papers to the editor of *American Sociological Review* was about the reviewers' incompetency (Simon, Bakanic, and McPhail 1986). In addition, having a favorable referee's report does not guarantee publication. For example, 22 percent of papers with favorable recommendations from two reviewers who suggested either to accept or accept with revision were rejected by the *American Journal of Public Health* in 1982 (Yankauer 1982).

The above reasons could help to explain the type II error, but what about type I errors (recommending to publish papers of low quality)? Endres and Wearden (1990) suggest that reviewers play an agenda-setting role in which they are more apt to find acceptable only the theoretical paradigm and research methodologies that are in vogue in the academies where they are employed or earned their doctorate degrees. Gordon (1979) found that physicists were more apt to give favorable recommendation to papers written by authors from universities with which they were affiliated. Manuscripts with content parallel to an editor's background were, on the whole, better treated and more likely to receive encouragement for revision and resubmis-



sion than to face rejection from the *Academy of Management Journal*, although after resubmission there was no difference in the rate of acceptance for original manuscripts foreign to the editor's scientific training (Beyer, Chanove, and Fox 1995). Factors other than quality or significance often are considered as part of publishing merit. According to Yankauer (1982), factors other than quality or significance accounted for one-third of acceptances and one-fifth of rejections. The former editor of *The Lancet* suggests that some of these factors are linked with past journal practices of admitting routinely covered topics, editors' inclinations, or geographical similarities (Fox 1965). As Lock (1985, 128) explained, "[V]alidation is not secured by the conventional peer review system because this is a role of the time."

### ***Favoritism and Self-Interest***

Universalism dictates that judgments should be based only on considerations of scientific merit (Merton 1973). Criteria such as status, sex, social upbringing, or membership in a particular social organization should never hinder fair decisions about paper acceptance. Unlike the assessment of grant proposals, the evaluation of a manuscript deals with finished work; the previous record of scientific achievement of the authors should be irrelevant. However, there is a significant body of literature, which suggests that particularistic criteria may influence many evaluative decisions (Willis and McNamee 1990).

One former editor of *Explorations in Economic History* complains that some referees find it apparently impossible to appraise a manuscript in a review without simultaneous appraisal of the author, and in consequence harsh commentaries are often found in referees' reports, influenced by partial judgments (Neal 1994). Authors have complained about preferential treatment given to other authors because of their academic status. Contributions from lesser known institutions may be less likely to be accepted for publication than comparable contributions from scholars at world-renowned institutions (Biggs 1990, 148; Garfield 1986a, 8). On occasion, editors and referees may compete for the same journal space (Biggs 1990; Campanario 1996; Chubin 1985; Gordon 1977; Yankauer 1991), and in consequence they have been steered by self-interest to give negative comments. Referees may attempt to gain status and maintain their superiority by undercutting an author's career (Gordon 1977; Wilson 1978; Wright 1970). As the young Thomas Henry Huxley once wrote to a friend (quoted in Mahoney 1979, 358), "I know that the paper I have just sent in (to the Royal Society) is very original and of some importance, and I am equally sure that if it is referred to the

judgment of my 'particular friend' it will not be published." Sometimes "poor" reviews underline a reviewer's perverted misconception that the author failed to cite his own work (Stossel 1985, 658).

Unwarranted rejection may also be fostered by editors' questionable conduct, such as deliberately sending articles to reviewers who hold opposite ideas (Cotton 1993). Reviewers may also purposely delay the appearance of research that competes with their own (Biggs 1990; Chalmers, Frank, and Reitman 1990; Grouse 1981; Meadows 1977; Rodman 1970). In the era of superspecialization, it is even more probable that a peer reviewer is also a scientific rival (Riggs 1995). Perhaps the most paradigmatic and quaint example of this assumption is the sentence from a referee's report that stated, "if this were true, I would have already described it" (quoted in Riggs 1995, 255).

The referees' anonymity can exacerbate the climate of abuse. Under such circumstances, the act of manuscript submission automatically exposes an author to the viciousness and malignant envy of an anonymous rival should he or she happen to be a reviewer. Crandall (1982, 207) formulates some of the harshest criticisms against the peer review system when he asserts that "the editorial process has tended to be run as an informal, old-boy network which has excluded minorities, women, novel researchers, and those from lower-prestige institutions." Sharp (1990) identified seventeen potential sources of bias in manuscript evaluation, including "known antagonisms, including those noted by author," "known competitors, including those noted by author," and bias "for or against certain institutions/individuals."

If a journal is owned and/or sponsored by a professional association, routes for appeal against perceived abuse probably exist; however, for publications which are commercial ventures, there is no means for recourse against abuse of power and authority, and it is extremely difficult to publicize these cases. Most of the journal guidelines deal only with the obligations imposed on authors. There is no universally available guide which spells out obligations and responsibilities of editors toward the authors, although such guides have been issued for editors in biology and biomedicine (see the publications of the Council for Biology Editors, for example). Among other exceptions, the journal *Addiction* attaches a notice to submitted manuscripts that reminds referees of the rules of fair reviewing (Edwards et al. 1995); the *British Medical Journal* prints a list of rules on the back of every letter it sends to a referee; and the leading journals *Science*, *Nature*, *New England Journal of Medicine*, and *Journal of the American Medical Association* also send guidelines to referees (Marshall 1995). As noted above, *The Lancet* was the first journal to appoint an ombudsman.

Since publication in leading journals is considered evidence of competence, if an author manages to "put" a few papers in leading journals thanks

to the interference of particularistic criteria, the result may be taken as a sign of excellence according to strictly universalistic criteria. The state of reward or recognition seldom slides backward (Cole and Cole 1973). Consequences can be dramatic for novel scientists who are at the doorsteps of their careers and encounter great difficulties in publishing their first papers.

In a study of favoritism, Jacobson (1986) found that between two-thirds and three-fourths of the political scientists and sociologists surveyed believed that the peer review system for scholarly journals in their field was biased in favor of scholars from prestigious institutions. According to Bakanic, McPhail, and Simon (1987), many scholars believe that leading journals are likely to publish only those manuscripts that reflect the editors' theoretical, methodological, or substantive interests. However, of the fifteen editorial practices considered in a survey of marketing faculty by Sherrell, Hair, and Griffin (1989), the two deemed least ethical were (a) favoritism to friends and personal associates by an editor or reviewer and (b) selecting referees that are biased for or against the manuscript's content in order to ensure acceptance or rejection.

Institutional favoritism may be the most pervasive. Harvard psychologist Robert Rosenthal (1982) describes how, while he was a young member of the psychology faculty at the University of North Dakota, he was unable to publish fifteen to twenty papers he wrote. Within a few years of his move to Harvard, most of these articles had been accepted by the same journals that had previously ignored them. The editor of *Southwestern Political Science Quarterly* reports that one advisory editor wrote about a manuscript submitted from elsewhere in the United States: "I believe authors who earn their living in the Southwest should have priority in claiming space of the *Quarterly*" (Bonjean 1994, 113).

Some have tried to study systematically the influence of particularistic criteria exhibited by editors and referees at the moment of the final decision. Kerr, Tolliver, and Petree (1977) surveyed reviewers for management and social science journals and found that 30 percent of reviewers acknowledge that an author's good reputation in the area of interest would add to the probability of acceptance. Similarly, Rowney and Zenisek (1980) surveyed psychology reviewers about the criteria they used in evaluating manuscripts. Referees frequently mentioned "strong reputation" as one of the criteria that influenced acceptance.

A different approach is to analyze the actual referees' reports. Spencer, Hartnett, and Mahoney (1986) designed a psycholinguistic analysis to measure bias, emotional factors, sensibility levels, motivation, and a series of personality-related features. Their analysis revealed that 40 percent of the reports contained more than 25 percent emotional persuasions and unsubstan-

tiated comments, indicating intrusion and prejudice during the review process. Some editors report, in fact, having reprimanded referees for occasional ad hominem remarks (Scarr 1982, 239).

Other researchers have studied the relative influence of the reviewers' or authors' status in manuscript acceptance. The Zuckerman and Merton (1971) study, one of the first, showed that there is no consistent relationship between referee acceptance or rejection of physics manuscripts and the relative standing of authors and referees. Lock (1985) followed the fate of 1,588 manuscripts submitted to the *British Medical Journal* and found no difference in recommendation between external reviewers with academic positions and those without academic affiliation.

Ernst and Kienbacher (1991) have demonstrated that there does exist, however, a "national publication bias." They examined all the papers published during 1990 in four journals in Britain, Sweden, the United States, and Germany and showed that an editor was more likely to accept articles from authors who were from the country in which that particular journal was published. Peters and Ceci (1982a, 1982b) claimed to demonstrate the existence of bias toward authors from high-prestige institutions, but many commentators rejected these conclusions, claiming that there was a variety of methodological flaws in the study design, including lack of a control group and the small number of journals "used" as subjects (much of this criticism can be found in the same issue in which the Peters and Ceci article appeared).

Highly talented scientists, of course, tend to be more frequently associated with prestigious institutions. When Koch-Weser and Yankauer (1993) evaluated the authorship characteristics, editorial processing, and fate of a set of manuscripts submitted to the *American Journal of Public Health* in 1989, they found that the acceptance rate of manuscripts whose first author was a citizen of a developed country was substantially higher than that of manuscripts whose first author happened to be a subject of a developing country. Many such manuscripts dealt with routine local service statistics, however, or failed to stir commotion in the field of international health. Prescott and Csikszentmihalyi (1977) further elaborated on the influence of affiliation with a prestigious or nonprestigious academic institution and publishing patterns in psychology journals; they observed that, in the three most theoretical (and possibly most influential) journals, institutional prestige appeared to be correlated with enhanced probability of having the article published. Patterson (1994), editor of the *American Political Society Review*, developed similar results for political science manuscripts: 28 percent of submitted manuscripts and 51 percent of those published came from more prestigious departments, whereas 55 percent of submitted manuscripts and 40 percent of published papers came from departments rated below the top forty on

prestige. Such results may, of course, be related to the fact that top departments are larger, are more research oriented, and tend to attract the most active researchers and scholars.

Referees and editors could be influenced by the author's track record. Manuscripts with some "in press" self-references are more readily accepted than the same manuscripts with "in press" references credited to others (Mahoney, Kazdin, and Kenigsberg 1978).

Wilson (1982) has proposed that bias in the journal review process is present toward the investigators but not toward the institutions. Perhaps favoritism shown to an individual who has previously contributed significantly to a given subject matter reflects the overall accuracy of an editorial decision. Gordon (1979) studied referees' evaluation patterns in articles submitted by British physicists, dividing a sample of authors and referees into two sets corresponding to major and minor universities. His results indicate that major university referees for two British physical science journals were more favorable to papers from major universities than to those from minor universities, and that this was not true for referees at minor universities. Abt (1987) demonstrated that the final acceptance rate in *Astronomical Journal* was higher for papers by well-known astronomers than for papers by all other astronomers: 95 percent of well-known astronomers' manuscripts were accepted for publication compared to 83 percent for a control group. A study of reviewers' recommendations and editorial decisions for brief reports and major papers submitted to *Journal of Pediatrics* demonstrated that for the brief reports, lower institutional rank was associated with lower rates of recommendations for acceptance (Garfunkel et al. 1994), and for the major papers, there was no relationship between institutional rank and either the reviewers' recommendations or the acceptance rate.

Laband and Piette (1994a) used a more subtle approach, originally intended to study the impact of blind peer review; however, their results can be extrapolated to the issues addressed here. According to this approach, referees knowing the authorship beforehand could substitute the value of the previous contribution for the evaluation and forecast of the marginal contribution contained in the new manuscript under present consideration. This latent form of bias has also been noted by Lock (1985), who refers to it as a "halo" effect, in that decisions by reviewers and editors may be biased in favor of better known authors. Institutional affiliation and authors' additional personal factors could tip the balance of referees' evaluations. Laband and Piette (1994a) sampled articles published in economics journals during 1984 and studied the citations to these articles. Their results showed that articles published in journals using blind peer review were more cited than those published in journals that used non-blind peer review methods. They controlled for attributes

such as article length and journal quality. From these results, Laband and Piette concluded that, when forced, reviewers do substitute particularistic criteria (based on the author's prestige) for universalistic criteria (based on the manuscript's merit) in their evaluation of manuscript content.

Beyer, Chanove, and Fox (1995) followed the fate of 400 manuscripts submitted from 1984 to 1987 to the *Academy of Management Journal*, which uses a blind referee system. They investigated the influence of professional rank, department rank, and research funding on the reviewers' recommendations and found that associate professors earned better recommendations from reviewers than full professors.

## ***Special Policy Issues***

### ***Particularistic Criteria***

Although universalism ordinarily should dictate coexistence in the scientific community (Merton 1973), there are occasions in which particularistic criteria are exercised in the scientific publication process. Are there instances when personal or ascriptive interests should have a place in the editorial process? Some circumstances do, in fact, seem to justify the differential treatment of authors. The editors of *Personality and Individual Differences*, for example, state that they are inclined to give slight preference to submissions from countries whose authors have not been well represented in the past, provided that the quality justifies such decision (Eysenck and Eysenck 1992, 396). Many editors in economics have complained about a shortage of exceptional papers, leading them to scout academic meetings in order to identify innovative and original papers (Laband and Piette 1994b). Some journals, however, find this practice unethical. Many other journals invite famous scientists to write special articles that may escape the scrutiny of the usual peer review. Members of the U.S. National Academy of Sciences may publish in the *Proceedings of the National Academy of Sciences* without the ordinary external refereeing. In these situations, particularistic decisions may not be necessarily harmful if they are intended to benefit science and to increase the quality and status of a given journal, although, as noted, there are critics who disagree.

In the biomedical sciences, there are other circumstances that might justify the use of particularistic criteria in the evaluation of scientific manuscripts. The increasing dependence of biomedical research on private funding and the growing number of researchers linked by research projects to pharmaceutical companies has resulted in new disclosure policies at many journals. The



reasoning, according to Drummond Rennie, is that clinical papers are so "close to the prescription pad" (quoted in Barinaga 1992a, 618). According to some recent studies, the problem is far from trivial. Krinsky et al. (1996) found that 34 percent of a sample of articles published in 1992 in the life sciences had a first or last author who had a financial interest in the described research (financial interest was defined as the authors being on a patent or patent application, serving on a scientific advisory panel of a biotechnology company involved in a related product, or serving as an officer or shareholder of a company with commercial ties to the research). In another example, the value of shares in one company rose from \$1.50 to \$61.12 after publication of a journal article favorable to the company's new anti-obesity drug (Wadman 1996). As a result of such circumstances, journals are asking prospective authors to reveal in advance any conflict of interest which could influence their conclusions (Rothman 1993). According to Schulman, Sulmasy, and Roney (1994), nine of fifteen major medical journals report that they now request disclosure of the financial arrangements between the author and the sponsor of his or her research.

This policy can lead sometimes to extreme situations. For example, in 1990 the *New England Journal of Medicine* refused to publish review articles or editorials written by someone related to a "company whose product figures prominently in the article or with a company making a competitive product." According to Rothman (1993, 2784), this *New England Journal of Medicine* policy "dispatches any pretense that a work should be judged on its merits alone."

Other problems may arise when a scientist with commercial ties is asked to evaluate a paper with potential conflict of interest. Scientists seem to agree that it is inappropriate to referee papers from companies in which they have financial interest (Barinaga 1992b), and similar disclosure rules could be established for referees. However, journal editors may be worried about diminishing the potential referee pool in many areas of biology where it is hard to find a researcher who does not hold biotech equity (Barinaga 1992a).

Discussion about disclosure and conflict of interest probably will intensify in the near future. However, conflict of interest is not only limited to money. According to the *Science* guidelines, the range of factors that might create a conflict of interest could include sexual orientation (in studies on sexual behavior) or belief in specific scientific theories. Prospective authors who submit manuscripts to *Science* are asked to examine their own biases honestly because they are now being asked to reveal "any relationship that they believe could be construed as causing a conflict of interest, whether or not the individual believes that is actually so" (Marshall 1992, 624).



### *Longstanding Connections and the Invisible College*

Most scientists believe that editors behave as true gatekeepers; that is, editors are obliged to act without bias and to identify only those papers with the highest marginal contributions to the science (Smith and Laband 1995). Crane (1972) coined the term *invisible college* to denote a small community of scientists who exchange information and cultivate a position of power within a given branch of a discipline. The members of an invisible college apparently are acquainted with each other and probably read and criticize colleagues' work in manuscript form. Invisible colleges could result in favoritism in the publication process.

Pfeffer, Leong, and Strehl (1977) examined the relationship between institutional representation on editorial boards and institutional contributions to the disciplines of chemistry, sociology, and political science. The effect on publication outcome of institutional representation in editorial positions was assessed, controlling for the measures of institutional quality and size. The institutional representation on editorial boards had a strong influence on publication in political science and sociology but not in chemistry. Yoels (1971) found a striking similarity between the schools that ranked in the top ten for doctoral origins of editors on *American Sociological Review* and the schools that ranked in the top ten for the doctoral origins of the contributors. Shamblin (1970) found that the journals edited by certain universities tended to publish a larger proportion of papers from their own alma maters. For example, the University of North Carolina ranked first in papers by its recent graduates in *Social Forces*, which is edited at the University of North Carolina, and the University of Chicago ranked first in publications by faculty and first in publications by recent graduates in *American Sociological Journal*, which is edited at the University of Chicago. Cole and Bowers (1973) reported that a handful of schools dominated six communication journals, and Yotopoulos (1961) showed that some journals edited at a particular university tended to publish a higher proportion of articles by authors working at that university. Laband (1985) offered further evidence of this effect by showing that a large fraction of articles published in the *Journal of Political Economy* were authored by scholars with ties to the University of Chicago, and in the 1970s, articles by Chicago faculty were longer than articles by other authors. The fraction of Chicago-affiliated authors in that journal has been reduced over the past forty years, however (Siegfried 1994). The editor of *Econometrica* had to admit that many coeditors of that journal were also frequent authors in the journal (Deaton et al. 1987). A study of the editorial decision making for *Rural Sociology* showed that five factors

appeared to influence acceptance. One of these factors was membership in the Rural Sociological Society. However, the investigators attributed this difference to the fact that membership helped authors write manuscripts that were appropriate for the content of the journal (Warner et al. 1985, 618). Crane (1967) examined academic similarities of the contributors and editors for three social science journals and showed that academic affiliation and doctoral institution were similar for journal editors and contributors.

In a study of school psychology journals, Kawano et al. (1993) demonstrated that, from 1981 to 1986, 26 percent of editorial board members published articles in the journal or journals for which they served as board members, whereas 8 percent published articles in journals with whom they had no affiliation whatsoever. On average, 41 percent of the articles in the three journals were authored or coauthored by at least one editorial board member. In one journal, the ratio of the number of editorial board members to the number of nonaffiliated authors was only 13 percent, but more than 40 percent of the articles were authored or coauthored by at least one of the editorial board members.

Willis and McNamee (1990) studied the institutional connections of editors and authors in leading sociology journals, finding a pattern of connections that exceeded chance alone: "the links between editors and authors represent spheres of influence that increase the probability of publication in the elite journals within the field, thereby contributing to the persistence of accumulated advantage over time" (p. 374).

The relationship between editorial board members and authors may contribute to published papers of a higher quality, of course. A scenario in which editors use their position and influence to seek out and publish good papers could not be harmful. For this reason, one must also evaluate the quality of the output to see if there are qualitative differences between the papers authored by scholars with professional ties to an editor and those authored by scholars unknown to the editor. Schaeffer (1970), for example, selected a set of APA and non-APA journals and classified papers into three groups: (a) at least one of the authors was an editor of the journal, (b) at least one of the authors was affiliated with the same institution as an editor, and (c) none of the authors was an editor or was affiliated with the same institution as an editor. He found some evidence of favoritism toward editors but, interestingly, the more "rigorous" a journal is, the less likely it is to publish papers by anybody other than its editors or its professional staff.

Using a similar approach, Laband and Piette (1994b) analyzed citations to articles in twenty-eight top economics journals during 1984, identifying papers in which author-editor connections were obvious (i.e., whenever any author or coauthor was affiliated with or received the Ph.D. from the same

university as the editor). Nearly one-quarter of the entire sample showed such an author-editor connection. In addition, regression analyses revealed that the sources of the most heavily cited papers were the editors' colleagues, not people who had graduated from the same departments as the editors. In a follow-up study, Smith and Laband (1995) used citation analysis to study the impact of papers published in fifteen accounting journals, excluding self-citations from the counts. They found that the mean number of citations to published articles for which they could identify an author-editor connection was more than triple the mean number of citations to articles without such a connection; furthermore, authors with editorial connections tended to publish in higher status journals and had more citations in the previous five years than authors with no editorial connections. Almost one-half of the papers studied received no citations whatsoever over the five years after publication. Of these uncited papers, 72 percent were by authors lacking connections to the editors from the relevant journal. In addition, 63 percent of papers written by authors with no editorial connections were not cited in the five years subsequent to publication. In sharp contrast, only 31 percent of papers written by authors with editorial connections were uncited during the five years following publication. Smith and Laband concluded that, rather than printing shoddy material written by colleagues or friends, the editors of these journals lean on their professional connections to locate some really good papers. A practice interpreted by many scholars as favoritism, therefore, may in fact serve to enhance efficiency in the knowledge market.

There are other possible explanations for such findings. Competent researchers tend to be associated with other competent researchers (Schaeffer 1970). It would not be unusual for papers by overachievers to be highly cited; that is, a link between authors and editors may reflect not particularism but high quality. This issue unquestionably merits further research. Perlman (1982) attempted to do so in response to the Peters and Ceci study. Perlman selected thirty articles from the *Journal of Abnormal Psychology* written by authors affiliated with prestigious institutions and thirty other articles written by scholars at lower prestige institutions. He discovered that articles by scholars affiliated with high-status institutions were cited considerably more often. Similar results were obtained using a second sample drawn from the *Journal of Personality and Social Psychology*, which (unlike the other journal) uses blind review. Perlman concluded that editors may be justified in using institutional prestige as a factor in decision making.

Campanario (1996) studied competition for journal space among referees, editors, and external authors and its influence on journals' impact factors. He reviewed papers published in eighteen educational psychology journals and classified them according to the authors' connections to the given journal that

published them. A paper was credited to a journal-related author whenever one or more of the authors was a referee or an editor for that journal for at least one year or four issues prior to the publication of a paper that he or she authored or coauthored. The results showed that the percentage of journal-related authors and the use of journals by them varied across journals. For example, the percentage of a journal's space occupied by papers authored or coauthored by journal-related authors varied from less than 1 percent to 64 percent, with a mean of 34 percent. There was a positive relationship between the use of a journal by a journal-related author and the journal's impact factor. The most obvious interpretation of this analysis was the following: authors serving as editors or referees in educational psychology journals tend to care about the quality of these journals.

From the above, we might conclude that more research is needed to clarify the influence of connections between authors and editors because previous research has lacked the necessary controls for the quality of published papers.

### *Double-Blind Review*

In an attempt to eliminate some of the drawbacks of the peer review system, many journals resort to a double-blind review system, keeping the names and affiliations of both authors and referees confidential. Double-blind review is motivated by a desire to preserve anonymity and thereby assure fair play. As Rothman (1993, 2782) pointed out, "informed judgment is not a better judgment." Double-blind refereeing has been acclaimed by some authors as a panacea. Others postulate that shifting to double-blind review would help remedy numerous imperfections or would at least be a step in the right direction (Armstrong 1982; Lock 1991; Nelson 1982; Palermo 1982; Perlman 1982; Presser 1982; Rosenthal 1982; Scarr 1982; Tax and Rubinstein 1982; Wilson 1982).

The extent of the use of double-blind review varies among disciplines. Blank (1991) reports that among thirty-eight well-known journals in different fields, eleven used double-blind refereeing. Other surveys suggest that the majority of physical and life science journals do not practice double-blind review and that this approach is more common in the social and behavioral sciences (Yankauer 1991). According to Cearly and Alexander (1988), fewer than 20 percent of selected English-language medical journals use double-blind review.

The main arguments that critics brandish against double-blind review were summarized by Ceci and Peters (1984) as follows: it has little effect on quality, fairness, and reliability of reviews; it makes it possible for authors to

exaggerate their publication record; it enables authors to omit crucial information required for successful replication; and it restricts the development of a constructive relationship between authors and editors. In addition, the administrative costs associated with double-blind review are significantly higher, and it has been argued that it is almost impossible to disguise an author's identity, particularly if the author does not want to remain anonymous (Deaton et al. 1987; Over 1982). Citations are especially revealing. In highly competitive disciplines, in which a handful of researchers dominate a research front, the research methods, writing style, and other hints can almost automatically reveal an author's identity to a veteran referee. Faced with growing instances of fraud, data manipulation, fabrications, and other misconduct, some scholars have also suggested that professional affiliation should be required to prevent these abuses. For example, some argue that information about the authors' institutional affiliation helps referees evaluate manuscripts because they constitute presumptive "proof" that the research described was actually done. However, a prestigious affiliation is not always guarantee against fraud (LaFollette 1983).

Analysis of a survey by the Institute of Mathematical Statistics (IMS) indicates strong support for double-blind refereeing in IMS journals (Cox et al. 1993). Some 46 percent of respondents agreed or strongly agreed with the statement that the IMS should institute double-blind refereeing, whereas only 29 percent disagreed. Studies of reviewers' opinions about the usefulness of double-blind review report varying proportions in favor of this approach: from 39 percent to 89 percent of referees in different areas were in favor of double-blind refereeing (Yankauer 1991). Interestingly, Yankauer (1991) found that of forty-seven referees who were opposed to double-blind review, fifteen justified their opposition by claiming that knowing an author's identity did not influence their judgments, whereas fourteen referees argued the opposite (that knowing an author's background and publishing record contributed to an enhanced judgment, even a wrong one, of a given paper). The above arguments were echoed in a resolution of the Econometrica Society (Deaton et al. 1987).

The notion that an experimented referee can identify the author of a given paper in a specialty journal has been used by many to derogate the claim of an advantage to double-blind review (Biggs 1990; Bradley 1981; Crane 1967; DeBakey 1982; Garfield 1986b; Pfeffer, Leong, and Strehl 1977; Pipke 1984). This opinion seems to be widely shared by referees. In a survey of 131 referees from the 1981 APA directory, 72 percent of participants estimated that authors' identity could be deduced (Ceci and Peters 1984). This figure is close to the one reported by Bradley (1981). The *Canadian Medical Associa-*

*tion Journal* reversed from double-blind refereeing to single-blind in 1990 after concluding that many reviewers could easily guess the identity of the authors (Morgan 1984; Rennie 1992, 1993).

Some analysts have tried to check these perceptions. One common methodology consists of inviting referees to identify the authors of manuscripts from which authors' names are deleted. On occasion, referees are also asked to rate their detective-like skills in the identification process. Using this method, Rosenblatt and Kirk (1980) found that two-thirds of the referees from the *Journal of Social Service Research* were unable to identify the authors or did not attempt to guess. In a broader study, Ceci and Peters (1984) asked referees of six psychology journals to guess authors' identity and to rate their confidence on a 5-point scale. When mechanical detections (due to self-citations, unremoved acknowledgment notes, and so on) were excluded, almost 26 percent of the 146 referees in the experiment were able to identify at least one author of the manuscripts they reviewed. There were no significant differences among journals. In addition, the mean confidence was low, suggesting that many referees' detections were weak suppositions. When Yankauer (1991) asked reviewers from the *American Journal of Public Health* to identify author and institution in the manuscripts they reviewed, he found that although referees estimated that they could identify the authors and/or their institution 47 percent of the time, the identification was correct only 39 percent of the time. The main clues which helped in identification were self-citation (62 percent) and personal knowledge (38 percent). When self-citation cases were excluded, double-blind review was successful 83 percent of the time. Similar research found that the reported success of double-blind review in *Journal of Neuropathology and Experimental Neurology* was 66 percent (when 55 percent of all eligible manuscripts were eliminated because of self-referencing) and 73 percent in *Journal of General Internal Medicine* (when self-referencing was eliminated) (McNutt et al. 1990; Moosy and Moosy 1985).

Other authors have attempted to investigate the effects of double-blind review on the quality of reviews and its probable effects on the quality of manuscripts. For example, McNutt et al. (1990) sent manuscripts for double- and single-blind review to the referees of the *Journal of General Internal Medicine* and found that blinded reviews were of better quality than non-blinded reviews, even though single-blind and double-blind reviews did not differ in their recommendations concerning publication. Similarly, Fisher, Friedman, and Strauss (1994) conducted a randomized controlled trial to study the effects of double-blind review on acceptance of research papers in the *Journal of Developmental and Behavioral Pediatrics* and discovered that 46 percent of all blind reviewers were able to identify the authors. Contrary



to their initial hypothesis, the researchers found that double-blind refereeing favored authors with previous publications, whereas reviewers of nonblind methods made no distinctions. The researchers interpreted this finding as a proof that reviewers using blind review could have recognized the author on the basis of the quality of the manuscripts this author had already published. Laband and Piette (1994a) drew similar conclusions.

Blank (1991) studied the effect of double-blind versus single-blind review in the *American Economic Review* with a randomized experiment and found that 55 percent of referees of double-blind-reviewed papers could not identify the authors. However, there were some differences in identification attributable to the institutional affiliation of authors: only 29 percent of papers by authors from the top-ranked universities were truly "blinded." In 57 percent of the papers by authors from low-ranked universities, the identity of the authors was not discovered. And so it seems that blinding is most effective for lesser known authors. In addition, acceptance rates and referees' ratings were lower when the reviewers were unaware of the authors' identity. Interestingly, and contrary to expectation, the evaluation of manuscripts by authors from top-ranked universities and from low-ranked universities was largely unaffected by the blinding.

## Conclusions

For many years, the peer review system has been a kind of sacred cow in science. Only a few heterodox scholars challenged the usual reviewing practices. Recently, many criticisms have been voiced, and an increasing body of research exists. Nevertheless, we are just starting to know how this quality control mechanism actually works. We know almost nothing about the actual review mechanism, and little about the criteria of appointment of editors and referees or the overall standards used in manuscript evaluation. This conclusion is even more disturbing if we note that there is some evidence showing that editors and referees may sometimes be appointed because of particularistic criteria. Although the common perception is that editors and referees are chosen from among the best qualified researchers and scholars, some results indicate that in some instances they are less productive and less influential than others in their respective fields. On other occasions, the more prestigious authors are not serving as referees because they are too busy to review manuscripts, and so younger, less experienced scientists are more likely to serve as referees. Many editors complain that it is not easy to recruit external referees. In most cases, however, editors and referees tend to be well qualified and experienced as authors. Editors and referees spend a lot of time



working for journals. This hard work is generally unpaid, but the prestige and recognition that they earn within their field seem to be important rewards. It is not rare that editors and reviewers often serve several journals simultaneously.

The training of reviewers as evaluators is an important problem in the current peer review system. For example, only 2 in 76 social and behavioral science editors who were asked whether their reviewers "receive any training, besides a general instruction sheet" answered positively, and they stated that the training was not exhaustive (Crandall 1991, 143). Thus, reviewers are expected to learn on the job at the authors' expense. Similarly, the official opinion of the APA, as published in a recruiting advertisement, is that "the experience of publishing provides a reviewer with the basis for preparing a thorough, objective evaluative review" (Crandall 1991, 143).

As with other evaluators, scientists are subject to cognitive distortions of various sorts. It has been demonstrated that there are biases against replication and against negative results. In some areas, this bias is exacerbated because of the scarcity of space in journals. Evidence demonstrates that only a small percentage of articles replicate previous studies. Knowing that routine publication of replicated results is almost impossible, basically due to the lack of journal space and/or because scientists, referees, and editors prefer novelty, unpublished results or raw data ought to be at least made available to public scrutiny. However, as most scientists know, the current editorial systems avoid this concept. Raw data are rarely available or accessible to prospective future authors, not to mention a total lack of channels through which the free flow and exchange of raw data could take place. Even more, current publishing practices often avoid providing reviewers with statistical information on which to base a review. Usually, only the minimum summary statistics from a research project are included in an article.

The bias against negative results is even more damaging, and it results in an artificial increase of the effect of new treatments due to the suppression of many negative results. The widespread practice of significance tests favor such approaches. Fortunately, there are some changes in the above patterns. Recently, there were some shifts in the APA editorial policy regarding the use of statistical significance tests. The 1994 *Publication Manual of the American Psychological Association* states that "neither of the two types of probability values reflects the importance of magnitude of an effect because both depend on sample size. . . . You are encouraged to provide effect-size information." The journals *Measurement and Evaluation in Counseling and Development* and *Educational and Psychological Measurement* are now including similar guidelines.

Journal peer review is thus an unreliable quality control mechanism. Referees often disagree on the value of a given manuscript. Even when referees agree on the value of a given manuscript, they often disagree on the recommendations to the editors about its publishability. Some low reliability could be an advantage because it could be the result of evaluating a contribution from different viewpoints. Problems occur, however, when editors use such rules as "accept papers if they are favorably reviewed by all three reviewers." With such bureaucratic rules, unorthodox or very innovative views could be rejected because of the low reliability of journal peer review.

Commoner (1978, 25) once compared a rejected manuscript to a venereal disease: "It is a far more widespread phenomenon than one would guess from the frequency of personal accounts. Most victims are too ashamed of the event, or too worried about its effect on their careers to talk about it." However, under the current fever of publication, most rejected papers are successfully "placed" elsewhere. Thus, instead of selecting the best manuscripts for publication, referees are in fact just sending the manuscripts to a more proper place. The fact that so many papers rejected by journals are published elsewhere clashes with the frequent claim that rejection is based on a lack of scientific quality.

In addition, as demonstrated by the research cited above, inappropriate or unjust rejections are common in both the hard sciences and the social and behavioral sciences. Even Nobel-class articles have been rejected outright by referees and editors who did not grasp their potential and innovativeness. We may thus need new channels for communicating unorthodox theories and views, and some new fresh journals are appearing to fill this void, such as the journal *Speculations in Science and Technology*. In economics, for example, journals like *International Journal of Forecasting* and the *Journal of Management and Iconoclastic Papers* seek papers with controversial findings, call for interesting papers, and publish papers that challenge common practices and beliefs.

On the other side of the coin are the many documented instances of flaws in papers published in peer-reviewed journals. Many papers are being published in the biomedical sciences that are contaminated by potentially invalidating methodological flaws. In addition, instances of articles published in reputable journals which contain incorrect theories or flawed results are not uncommon. Of course, to ask reviewers to ascertain accurately which contributions will turn out to be wrong is more than the peer review system can do. Peer review cannot assure error-proof validity.

The opportunities for particularistic criteria which interfere in the publication process are considerable. These opportunities are fostered by the anonymity of referees and by the lack of outlets or complaints. The interfer-

ence of particularistic criteria can lead to the unfair accumulation of advantage by individuals or research groups. The consequences of particularism for the individual scientists and the advancement of disciplines could be especially dramatic in the social sciences, in which paradigms are less well developed and there is a greater paucity of publishing space than in the experimental sciences. Some evidence indicates that an author's status can influence, and often does influence, the referee's decision. However, higher professional and institutional prestige is also associated with higher quality of submitted manuscripts, and there is evidence that some journals publish more articles from the universities to which they are linked. In addition, sometimes editors have to use particularistic criteria to improve the quality of published research, using professional connections to identify good papers or attempting to deal with conflict-of-interest issues through prepublication criteria such as the requirement to disclose investments or affiliations.

Double-blind review has been regarded as a kind of panacea for some of the current problems in scientific publishing. Provided that self-citations and other cues are removed, it seems that blind review of manuscripts can be achieved in most cases. Moreover, referees working under a double-blind review system tend to write better reviews; but, again, we need more research.

We should keep in mind that in scientific publication, the ultimate decision is that of the editor. Good editors can moderate negative reports, reject bad papers, help authors to reformulate their conclusions, and ask for more experiments. In addition, new editorial roles, such as that of the ombudsman, can help editors or even become a kind of editor's guardian.

Peer review will probably be used for many years as the primary mechanism of control and selection in academic journals. However, as new technologies are changing the communication patterns of scientists in some hotly disputed areas of research, such as particle physics, researchers are leaning heavily on electronic mail to report their results. Few scholars working in these fields wait to read papers in the usual printed format but instead resort to network scanning.

I have tried to address some of the issues of peer review about which scientists frequently voice concern. However, I will finish with one question for which I have no response: Could science survive if the peer review system were suppressed?

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