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Research Ethics, Manuscript Review, and Journal Quality

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Chapter 5

How Journal Editors came to Develop and Critique Peer Review Procedures

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ABSTRACT

Editorial peer-review procedures did not develop to detect fraud or even, originally, to establish the standards and authority of science. Peer reviewing evolved from the need of editors to choose among a surplus of submitted manuscripts and the growing inability of an editor to possess enough expertise to judge quality in all specialized fields that a journal might cover. Referring papers out began as early as the eighteenth century in some forms, but the practice was quite unusual until the twentieth century. Each journal came to the practice in a unique way, and occasional bits of evidence show how journals in the agronomy and agriculture fields are excellent examples of the variety of practices that developed among all scientific journals so that refereeing of some kind was commonplace by the mid-twentieth century. In the 1970s and 1980s, following some sociological investigation of editorial practices, journal editors began to question and critique peer reviewing. By the mid-1980s, general public and legislative concern over grant peer reviewing had intensified concern about wholly independent and various refereeing practices that were grouped together as editorial peer review.

Peer-review processes are essential in scientific publication today (standard works include Ziman, 1966; Lock, 1985; Chubin and Hackett, 1990, Chapter 4 and passim). Yet we know little about how peer reviewing developed; indeed, the reason that I have been asked to report on the historical background of present procedures is that I happen to have addressed the subject recently and discovered that we know only the most superficial facts about the origins of editorial peer reviewing (Burnham, 1990; Chubin and Hackett, 1990, provide a contemporary survey but do not contribute to knowledge about the history). What I am reporting to you today is therefore a summary of what I and a few others have uncovered to date.

Many of my findings are not those one might have expected. I was astonished to learn that, despite the fact that peer-review procedures govern the allocation of most scientific research grants as well as the publication of journal articles, no one has yet written a history of the peer review of grant applications, either (Burnham et al., 1987; MacLeod, 1971; Strickland, 1989). Moreover, it turns out that peer review of publications developed independently of peer review of grants so that the development of one kind of peer review had little to do with the development of the other. Finally, I found that, at least in the case of biomedical journals, until after World War II there was no movement as such to introduce peer review, that is, a movement in which there were many instances in which one journal adopted practices because another journal utilized them. Rather, each journal adopted such practices piecemeal, in response to conditions specific to that journal (Burnham, 1990; I have drawn on this publication freely).

The history that I can thus summarize for you therefore assists substantially in making sense of the current status and vicissitudes of peer-review processes. First, the great diversity in current peer-review practices as they work out in the real world of journal publishing can be explained in large part because they did tend to appear independently in each journal in response to idiosyncratic conditions. Since there is no historical basis for uniformity, none need be expected. Second, peer review did not come into journal publishing in order to combat fraud and misconduct. Peer review arose for other reasons in other circumstances, and when questions of misconduct recently started coming under public discussion, the editorial peer-review procedures in place were relevant to issues of misconduct only accidentally. In short, the institution simply was not designed many years ago to meet problems of the late twentieth century. The editors who over several generations adopted the custom of referring papers had in mind instead their own times and their own predicaments.

THE HISTORY OF SCIENTIFIC JOURNALS

Most of what we know about the history of scientific journals comes from material published in the journals themselves, and that means that we do not have much material. Until quite recently, most journals have not carried detailed descriptions of their editorial procedures. Moreover, for the most part, hardly any substantial records of journals per se exist in archival form—at least for the period before World War II. Instead, historians have had to piece events together from materials that show up only incidentally in various personal papers of prominent scientists or, occasionally, publishers. The result has been that the entire field of the history of scientific and medical journalism is not well developed, and it is for this reason, in part, that knowledge about the history of peer review processes is so scant.

Scientific journals first developed in the seventeenth century to systematize the letters and circular letters through which intellectuals interested in science had begun communicating their discoveries to each other. In France, in January 1665, *Le Journal des sçavans* appeared, edited by Denis de Sallo de la Coudraye and issued by a printer. In England, in May of the same year, the *Philosophical Transactions* of the Royal Society appeared. At first, the *Philosophical Transactions* was issued as a private venture of the secretary, Henry Oldenburg, but within a few decades, publication became more officially connected with the Royal Society. The *Philosophical Transactions* was in fact an attempt to deal with the enormous volume of correspondence that the Royal Society had engendered. In the decades that followed, large numbers of other journals appeared, and by the nineteenth century, the explosion in scientific communication with which we are all familiar was well under way (see the summary in Menten, 1980; Kronick, 1976; Houghton, 1975; Hall, 1965).

By the nineteenth century, journals came, in general, in three different models, each one of which had a separate relation to the development of peer reviewing. The first model was the proceedings or transactions of a scientific society. Often these proceedings consisted of papers presented at meetings or simply papers communicated by members. In many cases, the members believed that as members they had the *right* of publication there; in other cases, mechanisms developed to censor out inferior contributions, and these censoring mechanisms constituted one of the ancestors of peer reviewing. The second model was the one most common in continental Europe: the journal was the organ of a research institute in which the head of the institute published the results of research carried out in the institute, along with, occasionally, other compatible material. In the institute organ model, the director was the judge of what should or should not be issued, and as the expert in the field, he or she did not require any advice. The third model was the independent journal, and again the editor made all of the decisions as to what should or should not be published. The real process of peer review can therefore be traced mostly in the way in which editors of independent journals came to adopt procedures of peer review.

THE ORIGINS OF PEER REVIEW

D.A. Kronick (1978, 1990) traces the idea of peer review as far back as the eighteenth century. Peer-review procedures developed spontaneously in connection with several publications at that time, including the Royal Academy of Medicine in France and the Royal Society of Edinburgh, and Kronick quotes the preface to a publication of the Edinburgh group from 1731: "Memoirs sent by correspondence are distributed according to the subject matter to those members who are most versed in these matters. The report of their identity is not known to the author."

The most notable precursor of later peer reviewing appeared in 1752, when the Royal Society of London took over responsibility for publishing the *Philosophical Transactions* and set up a "Committee on Papers" to referee all papers to be published. The committee was further authorized to refer papers to "any other member of the Society who are knowing and well skilled in that particular branch of Science that shall happen to be the subject matter of any paper which shall be then to come under their deliberations," (quoted in Kronick, 1990).

Still another precursor of peer reviewing was the widespread custom of awarding prizes. Throughout Europe, particularly in the eighteenth century, learned academies and scientific societies offered prizes for essays that disclosed some new insight on a particular topic, and these essays were judged by committees of experts. Whether recognized by the French Academy of Surgeons or a small scientific society in a German provincial capital, the essay, it was assumed, would be published and honored as authoritative because the expert judges had chosen it (see especially Fischer, 1937; McClellan, 1985, passim; Crosland and Gálvez, 1989).

In the nineteenth century, however, these antecedent peer review customs spread only rarely to the editorial practices of independent journals. The reason was that another influence arose, namely, general journalism, which set the dominant model for scientific and medical publications. In that model, the journal became the personal organ of a powerful editor. So in the field of medicine, the great exemplar was the combative and colorful Thomas Wakley of the *Lancet*. A number of scientific journals even came to be known by the names of editors rather than by the proper names of the journals—*Nicholson's Journal* or *Silliman's Journal* or *Virchows Archiv* (the latter officially changed after the editor's death; see, for example, Lilley, 1948–1950). In these cases, the editor, and the editor alone, decided what was to be included in the journal.

Now it is entirely possible that the editor of a journal was indeed, like the director of the institute that published an organ, an expert, so that any paper he (or, rarely, she) judged was indeed thereby passed on by a "peer." Throughout the nineteenth century, specialty journals in different fields sprang up because members of a small group with a particular interest wanted to communicate with each other, and the narrowness of the subject made it possible for a single editor to be current, at least for a time, with all research in that field, as, for example, in early mineralogical journals and in a number of medical specialty publications (see especially Peterson, 1979). Again, an expert—the editor—was judging the papers. When in the 1870s astronomer Norman Lockyer, the editor of *Nature*, was unable to deal satisfactorily with botanical subject matter, his solution was not to set up peer reviewing but to appoint a subeditor with requisite expertise (Meadows, 1972, p. 31). And as late as the World War I period, to cite another example, the editorial decisions of the *Annalen der Physik* were made entirely by Max Planck and a junior colleague—although an appeal to a board of overseers from the Berlin Physical Society was possible (Pyenson, 1983).

AUTHOR RESPONSIBILITY

The great reform in nineteenth century scientific publishing was not introducing peer reviewing, however, but establishing responsibility of individual authors for their statements in the journals and phasing out anonymous contributions. This move was both positive—to establish priority of discovery—and negative, in that readers could use the identity of the writer to judge how authoritative a statement was (cf. Kronick, 1978). As WJ [sic] McGee, the American geologist, wrote in 1892, "Printed statements are worthless to the student unless vouched for by some individual in person; a thousand anonymous declarations that a meteor has fallen are disproved to the student by the simple utterance of a single reputable individual who backs his utterance with his name; and even the lay reader is coming to regard the author's name as the sign manual of veracity."

The individual editor and the individual author of a scientific article therefore constituted the basis for credibility and authority in the nineteenth century and, it must be added, in many areas until the mid-twentieth century. Only slowly did a change in the nature and functioning of editing—the coming of peer review—begin, beyond the eighteenth-century precursors whom I have noted.

THE EVOLUTION OF PEER REVIEW

I emphasize that peer review came not only over a long period of time but only here and there. So gradual was it in developing that the first use of the term "referee" so far discovered goes back only to 1902, when in a footnote to an article in the *Philosophical Transactions* there was the notation, "Corrected as pointed out by the referee," (Katzen, 1980, p. 206; *The Oxford English Dictionary* traces "peer review" back only to 1971).

The pressure for this change from the individual editor's exclusive responsibility to some kind of peer reviewing derived from two circumstances: the increase in the number of papers submitted, so that many editors had to make choices; and the growth of specialization.

Before the early twentieth century, the problem for most editors (as their correspondence and editorial pleas reveal) was finding enough material barely to fill their pages, not choosing among a surplus of good papers. When the editor could not find enough publishable material, such a procedure as peer review was not even a possibility. Indeed, the custom arose of appointing editorial boards (under various names), not to judge the quality of submissions but to round up material that the hapless editor could print. Often there were cooperating editors or board members appointed from various geographical locations for the specific purpose of soliciting colleagues in the area for publishable items.

When, however, scientific activity in one field or another grew to the point that the editor of a prestigious journal found him or herself having to turn down large numbers of manuscripts, he or she needed help. The help came from others competent in the field, and it could come in the form of sending papers out to colleagues, or it could come in the form of an editorial board set up to help judge papers. But, again, the circumstances in each case differed, and each editor had to decide how much outside opinion he or she wanted, or could tolerate. I found one editor (quoted in Burnham, 1990), for example, who had tried both a set board of experts and open refereeing, that is, sending papers out to known experts, and he concluded that using a set editorial board for expert reviewing of manuscripts was "an awkward system, since no such committee can be depended upon for continuous service, and when most needed is likely to be unavailable." He therefore preferred dealing directly with outside referees.

From the beginning, an editor could and would send an occasional paper to an outside referee in cases in which the editor did not feel that he or she had the competence to judge the specialized material in the paper. In one instance, in 1831 the *Philosophical Transactions* referred a paper of Davy to Faraday, who was not a member of the Committee on Papers (Kronick, 1989, unpublished data). In another well-known instance, in 1871 James Dwight Dana, the editor of the *American Journal of Science*, referred a paper of young Henry A. Rowland to two physicists for opinions, since Dana lacked the expertise to judge the paper. On the basis of the referees' reports, Dana turned the paper down, only to be embarrassed by seeing it picked up by James Clerk Maxwell and published in the prestigious *Philosophical Magazine* (Reingold, 1964, p. 262–266).

The development of specialization in science involved the concomitant idea of the rise of expertise. Sometimes the expertise came in areas of investigation, but often it came implicitly or explicitly in areas of method or instrumentation (such as the entire field of microbiology) that excluded other scientists who had not used a particular technique or set of instruments. In any event, with the professionalization and specialization of scientists, single editors frequently found themselves dealing with papers about which they knew little. If they were in the happy position of being able to refuse papers, they could, then, turn either to outside experts of their own choosing or to an editorial board or some combination of the two. That was the typical way in which peer reviewing came.

What is perhaps most remarkable, however, is the way in which peer reviewing did *not* come. Individual editors and journals held out for decades, for a variety of reasons. I found, for example, that the editor of the *British Medical Journal*, Ernest Hart, in 1893 described for a gathering of the American Medical Editors' Association the way in which use of outside expertise in editorial decisions had made the *British Medical Journal* preeminent in the world—but not a single member of that editors' group, as far as I can find out, instituted any peer review system (Burnham, 1990). In some cases, editors held out because they could not admit that they were not experts. In other cases, they continued to need almost all material submitted to them. And the idea of the responsible, single editor died hard. As late as the 1930s, *Science* magazine was not based on regular refereeing (in 1938, for example, the editor, James McKeen Cattell, did send one paper out—but only as far as his son, McKeen Cattell, to whom he referred in a letter as "our referee in physiology," showing, incidentally, that he was well aware of the institution of refereeing papers; Burnham, 1990, p. 1327, courtesy of Michael Sokal).

EXAMPLES OF DIVERSITY FROM AGRICULTURAL SCIENCE

I do not wish to suggest that the record is entirely clear even as to the wide variations in practice, and perhaps the example of a few journals in the field of agronomy and in agricultural science in general can give some sense of what happened. The entire field of agriculture differs from that of many other parts of science in that a proportion of those active in the field have been applied scientists and, like some medical practitioners, either came to peer review late or not at all (an excellent summary is Lacy and Busch, 1982). In agriculture, too, the model was originally largely German, so that publications frequently were the organs of institutions and so not immediately likely candidates for full-fledged peer-review processes. Yet—again, over a period of time—peer reviewing nevertheless did come to many journals in the general field of agriculture, especially—and earlier—those close to pure scientific areas such as biochemistry in which editors more customarily referred papers out.

As in other fields of science, until historians find some good archival material (an outstanding example is Heichel, 1992), the record will remain murky. In 1916, for example, Jacob G. Lipman, the founding editor of *Soil Science*, publicly thanked his "consulting editors" for "their moral support," which suggests but—in the absence of further evidence—does not verify that he used his editorial board as just good-will sponsors rather than referees.

Moreover, every kind of practice continued to coexist with every other, right down to the present, although there is evidence that toward the last part of the twentieth century, peer-review procedures were becoming standard for journals that were oriented toward research (in 1961, of a sample of 156 scientific journals from all over the world, 71% used peer review—but only 21 out of 30 French, as opposed to 47 out of 49 American; Zuckerman and Merton, 1971, p. 75). At midcentury, a time when, for example, peer reviewing was becoming general among medical journals, there was evidence that the custom was already being established also here and there in the agricultural sciences. The *Journal of Agricultural and Food Chemistry*, which was published by the American Chemical Society, for example, from the first issue in 1953 noted that each paper would be "received for review," which presumably meant that refereeing procedures were in place. By 1980, when *Soil & Tillage Research* was founded, the editors announced from the beginning that "All articles will be critically refereed" and that papers "only of the highest level" would be accepted. *Reclamation and Revegetation Research*, founded in 1982, carried a similar announcement, that "Each manuscript will be reviewed by two or more reviewers."

It seems clear, however, that in many cases in agricultural science, the tradition of a strong editor persisted. When the *Journal of Dairy Research* was founded in 1929, for example, although there was an editorial board, the emphasis in the foreword was on the qualifications of the editor and his ability to judge matters in the field (Passfield, 1929).

But in agricultural research there was also the other complicating tradition that I have noted: publishing in an institutional organ or society proceedings. The best example of an institutional organ is perhaps the *Journal of Agricultural Research*, which started out in 1913 listing an editor. A year later it had an "Editorial Committee" of three, all from the Department of Agriculture, and then, in 1915, a committee of six, half from the Department and half from the Association of American Agricultural Colleges and Experiment Stations. In short order, then, this publication shifted from the institution executive to a board that embodied institutional authority and then expanded it. In agricultural research in general, the use of an editorial board or committee was in fact very general—but, again, it was not clear how much the board or committee embodied expertise that would pass for a peer-review panel, nor was it clear the extent to which the editor referred papers to the board. In any field, moreover, an investigator serving on the board might well be able to judge submissions from a broad or only a narrow range of subjects, depending upon the extent of specialization of the journal.

In many cases, the suggestion that the board constituted a peer-review panel was very strong—still assuming that the editor used the board in such a way. The *Journal of Agricultural Science*, for example, started out in 1905 with four editors; three-quarters of a century or more later, the only apparent change was that there were 10 more editors, an expansion that certainly suggested that peer reviewing was still contained within the group.

In the case of society proceedings, again the experience of agricultural scientists was typical of that of other groups. One of the purposes of the proceedings format was, as I have suggested, to permit every member who wished it the right to be published, emphasizing the rights of the author and not the viewpoint of the potential reader or the public. In 1909, for example, the American Society of Agronomy resolved that the Committee on Publication "be instructed to issue as large an edition as possible" of the *Proceedings*, although an Editing Committee was appointed at the same time. Similarly, the avowed object of the *Journal of Farm Economics* when it was founded in 1919 was to publish as many papers as possible from the meetings of the association.

Many associations in agriculture nevertheless moved to impose qualitative tests for papers that would be published in proceedings or transactions. In 1941, for example, the American Society for Horticultural Science added to the bylaws a provision that the Editorial Committee "shall have final authority to reject any paper deemed not worthy or unsuitable for publication in the Proceedings."

The issue in such provisos, over the years, was a new one—authority. It was not enough to say, as did many associations and editors, that the group or the journal was not responsible for opinions of contributors; in the nineteenth-century tradition, published papers would have carried the authority of the author's name. The growth of peer reviewing came to mean that publication in a prestigious journal gave a paper the imprimatur of the scientific community (Ziman, 1966). The increasing use of censors in the publication of proceedings, and of editorial boards that were active in judging papers, both involved acts of group validation as well as invoking the more abstract idea of expertise.

Moreover, the idea grew slowly that peer reviewing actually affected science itself because referees' comments returned to authors caused them to revise the research they were attempting to report, for example by forcing them to include control experiments. Such further integration of peer reviewing into science caused investigators of the late twentieth century to take the process for granted (see, for example, Wilson, 1978).

THE FIRST QUESTIONING OF PEER REVIEW

In these ways, the agricultural and agronomy journals reflected changes in science and also in society in general, particularly in the twentieth century—even though the reflections came in an accidental and piecemeal fashion, as in medicine and science in general. Moreover, every party involved in peer reviewing got caught up with another change in science and society, namely, the growing conviction of most people, both inside and outside of science, that the world of science was a meritocracy in which the most deserving emerged at the top of a hierarchy—a hierarchy in which scientific publication and peer review played a very large part (see, for example, Ziman, 1966).

Inevitably, some commentators came to doubt that this hierarchy actually reflected merit or even expertise, and eventually attention began to focus on the peer review system. Scientists were slow to pay attention to criticism, however, because everyone had to overlook complaints from disgruntled authors whose papers had been rejected by one journal or another. Therefore when a new group of researchers, the sociologists of science, began in the late 1960s and 1970s to examine peer review as an institution of science, their work did not generate much interest. The first major publication on the refereeing process itself, by sociologists H. Zuckerman and R.K. Merton in 1971, found, to the authors' evident surprise, that peer reviewing in the *Physical Review*, at least, worked well, on the whole, eliminating much of the bias in consideration that might have been expected. "The referee system . . . provides an institutional basis for the comparative reliability and cumulation of knowledge," they concluded.

At that time (1971) Zuckerman and Merton were able to cite only two previous empirical studies, but soon there were others. Meanwhile, a most significant additional group began scrutinizing peer review and occasionally testing the process: journal editors, and it was they who began to raise questions most effectively. Among the first and certainly the most important was F.J. Ingelfinger, the legendary editor of the *New England Journal of Medicine*. In 1974, although not sharply critical, for a survey of his own practices showed relatively reassuring referee agreement, he nevertheless expressed some skepticism about both the detailed mechanisms of refereeing and the net value of the process, which took so much editorial and referee time and effort.

For almost a decade, from the mid-1970s to the mid-1980s, additional discussion and research defended and attacked the idea and particularly the mechanisms of peer reviewing. Among the authors were not only editors and sociologists but, now, disgruntled authors (who finally were speaking out publicly) and antiestablishment writers (for example, Barry Commoner, 1978, who wrote, "The peer review system appears to be not some minor fault in the housekeeping of science, but a threat to its basic purpose"). In 1982, Steven Harnad, editor of *Behavioral and Brain Sciences*, published a controversial study by D.P. Peters and S.J. Ceci (1982), with a comprehensive bibliography of opinion and experiment to that time. Peters and Ceci found that editors rejected eight out of nine articles that had appeared earlier in their journals when the articles were resubmitted in a disguised form as coming from nonprestigious authors and institutions. This indication of the unreliability of refereeing (and in addition only 3 out of 38 editors and referees detected the deceit) set the stage for a thorough discussion of peer reviewing by 54 knowledgeable editors and researchers (Harnad, 1982a; a summary of empirical studies is Patterson and Bailar, 1985).

The most striking finding of the various researchers and commentators from the 1970s and the beginning of the 1980s (as summarized in Harnad, 1982a) was the range of practices that passed for peer review. Further, there were numerous specific suggestions not only to dispense with peer review but to improve it in its details. The most intense debate was over the question of whether the process should be anonymous, or whether the referee should sign his or her name. Closely allied was the question of whether or not the referee should know the identity of the author. And some editors raised the question of how many referees were necessary to get a valid result.

All of this discussion proceeded against the background of additional experimental or empirical studies that provided conflicting evidence about the reliability—consistency of results—of the peer-review process (for example, compare research such as that of Peters and Ceci with that of Yankauer, 1979; and Dixon et al., 1983). Perhaps the inconsistencies should not have been surprising, for one fact that investigators had established was that standards and circumstances varied greatly from journal to journal. Particularly striking was the fact that very few papers were turned down by hard science journals but a large percentage were rejected by biological and especially social science journal editors, showing that each discipline had a typical profile of acceptance percentages, which changed the significance of peer-reviewing procedures from one field to another (see Hargens, 1988 and 1990, which summarize the literature).

NEW CONCERNS ABOUT PEER REVIEW

What was not strongly in evidence in the discussion of peer reviewing in the 1970s and early 1980s was great concern over the ability of refereeing to prevent fraud, deception, and other misconduct. A British biochemist, R. Jones, was one who noted incidentally in 1974 that there was a certain amount of "malpractice" in the peer-review system, but he had in mind just correcting abuses such as a referee's delaying publication of the work of a rival, and even then Jones's mention of abuse was incidental. Similarly, in 1982 when J.S. Armstrong (p. 89) mentioned the problem of fraud, it was an incidental concern that he subordinated to the general category of hoax, clearly at that time a more immediate danger for editors, who occasionally had to deal with investigators who were high-spirited and perhaps irreverent practical jokers.

Nevertheless, two developments began to complicate the efforts of editors, authors, and sociologists to discuss peer reviewing. One was the concern already voiced in the late 1970s by legislators and scientists alike over *grant* peer reviewing, concern that was based on continual complaints about "old boy" networks and on recurring awareness of geographical injustices in awards (see, for example, Cole et al., 1980; and Cole and Cole, 1981). The second complicating development was a series of press reports on dishonesty among researchers, including investigators at major institutions, institutions that, some researchers suggested, were favored in the peer-review process (see, in general, Chubin and Hackett, 1990).

By the middle 1980s, as was made clear in a new symposium on peer review in the Summer, 1985, issue of *Science, Technology, & Human Values*, the general public and government policymakers were becoming deeply concerned with all kinds of peer reviewing—including, ultimately, editorial refereeing procedures. These concerns intensified as press reports of possible fraud in scientific research escalated. As a consequence, then, to the previous concerns about fairness and mechanisms, there was added the explicit responsibility of editors to *police* science. "Journals such as this," wrote John Maddox in *Nature* in 1989, "are now often presented with the uncomfortable need to decide whether loose use of language, or inadequate experimental data, are consequences of authors' haste or of some more sinister concealment of the whole truth. This does not happen often, but that it should happen at all is a serious matter, requiring the cultivation, by referees and all others concerned, of an over-suspicious frame of mind."

Serial indexes such as *Index Medicus* and *Reader's Guide* show that publications on the general subject of peer review suddenly increased very greatly in professional scientific and medical journals in the mid-1980s, and the continuing volume of publication made it clear that not just editors were commenting. Indeed, the comments tended more frequently than earlier to be opinion, with only a few additional empirical studies appearing in the listings. Still another symposium, and this one specifically on peer reviewing in journals, took place in 1989 and was largely published in *JAMA* (*Journal of the American Medical Association*), 9 Mar. 1990. It provides an appropriate ending to my narrative. Many of the papers still belonged to the category of observation and opinion, and such research as was reported there was still mostly on the level of one specific example or another of the way in which one kind of peer review or another did or did not work (usually examples of practices and outcomes at a particular journal, such as the *British Medical Journal*—Gardner, 1990). As the symposium convener (Rennie, 1990) observed afterward, at least the symposium showed how much more research was still needed. Moreover, the participants were continuing to explore, albeit with more sophistication, the very same sets of problems that writers of the previous quarter of a century had identified: (i) incidence of bias, (ii) standards and guidelines for peer reviewing, (iii) reliability of results, and (iv) the role of referees in enforcing standards of science.

CONCLUSION

In that symposium and in myriad other discussions of peer review, statespersons of science tended to speak as if the institution of refereeing papers had originated in some pure form and had somehow become corrupted. Evidence of the past of peer reviewing, however, suggests a very different history. The irregular origins of peer-review procedures over many decades explain why an institution that grew in specific circumstances under local conditions, under the guidance of however well-intentioned multitudes of scientists from all different disciplines, should present such a variety of faces and problems.

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