HARRIET ZUCKERMAN AND
ROBERT K. MERTON

Patterns of Evaluation in Science: Institutionalisation, Structure and Functions of the Referee System

THE referee system in science involves the systematic use of judges to assess the acceptability of manuscripts submitted for publication. The referee is thus an example of status-judges who are charged with evaluating the quality of role-performance in a social system. They are found in every institutional sphere. Other kinds of status-judges include teachers assessing the quality of work by students (and, as a recent institutional change, students officially assessing the quality of performance by teachers), critics in the arts, supervisors in industry and coaches and managers in sports. Status-judges are integral to any system of social control through their evaluation of role-performance and their allocation of rewards for that performance. They influence the motivation to maintain or to raise standards of performance.

In the case of scientific and scholarly journals, the significant status-judges are the editors and referees. Like the official readers of manuscripts of books submitted to publishers, or the presumed experts who appraise proposals for research grants, the referees ordinarily make their judgements confidentially, these being available only to the editor and usually to the author. Other judges in science and learning make their judgements public, as in the case of published book reviews and the often important review articles which assess the " credibility " of recent work in a special field of knowledge.

Although the referee system has its inefficiencies, practising scientists see it even in its current form as crucial for the effective development of science. Professor J. M. Ziman puts the case emphatically:

> The fact is that the publication of scientific papers is by no means unconstrained. An article in a reputable journal does not merely represent the opinions of its author; it bears the *imprimatur* of scientific authenticity, as given to it by the editor and the referees he may have consulted. The referee is the lynchpin about which the whole business of Science is pivoted.[1]

The chemist, Professor Leonard K. Nash, describes the " editors and referees of scientific journals " as " the main defenders of scientific ' good

[1] Ziman, J. M., *Public Knowledge: The Social Dimension of Science* (Cambridge University Press, 1966), p. 148.

taste ' ".[2] Professor Michael Polanyi suggests that although there are of course many cases of disparate evaluative judgements about particular works in science, the structure of scientific authority has generally operated through the years so as to exhibit a remarkable degree of concurrence. He states, for example:

> Two scientists acting unknown to each other as referees for the publication of one paper usually agree about its approximate value. Two referees reporting independently on an application for a higher degree rarely diverge greatly.[3]

Observations of this sort attest to the great significance scientists ascribe to the referee system. Yet until recently, the referee system itself has not been systematically examined and assessed. Professor Gordon Tullock, an economist, has remarked that " Given the importance of these editorial decisions for science, the absence of research into them is surprising." [4]

In this paper, we undertake an inquiry into four aspects of the referee system. We deal first with the faint beginnings in the latter seventeenth century of the institutionalisation of evaluative judgements into a system of roles and procedures. We then examine and explore the implications of patterns of differences in the rates of rejecting manuscripts submitted to contemporary journals in fifteen fields of science and learning. In the greater part of the paper, we draw upon fairly recent archives of *The Physical Review* (which the editors kindly made available to us for the purpose) to identify and analyse patterns of decision by editors and referees. Finally, on the basis of these historical, comparative and quantitative analyses, we consider the significance of the referee system for individual scientists, scientific communication and the development of science.

[2] Nash, Leonard K., *The Nature of the Natural Sciences* (Boston: Little Brown, 1963), p. 305.

[3] Polanyi, Michael, *Science, Faith and Society* (Oxford University Press, 1946), p. 37. The evidence on the extent of agreement by referees has only begun to be assembled, but indications are that it varies appreciably among different fields of science and learning. We have found, for example, that in a sample of 172 papers evaluated by two referees for *The Physical Review* (in the period 1948–56), agreement was very high. In only five cases did the referees fully disagree, with one recommending acceptance and the other, rejection. For the rest, the recommended decision was the same, with two-thirds of these involving minor differences in the character of proposed revisions. In two biomedical journals, however, Orr and Kassab found that for 1,572 papers submitted over a five-year period and reviewed by at least two referees, " they agreed that a paper was either acceptable or unacceptable 75 per cent. of the time " (as compared with the 62 per cent. that could have occurred by chance). Orr, Richard H., and Kassab, Jane, " Peer group judgments on scientific merit: editorial refereeing ", presented to the Congress of the International Federation for Documentation, Washington, D.C., October 15, 1965. For one journal of sociology, agreement to accept or to reject occurred in 72·5 per cent. of 193 pairs of independent editorial judgments (as compared with the 53·9 per cent. that would have occurred by chance). Smigel, Erwin O., and Ross, H. Laurence, " Factors in the editorial decision ", *The American Sociologist*, V (February 1970), pp. 19–21. Systematic comparisons of variability in the extent of agreement in referee judgements would identify differences in the extent of institutionalisation of different fields of science and learning.

[4] Tullock, Gordon, *The Organization of Inquiry* (Durham: Duke University Press, 1966), p. 148.

Institutionalisation of the Referee System

The referee system did not appear all at once as an integral part of the social institution of science. It evolved in response to the concrete problems encountered in working toward the developing goals of scientific inquiry and as a by-product of the emerging social organisation of scientists.

The new scientific societies and academies of the seventeenth century were crucial for the social invention of the scientific journal [5] which began to take an enlarged place in the system of written scientific interchange which had hitherto been limited to letters, tracts, and books. These organisations provided the structure of authority which transformed the mere *printing* of scientific work into its *publication.* From the earlier practice of merely putting manuscripts into print, without competent evaluation of their content by anyone except the author himself, there slowly developed the practice of having the substance of manuscripts legitimated, principally before publication although sometimes after, through evaluation by institutionally assigned and ostensibly competent reviewers. We see the slight beginnings of this in the first two scientific journals established just 300 years ago within two months of each other: the *Journal des Sçavans* in January 1665; the *Philosophical Transactions* of the Royal Society, in March of the same year. The *Journal* was a conglomerate periodical which catalogued books, published necrologies of famous persons, and cited major decisions of civil and religious courts as well as disseminating reports of experiments and observations in physics, chemistry, anatomy and meteorology. The *Philosophical Transactions* was "a more truly scientific periodical . . . , excluding legal and theological matters, but including especially the accounts of experiments conducted before the [Royal] Society." [6]

Although not the official publication of the Royal Society until 1753, the *Transactions* was first authorised by its council on 1 March, 1664–65 in these sociologically instructive words:

Ordered, that the *Philosophical Transactions,* to be composed by Mr. [Henry] Oldenburg [one of the two Secretaries of the Society], be printed the first Monday of every month, if he have sufficient matter for it; and that the tract

[5] First privately printed in 1913, the classic and still useful monograph by Martha Ornstein deals with the subject in chapter VII: *The Role of Scientific Societies in the Seventeenth Century* (Chicago: University of Chicago Press, 1938), (3rd edition), see also Brown, Harcourt, *Scientific Organisations in Seventeenth Century France* (Baltimore: Williams and Wilkins, 1934).

[6] Porter, J. R., "The Scientific Journal—300th Anniversary", *Bacteriological Reviews,* XXVIII (September, 1964), pp. 211–230 at 221. In this short account of the institutionalisation of the referee system, we have drawn upon Barnes, S. B., "The Scientific Journal, 1665–1730", *Scientific Monthly,* XXXVIII (1934), pp. 257–260; Garrison, F. H., "The Medical and Scientific Periodicals of the 17th and 18th Centuries", *Bulletin, Institute for the History of Medicine,* II (1934), pp. 285–343 ; McKie, D., "The Scientific Periodical from 1665 to 1798, *Philosophical Magazine* (1948), pp. 122–132 ; Kronick, D. A., *A History of Scientific and Technical Periodicals* (New York: The Scarecrow Press, 1962).

be licensed under the charter by the Council of the Society, being first reviewed by some of the members of the same. . . .[7]

Much relevant information is packed into this summary of an organisational decision. Prime responsibility for the new kind of periodical is assigned to one person, Oldenburg, for whom there does not yet exist the designation of editor, to say nothing of specifying his obligations in the editorial role. Before long, in trying to meet the problems of maintaining the journal, Oldenburg, together with concerned colleagues in the Society, introduced various adaptive expedients which ended up by defining the role of an editor. The council also recognised the immediate problem of having " sufficient matter " for this newly-conceived periodical and institutional devices were gradually evolved to induce scientists to contribute to the journal. What is perhaps most significant here is that the council, as sponsor of the *Transactions*, was involved with its fate and wanted to have a measure of control over its contents. These adaptive decisions provided a basis for the referee system.

As with the analysis of any case of institutionalisation, we must consider how arrangements for achieving the prime goals—the improvement and diffusion of scientific knowledge—operated to induce or to reinforce motivations for contributing to the goals and to enlist those motivations for the performance of newly-developing social roles. As we have noted, the first problem was to get enough work of merit for publication. In part, this was a problem because of the comparatively small number of men seriously at work in science. But it also resulted from the circumstance that, intent upon safeguarding their intellectual property, many men of science still set a premium upon secrecy (as is evident in their correspondence with close associates). They maintained an attitude and continued a practice of (at least, temporary) secrecy which, as Elizabeth Eisenstein has impressively suggested, was more appropriate to a scribal culture.[8] With the advent of printing, however, findings could be permanently secured, errors in the transmission of precise knowledge greatly reduced and intellectual property rights registered in print. Printing thus provided a technological basis for the emergence of that component of the ethos of science which has been described as " communism ": the norm which prescribes the open communication of findings to other scientists and correlatively proscribing secrecy.[9] But it appears that this norm

[7] Weld, Charles R., *A History of the Royal Society*, Volume I (London, 1848), p. 177.

[8] Eisenstein, Elizabeth L., " The Advent of Printing and the Problem of the Renaissance ", *Past & Present*, Number 45 (November, 1969), pp. 19–89; esp. pp. 55, 63 and 75–76. " Many forms of knowledge had to be esoteric during the age of scribes if they were to survive at all. . . . Advanced techniques could not be passed on without being guarded against contamination and hedged in by secrecy. To be preserved intact, techniques had to be entrusted to a select group of initiates who were instructed not only in special skills but also in the ' mysteries ' associated with them."

[9] For an analysis of " communism ", universalism, organised scepticism and disinterestedness as basic institutional norms of science, see Merton, R. K., *Social Theory and Social Structure* (New York: Free Press, 1968) (enlarged edition), pp. 604–615. For extended analyses of this normative structure, see Barber, Bernard, *Science and the Social Order*

did not fully develop in response to the new technology of printing; ancillary institutional inventions served to facilitate the shift from motivated secrecy to motivated public disclosure.

Before the *Transactions* were inaugurated, the Royal Society had adopted one such institutional device to encourage men of science to disclose their new work. The Society would officially establish priority of discovery by recording the date on which communications were first received. As Oldenburg put it in reassuring terms to his friend and patron, Robert Boyle: "The Society alwayes intended, and, I think, hath practised hitherto, what you recommend concerning ye registring of ye time, when any Observation or Expt is first mentioned." And, he adds, making the function of this practice altogether manifest, the Royal Society

> have declared it again, yt it should be punctually observed: in regard of wch Monsr. de Zulichem [Huyghens] hath been written to, *to communicate freely to ye Society,* what new discoveries he maketh, or wt new Expts he tryeth, the Soceity being very carefull of registring as well the person and time of any new matter, imparted to ym, as the matter itselfe; *whereby the honor of ye invention will be inviolably preserved to all posterity.*[10]

Soon afterward, Oldenburg writes to Boyle again and even more emphatically reiterates the function of this institutional practice:

> This justice and generosity of our Society is exceedingly commendable, and doth rejoyce me, as often as I think on't, chiefly upon this account, yt I thence persuade myselfe, yt all Ingenious men will be thereby incouraged to impart their knowledge and discoveryes, as farre as they may, not doubting of ye Observance of ye Old Law, of Suum cuique tribuere [allowing to each man his own].[11]

Even before he became editor of the *Transactions*, then, Oldenburg had occasion to note that men of science might be induced to accept the new norm of free communication through a motivating exchange: open disclosure in exchange for institutionally guaranteed honorific property rights to the new knowledge given to others.

In the course of looking after Boyle's writings, the future editor of the *Transactions* came upon prompt publication as another device for preserving intellectual property rights. For, like other scientists of his time, Boyle was chronically and acutely anxious about the danger of what he described as "philosophicall robbery", what would be less picturesquely described today as plagiarism from circulated but unpublished manu-

(New York: Free Press, 1952), chapter IV; Storer, Norman, *The Social System of Science* (New York: Holt, Rinehart and Winston, 1966), pp. 76–136; Cournand, André and Zuckerman, Harriet, "The code of science", *Studium Generale* XXIII (1970), pp. 941–962.

[10] *The Correspondence of Henry Oldenburg,* Volume II, edited and translated by Hall, A. Rupert and Hall, Marie Boas (Madison: University of Wisconsin Press, 1966), p. 319, italics added. We have drawn extensively on the volumes of this correspondence which provide an incomparable storehouse of information on the early days of the *Transactions.*

[11] *Ibid.*, p. 329.

scripts. Boyle felt that he had often been so victimised.[12] As his agent, Oldenburg arranged for quick publication of a batch of Boyle's papers, writing him reassuringly: "They are now very safe, and will be wthin this week in print, as [the printer] Mr. Crook assureth, who will also take care of keeping ym unexposed to ye eye of a Philosophicall robber."[13] Later, as editor of the *Transactions*, Oldenburg could draw upon this motivation in having Boyle agree that he would "from time to time contribute some short Papers, to that Design you are monthly & happily prosecuting . . . ",[14] Boyle all unknowing that in this way he was helping to institute a new form for the dissemination of knowledge which would eventually become identified as the "scientific paper".

Boyle did report, however, another motive for contributing to the newly-invented journal. Almost in so many words, he saw this as a way for the scientist to have his work permanently secured in the archives of science, as he went on to say of Oldenburg's request to contribute to the *Transactions*:

> I mightly justly be thought too little sensible of my own Interest, if I should altogether decline so civil an Invitation, and neglect the opportunity of having some of my Memoirs preserv'd, by being incorporated into a Collection, that is like to be as lasting as usefull.

The fugitive nature of letters as the more familiar means of communicating short reports on scientific work may have emphasised by contrast the potentially enduring character of a journal, particularly one sponsored by a scientific society. In any case, we find in Boyle's remarks an early intimation of the scientific journal as a scientific archive.

Another motive could be harnessed to the developing innovation of a scientific periodical. Property rights in discovery were sought after by scientists primarily as individuals but occasionally also as nationals.[15] As A. R. Hall and M. B. Hall, the editors of the Oldenburg correspondence, observe, by 1667, Oldenburg was eager "to demonstrate English priority [on the filar micrometer] and careful to put it in print in the *Philosophical Transactions*. Similarly he took much care to insist in the *Philosophical Transactions* that it was the English, not the French or the Germans, who had invented the idea of injecting medicines into the veins and of practising blood transfusion between animals".[16] Such interest in

[12] So disturbed by plagiarists of his work was Boyle that he prepared a document, later running to three folio pages of print, itemising all the ingenious devices for thievery developed by the grand larcenists of 17th-century science. See *The Works of the Honourable Robert Boyle,* Volume I, Birch, J., ed., in six volumes (London, 1772), pp. cxxv–cxxviii, ccxxii–ccxxiv and also his letter in *Correspondence of Henry Oldenburg,* Volume IV, p. 94.

[13] *Correspondence of Henry Oldenburg,* Volume II, p. 291.

[14] *Ibid.,* Volume III, p. 145.

[15] On conflicting national claims to priority, see Merton, R. K., "Priorities in scientific discovery," *American Sociological Review,* XXII (1957), pp. 635–659 and "Resistance to the systematic study of multiple discoveries in science", *European Journal of Sociology,* IV, 1963, pp. 237–282.

[16] Hall and Hall, in the introduction to *Correspondence of Henry Oldenburg, op. cit.,* Volume III, p. xxv.

national priority could also be drawn upon to press reluctant scientists into contributing " sufficient matter " to the new publication. Thus, the mathematician, John Wallis, who played a large part in the early history of the *Transactions*, could argue the case for ensuring national priority in connection with the much-advertised claims of the French to having initiated blood transfusion:

Onely I wish that those of our Nation; were a little more forward than I find them generally to bee (especially the most considerable) in timely publishing their own Discoveries, & not let strangers reape ye glory of what those amongst ourselves are ye Authors.[17]

Through these and kindred institutional devices, the new scientific society and the new scientific journal persuaded men of science to replace their attachment to secrecy and limited forms of communication with a willingness to disclose their newly-found knowledge.[18] But institutionalisation is more than a matter of changing values; it also involves their incorporation into authoritatively defined roles. As the organisation sponsoring the *Transactions*, the Royal Society provided the power and authority which enabled it to institute new roles and associated rewards for acceptance of these roles. True, in its early days, the Royal Society included many members with little or no scientific competence. But, what was more consequential for the process of institutionalisation, it included all English scientists (and many foreign ones) who were producing significant scientific work. As a result, it was widely identified, both in England and on the Continent,[19] as an authoritative body of scientists.

This authority based on demonstrated competence provided mutually reinforcing consequences for scientists in their triple roles as members of the Royal Society, as contributors to the *Transactions* and as readers of it. These consequences shaped the early evolution of the scientific journal and the referee system in several ways. First, growing numbers of scientists seeking competent judgments of their work turned increasingly to the Royal Society. Thus, the distinguished astronomer Hevelius wrote of his important work *Cometographia* that " as soon as it is published I will make it my first care to submit it to the high judgment and due consideration of the Royal Society ".[20] The French astronomer and engineer, Pierre Petit, paid his respects to the " celebrated Society, to which judgment I submit all my ideas ".[21] Nor were these merely polite

17 *Ibid.*, Volume III, p. 373.

18 Through the process of socially induced displacement of goals, this value of open communication would eventually become transformed for appreciable numbers of scholars and scientists into an urge to publish in periodicals, all apart from the worth of what was being submitted for publication. This development would in turn reinforce a concern within the community of scholars for the sifting, sorting and accrediting of manuscripts by some version of a referee-system

19 Hall and Hall, in the Introduction to *ibid.*, Volume II, p. xxi.

20 *Ibid.*, Volume II, p. 1938; see also Volume IV, p. 448.

21 *Ibid.*, Volume II, p. 595. For other cases in which the Royal Society was asked to sit as a court of scientific judges, see *ibid.*, Volume III, pp. 6, 171, 219 and 298.

phrases. As the Halls observe, it was not long before the "practice of writing for publication in the *Philosophical Transactions*" was greatly increasing among European men of science.[22] This new practice of writing *directly* for publication in a journal constituted another appreciable change in the evolving role of the scientist. With the composite institution of learned society and learned journal at hand, scientists began to seize the new opportunity of having competent appraisals of their work by other authoritative scientists,[23] a pattern of attitude and behaviour which is basic to the referee system.

The practice of having scientific communications assessed by delegated members of the Royal Society might have affected the quality of those communications. Communications intended for publication would ordinarily be more carefully prepared than private scientific papers, and all the more so, presumably, in the knowledge that they would be scrutinised by deputies of the Society.

The constituted representatives of the Royal Society, looking to its reputation, were in their turn motivated to institute and maintain arrangements for adequately assessing communications, before having them recorded or published in the *Transactions.* They repeatedly express an awareness that to retain the confidence of scientists they must arrange for the critical sifting of materials which in effect carry the *imprimatur* of the Society. Thus, the president of the Society, "before he will declare anything positively of ye figure of these Glasses, will by a gage measure ym; and if ye Invention bear his test, it will pass for currant, & be no discredit to ye Society, yt a member of theirs is ye Author thereof." [24] Or, as the editor-secretary Oldenburg later reported to Boyle, the matter could not be too carefully studied "before we give a publick testimony of it to ye world, as is desired of us".[25] The Society was also beginning to distinguish between evaluated and unevaluated work which came to its notice. On occasion, this involved the policy of "sit penes authorem fides (let the author take responsibility for it): We only set it downe, as it was related to us, without putting any great weight upon it." [26] In the course of establishing its legitimacy as an authoritative scientific body, the Royal Society was gradually developing both norms and social arrangements for authenticating the substance of scientific work.

22 Hall and Hall, in the introduction, *ibid.*, Volume IV, p. xxiii.

23 In a series of papers Merton has developed the idea that this concern of scientists with having appropriate recognition of their work by peers is central to the workings of science as a social institution. See, for example, Merton, "Priorities in scientific discovery", *op. cit.* This idea has been instructively advanced by Norman Storer as involving a concern with competent appraisal; see his *The Social System of Science, op. cit,* especially pp. 19–27 and 66–73.

24 *Correspondence of Henry Oldenburg,* Volume IV, pp 223–224.

25 *Ibid.*, Volume IV, p. 235. In the event, a short account of these optical instruments was soon published in the *Transactions.*

26 *Ibid.*, Volume IV, p. 235. It is of some interest that in response to the flood of manuscripts today, with its overloading of facilities for refereeing, some journals are adopting the same policy, allowing some papers to be published though unrefereed, providing that a note appended to the article testifies to its not having been refereed.

Ingredients of the referee system were thus emerging in response to distinctive concerns of scientists taken distributively and collectively. In their capacity as producers of science, individual scientists were concerned with having their work recognised through publication in forms valued by other members in the emerging scientific community who were significant to them. In their capacity as consumers of science, they were concerned with having the work produced by others competently assessed so that they could count on its authenticity. In providing the organisational machinery to meet these concerns, the Royal Society was concerned with having its authoritative status sustained by arranging for reliable and competent assessments.

There are intimations, even in this early period, that individual scientists in their role as informed consumers would begin to affect the process making for control of the quality of publications in journals. When the editor or the Royal Society slipped up by allowing dubious materials to be published in the *Transactions*, as they not infrequently did, readers would on occasion register their protest. The French astronomer Auzout, for example, censured the editor for printing unauthenticated and doubtful accounts:

> Some of our virtuosi are surprised at your speaking in your journal of parabolic lenses. Your students of dioptrics know that they are worthless and whatever fine promises are made, when these seem contrary to reason one ought not to speak of them until the results have been seen; for it is not very urgent to know what charlatans may promise.[27]

We have no evidence that such critical responses actually made for greater care in subsequent editorial decisions. The point is, however, that the newly instituted journal, unlike the printers of books at that time, provided an arrangement through which members of the scientific community could affect editorial practices. Through the emergence of the role of editor and the incipient arrangements for having manuscripts assessed by others in addition to the editor, the journal gave a more institutionalised form for the application of standards of scientific work.

Efforts to cope with immediate problems produced other adaptive changes in the learned journal. By the end of the seventeenth century, there were signs of role-differentiation, especially in journals dealing with diverse fields of knowledge, in the form of a staff or "board" of editors. The *Journal des Sçavans* for one example had by 1702 assigned responsibility for particular departments of learning to each of a staff of editors who met weekly to review copy.[28] Other aspects of the journal developed more slowly. It took a century for the format of the scientific

[27] *Ibid.*, Volume III, p. 111 and, in the editor's translation quoted here, p. 114.

[28] Barnes, Sherman B., "The Editing of Early Learned Journals", *Osiris*, I (1936), pp. 155–172 at pp. 157–159.

paper to become more or less established and even longer for the scholarly apparatus of footnotes and citations to be generally adopted.[29]

Almost from their beginning, then, the scientific journals were developing modes of refereeing for the express purpose of controlling the quality of what they put in print.

Patterns of evaluation in the Sciences and Humanities

Turning from those early days to the present, we find that some version of the referee system has been widely adopted. In the physical and biological sciences, for example, a recent survey of 156 journals in 13 countries found that 71 per cent. made some use of referees.[30]

What, then, are the gross outcomes of the evaluation process by editors and referees of journals in the principal fields of science and learning? Are there pronounced differences among the various disciplines? Are observed variations in outcome random or patterned? To explore these questions, we have compiled the rates of rejections in a sample of 83 journals in the humanities, the social and behavioural sciences, mathematics, and the biological, chemical and physical sciences.[31] (The results are shown in Table I, with the disciplines ranked in order of decreasing rates of rejection.)

The figures exhibit marked and determinate variation. Journals in the humanities have the highest rates of rejection. They are followed by the social and behavioural sciences with mathematics and statistics next in line. The physical, chemical and biological sciences have the lowest rates, running to no more than a third of the rates found in the humanities.

Confirming this empirical uniformity are subsidiary patterns of deviant rates within disciplines which virtually reproduce the major patterns. To begin with, consider the field of physics. The 12 journals had an average rejection rate of 24 per cent., with the figures for 11 of them varying narrowly between 17 per cent. and 25 per cent. But the twelfth journal, the *American Journal of Physics,* departs widely from this norm

[29] Porter, *op. cit.*, p. 225; de Solla Price, Derek J., "Communication in Science: the Ends—Philosophy and Forecast", in de Reuck, Anthony and Knight, Julie, eds., *Ciba Foundation Symposium on Communication in Science* (London: J. & A. Churchill, 1967), pp. 199–209 at p. 200.

[30] International Council of Scientific Unions, *A Tentative Study of the Publication of Original Scientific Literature* (Paris: Conseil International des Unions Scientifiques, 1962) There are marked variations by country: for example, only 2 of 49 journals published in the United States in contrast to 9 of 30 French journals made no use of referees.

[31] A first list was drawn from Bernard Berelson's compilation of leading journals in his *Graduate Education in the United States* (New York: McGraw Hill, 1960). This list was supplemented by other research journals published under the auspices of the major associations of scholars and scientists. In all, the editors of 117 journals were queried by mail; responses were received from 97 of them and usable information from 83. The *Physical Review Letters* in physics and similar journals in other sciences are excluded from this list since they are especially designed for "rapid publication". On the special problems confronted by such publications, see Goudsmit, S. A., "Editorial", *Physical Review Letters*, XXI (11 November, 1968), pp. 1425–1426.

TABLE I

Rates of Rejecting Manuscripts for Publication in Scientific and Humanistic Journals, 1967

	Mean rejection rate %	No. of journals
History	90	3
Language and literature	86	5
Philosophy	85	5
Political science	84	2
Sociology	78	14
Psychology (excluding experimental and physiological)	70	7
Economics	69	4
Experimental and physiological psychology	51	2
Mathematics and statistics	50	5
Anthropology	48	2
Chemistry	31	5
Geography	30	2
Biological sciences	29	12
Physics	24	12
Geology	22	2
Linguistics	20	1
Total		83

with a rejection rate of 40 per cent. In the light of the general pattern of rejection rates, we suggest that this seemingly deviant case only confirms the rule. For this journal, alone among the twelve assigned to physics in Table 1, is not so much a journal *in* physics as a journal *about* physics. It publishes articles dealing primarily with the humanistic, pedagogical, historical and social aspects of physics rather than articles presenting new research in physics. Accordingly, it diverges from the relatively low rate characteristic of the physical sciences in the direction of the substantially higher one characteristic of the humanities and social sciences.

We find similar patterns within other disciplines. The two journals in anthropology for example have an average rejection rate of 47·5 per cent., considerably below that for the other social sciences. But this is a composite of drastically different rates for the two journals. The *American Anthropologist,* devoted largely to social and cultural anthropology, approximates the high rejection rates of the other social sciences with a figure of 65 per cent., while the *American Journal of Physical Anthropology* with a figure of 30 per cent. approximates the low rates of the physical sciences. We find much the same difference in psychology. The journals devoted to social, abnormal, clinical and educational psychology average a rejection rate of 70 per cent. while the journals in experimental, comparative and physiological psychology diverge toward the physical sciences with an average of 51 per cent. Consider only one more case of this confirming finer pattern within the gross pattern, this time for subjects ordinarily assigned to the humanities. The journals of language and literature in the humanistic tradition have an average rejection rate of 86 per cent., whereas the journal, *Linguistics,* adopting mathematical and logical orientations in the study of language, has a rejection rate of 20 per cent., much like that of the physical sciences.

The pattern of differences between fields and within fields can be described in the same rule of thumb: the more humanistically oriented the journal, the higher the rate of rejecting manuscripts for publication; the more experimentally and observationally oriented, with an emphasis on rigour of observation and analysis, the lower the rate of rejection.[32]

These variations in the institutional behaviour of learned journals may in part reflect differences in the extent of agreement on standards of scholarship in the various disciplines. It appears to be the case that the journals with high rejection rates receive a larger proportion of manuscripts that in the judgement of the editor and his referees are not simply debatable border-line cases but fail by a wide margin to measure up even to minimum standards of scholarship. This suggests that these fields of learning are not greatly institutionalised in the reasonably precise sense that editors and referees on the one side and would-be contributors on the other almost always share norms of what constitutes adequate scholarship. In the case of one journal, for example, which rejects nine of every ten papers, about 40 per cent. are promptly turned down by the editor as hopelessly inept and unpublishable in any learned journal. The editor of a journal with a final rejection rate of about 80 per cent. perceives the standards employed by his referees as more demanding than those employed by other journals in the field. He himself rejected more than 40 per cent. of incoming manuscripts, explaining that they

> . . . were manuscripts which I judged to be extremely unlikely to survive our rigorous screening no matter who reviewed them, so I carefully reviewed them myself and typically sent the authors a one- to three-page, single-spaced letter explaining why we could not accept it here and how they might revise the manuscript for submission elsewhere or how they might improve on the present study so as to do some publishable research.

And the editor of another journal with a high rejection rate of 85 per cent. reports that about 20 per cent. of incoming papers "were so clearly unacceptable that I didn't want to waste a referee's time with them. . . . We still get a flow of articles of a thoroughly amateurish quality."

[32] The empirical solidity of this rule of thumb is illustrated by an episode which occurred in the course of our survey of journals. The editor of a journal in chemical physics reported a rejection rate of 75 per cent., far above figures for the other journals in physics and chemistry. Taking note of this anomalous figure, we asked the editor to account for it only to have him report that it was simply a clerical error; he had reported the rate of acceptance, not the rule-like rejection rate of 25 per cent.

The influx of manuscripts judged to be beyond all hope of scholarly redemption testifies to the ambiguity and the wide range of dispersion of standards of scholarship in the discipline, all apart from the question whether the institutionally legitimated editors and referees or the would-be contributors are exercising better judgement. We do not know the comparative frequency of these reportedly unsalvageable manuscripts in different fields but the testimony of editors suggests that it is considerably higher in the humanities and the social sciences.

There are intimations in the data also that the editors and referees of journals with markedly different rates of rejection tend to adopt different decision-rules and so are subject, when errors of judgment occur, to different *kinds* of error. Editors and referees, of course want to avoid errors of judgement altogether. But recognising that they cannot be infallible, they seem to exhibit different preferences. The editorial staff of high-rejection journals evidently prefer to run the risk of rejecting manuscripts which the wider community of scholars (or posterity) would consider publishable (or even, perhaps, important)—an error of the first kind—rather than run the risk of publishing papers that will be widely judged to be sub-standard. The editorial staff of low-rejection journals, where external evidence suggests that the decisions of scientists to submit papers are based on standards widely shared in the field, apparently prefer to risk errors, if errors there must be, of the second kind: occasionally to publish papers that do not measure up rather than to overlook work that may turn out to be original and significant. Thus the editor of a journal which rejects only one paper in five acts on the assumption that a manuscript is publishable until clearly proved otherwise. As he puts it, "If the first referee recommends publication, as received or with minor revision, that is usually sufficient. If the first referee's opinion is negative, or undecided, additional referee(s) will be consulted until a consensus is reached." Editors of another journal in this class note that they "have generally published 'borderline' papers—those on which referees' opinions differed". Put in terms reminiscent of another institutional sphere, the decision-rule in high-rejection journals seems to be when in doubt, reject; in low-rejection journals, when in doubt, accept.

The actual distribution of these decision-rules and their consequences for the quality of scholarship in the various disciplines still remain to be determined. But even now it appears that the rules will have different consequences for scientists and scholars at different stages of their development. The Coles and Zuckerman have found that collegial recognition of the work of young scientists is important for their continued productivity.[33] This suggests that the discouragement of having papers rejected

[33] Cole, Stephen and Cole, Jonathan R., "Scientific Output and Recognition: A Study in the Operation of the Reward System in Science", *American Sociological Review*, XXXII (1967), pp. 377–390. Zuckerman, Harriet, *Nobel Laureates in the United States*, Columbia University doctoral dissertation (1965), Chapter X provides qualitative evidence of the reinforcing effects of such recognition.

may be more significant for the novice than for the established scholar. The multiplicity of journals [34] need not entirely solve the problem for him. Since his research capabilities still require institutional certification, it can matter greatly to him whether his paper is published in a journal of higher or lower rank. Rejection of his paper by a high-ranking journal might be more acutely damaging, more often leading him to abandon his plans for publication altogether.

Whatever their consequences, the marked differences in the rejection rates of journals in the various disciplines can be tentatively ascribed only in part to differences in the extent of consensus with regard to standards of adequate science and scholarship. Beyond this are objective differences in the relative amount of space available for publication.[35] Editors of all journals must allocate the scarce resources of pages available for print, but not all fields and journals are subject to the same degree of scarcity. Journals in the sciences can apparently publish a higher proportion of manuscripts submitted to them because the available space is greater than that found in the humanities. Take the case of physics. The articles in journals of physics are ordinarily short, typically running to only a few pages of print, so that the " cost " of deciding to publish a particular article is small and the direct costs of publication are often paid by authors from research grants.[36] The increase in available journal space, moreover, has been outrunning the increase in the number of scientists. The number of pages published annually by *The Physical Review* (and *Physical Review Letters*), for example, increased 4·6 times from 3,920 pages in 1950 to 17,060 in 1965; during the same interval, the number of members of the American Physical Society increased only 2·4 times. Preliminary counts for the humanities and social sciences do not show the same disproportionate increase in journal space beyond increase in the numbers of scholars. By way of comparison, the number of pages available in the official journal of the American Sociological Association remained about the same between 1950 and 1965, while the membership of the Association increased two and a half times.

Observations of this sort deal only with the final outcomes of the

[34] It has often been suggested that papers which are at all competent eventually find their way into print. Tullock, *The Organisation of Inquiry, op. cit.*, p. 144; Storer, *op. cit.*, pp. 132–133; Hagstrom, Warren O., *The Scientific Community* (New York: Basic Books, 1965), pp. 18 onwards. But only now is there the beginnings of evidence on the proportion of papers published by journals of differing rank which are first, second or n^{th} submissions for publication. See Lin, Nan and Nelson, Carnot E., " Bibliographic Reference Patterns in Core Sociological Journals ", *The American Sociologist*, IV (1969), pp. 47–50. Beyond this, nothing is known about the use made of papers which have been published only after having circulated through the editorial offices of several journals.

[35] We are indebted to Dr. Jonathan R. Cole for suggesting this line of inquiry.

[36] The effects of " page charges " to authors on patterns of publication in scientific journals constitute a complex problem in its own right which is being studied by Belver Griffith, Frances Korten and the Center for Research in Scientific Communication, at The Johns Hopkins University.

evaluative process as registered in comparative rates of rejecting manuscripts for publication. Of course this gross information tells next to nothing about the process of evaluation itself. This we can examine in some detail by turning to the scientific journal for which we have the needed archival evidence, *The Physical Review.*

Evaluative Behaviour of Editors and Referees

First, a few words about *The Physical Review.* It publishes 72 issues a year (and two index volumes) in addition to weekly publication of short research reports in the *Physical Review Letters.* It makes up six per cent. of the world's journal literature in physics (together with the *Letters,* nine per cent.). We can gauge the relative scale of this publication by noting that in 1965 *The Physical Review* itself—excluding the *PRL*—published more literature in physics than all 53 journals published in Germany, once the world centre of physics.[37]

All this quantity need not, of course, make for high quality. But it turns out that in the 1950s as now, *The Physical Review* ranked far ahead of all other journals of physics in extent to which it was used in further research. Papers published in it were far more often cited than those published in any other journal of physics and cited more often than if it were simply holding its own—that is, getting the same share of citations as its share in the physics literature. In such leading journals as the Italian *Nuovo Cimento,* the Russian *Journal of Experimental and Theoretical Physics* (*JETP*), and the *Proceedings* of the Physical Society of London, the *Review* is cited far more often than these journals themselves[38]:

> 36 per cent. of the references in *Nuovo Cimento* are to *The Physical Review* but only 17 per cent. to all Italian journals combined;
>
> 22 per cent. of all references in *JETP* are to the *Review,* compared with 15 per cent. going to the *JETP* itself;
>
> 34 per cent. of the references in the *Proceedings* are to the *Review,* compared with 9 per cent. to the *Proceedings* itself.

This widespread use of work published in the *Review* is all the more notable since there is a general tendency for papers in each journal to cite other papers in the same journal. Kessler sums up his findings on patterns of use in the contemporary literature of physics by noting that " *The*

[37] Kennan, Stella and Brickwedde, F. G., *Journal literature covered by Physics Abstracts in 1965* (New York: American Institute of Physics, 1968), 68–1, Appendix II.

[38] Kessler, M. M., *Technical Information Flow Patterns* (Cambridge, Mass.: Lincoln Laboratories, (Massachusetts Institute of Technology), pp. 247–257, reporting data for the year 1957 and Kessler, M. M., " The MIT Technical Information Project ", *Physics Today,* XVIII (March, 1965), pp. 28–36 at p. 30.

Physical Review is truly a definitive journal for physicists. It commands overwhelming dominance over all other journals as a carrier of information between physicists of all lands."[39]

The behaviour of physicists, both as consumers and producers of research, testifies to much the same judgement. As consumers, some 77 per cent. of the 1,300 American academic physicists queried by the Coles reported that the *Review* is among the journals they read most often (no other journal being mentioned by more than 25 per cent. of the sample).[40] As producers, the archives testify, physicists preferred to have their papers published in the *Review*, maintaining that this would give them greater visibility to their colleagues around the world. Plainly, we are dealing here with the outstanding scientific journal in its field. What, then, have been its patterns of editorial and referee evaluation?

Sampling the Archives of The Physical Review[41]

The basic data consist of the archives of the *Review* for the nine years between 1948 and 1956, containing correspondence between authors, editors and referees, records of decisions made by the editors, the allocation of manuscripts to referees, their evaluations and the final disposition of the papers. This provides a rich body of materials, both quantitative and qualitative, for analysing the infrastructure of scientific evaluation in a journal of the first class. More particularly, it enables us to find out how the workings of this structure are affected by the stratification system of science.

Consider first the population of physicists submitting manuscripts and the gross outcomes of the evaluative process. In this nine-year period, a total of 14,512 manuscripts were submitted (a little more than half of them had a single author). In this report, we deal primarily with the papers with a single author of which 80 per cent. were ultimately published. The sample we have drawn from these voluminous materials is based on a conception of the stratification system of science as a distinctive compound of egalitarian values governing access to opportunity to publish and a hierarchic structure in which power and authority are largely vested in those who have acquired rank through cumulative scientific accomplishment. It is a status-hierarchy, in Max Weber's sense, based on honour and esteem. Although rank and authority in science are *acquired* through past performance, once acquired, they then tend to be *ascribed* (for an indeterminate duration). This combination of acquired and

[39] Kessler, *Technical Information Flow Patterns*, p. 249.

[40] Cole, Stephen and Cole, Jonathan R., " Visibility and the Structural Bases of Awareness of Scientific Research ", *American Sociological Review*, XXXIII (June, 1968), pp. 397–413 at p. 412.

[41] We are indebted to Professor Samuel A. Goudsmit, editor-in-chief of publications for the American Physical Society, for having made these archives available to us in 1966. He has recently described the editorial and refereeing procedures currently adopted by *The Physical Review* in " What Happened to My Paper? ", *Physics Today*, XXII (May, 1969), pp. 23–25.

ascribed status introduces strains in the operation of the authority-structure of science, as has been noted with great clarity by Michael Polanyi and Norman Storer.[42] These strains may be doubly involved in the processes of evaluating scientific work. In one direction, judgements *by* scientific authorities (whose status largely rests on their own past performance) may come to be assigned great or even decisive weight, and not simply because of their intellectual cogency. In the other direction, judgements *about* the work of ranking scientists may be systematically skewed by deference, by less careful appraisals involving less exacting criteria, by self-doubts of one's own sufficient competence to criticise a great man or by fear of affronting influential persons in the field. Although based on status acquired through assessed accomplishment, the hierarchy of excellence in science can militate in both ways against the unbiased, universalistic evaluation of scientific work.

With this stratification system of science in mind, we have drawn a sample of the contributors in the 1948–56 archives of *The Physical Review* which is stratified into three levels of institutionalised standing based on appraisals of past scientific work. In the first rank are *all* the physicists submitting manuscripts who, by the end of the period (1956), had received at least one of the ten most respected awards in physics (such as the Nobel prize, membership in the Royal Society and in the National Academy of Sciences).[43] These number 91 in all, with 55 of them having submitted papers of which they were the sole authors. The physicists of the second rank, although they had not been accorded any of the highest forms of recognition, had been judged important enough by the American Institute of Physics to be included in its archives of contemporary physicists. All 583 of the physicists in the American Institute of Physics list who had submitted manuscripts to the *Review* during this period make up this intermediate rank, with 343 of them having sent in manuscripts of which they were the sole authors. The remaining 8,864 contributors comprise the third rank in this hierarchy. They are not included in their entirety but are represented by two successive 10 per cent. random samples, yielding a total of 1,663 authors, with 659 of them having submitted manuscripts of which they were sole authors.[44]

[42] Polanyi, Michael, *Personal Knowledge* (London: Routledge & Kegan Paul, 1958), especially Chapters 6–7; Storer, *op. cit.*, pp. 103–134.

[43] See Cole and Cole, *op. cit.* (1967), p. 383 for the prestige-ranking of awards by a sample of 1,300 physicists.

[44] A first 10 per cent. random sample was selected from the physicist-authors remaining in the files after all cases of top-ranking and intermediate authors were removed. This sample of third rank authors numbered 866, with 355 of them having submitted papers which had a single author. Analysis of this first sample involving three or more variables led to results sometimes based on small numbers. To check these results, we drew a second 10 per cent. random sample of the remaining third rank authors, this yielding 797, of whom 304 had submitted papers with a single author. As it turns out, the results for the successive samples are so much the same—they vary by no more than three percentage points—that they are reported only in the aggregate.

For some special analyses, we also identified a mobile subgroup in the status-hierarchy: the 49 contributors who were in the intermediate rank during the time covered by this study but who later moved into the most eminent stratum. In effect, these physicists were observed in the course of their ascent, after having achieved a measure of distinction but before receiving the highest recognition. It will be of interest to find out how the system for evaluating manuscripts dealt with physicists whose work was later to earn them great esteem.

The 354 referees who evaluated the manuscripts with a single author submitted by our sample of authors were stratified in the same way, with 12 per cent. of them turning up in the first rank, 35 per cent. in the second, and the remaining 53 per cent. in the third.

The sample of contributors and the derivative sample of referees were designed with an eye to the general problem of the interplay between the hierarchical structure of authority and the evaluation of scientific work. More specifically, we want to examine the extent to which universalistic and particularistic standards were utilised in evaluating the papers submitted to *The Physical Review* by physicists of differing rank. Since this is our purpose, we shall limit our analysis almost entirely to papers with one author, for reasons both substantive and procedural. Substantively, it turns out that papers with more than one author, largely reporting experimental results, have so high an acceptance rate (over 95 per cent.) that they can exhibit little variability in evaluations of the kind we want to investigate. Procedurally, it is the case that the rank of the single author can be unambiguously and realistically identified. But not so in the case of papers by several hands, with their varying numbers of authors, often of differing rank.

Drawing upon the samples of authors and referees, we want to examine four main sets of questions. First, do contributors variously located in the stratification system differ in the rate at which they submit manuscripts for publication in the *Review*? Second, are there patterns of allocating manuscripts to referees variously situated in the status-hierarchy and are these allocations related to the status of authors? This leads directly to the third question: are there differences in rates of acceptance depending upon the professional identity of the physicists submitting the manuscripts? And finally, are any such differences in rates of acceptance linked to the relative status of the referees and authors?

Status-Differences in Submission of Manuscripts

It has long been known that eminent scientists tend to publish more papers and not only better ones than run-of-the-mill scientists. It comes as no surprise, then, that they also submit more manuscripts for publication. Among those physicists who submitted manuscripts, produced by themselves alone, to *The Physical Review* during the nine-year period, physicists of the highest rank averaged 4·09, the intermediates, 3·46 and the physicists of the third rank averaged 2·02.[45] These differences between the strata are presumably all the greater in the population of physicists at large than in this self-selected population of would-be contributors.

The differences in rates of submission of papers are especially marked when it comes to the most prolific physicists in the sample. The physicists of the highest rank submitted 15 or more papers to this one journal at 12 times the rate of the rank-and-file, with 18 per cent. of the highest-rank physicists, 11 per cent. of the intermediates and 1·5 per cent. of the third rank having sent that many (single- and multi-author) manuscripts.

This pattern of submission-rates also contains a striking prognostic result. The 49 mobile physicists in the sample—those who had not attained eminence by the mid-1950s but did so afterwards—were the most prolific of all, with a whopping 47 per cent. of them having submitted as many as fifteen papers to the *Review*. Plainly these were physicists at a peak period of their productivity. Six of them have since received the Nobel prize and from the look of things they will be joined by others from this group of the most prolific authors. We catch here, as with a camera, a phase in the process through which early productivity is converted into later recognition by the social system of science.

To this point, the data on submission of manuscripts merely confirm earlier findings on status differences in the number of published papers. This, it might be said, is only to be expected. In general, the more manuscripts submitted, the more find their way into print. But this does not mean, of course, that the ratio of submitted to published papers is the same for the several strata of scientists. This would assume that scientists of every stripe adopt the same standards of what constitutes a paper worth submitting for publication and that the refereeing process results in uniform rates of acceptance for scientists at all levels of the stratification system.

A first intimation that these assumptions are unfounded is provided by the rates of submission and of acceptance for papers by physicists affiliated with the seventeen foremost university departments and with less distinguished ones.[46] Among the physicists submitting any single-author manuscripts at all, those in the leading departments submitted only

[45] The differences in submission-rates are greatly amplified for all manuscripts (of both single and multiple authorship), as might be expected in view of the greater facilities and opportunities for collaboration enjoyed by ranking physicists. When each author of a manuscript of multiple authorship is credited with a submission, the mean rates for the nine-year period run to 9·72 for the top-ranking physicists, 7·89 for the intermediates and 1·97 for the third rank.

[46] See Keniston, Heyward, *Graduate Study and Research in the Arts and Sciences at the University of Pennsylvania* (Philadelphia: University of Pennsylvania Press, 1959) for ranks of physics departments as judged by department chairmen in 1957. To Keniston's top fifteen departments, we added California Institute of Technology and Massachusetts Institute of Technology since technological institutions were not included in his survey. There are no comparable rankings of the quality of industrial laboratories or independent research organisations.

slightly more, with an average of 2·62 compared with 2·49 for the others.[47] But when it comes to actual publication, not submission, the picture changes. Some 91 per cent. of the papers by physicists in the foremost departments were accepted as against 72 per cent. from other universities (producing average acceptances of 2·36 and 1·79 papers, respectively).

This result sets the general problem quite clearly. What patterns of evaluation intervene between the submission of papers and actual publication to produce this result? How does it happen that the physicists from the minor departments who are submitting almost as many single-author papers as their counterparts in the major departments end up by having significantly fewer of them published? The question is critical because the gross empirical finding lends itself to sharply different kinds of interpretation

One interpretation would attribute the departmental differences in acceptance rates to the operation of the stratification system. It holds that the work of scientists in the upper strata is evaluated less severely, that these authors are given the benefit of the doubt by editors and referees, because of their standing in the field or affiliation with influential departments, and that all this is reinforced by particularistic ties between authors and referees. This hypothesis suggests that the status of both author and referee significantly affects the judgement of manuscripts, so that work of the same intrinsic worth will be differently evaluated according to these considerations of status.

Another interpretation would ascribe the different outcomes of the evaluation process principally to differences in the scientific quality of the manuscripts coming from different sources. This hypothesis maintains that universalistic standards tend to be rather uniformly applied in judging manuscripts but that, on the average, the quality of papers coming from the several strata actually differs. On this view, scientists in the departments of the highest rank tend to be positively selected in terms of demonstrated capacity, have greater resources for investigation, have more demanding internal standards before manuscripts are submitted, and are more apt to have their papers exactingly appraised by competent colleagues before sending them in for publication. On this hypothesis, it is not a preferential bias toward the status of authors and their departments which makes for differing acceptance rates by referees, but intrinsic differences in the quality of manuscripts which in turn are the outcome of joint differences in the capabilities of scientists and in the quality of their immediate academic environments.

We should repeat that although the two interpretations differ in their

[47] It should be emphasised here that these rates of submitting manuscripts do not of course register actual differences in the "per capita productivity" of departments of different rank. Since they are confined to physicists who contributed at least one manuscript to *The Physical Review* in this period, these figures take no account of the least productive physicists who are probably present in quite different proportions in departments of differing rank.

conceptions of what goes on in the evaluative process, they are not contradictory in the sense that one necessarily excludes the other.[48] Both universalistic and particularistic standards might be *concretely* involved in the actual process of evaluation, but to varying extents and in different parts of the stratification system of science. We want to estimate the extent to which one or the other of these standards is adopted and the structural arrangements that make for use of one or the other.

It is no easy matter to disentangle these components of evaluation. The standing of physicists in their field, the Coles have found, is highly correlated with the quality of their previously published work as this is assessed by fellow-physicists on all levels of status.[49] This status, earned in part by past work, may be variously bound up with editorial judgements of the quality of their new work. If all papers submitted by Nobel laureates, for example, are accepted for publication, there remains the question whether some of these would have been rejected had they been submitted by scientists of distinctly lower standing. Correlatively, if scientists who enjoy the greatest prestige have been doing work of high quality in part because their critical associates and they themselves have demanding internal standards, then the manuscripts they decide to submit for publication are apt to be rigorously pre-selected, with consequently high rates of acceptance by referees applying similarly universalistic criteria.

These difficulties of analysis could be largely avoided if authors were altogether anonymous to referees. But arrangements designed for this purpose work imperfectly.[50] Various kinds of clues in manuscripts often provide unmistakable signatures of the authors, particularly, perhaps, the eminent ones. In any event, *The Physical Review* has not tried to provide for anonymity of manuscripts, for reasons emphatically set forth by Goudsmit:

Removing the name and affiliation of the author does not make a manuscript anonymous. A competent reviewer can tell at a glance where the work was done and by whom or under whose guidance. One must also remove all

[48] We note in passing that there seems to be a strong tendency to adopt one of these interpretations *to the exclusion* of the other. The first interpretation seems congenial to those who conceive of the social institution of science as dominated by influence and the exercise of power (decidedly, not intellectual power), with the evaluative system having little to do with universally applied standards for judging validity and scientific significance. The second interpretation seems congenial to those who allow no place at all for social exchange in the institution of science, with the system of evaluation involving only the exercise of universal standards, subject to some margin of socially unpatterned errors in judgement. We hazard the guess that amongst those who seize exclusively upon one or the other interpretation, the first is more often adopted by scientists in the middle and lower reaches of the stratification system and the second, by those in the upper reaches.

[49] Cole and Cole, *op cit.* (1967), pp. 384–390.

[50] In a study of social science journals, Professor Diana Crane concludes that the effort to maintain anonymity of authors does not affect differentials in rates of publication by authors from major and minor universities. As she implies, the findings are highly tentative since they are based entirely on actual patterns of publication, without taking into account patterned variations in the rates of submitting manuscripts. See Crane, Diana, "The Gatekeepers of Science: Some Factors Affecting the Selection of Articles for Scientific Journals", *The American Sociologist*, II (1967), pp. 195–201.

references to previous work by the same author, all descriptions of special equipment and other significant parts of the paper. Nothing worth judging or publishing would be left.[51]

The archives of the *Review* nevertheless provide evidence enabling us to move a certain distance toward identifying basic patterns in the evaluation of scientific work.

Patterns of Allocation to Judges

A first phase in the evaluation process is crystal-clear. The higher the rank of authors in the prestige-hierarchy, the greater the proportion of their papers which are judged by the two editors—either singly or in tandem—without going to outside referees. Of the manuscripts submitted by the physicists of the highest rank, 87 per cent. were judged exclusively by the editors, in contrast to 73 per cent. of those coming from the intermediate rank and 58 per cent. of the rest. As we shall see, it is the more problematic papers which are sent to outside referees. All this has the immediate consequence, that the higher the rank of the physicist, the more prompt the decision taken on his manuscript (Table II), a matter of concern to many scientists, especially those wanting to safeguard their priority.

TABLE II

Duration of Editorial and Refereeing Process for Published Papers, by Rank of Author (Physical Review, 1948–1956) [a]

	Rank of author		
Duration	Higher rank physicists %	Intermediate physicists %	Third rank physicists %
Less than 2 months	42	35	29
2–4 months	47	45	41
5 months +	11	20	30
Total	(202)	(1027)	(972)

[a] This table and all subsequent ones are based on a sample of manuscripts with single authors submitted to the *Review* during this period.

The referee system calls for evaluation of manuscripts by experts on their subject. It should come as no surprise, therefore, that the outside referees were drawn disproportionately from physicists of high rank. Compared with the 5 per cent. of the 1,056 authors (themselves in some

[51] Goudsmit, S. A., *Physics Today*, XX (January, 1967), p. 12.

measure a selected aggregate), almost 12 per cent. of the 354 outside referees assessing their papers were in the highest rank. Moreover, these 12 per cent. of the referees contributed one-third of all referee judgements. They refereed an average of 8·5 papers compared with 3·8 for the intermediates and 1·4 for the rank-and-file. And although some 45 per cent. of the referees were under the age of 40, thus giving major responsibility to the relatively young, it should also be noted that research physicists are a youthful aggregate, with fully 74 per cent. of the papers submitted to the *Physical Review* coming from men under 40. Much the same pattern of stratification is found when referees are classified according to the rank of the institution with which they are affiliated, rather than their individual rank. For example, about two-thirds of all referee judgements were made by physicists in the 17 major departments of physics in universities, the Bell Laboratories and the Institute for Advanced Study.

The composite portrait of referees is clear enough. Whether gauged by their own prestige, institutional affiliations or research accomplishments, they are largely drawn from the scientific elite, as would be expected from the principle of expertise.

We want now to consider patterns of allocating the referees to authors of varying rank. The possible patterns are describable in four models, which can be designated as the " oligarchical " model, the " populist " model, the " egalitarian " model and the model of expertise.

In the oligarchical model, the established elite of science alone has the power to judge the work of those beneath it in the status-hierarchy. The second model corresponds to a populistic view which assigns power of judgement to " the people ". The strictly egalitarian model, by contrast, calls for a policy in which papers are assessed only by juries of status-peers. And last, the model of expertise calls for the allocation of manuscripts to referees who, regardless of rank, are especially competent to judge them.

It is easy enough to construct the distribution of cases in our data which would correspond to an oligarchical policy for allocating referees to authors. This would require all manuscripts to be evaluated by judges ranking higher than authors with the exception of those submitted by the highest rank of scientists. Having reached the top of the status-hierarchy, they would be exempt from oligarchy and judged by peers. Put in terms of our data, this model would have physicists of the highest rank evaluating all the manuscripts by the intermediate physicists and these two ranks in turn would be charged with assessing the work of the third rank. As the lowest stratum, the third rank would do no refereeing at all. A glance at Table III is enough to indicate that the actual pattern of allocation diverges greatly from this oligarchical model. " Status-inferiors " do much more refereeing and, by the same token, " status-superiors " far less than this model requires.

The second model, expressing a populist view, would have manuscripts judged exclusively by physicists ranking lower than authors. This model is of course at odds with the traditional ethos of science which holds that the quality of scientific accomplishment is the determinant of the status ascribed to scientists. It turns out that the data on actual allocations diverge very widely from the populistic model. Relative to their numbers, lower-ranking physicists do little refereeing altogether and also referee far fewer papers by the intermediate and highest ranking physicists than would be the case under a populistic allocation.

According to the strictly egalitarian model, papers would be assessed exclusively by status-peers. The actual distribution, as Table III shows, departs very widely from this model also. As can be seen by aggregating the cases in the left-to-right diagonal, only about a third of all judgements are made by status-peers of authors, and there is a widening deviation from the model for the lesser ranks of physicists.

In short, the actual patterns of allocation of referees to authors approximate to none of these models.

TABLE III

Rank of Referees Assigned to Authors of Differing Rank

(Physical Review, 1948–1956)

	Rank of referees			
Rank of authors	Higher rank physicists %	Intermediate physicists %	Third rank physicists %	Total judgements by referees
Higher rank physicists	50	31	19	(36)
Intermediate physicists	38	41	21	(394)
Third rank physicists	27	46	26	(653)
All authors	32	44	24	(1083)

The principle of expertise requires that referees should be assigned to manuscripts on the basis of their competence. The data presented in Table III are at least consistent with the principle of expertise once it is assumed that demonstrated expertise is substantially (*i.e.*, imperfectly) correlated with rank in the hierarchy of prestige. On this assumption, the data would exhibit a preponderance but no monopoly of refereeing by physicists ranking higher than authors. Authors would occasionally outrank referees in prestige (if not in competence) and judgement by peers would be relatively more frequent for the successively higher ranks of authors. These patterns turn up in the actual data recorded in Table III as we see for the example of judgements by status-peers accounting for 50 per cent. of the papers by top-ranking physicists, 41 per cent. for the intermediates and 26 per cent. for the rank-and-file.

This suggests, although it does not demonstrate, that expertise and competence were the principal criteria adopted in matching papers and referees. That papers by distinguished scientists were assigned for review to others of like stature need not mean, therefore, that an inner circle of physicists were being asked to pass judgements upon one another's work in a closed system of mutual support. The principle of expertise would lead to such allocations just as it would to the observed pattern of referees more often outranking authors than conversely.

In any case, we now know that the more highly placed physicists had power disproportionate to their number in deciding what was to enter into the pages of *The Physical Review*. How did they act in these positions of power?

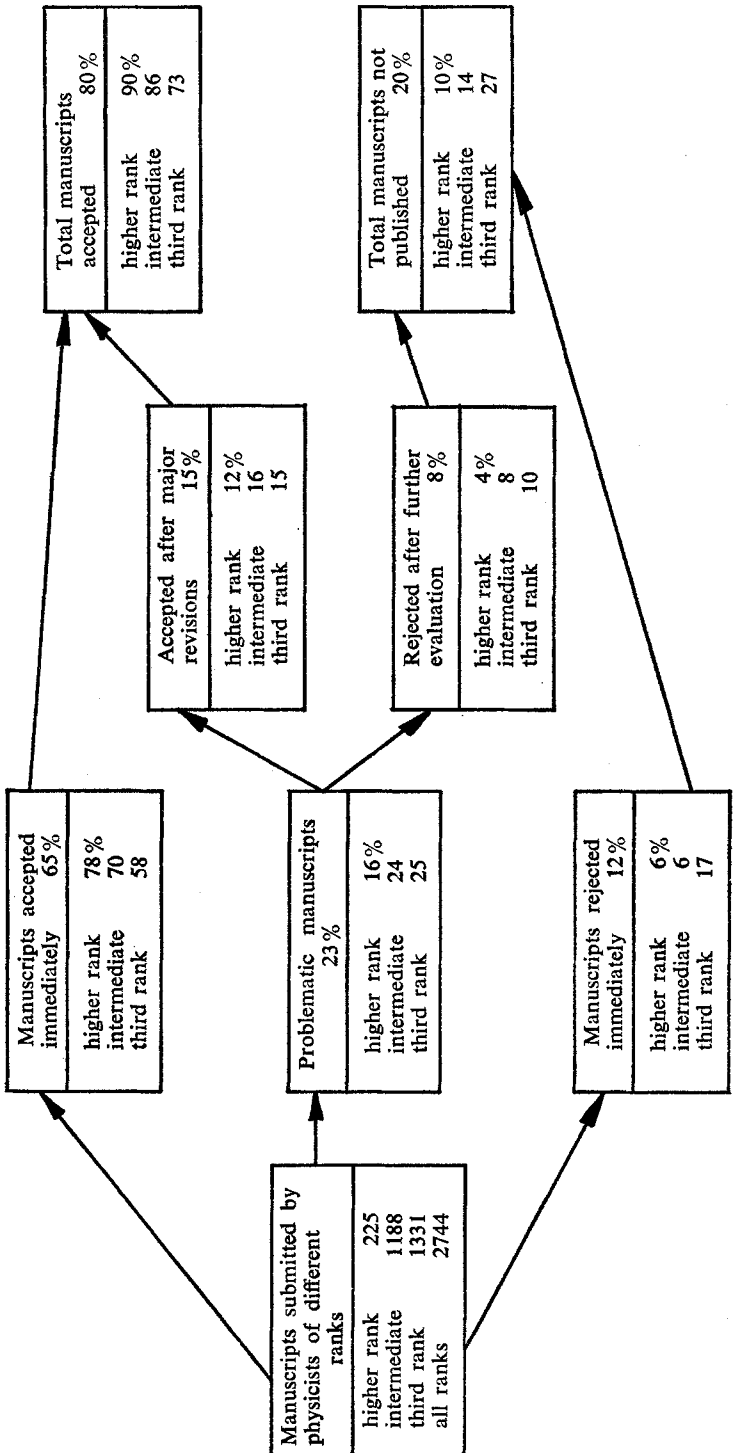
Status Differences in Rates of Acceptance

Since the anonymity of authors cannot be uniformly assured, it would require a strict experimental design to find out decisively whether papers *of the same scientific quality* are assessed differently by referees according to the status of authors. Both ethics and practicality rule out the draconian experiment in which matched samples of referees, all unknowing, would independently judge the same manuscripts variously ascribed to physicists of different rank, in order to determine the extent of status-linked evaluations. Nor can we approximate the intent of that experimental design by adopting the number of citations to published papers as measures of quality to see whether papers rejected by *The Physical Review* but published elsewhere are of the same quality as those accepted by that journal.

At best, we can bring together data which provide cumulative intimations of the extent to which judgements by editors and referees relate to the status of authors. We begin by examining the successive disposition of manuscripts as this is summarised in the abbreviated flow chart of the refereeing process (Chart I). It turns out that 90 per cent. of the manuscripts submitted by top-ranking physicists have been accepted for publication compared with 86 per cent. for the intermediates and 73 per cent. for the rank-and-file. These stratified rates are the outcome of a continuing process of evaluation (condensed into two phases in the chart). In each phase, the higher the rank of physicists, the better they fare. A larger proportion of their papers are accepted straightaway, a smaller proportion

CHART 1

Evaluation of Manuscripts with Single Authors By Rank of Author (The Physical Review, 1948–1956)



rejected outright and a smaller proportion treated as problematic, requiring further assessments before final decision. Of the manuscripts judged to be problematic, moreover, a larger fraction by the high-ranking physicists ultimately get into print.

Once again, the observed patterns lend themselves to quite different interpretations. They are consistent with the opinion that physicists of the first rank submit better papers on the average and that they are also better able than the others to rehabilitate their problematic papers. But the data can also be interpreted in particularistic rather than universalistic terms. For the observed patterns would also obtain if the editors and referees were especially reluctant to reject papers submitted by the most distinguished men in their field and reluctant also to judge them as needing further evaluation and revision.

Before turning to other evidence bearing on these alternative interpretations, we should consider the patterns of stratified differences within the context of other aspects of the refereeing process which can be reconstructed from the flow chart. We noted earlier that scientific journals with high rates of acceptance seem to prefer the decision-rule: when in doubt, accept. In the case of *The Physical Review*, this preference rule found several expressions. When it came to acceptances, the ratio of immediate decisions to the later, more problematic ones was over 4 to 1 (*i.e.*, 65 per cent. to 15 per cent.) compared with a ratio of only 1·5 to 1 (*i.e.*, 12 per cent. to 8 per cent.) for rejections. Among the problematic papers undergoing further evaluation, moreover, acceptances still preponderate but at only 1·7 times the number of rejections. The decision-rule also seems reflected in the fact that *The Physical Review* mobilised more institutional machinery to reject papers than to accept them: more judges were used on the average for rejected papers than for those ultimately published. And in accord with the general pattern of stratification, the higher the rank of physicist-authors, the fewer the judges involved in accepting their manuscripts.

These patterns, we conjecture, are generally reversed in journals with low acceptance rates where the decision-rule seems to be: when in doubt, reject. In those journals the early decisions presumably exhibit higher rates of rejection than the problematic papers sent on for further refereeing. For in the case of these journals, the presumption seems to be that the manuscripts they receive are not fit to print (at least in the particular journal) since they do in fact reject most manuscripts. Thus, for the journals in the humanities and social sciences, with their typically high rejection rates, it is the potentially acceptable paper which is problematic, while for the journals in physical science, such as *The Physical Review*, with their high acceptance rates, it is the potentially unacceptable paper which is problematic.

Another piece of evidence takes us a certain distance toward gauging the possibility that assessments of manuscripts in *The Physical Review* might have been affected by the standing of their authors. For this purpose, we note again that eminence and authority in science derive largely from the assessed quality of past and not necessarily continuing scientific accomplishments. We note also that in science, as in other institutional spheres, positions of power and authority tend to be occupied by older men. (Indeed, it has sometimes been said with mixed feelings that gerontocracy may even be a good thing in science; it leaves the young productive scientists free to get on with their work and helps to occupy the time of those who are no longer creative.) From these joint patterns, it would seem that if the sheer power and eminence of authors greatly affect refereeing decisions, then the older eminent scientists should have the highest rates of acceptance.

But, at least in physics, the young man's science, this is not what we find. It is not the older scientists whose papers were most often accepted but the younger ones. And these age-graded rates of acceptance hold within each applicable rank in the hierarchy of esteem (Table IV). Both

TABLE IV

Rates of Acceptance of Manuscripts, by Age and Rank of Authors (The Physical Review, 1948–1956)

	Rank of authors							
Age of authors	Higher rank physicists %	No.	Intermediate physicists %	No.	Third rank physicists %	No.	All ranks %	No.
20–29			91	287	83	385	87	672
30–39	96	80	89	519	77	440	85	1039
40–49	95	58	83	236	73	79	83	373
50+	80	87	71	126	50	14	73	227
No information on age							61	423
All ages							80	2734

eminence and youth contribute to the probability of having manuscripts accepted; youth to such a degree that the youngest stratum of physicists in the third rank had as high an acceptance rate as the oldest stratum of eminent ones whose work, we must suppose, was no longer of the same high quality it once was. Dr. Jonathan Cole's studies of citation and reference patterns of physicists lend support to this impression.[52] He finds that older physicists are less apt than younger ones to refer to currently

[52] Cole, Jonathan, *The Social Structure of Science*, unpublished doctoral dissertation, Department of Sociology, Columbia University (1969), Chapter 6.

influential work in their publications, this suggesting that their own work may no longer be as much in the mainstream. Evidently there comes a time in the life-cycle of physicists, even the most distinguished ones, when they can no longer count on having their papers almost invariably accepted in a major refereed journal such as *The Physical Review.* As Max Delbrück once observed, perhaps the chief function of unrefereed *Festschriften* was to provide a decent cemetery for oft-rejected manuscripts.

Relative Status and Differences in Acceptance Rates

Perhaps it is not the status of the author as such but his status relative to that of the referee which systematically influences appraisals of his manuscripts. Such biases in judgement might take various forms, depending on the pattern of relative status.

When referees and authors are status-peers, an hypothesis of *status-solidarity* would have it that referees typically give preferential treatment to manuscripts just as a counter-hypothesis of *status-competition* would have it that under the safeguard of anonymity, referees tend to undercut their rivals by unjustifiably severe judgements.

TABLE V

Referees' Decisions to Accept, by Rank of Authors and Referees (The Physical Review, 1948–1956)

Rank of authors	Rank of Referees: Higher rank physicists %	No.	Intermediate physicists %	No.	Third rank physicists %	No.	Total judgements by referees %	No.
Higher rank physicists	*	18	*	11	*	7	50	36
Intermediate physicists	55	150	62	160	62	84	59	394
Third rank physicists	54	179	61	302	59	172	59	653
All ranks							59	1083

* The number of manuscripts by higher ranking physicists submitted to outside referees, as distinct from editorial judges, was too small for statistical analysis.

When authors outrank referees, an hypothesis of *status-deference* would hold that the referees give preferential treatment to the work of men they respect or hold in awe just as a counter-hypothesis of *status-envy* would have them be more exacting of the work of superiors.

And when referees outrank authors, an hypothesis of *status-patronage* or sponsorship would maintain that referees are unduly kind and undemanding while a counter-hypothesis of *status-subordination* would have them overly-demanding.

Differing in other respects, these six hypotheses are alike in one: they all assume that the relative status of referee and author significantly biases judgements by referees, either in favour of the author or at his expense. More concretely, all assume that the rates of acceptance for each stratum of authors will differ according to the rank of the referees making the judgements.

The data assembled in Table V run counter to all the hypotheses. Referees of each rank accept the same proportion of papers by authors from every stratum. As it happens, the highest ranking referees accept somewhat smaller proportions of papers than their fellow referees but, again, this they do uniformly for authors of every rank. There is, in short, no preferential pattern, as can be shown redundantly but emphatically by condensing the components of Table V into three categories of relative status.

Relative status	Rate of acceptance %	Total judgements by referees No.
Referees outrank authors	58	631
Referees and authors: status-peers	60	350
Authors outrank referees	59	102

All this suggests that referees were applying much the same standards to papers, whatever their source. This is confirmed further by patterns of even-handed evaluation in the case of other relative statuses of referees and authors. Referees affiliated with minor universities, for example, are no more apt to accept papers submitted by authors from universities of similar standing than were referees from the major universities. And whatever the academic rank of referees, it did not affect the rate at which they accepted papers by authors in the various academic ranks. For this journal, at least, the relative status of referee and author had no perceptible influence on patterns of evaluation.

We may conclude that the status-composition of the physicists engaged in refereeing manuscripts for *The Physical Review* during the period is one thing; what the referees did in exercising their authority is quite another.

Functions of the Referee System

As the prime journal in its field, *The Physical Review* can be assumed to apply exacting standards. All the same, the editorial and refereeing process results in as many as four of every five manuscripts being accepted for publication (a fair number of them, after greater or less revision). Does this mean that referees are largely superfluous? Like other observers of the referee system,[53] we think not. Referees, collectively engaged in sorting out good science from bad, serve diverse functions for the various members of their profession: for editors, authors, the referees themselves and the relevant community of scientists.

For the editor(s), referees serve their prime function in the case of papers difficult to assess. At the extremes, as we have noted for *The Physical Review* and a variety of other journals, papers are comparatively easy to appraise and the editor(s) can sort them out. Manuscripts which, by the core standards of the field, provide sound, new, consequential ideas and information, clearly formulated and relevant to the particular journal, can be readily distinguished from their antitheses which are mistaken, redundant, trivial, obscure and irrelevant. But not all manuscripts exhibit these neatly correlated arrays of intellectual virtues or vices. It is the often sizable number of more problematic manuscripts which particularly require examination by experts on their subjects. Apart from this manifest function of furnishing expert judgement, the corps of typically anonymous referees sometimes serves the incidental and not altogether latent function of protecting the highly visible editor from the wrath of disappointed authors.[54] But what is helpful for the editor can of course be injurious to the author. The referee system is now under severe strain on the issue of enlarging the accountability of referees by removing their cloak of anonymity.[55] Since accountability is itself so much a component of the ethos of science, it may be that the practice of maintaining anonymity of referees will increasingly go by the board.

This will surely not be misunderstood to say that the interests of referees and authors are inherently at odds. Referees who conscientiously fulfil their role of course serve major functions for authors. They can and, as we have seen in the case of *The Physical Review*, often do suggest basic revisions for improving papers. They sometimes link up the paper with other work which the author happened not to know; they protect the author from unwittingly publishing duplications of earlier work; and, of

[53] The operation of the authority structure in science and the social structural basis of scientific objectivity have been most fully developed by Michael Polanyi, notably in his *Personal Knowledge* (London: Routledge & Kegan Paul, 1958); the discussion of the referee system is principally in Chapter 6. See also, Ziman, *op. cit.*, pp. 111–117; Storer, *op. cit.*, pp. 112–126; Hagstrom, *op. cit.*, pp. 18–19.

[54] Based on our sample of the archives of *The Physical Review*, a qualitative analysis of the tacit rules involved in rejecting a manuscript has been set out by Raffel, Stanley, "The Acceptance of Rejection", a paper presented at the meetings of the American Sociological Association, 1968.

[55] The pros and cons of referee anonymity are being strenuously debated in various fields; for examples, see the letters by Roy, Rustum, and Henisch, H. K., in *Physics Today*, XXIII (August, 1970), p. 11; Cahnman, Werner J., in *The American Sociologist*, II (May, 1967), pp. 97–98; Steinberg, A. G., in *Science* CXLVIII (23 April, 1965), p. 444.

course, as presumable experts in the subject, they in effect certify the paper as a contribution by recommending its publication. But like other men, referees are not uniformly conscientious in performing their roles. There are, it seems, differences in this respect among fields and among referees of differing kind so that the functions of refereeing for authors and consequently for the discipline are imperfectly realised. This is scarcely the first time that an institution devoted to evaluation confronts the problem of who judges the judges? A sorting and sifting of referees would seem as much a functional requirement of the referee system as the sorting and sifting of papers for publication.

The role of referee also serves functions and creates difficulties for the referees themselves. As experts in the subject, many referees are already informed of developments at its frontier. But especially in fields without efficient networks of informal communication or in rapidly developing fields, referees occasionally get a head start in learning about significant new work. Moreover, as some referees report, the role-induced close scrutiny of manuscripts, in contrast to the often perfunctory scanning of possibly comparable articles already in print, sometimes leads them to perceive potentialities for new lines of inquiry which were neither stated by the author nor previously considered by the referee. This unplanned evocative function of the paper often puts both referee and author under stress. What the referee defines as an instance of his having legitimately and appreciatively borrowed or learned from the manuscript, the author, not surprisingly, may define as an instance of pilfering or downright plundering, as he observes the referee going on to pursue and so, perhaps, to pre-empt the new line of investigation.

The basic and, it would seem, thoroughly rational practice of selecting experts as referees makes for its own stresses in the system. Some scientists have argued that it is particularly the experts who can exploit their fiduciary role to advance their own interests and so are most subject to possible conflict of interest. Here is one among many recent expressions of this view:

> The referee, or more often a member of his group or one of his graduate students, may be working on the very problem he is asked to judge. Of course we must rely upon his personal integrity not to "sit on" the submitted paper, take unfair advantage of the pre-publication information or be unduly critical of the work, thus "buying time" for his own people. He could, in fact, return the paper to the editor citing conflict of interest as his reason for no recommendation, but he cannot avoid the fact of being informed. The point becomes crucial in rapidly developing competitive fields and for publications such as *Physical Review Letters* or *Applied Physics Letters* where priority claims are important.[56]

[56] Prinz, A. G., in *Physics Today*, XXIII (August, 1970), pp. 11–12.

Plainly, the institutionalised concern with intellectual property[57] in science provides the context for these stresses on the referee system. Neither the context nor the stresses are anything new. The concern with intellectual property, which we found to play its distinctive part in the beginnings of the scientific journal, has created difficulties for the developing referee-system right along. Here, for example, is the young T. H. Huxley emphatically expressing his conviction that should "the great authority" on his subject serve as referee, he would never allow Huxley's paper to see print:

> You have no idea of the intrigues that go on in this blessed world of science. Science is, I fear, no purer than any other region of human activity; though it should be. Merit alone is very little good; it must be backed by tact and knowledge of the world to do very much.
>
> For instance, 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 judgement of my "particular friend" . . . that it will not be published. He won't be able to say a word against it, but he will pooh-pooh it to a dead certainty.
>
> You will ask with some wonderment, Why? Because for the last twenty years . . . has been regarded as the great authority on these matters, and has had no one to tread on his heels, until at last, I think, he has come to look upon the Natural World as his special preserve, and "no poachers allowed". So I must manoeuvre a little to get my poor memoir kept out of his hands.[58]

With all its imperfections, old and new, the developing institution of the referee system provides for a warranted faith that what appears in the archives of science can generally be relied upon. As Professor Michael Polanyi in particular has observed,[59] the functional significance of the referee system increases with the growing differentiation of science into arrays and extensive networks of specialities. The more specialised the paper, the fewer there are who can responsibly appraise its worth. But while only a few may be fully competent to assess, many more on the periphery of the subject and in other related fields may find the paper relevant to their work. It is for them that the role of the referee as deputy takes on special importance. When a scientist is working on a problem treated in a published article, he can serve as his own referee. He may, in fact, be better qualified to assess its worth than the official referee who helped usher it into print. It is not so much the fellow-specialist as the

[57] On intellectual property as a significant context for the behaviour of scientists, see Merton, R. K., "Priorities in scientific discovery", *op. cit.*, pp. 635–659; "Singletons and multiples in scientific discovery", *Proceedings*, American Philosophical Society, CV (October, 1961), pp. 470–486; "The ambivalence of scientists", *Bulletin of the Johns Hopkins Hospital*, CXII (1963), pp. 77–97; "Resistance to the systematic study of multiple discoveries in science", *op. cit.*; "Behavior patterns of scientists", published in *American Scientist*, LVII (Spring, 1969), pp. 1–23 and also in *The American Scholar*, XXXVIII (Spring, 1969), pp. 197–225.

[58] Huxley, Leonard, *Life and Letters of Thomas Henry Huxley* (London: Macmillan and Co., 1900), Volume 1, p. 97.

[59] Polanyi, *Personal Knowledge*, p. 163.

others making use of published results in fields tangential to their own who particularly depend upon the referee system.

Scientists also benefit from the refereeing of papers in their own special fields but for somewhat different reasons. They may often be equipped to test for themselves the substance of the papers on which they draw but to do so repeatedly would only subvert their motivation. The fun and excitement in doing science comes largely from working on problems not yet solved. The continuing rather than occasional need to recheck the observations, experimental results and theories advanced by others would seem an excellent means for depleting creative energies. By providing for generally warranted confidence in the research reported in accredited publications, the system of expert referees helps scientists get on with their own imaginative inquiries.

Editors of journals in many fields of learning remark, sometimes with an air of puzzlement, upon the willingness of scientists and scholars to serve in the anonymous and often exacting role of referee. In some fields, such participation is widely diffused. Almost 30 per cent. of a sample of high energy theorists in physics, for example, had engaged in refereeing and editorial work for journals.[60] A sense of reciprocation for benefits received from the referee system probably supports the motivation for serving in the role of referee as it becomes recognised that the maintenance of standards is a collective responsibility. For young scientists and scholars, there may also be the further symbolic reward of having been identified as enough of an expert to serve as a referee.

The very existence of the referee system, Dr. Simon Pasternack has suggested,[61] makes for quality control of scientific communications. In part, this control works by anticipation. Knowing that their papers will be reviewed, authors take care in preparing them before submission, all the more so, perhaps, for papers sent to high-ranking journals with a reputation for thorough refereeing. This would also make for the scientists' internalisation of high standards. Furthermore, Pasternack points out, even the " scientific journals that have little or no refereeing or editing . . . exist within a framework of the edited journals, which set the pattern and the standard ". The referee system may thus be raising standards adopted by journals ostensibly outside that system.

These observations on the functions of the referee system do not at all imply the contrary-to-fact assumption that it works with unfailing effectiveness. Errors of judgment of course occur. But the system of monitoring scientific work before it enters into the archives of science means that

[60] Libbey, Miles, A. and Zaltman, Gerald, *The Role and Distribution of Written Informal Communications in Theoretical High Energy Physics* (New York: American Institute of Physics, 25 August, 1967), p. 49.

[61] Pasternack, Simon, " Is Journal Publication Obsolescent?", *Physics Today*, XIX (May, 1966), pp. 38–43, at p. 40 and p. 42. Dr. Pasternack has been editor of *The Physical Review* since 1956 (which will be remembered as the end of the nine-year-period examined in this paper) and on its staff since 1951.

much of the time scientists can build upon the work of others with a degree of warranted confidence. It is in this sense that the structure of authority in science, in which the referee system occupies a central place, provides an institutional basis for the comparative reliability and cumulation of knowledge.[62]

[62] Several articles bearing on the subject of this paper have appeared since it was completed. Most directly relevant is the work of Richard Whitley on the operation of science journals. His study of an interdisciplinary journal and one in social science found that in both cases, editorial decisions on manuscripts were unrelated to the rank and institutional affiliation of contributors. (Whitley, Richard D., "The Operation of Science Journals: Two Case Studies in British Social Science", *Sociological Review*, New Series, XVIII (July, 1970), pp. 241–258.) In his study of 32 journals in social science, Whitley found that the older journals and those devoted to fundamental rather than applied science had tended, more than the others, to develop specific criteria for judging manuscripts. This is consistent with the hypothesis advanced in the present paper that differences among the disciplines in rates of rejection are associated with the extent of consenus on the criteria of adequate scholarship in the various disciplines. (Whitley, Richard D., "The Formal Communication System of Science: A Study of the Organisation of British Social Science Journals", *The Sociological Review: Monograph No. 16*, (September, 1970), pp. 163–179.) Whitley also found that the extent of control by professional associations over the communication system in social science was significantly related to the use of formal procedures for evaluating manuscripts. (*Ibid.*, p. 175).

Two studies based on surveys of journals in clinical, personality and educational psychology report substantial agreement among the editors of these journals on the criteria for judging the acceptability of manuscripts. Since these studies are not based on investigation of the archives, however, they cannot determine the possibility of socially patterned differences in the application of these criteria. (Wolff, Wirt M., "A Study of Criteria for Journal Manuscripts", *American Psychologist*, XXV (July, 1970), pp. 636–639; Frantz, T. T., "Criteria for Publishable Manuscripts", *Personnel and Guidance Journal*, XLVII (1968), pp. 384–386.)

Bearing directly upon the findings on differences in rejection rates by journals in the humanities and sciences reported in this paper is a survey of the importance assigned to various criteria for good scientific writing by members of 16 departments of social and natural science at a major university. The results indicate that "the harder natural sciences stress precise mathematical and technical criteria, whereas the softer social sciences emphasise less defined logico-theoretical standards". (Chase, Janet M., "Normative Criteria for Scientific Publication", *American Sociologist*, V (August, 1970), pp. 262–265.) We owe the information in this footnote to Mr. Aron Halberstam.