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THE SCIENTIFIC REFEREE SYSTEM

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Abstract

There has been very little written about the scientific referee system but a lot has been implied. It seems to be widely believed that the system works well, even though there are cases of disparate judgement. These however are usually explained away in an ad hoc fashion. We find that novelty is characteristically resisted by scientists and suggest reasons for this.

1. INTRODUCTION

The referee system began in the seventeenth century in England in the form of a collator who saw papers through the press. Because of the low quality of many manuscripts, this system was soon replaced by editing and refereeing. The practice of evaluating the substance of manuscripts soon developed. Referees were chosen on the basis of their expertise. "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 into print."[1] This is the current practice. Each journal has an editor who receives and judges papers or passes them on to reviewers for judgement.

Because short articles in journals have become the major publication method in science, referees and, to a lesser extent, editors have become the "lynchpin" about which the system pivots. This being so, it is not difficult to see the importance of editors and referees to the process of scientific communication and scientific advancement. The question that we raise here — and one raised by others — is how well does the system work?

2. THE RECEIVED VIEW

The majority opinion, which we will call the "received view", is that although the referee system does not work unfailingly, it approximates the ideal. This view is implicit or explicit in much of the writing of those sociologists who make the institution of science their speciality.[2,3] The assumption underlying this view is that scientists are largely aparadigmatic thinkers, testing ideas against nature. The individuals who act as referees are believed to be objective, disinterested, sceptical, sympathetic, open, tentative and hospitable to change.[4] Because of this and the fact that they are "experts", referees should be nearly infallible judges of work that falls within their areas of specialization. Papers published in "reputable" journals therefore

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"bear the *imprimatur* of scientific authenticity" because they do not merely represent the opinions of their authors but also those of the editor and the referees.[5] Consequently, the practice "of monitoring scientific work before it enters into the archives of science means that much of the time scientists can build on the work of others with a degree of warranted confidence"[6].

According to the received view, papers submitted for publication fall along a simple continuum. At one end are the very bad ones, written presumably by dullards; at the other are a few excellent papers, the "cognitive products" of the elite. In between are the majority, "hod work", as Darwin[7] called it. The editors' and referees' task is simply to assign each paper to its position and decide on a cut-off point.

Adherents to the received view, however, do not overlook the fact that the system does not work with unfailing effectiveness, but — and this is the point — their treatment tends to dismiss incompetent judgements as *ad hoc* events. Consequently, the received view does not address itself to the possibility that intellectual bias is an important factor in the assessment and acceptance of scientific work.

Traditionally and typically, supporters of the received view have painted a reassuring picture, one mirroring the Panglossian mode in which almost everything that happens, recurrent or otherwise, is seen as basically for the good. Anomalies are distorted into isolated and unusual events or are sometimes even construed as being of positive value, that is, when anomalies are admitted, some institutional necessity is used to justify them.[8]

Therefore, it would seem that the received view would predict that the rejection of important, novel work should be infrequent. Certainly important work should not be repeatedly rejected. Figure 1 (below) approximates what we take to be the received view. Scientific progress in this view is easily understood.

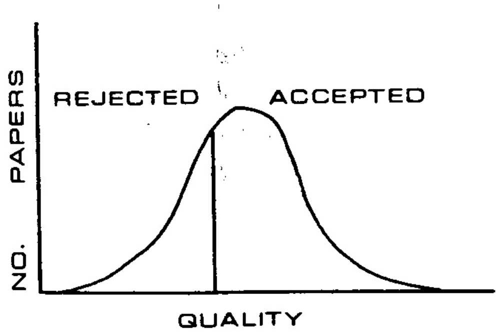

Figure 1. The Received View.

3. THE MULTI-PARADIGM MODEL

The necessity for a different interpretation than is provided by the received view begins with the realization that the frequency with which good papers are rejected and the extent to which scientists resist new ideas are not accounted for by the received view.[9] Inconsistencies between what occurs and what is predicted to occur are too major and too frequent to be explained away as occasional mistakes. In fact, what the "mistakes" suggest is that they are not mistakes at all, for as Taton[10] has shown, "the number of revolutionary discoveries which came into their own, only after hard battles, is legion."

We therefore reject the received view and propose another interpretation of these events.[11] According to this interpretation, scientists are not unbiased, objective, sceptical, disinterested, sympathetic, open, tentative or hospitable to change, but the opposite. Scientific judgement is in terms of prevailing opinion. It is paradigmatic.[12] Put differently, scientists are "encapsulated"[13], or as Hoyle[14] has said, "straight jacketed". Kantor[15] puts it simply, "established doctrine in science is more powerful than factual evidence".

Although Kuhn is responsible for popularizing the notion of paradigmatic science, this conceptualization has been around for a long time and has been variously expressed, for example by Kantor[16] and by Koestler[17]. Koestler[18] perhaps has captured the idea behind the concept of paradigmatic commitment as well as any. He sees typical paradigmatic behaviour as involving a "cognitive matrix with a distorted logic, the distortion being caused by some central axiom, postulate or dogma, to which the subject is . . . committed, and from which the rules of processing the data are derived". He[19] continues, "The amount of distortion involved in the processing is a matter of degrees . . . it ranges from the scientist's involuntary inclination to juggle with data as a mild form of self-deception, motivated by his commitment to a theory, to the delusional belief-systems of clinical paranoia . . . But," he concludes, "to undo a mental habit sanctified by dogma or tradition, one has to overcome immensely powerful intellectual and emotional obstacles."[20] Thus the scientist is committed to a world view from which reality is constructed, and things are fitted or "twisted" as Kantor[21] has said into conforming with that view.

Scientific work in these terms can be depicted as a three-dimensional field, as in Figure 2 (below). The large hill in the foreground represents the major paradigm of a discipline, for example, Functionalism in sociology. Most scientists accept the major paradigm, and when judging the work of others, they do so on the basis of its approximation to the standards of the paradigm. To the rear of the field are several small hills, which symbolize small coteries pioneering new approaches. Members of such coteries are working under assumptions different from those of the majority. These assumptions are not understood by adherents to the major paradigm, yet members of the coteries understand the major paradigm, for they work out from it — in fact, it is usually this they are rejecting.

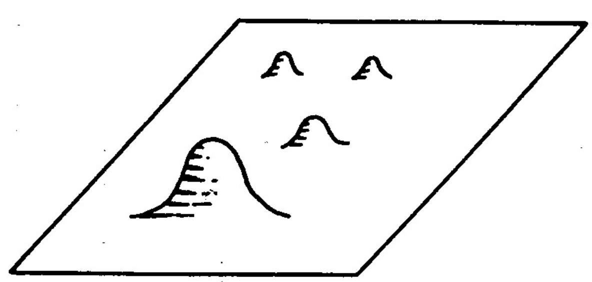

Figure 2. Multi-paradigm model

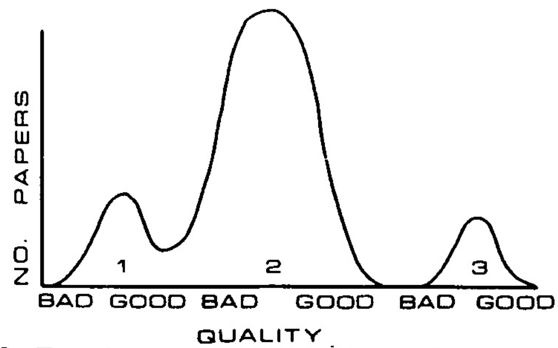

Figure 3. Two-dimensional view of the multi-paradigm model.

Visualized two-dimensionally, scientific papers do not fall into a single bell-shaped curve but rather into a series of curves in which there are good and bad papers (see Figure 3 above). Most referees can only judge papers that fall into one or two curves. But they attempt to judge all papers on the basis of their own standards. For example, if the referee is a mentalistic psychologist, to suggest that the current view of the nature and function of the central nervous system is traceable to the Patristics and is completely ascientific is to kill the paper before it is even born. Again, to suggest that Newton was badly confused about the part played by light in vision and that instead of seeing light, we see things by the medium of light, is to ask for trouble, and so on.(22)

What is being said by a heretic seems ridiculous to adherents of the accepted paradigm. To them, papers that are novel often appear to be unpolished, simple-minded, improperly referenced, misguided, poorly conceived, amateurish, or just plain wrong, depending on the degree to which they deviate from the paradigm. Their authors are considered, as Van Valen and Pitelka(23) put it, to be "presumptuous". But the reason for this is that the new knowledge or point of view is not part of the paradigm or goes directly counter to what is thought to be "fact". Therefore, the innovator is judged wrong or incompetent by the fact of showing glaring deviations from what is accepted or acceptable.

Storer(24) captures the essence of the problem when he says, "A contribution must be an understandable extension of knowledge, and if it fails to meet this test, either because others simply cannot understand its relation to current knowledge or because it seems to fly in the face of accepted standards of reasoning, it will be rejected and its author's credentials as a scientist will become suspect. This means that a scientist who is so far ahead of his colleagues that they cannot understand how he arrived at his conclusions or why they are important will be treated in the same way that a hollow-earth theorist is treated."

Because journals are usually controlled by adherents to the major paradigm, the consequences of submitting non-paradigmatic work are predictable.

4. CONCLUSION

While the referee system came into being in order to separate the trivial and incompetent from the competent at a time when it may have been possible and perhaps necessary to do so, the system also immediately became an obstacle to creativity and innovation, for although it does sort, it does so not only on a good-bad basis but also on a paradigmatic-nonparadigmatic basis, and herein lies its great and inherent weakness.

Whatever positive aims were envisaged by instituting the referee system and whatever positive goals it achieves, the seeds of dysfunction were sown at its inception, for given the paradigmatic nature of science and the encapsulation of scientists, the natural outcome can only be resistance to innovation and rejection of novelty. The weak links in the system are the referees themselves, for they like all scientists have repeatedly shown themselves to be unable "at any given time to distinguish between an idea that is entirely wrong and an idea that may be received as brilliant at some later date"(25).

In conclusion, let it be said that even though it would be hard to find a scientist who is not aware that some good papers are rejected and some novelty is resisted, it would also be hard to find a scientist who altered his own behaviour because of this knowledge. It is not surprising that individuals who have not experienced resistance accept the received view. Not having had work rejected for paradigmatic reasons, it is easy for them to say that this either does not occur or that it represents an unusual event in science. The Nageli-Mendel incident illustrates the problem perfectly. Here is the "Father of Genetics" trying patiently to explain what he has found to *the* expert on plant hybridization, only to be rebuffed and sent back to his garden. If supporters of the received view can only imagine this incident multiplied a thousand times and momentarily identify with Mendel, perhaps they can grasp the magnitude of resistance and the utter frustration that is involved in attempting to explain something that is novel to someone who is simply incapable of understanding — but someone who theoretically should be capable of understanding. Neither Nageli nor anyone else had any comprehension of what Mendel had discovered, no matter how Mendel explained himself, until quite independently three individuals rediscovered genetics. Others had to repeat Mendel's work, that is, *they had to experience what he had done*, before they could understand what he had said. All scientists should realize that when they review others' work, they are potential Nagelis. But characteristically scientists persist in failing to learn from history and so failing are condemned to repeat the past. Surely the "expert" must realize that if the history of science teaches nothing more than is contained in the Nageli-Mendel incident, it behooves him to admit to the possibility that he himself may be a little short of omniscient. Perhaps with this first admission, he can go on to teach himself to say, as Galileo taught himself to say, "I do not know".

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Dear Mr. Catt

Thank you for your letter and reprint. I'm glad that the referee paper was of some interest. The best work on the subject is Lindsey (see note 11) who summarizes most work up to 1978 or thereabouts. I don't know of any major work on this that has been done since then although one can find dissenting voices usually in the form of letters to editors in such journals as the New Scientist: eg. Physics Today, 1979 (April), Vol 32, 14-15, New Scientist, 16 July 1981, 178, but these amount to very little. I would be interested in your "The rise and fall...." paper.

Michael

P.S. Look particularly at the Mahoney references in the Lindsey book.