OPINION

A MODEST PROPOSAL FOR *GLASNOST* IN THE PEER REVIEW PROCESS

To my mind, one of the most important ideas to emerge in the social, political and economic history of mankind is that of **the open society**—where ideas (political and social) and products (goods and services in economics) compete in an open and free market. Each constituent in that commonwealth takes his own individual decision, exercising his own free will and his capacity for reason, and the total sum of all such decisions of the entire commonwealth of people will determine their form of government, their codes of social practice and the financial success or ruin of its small and large business and industrial enterprises.

It is this sort of openness that is still missing in our world of scientific publishing. Ideally, if the concepts of open and fair competition, the free entry and exit of ideas etc. were to be applied to our scientific community, then it would be necessary to let everyone publish and circulate his ideas in a free market of scientific enterprise, and allow the individual decisions of the scientific community add up to a vote of acceptance of the idea concerned. This is of course unreasonable and virtually impracticable—the economics of scientific publishing, strained as they are now, can never permit it. Hence the need for careful screening—the procedure we have all come to accept as the peer review process. We cannot do without it and have come to accept it as a fundamental condition, like motherhood, that one cannot argue against.

It is not really the concept of peer review that I find indefensible. It is the closed and secretive way in which it is currently practised that I find objectionable. Let us first examine how the peer review system normally works in most journals. When a manuscript reaches an editor's desk, he or an approved associate editor examines it to see if the contents are of interest to the invisible college that the journal serves, and then identifies two or three referees who can be expected to give an educated evaluation of the scientific merit of the work. When these reports come in (sometimes only one comes in, sometimes the two that come in are divided in opinion), the editor has to take an impartial decision based on his reading of these reviews to accept or reject the paper or accept after asking for revision. Throughout, the names of the reviewers remain unknown to the authors.

It is my thesis, after several unhappy experiences, that this lack of openness can lead to considerable misuse. Very often, the peers who do the review are the ones who have axioms to grind! They are the ones who have an **orthodoxy** to protect and are very quick to suppress any idea they consider **heresy**. If Copernicus or Galileo had to submit their ideas to peer review first, their efforts would never have seen the light of day. The anonymity that is now given to all peer reviewers is a licence to practice precisely this kind of scientific tyranny, giving them a mask behind which they can scrutinize and savage new ideas and stifle good and original research simply because it does not conform to the current fashions of accepted paradigms. Under such conditions, the free and fair publication of ideas leading to paradigm shift (to use a phrase made famous by T. S. Kuhn) can rarely or never take place at all. It will therefore be insurmountably difficult for **revolutionary science** to proceed at all whereas for **marginal science**, anonymous peer review is the ideal breeding ground.

The time has therefore come for some or all journals to examine this state of affairs more closely and take remedial action. Peer review should be conducted openly. The author(s) of a manuscript should not only know why their work is being turned down but also the identity of the person, or the school of thought that he subscribes to, that is turning down the manuscript. Knowing this helps to understand which school of orthodoxy has been challenged and whether there is a chance for a free trial of the ideas at all from such quarters. Stripped of this anonymity, many peer reviewers will be more careful and less likely to act out of plain malice or bias.

To reinforce my arguments, which have been impersonal so far, let me now count some of my own curious experiences in over 15 years of scientific research and after having communicated more than 60 papers to leading scientific journals. Most of my work, as would understandably be the case with most research workers, is work that can be categorized as marginal additions—very much in the nature of the marginal science that T. S. Kuhn has described. These are the papers I have found most easy to publish. The reviews are always kind and generous as the ideas described in such papers conform to accepted scientific paradigms and the peer reviewers are usually the same ones who have vested interests in the continued acceptance of the same paradigms. I have had considerably more difficulties with my better papers, especially where they seek to point out deficiencies in existing paradigms, and most, sometimes insurmountable difficulty with my most original ideas, especially where they directly confront and challenge the current orthodoxy and try to replace it with a new. It is instructive to scrutinize this aspect most carefully, drawing again from some of my own experiences.

I work in an area called the finite element method, a computational approach for structural analysis. I did, in 1983 to 1984, some studies on what is called the curved beam element. This is a model used to describe in numerical terms, the structural behaviour of an engineering structure like a curved arch or a circular ring. These elements were known for their notorious behaviour, if formulated strictly according to the then accepted norms of finite element practice. For about 15 years, it was believed that these finite element models did not account for what was called the *strain-free rigid-body motion of a curved element.* One could count about 30 to 60 papers (depending on what one chooses to include) representing this orthodoxy. I decided that the problem was not this at all, but something I called the need for *consistency of the membrane strain field.* Using these ideas, I believed then that I had succeeded in producing the most accurate curved beam available to date and I was also able to provide accurate error models (in a verifiable or falsifiable sense, borrowing Karl Popper's phrase) of how the inconsistent element behaved, justifying that the consistency paradigm had indeed a falsifiable, scientific basis. I communicated this paper to a journal. The reviews I received were contradictory. One review declared that '...the approach... appears to be very significant... assuming that this will be accepted in future as a fundamental contribution to the analysis of curved elements...'. The second review did not hesitate to dismiss the work as totally erroneous, restating the belief that it is impossible to construct the element without providing for the strain-free rigid body motion condition, and proceeded to declare that all the results given would be unpredictable and unreliable. Clearly, this peer review was protecting the territory of the old order—for the concept of consistency was heresy in the context of the old paradigm. Fortunately, in this instance, the editor was fair enough to give me the benefit of the doubt. However, I have learned that in many cases, the editors are not so bold, preferring not to transcend their usual role of a post-box.

A second incident is also instructive; both peer reviewers were agreed that the paper I wrote was not worth publishing but for diametrically opposite considerations. One reviewer declared that the work was wrong and inaccurate and was not worth considering. The other very helpfully commented that my work was obvious and a rehash of very well known ideas and that he himself had been teaching it to his students for several years. This led to a very interesting dilemma. If both reviewers had agreed that my work was false, that would have been grounds enough for outright rejection. Again, if both had agreed that it was obvious and well-known, that would have also served as grounds for summary rejection. But curiously, both recommended rejection—one, because he found my work to be new but not true and the other, because he found my work to be true but not new. Had the reviews been signed, and their identities mandatorily revealed to me, the referees would have been more careful about dismissing a piece of work in such a ridiculous way.

There are many more such incidents. I have met many who have had very similar experiences. I think, given the imperfection of the world we live in, it is understandable that this sort of situation is bound to arise. In the competitive scientific community, survival and material success is closely linked to the survival of ideas one promotes. The temptation to protect one's ideas using deliberate bias in peer review is made feasible because the perpetrator can safely hide behind the mask of anonymity that the journals provide him. I am convinced that peer review will be more effective if the reviews are all signed by the reviewers so that the defenders of orthodoxy can always be seen by the proposers of heresy. My arguments here will be incomplete if I do not include the following incident. In all my 15 years of work, I know of one reviewer who has always insisted on signing his name on the review. I have always admired that singular and courageous gentleman and I must declare that I have never had a better or more useful review from any one of the other reviewers so far.

Therefore, to conclude this modest proposal, let me make this recommendation to *Current Science*.

Let it be the first (?) journal in the world to adopt as its professed policy, the principle of an *open review system*. It need keep in its stable of reviewers, only those who understand and accept this principle—that their reviews will be signed and that their identities will be made known to the authors submitting the paper. It should also make it clear to authors that it will follow this procedure. Alternatively, it can allow authors to opt for a signed review or an unsigned review and these wishes can then be communicated to the reviewers and the process completed accordingly.

Openness, or *glasnost*, as the Russians have now come to know it, is a very noble principle—at the heart of it is the principle that justice must not only be done but must be seen to be done. I am sure many of your readers will like to comment on this suggestion. They may point out many obvious shortcomings even in an open system or may be because it is open. We will all benefit by an open debate on this in the pages of your journal.

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ANNOUNCEMENTS

New Developments in the Understanding and Treatment of Schizophrenia

Date: 6 December 1989
Place: Royal College of Physicians, London

Neuropharmacology relating to schizophrenia has progressed rapidly in recent years. For the first time since the development of neuroleptics in the 1950s, an antipsychotic with improved efficacy is available, and ongoing research promises to yield a range of new products with fewer side-effects and additional activity. This conference will bring together experts in the field to assess the progress that has been made to date, and to discuss what prospects the future holds.

New Pharmacological Approaches to Depression and Anxiety

Date: 7–8 December 1989
Place: Royal College of Physicians, London

Depression and anxiety are the two most common mental disorders, and while drug treatment for both conditions is available, it is not always satisfactory. This conference aims to present an overview of research into both conditions, outlining difficulties encountered and the limitations of current therapy. It will then examine in detail the new approaches to drug treatment now under investigation, outlining the potential advantages and reporting the latest results available for new antidepressant and anxiolytic agents.

Drugs Affecting Calcium Ions—Their Role in Modern Medicine

Date: 13–14 December 1989
Place: Royal College of Physicians, London

Calcium is a fairly ubiquitous ion found in cells and tissues that is thought to be involved in a wide range of physiological processes. It is therefore not surprising that abnormalities in the calcium system have been implicated in the pathophysiology of various disease states including cardiovascular, neurological and inflammatory. This meeting will provide a concise overview of the current states of calcium in the physiological control of cell activation and the current use of drugs affecting calcium in the treatment of various disease states. Furthermore, the meeting will review new areas of interest where drugs affecting calcium may be implicated.

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