

# CURRENT SCIENCE

Volume 59 Number 10

25 May 1990

## Peer review

'A modest proposal for *glasnost* in the peer review process'<sup>1</sup>, the author's central plea is that the anonymous peer-review process be replaced by an open review system. But the latter is not without deficiencies. The peer-review system, as it exists, is an honour system. Reviewers are expected to do their job as an act of community service without reward or remuneration promptly and objectively. They function under an unwritten moral code of conduct<sup>2</sup> which, by and large, has served well. It is not foolproof but a better alternative is hard to find.

The confidential and anonymous review process was evolved to preserve cordial human relations and to make rejection of papers humane, while concurrently encouraging the reviewer to express his scientific opinion objectively and honestly without fear of retribution, specially from powerful and influential authors. The system is, no doubt, open to abuse—a reviewer may delay the publication of a paper in order to get his out first, may be hostile to a different school of thought, may be plain ignorant or out of touch with the field but may not admit it, and so on. I have also witnessed the nobler aspects of the review process being practised—reviewers with conflicting interests promptly returning manuscripts, suggesting an abler colleague as a more suitable reviewer, making brilliant suggestions (anonymously), and so on, but these are seldom talked about because one expects such conduct.

The scientific community realizes the possible abuses of the peer-review system and even has a few built-in safeguards—multiple referees, an adjudicating editor, a rebuttal procedure, and, in some journals, the opportunity to express dissenting views on published papers. The journals of AIAA (American Institute of Aeronautics and Astronautics) sometimes publish papers not recommended by a referee and allow the referee to publish his dissenting views

alongside the paper! Finally, the author is always free to seek publication of his work in other journals with 'less-prejudiced' reviewers. He can even start his own journal, publish and distribute his work privately, etc.

Most modern states foster non-military funded science as an autonomous activity of self-directed intellectual enquiry. Over centuries science has won for itself certain social concessions—freedom from politics, religion, hierarchy, and even national boundaries. I would like to believe that this is so because the scientific community is viewed to be objectively self-policing by the world-at-large.

An open review system would perhaps make the reviewer more careful about his remarks. It would also open channels for politicking and sycophancy by allowing reviewers to gain favour with influential authors and exchange favours with friends and colleagues. These apart, imagine the fate and shattered morale of a young scientist when he finds that his paper was rejected by the world's leading authority, indeed his hero, on the subject. Surely, on compassionate grounds, he deserves an anonymous review? On the other hand, how does one coax eminent scientists refraining from doing any peer-review work because they would rather stay away from the resulting controversies, accusations, lost friendships, etc.? I suspect that an open review system would give rise to frequent (and unfair) accusations of reviewers stealing ideas from rejected authors.

Resistance to new ideas is a well-entrenched human trait. Scientists of all calibre, including Gauss and Galileo, have succumbed to tyrannical social pressures and have held their counsel on new ideas. No doubt, familiarity breeds acceptance, but resistance too breeds a perseverance among scientists committed to science and forces new ideas to be honed to perfection till their logical acceptance becomes inevitable. I think that, for this reason alone, resistance, to an extent, is welcome. In the realms of theoretical physics, which is

ruled by nothing but ideas, the community eagerly awaits new ideas. There is no denying that they did accept and give credence to non-Euclidean geometry, the theory of relativity, quantum mechanics, and so on, all revolutionary ideas of the highest order.

Resistance to new ideas is not so much in the theoretical and analytical branches of science, where an author's work can be independently verified against a stated logical framework, as it is in the computational and experimental branches, where fraud can and does go undetected because verification is either difficult or near-impossible (getting authors to part with their computer codes, raw data, calibration charts, etc. is almost impossible).

Scientific ideas take shape in erratic ways and personalities do dominate ideas. To quote H. Alfvén<sup>3</sup>, 'Fermi had such an authority that if he said *of course* today, every physicist said *of course* tomorrow.' What a scientist thinks today is largely a product of the thinking and the work of others who have patiently developed concepts, acquired and analysed data, and published their findings. At this stage of human evolution, ideas are in a state of flux. Therefore schools of thought and differences among them are natural. Ernst Mach refused, a hundred years ago, to believe in atoms<sup>3</sup>. Hannes Alfvén (Nobel Prize in physics, 1970), who helped originate the theory of cosmic rays that is now generally accepted, is now rejected by him<sup>3</sup>. Albert Einstein could not accept quantum mechanics (his famous 'God does not play dice'.) while the Nobel Committee steered clear of his theory of relativity and Mach did not believe in the theory either. Till a few years ago it appeared that the future, in principle, could be predicted with deterministic precision. Now the notion of randomness and unpredictability is beginning to look like a unifying principle. Ideas which appear ingenious today may appear silly tomorrow, and vice versa. And then there are changing fashions in ideas. The strength of science

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lies in its nagging and persistent search for logical structures in spite of human biases. So a logically structured idea rejected today has every chance of being recognized tomorrow. One can be thankful that science tries not to be dogmatic.

The real problem of manuscript evaluation is not with the peer-review system but with the eroding ethical standards in science. Today, pursuit of science is rarely an individual effort. It has become expensive. It needs government and hence political (and now increasingly, industrial) support for funds. Modern management of science too has produced a problem. It focuses on projects rather than on knowledge. Scientific research has become a means of livelihood. Authorship is bartered for career growth. Such a situation fits ill with the ideal, where, eventually, scientists are answerable not to their administrative bosses but to their scientific peers.

The scientific community needs, really, to reassert its ethical standards. Its leaders must set examples. Authors need to set standards for themselves and guard them when they become reviewers. Then I think the peer-review system will take care of itself honourably and confidentially.

1. Prathap, G., *Current Science*, 1989, 58, 1114.
2. See, for example: Ethical standards for publication of aeronautics and astronautics research, *AIAA Journal*, 1990, 28, 2.
3. Alfvén, H., *American Scientist*, 1988, 76, 249.

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Prathap<sup>1</sup> strongly argues in favour of open peer review of manuscripts sent for publication in scientific and technical

journals. Gowrishankar<sup>2</sup>, in his comments on Prathap's article, presents the opposite point of view and gives 'weighty counter-arguments against open review and in favour of the existing system'. One can easily mistake or misunderstand Gowrishankar's arguments as supporting orthodoxy and *status quo*.

The basic issue involved requires some impartial or objective consideration. A reviewer acting out of plain malice or bias, as Prathap mentions, may exist. For example, this writer has the sad experience of having his paper sent to a prestigious technical journal abroad rejected by the editor because of the unfavourable review given by a 'peer' reviewer. It turns out that the reviewer was a person whose earlier work on the same subject was found to be in serious error, even on certain fundamental points. This writer in his paper pointed out these errors and corrected them and got results radically different from that of the reviewer. Clearly, this antagonized or infuriated the reviewer, who was extra-critical and recommended strongly that the paper be rejected. The editor, who was not a specialist in the field, was helpless. Such things do happen. After all, how many are totally free of malice, bias or professional jealousy, and who likes to be and had errors even on certain fundamental points?

Are we not, to varying degrees, prisoners of traditional, conventional and orthodox thinking? Even the great, incomparable Einstein, one recalls, held on for a long time to his firm, orthodox belief in a deterministic world-view and would not accept a probabilistic world-view, even after quantum mechanics took firm root.

Secondly, who are really the peers of a scientist working, say, in a broad field like structural/fluid mechanics or solid state physics. Let us keep in mind that these are days when one goes in for specialization in narrower and narrower areas. An editor cannot be expected to be knowledgeable

in all areas. In selecting one or more reviewers for a paper sent for publication, the editor usually refers to a panel of reviewers that he might have compiled. In some special cases, this may not help. He may then seek the help of a scientist working in the same field or a closely related field. Or he may critically examine the list of references given at the end of the paper and select reviewers from this list. This is somewhat similar to the procedure usually adopted for nominating examiners for an MS or a Ph D thesis. In any case, the editor's task is quite often unenviable.

To deal with the problem of lack of two or even three competent reviewers. Quite often, this practice results in delays. There are also cases when a reviewer gets one of his juniors to review the paper and merely puts his signature to the review. There are even cases where the author comes to know who the reviewer is and tries to influence the reviewer. This unethical practice has been seen in the case of even thesis examiners at the MS and Ph D levels! In all these matters, there is no foolproof system. Conscience alone is one's friend, philosopher and guide.

To conclude, the worth and efficacy of the peer-review system depends very much upon the choice/selection of the peers—the job of the editor—and how sincerely, seriously and objectively the review is done. No system—the peer-review system included—that the human mind devises can be infallible or one hundred per cent foolproof. Isn't man's imperfection reflected in everything he devises or creates, including scientific theories?

1. Prathap, G., *Current Science*, 1989, 58, 1114.
2. Gowrishankar, J., *Current Science*, 1990, 59, 7.

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