



Peer review: proposals and papers are in different categories

One of the greatest hindrances to research at the beginning of the 21st century is that it is practically impossible to do anything that one cannot do oneself with nothing more than pencil and paper (and possibly a personal computer) without convincing a committee that it is worth doing. The mysterious process of discovery that has worked so well in the past and delivered countless results pushing knowledge forward to an almost unimaginable (and unpredictable) degree and resulted in countless things of great use to mankind has largely been replaced by the straitjacket of writing a proposal in a very stilted and constrained fashion according to the rigid prescriptions of research councils and submitting them to a panel of reviewers. It is almost axiomatic that committees do not have imagination, hence what they favour is the pedestrian and incremental. Furthermore they are themselves in thrall to accountants, hence the anticipated results from the proposed research must be assessable by ticking boxes corresponding to coarse and well defined categories. Finally, there is the doubt about the necessity of the proposed work arising from the fact that if the research plan can be specified in the minute detail required by the research councils, surely the outcomes can be predicted sufficiently well from existing knowledge and there is no real need to undertake the research at all.

Lest it be thought that commercial companies from the high technology sector still allow their scientists the freedom to explore, industrial research has been badly affected by the trend for firms to concentrate on what they call their core business. Anything open ended and exploratory (which in the past, when supported by companies, has often yielded extraordinarily good returns in the medium term on the investment) cannot be contemplated in this context, and again we are left with pedestrian and incremental improvements that look backwards rather than forwards.

It is therefore highly inappropriate for research proposals to be peer-reviewed at all. O.G. Selfridge's proposal for randomly selecting which work should be supported (bearing in mind that in principle the scientists proposing work have been themselves carefully selected through years of stringent examinations) has at the very least the merit of not requiring a ponderous and expensive bureaucracy to administer it. The present mode of proposal selection also involves the honorary participation of a huge

number of scientists, who must spend a considerable time on reviewing proposals. Furthermore, the process all too often brings to the fore human failings of partiality. Since a good proposal is by definition concerned with an unknown future, reviews have to be largely based on unqualified opinions rather than an objective, evidence-based appraisal.

In strange contrast, where peer review is very necessary, namely as part of the publication process of disseminating scientific results as papers in journals, it seems to be carried out ever more cursorily. This is partly due to the seemingly uncontrollably growing volume of papers submitted to journals. Were they all to be reviewed with appropriate thoroughness, no one would have time to do any research work any more (although presumably this would then lead to the flood of submissions drying up, hence the overall process should be self-regulating, but perhaps over a rather long timescale). It is also due to the growing popularity of e-print archives, online-only journals and personal websites, contributions to which are either not peer-reviewed at all, or only lightly. Furthermore, the online-only journals are typically financed by the authors paying for posting papers, in other words they are accepted on the same basis as advertisements in newspapers and magazines.

Human psychology is powerfully at work here. A web resource can never have the stability of a printed paper, and instinctively one will devote less care to something that can be instantly updated at any time. To be sure, ever since scientific journals in the modern sense began, the peer review of submitted manuscripts has also had its critics,¹ fuelled by notorious examples of significant ground-breaking work being rejected (e.g., Maiman's first experimental demonstration of the laser), and even more notorious examples of fraudulent work being accepted (e.g., the papers of Schön and Batlogg). But usually the significant work is swiftly published elsewhere (and nowadays there are so many journals that this could scarcely be a problem, although in the 19th century, for example Mayer's work on heat was harshly refereed and never resubmitted, thereby probably delaying the development of the field by at least a decade), and the fraudulent work is usually swiftly discovered. Hence, it seems that the criticisms have nowhere near sufficient weight to discredit the process as a whole. The ultimate responsibility rests with authors, who should not submit work to journals until it is worthy of worldwide dissemination and archival storage!

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¹ This matter has been explored in considerable detail by scientometricians. See, for example, J.S. Armstrong, Peer review for journals: evidence on quality control, fairness, and innovation. *Science and Engineering Ethics* 3 (1997) 63–84.