

SO WHERE SHOULD WE PUBLISH?*

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In a recent contribution to *Science and Public Affairs* (Lachmann and Rowlinson, 1997), the Biological and Physical Secretaries of the Royal Society suggest that the increasing use of bibliometric analysis based on impact factors and citation counts is corrupting the peer review process. They suggest that there is a growing preoccupation with where a paper is published, rather than what it says. Bibliometric measures tend to bias publication towards US journals, where the scientific community is largest, which in turn creates problems for those journals due to overload. Lachmann and Rowlinson reject the practice within the Royal Society's own peer review process and deplore the tendency of the Research Assessment Exercise to encourage it.

Is this a problem within psychology? I think there is no doubt that it is. My attention was recently drawn to a circular in one psychology department that was concerned to encourage publication in 'high status' journals, which, it was suggested, means American Psychological Association journals first, Psychonomic Society and other North American journals second, and non-North American journals third. In terms of the commonly used bibliometric measures, I suspect that this is broadly true, but it caused me to reflect on my own publication pattern and to note that of the dozen papers that I regard as my best, not one was in either an APA or Psychonomic Society journal. Why not? Certainly not because of an explicit strategy, though I must confess that I think of APA journals as somewhat conservative, and inclined to reject anything that does not convince all of the, often somewhat staid, referees. The greater the pressures to publish in such journals, the greater the conservatism is likely to become. The bibliometric measures in question tend to emphasize 'impact' (citation within the first two years) and more general citation rates. Such measures will inevitably tend to favour short-term factors such as concern with the currently fashionable, and a tendency to use conventional and hence unobjectionable measures.

At a theoretical level, pressures for the rapid and fashionable will tend to encourage the sort of simplistic 'oh yes it is! - oh no it isn't!' controversy which has been all too common in psychology over the last 30 years, in which one oversimplification is pitted against another: serial versus parallel processing; analogue versus propositional imagery or more recently conscious versus unconscious learning. At an empirical level it is liable to encourage the 'experimental goldmine' based on a simple paradigm allowing endless manipulations. These typically involve countless variations on an established

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theme; studies of implicit memory provided the goldmine of the 1990s. A more recent golden opportunity is offered by the rediscovery of the ‘false memory effect’ originally reported by Deese years ago (Deese, 1959). He found that if participants were asked to memorize a list of words, all of which were associations to a single initial non-presented word such as ‘sleep’, then they would subsequently falsely recognize the core word.

So what should we do; where should we publish? It is important to accept that there are no easy solutions. It is simply not possible for assessors to read and judge all the papers included, for example, in the RAE submissions, or for that matter, all the publications of people who apply for jobs. It almost certainly is the case that publishing in high impact journals does indicate a good level of competence and diligence. The danger however, is that we implicitly elevate competence above originality. In doing so, European scientists lose a major advantage, namely that we do not need to become entirely part of the North American scene. Just like North Americans, we tend to read our own journals and attend talks at our own local meetings. Consequently it is much more acceptable for European scientists to publish in their own journals than it would be for their North American equivalents to publish here. Furthermore, because the pressure of submissions is not so great, there is more scope for originality. Admittedly, the papers may be less likely to be widely read in North America. However, any important new work is likely to generate subsequent, less controversial work which can then be published in the more conservative North American journals.

In conclusion, while it is unrealistic to expect the bibliometric pressures to change, we should recognize that, as European scientists, we actually have an advantage in having a series of good journals that will accept novel ideas and publish them in a shorter time. As evaluators of science, we need to remind ourselves that estimates based on where something is published are at best guides to competence rather than originality. Finally, we should value our journals and try to ensure that they continue to be able to compete in terms of originality and quality with those of our more overloaded North American friends.

REFERENCES

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