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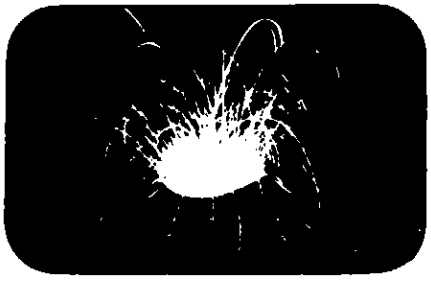

Pyroclasts

RE: NSERC: Criteria of the Earth Sciences Grants Committee, ... and its reviewers

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This year the inevitable disaster caught up with me: my grant has been reduced. Inasmuch as I have had the distinct impression for some years that I have been receiving more than I am worth, I cannot complain at that. However, the committee also sent me some corrective notes which upset me considerably. Briefly, I am accused of non-productivity (... an average of only two papers a year in refereed journals or proceedings ...) and of not favouring the refereed journals (it is true that a lot of my work has appeared in books emanating from conferences). I think that it is past time the well-known attitudes underlying these criticisms were themselves critically examined.

For my two papers a year, I will claim that, if you show me a field-oriented earth scientist in a Canadian university who consistently generates more than two papers a year, I will, in most cases, be able to demonstrate a scientist who is (a) publishing the same paper more than once, or (b) boiling pots, or (c) dividing results into least publishable units, or (d) not giving proper attention to university responsibilities. None of which is helpful.

A further important issue underlies the situation. NSERC funds generally support research groups headed by a professor/grantee and including students and other research associates. It is a standard practice for the group leader to co-author most or all of what emanates from the group just to establish productivity in grants competitions. But in many instances this becomes rather unethical. It has been my practice to have my students publish by themselves when my role in the research has been no more than that of advisor and discussant (a frequent situation for any group leader). The grant is acknowledged. I send a list to NSERC of these results: they clearly do not count. The ethical issues surrounding add-on bylines have turned serious in some recent cases in the most highly competitive fields of science. We do not face such problems in Canadian earth science, but it remains unsettling that assessment procedures appear to encourage the practice.

Whether to publish in primary, refereed journals. I think that it is time to face the fact they are now considerably over-valued as a test of quality. I am told that conference proceedings "unavoidably have a weaker refereeing system than journals". Well, all of the best and most critical reviews I have received in the past ten years have been made of conference papers. There are good reasons for that. If you choose your conference well, you are dealing with the most competent and interested critics. You have an editor who is well informed in the specific subject, so that neither reviewer nor author can slip across a weak claim. Contrary to the mythology, I have not yet encountered a conference featuring volunteered papers in which the editors were not subsequently prepared to reject substandard work. (Invited papers, of course, entail another sort of critical assessment before the fact.)

On the other hand, journals often receive relatively superficial reviews from busy individuals with no special interest in the manuscript that has arrived unannounced in the mail. The commercial presses have now generated too many journals chasing too little top-quality work. Consider this recent plea from an editor: "To be frank, at this stage we need papers and I am willing to accept manuscripts on appropriate topics that are based on sound science, even if severely deficient in other respects (*e.g.*, poorly written, badly organized)". Clearly, there are journals and there are journals: the foregoing did not come from Saskatoon! [*Editor's note: the editorial office of the* Canadian Journal of Earth Sciences *is in Saskatoon*] There are also matters of editorial taste. The good journals are conservative: acceptable papers (in earth science) must contain data, and usually new data. My most influential papers have been a retrospective analysis that was published in a "review journal" (it definitely was not a review), and an "idea paper" in a conference proceedings volume. Both [were] very unlikely candidates for a major journal.

The upshot of this is obvious. There is no quick-count substitute for a considered review of an author's work and a thoughtful assessment of its quality. I have had the impression for years — and it has been, in my mind, one of the impressive aspects of the NSERC procedure — that this is what has guided the assessments of grant applications and grantees. I am very disturbed at the suggestion that the technicians and number spinners might be gaining ground.

I should emphasize, in closing, that the grants committee had some other, helpful things to say about the state of my work: what is rehearsed above probably is not the major basis for my fall from grace. The comments that I received did, however, signal that these issues need a thorough airing.

Accepted 29 September 1987.