

Letter to the Editor

Dear Editor,

I would like to make it clear at the outset that I really do have a high regard for certain components of NSERC's awards and scholarship program; but having received the results of the 1989 NSERC Operating Grants competition informing me that my already too low grant was to be made lower still, I now have a great deal more sympathy with those who have written to you, and to other journals, about similar experiences. Why has it taken me so long to write? Read on.

Since I am not the complaining type (witness the fact that I have not written to you before to complain about my grant level) I would probably, after a bellow or two of rage, have shrugged my shoulders and set out to do what I could with what I had were it not for the fact that The Committee (I have to talk about The Committee in the personal form because that is the way all communications from NSERC read) committed one of the very few sins I cannot ignore. They were guilty of bad science. The Committee's stated reasons for reaching their decision to reduce my grant by nearly 9% were in direct conflict with the facts. I quote "In reaching its decision to reduce Dr. Beck's grant slightly, the Committee is of the opinion that Dr. Beck had reached a plateau in his research program with evidence of perhaps even a small decrease in publication rate." The alleged decrease was demonstrably incorrect. In addition, they decided to play word games. Although the reduction in my grant was "slight", I was informed that my grant was still "significantly above average" for my discipline group; the award was just over 8% larger than the average for that year. Although it is nice to know that if the government cuts NSERC funding 8 or 9% they might regard it as slight, the patronizing attempt to soothe me (plus some later ones) combined with the lack of logic persuaded me to persist in questioning the reasons given for decision.

In my correspondence I pointed out the above and other inconsistencies and asked a number of questions. After four or five months I received a reply which contained little new information and did not answer the questions I had asked, but it did contain the following astonishing statement "With regard to the Earth Sciences Committee comments about your productivity. NSERC regrets this statement was made. We wish to assure you that the committee generally assesses the quality of the contributions made rather than the number of papers." — a sort of "by golly you were right, we can't nail you on the number of publications so how about quality — that is a little more difficult to quantify" attitude. Maybe so, but I intend to try anyway.

First, I am sure all grantees out there, especially those who have been told they are being cut because of a low publication rate, will be glad to know that it is quality and not quantity that counts (Mike Church please note; John Shaw, don't bother trying to publish those four rejected papers — they're not going to count).

Second, if by implication my work is not first rate then let's be generous and say it is at least as good as second rate; I am sure the editors of the *Journal of Geophysical Research*, *Geophysical Journal International* and the *Canadian Journal of Earth Sciences* (amongst others) are going to be rather unhappy to learn that they have been publishing so much second rate material.

Third, all thirteen referees who have assessed me over the last three competitions (*i.e.*, about a decade) as excellent to outstanding are going to be unhappy to find they have been doing such a rotten job in NSERC's eyes. But it does raise an interesting question. If it wasn't my published work they were judging me on, what was it? Maybe my beauty? And if The Committee members had other information that was not available to the referees, what was it? Perhaps there was something negative in the Site Visit report? Interestingly enough my request for copies of all written material pertaining to the decision

made on my grant application elicited the information that one document mysteriously was missing — the report of the Site Visit Committee; I do hope that a copy of that material was eventually found in time to be made available to this year's committee members.

Anyway, because trying to extract explicit information from NSERC quietly is like trying to pour hot air into a bathtub, I have to pose my questions publicly to the individual members of the 1989 Committee and ask them to respond, preferably in public, but directly to me if they wish (i) How did they reach the conclusion that my publication rate had declined in recent years? (ii) How did they reach the conclusion that the quality of my work was less satisfactory than before? (iii) Whatever criteria were used, were they uniformly applied to all applicants? (iv) How did they reach the conclusion that my grant should be reduced and not maintained or increased?

Frankly, it is becoming increasingly obvious that too many decisions of the Committee are inconsistent, a point I made some years ago, and the information on the rationale for the decisions reached, especially negative decisions, is so sanitized that it might as well be non-existent; certainly it is not very helpful to applicants whether they are successful or unsuccessful in the competition. Naturally, in a human system we have to expect human failings, but I think the frequency of inconsistent and poor decisions is too high.

One of the problems is NSERC's insistence on procedures that effectively erect a fortress-like wall between it and the constituents it is supposed to serve. Why should reviewers and referees be anonymous? Why should writers of the Committee's comments be allowed to hide behind the collective noun? The much abused rule of anonymity allows too many bad decisions to be perpetuated. The thirteen reviewers of my work were kinder to me than I would have been to myself, so obviously I have no personal complaints there; however, I do know colleagues who have been very badly treated because of inaccurate, and sometimes vicious, remarks in an anonymous referee's report — remarks I am sure would never be made openly. Let's do away with anonymity.

As anyone who has asked me to review a grant proposal or paper knows, I refuse to referee them anonymously, mainly on the grounds that it is only fair that the author or applicant knows who the critic is. Although my basic opinions remain unchanged, the knowledge that I would be known forces me to be more circumspect (and I hope more constructive) in how I set out those opinions; and because I have to be more careful and thorough in my readings, I occasionally discover that I have misread or misunderstood something on my first run through. I have not found that refusing anonymity causes me any problems (or perhaps it has?). Certainly I feel we will not get more informed, open and improved decisions until we have a more open decision making process.

Another problem is the so-called "star system"; who was it said — never has so much been given by so many to so few for so little gain. But let's leave discussion of that to another day.

References

- Church, M., 1987, RE: NSERC: Criteria of the Earth Sciences Grant Committee, ... and its reviewers: *Geoscience Canada*, v. 14, p. 235.
 Shaw, J., 1988, Nothing new, nothing new: *Geoscience Canada*, v. 15, p. 291-292.

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