

Peer Review: A Case History from the Australian Research Grants Committee

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Peer review determines what science is done and by whom. Recent literature on the fairness of peer review is highly controversial, in part because of a failure to examine actual case histories. The author presents quotations from correspondence relevant to his own repeated failures to obtain research grant support from ARGC — correspondence produced largely after intervention by a Member of Parliament.

The Australian Research Grants Committee (ARGC) was established in 1965 to advise the Commonwealth government on means 'for the stimulation of high-level research to be carried out by individuals or research teams' (ARGC, 1976). Its primary task has been to decide which research proposals the government should fund. Research grant applications are received 'from established research workers in universities, although persons working outside universities normally would not be debarred from applying unless they were working in government authorities' (ARGC, 1976).

ARGC's method for 'determining the projects and investigators most deserving of support' is, according to the same source, 'a system of confidential reports from referees nominated by the applicant and from expert assessors selected by the Committee'. Decisions on research proposals are made by sub-committees of the ARGC which specialise, though still ranging over wide fields of knowledge: humanities and social sciences; physical sciences; chemical sciences; biological sciences; earth sciences; engineering and applied sciences; marine sciences; and upper atmosphere.

Three possible fates await a research proposal:

1. It is considered worthy of support.
2. It is doubtful in some respect; a final decision is not reached until the applicant is interviewed personally by the appropriate sub-committee.
3. It is considered unworthy of support (and no interview is held).

The crux of peer review of research proposals is that it is judgement by other working scientists. There are two levels of judgement:

1. The written opinions of assessors (ARGC uses this term only for the individuals it chooses) and of referees

(ARGC uses this term only for individuals nominated by the applicant).

2. The decisions of the sub-committee, including: the choice of assessors, whether or not an interview is to be granted, whether or not the interviewed applicant successfully answers criticisms, and whether or not the proposal is funded.

Controversy over peer review

The growth of government-sponsored research is largely a post-World War II phenomenon, perhaps the most significant change in the structure of Western science. Because modern science is expensive, beyond the means of salaried individuals, government research-granting organisations largely determine what research will be done and by whom. This has a number of extremely serious implications for the public in general and for the scientific community in particular.

Given the power of peer review, and given that the process is conducted by small groups in secrecy, it is not surprising that peer review has become controversial. Critics of present practices in peer review argue that:

1. New ideas may be rejected — 'resistance to discovery' as documented by the sociologists and historians of science (e.g. Merton, 1963).
2. Personal and political factors may intrude on the decision process — for example, rivalry for status, conflicts over priority, personal likes and dislikes, political victimization, sex discrimination, 'old school tie' or 'old boy' network, etc. (e.g., Bernal, 1939; Greenberg, 1967; Watson, 1968; Hagstrom, 1974).

A statistical analysis of the evaluative procedures of the National Science Foundation (the American equivalent to ARGC) — funded by the same organisation it was studying — concluded that there was 'no evidence to substantiate recent public criticisms' (Cole *et al.*, 1977). A similar conclusion of 'fundamental soundness in NSF [National Science Foundation] peer review' was reached by an American congressman and his aide (Symington and Kramer, 1977). To my knowledge there has been no comparable study of peer review in Australia, although there has been criticism of the ARGC (Fowler, 1975).

However, the conclusions of these American investigations have been challenged. There have been claims of 'demonstrable mismanagement in specific cases' (Wilsdorf, 1977). Abuse of peer review was a major factor in the firing of the director of one of the divisions of the US National Institutes of Health (Smith, 1978).

Challenge to the statistical survey mentioned above has recently come from the distinguished Professor of Machine Intelligence at the University of Edinburgh (Michie, 1978) who points out its lack of sensitivity in detecting the five or so per cent of cases where assessments of research proposals are not made as objectively as might be desired.

Other criticisms can be made of the same statistical survey. The nine variables explained only 11% of the variance. The most highly correlated variable was the number of times the applicants' published papers were cited by other scientists (in *Science Citation Index*) — but even that important variable explained only six per cent of the total

variance. As Cole *et al.* (1977) comment: '... given the heavy emphasis the NSF places on past performance as one of the two most important criteria in evaluating research proposals, it is somewhat surprising that measures of past scientific performance do not show a stronger influence on the probability of receiving a grant.'

It is indeed 'somewhat surprising', for the Coles have also found, in other studies, that recognition and reward in science correlate strongly with quality as conveniently estimated by *Science Citation Index* (Cole and Cole, 1972, 1973; see also Lawani, 1977).

Cole *et al.* (1977) also reported that the degree of agreement between assessors evaluating the same proposal was different in different fields. The lack of consensus was especially great for proposals in ecology and in meteorology, fields known to have been the subject of bitter rivalry and much controversy. Agreement between assessors was twice as good in modern algebra as in ecology. Much of the Coles' research has been on the physics community. Mathematics and physics are 'hard' sciences, subjects of strong coherence and consensus. The biological sciences are 'soft', with conflict between separate schools ('invisible colleges') which differ in objectives, methods, indeed in their paradigms (Manwell, 1978). The data of Coles *et al.* (1967) will need to be partitioned to show the outcomes for research proposals evaluated by assessors in the same and in different 'invisible colleges' in the 'soft' sciences.

The importance of this reservation on the statistical study by Coles *et al.* (1977) becomes clearer in the light of an anecdote reported by Michie (1978) in his article on peer review:

A fairly senior official of a non-British agency, whose nationality I shall not disclose, once told me that if for any reason he felt justified in short-circuiting the system in order to get a given result, he would make a judicious selection of referees — either the scientist's particular friends or his particular enemies, according to which result he wanted. He needn't have told me. I knew it already. In his heart of hearts so does any scientist who has been in the game any length of time.

As scientists, of course, we dislike anecdotal or personalised accounts. We prefer 'hard', quantifiable data. However, as Coles *et al.* (1977) admit, they tested for only one type of bias ('the old boy network'), and this bias lacks 'conceptual clarity'. To improve conceptual clarity in the hypotheses it is important to examine actual case histories.

The case history

It is the Committee's wish that its practices should be as unmysterious as possible. What follows is an attempt to explain these practices, as precisely as is consistent with the flexibility the Committee tries to maintain. (ARGC, 1978)

Starting over six years ago, I attempted to track down what might have been an error in the peer review process as used by the ARGC. The difficulties I encountered are such as to require now open publication, because my private attempts to effect some modest reform have met resistance. In presenting my own case history, the onus is clearly on me to present as unbiased an account as possible. Reliance is placed on quotations from correspondence, largely with the present Chairman of the ARGC. An earlier draft of this article was sent first to the present Chairman of the ARGC with the offer that I was willing to correct

any errors. His reply, dated 7 July 1978, states that he would '... not comment on your manuscript in any way. This does not signify concurrence with your account. History involves not just facts, but the basis of their selection.'

Before examining the facts, I summarise the order of events. Prior to arriving in Australia in 1969, I applied for, and received, a small ARGC grant to study genetic variation of proteins in introduced and native Australian animals. Although several publications came from this research, the grant was summarily terminated after three years without any explanation being given at the time, nor any answer to my enquiries. Three subsequent grant requests, including a request for renewal of the original grant, were made in 1971, 1972 and 1976. These were rejected without interview and without answer to my enquiries. Assessors' comments were not produced until June, 1978. In 1976 I received help from a former Vice-Chancellor who visited the Chairman of the ARGC to find out what had happened to these research grant requests.

In June 1977 the Chairman of the ARGC sent me a proposal to assess which was on a topic entirely distinct from my publications. This allowed me finally to establish some kind of communication with ARGC — although it required the efforts of one university department Chairperson, another Vice-chancellor and a Member of Parliament to prise out some remaining correspondence from ARGC. At least in recent years, the paraphrasing of assessors' comments is supposed to be routinely available to rejected applicants (ARGC, 1976, 1978).

Any responsible reader who doubts the accuracy of the following quotations, or who wonders if they have been taken out of context, may see xeroxed copies. No blame is assigned to any past or present member of the ARGC.

Bureaucratic delay

Starting with the request from ARGC that I assess a research proposal, in June 1977, an exchange of correspondence occurred which continued until November of the same year, at which stage my main questions were still unanswered. The following June, two letters from ARGC provided the answers which made it possible to piece the story together.

My first theme is simply the delay which has characterised this exchange of correspondence.

Upon receiving the request to assess a chemical research proposal, I gave the reasons for declining this request in a letter to ARGC dated 17 June 1977. The Chairman of ARGC's reply is dated 8 August 1977, nearly two months later.

Dissatisfied with this (uninformative) reply, I wrote again to the Chairman of ARGC on 17 August 1977 and, when no reply was received, yet again on 20 October 1977. My department Chairperson also wrote to the Chairman of ARGC on 27 October 1977. The Chairman of ARGC finally responded by letters both to me and to my department Chairperson, dated 15 November 1977 — thus, three months after my August letter.

As I (and others) felt that the information received was not only still inadequate but also self-contradictory,

I wrote a fourth letter on 22 November 1977. When further letters from my department Chairperson, and now also from the Vice-chancellor, failed to elicit any additional explanation from the Chairman of ARGC, I asked my Member of Parliament to obtain a reply – and, in particular, to obtain the assessors' criticisms of my three research proposals submitted in the years 1971-1976.

After an interval of approximately seven months I received a letter (15 June 1978) from the Chairman of the ARGC, which begins: 'I am replying to your letter of 22 November 1977 . . .' This same letter ends: 'In conclusion, I would like to say that . . . I have not conducted this correspondence with any kind of reluctance. . . .'

It is not just the understatement that bothers me. It is the delay in receiving the assessors' criticisms for the whole period 1971-1976, which were in fact finally forwarded a few days later, on 21 June 1978. Note how the dates overlap the 15 April 1978 deadline for applications, thereby denying me both the opportunity to reply to the criticisms and the opportunity to submit a revised application in 1978.

Can ARGC keep track of its records?

The 8 August 1977 letter from the Chairman of ARGC to me had expressed some displeasure with my comments of 17 June on the assessment request, and emphasised: 'This statement [of repeated rejection] does not conform with our records which show only one application from you since your original grant.'

In the next (15 November 1977) letter from the Chairman of ARGC I received the following statement:

I am pleased to reply as you have asked, especially because I find I must correct and account for one of my own statements, which was that our records show only one application from you . . . ; however, through a procedural error, this file did not contain a note to the effect that a previous file has now been transferred to Archives. This earlier file has now been retrieved; it contains material from 1968 to 1971 and it is quite clear that my earlier statement was entirely wrong.

It is not just the error that is worrying, it is the circumstances. As mentioned earlier, I had asked a former Vice-chancellor for help when I could elicit no reply from ARGC. In his 15 November 1977 letter to me the Chairman of the ARGC says he assumed that I ' . . . did accept my statement of 8 August 1977 that [the former Vice-chancellor] was satisfied that your *proposals* had been treated on merit' (my italics).

The use of the plural here, in reference to a letter (8 August 1977) that had said specifically that only one proposal had been rejected by ARGC, struck me as odd. Only in his third letter (15 June 1978) did the ARGC Chairman explain that ' . . . my reference was to the two proposals (or sub-proposals) on snails and sheep' – to which I shall be referring in more detail below – and that 'I showed [the former Vice-chancellor] all the papers relating to your 1976 application, and no others.'

Nevertheless, elsewhere in the correspondence the sheep and snails are referred to by the Chairman of ARGC as one proposal. Single versus plural may seem a trivial distinction; but the letter of 15 November 1977 had given me the wrong impression – that the former Vice-chancellor had been shown all the files and rejected proposals, whereas in

fact he had been shown only those relating to the last rejection.

Assessors' criticisms – non-renewal of grant

As mentioned, after from two to six years of effort – the elapsed time depending on the particular research proposal involved – I received in a letter of 21 June 1978 the paraphrased comments on my three separate grant proposals (including that for renewal of the original '69-'71 grant). The paraphrasing was prepared by the Acting Secretary of ARGC, with 'the imprimatur of the Chairman and the members of the plant and animal biology sub-committee'.

As regards the termination of the 1969-1971 grant by ARGC, the paraphrasing is as follows:

. . . the reports of your assessors were such that your project could not be rated as highly as those which received the Committee's recommendation. One of the assessors of this project acknowledged that the triennial report which was submitted with it showed that a considerable amount of work had been performed for a relatively small outlay of equipment and maintenance. He considered that you were obviously knowledgeable about protein chemistry and about some Australian animals. He pointed out that your work up to this application could be described as data collections over a wide range of animals rather than as being based on any firm hypothesis. He allowed that this was inevitable in the early phases of this kind of work. However, he was concerned about your failure to develop some general theory after three years of work. It was the opinion of this assessor that you would not make any outstanding discoveries in the course of this work; however he was reasonably confident that you would continue to accumulate a lot of data. Your second assessor echoed these sentiments. He was worried that you seemed to be following too many lines of enquiry; he felt that there was no direction to your proposal. He was afraid that some of the investigations you were proposing would never bear fruit while on the other hand a few of them could possibly lead to rather interesting results.

This paragraph has provoked strong reactions from everyone who has been shown the correspondence:

1. How many triennial reports document 'any outstanding discoveries'?
2. How many investigations financed by ARGC 'never bear fruit'?
3. Neither assessor produces a single specific criticism of the published or proposed research.
4. Even though one assessor has an adverse opinion about following several projects at one time, it is conceded by the other assessor that 'a considerable amount of work has been performed for a relatively small outlay'. Even the assessor who objects to following too many lines of research admits that some of these lines 'could possibly lead to rather interesting results'. My research style happens to be to learn a few techniques as well as possible and to apply them to a variety of zoological problems; this has had the advantage that other investigators have confirmed my experimental work and that large amounts of money are not spent on a wide variety of equipment which is only occasionally used.

Assessors' criticisms – and how ARGC uses them

I now consider the most specific criticisms, which were confined to the last (1976) rejected proposal, on biochemical variation in sheep and snails as a study in mechanisms of evolution:

Assessor opinion 1:

One of your assessors was of the opinion that the work concerning

the snails was interesting and well conceived. If you had limited your proposal to this study he would have given it his unqualified support. As it was, he believed the sheep project to be ill conceived. He thought that although there may be merit in studying the extent of genetic variation in breeds of sheep, as an indication of the cumulative effects of selection and population size bottlenecks on the level of electrophoretic variation, it was quite unrealistic to contemplate using such information to unravel the evolutionary history of the species.

Assessor opinion 2:

Your other assessor felt that you and Ms Baker have contributed a number of good papers to the literature on protein and enzyme polymorphism and he stated that he would like to see your work continue. Nevertheless he was unable to give his support to this particular proposal. As far as he was concerned the basic concept behind your proposal was unsound and he doubted whether snails and sheep were good research material in attempting to decide between hypotheses of drift and selection. He pointed out that clines are not necessarily indicative of selection, so that the work on snails would be likely to produce equivocal results. He also believes that your application of Nei's genetic distance parameter to sheep is invalid since the genetic history of Australian sheep almost certainly includes bottlenecks in population size, and that Nei's parameter assumes an approximately constant population size. He felt that your intended use of low frequency variants to arrive at a 'correct' figure for time for the Merino-British breed divergence was also invalid since, in his opinion, Nei's parameter can apply only to a random sample of loci.

The first point to note is the contradictory opinions in relation to the snail part of the proposal. The second assessor correctly criticizes the notion that a cline *per se* is evidence of selection — but we made no such statement in the proposal. The proposal emphasised that a replication of the same clines in similar environmental gradients for this introduced species in Australia and for its native counterpart population in Europe and the Near East would be evidence for selection. Indeed, the first assessor appears to have understood our objectives on the snails, in view of his support for this part of our research proposal.

Incidentally, when the Chairperson of my department wrote (27 October 1977) to the Chairman of ARGC, he received a letter (15 November 1977) in which the Chairman states:

In the 1977 [sic! in fact, 1976] case, as I have told Professor Manwell, the assessors did acknowledge the standing of the investigators, but *they* were critical enough of the strategy of *each* of the two elements of the proposal [note, here again, the singular, not plural] not to rate the entire proposal highly enough. (my italics)

In contrast, the paraphrase later approved by the Chairman himself and the appropriate sub-committee of ARGC indicates that while both assessors criticised the sheep part of the proposal, one of them advocated 'unqualified support' for the work on snails! Thus the ARGC Chairman's comments to my departmental Chairperson, while obviously helping to rationalise ARGC's actions, are in contradiction to the Acting Secretary's paraphrase.

The second point to note is the general criticism by the second assessor of the use of Nei's genetic distance parameter. The comments are similar to comments made in the symposium volume *Population Genetics and Ecology*, where Nei himself answers them (Karlín and Nevo, 1976); Nei has since shown that the bottleneck effect is significant only for very small populations, kept small for a long period of time.

It is thus not unreasonable to guess that the second assessor is, or is a close colleague of, a particular researcher who, as it happens, has also published his own theory of genetic distance and had some difference of opinion with Nei. This putative assessor has been well supported by ARGC for work which has included studies of genetic distance in species for which the history of population size and bottlenecks is far less well known than for Australian sheep.

Without asking the reader to enter further into the debate about drift versus selection — a very controversial topic, considered by many the outstanding problem in evolutionary theory — it should be pointed out that, thanks to the generosity of this university's former Vice-chancellor and the present Chairperson of the Department of Zoology, it was possible for us to complete a portion of the proposed studies on sheep. Papers are published in the *Australian Journal of Biological Sciences*, *Animal Blood Groups and Biochemical Genetics*, *Biochemical Journal*, *Comparative Biochemistry and Physiology*, and *Genetical Research*; the last mentioned will provide the reader with a full discussion of the assessors' specific criticisms, published about one year before I received the criticisms (Manwell and Baker, 1977; see also Manwell and Baker, 1978, for comments on clines). The fact that the referees and editors of these journals felt such criticisms did not invalidate the contributions in these papers should carry some weight.

Assessors and referees

Earlier I mentioned receiving in June 1977 a request by ARGC to assess a research proposal in a field quite outside the area of my professional competence. This raises three questions:

1. How frequently are assessors chosen who are so far outside the subject?
2. Will members of the ARGC be able to detect errors in assessments? Examples of failure to do this have already been provided in this paper — even something as obvious as the ARGC's failure to note that an assessor criticised a statement we had never made.

Using *Science Citation Index* and *Social Science Citation Index* (Cole and Cole, 1973; Lawani, 1977), I find that only some members of the ARGC publish over a range of disciplines or are frequently cited. Other members have remarkably small citation records. Citation, of course, occurs for a number of reasons, though scientists seldom cite a paper just for its errors. Given the wide range of fields that each sub-committee of the ARGC must cover, it is imperative that members be chosen who have broad interests and high scientific standing.

3. If my research is as inadequate as ARGC's treatment suggests, then should I have been asked to assess another applicant's proposal, especially one on the physical biochemistry of vanadium? To quote the Chairman's 15 June 1978 letter dismissing the favourable comments of my referee: 'the committee is more assisted by the views of referees and assessors who are directly in the field of the research'. (Such a comment implies that the referee I nominated had offered an opinion on a research proposal he did not fully understand. This could, I think, have been refuted by looking his name up in *Who's Who*,

or by inspecting the 1976 *Philosophical Transactions of the Royal Society*, London, Series B.)

Testable hypotheses

The ARGC's (1978) 'Advice to Applicants' says: 'Applicants who are unavailable for interview may be interested to know that if the assessors' reports are of a nature than [sic – that?] an interview would be seen as highly desirable, then the Committee's general practice is to seek further assessors' opinions.'

In my case there were three proposals and six assessors. No interviews were scheduled for the first two rejections. ARGC did schedule an interview for the third rejection – even though the research proposal had stated clearly that I would be on study leave in England at that time. Consistently, then, in my case the practice of 'further assessors' opinions' was not used and ARGC did not give me a chance to answer what turned out to be easily answered criticisms.

Three hypotheses are offered for testing:

1. The quality of my research proposals and the quality of my published research are sufficiently bad to account for the rejections received from ARGC. A bit tough on the ego but we have to face it.

The Chairman of ARGC wrote to me (15 November 1977): 'After all, about 50% of initial applications cannot be funded.' In fact, the most recent relevant document from ARGC (1978) gives a 43% failure rate for new applications, and my applications were rejected when the failure rate was even lower. From the 1973-1975 Triennial Report (ARGC, 1976) I find that only 27% were rejected – and for the biological sciences, only 22% were rejected. For renewal requests the rejection rate was then less than 2%.

Therefore, assuming that my rejections were independent events, they have a combined probability of roughly 0.001 – the beginning of the range of 'very highly statistically significant'. By congruence, my publication and citation record should also be somewhere in the bottom 0.1% of all ARGC applicants. Unfortunately for this first hypothesis, it is closer to the top 1%.

ARGC has funded other scientists whose research proposals indicate that they expect considerable assistance and advice from me and my wife. If my research is so poor as to justify the treatment I have received from the ARGC, then why support others whose research proposals indicate dependence upon me? It appears that other hypotheses should be considered.

2. Such a case history is not unusual and can occur simply from the uncertainties inherent in the system. This hypothesis could be tested by seeing if a large number of other unsuccessful applicants can match this case history or surpass it.

3. Specific negative discrimination. Although Cole, Rubin and Cole (1977) conclude '... the scientific enterprise is an exceedingly equitable, although highly stratified, social institution in which the individuals who produce the work that is most favorably evaluated by their colleagues receive the lion's share of the rewards', there is evidence in favour of hypotheses 2 or 3 in part of their statistical analysis. When a subset of NSF proposals was chosen where the assessors had given *equal ranking*, there was

clear evidence that the research granting agency favoured those applicants who had already received funding for other grants; to quote Cole *et al.* (1977):

Among scientists whose proposals received medium ratings, for example, 61 per cent of those who had been funded within the past five years were awarded a current grant, whereas only 41 per cent of those who had not received funds from the NSF in the past five years were awarded a current grant. Clearly a good funding record gives rise to a slight competitive advantage.

I would argue that an increase of from 41% to 61%, a 50% increase, is hardly 'slight' – especially given the weakness of the correlation of success or failure with other variables in their study. The results of this part of the Coles *et al.* (1977) study, despite the lack of sensitivity in such a statistical approach, are evidence that there is sufficient favouritism or 'grantsmanship' so that the probability of funding is increased by 50%, not on resolution by additional assessors' evaluations, not on the applicant's previous publications or the extent to which other scientists cite them, but simply on whether or not previous research grant applications (which might not even be in the same speciality) were successful!

This raises the same question that a distinguished chemist with long personal experience as an 'insider' (program director of a branch of the US National Science Foundation) has asked (Wilson, 1975): Do granting agencies place more emphasis on the topic itself, and on possibly inflated claims of what the researcher might accomplish, than on what the researcher has actually done, or might reasonably expect to do? Does the system select for excessive 'grantsmanship' at the expense of more realistic aims and of actual accomplishments?

My three hypotheses can be regarded as 'natural selection', 'random drift', and 'unnatural selection', respectively. Are there any other facts which might assist in classifying the case history?

In 1971, the author and his wife criticised the SA Department of Agriculture's fruit-fly spraying program. Within a few weeks of those criticisms (repeated in the following years by other scientists with ultimately some improvement in the program) an attempt was made to dismiss the author from his position. It required a four-year fight to have the author's name cleared of charges, laid by a very eminent Australian scientist, which are now officially recognised as 'a number of errors' (Vice-chancellor's statement, 3 June 1975, available from the Registrar, University of Adelaide, SA, 5001).

It was shortly after the sacking attempt that the author's ARGC grant was terminated without cause being given. The ARGC grant was not mentioned in the 1971 formal complaint against the author – although subsequently an attempt was made to use ARGC's actions against him.

There are the three hypotheses and the facts. I am interested to hear from other scientists in the hope that some changes can be made in peer review which will improve its function.

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