The review process for applied-research grant proposals: suggestions for revision

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Résumé : À l'heure actuelle, les comités d'examen des demandes de subvention de recherche se comportent comme s'ils avaient pour fonction principale de trouver des raisons de rejeter les demandes. Divers aspects du processus d'examen encouragent une étude indûment critique des projets, avec pour résultat que près de 90 % des demandes sont rejetées. Un certain nombre de modifications du processus d'examen sont proposées : désigner l'un des membres du comité comme le défenseur de chaque demande, encourager une plus grande interaction entre examinateurs et demandeurs, évaluer les demandes plutôt que de décider simplement de leur acceptabilité, financer toutes les demandes qui répondent à des normes minimales, fournir aux demandeurs des explications détaillées pour qu'ils améliorent leur demande, qu'il y ait ou non financement éventuel, et enfin permettre aux demandeurs d'élire les membres des comités. Bien que de telles révisions puissent augmenter le nombre de «mauvais» projets de recherche financés, elles amélioreraient les chances de financement des projets valables, surtout les projets novateurs, tout en garantissant des recherches d'une plus grande actualité.

If you want to make a career of applied health research you must learn to deal with rejection: our peers reject 70% to 95% of all applications for research projects submitted to agencies that provide funding for applied health research (e.g., the National Health Research and Development Program [NHRDP] and the Ontario Government Health Care Systems Research Committee). Why are so many projects considered of too poor quality for funding? Is it failings in the researchers or in the system? I believe that one major problem is the review system, which leads to the inappropriate rejection of many proposals that would produce useful information.

I have served as an internal member of research review panels for various funding agencies over the last 10 years and currently am chairman of a committee for the NHRDP. Each year I have read more than 100 proposals and provided detailed reviews for about 25. From this experience I have identified a number of factors in the functioning of review committees that I believe lead to the inappropriate rejection of research proposals.

First and foremost, review committees appear to adopt a stance of "guilty until proven innocent": they spend a lot of time looking for reasons to reject proposals. This position arises from the laudable objective of trying to keep poor research from being funded. But most committees try to achieve this objective by imposing standards of perfection that can rarely, if ever, be met, rather than rejecting only proposals that have no chance of achieving their objectives. For example, many committees require random allocation of subjects to interventions, a standard that could never be achieved in a study of the impact of poverty on birth outcomes. In many cases committee members use the minor flaws found in any proposal as reasons for rejection rather than as sources of feedback to researchers to improve their project.

The tendency to demand perfection is compounded by the group dynamics of review committees. Panel members, especially new ones, often seem to compete with each other to detect obscure flaws in a proposal to show that they have the "right stuff." At a recent review meeting we all agreed before the meeting that we needed to be more "relaxed" in rating proposals. Despite this, once the discussions began, the decision making reverted back to the tough, usual standards, and we rejected about 70% of the proposals. This illustrates the differences between individual and group decision-making standards and reflects the organizational structure and mandate of review committees.

A third problem is committee members' lack of expertise in the area of research being judged. In contrast to most basic-science review committees, which tend to address fairly specific content areas, applied-research review committees tend to consider projects on a wide range of topics and with varied research methods (e.g.,

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from sociologic studies to randomized clinical trials of surgical procedures). This inevitably leads to expertise deficiencies on the panel. One way to address this is through external reviews by experts. However, these reviews vary greatly in their depth of criticism. Furthermore, without internal committee expertise the extreme or conflicting opinions of external reviewers might have undue influence on committee deliberations. Often when confronted with conflicting opinions in a field in which one has limited expertise one tends to abdicate decision-making authority by rejecting the work, with the rationalization that "the researcher can sort this out and submit a revised proposal." This problem is further complicated by the failure of most committees to keep minutes and thus to provide applicants with a clear indication of why the proposal was rejected. How many of us have been notified of a rejection only to find three glowingly positive external reviews and two internal reviews recommending approval?

Fourth, committee members are often unaware of (or choose to ignore) the need to conduct research in a timely fashion to preserve collaborative teams or to take advantage of "windows of opportunity." For many applied projects, especially those evaluating new programs or health care delivery approaches, the research requires collaboration between various agencies or people. Often these groups need to delay innovations or to alter practice patterns to permit the evaluation to proceed. Even if funding is provided on the basis of the initial request the regular proposal review cycle involves a delay of 5 to 10 months after submission. If a resubmission is required the delay can be up to 24 months. More frequent submission deadlines would not greatly reduce the delay, because the applicants often would not have time to revise the proposal before the next deadline. In many cases the extra delay for a resubmission leads to the coalition breaking up and the project being discarded. Yet, review committees often adopt the position that the project is close to being acceptable but could be "tightened up," so they turn it down believing that the investigators will submit a revised version if they are really serious about the project. In many cases the points raised by the committee are controversial or have been dealt with by the applicants in the detailed internal research protocol, but the information could not be presented in the final proposal because of page limits.

Finally, the peer-review process as usually practised is often unreliable.[1,2] For example, a colleague recently submitted similar versions of a proposal to two major funding agencies (a common way to increase the chance of being funded but a drain on reviewer resources). One agency rated the project as excellent and recommended funding; the other said it had major flaws and rejected it. Which agency was right? How often does the peer-review process fail and the committees never find out because there is no mechanism for feedback from the applicants?

The underlying theme is that review committees are effectively designed to reject research proposals rather than to fund them. In many cases they become so good at their job and reject so many proposals that the funding agency's research budget is not fully spent! It is almost unheard of for an applied-research proposal that passes peer review not to be funded because the agency ran out of money. How can we convince governments that we need more money for applied health care research when we give the impression that we are not capable of producing enough good work to spend all the currently available money?

We need a new review system in which the primary mandate of the review committee is to help researchers produce the best research they can. The committee should no longer sit as a tribunal and determine whether a project lives or dies. Rather, there should be an assumption that all projects meeting minimal standards are eligible for funding, which should lead to a rate of rejection on methodologic or relevance grounds of as low as 10%. The only proposals that should be rejected are the few that have no research methods, are unethical, are outside the mandate of the funding agency or have no scientific basis. Ideally all other proposals should be funded, if there are sufficient resources.

To implement this new system several changes to the old system would be needed. First, we need more interaction between review committee members and applicants. Several ways of achieving this could be considered. Committees could designate two internal reviewers — one to lead the debate on why a project should be funded and the other to argue why it should not. Committee members would be permitted (or even encouraged or required) to contact applicants to ensure that the best case has been presented. Perhaps a telecommunications system such as electronic mail or computer bulletin board could facilitate interaction between committee members and applicants. This process would reduce the risk of a simple misunderstanding leading to rejection and help ensure a balanced discussion.

Second, there should be no more "reject" or "accept" decisions. Instead, all proposals should be ranked in terms of their strength and relevance. This process should be designed so that a study with exploratory or "weaker" methods (e.g., a case–control study of a topic on which little work has been done) could be ranked highly. Currently, such an outcome is practically impossible, because committees are obsessed with methodologic strength as the primary criterion for rating proposals. The review committee should make no recommendation about the cut-off point for funding. Rather, it should act as if all ranked proposals would receive funding. The list of ranked proposals should be made available for public review, which might place pressure on funding agencies to increase resources.

The third change, and perhaps the most difficult to achieve, involves the agency agreeing to provide funding for all ranked proposals. Of course, if any agency were able to accept this challenge the need for ranking would be eliminated, and the review committees could concentrate on helping researchers improve project quality. In either case the committee should not be required to consider the funding agency's annual budget during the review process.

Who should sit on the review panels? This is the area of my last suggested change. The pool of applicants to each committee should elect committee members to 2-year terms. Currently, members are selected by the funding agency. Under the revisions proposed here, committee members would become more responsible to the applicants and their credibility in providing critical and helpful feedback would become extremely important. If committee members were elected, researchers would have a say in who would best perform the role, the committee would be more representative of the needs of the research community, and applicants would be more supportive of committee decisions. Defining the electorate of a review committee would be challenging. It would likely include past and present recipients of grants and members of organizations with congruent objectives who are eligible to apply for funding. Perhaps researchers would be permitted to select some number, say two, of committees for whose membership they would vote. Of course, this process (and perhaps the entire review process) would be simplified if funding agencies cooperated in conducting reviews, possibly through a mechanism similar to that used for students applying to medical school. Democratization of the review process has also been proposed in Britain by Lesurf,[3] who also argued that the process must be more open.

Implementation of all these revisions could lead to an increase in the number of proposals being eligible for funding, which would have financial implications for funding agencies. A commitment by an agency to fund all submitted projects would lead to enhanced productivity in applied health care research and would ensure more effective use of the many career scientists already funded. Such researchers could do their work instead of continually trying to obtain funding. Finally, the cost may be less than expected if funded projects relating to methods of improving the efficiency of the health care system lead to reduced health care costs or more efficient expenditures. These suggestions for increases in research funding are consistent with recommendations from the government and such groups as the Canadian Public Health Association.[4,5]

Given current fiscal realities it is unlikely that applied health care research can expect a large infusion of cash. However, funding agencies could implement aspects of my proposal related to committee structure and review process without immediately committing to increase funding. This would provide an opportunity to examine the type of proposals that are deemed acceptable for funding but are not funded. The current review process shields the agency from this knowledge and removes much of the incentive and pressure for increased funding. The proposed changes should also lead to a fairer review system, because applicants would have an opportunity to ensure that the committee really understands their proposal.

Some people will argue that my suggested revisions will lead to the funding of a lot of "bad" research. If all projects meeting minimal standards were funded, there could well be an increase in such research being supported. But, in epidemiologic terms, I am proposing a reduction in specificity and an increase in sensitivity, which would lead to a reduction in the number of "good" proposals being rejected. Research would be more timely, and researchers would improve their productivity because they could concentrate on designing good projects rather than on trying to outguess what the review committee considers fundable. Also, detailed critiques would help researchers improve many of the weaker projects.

These suggested revisions may be somewhat idealistic: Why would researchers agree to serve on a review committee to provide comments that would help other researchers improve the quality of their work and the validity of their conclusions without receiving a tangible reward, such as authorship credits? However, with the new system committee members who provided good advice would be valued by the research community. I think that this role could become as important as that of thesis supervisors providing assistance to graduate students, although applicants should require less input than students. The academic community would be challenged to find a way of converting this peer recognition into tangible rewards through promotion, payment from funding agencies or possibly some form of coinvestigatorship if the involvement of the reviewer were substantial. If viewed as challenges rather than problems these issues need not preclude revisions to the review process.

Many of my suggestions were derived from an informal, unfunded participant-observation study. However, the review system should be amenable to a more formal research study. I wonder if there is a review committee that would recommend funding?

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