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THEME SECTION

Peer review: journal articles versus research proposals

Idea and coordination: **Hans Ulrik Riisgård***

Marine Biological Research Centre (University of Southern Denmark), Hindsholmvej 11, 5300 Kerteminde, Denmark

Contributors: **Hartmut Barth, Poul Scheel Larsen, Peter Roepstorff, Ferdinando Boero, John Dolan, Ulrich Sommer, Valerio Zupo, Jan Vermaat, Thomas R. Anderson, Ron T. Kneib, Bente Aa. Lomstein, Ian Jenkinson, Sandra E. Shumway**

ABSTRACT: The focus of this Theme Section is peer review of journal manuscripts versus that of research proposals, both of which can be intellectually rewarding. A number of well-established scientists who were invited for comments underlined that whereas a manuscript submitted to a journal is a completed piece of work, suitable to be peer reviewed using well-defined criteria, a research proposal is a plan of action and not an end product in itself, and so requires different assessment criteria. Although it is often transparent how reviewers' comments on a manuscript are translated into editorial decisions, this may not always be the case with the verdicts of funding agencies on research proposals. Financial compensation may play a role in the willingness of reviewers to undertake the task, and most (but not all) of the scientists thought that such remuneration would be a fair reward. Compensation may be a practical incentive for reviewers who are not citizens of the country served by a particular funding agency, and who are excluded from applying for related funds. Grant awarding agencies in different countries should compare their procedures to assess the merits of different approaches to assessing research proposals.

INTRODUCTION

Publication of scientific articles in peer reviewed journals is an important part of being a scientist and it seems fair to review for those journals in return for having one's own manuscript submissions reviewed (see the MEPS Theme Section coordinated by Riisgård 2000). In subsequent MEPS Theme Sections (TS), misuse of the peer review system by repeated resubmission of unchanged manuscripts (Riisgård 2003), and various aspects of quality and its assurance in science publishing (Browman & Kirby 2004) have been discussed. The present TS focuses on peer review of journal articles versus that of research proposals.

CIRCULATED LETTER

In order to start the discussion, I circulated the letter below to a number of well-established researchers, research councils and program managers:

Within the first 3 months of 2004, I have already received half a score of review requests from scientific journals, all of which I have done without complaining, even though I have not yet published in several of the journals. In addition, I have been requested to review research proposals submitted to several granting agencies, including the Netherlands Organisation for Scientific Research, the United States Department of Commerce, and the USA National Science Foundation, where I am ineligible to apply for funding. Although I have for some years responded positively to requests from different organisations and boards to review proposals, without reflecting too much about the reasonableness of spending my time doing this, working against the clock has now pushed me to make up my mind. I have decided not to do review work for research councils, organisations etc. to which I cannot apply for support of my own research.

After having read my substantiated refusal to review one more USA research proposal, the Program Manager promptly responded by sending me an email response prefacing her general remarks by saying that she has been providing reviews for a well-known marine journal for many years, in her spare time, since it is not part of a Program Manager's job. She admitted that there are flaws in every peer review system, but asked if I would

*Email: hur@biology.sdu.dk

prefer that she arbitrarily decides which is the best science, or that a panel of limited expertise decide without the benefit of individual mail reviews. Finally, the Program Manager wrote that if I had concrete suggestions for helping to improve the proposal review process I should let her know, because just refusing to do reviews does not improve the system. In my reply I said that during the last 5 years I had done review work for the Research Council of Norway in my 'spare' time, because the external referees are being paid for doing review work for the Norwegian state, to which I, being a Danish citizen, cannot apply for research money. The Norwegian model seems to be quite fair to me, and therefore my concrete suggestion for helping the Program Manager to improve the review process is to pay non-USA citizens to act as external referees.

My inquiry to a number of research councils and research program managers was almost fruitless. I did receive the contribution below:

COMMENT FROM THE EUROPEAN COMMISSION

Hartmut Barth (Directorate General Research of the European Commission). I have been working for more than 20 years at the research management department of the European Commission, which has long applied the peer review of research proposals with great success. I cannot be counted as a person critical to peer review.

Despite the fact that national research funding in many EU Member States is decreasing in favour of increasing research budgets at EU level, which leads to increasing competition for budgetary support at the European scale, and hence a higher dependence on peer review results at the European level, I have not experienced a substantially higher number of complaints (mainly by unsuccessful applicants) about the results of our peer review system. On the contrary, not only the independent review experts on behalf of the Commission, but also most of the applicants (scientists) are realising the high level of transparency, quality and fairness of the peer review system applied by the Directorate General Research of the European Commission. On several occasions I have heard the argument that their national funding bodies should apply similar peer review procedures. Complaints by a minority of scientists whose research proposal failed cannot be completely avoided by any of the various possible evaluation and selection procedures. This is understandable, considering that their own job or that of young scientists who expected to be employed on a contractual basis often depends on third party funding.

The European Commission reimburses travel costs and pays daily allowances and an expert fee (honorarium) to the independent evaluation experts. The fee also applies in the case of 'remote evaluations', where the experts first work at their home institution to evaluate proposals individually and then form an 'expert panel' which gathers for 'consensus meetings' at the European Commission offices in Brussels to achieve a consensus on the overall evaluation of each proposal.

COMMENTS FROM SCIENTISTS

The comments from invited scientists are arranged chronologically by time of receipt, but all respondents have had the opportunity to read all contributions and to adjust their own comments.

Poul Scheel Larsen (Technical University of Denmark, Kgs. Lyngby, Denmark). Many reviews of journal manuscripts and research are intellectually rewarding, since they entail early insight into new ideas in the field. This is also true to some extent for efforts of foreign members of committees evaluating doctoral degree work.

However, considering the increasing workload of a recognized specialist, I agree with the editor of this Theme Section that it is logical and reasonable to remunerate any 'out-of-the-system' evaluator or reviewer when it comes to research proposals and degree work. This has long been recognized and practiced by Scandinavian universities and national research councils. I do not understand why the USA National Science Foundation, for example, does not follow this practice. Money and power corrupts, as the saying goes, but such considerations can hardly be the reason, since remuneration remains almost symbolic in practice.

And in all cases considered, including journal reviews, it is difficult to understand why the institution requesting the review does not have the courtesy to inform the reviewer of the outcome of the process.

Peter Roepstorff (University of Southern Denmark, Odense, Denmark). Peer reviewing is an integral part of quality assessment in science. For me, however, it has reached a level where the time required for reviewing is beyond my capacity. On average I get 2 to 3 requests to review manuscripts per week and a similar number to review research proposals and reports from research projects, plus 1 to 2 requests per month for site visits. This is not manageable, even if I used all my time for this, totally ignoring my own research, my students and my family. I must refuse a majority of these tasks, but this also that takes time, because it is often followed up in mail exchanges or telephone calls.

Remuneration is offered for some research proposals and site visits. This is reasonable, because the tasks are mostly performed in the evenings and at weekends,

constituting a considerable burden for family life and preventing me from engaging in any leisure activities at home. Decisions on which tasks to accept and which to refuse are mainly influenced by the quality of the journals and research programs.

Remuneration plays a minor role, but may influence the choice when several requests arrive simultaneously. The choice is often, should I do the review or e.g. repair my house? However, the main problem that I see is that the time required for peer reviewing now is so great that some of the reviewing will be done superficially and thus lose value. I have seen several examples of this recently.

In my opinion there is a need to revise the entire peer review system to bring it back to a reasonable level and to ensure that the quality is good. In another words: we must find ways to reduce the contagious disease 'evaluitis'.

Ferdinando Boero (University of Lecce, Italy). The problem of reviewing is vital for many of those who are in mid-career. Editors and program managers do not dare ask for help of the 'giants' in a given discipline, and they do not care about the opinion of the newcomers. So there are just us, the guys who have been in this business for some time and who have attained some 'respectability'. We do it because — especially at the beginning — we are flattered to be consulted. Then we become slaves of the system.

I spend more time in reviewing articles and projects of other people than in actually writing and revising my own articles and projects. I have accepted appointment to the editorial boards of several journals, and for this work I have rewards: a free subscription and an enhanced status, as I am recognised as an 'authority' by having my name printed on the cover of every issue. However, there is no reward for all the reviews I do for other journals. Some scientists include in their curriculum a list of the journals that consulted them as reviewers, but this is something to write if you have little more to show. The principal reward for reviewing articles is reciprocity: I will do it for others because they will do it for me.

As for the review of projects, even the Italians pay for reviews of their Projects of National Interest. Moreover, they pay both foreign and national reviewers. I think that this is fair. As noted by other contributors to this Theme Section, in the case of national funding agencies there is no reciprocity: if you are a foreigner you will not be eligible for funds from that source.

John Dolan (Station Zoologique, Villefranche-sur-Mer, France). Science is a social activity. Discoveries or demonstrations are meaningless outside the context of knowledge shared by the scientific community. Communication is then an integral part of science. For better or for worse, our system for the validation and communication of new knowledge is, at present, the publication of peer reviewed 'articles' in 'scientific journals'. Is peer review of research proposals a valid extension of this system? Should the same reasoning and rules apply? Perhaps it is worthwhile to review very briefly the commonly accepted reasoning and rules with regard to anonymous peer review of scientific articles.

Peer review is used to assure scientific soundness. Scientific articles presumably report objective facts and so can be judged in an objective manner. By sending submitted manuscripts to reviewers, editors ask those who are expert in the field to render decisions as to the soundness of the science. In general, reviews are written anonymously, to facilitate impartiality, and reviewers are expected to avoid conflicts of interests and/or to exploit in any manner the ideas or data in submitted manuscripts.

Given that the common body of knowledge lies in published articles, we have a communal interest in assuring the quality of scientific publications and therefore in abiding by the rules. Furthermore, it is generally recognised that it is indeed unreasonable to ask colleagues to examine your work in a fair and timely manner if you refuse to do so for others.

However, it should be mentioned that the existing system of researchers freely giving their time to review manuscripts without reserve is evolving in some aspects. For example, recently the nature of the scientific journal has become a focal point. Some researchers are now making a distinction between 'for profit' and 'non-profit' journals or 'subscription-based' vs 'free-distribution' journals and refusing to review for certain kinds of scientific journals.

The question, however, is with regard to research proposals. Does one have a scientific obligation to review research proposals? The arguments for peer review of scientific publications are not all directly applicable. It must be recognised that research proposals are propositions. Questions to be tested are presented. Proposals are not direct, but rather potential, additions to our store of knowledge. Hence, research proposals must be judged subjectively quite simply because they cannot be judged objectively. In addition, very commonly judgements are solicited on factors (investigator 'potential', societal 'value' etc.) completely and utterly unrelated to the scientific soundness of a project. It could be argued that using a lottery approach (random selection) is potentially a superior method of evaluating proposals because many good questions or investigators may be simply unfashionable and therefore unfundable. Hence, the argument that reviewing for the assurance of quality control and the common good does not apply with any certainty.

There is, however, a moral or ethical obligation on the basis of the notion that it is unreasonable to avoid work one expects others to do. Those who refuse to review research proposals should then only be those who never have, and never will, submit research proposals that require peer review. This would seem to apply to very few researchers. Even the identity of funding agencies is difficult to predict for most investigators. For example, a European or Asian researcher may consider the USA National Science Foundation a very unlikely source of funds, but many exchange programs are administered by the National Science Foundation and a European or Asian scientist may find themselves one day soliciting a USA foundation for themselves or to host scientists from the USA.

A separate issue is that of payment for the activity of reviewing. In contrast to manuscript reviewing, the demands on time and energy vary considerably in evaluating proposals. This can vary from spending a few hours reading and dissecting a proposal, to 1 or 2 days for a site visit, or joining a review panel whose work requires a week, both days and evenings, in a conference room arguing about potential qualities. Few would argue that large inconveniences should be compensated, especially given the fact that the purely scientific obligation to fulfil such tasks is weak.

Overall, it should be recognised that there are 2 main arguments for spending one's time evaluating someone else's ideas for a research project: (1) you want your own projects evaluated; the reasonable evaluation of research proposals rests upon the fact that if we all participate, the system which exists will function (perhaps not optimally, but it will function). (2) If you refuse to participate, someone less competent will do the job!

Ulrich Sommer (Leibniz Institut für Meereswissenschaften, Kiel, Germany). Like most of us, I feel overloaded with requests to review manuscripts and grant proposals. On average, I accept ca. 70% of the requests. So far, my decisions to decline or to accept have not been very systematic, but there are 2 main reasons for declining: If the manuscript/proposal is too distant from my own field of research, and, if I already have too many manuscripts/proposals on my desk.

So far, I have not yet discriminated between journals and funding agencies or against funding agencies to which I have no access myself. Financial compensation has not yet played a role in my willingness to act as a reviewer. Nevertheless, I have a strong feeling that a financial compensation by funding agencies would be fair, while I do not expect such compensation from journals. Reviewing for journals is intellectually more rewarding and less frustrating than reviewing for funding agencies. Both as a reviewer and as a writer of articles/proposals, I have in most cases considered the reviewing and decision-making process of journals as being fair, while this was not always the case with funding agencies, for 2 reasons:

(1) The second circuit: Most journals permit a second round of reviewing which offers the possibility to remove misunderstandings and to improve the paper. Most funding agencies do not offer this possibility. Some journals have recently implemented a 'one-revision-only' policy and I have the strong impression that this has led to a substantial loss of fairness in the decision making process.

(2) Transparency: Both for the author and for the reviewer, it is usually quite transparent how the reviewers' comments are translated into editorial decisions. This is not the case with funding agencies, even if they inform their reviewers about the final decision (only some funding agencies do so). It seems that funding agencies are only willing to fund projects which receive no critique at all. Any comment by reviewers, that some things could or should be done differently, is taken as a 'no', while such comments could really be an expression of keen interest. Funding agencies seem unaware, that good and innovative research is usually controversial, while uncontroversial research is usually boring. Consequently, whenever I want to support a proposal, I am forced to lie and write that every detail is fine. Of course, this might be a prejudice, but this prejudice results from the non-transparency of the decision process.

In summary, most scientific journals have a well-organised and transparent system of decision-making, while most funding agencies have an obscure one. Obviously, it is more fun to work for the transparent system.

Valerio Zupo (Stazione Zoologica 'Anton Dohrn', Ischia, Italy). I agree that there are significant problems with the peer review system and that the process needs to be revised to ensure its quality. Without a doubt, it represents a constant preoccupation in scientists' daily activities and deeply influences our financial resources, our way of working and the result of our common efforts.

In several cases, the opinion of reviewers triggers the funding of some types of research, to the disadvantage of others: this shapes the lines of scientific research in the world. Therefore, reviewers have a great responsibility. However, to the basic question 'should this job be remunerated?' I continue to answer negatively. Actually, the remuneration could be minimal and merely symbolic, to avoid corruption, but what would be the advantage of this? In fact, the amount that would be paid could not possibly compensate for the time involved and the great responsibility; it is a time-consuming occupation, just like other 'unpaid' duties of scientists. In some periods the amount of unpaid

work may exceed our possibilities, but saying 'no' quite resolves the problem. Guaranteeing quality is an entirely different problem. The system should ensure high-level evaluation, to avoid both rejection of good projects (or papers) and financial support (or publication) of poor research. This result is yet to be attained; bad reviewers have the same influence on the decision-making process as other reviewers. I am sure that anyone who has ever sent project proposals to public offices (or manuscripts to scientific journals) has had to face similar problems; however, how can the system work if the reviewer is superficial, or less competent than the author? (and this happens frequently nowadays). This is a shame and it dramatically slows down our activities, pushing us into a time-consuming process of submission and re-submission to obtain funds for important investigations. I am sure that if I could stop wasting time due to the influence of low-quality reviewers I would gain sufficient time to do several reviews weekly.

In conclusion, reviewers should offer a useful contribution, even when they reject a project (or manuscript), by providing competent suggestions; all too often they just demonstrate their incompetence and/or superficiality. My suggestion is to improve the system by having program managers and research councils (as well as journal editors) request an evaluation of the work of referees from proposers and authors, e.g. by means of specific forms sent to them along with the accepted or rejected proposals (or manuscripts). Referees could be evaluated by a system of points (e.g. a score of +3 for positive feedback after they recommended rejection, a score of +1 for positive feedback after they recommended acceptance, a score of −3 for negative feedback after recommending acceptance, etc.). The reviewers could receive an annual report on their personal 'evaluating factor' from each program manager (and journal), and this could be an attractive form of remuneration and attain academic value. This would permit program managers and scientific journals to rank their reviewers and authors and to improve the efficacy of the system. I am sure this would remunerate our work more than 'symbolic' fees. Moreover, the feedback mechanism would enhance the quality of funded research (and of published papers) and increase the influence of proposers and authors, which are a significant part of the mechanism, and it would maintain the anonymity of the actors (proposers and reviewers). The process could be traced and certified by international centres for impact factor (IF) assessment, and these could also record the outcomes of completed projects and the IF of papers. My suggestion entails various problems. One danger is the overload of good reviewers: if we have a list of good, average and poor reviewers, anyone will choose to send projects to good reviewers. In this case, remuneration could be considered for high scoring reviewers, and it should not be merely symbolic. In conclusion: (1) we need to improve the review system; (2) the mechanism could equally apply to research programs and scientific journals via a common system of judgement and IF assessment, and (3) remuneration of good reviewers should be the *result*, not the *instrument* to reach this ambitious goal.

Jan Vermaat (Vrije Universiteit Amsterdam, The Netherlands). I will discuss (1) the difference between reviewing proposals and manuscripts, and (2) the remuneration issue. These 2 matters should not be mixed up.

(1) Reviewing manuscripts versus proposals: In my experience 'evaluitis' is less virulent in project evaluation than in manuscript review, simply because the flow of project evaluations across my desk is less voluminous, and because those requests that I get are well organised. I find the review of proposals interesting (what new work are people trying to get funded? How clever do they build their arguments?), but it is certainly subjective, as well as favouring conservatism (answer the question: 'could you rate the quality of the research consortium?'). Here lies the major difference between proposals and manuscripts. In proposals, we judge 'promises' and not ANOVA tables and tests of hypotheses. How to judge promises? This justifies the solicitation of several 'replicate' reviews. Still, I think that review panel members should get some training in Multi-Criteria Analysis, and they should give originality more weight than solid reputation, despite the risk this entails for the funding agency.

(2) Remuneration: I think there is a consensus among us scientists that reviews from scientists who are ineligible for funds from the granting agency require remuneration. Agencies worldwide should review and compare their procedures. I wonder though how much influence we scientists have. Riisgård's simple 'code-of-practice' is practical; Roepstorff's 'burden for family life' to me seems rather a matter of private choice and priorities. The major European 'market' for proposal reviews lies in Brussels. Here, reviewers are paid for travel and work, though the administrative burden is quite a cost. Possibly, national characteristics across Europe will erode in the coming decades, but I am not sure whether that is my preference. I happen to come from a small country.

Thomas R. Anderson (University of Southampton, UK). Reviewing manuscripts is a reasonably straightforward, though time-consuming, process. A manuscript is a completed piece of work, which can be assessed using various well-defined criteria such as technical accuracy, originality, relevance and clarity. If each and every factor meets a minimum standard,

albeit after suggested improvements, then publication can be recommended.

Reviewing proposals is quite unlike manuscript review: a proposal is a plan of action and not an end product in itself and so requires different assessment criteria. Unlike manuscripts, proposals are in direct competition with each other and therefore need to be ranked. Key additional criteria for reviewing proposals include the credibility of the principal investigators and the degree of risk. Other criteria such as 'social impact' have also been introduced, although many reviewers take little notice (Mervis 2001). Many proposals that I have encountered have a 'trust me' element to them, particularly where the proposers are established experts in their field. Maybe this is fair enough, but it is important to encourage funding of the brightest ideas that, more often than not, are produced by innovative young researchers. I find it difficult to objectively score proposals given disparate assessments. Different referees and the review panels themselves may put varying emphasis on aspects such as originality and degree of risk. So, is it fair, for example, to ask a reviewer if s/he considers a particular proposal to be in the top 10% worthy of funding? Perhaps it would be better to ask reviewers to separately score individual criteria without providing an overall mark. That would be done by review panels and program managers together, under agreed guidelines for weighting the various criteria.

I review manuscripts, without thought of financial remuneration, for a variety of journals, but mainly those in which my own work has, or might in future, be published. I feel a moral obligation to undertake this reviewing, to return the compliment for those poor souls who have to plough through my manuscripts! The proposal review system, in its widest context, operates in much the same way. International peer review provides the cornerstone of independent feedback. Nevertheless, do I feel morally obligated to review for the USA National Science Foundation? I am not sure. All my reviewing is done out of work hours, and I have to say that some sort of remuneration would be most appreciated.

Ron T. Kneib (The University of Georgia Marine Institute, Sapelo Island, GA, USA). The crux of the problem from the reviewer's viewpoint is in balancing obligations (professional and personal) and time — our most valuable and limiting resource. For most people, including scientists, time management comprises a multitude of diverse individual decisions based largely on some personal priority system. Some time allocation decisions may be purely altruistic and others are made in the belief that they will achieve the greatest personal gain in either the short or long term. Regardless of how we view the responsibilities and benefits of the peer review process, participation in any specific case remains the choice of the individual to whom the request is being made.

I generally agree with the distinctions between manuscript and proposal reviews expressed by Dolan, but would like to expand on this within the context of the previous paragraph. The consequences of the decision to recommend for or against the funding of a proposal, compared to the publication of a manuscript, are quite different. For proposals, jobs or training opportunities may be at stake. There is also the issue of empowerment — investigators associated with large sums of grant money tend to have greater access to key decision makers, both within and outside their own institutions, and are more likely to influence the direction of future funding. Clearly, researchers are rewarded for investing time in the generation of proposals, but are rarely recognized or tangibly rewarded for either the quantity or quality of their review work. For these, and many other reasons, peer review of proposals may have more far-reaching consequences and greater responsibilities for the subset of the scientific community willing to provide them.

Decisions to invest quality time in community activities — for either altruistic or selfish reasons — also may depend on how strongly individuals feel they are integrated within the community served. It should be easy to understand the reluctance of scientists to spend their time serving the goals of a granting agency to which they are ineligible to apply, or which repeatedly rejects their own research proposals. Of course, this reduces the potential pool of reviewers, and places a greater burden on those who choose to remain active participants in the peer review process. Furthermore, scientists may find it difficult to evaluate objectively any proposal that includes goals other than those designed to advance the field in which they possess expertise. Proposals often are solicited in certain areas to meet perceived needs, some of which may be motivated more by politics than by science — both within and outside the scientific community. Any implied promise that the review will have an impact on the direction of the field may be, in effect, an empty one, because the decision to fund projects in that area has been made already.

Grant programs also have become a political means of directing social change. In particular, this seems to be the case within large public agencies like the USA National Science Foundation, which requires applicants to address (and reviewers to evaluate) explicitly the broader impacts that the funded activities will have on society, including teaching and enhancing participation of individuals identified as 'under-represented' by virtue of their gender, ethnicity, geographic location, or any number of other demographic characteristics. As citizens of a society, scientists may have opin-

ions regarding such issues, but not the objective expertise to evaluate the proposed impacts in this category. In particular, a potential reviewer who is not a citizen of the country served by the granting agency might be very uncomfortable making such judgements.

Turning to the specific example presented in the Introduction to this Theme Section, there is a positive aspect in Riisgård's recent experience. Scientists from North America have been criticized sometimes for being — shall we say — indifferent to the work of the broader global community of scientists. So, it can be viewed as a positive step that funding agencies in the USA are actively seeking out the opinions of colleagues from other countries in an attempt to make the research they fund of greater global relevance.

Ultimately, individuals determine whether or not they have the time or expertise to provide a fair and thorough review in response to any specific request. This is as much a part of the current peer review system as any other element of it — not perfect, but perhaps as good as it can be. Complaints and problems with the peer review system likely will continue until scientists — along with the rest of society — can accept that there are limits to growth in any endeavour that consumes limited resources (including time), and that sustainable activity requires a focus on quality over quantity.

Bente Aa. Lomstein (University of Aarhus, Denmark). Basically, I find that the peer review system is excellent and I cannot think of any other way to ensure the quality of scientific work. I find it fair and natural that reviews for scientific journals are based on the principle that you need to have someone reviewing your own work and you will therefore need to 'pay back' by reviewing manuscripts for other scientists. In a scientific lifetime you may gain more from the review system when you are young and then pay back later in your career when you have become more experienced. However, I sometimes feel frustrated about having too many manuscripts from the same journal to review; a frustration that I know is shared by many of my colleagues. I suggest that journals might overcome this problem by defining a limit on how many manuscripts they send out to individual reviewers per year and make this number official to reviewers. This action may eventually expand the entire reviewer group.

My greatest workload in terms of reviews has been as reviewer of research proposals as a member of the Danish Natural Science Research Council and other committees. Despite the fact that I have gained insight from this work and often enjoyed it, it is also true that this obligation has taken up time from my own research and family and I am sure that I will never be able to submit a number of proposals that will equal the number that I have evaluated.

With regard to the question whether reviewers should be remunerated: In my view, payment cannot give you back the hours that extensive review tasks take away from your own research and other obligations, and I am not in favour of ordinary payment for these jobs. On the other hand, I think a symbolic remuneration is fair, and maybe even necessary, to acknowledge reviewers of research proposals. One should keep in mind that most of us perform reviews at times where at least our families expect us to concentrate on them.

Ian Jenkinson (Strategic Editor; former Acting Editor-in-Chief, Journal of Plankton Research, France). I think that most scientists understand the difference in what is required between reviewing a manuscript and a research proposal, particularly as guidelines are often supplied.

There is debate, however, about whether payment should be made, particularly for reviewing research proposals. I see 2 types of problem: effort, and ownership.

Effort: Cheques from foreign countries for less than about $/€100 may just not be worth the trouble. To give an example, I once received a cheque from the UK for US $20 for a small article, but my bank deducted $12 for accepting it, and 2 years later I had to make a written declaration to the French exchange control authorities to explain what it had been for.

Ownership: Although scientists work together in a worldwide village, there are still big geographical, cultural and political differences in their real or perceived legal, fiscal and moral rights and duties. Reimbursement of travel and hotel bills for attending evaluation meetings is probably uncontroversial. However, are scientists allowed to receive undeclared *per diem* payments in addition to their salaries? Should such payments be remitted to one's employer or declared for different taxes and/or social security deductions? Does keeping such payments constitute a minor form of theft, or may the scientist proudly declare the funds generated, and also spend the cheque on tax-free cigars or perfume at the airport? Is there an amount, $100 a go, say, or $1000 per year, below which there could or should be total tolerance?

Sandra E. Shumway (University of Connecticut, Avery Point, CT, USA. Editor, *Journal of Shellfish Research, Journal of Experimental Marine Biology and Ecology, Harmful Algae*). Peer review is the very basis upon which our profession operates, be it journal articles, grant proposals or individual reviews for promotion and tenure. While one may debate the pros and cons of this system and the quality of the reviews, the process is here to stay. Reviewing manuscripts and proposals is an integral part of a scientist's position in the global academic community. As long as I am con-

tributing manuscripts and proposals for consideration, I will expect to provide reviews to others when asked.

Having read some of the prior submissions to this forum, I feel compelled to comment on a few. While I wholeheartedly agree that it would be nice to know the outcome of each review, I can say from experience that it adds many hours to the process if one tries to send notes and, inevitably, explanations to reviewers about why a paper or proposal was accepted or rejected, especially if this decision goes against the reviewer's comments. I point out regularly that reviews are opinions, not votes. It is up to the granting agency or editor to make the final decision as to the acceptability of a paper or proposal. There are more often than not many factors to be taken into consideration and one can only hope that the person making the decision is as well informed as possible — and the way they become well-informed is through the peer review process!

Going outside the country for reviews, be it the National Science Foundation in the United States, the National Research Council of Canada or any other agency, reduces the likelihood of conflicts of interest and generally improves the system by reaching out to a broader audience. I agree that NSF proposals are a burden and they are considerably more lengthy (and weigh more) than proposals from most agencies. Unfortunately, that is the system and much of the material can be ignored in the review process, as it is simply necessary paperwork for the agency. The heart of the proposal is the 15 pages of text, hardly an excessive reading assignment.

For those who feel they are being exploited or do not have the time to review, they need simply to inform the person or agency requesting the review that they cannot accommodate the request.

As an editor, I am continually searching for reviewers and I try to get the best available. These also tend to be the busiest people, so I understand completely when they are unable to provide reviews. I greatly appreciate it when they tell me they cannot do it, rather than simply ignoring the request, and I ask them for other suggested reviewers. This is a good way to become familiar with new players in the field and to provide them with the opportunity to participate in the review system. Not only does this enhance the roster of reviewers, these younger investigators tend to be very conscientious, diligent and capable reviewers.

However, I can certainly see the point made by Riisgård with regard to the different countries and not being able to apply to them for money, but still being asked to review proposals. With regard to the National Science Foundation and some others, I do not see that one actually knows that they will never apply to them for funding, even if they are from a different country. NSF has many programs for international collaboration and many scientists who travel and have colleagues in other countries may well find themselves applying to various agencies for funds. That having been said, I really do not see that being able to apply to an agency for funding should be a prerequisite for reviewing proposals; this is just part of the role of a scientist in the international community. With regard to the comments by Sommer, who notes that funding agencies are only willing to fund projects that receive no critique at all, I think in many instances he is correct. In addition, different countries have different standards for review and the competition has become so difficult that proposals that do not receive all 'top scores' are frequently not competitive. I believe we have generally lost the spirit of academic debate and the sense of the review system as a means of improvement. I could not agree more with the comments of Zupo regarding incompetent reviewers — especially of grant proposals, where there is usually no chance for rebuttal. If potential reviewers do not feel they have the expertise to comment on a particular piece of work, they should simply say so and return the proposal, not ramble on about what they think it means and criticize the work regardless — a not uncommon practice. Moreover, granting agencies have an obligation to locate reviewers who are not only competent to review a particular proposal, but who will do so with diligence and fairness, and not give a cursory read and assessment — another reason why we all have an obligation to review proposals and manuscripts when asked. In summary, if one is submitting manuscripts and proposals, they must expect to provide reviews of the same when asked. If senior scientists are too busy, they should take the time to recommend some of their more junior colleagues. Paying reviewers is not the answer.

SUMMARY

Reviewing proposals is quite unlike manuscript review. Scientific journals report objective facts that can be judged objectively. A manuscript submitted to a journal is a completed piece of work, suitable for peer review that follows well-defined criteria. If certain minimum standards are fulfilled and suggested improvements are made, publication can be recommended. A research proposal is a plan of action and not an end-product, and therefore requires different assessment criteria. Unlike journal manuscripts, research proposals are in direct competition with each other and need to be ranked. Key additional criteria for reviewing proposals include the credibility of the principal investigators and the degree of risk. Other criteria are e.g. 'social impact', although scientists may find

it difficult to evaluate goals other than those designed to advance the field in which they possess expertise. Some grant programs have become political means of directing social change. Thus, the USA National Science Foundation explicitly requires applicants to address (and reviewers to evaluate) the broader impacts that the activities to be funded will have on society. A reviewer who is not a citizen of the country served by the granting agency may therefore feel very uncomfortable about making judgements on that country's political needs.

The consequences of a recommendation for or against the funding of a proposal are quite different from recommendations on manuscripts. Researchers may be rewarded for investing time in the generation of proposals, but they are not rewarded for either the quantity or quality of their review work. Most scientific journals have a well-organised and transparent system of decision making, while most funding agencies have an obscure one.

Reviewing for scientific journals is based on the simple principle that you need to have someone to review your own work, and therefore you must repay by reviewing manuscripts submitted by other scientists. If a researcher is submitting manuscripts and proposals for consideration, s/he must expect to provide reviews of the same when asked. When a funding agency engages reviewers from outside the country, it reduces the likelihood of conflicts of interest and generally improves the system by reaching out to a broader audience. However, it is easy to understand that scientists can be reluctant to spend their time serving the goals of a granting agency to which they are ineligible to apply. This reduces the potential pool of reviewers and places a greater burden on those who choose to participate.

Hitherto, reviewers do not seem to discriminate very much between journals and funding agencies, or against funding agencies to which they have no access themselves. They may decline to review a manuscript or proposal that is too distant from their own field of research, and they may generally decline when they are overcommitted. Acceptance and refusal of the task are also influenced by the quality of the journals and research programs in question. Review of both journal manuscripts and research proposals can be intellectually rewarding, since they entail early insight into new ideas. But reviewing for journals is intellectually more rewarding and less frustrating than reviewing for funding agencies.

Reviewing for journals is fair, while this is not always the case with funding agencies. While most journals permit a second circuit of reviewing, offering the possibility to overcome misunderstandings and to improve the manuscript, most funding agencies do not offer this possibility. The translation of reviewers' comments into editorial decisions is usually quite transparent, but this is not the case with funding agencies. Funding agencies often seem willing to fund only those projects that receive no criticism, and reviewers who want to support a proposal are almost barred from making suggestions for improvement.

Recognizing the increasing workload experienced by established specialists it appears reasonable to remunerate any 'out-of-the-system' evaluator for the review of research proposals. This has been recognized and practiced by Scandinavian national research councils, but not by the USA National Science Foundation. For most reviewers, remuneration only plays a minor role, but it may influence the decision to accept or decline the task when time is limited. Most scientists think that a financial compensation by funding agencies would be fair, while they do not expect such compensation from journals. Thus, there is a certain degree of consensus among scientists that reviews of research proposals by external scientists who are ineligible for the agency in question requires remuneration. Grant awarding agencies in different countries should compare their procedures to assess the merits of different approaches to assessing research proposals.

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The text of this and 2 previous Theme Sections dealing with the peer review system is available at the Inter-Research (IR) web site (www.int-res.com/journals/meps/themeSections.html). The debate continues on IR's Forum on the 'Peer review System' (www.int-res.com/forum/peer_review.html). Readers are invited to express their opinions. Please send your text (brief and concise) to the Forum Editor, Hans Ulrik Riisgård, email: hur@biology.sdu.dk