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THEME SECTION

The peer-review system: time for re-assessment?

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ABSTRACT: Referees are the backbone of quality control. They need more recognition for their work. In an open exchange of opinions among a number of leading editors and experienced reviewers one suggestion has wide support: It should no longer be 'free' to submit a manuscript to a scientific journal. While cash payment for reviews is not considered a good idea, a 'payback in kind' system is favored: i.e., if you want to submit papers to a journal you must be willing to review for that journal.

INTRODUCTION

It is difficult for many reviewers (referees) to cope with all the manuscripts (mss) that they receive for review. I am a member of the editorial boards of 3 journals (Marine Ecology Progress Series; Journal of Sea Research; Sarsia). With reluctance I have decided that I can only review mss for these 3 journals. This decision was not an easy one for me because I intend to continue to submit mss to other journals; it only seems fair to review for those journals in return for having my submissions reviewed.

Although the editors of 2 major aquatic journals have emphasized the central role that referees play in the scientific process (Kinne 1988, 1999, Fee 1998), their critically important role in controlling quality remains for the most part hidden; it needs to be much better acknowledged. To advance professionally scientists must publish, especially in the top journals. Up to now authors have received the benefit of the referees' professional work gratis. A modest ms submission fee passed on to reviewers would seem to be in order and might reduce the number of second-rate mss (more than 50% of all mss are rejected by the top journals).

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Further discussion needed

Very few people seem to have concerned themselves with the role of referees. But Otto Kinne (Editor, MEPS) has expressed his opinion in 2 publications (Kinne 1988, 1999). The following extracts from his 2 publications may perhaps stimulate further discussion about the role of referees and how to give them the appreciation they deserve:

Kinne (1988, p. 276/277): 'Editors select and motivate referees, study their reports and make carefully considered decisions on the future of the mss.... The editor must see to it that the peer review process remains prompt, constructive and unemotional.... It is truly astonishing, yes even admirable, how well, in general, the peer review process works. This seems largely due to the fact that most referees are established high performers dedicated to science, and so accustomed to striving for quality and correctness in their own work that they unselfishly apply their rigid standards also for evaluating the work of others.... Referees are the backbone of quality control. The editor absolutely depends on this cream of science. The editor-referee cooperation is the best, and the only, mechanism presently available for assuring sustained quality in primary publications.... The editor should explore means of public acknowledgement and compensation for the referee's efforts, e.g., publish the names of referees who have worked for him during the past years and make free copies of the journal available to them.'

Kinne (1999, p. 2/3): Some colleagues are not aware of 'the difficulties in finding good, reliable reviewers willing to put their own work aside in order to help others, sometimes even competitors. Reviewers deserve a big pat on the shoulder.... Reviewers will increasingly demand some sort of compensation from the publisher, the more so, should they be asked to perform outside their normal working hours. At MEPS we go some way

towards compensation: We publish the names of our staff reviewers, who process per person and month on average 1 or 2 mss, and make free copies available to them' (amounts to an annual value of DM 5000).

Also Everett Fee (Editor, Limnology & Oceanography) has written about the importance of referees (Fee 1998, p. 22): 'Any journal, especially one published by a society, is a community project. L&O has long been a top-rated journal in aquatic science, and I believe that L&O's reviewers are the key to its success. L&O reviewers are notoriously picky, and any ms that can satisfy them has earned its place in the sun. More than 400 mss are submitted to L&O every year. Almost all are reviewed by 2 (occasionally 3) reviewers, and most are re-reviewed at least once more. Finding 1000 reviewers a year and getting them to deliver promptly is no small task. Right now, participation in the review process is highly variable: some ASLO (American Society of Limnology and Oceanography) members review 5 or 6 mss a year, while others refuse all review requests. It is particularly unfortunate that in the latter group are experienced veterans who publish regularly in the journal; their opinions would be especially valuable. Ultimately, the quality of L&O is the responsibility of the people who read and comment on what is submitted and published in the journal. The whole community must participate in the process of quality assurance. Reviews are the most important determinant what will and what will not be published in L&O.' In a subsequent e-mail message thanking me for reviewing for the journal Everett Fee stated that 'Peer review is the heart of the scientific enterprise. Everyone who is called upon to review for L&O is busy. Only selfless individuals are willing to devote so much time to reviewing the work of others.'

Prestigious illusion

Most scientists think that refereeing and serving on editorial boards promote their careers. However, this is probably not true for the majority of referees, since rarely are these efforts officially acknowledged. In fact, anonymous referees receive little or no credit for such work in job applications or negotiations for wage increases. I agree with Otto Kinne that it is astonishing how well the peer-review process works in light of the scant acknowledgement and compensation that referees receive.

Intending to start a discussion, I circulated the above material to various leading editors and experienced reviewers for comment. From their responses (below), it appears that there are many aspects to take into consideration before the traditional peer-review system can be improved. This is clearly a sensitive and pressing subject.

COMMENTS FROM VARIOUS SCIENTISTS

Gerry Quinn (Associate Editor, Australian Journal of Ecology): Besides being on the editorial board for the AJE, I also referee mss for a variety of other journals. All this contributes to reduced output of my own research results. Your time estimate of 15% is realistic. At least in my case, payment for refereeing would not work. Payment will not make additional time available because most of what I need to do cannot be easily done by a paid employee, i.e., being paid for refereeing will probably not free up time for me. We must restrict ourselves to a certain number of refereeing tasks per month and if we receive a request to referee a ms once we have reached our limit, then we decline. I disagree with your suggestion that authors should pay to have their ms refereed. Your suggestion might cause real difficulties. Refereeing mss, like reviewing grant applications, writing references for job applicants etc., is just one of those tasks that scientists have to do as part of their job and career. We need to get these additional tasks recognized by our employers and others so that they are considered as part of our work.

I strongly support the argument, made by many colleagues, that a scientist's refusal to referee mss for a journal should also mean that mss from that scientist would not be considered for publication in that journal. The only downside is that regionally based journals will find it difficult to get referees because most will tend to accept mss to referee only from journals in which they like to publish. There is an extensive recent literature, especially in medicine, on the peer-review process. My cursory scan of recent titles indicates that this discussion focuses mainly on how effective the process is in maintaining quality control and whether it approaches intellectual censorship, rather than the workload of reviewers. Formal acknowledgement of refereeing in terms of career development would help. It would also help if journals restricted the number of referees per ms to no more than 2 (some, like MEPS use at least 3).

Everett Fee (Editor, Limnology and Oceanography): The issue needs to be seriously addressed. When we receive a list of potential reviewers for a new ms we inspect the reviewing history of everyone on the list and do not request a review from anyone who has done 2 or more reviews for the journal during the last year. Our reviewer database also contains a 'notes' field where we store information about the person's current reviewing status. These procedures have virtually eliminated the problem of reviewer overload at L&O. Of course, this does not solve the problem globally. But with modern technology, it is not inconceivable that such information could be stored on the Web in a form accessible to all journals. A vexing problem is scientists that decline (or simply do not answer) all

review requests. Almost all people who refuse to review for the journal are senior scientists, and would not be persuaded to review by offers of monetary compensation. Ironically, they continue to submit mss to the journal, taking it for granted that they will be reviewed. It would be fair to reject without review any submission that is authored or co-authored by someone who was asked 3 or more times during the previous 2 years to review for the journal but accepted none. Until there is a penalty associated with refusing all review requests, the problem will not go away.

I doubt that public acknowledgement of reviewers would have a significant positive effect. A few months ago I proposed to the ASLO Board that L&O reviewers be acknowledged by listing their names in the last issue of each volume. Although there were a few more yes than no votes, I was persuaded to not do this because of potential problems. I was particularly concerned about possible negative consequences of identifying (or limiting) confidential sources.... L&O receives a very wide variety of mss and we occasionally solicit reviews from people whose expertise is far from the aquatic sciences; it is therefore quite conceivable that authors of an 'oddball' ms could deduce that a listed person reviewed their ms. Another Board member said that some people wouldn't want us to list their names publicly, and that we should ask permission from each reviewer before listing them. This would have involved contacting more than 650 people — a mammoth task given that the L&O office staff consists of 2 people.

Poul Scheel Larsen (Technical University of Denmark): The proliferation of scientific publications, including the increasing number of journals, during the last few decades is related to the publish-or-perish syndrome. This syndrome has evolved for essentially 2 reasons. One is the drive of the individual researcher for excellence and competitive performance. Another is the system's need for some objective yardstick for hiring and promotion of staff. There is a strong positive feedback between these 2 mechanisms. One might ask if science would gain by a return to past times' traditions of journals publishing only substantial research contributions, i.e., comprehensive studies representing perhaps 1 to 2 years of research, rather than 'fast jobs' based on 1 to 2 months of work. To ensure the rapid dissemination of the latter another medium (e.g., the Web) could be envisioned.

Sandra E. Shumway (Editor, Journal of Shellfish Research; Journal of Experimental Marine Biology and Ecology): Your discussion regarding refereeing is intriguing to say the least. I do not believe that paying reviewers is an answer. Not only does this add a financial burden to already strained journal budgets, it may also encourage less qualified individuals to participate just for the money. The perceived prestige associated with reviewing and serving on editorial boards is difficult to assess. It is also difficult to determine just what role serving in these capacities plays in careers. I am not certain that the referee system is ready for reassessment, but rather I think it is ready for some support and an injection of enthusiasm. As editor of 2 journals I see first-hand how many senior scientists refuse to review mss because they are 'busy'. Well, everyone worth their salt is busy and these same scientists expect their mss to be reviewed by colleagues when submitted for publication! In addition to the approximately 300 mss that I handle for the 2 journals, I review an average of 36 mss per year for other publications. I believe this is an integral part of being an active scientist and do not begrudge the time spent. I frequently send mss to colleagues with specific notes asking them to have a qualified graduate student or post-doc participate in the process with them. This not only eases the burden for the senior scientists, but is a great learning opportunity for the newcomers. It is up to the scientific community to 'police' themselves and the means to do this is reviewing. Reviewing is also a means of helping colleagues to improve their presentations — an aspect especially prominent where English is the second language. With regard to compensation, I view the final product as payment. Knowing that my colleagues appreciate my efforts and to be able to look back at an issue of the journal and feel a genuine sense of satisfaction in knowing that good science has been improved by the review process, put forward to the scientific community, and preserved for generations to come is my real 'payment'.

Josep-Maria Gili (previous Editor, Scientia Marina): When I was the editor of Scientia Marina we received many mss to be reviewed by external referees. It represented a major effort. I did not review many mss directly but I revised all mss and review comments. During several months of my 6 years as editor it was a tremendous task only encouraged or compensated by the final result: to edit a good journal which covers many aspects that the leader journals were not able to. The experience from this 'second line' journal is a bit different from that involving the more prestigious journals. These journals suffer more directly from the pressure to publish for curriculum purposes. The reviewers receive too many papers into which they only put little effort because they prefer to work for leader journals. It is difficult to convince potential reviewers to take care of papers submitted to a 'second line' journal. I want to remind you about the responsibility of the authors; they should avoid overloading the peer-review system.

Thomas Kiørboe (Contributing Editor, Marine Ecology Progress Series): Referee work takes a lot of time,

often too much time. Of course, getting paid for doing referee work would be very nice, but I am not convinced that it would solve the problem. Why should payment reduce the overall review load? And if it did, how could one be sure that the mss that never made it to the review stage were the poor ones? I can think of at least 3 reasons why you get so many mss to review: (1) you are a well-respected scientist, (2) you agree to serve as a member of various editorial boards, because it is prestigious, and (3) you agree to review all the mss that you receive. Many journals now have the practice of asking potential referees beforehand whether or not they are willing to review a particular ms. This gives you the oppurtunity to decline, and to select only those mss that deal with topics of your genuine interest. It is easier to say no when asked, than to return a ms. My point is that to a large extent you are yourself in control of how many mss you review. Over your entire career you should on average review about 3 times as many mss as you submit yourself — somewhat less when you are young, and somewhat more when you are established. Mss in excess of that are beyond your strict duty, and you can adjust the numbers by turning knobs (2) and (3). A related problem is that editors often have difficulty identifying new referees. One solution to this problem is therefore to encourage referees to pass the ms on to a younger colleague at his/her lab. This has 2 advantages: the referee burden is better distributed, and young scientists may learn to write a review under the supervision of an established colleague. The review should of course be returned to the editor in the name of the younger scientist, who thus becomes a member of the referee community.

Lars Hagerman (Regional Editor, Marine Biology): Review work is heavy and time-consuming. Besides being a Regional Editor for Marine Biology I am also on the board of Oceanologia. The editorial work for Marine Biology is getting more and more time consuming. It takes at least 1 to 2 days per week. For the moment I have 17 mss for Marine Biology in circulation, sent out for reviewing or for revision. On top of that I have 4 mss I shall review for other journals. As an editor the main problem is to find reviewers who are willing to review the ms, who are good reviewers and who want to do it in the stipulated (= reasonable) time. And a good reviewer within her/his field cannot be used too many times! This also means that it is increasingly difficult to find good reviewers. Within certain research areas people work close together internationally and it is very difficult to find reviewers outside the group who have worked and published together. Reviewers should be acknowledged better so that their work can be credited in the same way as for instance supervision or teaching. I do not think author-paid reviews will help. There will quickly be problems where authors for various reasons cannot raise money. If reviewers should be paid, then payment should come from the journals but this will create other problems. Journals have different economic abilities and what about all the rejected mss (more than 50%). Many rejected mss are sent to other journals and they can then circulate in the system... I do not have a solution to the problem.

Peter Beninger (Universite de Nantes, France): Peer review is the best means available for quality control in science. As such, its role in the advancement of science is absolutely crucial. For those who review mss regularly, the costs of this activity are high. Even at the moderate rate of 2 mss per month, I'm still looking at an average of 100 days per year in which ms reviews are a major part of my preoccupations. In some cases, the process is considerably extended, such as when I recommend extensive revision and re-submission for further review, or when an editor connot make a decision. The benefits of reviewing are disproportionately minute compared to the costs. Reviewing mss has never helped me obtain either tenure or promotion, obtain release time or funding for research, or otherwise enhance my career Finding good, reliable reviewers is the most difficult problem for journal editors. Every editor I have met has confirmed this. There may be a tendency among some overly stressed reviewers to do 'bare bones' reviews. It seems to me that the solution lies in rewards. With the exception of society journals, which are essentially non-profit, scientific publishers are reaping profits from activities based largely on work they haven't had to pay for: that of the researchers themselves, that of the reviewers, and often that of the editors. If these people actually received honorariums for their contributions, journal prices would go through the roof However, that doesn't mean that no recognition is the only alternative.

My first recommendation would be that well-endowed scientific publishers show some recognition for this work, in the form of either yearly tokens of appreciation at least to editors and reviewers, or through the creation of research funds or scholarships to which scientists and their students might apply to assist in financing their work. It's always puzzled me how these publishers have overlooked such an obvious source of publicity and chance to generate a good image. This would be a win-win situation for everyone. Other tokens of appreciation could be complimentary journal copies, at least of the issues in which mss we've reviewed appear. Of course, the many excellent society journals do not have the resources to provide material tokens of appreciation, but most of them are already well-known for their efforts on behalf of their members, despite their often precarious budgetary situations.

My second recommendation would be that all editors send letters of appreciation to their reviewers at the year's end — so few of us actually ever receive praise from our administrative hierarchy, a gesture such as this would go a long way to renew our commitment to this voluntary activity. Even form letters, signed by a real editor's pen (not e-mail, there's a limit to how cheap you can get before a gesture becomes meaningless) would be a great morale-booster, and they may count as positive points for promotion and tenure.

With respect to the problem of reviewer overload I don't see it as inevitable. When time constraints make review impossible, we can send the ms to another colleague who we know will review it and who is just as capable of doing so. I go to great lengths to re-route mss this way, rather than turn them down completely. There is no reason to feel guilty about it; even re-routing a ms is a positive contribution. The present lack of recognition, at all levels, of the reviewer's conscientious activity, is doing a great disservice to science.

Alan Tessier (Associate Editor, Limnology and Oceanography): The core issue is the reward system: are the rewards of a reviewer consistent with the general reward system in science? Obviously not, time spent reviewing someone else's ms does not give as much reward as the same effort devoted to writing your own ms. But I argue they should not be equal nor even compared. They are measured in different currencies. Can I compare the reward of volunteering time to help out at a local elementary school, with what I could have gotten from more time devoted to my career? No, they satisfy different needs.

There is clearly prestige associated with being an Associate Editor and being an Editor-in-Chief brings greater prestige. The AE duties often include financial reward, e.g., subscription fees, payment, etc., and for some journals the job of Editor-in-Chief is a paid career. But a reviewer receives limited prestige — a small note on a CV counts little in an annual evaluation, and generally no financial reward or other perks. But we routinely reward individuals who feel no responsibility to act as reviewer themselves. We review and publish their papers, a powerful action that has a direct effect on their careers. The solution is simple. If you wish to publish in a journal, then you should be willing to review for that journal. As a general guideline, the required review rate should be close to the inverse of the acceptance rate times your submittal rate. So if I submit 1 ms every year to a journal I should agree to review at most 3 mss each year for that journal assuming an acceptance rate of about 33%. It should be taken for granted that this review rate will be less early in one's career and greater later on.

Carlos Duarte (Associate Editor, Limnology and Oceanography): Colleagues who repeatedly reject mss to review for a journal should not be allowed to publish in the journal. This is particularly important for a society journal, rather than for a product of a profit-oriented publisher. My number 1 concern is quality of reviews. It does require an effort to keep alert to new, promising members of the scientific community who have shown excellence in their work to avoid the danger of always using the same names. If we had the results from a hypothetical questionnaire on the number of reviews sent to different scientists, we would certainly find that it is rather skewed, with about 10% of the scientists receiving 80% or more of the reviews. The dangers are overload, and to effectively limit the development of the field by having a few people control what is published. I am particularly concerned about the latter, for I find that our community is overly conservative and that our biggest philosophical problem is the great resistance to new concepts and ideas.

John Raven (Associate Editor, Limnology and Oceanography; European Journal of Phycology; Journal of Phycology; Plant Cell and Environment): Refereeing should be better rewarded; the flip side is that not refereeing should be punished. However, I generally believe in carrots rather than sticks as motivators. I cannot see how a cash reward will work. I do not refuse payment for reviewing when it is (occasionally) offered (e.g. by certain East Asian journals) but it is not a major consideration. I cannot pay anyone to do what I should or would be doing were I not reviewing. The British Royal Society journals offer 50 free reprints of the next article a reviewer publishes in one of their journals. Again, not a major draw; offprints are perhaps rather archaic items in today's world of photocopiers and information technology. As for sticks, I am in general agreement with a 'no refereeing, no ms submission' rule.

Jack J. Middelburg (Associate Editor, Limnology and Oceanography): I limit my reviewing activity to 1 ms per week; that is, all other mss that arrive in the same week are either returned (explaining the reason) or passed on to one of my colleagues, unless the subject is highly specialized. I sometimes feel guilty when I must return mss, but reviewing 50 mss per year is enough for one person! Increasingly I pass mss for review to post-docs at our laboratory or at other institutes/universities. This also trains newcomers if the senior person agrees to look over the review to ensure that the tone is constructive and that nothing significant has been missed.

Paying reviewers and/or asking a fee from authors before consideration of a ms might indeed result in the submission of fewer, better prepared mss, but it may also induce a bias because rich, more senior scientists have more money than starting, young scientists. Paying reviewers could attract some scientists, but it

would not be a significant incentive for the most productive people, because they primarily lack time. One of the problems of our review system is that we do not fully exploit the entire community; no single person can be aware of the entire field.

Regarding the unwillingness of some senior scientists to review, I am in favor of a penalty/credit system. For instance, initially every scientist would have the right to publish 3 papers, but after that reviews (credits) must be gained to allow further submissions. The 3-ms credit buffer is to allow young scientists to get started. Technically this would not be such a big effort, because most (commercial) publishers already maintain a reviewer database with information on decline/acceptance of reviews, the turnover time, etc. The credit/penalty system should also apply to co-authors but with some attenuation. In this way, people at the top of the scientific pyramid will be required to fulfil their reviewing duties.

Michael Lesser (Associate Editor, Limnology and Oceanography): I agree that money is not the answer, but do think that: (1) active reviewers need to be recognized in some way, and (2) that penalizing those submitting mss to journals they refuse to review for is going to be a difficult sell. I'm in the camp that says this is part of the profession that we bought into (rather than pounding nails; also a noble profession!). The challenge is getting through to a new generation of scientists that reviewing is part of our professional responsibilities and is actually a tremendous opportunity to learn about new areas within our respective fields.

Antoine Grémare (Observatoire Oceanologique de Banyuls, France) The review by qualified scientists appears to me as the only suitable way of selecting (but also often improving) mss. The reason for this is that there are no quantitative criteria to judge new research work. You simply rely on the expertise of selected scientists. Given the high level of specialization and the necessary maturation time, there are only a few such experts per research field. I agree that peer-review has worked well until recently. However, during the last decade, there has been a great increase in the number of submitted mss. This increase mostly resulted from the necessity of publishing in order to get a permanent position and/or to progress in the career This trend may prove dangerous to the whole system. In spite of the effort made by the referees, there are more and more questionable published papers. How do we improve the peer-review process? I have no real response to this. Paying referees is probably not the solution. This would not save time and may have serious drawbacks. On the other hand, a benefit of the review system is the rapid access to new scientific works, which may occasionally influence or even improve the reviewers' own research.

Jon Cole (Associate Editor, Limnology and Oceanography): My own take is one of 'if it ain't broke don't fix it' The existing peer review system does seem to work remarkably well. The review load is large but not unrealistic. Consider a productive researcher who publishes 4 first authored papers per year Assume a journal rejection rate of 50% and assume that 3 reviewers read each paper. So, this one scientist caused $2 \times 4 \times 3 = 24$ reviews to occur in one year. I would assert that this productive person owes the system roughly 24 reviews. A more normal output of 2 first authored papers per year creates a review debt of 12. If we had a mechanism to spread the review process equitably over the community, the burden for the average scientist ought to be around 10 reviews per year (my guess). Recognition for this work needs to occur better than it does in most places. This recognition should come from one's own institution. Thus, we need to educate deans, department chairmen and directors that there is in fact a great deal of prestige in having these significant gatekeepers of science on their staffs.

Ole Naesbye Larsen (Departmental Chairman, Institute of Biology, Odense University): The annual report of Odense University lists faculty members' scientific endeavours as regards publications and other activities such as organizing and attending meetings, field work, and journal affiliations as editors or editorial board members. I have suggested to the Dean that we extend this list by incorporating peer-reviewing activities in the form 'In 2000 NN reviewed 35 mss for Nature, 47 for Science, and 2 for Limnology and Oceanography'. This would recognize and give the official stamp to an otherwise extremely important but often overlooked aspect of the scientific process. Many scientists already list the journals they are reviewing for on their home pages. But anyone can list anything on his home page.

Ideally, peer-reviewing should act as a quality control system rejecting mss with low news value or inferior methodology but improving and recommending important mss. The underlying assumption is the existence of a universal concept of scientific importance. But is it true, for instance, that a study is unimportant if its findings are negative or its news value is low? In medicine several studies must reach the same conclusion before a new concept is approved. In biology, however, repetitive studies are unheard of. This makes the system very conservative. I have found that the few times I had something really new to report the referees gave me an extremely hard time, while bread-and-butter mss passed the system with only minor modifications. I think these issues need to be addressed in a re-assessment of the peer-review system. I do not believe in payment for reviewing unless in the form practised by the Royal Society, which offers 50 extra free offprints of the next paper the referee publishes in

Proceedings or Transactions. Alternatively, commercial journal editors could possibly be urged to award a lifelong free subscription to the journal after say, 100 quality referee reports.

Jan J. Beukema (Editor, Journal of Sea Research): Commenting from my own experience (15 yr as an editor and >30 yr as a reviewer), I would in the first place stress the importance of the peer-review system to maintain the quality level of papers published in refereed periodicals. Therefore, the emphasis in the present discussion should be on the maintenance and strengthening of review procedures, i.e., which factors contribute to speed, quality and continuity of the refereeing process. Editors receive review reports of highly variable quality, bearing the marks of strongly different efforts. Some of the reviewers feel their responsibility to editors and authors and do a thorough job, often taking half a day or more of their valuable time. Others confine themselves to stating a simple and time-saving advice such as 'interesting paper' or 'publish as it is' on mss with several shortcomings. Mss should be reviewed by at least 3 experts and even then an element of chance cannot be excluded. Of course, such elements are restricted by the critical attitude of editors who are aware of the peculiarities of their advisers.

Editors cherish reviewers who deliver balanced and constructive comments. With a minimal number of 3 referees per ms, they have to bother benevolent experts several times a year. I try to avoid bothering any colleague more than 3 times a year. It is a wise practice for editorial offices to keep a record of the burden of each reviewer. Editors can restrict the inconvenience by proper selection of both submitted mss (returning prospectless mss immediately to the author) and intended reviewers (inviting only colleagues who are expected to be really interested in the content of the ms). Similarly, referees can protect themselves against overloading by being selective (accepting only mss well within their field of expertise) and restrictive (accepting only mss from periodicals to which he/she feels some affinity by earlier or intended submissions of their own mss).

At present, most periodicals employ a large number of associate- or co-editors who are often called upon to evaluate mss. They receive free copies of the periodical and are further rewarded by the prestige of being a member of the editorial board. Editors keep an eye on the quality of referee reports and are likely to invite the best new reviewers to join their board (and take leave of failing board members). The advantages of a ms evaluation system in which a large editorial board plays a substantial role are numerous and clear. Such a system which is mutually advantageous to editors and reviewers will be tenable and beneficial to all parties concerned: publishers, editorial offices, reviewers and authors. It will speed up and improve the refereeing procedure without much additional office work. Any reviewer can benefit by ultimately becoming an active and recognized member of a true expert system.

Karsten Reise (Review Editor, Marine Ecology Progress Series; Editorial Board, Journal of Sea Research; Field Editor, Helgoland Marine Research): Perhaps there is no way out. We have to rely on a good deal of idealism. One cannot enforce quality and fair judgement by a referee. I have learned from almost all mss that I have refereed at least something. Thus, there is also a little benefit from doing this work. Senior scientists should more often involve qualified younger scientists of their group in the review process.

Don Canfield (Associate Editor, American Journal of Science; Limnology and Oceanography): The review process is a source of great frustration to many scientists and journal editors. Nothing takes the joy out of my week like arriving Monday morning and finding 3 new mss to review, to place on top of the 3 as yet unreviewed mss received in the previous few weeks. Until last year I dutifully reviewed all mss and proposals sent to me. I have made a hard decision, quite similar to yours. That is, there are only a certain number of journals I will now routinely review for. These are the journals that I most respect and in which I normally publish. In this way, I 'payback' to the journals that I use, and I am comfortable with this decision. Still, I review far more than the 2 to 3 reviews that might be expected per first-authored paper that I publish. I find that I do turn back some mss from my favorite journals depending on other reviewing commitments at the time. I am also comfortable with this. One step that journal editors could make in easing the reviewing process is to consider mss for language, content, and novelty, before sending them out to review (this is routinely done at Limnology and Oceanography). Some mss may not pass this test, and these should be returned to the author with a polite note expressing why. A rejection rate without review of 10 to 15% seems realistic to me for most journals. I acknowledge the reviewing process as essential to quality control and I value reviewers' comments in making my own editorial decisions on mss. As an author I value the criticisms and comments from my colleagues. Especially the negative comments have improved my papers, and in some cases have forced me to reconsider my own results. Far above any financial reward or other compensation or acknowledgement for my own reviewing, the reviews of my own mss are payback enough for me.

Otto Kinne (Editor, Marine Ecology Progress Series; Marine Biology; Diseases of Aquatic Organisms): There is a tremendous demand for more science. We wish and need to know more about the world in which we live and we must make sure that our children will

have a chance to live in a world which modern societies are modifying and degrading. Indeed, science is growing — and with it a flood of new information. Next to producing information the control of its quality is of paramount significance; in fact, quality control has never been more important than today. Only critically screened, selected and tested information can deepen our insights, assist in solving our problems, promote advancement of science and keep the increasing information soup digestible.

Quality control and improvement are the responsibilities of qualified scientists, not least of editors and reviewers (referees). Politicians, administrators and other leaders engaged in financing and guiding science should understand and honour the fact that the work of editors and reviewers is an essential and crucial part of the scientific process; it must be seen and supported as part of a scientist's basic duties, just as research (and teaching). And publishers must seek out better means of compensating editors and reviewers. Many editors and reviewers selflessly invest a lot of work, much dedication and a considerable amount of their own time. It is unfair to continue to rely on this side of the medal without adequate support from the other

Provided both sides accept their responsibilities for strengthening quality control and improvement, there still remains the problem of finding a sufficient number of qualified and reliable scientists willing to make themselves available as reviewer I am pleased to see myself in agreement with other contributors to this Theme Section (Gerry Quinn, Everett Fee, Alan Tessier, Carlos Duarte, John Raven, Jack Middelburg): authors who consistently refuse to participate in the review process without convincing arguments cannot expect their own mss to be reviewed by others. Their mss should be rejected and returned without review. Of course, such a practice can work effectively only if the editors — at least those of the major journals — co-operate. I am calling here for organizing such co-operation.

Quality controllers must respect different perspectives and diverging or contrasting findings, facts and interpretations — as long as these are derived from solid grounds. Science gains and matures from intellectual freedom and diversity; it draws from a variety of talents, expertises and experiences. It is the responsibility of editors to avoid degeneration of quality control into opinion control and to resist attempts at censorship. This is why Inter-Research Journals engage at least 3, often up to 6 or even more reviewers for 1 ms. We make the full text of reviews (anonymously) available to the authors and ask them to accommodate criticism or convincingly refute it. Authors may protest reviewer and editor actions, and they may argue against ms rejection. Hence 1 ms may undergo several re-reviews. Our carefully trained copy editors and typesetters are known to provide assistance to authors whose ms is accepted and to help those whose mother tongue is not English. All this is time-consuming and quite costly, but in our opinion it is the best way to support authors and to avoid misuse of the powers of reviewers and editors.

SUMMARY

It is up to the reader to assess the opinions expressed above and to form their own. I will, however, attempt to summarize and emphasize some points that recur in the contributions.

One source of the problem seems to be the 'publish-or-perish syndrome,' which exists for 2 reasons: the desire of the individual researcher to achieve excellence in his/her chosen field, and the need for an objective standard on which hiring and promotion of scientific staff can be based. The result is an explosion in the number of submitted mss.

Scientists who consistently refuse to review mss should not expect to have their mss reviewed by others. For journals published by commercial firms it would not seem unreasonable to pay reviewers. Although scientific publishers are reaping profits from the unpaid work of referees, some people fear that payment of honorariums would make journal prices increase even further and suggest that compensation should take other forms, e.g., by providing free copies of the journal or endowing research funds or scholarships.

Review work reduces the output of the referee's own research. A time estimate of 15 to 20% seems to be realistic for many referees, although some referees who also act as editors may spend considerably more time on editorial work. Several referees do not believe that the review system is ready for re-assessment, but rather that it needs support and an injection of enthusiasm. Scientists must participate in the referee system. Most colleagues appreciate a good referee's efforts and a referee may feel a genuine sense of satisfaction in knowing that the quality of the journal was improved by his/her efforts. Many referees agree that it can be a pleasure to be a referee for a fine ms.

Over an entire career a scientist should on average review about 3 times as many mss as he or she submits; somewhat less when young, somewhat more, when established. It is fair to decline to review more mss than that. Refereeing mss is a task that scientists have to accept as part of their job.

Using a new 'global-reviewer-database' journal editors could store information about their referees' cur-

rent reviewing status and joint co-operation could perhaps eliminate the worst problems of reviewer overload. With modern technology it is conceivable that all relevant information could be stored on the Web in a form accessible to all journals.

It is unreasonable to expect extensive use of referees without compensation, but most referees reject the idea of payment for doing reviews. Various reasons were cited: Payment would not make additional time available for the referee; it will not reduce the overall review load; and research granting bodies would not allow funds to be used for this purpose.

Acting as a reviewer and serving on editorial boards should be recognized as an essential part of the scientific process. Nevertheless there is a lack of examples where reviewing mss has helped a referee obtain tenure, promotion, free time, or research funding. The contents of this Theme Section have convinced me that the peer-review system is ready for critical re-assessment.

Acknowledgements. This text has been reviewed by all the experienced referees mentioned. They deserve more than a pat on the shoulder. I highly appreciate Everett Fee's first and Otto Kinne's second crack at polishing the present TS.

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The text of this Theme Section (TS) is available at the Inter-Research web site (www.int-res.com/abstracts/meps/v192/p305-313.html). The number of visits to Inter-Research web pages is increasing impressively (presently ca 165 000 successful requests and 73 000 pages per month). The TS will therefore become known rapidly and widely.

In view of the great significance for science of this TS we wish to keep the discussion going and have installed the Forum 'Peer-Review System' at www.int-res.com/forum/peer_review. Contributions to this Forum should be sent to Hans Ulrik Riisgård, MEPS Contributing Editor, e-mail: hur@biology.ou.dk, fax: +45 65321433