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THEME SECTION

Quality in science publishing

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Introduction

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The purpose of this Theme Section (TS) is to discuss various aspects of quality, and quality assurance, in science publishing. While we will each have our own views about exactly what this means, the editorial policy statement of the Council of Science Editors uses the following adjectives in reference to the quality of a scientific journal's content: accurate, valid, reliable, credible, authoritative, relevant to the journal's scope and mission, readable, and comprehensible. A standard dictionary definition of quality control is: '…a method of establishing and maintaining a high level of quality in a product or process through careful organizing, constant checking and painstaking corrections.' In this context, most of us would probably agree that the publisher, the editorial board, the reviewers and (not least) the authors are all involved in creating, defining, measuring and maintaining quality. Thus, we asked a cross-section of people with experience of one or more aspects of science publishing to present their views on the theme. The main points of consensus that emerge from the essays follow.

Most contributors identified the importance of good scientific writing. When a manuscript has not been well written by the author(s), thorough and professionally astute editing is a necessity. This requires great care to preserve the exactness of the text despite possibly significant revisions, restructuring, and/or deletion of what is unsuitable, superfluous and/or badly written. This task is shared by the reviewers of the manuscript's scientific content, and by copy editors. The former focus on what is, or is not, essential to the scientific story being told, and the latter on the clarity of the prose and the correctness of the grammar and syntax. This process takes time, but it is worth waiting for if the result is something that will be more easily read. This is the essence of *production quality*.

The fundamental importance of good scientific judgement in deciding a manuscript's fate was mentioned by all contributors. This crucial decision must be taken by a hands-on Editor who is an acknowledged authority in the subject area of any particular article, and who is a respected scientist capable and courageous enough to arbitrate openly and fairly when confronted with divergent views among authors and reviewers. The sheer number of manuscripts received by our discipline's most successful scholarly journals (over 700 per year at MEPS alone), and the wondrous breadth of their subject matter, makes this an extremely challenging task. The views expressed here suggest that the 'Subject Editors plus Editor-in-Chief' model is probably the best way of assuring *scientific quality*, as well as being more transparent and accountable than alternative models of editorial decision making. Authors themselves are best positioned to decide upon the most appropriate Subject Editor to handle the evaluation process. There should be a sufficient number of Subject Editors to cover the journal's scope: no more, lest they become idle and their title meaningless, and no less, to ensure that the workload is reasonably distributed and that they are only responsible for papers that fall within their own field of expertise. Subject Editors must be actively engaged in the review process, able to dissect the arguments of authors and reviewers alike and to provide guidance and leadership, and thus arrive at decisions that are transparent and well founded. Their decisions should still be open to appeal to an Editor-in-Chief, a person of recognised eminence and broad experience, and that decision would be final. The Editor-in-Chief, presumably in consultation with the rest of the Editorial Board, and the Publisher, would set a common Editorial policy and ensure that it is consistently applied. This system is not yet widely applied in marine science publishing, but it is the most common decision-making framework

*Contributors are presented in alphabetical order

for biomedical journals. MEPS itself operates under a model that has many of these elements.

While views on future prospects in electronic and 'open access' publishing differ, there is consensus on the continuing need for an effective peer review system, to control and enhance the quality of the published product and to help scientists identify articles that are worth reading. Electronic publishing has made it simple for scientists to instantaneously disseminate their work across the globe, but readers must be wary of work that has not been peer-reviewed prior to publication. Paradoxically, if scientists choose to publish their work on the internet, without submission to a recognized journal, they may never reach their intended audience if that audience prefers to scan tables of journal contents each month rather than sift through the results of internet searches that produce irreproducible (over time) results. Unquestionably, as a distribution medium, the internet is unrivalled. What we see in the essays that follow is that the process of quality enhancement through peer review, and the collection of related papers into journal form (whether in print or online), is still respected; any new technological development(s) in science publishing should serve rather than subvert this process.

We hope that readers find these essays thought provoking, and that this TS will increase our profession's resolve towards producing and maintaining the highest standard of quality in science publishing, at all definitional levels. We have thoroughly enjoyed putting it together and are grateful to the contributors for their thoughtful and eloquent essays. The content of these essays should also result in a broader and deeper recognition of the immense contribution made by the staff of Inter-Research, who all work conscientiously in tireless devotion to this journal. Finally, we thank 'The Professor', Otto Kinne, Editor and Publisher of *Marine Ecology Progress Series*, for encouraging us to develop this TS and for his unparalleled role in modern marine science.

Between the Scylla of hidebound conservatism and the Charybdis of mindless speculation

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The 'quality' of a scientific publication is not an absolute but must be assessed in relation to a journal's mission. It should be judged primarily by its disciplined intellectual rigor, bearing in mind what course the publication aims to take, as between the Scylla of hidebound conservatism and the Charybdis of mindless speculation.

One commonly applied measure of the 'quality' of a periodical, perhaps most overtly in the social sciences, is its rejection rate: the higher that rate, the higher the presumed quality. In the natural sciences, a somewhat similar criterion is invoked, perhaps less overtly and certainly less quantitatively, when kudos comes for being published in journals 'hard to get into', such as *Nature* or *Science* or the *Journal of the American Chemical Society*. But consider the professed, or implicitly taken-for-granted, aim of these 'top' journals. Actually, there are 2 aims: that what gets published should be ground breaking; and that what gets published should not be in error. But it seems not to be commonly understood that these aims are incompatible. The first implies a willingness to be often wrong, at least to some degree, because it is always difficult to judge the validity of something that is without precedent. On the other hand, the second places high barriers in the way of anything so novel as to call into question ideas that have hitherto been widely accepted. Between these 2 incompatible aims, no journal can avoid making its own choice, at least implicitly, in which direction to lean. The judgment of a journal's quality should then be based upon how well it performs its chosen task, not according to whether one agrees or differs with the journal's aim of emphasizing novelty over reliability or vice versa.

The history of science offers ample illustrations that the time needs to be ripe for any given advance to be accepted by the conventional wisdom of the mainstream community. Truly novel scientific claims are at first typically resisted, even though some of them later turn out to be genuine advances. Some such claims have been highly premature, that is to say decades ahead of their contemporary *Zeitgeist*, for instance Wegener's continental drift or Mendel's laws of heredity (Barber 1961, Bauer 2001, Hook 2002). Novel claims may have to do with data or facts; or with new means for obtaining data; or with some new way of looking at the data. Normal progress in science involves the accumulation of information in the absence of startlingly novel claims: most scientific work adds detail without upsetting the existing body of data, methods, and theories (Kuhn 1970). Scientific revolutions involve something strikingly contrarian in at least one of those 3 aspects (Bauer 2001).

History teaches that much of what we publish will turn out to be flawed in some way. Given that we cannot always be right, the journal I edit is deliberately open to far-ranging claims, willing to be often wrong in order to grant a hearing to topics of which only a few are likely to

bear fruit, but believing that those will likely have a major impact once they become accepted in the mainstream. So, for us, 'quality' has to do with methodological issues and not with immediate factual correctness: Does the presented evidence come from sources that are not obviously unreliable? If experiments were performed, were protocols and controls appropriate? Does the discussion of the possible import of the evidence respect principles of logic and consistency?

The primary, perhaps only, safeguard of quality is the day-to-day decision-making by the editor(s). If a journal's explicit policies are to be realized in practice, not only the editors but also the manuscript reviewers must deliberately abide by them. An editor's responsibility is, first of all, to make a good choice of reviewers, people with relevant competence and who are likely to judge the validity of the evidence and the soundness of the discussion without being unduly prejudiced by their own prior beliefs. That is no mean task, for none of us can be entirely free of bias toward what we believe to know. Moreover, as the popular aphorism has it, a totally open mind would let the brain fall out; or, as Chesterton (1936) understood, an open mind has the same function as an open mouth, namely to shut itself again on something solid. So, every potential reviewer who knows *anything* will, thereby, have a bias against *something*, and any potential reviewer who has no biases is likely to be lacking in relevant knowledge.

Therefore, an editor's responsibility to exercise judgment begins rather than ends with the choice of reviewers. There is, after all, no law that reviewers' judgments must be accepted. Many of us have in our files copious illustrations that editors should have overruled reviewers who got their facts wrong, or who offered their own interpretations as the only legitimate ones, or who vented personal spleen, and so on. If an editor lets reviewers get away with such unwarranted critiques, then that represents a low quality of editorial performance. Authors of papers have a right to expect that reviewers should be held to the same standards of sound knowledge and valid logic as is expected of the authors of manuscripts. It is an editor's responsibility to hold all concerned to those standards.

The rules of the game in science publishing

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In the game of communicating science, it is routinely assumed that players know both the overall rules of the game, and the rules specific to the position(s) that they are playing. I herein assert that this false assumption underlies many of the problems associated with achieving and maintaining quality in science publishing.

All of my professional experience leads me to the uncomfortable conclusion that too few of our colleagues have received explicit and thorough instruction in their roles as authors, peer reviewers, arbitrators, or editors. In July 1983, Brian Marcotte (my Masters advisor) asked me to assist him in assessing a manuscript that he had received for review. Using that manuscript, and several others that arrived thereafter, Brian patiently and conscientiously instructed me in my role as a reviewer, and 'peer reviewed' my peer reviews. He also spent a great deal of time and effort teaching me about scientific writing, the preparation of illustrations, and the critical choice of which scholarly outlet was best suited to an article's subject matter. This anecdote illustrates that, for each and every position that we play in the game of science publishing, achieving the highest degree of skill requires intense training from a competent and experienced mentor. We must also recognize that, just because we can play one of the game's positions with great skill, does not mean that we can play all of them equally well. At least not without training anew, each time a new position is taken up. Further, the skills required to produce a quality product, or to assess the product's quality, must be continually upgraded and honed, throughout our careers. Complacency and quality are incompatible.

In addition, in order to assure an even playing field, we must all conduct ourselves according to an explicit, easily accessible, widely accepted and routinely taught 'rules of the game'. For many reasons — which are beyond the scope of this TS — the degree to which we achieve all of this is limited, and highly variable. Following from this, the enormous range in the level of competence that we all encounter during the process of publishing articles in the scientific literature should come as no surprise.

A more insidious contributor to the uneven level of competence exhibited at all 'positions' in the publishing game is the ubiquitous psychological phenomenon of being 'unskilled and unaware of it' (sensu Kruger & Dunning 1999, Dunning et al. 2003, Edwards et al. 2003). To a highly variable extent, we all carry what Kruger & Dunning (1999) refer to as 'the dual burden': the very fact that we are unskilled at some task (and/or incompletely aware of the rules) leaves us unable to realistically judge both our own performances, and those of others. In the context of the preceding paragraphs, sufferance under this dual burden may most often reflect only that the players on the team are poorly trained. Several contributors to this TS recount

anecdotes that are fully consistent with these contentions; how an author, a reviewer, or an editor did not seem to know (or at least did not do) their job. I have no doubt that everyone who reads this would be able to contribute their own such anecdote(s).

In an attempt to bolster the assertions laid down above, I conducted an informal survey of colleagues who have been editors or editorial board members of aquatic science journals. I asked if they were familiar with the details of the Editorial Policy Statements approved by the Board of Directors of the Council of Science Editors (CSE; www.councilscienceeditors.org). I also asked if these guidelines had been formally and explicitly discussed with them at the time that they were recruited as editors. The majority had never been members of the CSE, and some were unaware of its existence. Only a minority were familiar with the material in the CSE guidelines, and/or had openly discussed such issues prior to putting on the editor's cap. While this is clearly not a 'scientific' poll (I admit to being an unskilled poll taker), it is instructively and disturbingly consistent with the assertions made above. I contend that the results would be similar if an analogous survey were conducted of authors and reviewers.

We *can* take steps to improve the situation. As a start, everyone involved in publishing science, and particularly those mentoring students, should familiarize themselves with the guidelines for authors, reviewers, and editors set out by the CSE. The CSE also makes available, and/or recommends, instructional resources for authors, reviewers and editors. Several of the contributions to this TS make thoughtful suggestions along these lines, as has Otto Kinne (1988). We can also all take it upon ourselves to more routinely discuss with our students and colleagues the fundamental nature of the various roles in science publishing, and the ethics surrounding each. The CSE takes up many of these. On a more specific and case-by-case basis, each of us can, and should, make it clear to authors, reviewers and editors when they have clearly not exhibited an appropriate level of skill, or have engaged in unsportsmanlike conduct. Editors can routinely make reviewers aware of the weaknesses and strengths of their critiques by sending all of the reviews of a given manuscript (and the basis for the decision on the manuscript's fate) to everyone involved, and not only to the authors. The manner in which this is done should always be constructive. Finally, the team's players should never be overworked, as this can only result in poorer play.

The highest standard of quality in science publishing can only be achieved when every member of the team knows their role and plays it with experience, skill, and dedication.

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Achieving and maintaining quality in scientific publishing

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Some of what we see published in scientific journals is of excellent quality, is widely read and highly cited, and proves valuable for years or decades after it is published. A lot more, however, while reporting on sound and carefully conducted experiments, is not read by many scientists, ends up rarely being cited by others, and is soon forgotten. What is the difference between these types of papers? What can editors do to identify important, citable submissions? What is the role of all the participants in the publishing process to help ensure that a journal publishes only the best manuscripts?

There are no easy answers to these questions, but there are a lot of things editors and publishers can do to improve the quality of their journals. These range from things that can be done to attract the very best manuscripts from the best scientists to the procedures that are used to vet those manuscripts, make the best decisions, and provide authors with the best service possible.

Publishers. What can the publisher do to help his journal and editor attract the best papers? Publishers can ensure that they put out a quality product in a timely fashion. Authors look for and expect journals to come out when they say they will; they have little patience with journals whose issues are frequently late. Similarly, authors expect that their manuscripts will appear well copy-edited and proofread, with attractively laid out figures and tables, in an attractive finished product, whether that is a hard-copy journal or an electronic journal. Readers, who, after all, are potential authors for the journal, expect to find well written and edited text, useful supplementary material, all put together in an attractive package that makes up each issue of the journal. They don't have patience with inaccurate citations in a paper's literature cited, and they find it frustrating and time-consuming to correct mistakes while they are reading.

In addition to producing a quality product, publishers should ensure that the offices of their editors are sufficiently well supported so that they can do their jobs well. This does not mean that they must work in luxurious surroundings, but they must have an adequate budget to handle the peer review process efficiently, well, and in a timely fashion, with some funds for soliciting manuscripts and for working with authors and readers.

Editors and Editorial Boards. Editors set the tone for a journal and largely determine what will be published. Their reputation and all of their actions, from choosing editorial board members and referees to acceptance or rejection of each manuscript, will loudly and clearly set the tone for the journal. If they expect to attract the best manuscripts from the best scientists around the world, they will have to ensure they appoint some of the best and most respected scientists to serve on their editorial board. Careful consideration should be given to each and every editorial board member appointed. If your journal is an international journal (or you want it to be one) then you should search the world for the very best scientists in each discipline or sub-discipline for your editorial board. A careful and wise selection of editorial board members is probably the best way to begin the process of attracting the very best manuscripts from the best scientists in whatever disciplines the journal is interested in. With a good flow of excellent manuscripts, an editor will find it much easier to achieve and maintain a quality journal. However, it will still take a lot of time and effort to identify the best potential associate editors and then to persuade them to accept your offer to be associated with your journal. Also, it is not something you can do once and then forget about. You will have to continually and regularly review your board and replace those members who are retiring and add new and fresh names from other disciplines or areas you want represented in the journal. These board members also can be used in a variety of other ways, of course, and for different tasks. You may run a decentralized operation, and these board members may receive submissions directly from authors and handle the entire peer review system within their sub-discipline of the journal. In a more centralized journal, you may ask for their advice about particular manuscripts or have them help select referees. In some journals, they may meet regularly and often to vet each manuscript after peer reviews are received.

Another very effective action that an editor can take to attract new authors and their best manuscripts is to attend the annual meetings in the disciplines his/her journal covers. After hearing a particularly good talk about some exciting topic, talk to the scientist and indicate you are interested in seeing their best manuscripts. I have found this to be quite effective, and sometimes even found that potential authors were unaware that my journal would be interested in papers in that discipline. An editor also can see if a society will let him/her speak briefly at its business meeting, presenting data about past submissions, statistics on timeliness, and an invitation for new submissions.

Once they are dealing with submitted manuscripts, one of the most important tasks of editors (or, if delegated to them, of associate editors or other editorial board members) is the selection of referees to provide input and advice. Most editors probably spend too little time on this important task, possibly simply because they are oversubscribed. However, the editor's later task in deciding whether to accept or reject a particular manuscript will be made much easier if high quality referees who are qualified and able to give advice are initially selected. Spend some time at it. Spend time to ensure that you understand the manuscript sufficiently well so that you can determine the specific sub-discipline from which to choose referees. Maintain a good database of referees, so that you can keep track of them not only by discipline and history of use, but also by the quality of past service to the journal. Then hope they perform as well as they can and do provide good, useful advice, and especially that they comment critically on the novelty of the manuscript and how far it advances the science.

Once the referees have provided their advice, however, the editor faces a most critical task. Ultimately, the editor is the decision maker (the fabled role as gate keeper). He/she must decide what goes into their journal. An editor decides on acceptance, although part of that responsibility could be delegated to associate editors. Referees, however, only provide advice to the editor; they do not decide on acceptance of manuscripts. Referees may provide great and insightful advice or shallow and useless advice, but it is up to the editor to consider and evaluate that advice and make the decision on acceptance. That acceptance or rejection, repeated over and over again with each manuscript that an editor handles, will cumulatively establish the quality and reputation of the journal.

What manuscripts should an editor accept? One of the most effective ways that an editor can influence the quality of his/her journal is by their selection of novel manuscripts that advance the science. While I was editor of a journal, I used to encourage my associate editors, and now I encourage the editors of all of our journals, to look long and hard at the novelty of the manuscripts they handled. I didn't want the journal to publish reports of solid, well-conducted research, if that research represented pedestrian science, the results of which only confirmed what had been

reported in a dozen previous papers. I encouraged the associate editors to recommend the rejection of such manuscripts and instead be more willing to recommend the acceptance of manuscripts that were novel, and opened up new areas for our readers, even if those manuscripts contradicted current ideas or gospel. This is not easy to do and it certainly involves an incremental process. Remember, however, that novelty can come in many guises. For example, a manuscript may approach a problem in a novel way. The sampling strategy may be novel or the sampling may be particularly comprehensive. An author may use a novel technique to address the problem. A manuscript may have a particularly elegant analysis, or its conclusions and implications may be especially insightful.

Must one only publish novel manuscripts to make a journal interesting and attractive to readers and potential authors? Of course not! What one does want, however, is for your readers to be interested, informed, and challenged by what you publish. Manuscripts that confirm established wisdom or incrementally add to the body of knowledge may be important even to a journal at the cutting edge. That will be up to you and your editorial board to decide. Confirmation of old ideas, particularly if it is done critically and elegantly, is an important part of science. And remember that it is particularly difficult to reject sound but otherwise pedestrian manuscripts. However, an unrelenting search for the novel and exciting manuscripts will likely pay off in expanded readership and the receipt of even more excellent manuscripts.

What else can an editor do? He/she can solicit critical reviews of topics in emerging fields. These are well received by readers, especially students and established researchers who are shifting directions. They are also frequently highly cited, so that even more scientists and potential authors become aware of your journal. None of these suggestions will turn a journal around or increase its quality overnight. But by working on several of them over time with some patience, an editor is likely to see marked improvements.

The role of printers and publishers in 21st century scholarly publishing

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This essay was written from the perspective of a printer with more than 30 yr experience in manufacturing scholarly journals. I discuss whether, in the 21st century, a printer or publisher can still add value to the dissemination of scientific and scholarly information. Following a brief history of journal production, I consider the impact of recent technology and how the process has changed in the last decade or so. I conclude that, while less value can be added now than was possible in times past, the printer and publisher each still can add quality to the information being presented, increasing its usefulness and making it easier for the user community to access.

The French *Journal des Scavans* and the British *Philosophical Transactions of the Royal Society of London,* the world's first scientific journals, were both established in 1665, a little more than 200 yr after Gutenberg had been credited with the invention of movable type. Prior to Gutenberg's invention, information was difficult to obtain, even by people who were wealthy and literate. Since books had to be copied by hand, mass production of scholarly treatises was impossible. Distribution was also slow and cumbersome. Whether the world was ready for an invention like movable type, or movable type itself transformed the dissemination of information, is open to debate. Nonetheless, by the time the early journals found their way into printed form, printers were able to set metal type by hand letter by letter, and print many copies of a publication from one instance of the original 'template', thereby making it possible for many people to access the same document in a relatively affordable and convenient manner.

In even earlier times scholarly information was communicated from one scholar to another mainly by personal letter. Letters were delivered by couriers on foot or horseback, so one can imagine that the spread of knowledge moved at a slow pace. Each academician was responsible for the veracity and quality of his own composition. As long as the number of scholars remained small, their ability to communicate with each other on a one to one basis was adequate. But with widespread literacy and the propagation of universities throughout Europe came a demand for printed production of scholarly documents.

For purely mechanical reasons, it would have been very difficult in the 17th century for a scholar to publish his own journal. The typesetting and printing crafts required specialized skills and equipment. The handset type was arranged in a type case in a pattern that only printers were familiar with. The letters themselves were mirror images, since they had to produce a right-reading version when inked and applied to paper, thus the admonition 'mind your p's and q's'. Illustrations, if any, were hand engraved on wooden blocks by artists, the first scientific illustrators. Printing and binding were crafts which took tradesmen several years of apprenticeship to be considered qualified to undertake. In those days, the value a printer

could add to the publication process was unquestioned.

The early printers were also publishers.[1] In an effort to control the publishing process and prevent piracy, which was rampant at the time, printers campaigned to establish ownership rights for texts, whence came the concepts of intellectual property and copyrights. If an author could be persuaded to transfer ownership or copyright of his text to the printer/publisher, the author would be prevented from selling his information to a second or third publisher, and piracy of the information by another publisher became illegal, although this did little to prevent information theft in the early days of publishing. Eventually, however, the rights of authors and publishers became protected, which provided ways for them to be remunerated for their mutual efforts.

Entrepreneurial printer/publishers soon found ways to add other kinds of value to the publishing venture. Printers provided proofreading and redaction of the manuscripts. As the market for the literature expanded, publishers became responsible for finding buyers for the journals. Printers established distribution networks for their journals, as well as accounting procedures that led to rudimentary subscription management. But mainly it was the difficulty of acquiring the skills and equipment necessary to produce journals that kept the printer in the journal business and kept others out of it.

Printing technology advanced during the 500 yr from the 1450s to the 1950s, but not much. Type could be set faster on a Linotype machine than by hand, and printing was done more efficiently on the modern presses of the time, but the process, which still entailed creating a reverse image in metal that was inked and applied to the paper, continued to require specialized skill and a considerable capital investment in machines. Like his predecessors, the printer in the mid-20th century was essential to scientific publishing. Type was set by Linotype or Monotype operators in hot metal from hard copy manuscripts provided by the authors. Since every paper was re-keyboarded, errors were introduced, and the printer was responsible for careful proofreading; in many cases the typesetters and proofreaders knew the journal style better than the journal editors.

Tables and mathematics posed special problems to the typesetters of the time. Since the type was set on lines or 'slugs' of lead, special modifications to the machines were needed to permit large characters such as sigmas or integral symbols to overhang the slugs above and below. Forcing information in tables to line up in columns was also difficult. Thus, as in the early days of printing, highly specialized equipment and operator skill restricted the ability to print journals to a very few companies.

Illustrations were printed from thin zinc or copper plates mounted on blocks of wood. Scientific illustrations required finer line screens than conventional printing. Making these 'engravings' or 'cuts' was more difficult for journals than for other kinds of printing. Printing them required more effort as well, since the fine screens tended to plug with ink on the press. Illustrations that had been prepared painstakingly by scientific and medical illustrators were treated with a great deal of respect by the printer. One of my first jobs at Allen Press was to check over illustrations for broken lines and letters and the like, before sending them out for engraving. We signaled the problems to the engraver using light pencil markings on a piece of tissue paper we taped to each illustration. As discussed below, this level of quality control viewed in the context of 21st century technology and processes seems overzealous if not gratuitous.

Publishing diverged gradually from printing after 1850, but even in the early 1970s I can remember visiting the University of Chicago Press, which still had its own full-scale printing operation. During the late 1800s and early 1900s the publishers of scientific information became divided into 3 broad categories: the not-for-profit learned societies, the University presses, and the commercial publishers. In some cases the societies teamed up with one of the other 2 groups in a cooperative publishing venture, but the 3 camps became distinct from one another and each had its own niche and way of doing business. In general, the not-for-profit societies were run by volunteers and had unpaid editors. The University presses also worked mostly with unpaid editors, but their operations had higher overhead costs in terms of staff and infrastructure, which they could justify by giving a journal the imprimatur of a highly regarded press. The commercial publishers tended to pay their editors and offer free reprints to authors, thus attracting some of the best editors and authors to their journals, and established highly effective marketing platforms in order to court institutional subscribers willing to pay high prices for journals of stature.

In 1960 The Institute for Scientific Information (ISI) was created and started ranking journals according to citations. This helped start a trend towards more competition among journals and among the 3 kinds of publishing groups. Increased specialization and the sheer

[1]Printing is the mechanical process of typesetting, page layout, press preparation, printing of covers and text, binding and mailing. Publishing is the management and financial control of the publishing venture. The 2 terms are often used interchangeably but they should not be conflated, even though some companies provide a mixture of services to the academic community

volume of information needing a publisher caused a proliferation in the number and variety of journals. At the same time, library budgets seemed able to expand as fast as was necessary to acquire the best journals, regardless of the price. Publishers began to view large institutional libraries as an inelastic market for crucial core journals, that is, a market where demand was little affected by price. This engendered a price spiral, particularly with respect to the commercially published titles, that led by the 1990s to the current 'serials crisis' forcing libraries to be more selective in their acquisitions. This crisis has put new pressure on all 3 kinds of publishers to cut costs, since library subscriptions, the backbone of any academic publishing operation, began to decline rather than grow as they had for the previous 350 yr or so.

Major changes in publishing technology occurred in the late 1960s, and again in the late 1990s. The earlier change was the transition from metal type and letterpress printing to computer controlled electronic typesetting and offset printing. Computer typesetting meant that anyone possessing keyboarding skills could, with a little training or experimentation, create pages, including the tables and mathematics that had once challenged even many trained printers. Offset presses still required heavy capital investment as well as skilled operators, but a portion of the value added manufacturing process (typesetting and page layout) could be segmented and done elsewhere.

The later change was the advent of Internet publishing. The most forward thinking (and wealthy) publishers were doing online publishing by 1995. It became clear to us at Allen Press that 2 eventualities were likely to occur: a significant reduction or perhaps even elimination of the need for printing of journals, and a transition from print to online publishing. The very high setup costs involved in printing a journal provide strong motivation to eliminate printing entirely rather than to shift gradually from print to online. As long as even one copy of a publication is printed on paper, about two thirds of the cost of production remains. At a time when publishers' revenues are being squeezed, as is the case today, one of the first places they look to cut costs is their printing vendor, either by cutting out services, or by taking some activities in house, or simply by demanding more competitive prices.

If printing is eliminated entirely, in theory an author can write his/her article, make pages in a program like QuarkXPress or Word, edit and proofread the text personally, create electronic versions of the illustrations to incorporate into the pages, and post the article to a personal or university web site. In fact, this is already being done in such communities as physics, in the form of preprints or e-prints. In an interesting way we may have come full circle from the hand carried letters that scholars sent to each other in the very earliest days of science, to the 21st century phenomenon of unreviewed author preprints.

What value can the printer or publisher add in this environment, where anyone can be their own publisher? Certainly neither the printer nor the publisher can wield the kind of control over the process that was possible when only those who possessed special skills could lay out a page or make copies of a journal. I take the fairly radical view that the days of the journal printing operation in its present form are numbered, and the demise of journal printing will happen sooner than we think.

Few if any of the quality control measures for which printers were responsible 25 or 30 yr ago remain integral to the publishing endeavor. Instead of rekeyboarding the manuscripts, which requires proofreading, electronic files of the articles are uploaded to an FTP site and converted via automated processes into pages ready for printing or online publishing. The rare article that is received as a paper manuscript is sent to a vendor in a foreign country where labor costs are inconsequential, to be 'double keyed', where 2 operators retype the article so that it can be compared letter by letter by a computer, and then where discrepancies exist a third operator makes a correction.

Typesetting software, even off-the-shelf programs like Quark and Word, can handle and display math and tables with relative ease. Articles with heavy math such as those published by the American Mathematical Society and the American Physical Society are submitted in TeX or LaTeX templates for automated conversion into pages. Illustration quality seems to be far less appreciated by the authors and readers of scientific publications, now that the figures are submitted electronically and are mainly viewed on a computer monitor with limited resolution. Illustrations with broken lines or letters, bitmapped images, and halftones that lack clarity and detail have become the norm.

At the risk of sounding crotchety and embittered, I do sometimes wonder why the highest standard of quality was required when someone other than the author or editor was responsible for (or could be blamed for) its absence; yet now many of the same aspects of quality are considered irrelevant. We used to reset countless lines of type over minor style points caught by the editors, or because of errors missed by our own proofreaders. Today, when glancing through some of the journals we print from pages submitted to us in final form by our clients, I often notice typographical and grammatical errors that would never have found their way into print a few decades ago. For years librarians campaigned for printers to use acid free

paper so that journals would last for centuries, and now they cannot cancel print subscriptions fast enough in favor of fragile electronic products.

At one time journal publishing was a leisurely and gentlemanly endeavor where 'it doesn't matter how long it takes as long as it is done right.' Today publishing in a hurry and at low cost has become paramount. In these days of do-it-yourself publishing, how can the printer or publisher continue to provide a useful service to the community? Here are a few ways that the printer and/or the publisher may remain relevant to the process:

(1) Applying a consistent structure to the articles in a journal leads to significant benefits such as searching full texts and linking from references. The printer or typesetter can make the article into what amounts to a database by use of XML (extensible markup language) or SGML (standard generalized markup language) coding at the same time that the paging process is undertaken. Consumer friendly SGML and XML editors and parsers will soon be available, but up to now the individual author would have a hard time providing the consistent structure needed for complex and highly developed online publishing systems.
(2) Even though production values are less appreciated than they once were, publishers can enhance the printed journals (for as long as printed journals survive) by such initiatives as a full color cover and more color images in the text.
(3) On demand printing (a glorified form of photocopying with finishing or binding equipment attached) makes printing of a very small number of copies more affordable. A market may continue to exist for this technology, for the few libraries and individuals who insist on receiving a printed journal.
(4) However well author posted preprints can succeed in a discipline like high energy physics, where most of the authors know each other and validation is less important, peer review is still mandatory for most journals, and some printers and publishers (including Allen Press) have developed sophisticated online peer review tracking systems that greatly facilitate this important aspect of publishing a journal.
(5) Many small society publishers who have operated on a break-even basis over the years do not have the financial wherewithal to make the transition to online publishing and peer review. Publishers can offer a package of services that includes the online services at no overt charge to the society.
(6) Online publishing users prefer to be able to search content within one aggregated database. Publishers can develop discipline-based aggregations that make the user experience more fruitful and that, at the same time, lower the cost of online publishing because of economies of scale. A good example of this is BioOne, a nonprofit aggregation of journals in the biological and environmental sciences founded and supported by Allen Press (www.bioone.org).
(7) Since online publishing is not faced with the same costs as print, publishers can include new kinds of content such as datasets, sound, and video in the online version of the journal that would not have been affordable or possible in print.
(8) To help counteract library subscription losses, publishers can tap into journal markets (e.g. international sales) that print journals have a hard time approaching.
(9) Some societies, particularly smaller ones, are not very good at planning and financial management. Publishers can help societies make businesslike decisions.
(10) A University press or commercial imprint can add prestige to a journal masthead.

The final 2 issues facing all publishers today deserve further discussion: archiving the online content and open access.

The cost of archiving online journals and migrating the content to future platforms is potentially enormous. Printed journals had a significant advantage in this respect: once the publishing organization had paid for the expenses of peer review, editing, manufacturing, and mailing, its financial responsibility for the information's survival was fulfilled. With hundreds or thousands of copies of the publication in institutional libraries and society members' offices around the world, safety of the information for at least a few hundred years was assured.

In the case of online electronic journals, some entity must undertake rather expensive measures to ensure that the content will survive over time. Most publishers offer a right of perpetual access for subscribers to a given issue or volume of a journal. This is a holdover from the print environment that may need to be reconsidered for electronically published journals. With perpetual access, publishers will incur ongoing costs to provide future access to current subscribers who pay nothing for the access.

Ensuring the survival of electronic archives is an expensive proposition. If the publisher assumes this responsibility, there will be a need for replacement of computer hardware, increased bandwidth for access, mirror sites for redundancy in the case of failure or disasters, and migrating the systems and content forward to new systems that inevitably will make the existing systems obsolete. Somewhat like Social Security, the new subscribers somehow will have to cover the costs for all the data that has been accumulated over the his-

tory of the online publication. Perhaps the Library of Congress, or some large institutional repositories, or organizations similar to J-STOR may take over this archival function. But at the present time most publishers have paid lip service to archiving their content, knowing full well that when they can no longer afford it, go out of business, or are acquired by another company, this promise may become unfulfilled.

It will be difficult for an organization like the Library of Congress to take over archiving, because of the lack of standards in online publishing systems. Some publishers use SGML, others XML, and still others PDF to display their content. Even those who use SGML or XML have different document type definitions (DTDs), which prevent other publishers from using their information without altering the structure of the data. For a printed journal, it made no difference which printing press or typesetting device the printer used for production, but for the electronic journal the process used makes a great deal of difference. PubMed has created a DTD which may become the standard for biomedical journals, which will be a step in the right direction.

Open access is perceived as a threat by many publishers and as an opportunity by others. The journals pricing crisis mentioned above has led a vocal group of scientists and librarians to attempt to change the model for scientific publishing from a subscription fee based system to an author pays system. The movement for open access is aimed primarily at the largest of the commercial publishers who charge high prices for some of their journals, and whose profits are considered by some to be excessive. However, other organizations, including learned society publishers and university presses, are potentially affected by open access since they also support their publications with subscription fees.

The motivations for open access are complex, but in general its proponents feel that the system is unfair when scholars provide their articles to publishers free of charge, and then publishers in turn charge high prices for subscription access at the same institutions where the information was created. Much of the information is government funded, so why should it not be freely available to the taxpayers who paid for it? Subscription access tends to limit the availability of the information to users at a few hundred or a few thousand institutions. Free access to scientific information may help improve health, or living conditions, in less developed countries.

Opponents would say that the present system of subscription access has served the community well for centuries, and that open access threatens the underpinnings of scientific publishing. Many publishers already offer access after a short period of time such as 6 months or a year. Others offer free access to users in developing countries. The author pays model is not financially sustainable or scalable.

I would not wish to take a position on the merits of open access, but do want to mention it as an issue all publishers need to be aware of, and to point out that, unlike author self publishing, open access does not necessarily change the quality of the information being presented one way or the other. Organizations like the Public Library of Science who offer open access journals are still doing peer review, copy and technical editing, and production quality control just like subscription access publishers have always done.

In conclusion, printers and publishers of scientific journals have encountered considerable change in the way they do business in the past 30 to 40 yr after more than 300 yr of stasis. Quality standards have diminished, mainly because of cost pressures. However, the printer and publisher still have a secure position as long as they can offer services and products to the academic community that authors, editors, students, and researchers find useful and convenient. Validation by peer review will remain a key ingredient in academic publishing regardless of what financial model is adopted by publishers. High quality production standards remain important as long as they do not threaten speed of publication and low-cost or no cost-access to scientific information.

Ethical responsibilities of referees

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My research career began in 1962 and, thus, I have 40 yr of research behind me and have had quite a few papers published and received a number of research grants over this period. I sit on the editorial boards of 5 marine science journals and have spent time on research council committees in the UK, Sweden, Finland and Norway. On average, I receive 2 papers a week to review and research grant applications from many different countries in addition to those mentioned. Since both papers and research grants are subject to peer-review I will comment on the peer-review system in general.

Henry Bauer has pointed out the difficulties of getting controversial ideas published, and taken up the key issue of the responsibilities of editors. Here, I want to emphasize another aspect of the peer-review

system, namely that of the responsibility of the referee. Reviews are usually made anonymously and far too often they are not objective, nor need the criticisms be fair; and this is one aspect that I want to emphasize.

Researchers at the start of their careers might think that there is a bias towards them receiving negative reviews of papers and/or research grant applications compared with more senior researchers. I can assure them that this is not so. This past 2 wk I have received 2 rejections for research grant applications and one rejection from a paper that actually got to the review phase in *Nature*. I do not object to the peer-review system being such that my submissions are rejected, but I do object strongly to the often unclear, biased, and plain wrong comments in referees' reports. Although I use personal examples below I believe that these are symptomatic of reviews in general and am quite sure that many readers will have received similar ones. One example is the comments of a referee in the paper we submitted to *Nature* on a new method for measuring an aspect of marine biodiversity. The referee, although highly positive in general wrote, 'Is it reasonable to compare equatorial and European faunas?' Such comparisons are a central issue in biodiversity research in general and should have been known to anyone familiar with the field, which the referee should have been. Likewise, on one of my rejected research grant applications the opening statement was 'The scientific questions to be addressed in this proposal are mostly old ones…' No explanation was given. Does this mean old and therefore, totally uninteresting? Or old and unsolved and therefore, exciting? Without an explanation all the editor or research council panel giving out the money can do is to interpret this in the most negative way they can. But is this fair? Do not referees have any ethical responsibility to write clear statements of what they mean? After all it takes a great deal of time to prepare a paper and research grant application. The referee has a responsibility to be objective, fair and to write clear statements that are of value to the writer in helping them to improve their science. Above all the review should not reflect the biases and prejudices of the referee, but be objective.

Another aspect of the referee system is the responsibility that the referee has to her/his discipline as a whole. Over the course of my career I have had many experiences showing that marine biologists seem to delight in being highly critical of each other, or as one colleague put it, 'Collecting all your wagons in a ring for protection and then shooting inwards'. Yet we know that if there are any negative comments then the paper being reviewed will not appear in *Nature* or *Science*; or if the research grant application does not get 'excellent' on all aspects being reviewed, it will more likely than not be rejected. So what does this negative approach by referees of marine biological science achieve? Firstly, a negative review for a paper submitted to the very best journals will stop a marine biological paper appearing and thereby stop our discipline being promoted in that journal. Wouldn't it be a more sensible tactic to say in a referee's report that this paper has some interesting and important points and deserves to be published and then you would have a chance also to get your critical reply in a top journal too? Instead we shoot ourselves in the foot and our discipline does not get the coverage of other disciplines, which are not so prone to being negative about others' research. It is well-known that highly expensive research fields with small communities such as astronomy need to act in a coordinated manner to promote their field and get funds, which they do in a highly successful manner. Likewise physical oceanographers spend a great deal of time together at sea and have time to prepare coordinated research proposals. They are not foolish enough to 'shoot each other' by writing overtly critical reviews of papers and grant proposals.

Another problem with negative reviews is that the referee might succumb to the temptation to shoot down a senior researcher by commenting in a negative way. But the problem is that this might not achieve the referee's objective since whether or not the senior scientist gets a paper in a top journal or a research grant will make very little difference to the career of an established researcher. But it does mean that she/he will be unable to employ and help young scientists in developing their careers. It is the young scientists that suffer, not the established scientist.

In summary, it seems to me that very often too little thought (and often too much haste) goes into assessing the consequences of the report that is being written. I suggest that all referees need an ethical code of practice. Before sending the report you have written you should ask yourself to be honest about the following questions:

(1) Is my review objective or does it reflect my biases and prejudices?
(2) Is my review fair and clear?
(3) Will my review help the receiver to develop a better paper or research grant application?
(4) Will my review help the discipline as a whole?

If this proposal is used by referees it might help to overcome the problems of lack of clarity and hopefully what I see as the more serious problem that marine biology as a discipline gets less publicity in the best journals and possibly fewer grants in comparison with other disciplines.

Electronic publishing of science and the maintenance of quality

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Publishers are an essential part of the production of the scientific literature but the field is not best served when the publishing business in only interested in earnings per share. Open Access or publication on the Internet are likely to become more important in the future and they will bring about significant changes. There will still be a role for editors who will continue to be guardians of quality and a filter to reduce the volume of papers. The peer review process generally works well and is likely to be retained into the foreseeable future. Anonymous refereeing should be replaced by open assessment. Accurate production of papers is essential and publishers play a big part in ensuring that manuscripts are professionally proof-read. The advent of electronic publishing is altering the financial structure of scientific publishing. Learned societies who have benefited greatly from the profits earned by their journals will still maintain their income stream but may have to alter the ways in which they foster publication as more and more use is made of electronic publication.

The role of publishers, editors and others involved in production. Publishers of scientific literature can be divided into 2 types; those in the business just to make money and those with a wider remit. The latter have to make money too but also attempt to serve the research community with more than just the publication of a journal. The first group of publishers give the appearance of being interested only in earnings per share. As a result they have in the past charged institutions very high prices for their journals and books. The second group of publishers often have contracts with learned societies for the publication of their society's journal. Mostly, the subscriptions asked for such journals are considerably less than those charged by the purely commercial publishers. Examples of the first type of publisher would be Kluwer and Reed Elsevier and of the latter Academic Press, before it was taken over by Reed Elsevier, Blackwell Publishing and the University of Chicago Press.

The big commercial publishers have faced a much harsher financial climate over the past 10 yr as Universities and research institutes have been forced to reduce the amount they spend each year on journals and books. In my own department at the University of Leicester we had a spell when each year we had to review our journal subscriptions and reduce the amount spent by 10 to 15%. The journals costing £1000 a year or more were very vulnerable unless they had a hugely respected reputation. By cutting one expensive journal it might be possible to save 5 or so cheaper ones. As a result of this some of the larger companies such as Elsevier were forced to review their pricing policy and hold subscriptions constant or increase them by the smallest margin possible. There have also been movements made in the scientific community to create non-profit making organisations to publish science and this is putting further pressure on publishers.

Publishers play an important role in how science is presented to its audience. For example, in the early years of the *Journal of Fish Biology*, Academic Press took the financial risk of launching the journal. Without them, the journal would never have become the important publication it is today. If done in collaboration with a learned society, publication can yield benefits both to the publisher and to the science that is reported in the journal. For example the Fisheries Society of the British Isles has received over the years an increasing income from the journal, shared with the publisher, that has been used to foster fisheries science through supporting PhD students, providing travel and research grants and running conferences.

Publishers should also be responsible for marketing. Many have significant networks of contacts worldwide and they can do much to promote a journal. This role is particularly important for journals originating from learned societies who do not have the resources or expertise to market a journal effectively.

Open Access publishing, in which the scientist will pay a publisher to have his/her paper produced, is currently being considered as an alternative model. This approach will mean that authors will have to pay to have their papers published with fees starting at around US$500 per paper. This system will make papers freely available to readers, which is good, but may disadvantage scientists who do not have funds to pay for publication. The prospect is that libraries, freed from paying large subscriptions for journals, will be able to divert the funds to paying for staff publications. The trouble with this is that within large institutions there are competing demands for funds and the sudden release of a substantial amount would be a tempting target for University administrators. The money could soon be siphoned off to projects other that that of paying for staff to publish papers. It is possible that publishers will compete with each other to offer the lowest price, which could lead to some interesting inversions of publication reputations.

Editors play different roles on different journals. Some take a micromanagement approach and work to bring every article published into a stylistic line. Others rely more heavily on referees to make the technical judgement and others are almost managers who oversee a team of assistant editors who do the detailed work. There is always going to be a role for an editor in that there has to be someone who manages the refereeing system and makes judgements as to what should be published and what should be left out. With an increasing use of the web, the editor may do less to influence the content of a journal and more to limit the number of papers that appear on a server or journal site. My assumption is that electronic journals paid for either by the author or by readers on a pay as you read basis will be able to publish greater numbers of papers. The critical job for the editor then will be to decide on what is worth publishing and what is not in terms of the interest and significance of a paper. No longer will the editor look to the balance of the content, matching up an applied paper with a non-applied one and in general trying to create a product that will fulfil the expectation of the purchaser. The editor will be more of a policeman trying to keep the mass of published material to a usable size.

The peer review process. The peer review process works well but the tradition of anonymity can create problems. I find that about three quarters of referees are fair and try to do the best they can. The remainder can either be sloppy, providing very little to help either the editor to decide on a manuscript or an author to improve the clarity of their manuscript, or they can use their anonymous status to be cruel and destructive. Academics are often remarkably conservative in the way they think manuscripts should be written, methods used and statistical tests done. For such people, if a manuscript does not follow the way they think it should be written, then it is wrong. In my view, the anonymity of referees should be removed and every referee should be required to defend publicly their critique of a manuscript.

Scientific activity is so split into specialties that it is often the case that there is only a small group of people who are competent to referee a particular topic. The members of this group may know each other well. In this situation, each person's manuscript will be refereed by a small and known group of people leading to the possibility that rivalries and alliances can develop with some members being harshly dealt with and others being given an easy time. Members of these small specialist groups will not only referee each other's manuscripts all the time but also each other's grant proposals and applications for promotion.

Would open peer review improve this situation? The existence of small specialist groups would not be dealt with but at least each member would be able to have an open discussion with like-minded colleagues about the merits of their work. The job of the editor in a traditional journal would be made harder by open peer review. In many present journals, because space is so limited, excuses have to be found for rejecting papers that are not flawed. The editor can hide behind the harsh judgment of an anonymous referee and reject a manuscript even though a second referee might have liked the paper and the editor can also see that the manuscript has merit. Open Access publishing might solve this dilemma. As the author is paying, the space available in a journal should not be so limited. At present each journal has a page budget determined by its circulation and subscription price. With the limit on costs removed, it would be possible to publish more papers that are not flawed methodologically but do not have great generality.

The extreme extension of this would be publication on the internet with no refereeing. In effect the reader is being asked to be the referee. Papers that are flawed would be ignored whilst those that have something to say and are methodologically sound would be cited frequently. The problem with this is that readers will not have the time to plough through hundreds of articles, reading them in enough detail to determine whether the studies have been done properly and that the paper says something of interest. This line of thinking leads to the conclusion that there has to be some sort of refereeing and editorial process so that the scientific literature retains quality and is limited to a manageable volume.

Technical aspects of scientific publishing. How a journal looks can have an important influence on how it is perceived although there are no fixed rules about design. Publishers tend to take the lead in proposing changes in cover design, format and layout. Whereas 10 yr ago most journals had a 17 × 24.5 cm size, many have now gone for the 21 × 27.5 cm format. Publishers argue that the bigger size allows 2 columns of text per page, which makes for easier reading. Librarians storing hard copy editions might dispute the usefulness of the change in that the altered size makes the shelving of long journal runs a problem. Some journals have retained their style for many years and continue to do so today. An example is the *Quarterly Review of Biology* and many might now think of it as looking 'old fashioned'. In my view there is a lot to be said for a journal having a distinctive style, which is immediately recognised and has continuity with the past.

The critical thing about production is how it influences the quality of the product. It is essential that the

text is properly proof-read so that inaccuracies are avoided. In my experience some of the larger, commercially oriented publishers skimp on proof-reading or have it done by people whose first language is not English and who have no special knowledge of the field. As a result such journals often have more typographical errors than is acceptable, the English is non-idiomatic and there are sometimes technical errors that would not have been made had the proof-reader known something about the subject of the paper. Even if papers appear on the Internet they have to be proof-read.

The problem becomes particularly pressing with papers that contain mathematics. Many scientists are not trained mathematicians and do not know how to write mathematical symbols and formulae in the correct way. For example, symbols for variables should be italicised. A good journal will make sure that the mathematics is correctly presented. If there are many subscripts or superscripts it is essential that these be large enough in the finished product to be read without ambiguity. What looks readable on a typescript may be so small in the finished article that the subscript becomes ambiguous.

A final point relates to figures. These need to be produced in the final paper at a size that is easy to read with lettering of an appropriate size and style. Publishers usually have people with the experience to judge whether a figure is going to look good when printed and to make the necessary changes when it can be seen that the final output will be unreadable. Again, if figures are to be redrawn, it is essential that the person doing the job has some feel for the topic. It is all too easy to turn a good figure into rubbish through inappropriate movement of text or lines.

Economic considerations. Throughout my contribution so far I have discussed aspects of the economics of publishing but would like to add a few extra points in this section. The model for producing scientific literature adopted by the large commercial companies such as Reed Elsevier and Kluwer does not have much benefit for the science. Profits go to shareholders and academic and research institutes pay dearly for their subscriptions. In effect these publishers are financial drains on the scientific endeavour. Publishers that work together with learned societies adopt a model which is of greater benefit to the people producing the manuscripts on which the publishers are dependent. Learned societies share a proportion of the profits from journal publication and these can then be used to foster the science for which the society exists to promote. I have already mentioned the Fisheries Society of the British Isles, but other examples are the Association for the Study of Animal Behaviour (ASAB) and the British Ecological Society (BES). All these organisations use money earned from their journals to foster the careers and research of their members. Many graduate students for example are reliant on societies such as the FSBI, ASAB or the BES to provide small grants to enable them to attend conferences. Where else would this well targeted support come from?

Publishing on the web or Open Access publishing is going to change the model currently applying to the publisher/learned society partnership. In principle, there is no reason to believe that learned societies will lose their income from the journals they own. Agreements with the publishers will mean that a proportion of the profit earned from the publication process goes into the society's account. This is already happening with web access to electronic versions of journals such as Blackwells' Synergy system. Through agreements with institutional libraries, scientists have access to all the journals published by a certain publisher and the fee the institution pays for access is distributed between the journals that are available.

The current system whereby money from institutions is redirected via journal subscriptions to the special interests of a small area of science such as fish biology, animal behaviour or ecology, is elaborate. Would it not be better for the central funder of all the activity, the Government, to pay money directly to the learned societies who could then use it as they thought appropriate? This might be a relevant model but it would take away from the scientists the present control they have over their own affairs. Under the current system a learned society can use its income in the way it thinks best without interference from any other body. If the government gave money to the society, it would come with strings attached and with an increased burden of bureaucracy deriving from the government's need to feel that they were holding the society accountable. The present system diverts money from the government to learned societies but it becomes anonymous as it passes through the commercial publishing system.

In the back of my mind always lurks the feeling that internet publishing is going to become more and more common in the next 10 yr. I do not mean by internet publishing web access to journals, but scientists posting their papers on the web either on their own web sites or on web sites organised by interest groups. Such a system would be free to all but it also carries the threat of swamping the world with material of very variable quality. Search engines can be used to look for papers relevant to a particular topic but the process is likely to yield hundreds of papers with a quality varying from complete nonsense to globally significant articles. Who will have the time to filter out the rubbish

from the good? The conclusion is that there must be some system in place to filter out the junk. A possible route to take could be modelled on the Apple iTunes approach to music marketing where people can download tracks from the iTunes server for a fee of either $0.99 a track or around $9.99 for a whole album. Partnerships of publishers and learned societies could create appropriate servers for topic areas. Papers would be added to the server after refereeing and editorial work and could then be bought for a low price per paper. Scientists might also be charged a small fee for having their papers included on the server in the interest area domain.

Conclusions. It is in the interests of all scientists to maintain the flow of papers and to subscribe to a system that watches quality and reduces the danger of a flood of low quality publications. These constraints point to the continuation of the editorial system with some sort of refereeing. I have proposed that refereeing should no longer be anonymous although I do not expect this to be a majority view. The biggest changes are going to be in the way in which publication is financed. Funding through subscriptions for individual journals is going to be replaced by group fees for access to electronic versions of journals. The way this is evolving at present means that papers are still attached to journals but there is no reason why in the future, learned societies could not set up servers in partnership with publishers to publish articles on a wider range of topics than is currently accommodated in the standard printed volume. A pay as you view system already exists but it could become more widespread and much cheaper. Such a system would also charge the author for including their papers onto a server after suitable editing and refereeing.

Accountability, quality and efficiency in the information age

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For scientific journals to maintain quality their content must be controlled by their contributors. The Editorial Board must be active and self-selecting based on dedication and merit, with members that are credible to the broader community of contributors. The acceptance/rejection of papers must be based on the best scientific judgement, free from commercial and operational considerations. Kinne (2003) has spoken of the difficulties in recruiting, protecting and controlling editors and reviewers. For high-volume, high-quality journals these difficulties are compounded unless the Editorial Board and Production Team are proactive in harnessing, through the use of modern technology, the talent and goodwill of the scientists that contribute to the journal. A good publisher provides a valuable service but must act as servant rather than master, for its own good as much as for the sake of science, and must embrace new technology to better serve and to empower the global community of scientists that it represents.

Electronic publishing. Some of the issues surrounding electronic publishing have been discussed previously (Kinne 1999) and are elaborated upon in the essay by Paul Hart. To my mind, the internet provides an unparalleled distribution medium but the central issues of quality assurance for science publishing remain. The active researcher who wishes to keep abreast of developments in their field simply cannot read everything that is of possible relevance. We rely on others to raise published work to a level that is worth spending the time to read. This happens through peer review, and selection and collection of related papers into journal form. The claims of online-only publishers to be providing a revolutionary new service by being 'open access' (i.e. not charging readers for a subscription or per paper but charging the author per page) need to be examined critically. To the graduate student trying to get their work published after their funds have run dry, or to the scientist from a developing country, there is nothing particularly open about a page charge of US$500. Most important with reference to this TS is that there is no reason why page charges like this should increase quality. It is simply a different cost model, i.e. an alternative way for the publisher to recover costs and/or make money. The online nature of these journals is no longer revolutionary: most established journals also publish online, where they will include supplementary information (e.g. animations of model results, video observations) that utilise the electronic medium to great effect. In my opinion, while the internet serves as a unique forum for global discussion, and as a means of data collection and distribution, its use in science publishing as anything other than a complement to the printed page is exaggerated. Publishers have all but conceded the right of authors to obtain and distribute pdf files as it is literally impossible for them to stop people emailing pdfs to each other. However print editions are usually superior in terms of print quality and serve as historical archives. Some people cite the internet as being

most useful for rapid communication of important new findings or rebuttals. This facility is provided in both paper and electronic form through Can J Fish Aquat Sci, MEPS, *Nature* and other journals but the reader also knows that the paper will have been peer-reviewed. I would therefore encourage marine scientists to submit their papers to established and respected journals and to expect quality and timeliness from both the Editorial Board and the publisher. If either player fails you then make your complaint loud and clear, but bear in mind that they may be trying to do you a favour by improving the final product of your work through rigorous peer review and conscientious editing.

Quality of production. The largest part of the total cost of publishing a journal is associated with the salaries of the people responsible for production, and not with the actual printing and distribution. Any production process is an 'assembly line' of tasks that must be carried out sequentially. Scientific publishing consists of the following stages, each of which is often repeated until a satisfactory standard is reached: peer review, Editor's evaluation, revision, acceptance, editing, typesetting, proof-reading, printing. Though these tasks are sequential for each manuscript, they are usually going on simultaneously for the collection of manuscripts that will comprise more than one journal issue, or where the production team works on more than one journal. Maintaining a consistent work flow when there is no control over the input of papers is a difficult task for any publisher, hence some delay between acceptance and publication will always occur, but there is no reason why this delay should extend beyond 3 to 6 months. Many scientists overestimate their own writing skills (see the essay by Prof. Underwood) and during the production process considerable value can be added to a scientific paper. This is particularly important in ensuring a level playing field for scientists who are not native English speakers. A badly written/structured paper may as well not be published at all because the reader will come away with an incomplete understanding of the work or, feasibly, they will misunderstand it. Many would simply give up reading it. So scientists must be prepared to make a good effort at creating a respectably written article prior to submission and to tolerate some delay between acceptance and publication while quality assurance and enhancement is carried out. Reviewers should concentrate on scientific quality while copy editors focus on logical structure and syntax. There is a degree of overlap in the roles of reviewers and copy editors, in terms of the logical expression of the argument, but it is essential that reviewers concentrate their efforts on the scientific substance as their specific expertise is harder to find than good technical editing skills. Typesetters then ensure presentation that does justice to the text, tables, graphs and images without being offensive to the eye. This is highly technical (not to mention tedious) work which should not be undervalued. This is what impressed me most during my time working for Inter-Research: the dedication and skill of the copy editors and typesetters and the sheer volume of work that the team carries out to an industry-leading standard. If Inter-Research can exert this level of effort and remain profitable so can any other publisher worth their salt, and the marine science community should expect no less.

Use of the internet to facilitate the peer-review process. Email is already often used for manuscript submission and distribution to reviewers, but this just scratches the surface of how the internet may be used to help coordinate the peer review process. By coupling databases of Editors, reviewers and authors with web-based interfaces, the prospective author can submit their paper online, nominate an Editor and suggest referees, which the Editor may or may not choose to use. Editors can have full access to the list of reviewers and therefore avoid overusing them. With new referees being added to the database as they are recommended, and from the database of authors, the pool of reviewers would be large and constantly refreshed, and the 'payback in kind' principle (Riisgård 2003) more easily implemented. The net result would be better coordination of the review process, at least for the journal in question. Some smaller journals already do this, but it would be a shining example and perhaps a catalyst across the community if a high-profile journal such as MEPS empowered its contributors in this way. Much more would be expected of Contributing Editors in terms of the proportion of papers for which they have editorial responsibility, but the publisher could concentrate on reducing the effective workload of that responsibility through coordination and maintenance of the system. Modern technology can enable this while retaining an essential role for the publisher in quality assurance both prior to and after manuscript acceptance.

Concluding remarks. Authors, reviewers, Editors, copy editors and typesetters all have essential roles to play in maintaining quality in marine science publishing. Regardless of the publishing model used (commercial, society-based, open access) the essential purpose remains to communicate scientific research in the most effective and efficient way. It is important that the technology of the information age is used to promote global intellectual discourse and to serve rather than subvert the human processes of quality assurance in science.

Maintaining quality is primarily the role of the editor

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'It is quality rather than quantity that matters'
(Seneca, Epistles)

'The important thing in science is not so much to obtain new facts as to discover new ways of thinking about them'
William Bragg

The issue of quality in science publishing will not be fully explored in a brief set of editorials; it is as broad as the whole process of scientific education itself. Maintaining the quality of scientific publications is important for many reasons including the efficient advancement of scientific knowledge itself, the reputation of scientific journals and the reputation and career prospects of the scientist-author. Judging or measuring the quality of science publishing is the role of contributors and readers as well as reviewers and editors of the journals, but maintaining quality is primarily the role of the editor.

The issue of 'quality in science publishing' is in fact not really a purely scientific issue at all; it is one of judgment. Some more quotations are in order: 'As soon as questions of will or decision or reason or choice of action arise, human science is at a loss' (Noam Chomsky); 'I believe that a scientist looking at non-science problems is just as dumb as the next guy' (Richard Feynman). Unfortunately, the 'next guy' may be an editor!

Problems in science publishing. Science publishing has its fair share of potential problems, including cheating of various sorts such as plagiarism, fabrication or manipulation of data, double-publication, or lack of acknowledgement of co-workers. Although any author can be guilty of a misdemeanor, major crime is very rare, and fortunately we are all police. Science publishing is intellectually a largely self-regulating activity, because getting caught in cheating carries such high penalties. We are likely to be thrown out of the club and terminate our career. The issue of 'quality in science publishing' (as opposed to the issue of 'petty crime' in science publishing) is far less tangible.

What is 'quality' in science publishing? This seemingly simple question is not straightforward, but informed peer review and editorial judgment must still be at the heart of quality control. The various journal publication 'impact factors' are not really indices of quality of science publication, and I will not debate them here. They are really indices of journal 'status'. Low ranked journals are unlikely to receive or publish cutting-edge science, but highly ranked ones may publish poor science. The only index that I confess some respect for is 'citation half-life', i.e. the average number of years a contribution in a journal remains actively cited, a measure of how long it remains influential within its discipline.

I would offer that while quality should entail being as accurate and precise as possible, the process of truly making a significant contribution to a discipline is far more a function of being original than it is of being correct. Descriptive science will always be with us, but in my view it only deserves publication in tier one journals when entirely new structures or processes are described. Descriptive pieces can really only be reviewed in terms of their current technical accuracy. Here I will comment only on manuscripts that deal with ideas, concepts, questions and hypotheses.

Unlike some disciplines in philosophy, the search for truth is not a proper goal of science. Science proceeds by attempting to falsify formally stated ideas (hypotheses), which we temporarily accept as correct. The essence of good science is not just to advance new ideas, but to advance ones that can potentially be shown to be wrong. We should therefore remember that advancing an idea that can be refuted, i.e. offering a good idea that may be shown to be wrong, has greater value than presenting data or description that is obvious, pedestrian or unlikely to be challenged. Doing quality science is not playing safe. The important thing in scientific publication is *not* to aim for a perfect finished product, but to stimulate thought and enquiry by the scientific community.

The goal should be to offer a credible — not a 'correct' — piece of work, worthy of attention by the scientific community, who in due course will subject it to a process of natural selection. If it proves resistant to attack its ideas will endure; if it fails under assault it will be relegated to the garbage heap of extinct ideas. What endures we accept as 'good, current' science, but this is *not* synonymous with quality science or quality of scientific investigation. Quality science is challenging but vulnerable; it may or may not be rejected as incorrect, but it leads to enduring paradigms and understanding of the natural world.

A more cynical view (but, for our purposes, pragmatically useless) would be that quality *is* a post-hoc measure of the value of a piece of science; that is, quality science *is* synonymous with enduring science. But unless we believe in granting tenure or promotion posthumously, we need to seriously evaluate how we

should recognize the merits and quality of scientific contributions at the time of submission to a journal.

Who judges quality? Authors naturally hate to be rejected, but journal reputations thrive on it. When it concerns publishing, scientists as a whole are positively masochistic. The more they are rejected, the more desirable it is to publish in that journal. To paraphrase Groucho Marx, the attitude seems to be: 'I would not want to publish in any journal that would have me as an author'! We could be forgiven for concluding that journal quality is directly proportional to rejection rate! Clearly it should be less perverse than this, but the whole issue of 'quality' does beg the question: Who rejects or accepts manuscripts and on what basis? This is an important issue but there is little doubt in my mind as to the answer. At the risk of seeming simple minded (and invoking Monty Python and the 'bleeding obvious') — it is the editor who should accept or reject manuscripts, based on the advice solicited from reviewers.

Maintaining quality in science publishing is the prime responsibility of the editor; it is a grave responsibility and no easy task. Presumably none of us would argue that it is the prerogative of referees to *accept* a submitted MS — this is the job of the editor. Why therefore should it be the prerogative of a referee to *reject* one? Referees are requested by editors to provide advice. Editors who slavishly accept the advice of reviewers without dissent have simply abdicated their prime responsibility. It is the task of the editor to accept, modify or reject reviewers' advice, as appropriate.

How should editors evaluate quality? ***Recognizing the 'state-of-the-art' in a science discipline:*** All disciplines in science tend to follow a historical sequence from an initial descriptive phase involving: invention, discovery and exploration, through an analytical phase involving testing of hypotheses, asking questions and the investigation of processes, to a 'mature' phase of synthesis and unification. There may of course be a total revolution at any time, and this temporal sequence may be repeated or punctuated as new phenomena or levels of complexity are revealed within a discipline. The important thing for editors (and reviewers) to recognize is the current state-of-the-art within the research activities of their discipline, and the potential of a research contribution to lead to new understanding. It is against this backdrop of evolution and potential within-discipline that the merits of a contribution should be judged. This requires a combination of knowledge of the discipline, scientific insight, judgment and, yes — charity.

Distinguishing between poor science and unconventional science: Conventions in science often go unstated and unchallenged. We should always be prepared to challenge our conventions. Some conventions such as the format of a scientific paper, or the scientific review process itself, have remained fairly rigid because they have clear (if imperfect) virtues. But most importantly, we should be prepared to challenge authority. 'If an elderly but distinguished scientist says that something is possible he is almost certainly right, but if he says that it is impossible he is very probably wrong' (Arthur C. Clarke). We should guard against any tendency to substitute 'science by authority' for 'informed peer review'. The beginning graduate student with 'an ugly fact' defeats the Nobel Laureate with a 'beautiful hypothesis' (apologies to Thomas Huxley!).

Enthusiasms in science: When new ideas are advanced within a discipline, the scientific community reacts in characteristic ways. These are manifest as behaviours that I refer to as: 'the shiny red truck syndrome', 'constructive vandalism' and 'the rumble'. In the first behaviour, any new idea is subject to intense scrutiny and experimentation — rather like a group of young boys playing with a brand new toy truck until they discover how it works, or until they become bored with it in favour of the next 'shiny red truck'. In the second behaviour, ideas are subject to attack and disproof (vandalism), but a responsible group of scientists will realize that if an idea fails then they are obligated to replace it with an alternative idea that can next be tested (construction). In the third behaviour, groups of scientists may become locked into contrary positions (the rumble).

All of these behaviours are indications of natural selection in action; the first two at least are necessary components of maintaining quality in science. Such enthusiasms must be allowed to run their course, but a perceptive editor must judge when the scientific community has played (or vandalized) enough in any discipline. A perceptive editor (as umpire) will also realize that when 2 groups of scientist disagree, the most probable explanation is that they are both correct; they may simply have not yet recognized under what conditions each is correct or incorrect (i.e. they do not yet see the variable that unites their apparent differences).

One major current enthusiasm in science is for completed inventories. The Human Genome Project and the Census of Marine Life are examples. Such inventories have considerable potential value, but in my opinion such projects do not of themselves constitute quality in science (although the science and scientists involved may be of high quality). Geographic maps guide us around on land, but they do not by themselves constitute knowledge. The value of such inventories is that they provide the backdrop for asking important questions. Again, the central quality contribution to science is not just to add to existing data

banks by presenting a perfect finished product for publication, but to contribute new ways of thinking about such data.

How do we encourage the scientific community to recognize quality? This is the central role of the editor. Most importantly editors must:

(1) Encourage authors to explain the merits of their contribution, both in a covering letter to the editor *and* in the paper itself.
(2) Give clear direction and advice to reviewers as to journal standards, practices, requirements for a review, politeness in comments etc. This is often rather poorly done by journals, whether in the journal itself, in web pages, or in letters to reviewers. Advice on the mechanics of presentation is, however, *not* a substitute for guidance on judging the quality of a contribution.
(3) Ask reviewers to respond to specific questions about the quality of a contribution:
  a. Does this work contain real elements of originality? If so — what are they?
  b. Has the study been done well (accurate, precise, current methods etc.)?
  c. What is the *potential impact* of this piece of work? Does it have the potential to lead to new understanding?
  d. Does it explain observed facts beyond those presented within it?
  e. Will it stimulate new debate, thought, investigation within or beyond its discipline?
(4) Encourage reviewers to explain *precisely* the deficiencies of any manuscript in terms of purpose of study, methods, data analysis or sufficiency, logic or interpretation, novelty, significance to the field etc.
(5) Encourage reviewers to distinguish between poor or mundane science and unconventional but novel science.
(6) Encourage reviewers to explain whether — in their opinion — the study can be salvaged for publication, if its message can be strengthened, and, if so, how this could be done.
(7) Expect that reviewers will state their own level of confidence in their evaluation.
(8) Exercise judgement as to the probable impact of a submitted paper — if published. The essential question for an editor to evaluate is: 'If I publish this paper will it be good for the discipline and for the reputation of the Journal?'
(9) Take the initiative to stimulate re-evaluation of current paradigms and disagreements within disciplines.

Conclusions. The process of maintaining quality in science requires us to recognize a process of cultural scientific evolution. Because I believe that emphasis should be on maintenance rather than improvement in quality, the appropriate evolutionary paradigm is, I think, 'stabilizing selection' not 'directed selection'. The process of apprenticeship in science, entailing undergraduate study, MSc, PhD and Post-Doctoral studies already ensures high quality in science research; we follow a process of continuous instruction and mentoring. However, we should realize that 'Quality in Science Publishing' is a class from which there are no graduates — only critics, peers and mentors. Finally, I believe that the 'review plus editorial decision' process generally works well in maintaining quality. Improvement will only come by reminding ourselves what the standards are for the scientific community. Yes there are injustices, but — with what would we replace it? Who has a new paradigm of 'quality in science'?

It would be better to create and maintain quality rather than worrying about its measurement

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There is apparently widespread concern about the current marine ecological literature — or is it just a minority of marine ecologists who find a lot of papers to be minimalist, contrived, regurgitatory or boring? There is so much pressure on researchers to be accountable that — publications being the only tangible product of our labours — editors, referees, colleagues who are asked to comment on a manuscript are most seriously overworked. So, in all of this bustle, quality of papers is called into question.

Of course, no amount of commentary, even in an illustrious journal like MEPS, is going to remove the imperatives to publish. Many agencies considering applications for grants have explicit components of assessment by reviewers about the track record of applicants (the Australian Research Council, for example, insists on 40% of assessment for its Large Discovery Grants — the usual funding for academic researchers — to be about the applicants' track-record). By that they mean quantity and quality of the papers. Cases for tenure or promotion bristle with requirements to demonstrate how much of one's work is being published and how well it is perceived.

Here, I consider some of the issues from a personal and, no doubt, jaundiced view. The main message is a very simple one. It would be better to create and maintain quality rather than worrying about its measurement. Quality of a publication, like beauty, is inevitably in the eye of the beholder and so it should be. Unlike beauty, there have been no attempts even at quasi-units to measure quality of a paper (historical note: the unit for beauty is, of course, the milli-Helen, mH, defined as the amount of beauty required to launch a single ship and derived from Helen of Troy's beauty being capable of launching a thousand ships). The reasoning here is that the beholder may not be present yet, so a paper's quality may not yet be demonstrable. At the other end of the reasoning, even a paper of currently great quality may not yet be revealing its flaws. There may be perfectly valid, but very different, reasons for later readers to weigh differently a paper's contribution to their current thinking. So, at the very least, a measure of quality must be multivariate, with varying weightings to component variables and shifting values of the variables through time.

To illustrate the difficulties, consider the following brief examples of papers that have seriously influenced my own views about how scientific publications help develop philosophical thinking. These are *not* the best, nor the most influential, papers by these authors. Nor are they, however much I wish it were so, the only papers I have read. Nor, indeed, are they the only papers of influence, but they illustrate the point.

Joe Connell's (1970) paper on whelks and barnacles arrived when I was half-way through my PhD. It made it very clear to me, more so than his and others' earlier papers, that manipulative experiments under conditions in the field were necessary, practicable and interesting.

Joel Hedgpeth's (1953) paper is a masterly account of how to poke fun at pompous over-doing of simple data, in his case one observation. In it, he demonstrated scholarship in knowing well the full comparative context in which to interpret his finding (about which, see later). He deflated the notion that vast edifices of theory can be supported by too little data, although I am not sure that was his intention and have been too intimidated to ask.

Finally, Jeremy Jackson's (1981) review of how theories about competition come round in a cyclic fashion remains a timely warning that our capacity to measure originality of thought really depends on how well we know the previous literature. It remains one of the few papers that caused me to laugh with pleasure, as opposed to disbelief or derision.

Do these papers have 'quality'? In my personal development, yes. Do others think so? From a personal point of view, that is irrelevant. Could a measure of quality be proposed to measure it for these papers? Absolutely not, because it would depend on the state of mind of the reader, the relevance to the reader's current interests and current state of cynicism. Absolutely not, because the reasons for believing that these particular papers are of lasting quality are actually based on completely different aspects or components of the work. They are not even comparable.

Some apparent measures of quality. ***Survival or longevity:*** Science Citation Indices are based on 2 fundamental measures of a paper's influence: number of citations and half-life. The problems of the former are legion and well-known. Ecologists are apparently consoled by the notion that papers in the field are not cited as often as, say, those in quasi-medical areas of biology, but they last a long time. Thus, a paper published in the year 200x may stimulate all sorts of novel responses. Typically, a study done in response to it will take 3 to 5 yr, because it is usually graduate students who do the novel stuff. It then apparently takes 1 to 2 yr to publish the new studies. So, citations start to appear in $200x + 5$ to $200x + 7$. The argument then goes that the longer a paper is cited, the better it is because it clearly keeps influencing other work. The argument is sterile for all the reasons associated with numbers of citations. Longevity may be conferred on a publication because it is so truly bad that people keep finding new things wrong with it! It may survive because it makes it on to the 'A' list and gets cited *de rigueur* in the Introductions of papers (whether relevant or not — or even whether it was read or not), because other authors cited it and it is easier to use their scholarship than read the relevant, good papers. The traditional air 'Greensleeves' was apparently written by King Henry VIII and has survived to be played enthusiastically today; if longevity is a function of quality, it is worth questioning why the most common use of the tune in modern Australia is a discordant, atonal, electronic rendering by ice-cream vans!

The Journal: Quality of papers, it has been proposed, is easily assessed by the quality of the journal in which they appear. This argument is, of course, backwards — one might hope that the quality of a journal would be a collective property derived from the quality of its contents. Nevertheless, it is worth examining the proposition.

Are papers rejected because they do not have quality? In many cases, presumably, one hopes, yes. There are, however, scientific journals of very high stature that reject a majority of submitted pages *before* expert review, on the grounds that the material described is not of general interest to their readers. This is not actually an evaluation of *quality*, merely of popularity. Even if the evaluation were correct, it gives no clue as to a paper's worth. The evaluation can, of course, be a

self-fulfilling property in that the huge battalion of folk waiting desperately to read what they want to read in their preferred journal will not now find this material. It therefore cannot be of interest to them. Mercifully, despite such editorial pronouncements, many readers also read numerous other journals, although I could be confused about this looking at the northern hemisphere tendency to ignore the southern hemisphere.

I once lunched with Sir John Maddox, the distinguished former editor of *Nature*, and we discussed the title of a paper I would have liked to submit to his journal because it would be of general interest. He enthusiastically agreed, but I could not get the data. So 'The stochastic incidence of DNA-independent RNA-polymerase in quasars' remains unpublished. I predicted it would have been highly cited, though rarely read.

It is my experience that some journals can generally be expected to contain more interesting or better-done work than others. The variance in any journal is, however, distressingly large for those topics on which I am competent to judge. So, the journal, per se is not an unarguable measure of the quality of a publication.

Scope of Content: Many highly cited and influential papers are descriptions of methods. It as an area of sociological research, or perhaps necromancy, why some methods get named after their inventor, without further citation, while others involve an actual citation. This has little to do with quality, per se, although, presumably, a method that does not work does not get cited very often. It is, however, an undeniable fact that citations are much more numerous (and half-lives much longer) for reviews of a field than for individual, original contributions. In my own case (Underwood 1981), I have been fortunate to write a review about methods, thus generating truck-loads of citations, including by numerous people who did their analyses incorrectly, despite citing my paper which said to do the opposite. Now, apparently, the errors are my fault!

A review can be of extremely great quality and influential in causing new thought and providing direction because of the originality and forcefulness of its synthesis. Or, it can be a compendium of what has been done with little other intellectual input. Both get cited a lot, but the latter can rarely be considered quality. Reviews can also be before their time, so may not yet be perceived to be quality. Or they may currently be unpopular and ignored because their outcome is the realization that all is not currently well in some area of work. For example, I have reviewed numerous proposals for grants and been reviewer and editor of several manuscripts about urchins, kelp and keystone predators that have not mentioned the problems with many models and hypotheses as outlined by Elner & Vadas (1990). If these authors are wrong, their critics should be pointing this out and not just ignoring them. If the proposer of a grant or the author of a paper is really unaware of a major review in a well-known journal, the quality of papers must be in a serious downward spiral.

Creating and maintaining quality. Be aware that the worth of a publication will depend on numerous factors well beyond an author's control, including current fashions, the foibles of reviewers and so forth. There are, however, some components of a publication which are amenable to improvement and therefore increased quality. Among these are, in no particular order:

Do what you say you are going to do: It is currently fashionable, or perhaps necessary because of funding, for many reports to be supposedly about management of resources, conservation of biodiversity, restoration of habitat, etc., etc. It is quite poor quality to read this in the Title, Abstract and Introduction and then to discover that the paper is actually an account of work (however good the work is) on a small number of some organism in one place at one time, with no clear relevance to how the goals, as claimed, could be reached. The Discussion usually informs readers that, without this information, it will prove impossible to manage, conserve, restore, etc. This is poor quality. If the paper is about the bigger issues of application of knowledge, its contents should clearly identify, consistently throughout the paper, *how*!

The problem is general and old. Many papers, from their Titles and Abstracts, are about *causes* of a phenomenon or *processes* maintaining a pattern. The papers themselves turn out to be a description of the phenomenon/pattern, with speculation in the Discussion about causes and processes. Quality is presumably positively associated with honesty.

Maximise the publishable unit: Try not to write lots of pages of overlapping content, using parts of one data-set. It becomes tedious, as an Editor, Reviewer, Reader to keep having the helpless feeling of déjà vu and then to discover one has indeed seen most of the material before. Sometimes one has seen it all before (e.g. Ehrenfield 2000a,b). Do not grow a C.V. by minimalism. The list of papers gets longer, but where is quality? It is the job of editors and referees to turn déjà vu into jamais vu.

But stay focussed: In contrast, keep each contribution focussed. One aspect of quality is that future readers come across a much-needed nugget in a past paper. They won't if they can't find it. There is too little time to read current material. Reading past material is usually much more selective and buried gems will not get unearthed if buried too deeply.

Know the current material and context: It is distressing to read so many papers which do not cite anything older than 25 yr. With my increasing age, this is increasingly depressing. Many of the older papers are

good; some are better than more recent ones. There is no disgrace in following others. There is, however, poor quality in pretending not to, or not knowing that you are. The former is exemplified by the sort of phrase in a modern paper: 'Results demonstrate that process X influences organism A in habitat B. This has also been found (or, worse, has been confirmed) by Y (1925) and Z (1968)'. It is quite hard for someone to have 'also found' or 'confirmed' a 2004 result some 79 or 36 yr earlier.

A personal example of the latter was a presentation at a recent conference about a proposed/starting PhD topic. To avoid embarrassment, the details will not be identified, but the proposal was claimed to be based on the novel discovery that limits of distribution of organisms on seashores were not due to physical factors alone, but were influenced by recruitment of larvae. This was non-novel for me (Denley & Underwood 1979, Underwood & Denley 1984) — particularly because these earlier papers received considerable opposition from referees on the grounds that their propositions were so unlikely.

A well-known example is that of 'supply-side' ecology (e.g. as reviewed by Underwood & Keough 2000), which had been forgotten even though earlier papers discussed, described and provided experimental data for it (see commentary in Young 1987, Underwood & Fairweather 1989). There are many examples of ideas fading from view (Jackson 1981, McIntosh 1985, 1995). It is scarcely possible to claim much quality for a paper that does not refer to the work which preceded it.

Do the study well: Whether or not a paper is descriptive or experimental, its logic and conclusions must be robust. Therefore, the methodology — particularly the sampling and experimental design and statistical analyses — must be careful and appropriate. There is nothing wrong with unreplicated studies, despite the widespread misreading of Hurlbert et al. (1984), who never said that there *must* be replication. What is wrong are inferences and conclusions that require replication to disprove confounded explanations, but the inferences are reached from unreplicated studies. Inadequate controls also prevent valid interpretation of experiments. Ecology is a science whether descriptive (Underwood et al. 2000) or experimental (Paine 1977). It deserves increased quality of its components.

Write the contributions clearly: Whether or not the topic is currently considered 'important', or the contribution is or is not currently labelled to have quality, it will be better if written simply and clearly. Readers (including me) who constantly battle to understand English will assess a clear account to be of greater quality. It is worth reading each sentence aloud to discover the nature of punctuation. It is worth having your graduate students read it; revenge is a powerful incentive to hunt out incomprehensibility.

To increase quality of any publication, eschew obfuscation. Never use complicated structures, with numerous sub-clauses, some leading into a maze of hierarchical concepts, with concomitant need to revisit the start of the sentence in order to determine how to interpret what is happening, unless circumstances conspire to force difficult and intertwined structures, however elegant and grammatically sound, because it will only confuse the reader. No — please don't.

Final comments. ***We can all improve quality:*** Everyone involved from the author to the editorial production can enhance the quality of publications. All it will take is persistent professionalism. More abstract concepts of quality defy quantification and are historically erroneous. Future needs will create quality for papers currently thought pointless. Past quality has often faded when the theory being tested turned out to be completely wrong in some later revolution of ideas (Kuhn 1970). We may still play snooker using Newton's past contributions of quality, but we do not give too much praise to his coincidental studies in alchemy (Clark & Clark 2001).

Assessing quality remains unreasonable: One of the most succinct assessments of quality of a piece of research is Moore's famous assessment of Wittgenstein's doctoral thesis. Moore wrote: 'It is my personal opinion that Mr Wittgenstein's thesis is a work of genius; but be that as it may, it is certainly well up to the standard required for the Cambridge degree of Doctor of Philosophy' (Moore 1929; quoted in Edmonds & Eidinow 2001). Having been forced to read some of Wittgenstein's later work (Underwood 1990), I think this may, in fact, have been a commentary on then-prevailing standards of Cambridge doctorates! It is, however, usually taken to indicate that the work was of great quality. Who knows what Moore really meant?

Personal foibles: Correspondence about the lack of quality of my own publications will not receive a reply. I have enough of that from those near and dear and close to home (who have, in fact, usually read it). This contribution was written to try to help improve quality in the future. It is not so useful to look backwards at my errors — even though I am aware of Santayana's (1905) statement that 'those who cannot remember the past are condemned to repeat it'. In fact, my analysis of this is that those who cannot remember the past cannot know (and therefore care) that they are repeating it. I am also impressed by Guedalla (1920) 'history repeats itself: historians repeat each other'. Please feel free to repeat my views widely!

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