A Century of Science Publishing
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Chapter 21
Peer Review: The Holy Cow of Science

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Introduction

Success in science depends as much on the scientist's ingenuity as on recognition of his or her achievements by colleagues. Positive evaluation of established results by one's superiors is the basis of any career. Published reports of recent findings, clearly having survived critical examination by outsiders, are its stepping stones. The sum total of admitted publications is the canonization of science. Peer review is the name of the infrastucture which supports the system.

Tens of thousands of scientific journals, in one form or another, delegate decisions on what to publish and what not to peer reviewers. Widely acknowledged as this practice may be. it is under attack. There is much dissatisfaction with the results, bitterly characterized by one of its critics as suppressing novel ideas and enforcing the rules set by a scientific oligarchy. Biotech and pharmaceutical companies may side-step the scrutiny of peers by keeping important scientific developments secret or making them public in the 'popular' press to boost their stock value. The system is undermined by the potential of the Internet to make instant and unsupervised publishing facilities available to all. Yet, not many editorial boards of scientific journals have shown awareness of the weaknesses of peer review by moderating its autocratic character. Industrial innovations will anyhow have to pass judgment by independent experts before being admitted to the market. And even the most fervent advocates of Internet freedom despair of the anarchy which would result from unlimited dissemination of scientific contributions.

1. Peer prerogatives

The development of scientific communication in the western world initially rested on Latin as the Lingua Franca. Throughout the Renaissance, travel, studying facsimiles in cloyster libraries, and attending lectures by famous scientists had been the only way to further one's understanding of nature and people. The educated few kept in touch with each other through correspondence and and travelling students, forming international schools of learning. The invention of the

printing process opened the doors and let the less privileged in. Gradually, knowledge spread. For many of the new intellectuals, experimentalists and inventors using a language different from their own to publicize their exploits would be a waste of energy. The drive of these newcomers, patriotic sentiments and a general humanistic mood made Latin lose its right of way. However, in the heartlands of Europe growing interests in biology, mathematics, astronomy, physics and mechanical matters were shared by many. Linguistic borders could not remain closed. Polyglots like Marin Mersenne in Paris and Henry Oldenburg in London found ways to satisfy the urge for knowledge by inviting, translating and distributing personal communiciations from scientists everywhere.

Mersenne (1588–1648), a monk belonging to the Franciscan order of the Minims, made his cell in the monastry near the Place des Vosges in Paris the center of intellectual development, in France as well as in Western Europe. No inventor himself, his inquisitiveness was inspired by his religion. He felt that the discoveries of scientists would confirm the truths of his belief. He was a man of great personal charm and in his cell he entertained many of the great names of his days. His correspondence served to keep him appraised of as many novelties as his legion of admirers could muster and he himself distributed the news to whom it might concern.

One of his correspondents was the son of a German professor of medicine, who had found a niche in the scientific community in London. This man of many languages and immense curiosity was Henry Oldenburg (1617?–1677) a born diplomat and teacher. He became acting secretary to the Royal Society when this was established in 1662, and his communicative skills soon extended the Society as a meeting of persons into an 'invisible college': letters from all parts of Western Europe provided the subjects for discussion in the weekly meetings of the London group. Letters then became a medium in itself, their flow and contents moderated by Oldenburg. In 1665 he published, on his own accord, transcripts, if necessary translated into English, in a monthly journal, which he named *Phililosophical Transactions*: giving some accompt of the present Undertakings, Studies and Labours of the Ingenious in many Considerable Parts of the World.

Among Oldenburg's considerations there was one which appealed strongly to the scientists of his day. By making their findings public they could ascertain ownership. With so many industrious thinkers and experimentalists in so many countries, working in so restricted an area as science then was, similar paths inevitably led to similar results. Establishing priority was fundamental. Private correspondence or oral communications could not serve this purpose. Oldenburg first devised a scheme by which preliminary notes could be put in a sealed box in the

Royal Society's safe. But in his journal the date of the claim could publicly be tied down to the time of receipt. This feature of the printed paper has not lost significance. Today's peer reviewed scientific journals still stamp original articles with the date of receipt. Since so many other methods of distributing news are en vogue (conferences, pre- and e-prints) it can be argued that archiving is becoming their main function. According to the *Journal of Electronic Publishing* (1998; 4/2) already forty percent of all journal readings concern articles older than the current year and twenty percent of the consultations go back four to fifteen years.

Mersenne and Oldenburg, moderating the news on seventeenth-century's innovations, in terms of today acted primarily as editors or journalists. Their personal vision on what was important or not has, however, had major implications on the directions in which science expanded. This responsibility now rests on the editors of scientific journals. A few society journals excepted, most of them have delegated this to their advisers, their peer reviewers. Typically, for journals with international readership these are scientists of repute, affiliated to scientific organisations of considerable renown, (usually) department heads, male and of Scandinavian, German, Dutch, British or North American origin. Nobel Prize winners are the 'crême de la crême'. In the self-propelling world of science, as represented by today's typical scientific publication, the peers of today are yesterday's most cited authors in their fields. Today's most cited authors wil be tomorrow's peers. It all revolves around professional proficiency, sharing specific research traditions, the availability of money, publications in leading journals, honours received. There is more to peerdom than reviewing papers. Peers also award funds and/or promotions. In the literature some see science as a hierarchy run by one or more power elites, who control both the rewards and the means of publication.

Recognition by one's equals is the key to peerdom, knowing which ropes to pull and not treading on important toes are further requisites. Peers are network builders and when someone is asked to review a paper he carefully checks how his judgments may affect his position. The names of the authors may have been removed and he may exercise his judications in anonimity — for the insider the subject of the research and the bibliography cannot be mistaken as signs of its origin. Perhaps, in the future, this publishing party may be asked to review the paper that this reviewer or his group has in store. Are there any old scores to be settled? Conflicts of interest to be taken into account?

There are many different types of journals in many different scientific disciplines. If we assume that a journal publishing one thousand articles per year has a list of three hundred and fifty reviewers to draw from and that every article is seen by two of them, every reviewer will judge on average six papers per year. If all five

hundred thousand articles published in the peer reviewed journals were to receive the same treatment, this would amount to sixty thousand peer/paper contacts and involve ten thousand peers. Since there are approximately five hundred thousand scientists active as authors (each producing four papers as a member of a team of four) to be on a reviewers' panel means that one is in the top five percent of scientists. Gross as these approximations may be (is it perhaps the top ten percent?) this is why peers do not demand payment and, on the contrary, invest an average of three hours of their (organisation's) time in the article under review. Affirmation of their status and foreknowledge of the accomplishments of their colleagues/competitors are their rewards. Hard pressed for time as he may be, a scientist asked to review a paper very rarely refuses to do so. The smaller the discipline, the higher the scientist's standing, the more he is besieged. Nobel Prize winners can afford to refuse or name a replacement, important department heads may farm (part of) the job out to their subordinates and the depth of the review is of course a referee's responsibility, but it would seem that a call to review is hard to ignore. In the end a reviewer is found for every single article of the hundreds of thousands published each year in the thousands of peer-reviewed journals.

2. Reviewing papers

Science as published in journals with international distribution is a representation of the beliefs of a specific group of scientists that their interests can be expanded if a given set of rules is followed. These rules are of Western European provenance, deterministic in essence, emphasizing linearity of approach. Every secret can be unraveled. There is no effect without a cause. What our senses do not perceive cannot be known. Findings are valid only if verified through replication. A scientist spurning these rules will be ridiculed or ignored. And it is part of the game that anyone's claim will meet with scepticism until substantiated by someone else. This is most true for 'hard' science such as (bio)chemistry and physics, and mostly true for the life sciences, such as (clinical) medicine and pharmacology. But where 'power elites' are in disagreement over the basic rules, as often happens in the behavioral sciences, the fight for a place on the forum between rivals may end in a feud.

The peer reviewer is in the center of it all. The harder the science, the clearer the rules, the easier this gatekeeper's job. He will focus on matters of interpretation rather than on procedures as his colleague for a psychology journal may feel compelled to do. He will reject fewer papers because his authors respect the framework he himself helped to construct. His mind will only be challenged occasionally when a paper leads to conclusions for which on first sight the arguments challenge the familiar logic. Is the author a step ahead of the rest of the field? Is he a crank? A

genius? It is every reviewer's nightmare to reject a paper that later will appear to have revolutionised the world. However, as a rule, the reviewer is said to be wary of novelties; progress in the discipline should be orderly and not cause eyebrows to be raised.

On its way to print a manuscript will be scrutinized by a reviewer only if it has passed the journal editor's first appraisal. It may be rejected outright if it is obviously flawed or deals with a topic clearly outside the journal's scope. If admissible it is dispatched to the reviewers. Most journals select two reviewers for each paper. The selection is usually a match between the editor's impression of the article's subject, the ramifications of the bibliography and his knowledge of the competence of his reviewers. A former editor of the journal *Science*, Floyd Bloom, used as selection criteria the following questions. Who is a peer of this author in content area, in technique and when possible in experience? Which reviewers should be avoided for competitive conflicts and past disputes? How much of the submittal's data can one expect the reviewer to examine in detail? For example, should the statistical conclusions be re-calculated?

The *Journal of the American Medical Association* (*JAMA*) published instructions for peer reviewers. They should judge if the manuscript's contents are original, if the data are valid, if the conclusions are reasonable and justified, if the information is important, if the article is of general medical interest and if the writing is clear or can be made clear. Some journals may also request their reviewers to adequately support their judgments, and to be alert to the failure of authors to cite relevant work by other scientists, to respect the confidentiality of the manuscript under review and to return the manuscript immediately to the editor if a conflict of interest is apparent.

Imponderables are characteristic for the reviewer's work. There is no universal standard procedure. No studies are available to clarify the exact decision making process. The least that can be said is that, as in any human diagnostic activity, there must be false-positive and false-negative outcomes, leading to erroneous recommendations. Disagreement between reviewers is common and perhaps partly due to this.

If reviewers disagree the editor may want to consult a third reviewer or decide himself. Otherwise papers are accepted, with or without modification, or rejected. The reviewers' comments are passed on to the authors. It is not uncommon that a paper revised by its author, on reappraisal by the reviewers needs another revision followed by yet another re-appraisal. No wonder that between the date of receipt of the first version and the date of publication in these instances delays of as much as a year occur. In less problematic circumstances time lags of three to six months

are the rule rather than the exception, the main cause being reviewers' overload or inertia. Authors hate these delays. Journal editors try to minimize them as much as possible in order to attract the best papers.

It is often said, but not verifiably, that a paper rejected by one journal eventually finds a place in another. Authors like to begin their publishing spirals by submitting their work to the most influential journal in their field. No wonder that these usually have high rejection rates, some boast seventy five percent. The higher the rejection rate, the more prestigious the journal.

3. Flaws and fallacies

Where, as in most cases, peer reviewers' names are witheld from authors, secrecy is regarded as the best way to ensure objective appraisal of the manuscripts. It is exactly this aspect of the process which elicits severe criticism. Some see it as diametrically opposed to the openness, freedom of speech and mutual trust which the academic community has proclaimed as universal ideals. They say its rationale is to obscure conflicts of interest of a personal, collegial, or financial nature and editorial responsibility. They quote a series of illustrious cases were faulty papers were admitted and later had to be disavowed but no one in particular could be blamed. Nobody knows who has been responsible for rejecting papers which later proved to be classics, such as listed in Campanario's article with the rhetoric title 'Have referees rejected some of the most cited articles of all times?' (*Journal of the American Society for Information Science*, 1996; 47/4, 303–310)

Some journals have adopted a 'double blind' reviewing process. Names of authors and their affiliations are concealed. Ideally, eliminating these elements would free the reviewers from possible prejudices. Investigations of the procedure have produced different outcomes. Some indicate that referees are more citicial if they don't know the author's identity, which would result in lower acceptance rates for authors not belonging to the top-ranked universities. Often, however, reviewers are able to identify the author on the basis of clues in the bibliography, or personal knowledge of the author's work. Generally, the advantages of the double blind method have not been so convincing as to convert the majority of editorial boards to adapt their approach.

Acting as reviewers, eminent scientists sometimes reveal their human sides. Anecdotes dealing with errors made and prejudices shown could fill a book.

There was the well documented trick Douglas P. Peters and Stephen J. Ceci pulled on a range of reputable psychology journals. They resubmitted twelve research papers from prestiguous departments which these journals had published some time earlier. Names of institutes and authors had been changed into fictitious

ones. Only three of the thirty six editors and reviewers of these journals detected the fraud. Of the nine articles left, eight were rejected, in many cases 'because of serious methodological flaws'.

Reviewers, obviously receiving papers in fields in which they are active themselves, may in their own work benefit from the information in these papers. There was the twelve year battle between Immunex and Cistron, ending in Immunex paying twenty one million dollars to Cistron in damages and handing over the patent rights to the interleukin-1 protein. Settled out of court the dispute centered on the fact that the journal Nature had sent for review an article submitted by a Cistron-sponsored team to an employee of Immunex and that subsequently Immunex had filed patent applications concerning this protein, in which some of the errors made in the article were replicated.

There was the complaint of the Brazilian biochemist Franklin D. Rumjanek who upon his return to Rio de Janeiro after having worked in England for six years and having published (in his own words) a 'reasonable' amount of papers, found that he had serious problems in getting his manuscripts accepted: neither the policies of the journals to which they were submitted, nor the quality of his work had changed.

In 1977 Rosalyn S. Yalow won the Nobel Prize for her work on the radioimmunoassay of a group of hormones, the first report of which some twenty years earlier had been turned down five times by different journals.

In an article analyzing the reviewing process for her own journal, Susan van Rooyen of the editorial board of the highly esteemed *British Medical Journal* (*BMJ*) to which yearly some five thousand papers are submitted concludes: "The process is by its very nature subjective, prone to error, or more kindly, to the kind of variation expected of any diagnostic test". The journal had after normal peer review, accepted a paper on risk factors for death in the elderly. The same paper was then sent to four hundred and twenty potential reviewers from the *BMJ* database; in it eight errors were introduced. Of the two hundred and twenty one who responded, none identified all mistakes and only a few spotted more than two or three of them.

Bias was so prevalent in their disciplines that they felt that the peer review system merited reform. This was the opinion of some forty percent of the responders to a survey conducted in 1989 among five thousand scientists by the Office of Scholarly Communication of the American Council of Learned Societies (ACLS). Of these almost two thirds responded.

Peer review is also felt to obstruct the publishing process. The Assocation of Learned and Professional Society Publishers (ALPSP) in 1998/1999 undertook a study on the motivations and concerns of contributors to learned journals. The

members of this association sent almost eleven thousand questionnaires to contributing authors. Over three thousand were returned. Of these almost eighteen percent mentioned peer review as a major obstacle in their publishing endeavors. A major reason of concern was the delay caused by the reviewers followed by superficiality of reviews, hostile reviews and of unqualified reviewers. The responders also had the opportunity of making additional comments about the peer review system. To quote from the ALPSP's report: "…the issue of reviewer anonimity was raised quite frequently and there were comments both for and against it…the superficiality of reviews was cited as a concern and also a disappointment…there were quite a few comments which centered upon the dismissive attitude of reviewers to work from authors who were based outside the major centers of excellence".

4. Added value

On the steep road to success a scientist's first publication is an important step. Publishing results of one's work is the accepted escape from mediocracy. The paper is perhaps not immediately noted by every colleague everywhere in the world. But it is essential in getting a foothold in the faculty or department. Without publications to show young scientists may be asked to leave after a couple of years. And for promotion or a move the longer the list the better the opportunities. There is of course a difference in the ranking of the journals a manuscript can be submitted to, but everyone knows how difficult it is to pass the threshold of the top ones — the list must be built in any case.

A second basic and historically more prominent reason to publish is the scientist's aim to be associated with the developments in his field. Some will end with their name attached to a theory or discovery but every scientist regards the results of his findings as his own and seeks universal acclaim for them. Since many people (teams) may be on the same path towards the claimed results, it is important to be recognised as the first person (team) who made these public. Although for (potentially) lucrative inventions patents now serve as the safest way to secure property rights, in this respect not much has changed since Oldenburg's day.

Scientific journals accommodate the need to be visible and to put one's stamp on science. They provide a permanent record of the scientist's achievements. Important as they may be for career advancement, formal, peer reviewed publications are by no means the sole source of scientific information.

Science is, perhaps before all, a personal thing: a universe peopled with inquisitive, ambitious, competitive, often egotistic human beings. They need to look each other in the face, hear each other speak, be member of the group, draw attention of their peers, become peers themselves. Each person's efforts rest on a

fundament of earlier endeavors. These are usually in the public domain. Other people may be building on them too. It is risky not knowing what people with similar interests are doing. It is also dangerous to deviate from mainstream research. Communication is the name of the game. There is more to it than publishing, for instance the meetings and symposia industry. International meetings in major interest areas easily attract twenty to thirty thousand attendees. Every self respecting subdiscipline of two hundred devotees meets every two to four years. A mid-sized congress brings two to three thousand persons together. One wonders what would happen to the airline and hotel businesses if scientists stayed home. But without the thousands and thousands of presentations, panel discussions and poster sessions featured by these meetings — not to mention personal contacts during breaks, at receptions and in bars — the grounded scientists would also suffer badly. Informal information, intelligence, is the butter on their bread.

But keeping informed begins at the working place. Colleagues have eyes and ears too. Coffee corners, departmental lunches, informal get togethers, briefings, budget discussions, collective research are situations in which information abounds. Preprints of articles intended to be published and received by (e)mail from colleagues for comment can also be an excellent source. Peer reviewed journals complete the spectrum. It is however not necessary, not even possible, to read all that is published in one's field. In most working places keeping up with the literature is delegated to a number of colleagues. In this respect the librarian (information officer) often plays a key role by alerting his patronage according to the interest profiles they have made known. Scanning citations and abstracts has become an art by itself. It is also no uncommon practice to glimpse at the contents lists of those few journals one trusts to publish leading papers, or issued by the society one is a member of. One may restrict one's reading to articles originated in specific institutes or even to those with familiar authors' names. And it is often quite possible to stay aware of developments by not seeing primary journals at all but reading review articles, meta research and digests in special journals or serial book publications, materials which may have been authored by peers but have escaped peer review as such.

Peer reviewed articles are the outer shell of the learned world. They apotheosize the power elites and are instrumental in the distribution of funds. To advance the insider's knowledge they are less essential. Science as the expression of human curiosity and industrial impetus will not be doomed if peer review dissipates. This might even be a blessing in disguise, at least for the medical community. The British Medical Journal's editors have boldly stated that peer review is erroneously credited as "being a good method of keeping poor quality work from publication,

whereas the evidence suggests that with persistence even the most flawed work will eventually find a home. Considerably less than five percent of papers in current journals contain a message that is both scientifically sound and relevant to doctors".

5. Horror vacui

Opportunities to publish in journals have always been regulated by the inherent constraints of the medium. There was an end to the amount of paper which could economically be distributed. Contrary to a popular misbelief commercial science publishers did not expand their positions rigorously, cheating the academic library out of their last cent. Anyone involved in this business has an archive of non viable proposals, pushed upon them by members of the scientific community's subdivisions in dire need of establishing their own specialty journal. Commercial publishers certainly pulled a few tricks which earned them a reputation of greediness. But there must have been benefits for the other party as well. And as quasi monopolists they do not in principle differ from journal-publishing learned societies. If 'there is no such thing as a free lunch' there is also no such thing as a publication designed to make a loss.

It is no wonder that if the Internet is felt to threaten the existence of printed journals, publishers whatever their lineage find themselves in the same boat. What they are facing has been outlined by a working group composed of members of the American Association for the Advancement of Science and the (British) International Council for Science which in 1999 presented a proposal on standards and practices for electronic publishing in science to the International Association of Scientific, Technical and Medical Publishers (STM) [see Ch. 25]. Internet offers many advantages to the individual who wants to make his ideas public. The added value of the electronic medium was summed up. The speed with which it can disseminate information The size of the audience it can reach efficiently. The enhanced indexing and search capabilities. The hypertext linkages to a wide range of material. The ability to be updated and corrected as needed. The interactivity, which enables real-time exchanges between authors and readers. The multimedia functions incorporating video and sound into text.

Publishers of traditional media may read this as follows. Every scientist, crank or genius, neophyte or professional, may freely publicize his accomplishments with features they cannot (unless at great cost) provide. Their quasi monopolies will flop. Is it strange that they find many influential scientists (openly or discreetly) on their side? Peers have much to loose too. It is they who are editors of the journals (often at substantial remuneration). They are sitting on editorial boards. They are reviewing the articles submitted to the journals they serve. But these journals also

serve them, making them visible as the top of their league and giving them the power to decide on what will be published and how. They can make or break careers.

The publisher-peer complex has one last line of defence. Scientific publications cannot be taken seriously if not peer reviewed. The line has its weaknesses. Publications can be scientifically valid if not vetted by peers. Ironically, this was the outcome of the brainstorming of the same STM working group. They specified that for any document to be considered as a valid scientific publication it must be durably recorded on some medium. It must be publicly available (not necessarily free of charge). It should remain in the same form and at the same location, so that it is reliably accessible and retrievable over time. A bibliographic record must be attached to the document and if new versions appear, to each of these. Versions should be certified as authentic and protected from change (even when occasioned by the author).

The proposal of the working group mentions quality control (in whatever form) as a necessary feature only for documents which, after having progressed from a preliminary to a definitive stage are meant to "contribute to the production of useful knowledge" and thus must be certified. "To maximize its usefulness for science, a publication needs to have been vetted to ensure qualitiy and to establish a high level of trust among readers. This process is equally essential for electronic documents — indeed perhaps more so in view of the vast quantity of available information." Certification is, however, not regarded as a prerogative of the traditional peer reviewer. The proposal entrusts publishers, authors, professional organisations, research and archival institutions, funders of research with future quality control and with organizing the relevant infrastruture.

But here is the twist. The (STM) publishers' admonitions are not supported by the broader scientific community. In the ALPSP study referred to above significant concern was expressed that peer review should continue, "as its loss would be very detrimental to the quality of published work". Almost seventy percent of the responders were satisfied with the system in general terms against thirty percent who were more or less dissatisfied. Recommendations for improvement were rare. "Only a few people said that the idea of electronic peer review is appealing." And: "Overall, the only scenario that all authors would like to see happen more than it is at present is open peer review post publication". What they perhaps had in mind is something like the succesful (but time-consuming) editorial process adapted by the journal *Behavioral and Brain Sciences*, where printed articles are co-published with up to 30 peer commentaries, plus the author's response. As the editor, Stevan Harnad notes: "Peer commentary is no substitute for peer review; the journal is

very rigorously peer reviewed. The Internet offers the possibility of implementing peer review more efficiently and equitably and of supplementing it with what is the net's real revolutionary dimension: interactive publication in the form of open peer commentary on published work."

6. Peers will be peers

The number of (refereed) electronic journals in science, engineering and medicine has increased rapidly in recent years An electronic repository of preprints (the Los Alamos Physics Eprint Archive) pioneered by physicist Paul Ginsparg receives some 2500 submissions and services 30.000 hosts a week. It forced the American Physical Society to open an Internet link between one of its journals and the archive, also making manuscripts available before peer review. The interactivity, which is a basic element of the archive, in some aspects fulfills (peer) review functions. Authors are reported to be meticulous in updating their articles with changes suggested by colleagues. The project obviously benefits from the traditional self-discipline of physicists, which earned them low rejection rates in peer reviewed journals. It may be difficult to copy in areas where seventy five percent of submissions to journals is returned to the authors on first sight.

This is, however, exactly what is going on at the moment. As a director of the National Institutes of Health, Harold Varmus in 1998 created turmoil by planning to establish a stand-alone pre-print server for non-reviewed articles. He was subsequently forced to incorporate the concept into the existing PubMed bibliographic service of the National Library of Medicine, but the beginning was there. It may gain momentum by an initiative, which in 2000 intended to attract groups of researchers wanting to create their own niche journals on-line and allowing preprints to be stored in and retrieved from an e-print server closely co-operating with PubMed. Initiator Vitek Tracz, has already made his marks in the publishing industry. With BioMedCentral he promises to "make the publishing process much more efficient and flexible in both format and economic terms". Research reports should be published electronically with all the features the medium allows and be "available to all — globally, free and without barriers tot access". Will it work? It had not exactly a flying start. Equally hesitant to burn their ships behind them seemed scientists when, late in 2000, challenged by Varmus (by then ex-NIH) to refuse cooperation with publishers who would not make their publications freely available on PubMed six months after the date of publication. Varmus may have misjudged politically inspired misgivings regarding the monopoly the US-based repository would have. He may have underestimated the toughness of publisher/editor/author allegiances, embedded in years and years of intellecual, professional and

financial interdependency. But publishers were scared. There will be quite a few authors who simply do not appreciate the complexity of the marketplace. There will also be 'maverick' reviewers: those without established positions in the publisher-peer complex, dissatisfied with the way things work and believing that they should be paid for their efforts. And what about librarians who will cancel subscriptions on journals if the contents of these will be freely available? How crucial is a six month delay anyway for most printed publications if there are so many other ways of spreading vital news? If the number of responders to Varmus' call will gain critical mass commercial and institutional publishing traditons will fade away. And how will this affect the peers?

The editors of the *British Medical Journal*, when denouncing the value of peer review did so on the occasion of introducing an e-print server for clinical medicine and health research. They chose as a title for their article: Moving beyond journals: the future arrives with a crash. Prediction or prophecy? Perhaps just a premonition phrased to shock indolent readers. In their article a dichotomy between researchers and practicians is revealed. The latter "are likely to continue to want to receive predigested, well presented accounts of research that matters for their practice… a role that journals are likely to continue to have in the future". This does not sound like slamming doors in peer reviewers' faces. The distinction between researchers and practicians is also somewhat casual. It is self evident that in well-delineated scientific (sub)disciplines validation and presentation of preliminary research reports is subordinate to their 'news-value' and that e-print facilities will be appreciated. But the existence of peer-reviewed journals in heavily researched areas demonstrates that the availability of 'predigested, well presented accounts' is not without some consequence.

The balance between authors, peers and publishers is precious. It may shift. But will peers lose their pivotal role? Scientists use alternative validation systems in their day-to-day routines. For these to replace peer-reviewing, albeit in categorial environments such as specific disciplines or professional organisations, they need to be accepted as trustworthy. Adequate infrastructures must be established. The Internet may facilitate all this. But more is needed than isolated initiatives to cannibalize functioning validation structures such as the reviewing of scientific papers. Peerdom may don a different cloak but organized science needs its hierarchies.

Related readings

[1] Bloom, F. (1998) *Human reviewers: The Achilles Heel of Scientific Journals in a Digital Area.* Presented at INABIS'98 – 5th Internet World Congress on Biomedical Sciences at McMaster University, Canada, Dec 7–16th. Keynote Address.

[2] Boorstyn, D.J. (1991) *The Discoverers.* Harry N. Abrams Inc., New York.

[3] Editorial, (1999) Moving beyond journals: the future arrives with a crash. *BMJ*, **318**, 1637–1639.

[4] Daniel, H.D. (1993) *Guardians of Science.* VCH, Weinheim.

[5] Harnad, S. (1998) Learned inquiry and the Net: the role of peer review, peer commentary and copyright. *Learned Publishing*, **11**, 283–292.

[6] Lundberg, D. and Williams, E. (1991) The Quality of a Medical Article. *JAMA*, **265/9**, 1161–1162.

[7] Meadows, A.J. (1974) Competition and the urge to publish. In: *Communication in Science.* ch. 2, pp. 35–65.

[8] Moran, G. (1998) *Silencing Scientists and Scholars in other fields, etc;* Ablex Publishing Corporation, Greenwich Connecticut, London, England.

[9] Peek, R.P. and Newby G.B. (1996) *Scholarly publishing: the electronic frontier.* American Society for Information Science, The MIT Press, Cambridge Massachusetts, London, England.

[10] Peters, J. (1995) The hundred years war started today: an exploration of electronic peer review, *Internet Research, Electronic Networking Applications and Policy*, **5/4**, 3–9.

[11] The Association of Learned and Professional Society Publishers, (1999) *What authors want.*