Journals: redundant publications are bad news

Publishing the same work twice is unethical and casts doubt on the integrity of research.

Sir — We have developed an electronic systematic search tool to estimate the amount of duplicate publications in the 70 ophthalmological journals listed by Medline. Our results show that there is a considerable number of duplicate publications. If this holds true for other disciplines, it is bad news for research.

For our survey, we matched the title and author(s) of each of the 22,433 articles published in the 70 journals between 1997 and 2000 using a duplicate-detection algorithm[1], and found that 13,967 pairs of articles give a matching score of 0.6 or more. Of these, we manually reviewed a random sample of 2,210. We found 60 genuinely 'duplicate' publications and estimate that 1.39% of the analysed articles are redundant. Because of the very restrictive selection process and the impracticality of detecting all duplicate publications, and because the estimated amount of duplicates increases with lower matching scores (Fig. 1), we regard this estimate to be the tip of an iceberg.

Of the 70 journals, 32 were victim to duplicate publication — 27 journals published the first paper and 26 the duplicate, on average 6.4 months later (standard deviation 4.7, range 0–21.3 months). We found no statistically significant difference between the average journal impact factor of the first (1.13) and the second journal in which the duplicate article was published (1.42) (Wilcoxon-signed ranks test $P > 0.1$). The analysed publications were by 210 authors, suggesting by extrapolation that a total of 1,092 authors could have been involved in redundant publication during the time period that we analysed. The scientific conclusions of the original and of the duplicate(s) were identical in 88.3% of cases; we found slight changes in 6.7%; and major changes (different results despite identical samples, or omission of patients) in 5% of cases.

Duplicate publications are unethical. They waste the time of unpaid, busy peer reviewers and of editors; inflate further the already over-extensive scientific literature; waste valuable production resources and journal pages; lead to flawed meta-analysis; exaggerate the significance of a particular set of findings; distort the academic reward system and copyright laws; and bring into question the integrity of medical research. Republication of data yields no benefit other than to the authors.

It is important that journal editors can trust their authors. Although many duplicate publications are discovered by careful peer-reviewers or editors, they cannot provide complete protection. Scientific journals can combat redundant publication in various ways[2], but in practice the penalties for duplicate publication are minimal[3].

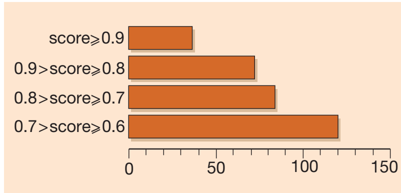

Figure 1 **Estimated number of redundant publications for matching scores of 0.6 or more, where 1 = total overlap.**

Proper deterrents are needed: for example, better education on publication guidelines, the introduction of registers for planned and ongoing clinical trials, and a change in assessment criteria from quantity to quality when papers are submitted for posts or grants. As long as publications remain the central requirement for academic advancement, a reasonable solution seems unlikely. Nevertheless, it is imperative that the problem of redundant publications be addressed, for it is the responsibility of all those who care about objective research and evidence-based medicine.

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Journals: how to decide what's worth publishing

Sir — Your News Feature (*Nature* **419,** 772–776; 2002) raises important questions about the reliability of peer review, but falls back on the justification often used by editors to shield themselves from widespread dissatisfaction with the system as currently practised: "If it ain't broke, don't try to fix it".

We believe it may never have been working in the first place.

Perhaps peer review, in its current form, cannot be expected to detect fraud. But can we even rely on it to improve the chances that what is published is the best science, communicated as accurately as possible, and that what remains unpublished is dispensable?

Various studies, mostly in biomedical journals, have reported only modest author satisfaction (at best) with the review process, irrespective of the quality of the review. Papers that eventually became very highly cited were often rejected by the journal of first choice. Peer review is costly, biased, can be inefficient, does not always identify important work, and can allow publication of articles with serious deficiencies or omissions.

Rather than falling back on the churchillian cliché quoted in your feature that peer review is the worst system in the world except for all the others, members of the research community should cooperate to answer several questions.

We need to know whether peer review (in whatever form) is more effective than alternatives. Does it identify submissions of higher quality than do other selection methods, or chance, or no selection? Does peer review significantly improve the clarity, transparency, accuracy and usefulness of published papers compared with the submitted versions?

If peer review in its current, descriptive form is ineffective or less than effective, we should experiment with more analytical forms of assessment. For example, the quality of a new study could be assessed in the context of a pre-existing systematic review of studies on the topic. Such a population approach may make it easier to assess the contribution of an individual new study. At the same time, assessment should be standardized and specific for different experimental designs, and peer reviewers should be trained to use a single, structured-assessment instrument.

Ultimately, it is the larger population of readers (rather than a possibly biased sample of referees) who should decide whether the changes made during review substantially improve the document as a record of a peer's contribution to science. New systems should be tried that involve readers in the review process either after 'traditional' review (as in post-publication commentary, rapid replies and the like) or by developing a 'definitive' text by consensus before publication. Language experts have been investigating readers' reactions to texts for many years; it is time for editors and publishers in the 'harder' sciences to use their methods to extract useful experimental data from these reactions.

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Journals: impact factors are too highly valued

Sir — Linda Butler in Correspondence (*Nature* **419,** 877; 2002) shows that researchers in Australia are publishing more papers since the number of publications was introduced as a performance indicator for research. Butler points out there is now "little incentive to strive for placement in a prestigious journal. Whether a publication is a groundbreaking piece in *Nature* or a pedestrian piece in a low-impact journal, the rewards are identical".

The point is well-made, but her phrase highlights another growing problem in measuring performance which, if unchecked, threatens to have a major impact on science policy and progress. The problem is an over-reliance on journal impact factors to judge the worth of scientists.

It is increasingly common to hear scientists making snap judgements about the quality of others' work simply by perusing the names of the journals in which they publish, with no actual attempt to read their papers. This is a dangerous habit, for quite brilliant work can appear in a 'lesser' journal, either because its subject is not currently fashionable or because its author has special reasons for preferring a specialist forum. The habit is also dangerous because it erodes the capacity of the research community to determine its own direction.

An ex-colleague of mine, for example, liked to publish his excellent work on nerve regeneration, which could have been published anywhere, in a very specialist surgical journal because that is where he thought it would be most likely to inspire immediate clinical use.

The professional editorial staff of very high-impact journals such as *Nature* have a primary responsibility to the success of their journal: circulation, advertising, impact statistics and reputation. Deluged with submissions from authors hopeful of publishing in a journal that will give them bench-credibility in a world of instant judgements, these editors must screen submitted papers to see if they meet the journal's needs before sending them out for peer review. Therefore, most submissions are rejected for reasons other than flawed scientific reasoning.

I have no criticism of this approach: it makes sense in the commercial world of journal production. The problem arises when scientists and administrators of science use the placement of papers to judge the worth of researchers, the worth of institutions, the best places to award grant money and the best places to fund fellowships. The more we couple allocation of resources to publication in 'top' journals, the more we are effectively handing over the direction of research to a small group of professional editors, who never sought this responsibility and who (excellent at their intended jobs though they may be) are unlikely to be the best people to bear it.

Most of us are, at least sometimes, the judges as well as the judged. If we do not consistently take the trouble to judge papers by their content rather than by their location, the direction of science will come to be determined, however unintentionally, by an editorial élite. We shall have only ourselves to blame.

Jamie Davies
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Bright students enjoy correcting the textbooks

Sir — Students aged 16–17 have been doing chemistry research at Westminster School for the past five years (see the News feature "Put your lab in a different class", *Nature* **420,** 12–14; 2002). Our projects all have their origins in the normal curriculum, as many points of quite elementary chemistry have not been investigated for half a century or more. With modern techniques we can amplify (and often correct) what is written in the standard textbooks.

Our first paper, on the addition of hydrogen halides to alkenes, has now been published (*J. Chem. Soc., Perkin Trans. 2,* 810–813; 2002), and other work is nearing completion. Students gain by having to think about a problem for a year or more, and experiencing the disappointments as well as the satisfaction inherent in original work.

Too often, bright pupils are put off from studying science because they think they will be asked nothing more demanding than to reproduce received wisdom. I hope that the presence of active research groups in high schools may help to correct this misconception.

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Animal research needs organized defence

Sir — Your Opinion article "Promoting animal research" (*Nature* **420,** 447; 2002) delivers a much-needed message.

Ten years ago, I volunteered to join a National Institutes of Health programme to educate young people about the need for intact animals in biomedical research. Local high schools and colleges were sufficiently receptive to encourage me to continue.

In the past four or five years, however, my approach to the education authorities has fallen on deaf ears, although all of the teachers, and many of the students, voiced praise for the programme early on. One teacher told me that they did not want to run into problems with animal activists by allowing me to speak. Most of the population is disappointingly uneducated about science and a significant percentage is anti-science.

Next month, I will send approximately 30 letters to local colleges and high schools in an attempt to rekindle the interest. I believe that major biomedical and medical societies, and journals, should constantly urge an educational campaign to deliver our message to the public.

People like myself are very willing to volunteer to speak, design handouts, and so on, but a central focus group is needed.

Charles G. Smith
Address supplied

DNA discrepancy

Sir — We should be able to trust any author, whether or not a scientist, to deliver an accurate description of the past. Indeed, your final editorial of 2002 exhorts scientists to work to retain the public trust (*Nature* **420,** 719; 2002). Thus, it is even more unfortunate that *Naturejobs* states in the same issue (*Naturejobs* 3; 19/26 December 2001) that Rosalind Franklin and Maurice Wilkins worked on DNA structure at the University of Cambridge: they were famously, of course, at King's College London.

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