

A grumpy scientific editor

Mike Augee

fossil@well-com.net.au

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I have a friend who was, until he retired, a minister in the New South Wales' state government for a while. His portfolio included several areas involving matters of conservation and natural resources. I asked him one day where he got information on which to base decisions that involved technical or scientific matters that were outside his background (Masters of Business Administration). The answer was that he relied on the technocrats in the relevant department. Also, of course, from the media. The "Daily Telegraph", "Five O'clock News" and other popular sources of information have a lot to answer for.

If we consider the sources of scientific and technological information available to the minister, they can be arranged in an upside down pyramid. Basic research papers in journals and similar publications would form the base; the primary source. At the next level are books written by authors with experience in research in the field about which they write. In my field of mammalogy these would be books like "Life of Marsupials" by Tyndale-Biscoe, (2005) "Physiology of Marsupials" by Ian Hume (1999), and the CSIRO (formerly UNSW) series of short books on specific mammals. Such books are anchored on basic research results, mainly from research journals. They are the secondary source.

At the next level are more popular and easier to read books written by scientists with a particular knack of good writing or authors who specialise in this area. Their content is much broader than these books cited above. The authors are people like Tim Flannery, Vincent Serventy, Jared Diamond, and E.O. Wilson. You can count David Attenborough among this lot even though his medium is mainly television. These are what I call the tertiary sources.

Beyond this of course are pop-science TV shows, newspapers, and anything published by a religious organisation. Here is quantity not quality; the top of the upside-down pyramid. Far too often these are actually opinion pieces, and it is rarely possible to ascertain the source of information included therein.

When they were doing higher education, the people who will later become advisors to politicians and 'experts' in government departments would have been exposed to the primary literature. As time goes by and busy careers restrict time for reading, they might, with luck, read from the tertiary level. The quality and appeal of publications and media at this level is crucial. However, that quality must always come back to the quality and availability of the primary resources - research publications.

Research publications are the base from which the dissemination of scientific and technological information builds. This is where the data are, and as such these publications are incredibly important.

I believe, however, that the primary research publications base is weakening. I am a bit grumpy about what is happening to papers that come to me as an editor. I have the feeling that there has been a worrying degeneration in quality. I am not talking about those things that have worried editors forever, such as spelling, punctuation, and quality of illustrations. In many ways those problems have been lessened with the use of spellcheckers and computer programs for preparing and refining graphs and figures. No, the problem is in content.

I believe that the content of papers that scientific journal editors are receiving now is debased. This is the reason I'm grumpy and the reason that I decided to raise this debate.

I am receiving manuscripts for the journal I now edit (*Proceedings of the Linnean Society of New South Wales*) which the referees say are technically correct. There is nothing really wrong with them that would allow rejection, but they are partial. They are just bits of research.

I would like to say to some authors: "Why didn't you do the survey in winter as well? Why didn't you sample the strata above and below? You really could have tested that by doing another technique. Maybe you should have done a uranium-thorium, as well as a thermoluminescent test". The resulting papers are somehow unsatisfying and seem incomplete. And, as any editor knows, even the skimpiest and most incomplete Results section will usually lead to a multipage presentation.

Discussion

The other sign of what is happening is the rising use of "weasel words". I guess I am a one-man campaign against the weasel word "indicates". I hate it. It means the authors haven't really shown anything, nor have they supported anything; the work submitted simply "indicates". I reckon we could test my hypothesis about the decrease in the significance of published papers if we could obtain a count of the number of times the word "indicate" has occurred in scientific papers over time.

There are also "weasel statements". I now refuse to print that overworked phrase "This work indicates further works needs to be done". Well, why wasn't it done? Why didn't the research team just wait to publish when the work was complete? The reasons have been shown in at least three submissions to this book (Calver 2013, Recher 2013 and Kingsford 2013 in particular). The bean-counters have taken over.

Career progression and funding have come to be under the control of bean-counters. Bean-counters deal with quantity; the concept of quality is unknown to them. Accountability, for grants as well as promotions, is required. More and more research workers are on contract, and

much depends on the number of papers published. Some contracts lay out specific directions such as the need to have published two papers a year, three papers a year, or even more in some cases. The way to do it, as editors are now seeing, is of course to publish small papers along the way instead of waiting until a project is finished or a research goal at least substantially realised.

To test this hypothesis, I took all of the papers by the Linnean Society during the decade 1980-1989. The average paper length was 23 pages. About 2008 I began to notice a change, and from 2008 to 2012 the average paper length was 17 pages. Papers are getting shorter. I recently published the largest volume that I ever published, it had 23 papers in it. Three of those I would be willing to have my name on them as an author. The rest I simply would not wish to be associated with. Not because they were wrong. They had gone through referees, sometimes they had gone through referees two or three times, and the Editorial Committee agreed to publish them. They were, however, simply fragments of research. They contained little data and much review and discussion.

The questions are: Who will put these fragments together? Who will take the time to read and evaluate a multitude of minor papers in fields that are growing more and more specialised? How will good, important research findings filter up into public policy and public awareness in general?

The correction to this trend will require some huge steps. To return the emphasis away from quantity back to quality is very difficult. Quality is hard to measure. Various indicators such as “impact” have failed to reward and thus encourage quality in publication. They measure quantity. Those indicators based on the nature of the journals in

which authors have published, fail entirely to deal with the realities of the internet.

Quality can probably only be measured by people who are familiar with the field of research under question. Academics and other researchers are not immune from the bean-counter syndrome, especially when they have risen through it, but they are still in a better position to evaluate quality than people, such as administrators, whose training does not lead them to recognize quality in science or technology. Accountability for research quality and the awarding of benefits that derive from it should not be in the hands of accountants.

Researchers who put their names on papers as authors need to take on responsibility not just for quality in terms of accuracy, but also for quality in the wider sense of completeness.

Editors can, of course, play an important role. Editors might find it hard to evaluate quality for publications not in their own field, but that simply requires good selection of referees. Editors, however, even more than referees, are in a position to make a decision about content. Is there enough data in a research manuscript to warrant publication? Is there even enough data to justify the Discussion? I intend to apply these questions much more strictly in future.

A final grump. Maybe all of this is futile. Maybe authors will find easy publication of minor papers on the net. Maybe the concept of thorough refereeing will fade away. Maybe future public policy and public information will be based on information from Wikipedia. Well, it does beat the “Daily Telegraph”.

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