

Hartemink, A. *Publishing in Soil Science – Historical Developments and Current Trends*. International Union of Soil Sciences, 2002. 196 pp. €17, softback. ISBN 90-6672-075-1.

My first reaction on seeing this book was to ask myself why I should review a re-issue of papers about papers. Two of the articles simply chart the progress of soil science and of the journal *Geoderma* in particular. Another compares soil research in the tropics with that in the temperate regions. There is enough new stuff to digest without re-reading this. Then I remembered that Hartemink's originals contained some nuggets of wisdom that are worth passing on, and so I have culled them for you, adding a little gloss. These appeared in the author's series of articles for the *Bulletin of the International Union of Soil Sciences*.

Many of us know that impact factors are distorting what we do to satisfy the bean-counters. However, it was nice to see our Journal way ahead of the field in 1997 with a factor of 1.81, even though we have slipped somewhat since (second with a factor of 1.56 for 2001). What I liked best was that on average the shorter are the papers in a journal the larger is that journal's impact. Perhaps it was to be expected; but Hartemink's message is one writers would do well to remember – if you want to be read then keep it short!

The same article reinforced something else that many of us knew: society journals, such as this one, contain about three times as much information per unit price as commercial ones do. Institutional librarians and money-bags take note.

Fraud is a worry in some fields of research, but happily Hartemink found no evidence of it in soil science. What is worrying is that the editors of almost all the leading journals in soil science reported attempts at dual publishing, i.e. authors' submitting virtually the same articles to more than one journal. The practice seems to be on the increase as scientists try to accumulate points for their *curricula vitarum* and now that it is so easy to reprocess texts in new formats. It might not be fraud, but it is certainly unethical and possibly illegal under copyright law. Most editors, once they uncover such attempts, reject the offending papers outright.

Several editors (myself included) see as unethical the appearance among the authors of the names of people who have contributed little or nothing to the work reported. Authors are the authorities for what is written, they carry the responsibility, and only those who have taken part in the research are in that position. Directing a laboratory, obtaining funds, and routine assistance do not confer scientific authority, and the people who do those jobs can be properly acknowledged at the ends of papers. Then there is the inclusion of names (usually of senior scientists and often without their knowing) on papers (usually by juniors) to add credence. This is both unethical and frequently embarrassing when papers are seriously criticized or rejected. Don't do it!

Hartemink pursues the notion of ethics into citation and referencing. Credit others for their results or ideas on which you have built; 'be generous', he says. And in this he is supported by other editors, some of whom I suspect have their eyes on the impact ratings of their journals. I learned a few months ago of an editor who almost demanded that contributors cite recent papers in his journal, the impact factor of which has risen dramatically as a result! I am somewhat out of sympathy with Hartemink on this matter. Many of the papers I receive contain quite unnecessary citations for what is common knowledge or readily inferred. It is as though their authors are afraid to shoulder responsibility for what they

write, that they must lean on others for support or blame them if things turn out to be wrong. I therefore welcomed Anthony Young's short contribution. When an editor asks for the source of an observation reply, 'This very article, I saw it, and now I'm telling you about it'. I mind the advice given to me by R.K. Schofield as I set out on my career: 90% of papers are of little consequence; try to identify the few really important ones on the subject, and concentrate on those. They are the ones to cite. Schofield certainly practised what he preached, and, looking back, I doubt whether he missed anything significant.

Hartemink closes this set of articles with the remark that the current culture in which scientists are encouraged to write ever more papers is self-defeating. People have not got time to read them, and 80% of them are never cited. He suggests, perhaps with tongue in cheek, that each scientist be allowed no more than two papers per year in peer-reviewed journals. That would concentrate their minds on the important science with ample data to back it, and 'salami publishing' would die. Interestingly, the British Road Research Laboratory had spotted something similar 40 years ago: anyone publishing more than two papers a year was suspected of spending too much time writing at the expense of too little at the bench or in the field!

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