

Publish or Perish (6) – Soil science for pleasure

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with contributions from

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*I can sympathize with people's pains, but not with their pleasures.
There is something curiously boring about somebody else's happiness.*
Aldous Huxley (1894-1963)

*Next to enjoying ourselves, the next greatest pleasure consists in preventing
others from enjoying themselves, or, more generally, in the acquisition of power.*
Bertrand Russell (1872-1970)

1. Introduction

I assume most of us have not chosen to become a soil scientist in order to get rich or to obtain a Nobel Prize. I guess most of us are soil scientists because of the great pleasure it takes to investigate, teach about or muddle through the upper few metres of the earth's crust. Of course those pleasure are different reflecting the wide interests of people; some are interested in microscopic studies, others prefer the pedon, field or watershed.

As opposed to the quotes from Huxley and Russell given above, I think everyone has the right to derive pleasures from his or her activities whether that is in the laboratory, field, lecture room or behind the computer screen. Since this series is dealing with publishing in soil science I will focus on the pleasures of publishing. As there is no publishing without writing, this article mainly deals with the pleasures of writing; first some general trends followed by my own experiences and those of some colleagues.

2. The trend

More and more soil science papers are being published each year, which is the result of (i) the increasing number of soil scientists, (ii) the increase in efficiency (e.g. computers), (iii) the recycling of ideas, and (iv) dilution of experimental data over various papers (iv)

combined efforts by teams of scientists. Above all it reflects the pressing atmosphere in many academic departments and institutions to publish. It may not necessarily reflect advances in soil science but it does show that many soil scientists are productive writers. Now I will discuss each of the causes for the increase in the number of papers.

Although exact figures are not available, the number of soil scientists increased largely in the 1960s to 1990s. Total number of ISSS/IUSS members increased from 3958 to 7042 between 1974 and 1998 (+78%) (van Baren et al., 2000), whereas over the same period the world population increased from 4.15 to 5.86 billion (+42%). This shows that the relative increase in members exceeded the relative growth of the world population. Part of the increase is caused by the influx of persons attracted not by science as vocation but as source of money and jobs (Phillip, 1991). Apparently, working as a soil scientist is pleasant, and many soil scientists like to write what they have found.

A second cause for the increase is the use of computers by which it is far more easy to create graphs, tables and figures and to copy-edit text. Computers and the electronification of literature searches and retrieval have greatly reduced the time needed for writing papers. I think the time needed to read a paper or comprehend a method or complex problem has not changed unless people have got cleverer. Not so likely, and the only thing that we know and that can be observed when browsing through old literature is that the quality of papers has much improved. Not necessarily the quality of writing which is on the decline due to a new form of language *Netspeak*, but the quality of the science. Well, at least that how it looks like but it may have to do with the fact that we know so much more.

So computers have contributed to the increase in the number of soil science papers. But not always. In a recent tribute to the great Australian soil physicist¹ John Philip (1927-1999) it was mentioned that he avoided computers and for much of his career relied on a mechanical calculator. He performed calculations lying on the floor, and drew graphs by hand (think of this next time you open a new spreadsheet or Sigmaplot). In the same tribute, a list of papers by John Philip is given and in his 45-years of publishing John published more than 300 papers, many of them single-authored and groundbreaking. So to some extent this shows that computers are not essential for a large publication record although John of course had secretarial assistance for his

¹ This tribute appeared in the *Australian Journal of Soil Research*, vol 39(4) 2001 and was prepared by close colleagues from CSIRO and ANU. It is puzzling why they call him "environmental physicist" instead of "soil scientist" or "soil physicist". It is perhaps exemplary for the changing of names from "soil science departments" into "departments of natural resources" and from soil science into bio-geo-ecology and alike? Selman Waksman who was awarded in 1952 the Nobel Prize in Physiology or Medicine for his studies of soil microbes that have led to the discovery of streptomycin, the first antibiotic remedy against tuberculosis, undersigned his letters with "soil microbiologist".

manuscripts and a draughtsman for the graphs. How many soil scientists do have that today? We have computers.

Another trend that has occurred in the publishing of soil science is that an increasing number of papers are based on desk studies using existing data. An analysis of all papers published in *Geoderma* between 1967 and 2001 revealed that in the 1970s and 1980s about 60% of the papers was based on studies conducted in the lab, but in the 1990s the share of lab papers declined to 40% (Fig. 1). Most remarkable is the large increase in papers based on desk studies to about 40% in the late 1990s. Although this is the trend in the papers of one journal only, it shows that soil science is increasingly becoming an office science. There is an increasing group of soil scientists that never touch the soil and mainly sits behind computer screens.

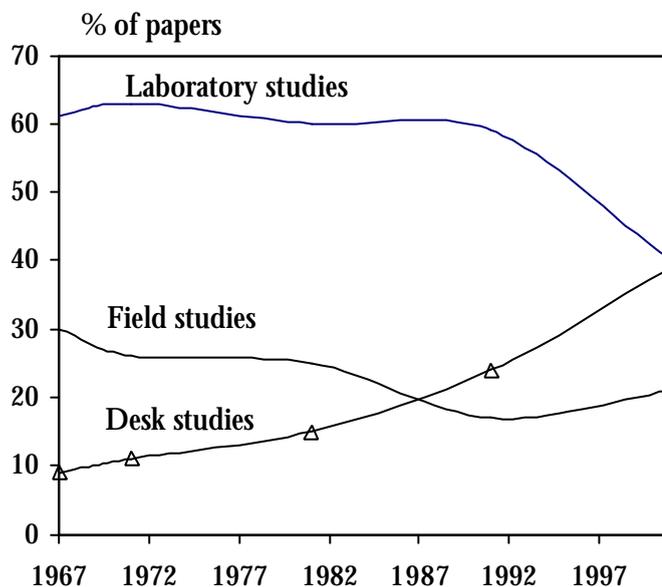


Fig. 1. Trends in papers from field, laboratory and desk studies between 1967 and 2001 in *Geoderma*. Based on 2079 papers. Modified from Hartemink *et al.* (2001).

The third and fourth cause for the increase in the number of papers is the recycling of ideas and the dilution of experimental data over various papers. A common complaint of journal editors is that manuscripts contain premature information or that papers contain too much overlap (Hartemink, 2000). More papers to fill a CV or departmental requirement. As Richard Webster recently unearthed an interesting remark from G.V. Jacks: "...most scientific papers advance the scientists rather than the science".

The last cause for the increased number of papers is the formation of large research groups, often interdisciplinary, which requires that each contributor should end up in the list of authors in the resulting paper. This could be called the author-contribution-dilution effect. It may be that co-authors are now listed who in the past would have been acknowledged like for example technicians, but it may also be that some people are automatically listed because they are heads of the group or institutional tycoons. There seems a fair deal of confusion on the rules for authorship, but there is a trend that there are more authors per paper. Not many data are available but the analysis of *Geoderma* papers showed that the average number of authors was 1.7 in 1967, 2.5 in 1990 and 3.1 in 2000. The number of pages per author decreased from about 9 in 1967 to 7.2 in 1996 (Hartemink et al., 2001). So authors contribute less per paper. Recently an editor of *The Lancet* mentioned to me that he had seen the first paper with more than 500 authors.

3. Some of my own experiences

Reading and writing are complementary joys although it is easier to read for prolonged periods than to write. Both reading and writing can give new insights. Reading can give direct new information, whilst writing can give insight through the ordering of information into a logical or desirable sequence and form. I can hardly imagine that it is possible to do one without the other, but I tend to think that some people write more than they read – and if they read it might be mostly their own manuscripts, proofs or final papers. The more I read the more I notice how poor some people express themselves. Devoted reading is the forerunner for effective writing (Janzen, 1996).

In writing a paper I have about three stages that gave me pleasure. The first and most enjoyable stage is the actual writing. It is the stage of putting information and thoughts together and making discoveries in the data. I have to create the right atmosphere (phone off the hook, email offline, cleared desk, door shut, music from Boccherini or Bach) but if that is provided and I have a clear framework, I may be able to type in a few thousand words. Later on in the train or the next day it appears that much of the text can be thrown away as it is irrelevant, duplicating or sheer nonsense. I will then double the remaining number of words, throw away again half of them and this process continues until the first draft. It seems a terrible inefficient process and although I become better with time, I still require a lot of dustbin space before a manuscript is finalised.

The second phase giving me pleasure, is the acceptance of the paper after revision. It is the time at which I have added something useful to the scientific community – I think. The pleasure is usually short. Seeing the final print and offprints is also enjoyable (your name in print!) but I have noticed that such joy diminishes with age. When my first paper was published I remember I read the offprint from A to Z but now I quickly glance through them and file them. Routine pleasures, and I rather read other work than my own. Despite this assumingly blasé attitude, there is something addictive about publishing and I think it is the first stage: the actual writing (of course with the knowledge that it may eventually end in print).

4. What others find

I have asked a number of colleagues to write a small section about the excitement and pleasures of writing, or where and how they write. I mentioned that their contribution should be of interest to young soil scientists but also for more experienced writers. Here is what they wrote:

Peter Buurman Peter.Buurman@BodEco.BenG.WAU.NL

During the first decades of my career, writing papers was a matter of typing a first version (I never hand-wrote manuscripts), and going through stages of cutting and pasting. The major influence on my writing, however, was not the introduction of the computer, but the fact that I spent a couple of years in a research institute in Indonesia, sharing a room with two colleagues. The colleagues were fun, but the room was also the waiting room for our boss, so all visitors stopped at our desks for a bit of short talk. It was as if we were doing our work in a corridor. In this situation, I developed an ability of acute concentration, which helped me very much in all writing matters.

I write, perhaps, in two different ways. In both cases, I write up everything that comes to my mind, and not always in a logical sequence - ordering is for the second version. If I have an idea where I am going, I start with the introduction, and write the whole concept in its proper sequence, indicating where I have to look things up or to add references. On the other hand, if I don't know where the data will lead me, I start with a detailed description of the results (I usually have a lot of analyses to interpret). Graphs of data are very useful. When I describe the results, I find what the trends are, which anomalies occur, and whether there are any data that I cannot trust. A detailed analysis of data gives ideas of how to present them both in writing and graphically. I find out what I

do or do not understand, which gives entries for discussion and conclusions. Especially with complex data sets (and most are), it helps to first look at the separate data and later find connections or correlation between data. That way, a paper writes itself. Even in published papers we sometimes find that authors have not made optimal use of their data, and I don't want this to happen. On the other hand: don't extrapolate your data too far.

Once I have a complete manuscript or section, I make corrections on the paper copy. The result never looks pretty: it is usually possible to improve phrasing, syntax or sequence, and the corrected manuscript is ablaze with red marks (Yes, for better contrast I annotate in red ink, both my own manuscripts and that of colleagues, even though this may seem offensive at first). After revision: throw away the previous version; if you keep it, it will hobble your progress. After publishing, throw away your manuscript, but keep the original figures.

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Pleasures of writing? I actually find writing hard work and so "pleasure" may not be an appropriate word I would use for such an activity. Also, in my dealings as editor, I have found that how people first react to rejection is an important indicator of long-term success. Many become withdrawn and afraid to continue to submit their findings for publication. Others simply ignore the review comments as stupid and thus don't really gain from them. The wise person is one who carefully sifts the comments and then takes those that are useful and puts them to work. I have tried to follow this latter approach in my work, but it is not always easy.

I had been an academic scientist for almost 15 years when I experienced my first rejection of a paper submitted for publication in a journal. A post doctorate had left me with a vast amount of data that seemed to be completely lacking in integration. I spent many hours plotting, replotting, writing and rewriting a paper on growth conditions that affect EPTC (a herbicide) degradation by various *Rhodococcus* strains. I was convinced there was an important story that needed to be told if I could just tease it out of the data. The work was not the best that had been done in my laboratory, but it did address several concepts related to how best to grow cultures of this Gram positive microorganism. The resultant manuscript was a rather modest package of 12 text pages along with two additional tables and four figures. I had no doubt that all of my hard work in analyzing the data would pay off. After all, I had experienced nothing but success in all of my previous journal submissions.

The rejection that came back from the journal was, therefore, a shock to me. Comments such as “The manuscript has serious deficiencies.....” and “although the conclusion that..... is not surprising, these results will be useful in further experiments” initially caused me to be defensive. After allowing some time to pass, so that I could gain a little more perspective, I reread the review comments and agreed that most were appropriate. I had simply overstated my ideas and their importance. The reviews gave me an outside perspective on where I should refocus my work and what areas of deficiency needed to be addressed in order to move forward. I also comforted myself with the thought that many great scientists have had papers rejected for publication and I need not become defeated. Instead, trying to act as professionally and objectively as possible, I decided to work through my temporary disappointment and move on. Taking what I had learned from the work of the post doctorate and the comments of the reviewers, I continued to sharpen my objectives and research methods.

I have since continued the work on EPTC degradation in soils and by cultured strains of *Rhodococcus*. Several Ph.D. students have completed dissertations studying the metabolic pathways of EPTC degradation and their genetic aspects. Several of the key concepts in this later work grew out of the paper that was rejected. Other work mentioned in the rejected paper was simply abandoned.

Is their joy in experiencing rejection? As most athletes will tell you after they retire, it is not the championships they miss as much as the joy of competition. The process of creating the data and writing it up for publication was hard work. The rejection was painful. Yet I experienced something in the entire process that was beneficial. In hindsight, I realized I did experience scientific growth during the hard work of organizing and writing the paper - and ultimately at least some of the work was proven useful and ended up in subsequent publications.

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A famous Australian supermodel, perhaps slightly more renowned for her physicality than her intellect, when questioned about her reading habits, replied that she read only what she wrote. Sometimes when I peruse journals I think we have catwalks of supermodels in soil science. I generally enjoy reading stuff I don't write and savour reading much better than writing. Nevertheless we are obliged to provide fodder for others.

The scientific paper is pretty much a straightjacket. It has its own structure and by the time the idea has been hatched, the work designed, the data gathered and analysed, the results mulled over and confronted by the ocean of literature, the writing is fairly straightforward. The pleasure for me comes in trying to take the utilitarian straightjacket

and transform it into an elegant garment that might have been designed by Yves St Laurent. Adding ribbons and sashes and sequins and fine needlework in the form of interesting quotations and turns of phrase, in writing down ideas that seem counter-intuitive or putting them succinctly in equations, or showing that all this was known a hundred years ago but we've all forgotten, or some brave new idea for the future. It's not free in form like writing modern poetry but there's a challenge and a pleasure in darting in the literary direction.

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My very first experience with publishing a paper goes back to the mid-1960s. Although I did not write the paper myself - I was one of the co-authors - I was extremely excited about it and showed it proudly to my parents. Seeing my name in print gave me an exceptional feeling and, in fact, I still have that feeling. It might show some pride, but don't we all have this?

In the 1960s and 1970s, my publication output was very low because there was no incentive. Soil chemistry and soil fertility studies were of local interest and ended up in reports. I am convinced that a lot of useful information has been published in reports, many of them written in other languages than English. At that time I even did not know the expression 'peer review'.

My first big experience with an international journal was with the *Soil Science Society of America Proceedings* (which has been renamed the SSSA Journal) in the early 1970s. I submitted a rather short paper covering thermodynamic calculations on the spontaneity of nitrogen reactions in soils. But it got rejected and I was very angry about the reviewers' comments. Apparently, they did not understand thermodynamics and the editor-in-chief followed the reviewers. I wrote a letter showing their wrong comments and my paper got accepted within a minimum of time. I was very proud again. Later on this little paper opened the door for a one-year fellowship in a laboratory in the US. I am still using that example to show to young scientists the importance of publishing good material in high-ranked peer-reviewed journals. One good innovative paper can change the direction of a scientific career. However, strange enough, when I returned to Belgium after my stay in the US, the publication pressure disappeared again because of the lack of incentives. It drastically changed 10 years ago. From the late 1980s and early 1990s research proposals ask for proof of expertise in a certain field. And the best proofs are peer-reviewed papers! Therefore, I continuously encourage my co-workers to publish in peer-reviewed journals. At all occasions I insist to publish results as soon as there is

enough material to proof a hypothesis. I insist the paper should be short, as I also prefer to read short papers.

The availability of journal information, ISI data, Web of Science, etc. has helped my laboratory to select the proper journals and we changed from one or two peer-reviewed papers per year to more than ten papers per year. If someone had asked me ten years ago to publish ten papers a year, I would have said this is not serious as I am not a publication machine. Now, I realise that it is indeed possible if one has in mind the publication of a paper already at the time of setting up the proposal. That is the way to go! Publishing provides the scientist the ultimate pleasure of carrying out research.

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Pleasures of writing? It is a hard graft at the time. When I was younger, before the days of word-processing, I used to get up at 5.00 a.m. and write for 2 hours before the day's work, like the novelist Anthony Trollope (1815-1882). The pleasure comes afterwards, on seeing the proofs, if you realize it is a job well done.

I've written five books, none of them best-sellers, but all appear to be appreciated by colleagues. In particular, reviewers seem to think that I know how to write: "A stimulating text", "The arguments are carefully weighed, cogent and lucid", "Entertainingly written and most carefully structured". I like that last one as I do try to make it readable and, as far as the subject matter permits, entertaining. (Did you think there were not many jokes in *Land Resources: Now and for the Future* published Cambridge UP in 1998)? They were 'censored' from the text - so to find them, skim through the Endnotes.)

So what advice can I offer to those who would like to write? First, a book should be written for the benefit of its readers, not its writer. Think who your readers will be, and write for them. Secondly, the KISS acronym (told to me by Pedro Sanchez): Keep It Simple, Stupid; rather making a concept sound difficult in order to show off that you know, try to understand it so well that you can explain it clearly. Thirdly, have lots of summaries: I now put them at the beginning of chapters, or in text boxes. And lastly, whilst presenting all the evidence with proper scientific objectiveness, once you have reached your conclusion, then seek to convince others, try to write in a persuasive manner.

The standard of Abstracts in most journals is terrible. These should be informative, not indicative, i.e. give the results, so that 95% of readers will be spared the burden of having to read the article. Every journal author should read advice by that finest of soil science writers, G.V. Jacks (1901-1977), "The Summary" (*Soils and Fertilizers* 24, 1961, 409-410). Jacks' own 'Abstract' of this? A poem: "Take out every surplus letter--boil it down./Fewer syllables the better--boil it down./Make your meaning

plain. Express it/So we'll know, not merely guess it;/Then, my friend, ere you address it, BOIL IT DOWN." Rudyard Kipling (1865-1936) said the same thing: "Words, sentences, even whole paragraphs that you have deleted from your first draft are like ash raked out from a fire: no-one knows it has been done, but everyone feels the warm glow".

Finally, cut down on references. The former, academic, need to acknowledge precedence can be overdone: "The rain in Spain falls mainly in the plains (Shaw, 1913)". I have been taken to task by editors asking what is my source for such-and-such an observation, when the answer is 'Young (2001)', this very article, I saw it, now I'm telling you about it!

Yes, I have enjoyed writing this contribution. But what matters is whether you have taken 'Pleasure in Reading' it.

5. Concluding remarks

Everyone has the right to derive pleasures from work but I am afraid the ever-increasing number of papers might become a burden to the soil science community as it requires (i) a large system to produce, and (ii) it is difficult to keep abreast of real developments. It would be better to use the money for real research. In many departments there is a rule on the number of publications per year, varying from 2 to 8 papers in international journals. Papers in conference proceedings, book chapters, newsletter articles or whole books are usually not counted or are given little credit. Most soil scientists have accepted the rule, know how to play the game, and produce vigorously. And with pleasure. But as we all know 80% of the papers are never cited. Pity of all the work.

It might be better if the rule was that each researcher was allowed to write a maximum of 2 publications in peer-reviewed journals per year. This would avoid the dilution of experimental data as scientists will only publish the real important and hot material and forget about the rest. For those who really enjoy the writing it would still be possible to write a monograph or a large web based version of their research paper. This proposal would not take away the pleasure of writing. Given the current publication culture it is likely that such proposal will not gain wide acceptance but I think it would be far more efficient and be good for science as well.

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