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Ettore Lanzarone  
Francesca Ieva *Editors*

# The Contribution of Young Researchers to Bayesian Statistics

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**Part VI**  
**Suggestions for Young Researchers**

# Chapter 39

## The Point Is... to Publish?

Fulvia Mecatti

**Abstract** Writing papers is an essential part of the research process. Researchers have a professional obligation of disseminating their results, making them available for others to use to enhance common scientific knowledge. Besides the fun of sharing their own ideas and views, to publish is essential in order to actually have a scientific career. Although scientific writing certainly has its own conventions and standards, I suspect there is no a unique true recipe making the trick. As a matter of fact I do not have any. However my quite long time in the academic arena has given me a pretty clear idea about how I do and do not like things done. In this paper I will be giving my personal view and rules, in the hope that sharing my own experience would do some good to others as it did for me.

### 39.1 From the Big Technical Rules to My WHW Rules

Shortly after being asked to give this talk I realized I said yes a bit too quickly and started having second thoughts. Of course I was grateful since addressing young statisticians is certainly a part of my job that I do love. I thought I could give them a good nice recipe for disseminating effectively the results of their research.

Except, I did not have any. I needed preparation.

So I started the easy way, surfing the Internet and in fact finding a huge amount of material: big technical rules “*Do this and that,*” “*Go this way not that,*” “*The right thing to do is...is not,*” and on and on along these lines. Sadly, they also contradict slightly too often. For instance somewhere in the process I came across this really convincing statement:

Make sure to do all the research *before* start writing

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F. Mecatti (✉)

Department of Sociology and Social Research, University of Milan-Bicocca Building  
U7 Via Bicocca degli Arcimboldi, 8, 20126 Milan, Italy  
e-mail: [fulvia.mecatti@unimib.it](mailto:fulvia.mecatti@unimib.it)

and a couple of Web pages after that I found myself totally disoriented in front of the following assertion:

Start writing early and often. Don't leave all the writing to the end and then try and write it all out in one go.

Trying not to be discouraged I went on and I was really starting to believe in this

Pack all the good ideas in the first pages of your manuscript, even into the abstract: readers are busy people and get bored quickly

only to stumble shortly after upon this

Disclose ideas slowly and leave the best results until the last: you will capture reader's attention from the beginning to the end

Should I choose the one to be trusted best and feed that to my young audience as the *truth*? Would you go and try to decide which one is *the correct way* to do things? I would not and in fact I did not. Thus, no big help over the Internet, except perhaps a raising suspect that there might not be such a thing as the big technical rules. Besides I knew for a fact that I was about to face much better net-surfers than I am. They were so not needing me that I decided to leave that to them.

Second in my list were books.

So I went to the library and in a couple of my favorite bookshops: shelves after shelves after shelves of confident books about *what to do and do not*, and *how to make it work*, in your mother language or in a second language. Tons and tons of literature, there already. They could go and help themselves, they did not need me either. In the end I made my decision: I would have left all that to them and gone personal. I would have made it clear that they were not going to be given some tight big truly rules about how to do or not to do things, for I was not sure I could do that or even it would exist. Instead I was pretty sure I could tell them how *I like* or *I do not like* things done, hoping *my* experience would do some good to them too as it did for me.

Thus it will be *my view* and *my rules* and only occasionally a few basic technical rules, so basic it would be impossible not to agree with. And in this case it will be no more than a couple of them. Mostly they will be *Why, How, and Where*, namely WHW rules. I will be talking about that in different contexts and declinations of the subject of disseminating the results of scientific research, statistical research indeed.

## 39.2 The Point *Is* to Publish

In my opinion, there is little point in having good ideas if you cannot communicate them to others. Besides teaching, being a researcher is the fun of a scientific/academic career. In the everyday life this translates being always in (at least) one of the following three statuses:

1. You have an article *nearly* finished.
2. You have an article *about* to be started (or two, three, etc., no actual limit here).
3. You are stuck somewhere in the *middle* of an article.

As a matter of fact writing articles is an essential part of the research process. But it is not the end. There is also the dark side, those gory details leading to the *publish or perish* paradigm. I am sure you know what I am talking about here, both despite of and due to your young academic age. As scientific authors you do write for the fun and pure joy of communicating ideas. However you also need to write for tenure and promotions. In order to actually have a scientific career, you do need to be published. When the goal is to go through the steps of an academic career you are essentially what you have published.

So the point *is* to publish and it is time for some WHW rules, assuming that something to publish already does exist. That is, we are not talking about the research process, instead we have already some nice research results worth disseminating. Thus *why* to publish? To publish is the main way to communicate your ideas and at the same time it is functional to your academic career. It is essential to be aware about that and to find soon your own balance between these two sides of the very same moon. Besides, research is a shared matter and it has its own ethic. Researchers have a professional obligation to both perform research and disseminate the results of that research, as objectively and as accurately as possible.

Science is not an individual experience. It is shared knowledge based on a common understanding of some aspect of the physical or social world. During the birth of modern science in the latter half of the 17<sup>th</sup> century, many scientists sought to keep their work secret so that others could not claim it as their own. Prominent figures of the time, including Isaac Newton, often avoided announcing their discoveries for fear that someone else would claim priority. The solution to the problem of making new discoveries available to others while assuring their authors credit was worked out by the secretary of the Royal Society of London, Henry Oldenburg. He won over scientists by guaranteeing both rapid publication in the society's Philosophical Transactions and the official support of the society if the author's priority was questioned. Oldenburg also pioneered the practice of sending submitted manuscripts to experts who could judge their quality. Out of these arrangements emerged both the modern scientific journals and the practice of peer review. Once results are published, they can be freely used by others to extend knowledge. But until the results are so widely known and familiar that they have become common knowledge, people who use them are obliged to recognize the discoverer by means of citations. In this way, researchers are rewarded by the recognition of their peers for making results public. [1]

Although digital technologies are creating new forms of publication, publication in a peer-reviewed journal remains the most important way of disseminating a complete set of research results. The importance of publication accounts also for the fact that the first to publish a view or a finding—not necessarily the first to discover it—tends to get most of the credit for the discovery.

### 39.3 How to Share and the RELUKE Rule

The leading tool for disseminating research results is a paper. A paper is an *organized* description of the research—from conjectures and hypotheses to conclusions—intended to *instruct* the reader. A statistical paper is on average 8–15 pages long. Of course there are also thesis and textbook. However, in statistical research, book writing is rare. It is mostly subsequent to a long research, summarizing and perhaps concluding a wide research project. This is also the case of a final thesis, each of us has at least a couple of experiences about. Let us keep it for another occasion since they are quite a different story; let us focus over papers and the key ways to share a paper. There are two main ways for academic authors to disseminate a paper

1. By presenting it as a talk or as a poster at a conference or at a seminar
2. By publication in a peer-reviewed journal

In Stats—where almost all my experience is—these are certainly the most frequent medium and also the most rated for both the bright high goal of disseminating scientific ideas and the dark dirty goal of making a living and getting promoted in academia.

*How to do it is the major challenge.*

A way to start that makes sense to me is to think and state what my objectives are, what I want from what I am doing, whether writing a paper for publishing or preparing a presentation for a talk. First I do not want to waste everybody's time: the author's, audience's, referees', and readers'. And second is the RELUKE rule. That is the re(ad), l(isten), u(nderstand), and (lu)ke rule: I want people to listen/read me till the end of my presentation/paper. I want people to understand what I am saying/wrote. And I want people to like it. Presenting or writing it is of course about you but not you alone. As already mentioned, a paper is a description of the research *and* is intended to *instruct* the reader. Never forget about the audience. Whatever you are doing for your research dissemination, whether preparing a talk or a manuscript, you are talking or writing *for* someone. The process is a constant in and out: it is about you the author, your results, and what you want to share (*in*); at the same time it is about your audience/readers, someone *else* who is going to listen/read you (*out*). Whatever the way you chose to share, it essentially means: know the focus of your paper *and* be clear of your intended audience, working out what you believe they already know and what they might not know. As a conclusion I would like to share another good rule I was given when I was a young researcher myself:

If you did want to learn how to write: *read a lot.*

If you did want to learn how to prepare and give a talk: go to conferences, attend seminars and *listen a lot.*

A golden rule indeed.

### 39.4 Presenting a Paper in a Talk: My WHW Rules

*Why* presenting a paper in a talk? Many journal articles do begin as talks presented in professional meeting such as a conference, a workshop, and a seminar. Public presentation is often the first step toward writing an article and trying to publish it. It is a good way to present intermediate results of a larger research. It certainly is a valuable opportunity for getting criticism and suggestion so that to refocus your research. It is an occasion for networking and meeting other people interested in the topic and researching in the same field. For me, presenting intermediate results at a conference helps in meeting deadlines and imposing discipline to my work.

*How?* A presentation with slides is the usual option. The challenge here is how to make it work, according to the RELUKE rule. You do have this precious thing of an audience; what you want is to make them listen to your talk, understand it, and like it. Assuming you have nice results to present, first of all: *time*. You are always allotted a well-defined time for your presentation, which you are going to meet out of respect for your audience, for the speakers scheduled after you, and for the organizers. Conference slots are usually no longer than 15 min; it might be 10, rarely 20. For a seminar the time is longer but still constrained, usually 45 min or 1 h. Thus you cannot put *everything* into your presentation, into your slides, into your talk. Maybe you are so lucky that your audience includes the guru(s) very experts of your subject. Except possibly them, no one else will have the time to do the mental process that costed you so long to produce your results. Be aware of your time and careful in selecting what you can give people in no more than those minutes. Secondly, *speak up* and discuss spontaneously your own slides. In Stats, reading a prepared talk is a rare practice. It might seem comforting, you may think to be better in control, but you will just result as boring. Not worth the effort! Third, control your speech *speed*. Speak slowly and loud, making your words clear, especially if English is your second language. Avoid sounding rushed or breathless. If you realize you are not going to make it in saying all you would have wanted to say, cut something from your talk. Less yet clear is far better than more yet incomprehensible. A good deal of rehearsal will do the trick here.

*Where?* You may be giving your talk at a seminar or at a conference. To give a seminar you are normally invited by the organizer, usually at a different institution from your own. In choosing a conference you should consider a basic classification. A conference may be *general* as for a periodic meeting of a statistical society; some examples are the biannual scientific meeting of the Italian Statistical Society every even year; the annual Joint Statistical Meeting of the American Statistical Association, every August joint with 6 other societies; the biannual meeting of the International Statistical Institute every odd year. Usually you do not need to be a member of the society to attend the meeting; however if you are going to present a paper it can be requested for a membership of at least one of the authors. Any statistical paper, concerning any fields of statistical research, submitted to the

**Table 39.1** Top ten tips for a really hideous presentation

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1.	Present slides very dense and in text size no greater than 10
2.	Pack your slides with many horrible formulae
3.	Abound in different text fonts and colors
4.	Use many acronyms and technicality that only you (and possibly some very experts in the field) can understand
5.	Take your cell calls in the middle of your speech
6.	Avoid originality and personality
7.	Read your slides instead of a spontaneous discussion
8.	Speak in a low voice and make a lot of ehm-ehmming
9.	Speak unclear and very quick
10.	Pass your slides fast and furious

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scientific committee of the conference, will be considered for the presentation. Otherwise a conference may be *specific*, meaning focused upon a special subject such as this very conference primarily intended for young Bayesians. In this case, before submission you need to be checking carefully if the subject of your paper meets the areas of interest for that specific conference. A conference, either general or specific, is usually organized in sessions that we can classify in 4 main categories:

1. Keynote or plenary session, where a senior speaker is invited for a lecture on a topic he/she is supposed to be an expert.
2. Invited or specialized session, where an organizer is in charge to think to a special topic, usually innovative or where research is very active, and to collect a small group of invited speakers who have published on that topic. It is common practice to add a discussant to the session, who is another speaker not presenting his/her own paper but in charge to start and animate the discussion with questions and comments over the papers presented in that session.

The remaining two types of sessions are probably more interesting and more likely for young statisticians.

3. Contributed session, which is a set of spontaneously submitted papers, sometimes gathered around a common topic but not necessarily.
4. Poster session, which is alternative to an oral presentation of your paper. You are allowed a space where to post your paper, displayed as a poster of fixed size, and you are requested to guard your own poster for all the session and be available to interested people for face-to-face discussion.

There is great variability in the review-acceptance-refusal process of a conference and each conference committee usually has its own rules. However any committee will state very tight deadlines for submission of title, abstract, short paper, long paper, and revised versions.

### 39.5 Writing a Manuscript for Submission: My WHW Rules

With a written paper, people has of course more time to read as compared with listening a 15 min talk. Sadly this not necessarily implies to pay attention. Readers are busy people. Assuming you have something to write about, writing a manuscript needs per se special care and skills. And it always takes far more time than planned.

*Why?* To think that your main objective is *to publish* is in fact a limited thinking. For me, the worst thing that could happen to my manuscript is not rejection. It would be to get published and then lay there unread and uncited. Still the reluke rule! We are all very busy (and sometimes slightly arrogant). As a consequence to pay time and attention to a paper not strictly related to our current research is a tough yet quick decision. I myself have the habit to decide to go on and read a paper on the basis of title, abstract, and author(s). This essentially means that if you are not yet an affirmed author—and you need to work hard and long to become one of them, presenting and writing a lot—chances are that you will get to be read on the basis of title and abstract. My suggestion is to put quite a lot of cure on those. Still, even if I decide to actually give the paper a go and read through it, I would start with the beginning and the end. That is, I would read the Introduction and the Conclusions. After that I normally would go through the entire paper as long as I was clear it would be potentially useful to the very research I am working on or I am interested in at that particular time (and in that case chances are I will be needing to read and reread several times and always spend over it more time than initially planned). My point here is you do put a lot of care into the Introduction and the Conclusions also. These are the parts I myself usually find most difficult to write.

*How?* Statistical scientific writing of course has its own formal conventions about article writing. We will be articulating this a bit but very quickly. Because the good news is if you accomplished the golden rule *read a lot* you will be absorbing subliminally such rules and conventions. Papers are mostly organized the same way, the key composing parts being summarized in Table 39.2. The Title is a challenge: it should be short but clearly telling the reader what your article is about. The Title and Keywords are fundamental for having your paper properly indexed and showing up in computer searches. A simulation is intended as an exercise carried on in a wholly controlled environment, usually a large set of Monte Carlo runs over artificially

**Table 39.2** Standard components of a statistical paper

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Title
Abstract and Keywords
Introduction
Notation
Method and Theoretical Results
Simulation and Application
Conclusions
Acknowledge
References
Appendix

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**Table 39.3** Top ten tips for writing truly boring dull papers [3]

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1.	Avoid focus
2.	Avoid originality and personality
3.	Make the article really really long
4.	Do not indicate any potential implications, applications, and developments
5.	Leave out illustrations (too much effort to draw a sensible drawing)
6.	Omit necessary steps of reasoning
7.	Use abbreviations and technical terms that only you and some specialists in the field can understand
8.	Make it sound unnecessary serious with no significant discussion
9.	Focus only on data
10.	Quote numerous references for trivial statements

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generated data. On the other hand an Application is performed by means of a *real* data set. Application results might be the main objective of the whole research as well as the way to show that the proposed method is actually applicable and able to produce an outcome of use. Both Simulation and Application are intended to offer clear empirical evidence to your claim and many journals rate both crucial for a manuscript to be considered for publication. Conclusions are meant to help the reader reorganize ideas. The Reference, as well as citation in text, must be double-checked: you do not want to give the reader ground to doubt your reliability. Finally your manuscript might need an Appendix for boring proofs and tricky analytical details.

As for some tips on writing well, I have just one: keep it simple *and* specific. Readers—especially the intelligent ones—have higher probability to be impressed by ideas and clear expressions rather than by elaborate constructions and excess of words. Rather say it once clearly, than several times verbosely. Vague and murky statistical writing is perfect ground for just one suspect: reduced to simple it does make little sense or it is too banal to be worth saying. On the other hand, simple does not mean trivial or superficial. You do not need to be wordily in order to be deep and accurate. Simple and specific serves both the author and the reader: it saves reader's time *and* author's reputation. Here are my personal rules to implement the simple and specific writing:

1. Avoid superlatives and go easy with adjectives: "*excellent*", "*the best and the most*", "*very useful*", and the like.
2. Be careful with adverbs and judging: "*the right thing to do*", "*obviously*", "*of course*", "*rather*", "*certainly*", and so on. Here the question is: *who for*?

You do not need much of that to be precise and meaningful. You can delete most of that with no sacrifice to meaning and sounding. My first drafts are usually heavily judgmental, stuffed of superlatives, and of *adding* things such as adjectives and adverbs. In revising I simply cut it away and very rarely I have finally found my

paper missing any of them. If English is not your mother language—as it is not for me—my main advice is: go and get a good spell and grammar checker, either in your editing software or over the Internet. Moreover, I have two preferred suggestions [2]

1. Prefer active verb over an excess of nominalization. For example, in

We conducted a review of the evolution of the method

review *and* evolution *and* method is an overload of nominalization. Reexpressing as *active verb* the statement is clearer, isn't it?

We reviewed how the method evolved;

2. Prefer active verb (if available) over a generic verb like *to do*, *to get*, and the like. For instance in

Modeling of the random-effects distribution nonparametrically has not yet been done

*done* is a generic, empty verb. How about rewording as

The random-effects distribution has not been modeled nonparametrically.

Enough, right? Also . . . don't you think that try and write keeping in mind all those *how to*, *avoiding*, *do and do not* would distract you from writing the ideas and results? Surely it does for me. So my hint is: first have a first draft done. Write for writing, just put down all the ideas and results, trying not to worry over the form, the good language, the embellishment. Only then, you do remember all the *good writing tips* and start revising your first draft. This basically means a lot of rereading and reworking your paper. For me this is the stage where time is at maximum risk of spinning out of control. Be careful and do not be carried away by obsessive revising: perfection is not to be reached in this world. And of course I have developed my own two tricks to stop obsessing over the rereading and reworking of a manuscript:

1. Be patient and leave the draft there for a while. Go for a walk, distract yourself with stuff. After some time, a couple of days or possibly a week, you go back to your manuscript for another reading with fresh attention. You will be amazed about how packed of overlooked details and typos to fix and rearranging your draft still needs. Of course this should not stop the submission forever, my suggestion here is: once it *is* enough. If you are lucky enough of not being a compulsive reviewer, be patient as well: try and not to submit the very second after writing the last word.
2. Look for a second pair of eyes (even a third could do). Ask one or two of your colleagues, advisors, or seniors to read your manuscript before submission. Be prepared for criticism and accept it graciously. If you bothered to ask, you should bother to listen. If you were just looking for a pat on your back or uncritical encouragement, shouldn't you have asked your pet?

### 39.6 ...and the Grand Final: Submission and the Publishing Process

We tend to think that having the paper written and submitted is the heavy part of the process. This is only the lucky case. Scientific publishing is a long and interconnected team effort, involving the journal, the author(s), and the reviewers.

*How* to choose the journal where to try and publish that very paper, with its peculiarity? A big name, an excellent reputation, mainstream, and a high impact factor are indisputable classic criteria. They usually do but not always. Maybe you need to fly lower, having a nice manuscript which is not first-class statistics and still worth publishing. For me the average time for that journal from submission till acceptance and publication is an important selection factor. This is why in my own reviewing activity I always do my best to meet deadlines. The acceptance/rejection rate of the journal, when known, is my second important selection factor. In case you have no idea where to start, look into your own paper and consider journals you are citing at your turn. Go navigating its Web page and check the journal editorial policy and board. Have a look at the latest issue, it is often available online; otherwise go to the library. In my experience I occasionally considered to start from the top, submitting to a major journal, in the hope of trying and getting a reputable high-level review, even if along with almost sure rejection. Nevertheless it helped both in revising the manuscript and targeting the journal where resubmit. First choices, even the most carefully made, happen to be wrong: be prepared to submit more than once, to more than one journal. However contemporary multiple submission is usually a big sin, which is either not tolerated or forbidden by most journals. Except for some e-journals that clearly declare permission of multi-submission and multi-publishing, you will be honestly submitting to one journal at a time. You will not be risking that the very same manuscript will be sent to the same reviewer from two different journals. That would damage unnecessarily your reputation.

And *what* will they do with your submission? After submitting a paper, be prepared to wait. Your submission has triggered a delicate interconnected serial process. First the editor in chief will be passing the manuscript to his/her associate editor with competence on that specific subject. Then two referees will be asked to peer-review the manuscript and will be given adequate time to fit this further commitment into their agenda, normally no less than 5 weeks. The peer-review process is usually but not always double-blinded: the referees are always anonymous to the author, not necessarily the other way round. The reviewers will motivate their own recommendation in a report to the associate editor which is in charge of the final decision about your manuscript. A third reviewer might enter the picture to disentangle two conflicting recommendations. Only at this point the submitting author is notified the final decision, normally alongside a report of comments and suggestions. It thus might take a while for the entire process to conclude. Wait patiently, but not forever. After a three/four months time of silent waiting you

should write a polite email to the editor asking when you may expect a response. The dreaded-looked forward final decision will be one and only one of three:

1. Rejected—often with a polite encouragement to resubmit
2. Accepted conditionally on major/substantial revision
3. Accepted with minor revision—the lucky case

A fourth option

Accepted as it is, no revision required

is a non-impossible event with probability close to zero. It can happen.

As a matter of fact, reviews tend to come with a lot of disappointment

they did so NOT get it!

*and* much Charlie Brown thinking

nobody understands me ... nobody appreciates my work ...

going back and forth between the two with different and personal intensity. We love statistics and we are passionate authors, that is, human and very Italian. Anyway, sooner or later you gotta get over it. So first take a good breath and some days to overcome the shock. Be sure to be over the emotional though natural part of the process before going back to rework your paper. That is, be sure you are just working on the paper (not making them see or making them be sorry). Talking as a reviewer myself, after all they *did* an effort. Peer review is a totally volunteer activity. Reviewers are experts, but human beings, they *can* be wrong (and sometimes even unnecessarily nasty). However, in most cases, they just did their job looking for weak points, obscure methodologies, and unconvincing statements, offering constructive criticism even when suggesting rejection, giving advice for moving a manuscript from the *unacceptable* class to the *publishable* status. If a reviewer misunderstood a point, that point probably needs to be made clearer. In every review (even in the wrong and gratuitously rude ones) there *is* some good for your manuscript and your research. The whole point of the refereeing saga is: it *is* a service and it is there for you to use. Find out of the reviewers' job how you can make your manuscript stronger and acceptable for publication. Look for the good in the review and use it.

When considering resubmission, revision should be *always* the case, whether or not you are going to choose a different journal after a rejection. In my experience, trying to submit to another journal without any revision normally does not change the final result, ending in another rejection and very similar reviewers' comments. Of course I know that for a fact ... for having tried myself. Also, you cannot exclude that the second journal would send your manuscript to the very same reviewers as the first journal did. And this would be very disrespectful of the reviewer job, no matter he/she was right or wrong. If you are resubmitting upon a conditional acceptance, my suggestion is to accompany the revised manuscript with a clear report about the revision you made; according to another WHW rule, you should be reporting about *What*, *HW*, and *Why* you did or did not in reworking the manuscript.

This may or may not include a point-by-point answer to the reviewers. After resubmission, some more waiting. But now you know what to expect. It will be the editor's decision to ask the reviewers if they were totally satisfied and if you had been addressing all the comments. The report just mentioned will be useful in that phase.

So, it might require quite a while and a lot of work, still the saga generally has its happy end: the editor congratulating you and asking for proofreading your galley proof.

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## References

1. National Academy Press (1995) On being a scientist. National Academy Press [http://www.nap.edu/openbook.php?record\\_id=4917&page=R1](http://www.nap.edu/openbook.php?record_id=4917&page=R1)
2. Little RJ, Wilson S. WRITE Statistics RIGHT! Tips on good writing style for R&M researchers. Bureau of the Census <http://sitemaker.umich.edu/rlittle/files/writestatsrev.pdf>
3. Sand-Jensen K (2011) How to write consistently boring scientific literature. *Oikos* 116:723–727. doi: 10.1111/j.2007.0030-1299.15674.x