On academic writing

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Abstract

Purpose – The purpose of this paper is to give advice on how to achieve more productive academic writing work by commenting on 13 rules for good work. This is an increasingly important aspect of academic work for young scholars as publication is the main basis for promotion and salary increases in Europe today.

Design/methodology/approach – The approach is to draw on many years of experience as editor of management journals and, when possible, give illustrative comments.

Findings – The claim brought forward by the entire list of academic writing rules is that by living up to them the academic will improve his or her publication record, which will effect life income.

Originality/value – There is an expanding literature on academic writing, mostly in book form. This paper provides the essentials in compressed form.

Keywords Publishing, Academic staff, Research

Paper type Technical paper

Introduction

Are there any useful tricks of the trade, as it were, to sustain a productive routine of writing for publication in academic journals?[1] Nowadays the drive towards international publication is stronger than ever in Europe. The trend is irreversible and strong. At the same time, teaching requirements remain the same and administrative duties tend to be distributed widely among faculty everywhere. There is a definite need to use time (writing time) more effectively, and even if work habits in academia are quite diverse and personal, there must be some lessons to draw by comparing experiences by those who have succeeded. Personally I have served as editor for many years, and as reviewer for a dozen journals. Here, are some rules for success I have drawn from editorial experience and from interacting with those who know:

(1) Make academic writing a habit.
(2) Academic work is writing.
(3) Choose problems that interest others as well as yourself.
(4) Work on several manuscripts at a time.
(5) Love your reviewers.
(6) Work on your English.
(7) Use references to indicate what ball park you are in.
(8) Beginning – middle – end.
(9) When submitting follow the instructions.
(10) Check that your manuscript fits the profile of the journal.
(11) Revision is the core of academic writing.

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(12) The editor has power, but it is limited.

(13) Work – finish – publish.

On rule 1 – "make academic writing a habit"

Wildavsky (1989) wrote a small book called *Craftways – On the Organization of Scholarly Work*. The book was full of good advice on how to get things done. I no longer remember all the advice except two; "keep conversations with students short and businesslike" and that "the essence of scholarly work is time management". This latter statement included the factual statement that academic writing is not a matter of waiting for inspiration, but instead of maintaining a habit. Wildavsky pointed out that we should work on schedule and establish rules, which could help us say no to all those interesting projects that tempt us to jump from one project to another without finishing any of them. Wildavsky means that we should set aside time every day for writing and we should make our environment aware of this so they do not disturb, and so they keep expectations high. ("How did your writing go today?"). The good academic writers I know use this regular scheduled writing pattern. Bill Starbuck[2] locked himself in his room every morning working on manuscripts, and spent the afternoon chatting with colleagues when he was a researcher in Wissenschaftszentrum Berlin and helped Scandinavian and other researchers along in the 1970s. Wildavsky also wrote in the morning. I myself, trying to be a family man, decided early on, when the children were small, that I would not work at home. That has become a habit, which I seem unable to break now that they are no longer there. I have to get a new habit started before I retire. Habitual writing is important because it assures quality. True that you sometimes get a brilliant idea by inspiration (once or twice in a lifetime if you are lucky), but working that idea out, articulating it and aligning it with other ideas and the work of previous authors require regularity and sustained attention.

On rule 2 – "academic work is writing"

It is also important to point out that academic work is writing, more than anything else. Surely one can enjoy a good discussion in the seminar and some people love to catch and hold the attention of the audience at lectures, but the thing that remains and reaches the large academic audience is the written text. Surely you can see this if you think of your favourite quotation and the person behind it. Have you met the author? Do you have an image of her or him? My favourite quotation is: "How can I know what I think until I see what I say!" by Karl Weick. (It turns out that he has quoted somebody else (Robert Graves) who wrote it in 1927, see *Sensemaking in Organizations*, 1995). I met Karl Weick for the first time some ten years ago at a workshop. He looked like I had imagined him, but he was a rather quiet person in the workshop – dominated the agenda without saying much. He is a writing man! He has dealt with most organizational problems before we discover them ourselves. Then we can turn to his texts for reference. He addresses us through his texts and we are duly impressed as we connect with them in our own writing. And the texts remain after we are gone. Better give them our best effort. There are some memorable speeches in the world, but let's face it most speeches are forgotten once the audience leaves the room.

On rule 3 – "choose problems that interest others as well as yourself"

Karl Weick was able to set the stage at the workshop I mentioned by explaining what he was working with in such a way that we, the other participants, came back to his problem, time and again, in our own presentations. He was working with forest fires and fire fighters at the time. And the problem he presented to us, which we could not get out of our minds, was, Drop your tools! He and his co-workers had found that in recent forest fires in the USA several fire fighters died with heavy tools in their hands. They could have saved their life by dropping their tools and running to safety. But instead they kept heavy tools, like chain saws, in their hands and died. There even was a foreman who ordered his men to drop their tools and run, but came after them and picked tools up and died himself! Why do not they drop their tools? (Why do not scholars drop their tools once in a while and try a different method?). The effect of this fascinating problem presentation was that Karl Weick got a lot of ideas from us during the seminar, because we would associate to his problem when presenting our own papers. I remember saying to him during a coffee break: why are they called "fire fighters?" Are they supposed to fight the fire? Is the "fighting" connotation promoting reckless bravery, masculinity, and the skilful use of tools . . .? Is there something in the word?

Another example of the same effect of telling your colleagues about your problem was the "floating information" thing told by Susan Starr and Geoffrey Bowker[3] at the same workshop. The Clinton administration had invested in different improvement project for six university libraries in the USA. Starr and Bowker participated in the evaluation of this commendable initiative. They had found that across all disciplines and faculties it was true that the more qualified the researcher the less use of the library. Doctoral students used the library more than PhDs, etc. The explanation was that the more qualified researchers were part of networks that they kept informed about their research problems. A positive effect of this is that if your colleagues find something that seems relevant to your problem they inform you about it (have you seem what XX wrote about YY in the ZZ journal! Here is a copy!). This kind of information that floats along e-mail connections they call "floating information." By keeping colleagues informed about what you are working on you get help for free! I had a benefactor like that many years ago. David Rosenberg was a sociologist driven out of his sociology department by Thatcher policies and working with accounting people at the UMIST in Manchester at the time. A colourful person he once had a memorable row with Aaron Wildavsky on matters of principle (door slamming and harsh words) at the serene Municipalities' conference retreat at a historical site outside Stockholm. That was where we become friends. Well, he used to send me books bought in second hand bookshops with greetings like "You need this! You owe me 2£ 50!" Others will work for you if you tell them about your problem.

On rule 4 – "work on several manuscripts at a time"

If you have got this wonderful opportunity to have the enjoyable work (we really should pay for the privilege rather than draw a salary!) as academic researcher you simply must publish. Usually, we are employed on taxpayers' money and we have to return the favour by reporting results. It is our duty to make our results available to the international research community and to practice. Usually, we are not very good at presenting our results to practice, but we should try. It is not enough to present it to students and make them learn it for the exams. That is probably the best way to assure that it is forgotten anyway! The publishing process, be it books or articles in journals, is time consuming. When I talk about publishing in seminars I sometimes get the complaint that the review process takes such a long time that a researcher cannot afford to wait for the outcome of the review process. He or she must go on in order to be at the research frontier!

This is flawed reasoning in several respects! First, the research front is what is published. If you do not publish you cannot, by definition, be at the research front. Second, if you think that you will fall behind the research front if you do not do what others did before they got their current publication out, try to remember that their articles have also gone through a long review process. You are already behind anyway. Maybe they are doing something else now?

Third, it is hard to know what should be considered the frontier at any given moment. Some articles are cited intensively for a while and then forgotten, others assume classical status slowly peaking 3-4 years or later on those nice curves generated by the SSCI to describe "impact." And even if the front moved away in one area it might be more stable in another. There are leaks between disciplines and one could always try to make the choice of journals wider by finding a writing partner in another discipline.

But it is true that it is frustrating to wait for the editor's decision. The best way to beat that feeling is to find something to do while waiting. Write an article! While you wait for the proofs for your next book, work on the next-next one! It is pure nonsense for a researcher to just sit and wait for the review process to run its course! It is, however, a problem to come back to the old stuff if you get a request from the editor to revise the article and if you are preoccupied with something else at the time. This is something that has to be managed by making it a routine. The best piece of advice in setting the stage for routinization of revision work is probably that one should not spread one's work over too wide areas. Because that might make it difficult to do a good job of revising articles, which have been in review for, maybe a year or more. My experience though, is that people do not shift between topics or theories so much that it is very difficult to come back to old manuscripts. And, what is more, what you are doing now may help you to improve what you did a coupler of years ago.

On rule 5 – "love your reviewers"

When you finally get the comments from the reviewers they may appear hostile and insulting at first reading. As editor I sometimes get letters from authors who are very angry with incompetent reviewers who have not even understood the simplest propositions. My answer is if friendly reviewers, who have volunteered to spend part of their busy life on this manuscript, and who are chosen because they are experts in the area, misunderstand, then there just might be something wrong with the way things are presented in the manuscript. In 90 per cent of the cases the fault is with the author and her or his way of expressing thoughts. It is childish to take offence from the reviewers. They do an important job and if they do not get the message try another way of formulating the text. This is the chance you get to have professional feedback based on the text alone (in blind review journals), not on friendship or respect or animosity. Use it to your own advantage rather than spend energy on cursing reviewers.

While speaking of reviewers it is appropriate to point out that the quality of a scientific journal comes from the review process. The difficulty today with journals in our area is, besides getting good, innovative manuscripts, to keep good reviewers happy. They do a thankless job in the shadow of anonymity and they do not like to be insulted by getting draft manuscripts to review. If there is anything that prejudices reviewers it is to get sloppy manuscripts to read. Sometimes I get cover letters saying that I was going to revise this manuscript anyway, but I thought I would wait until

I have comments from reviewers". This is not the way to work! Another advice (now I remember) from Wildavsky (1989): "Send in the 4th or 5th version of your manuscript to the journal!" Do not insult reviewers by sending them half ready manuscripts! Reviewers are busy professionals who love challenges but who do not want to correct draft manuscripts. The best way to prepare a manuscript for the journal review process is to have one or two review processes of your own, and a conference version, before the manuscript is submitted. The more reviewers you can have in your network the better manuscripts you submit, and the more productive you are. But above all pay close attention to reviewer comments. See all comments as indicators of opportunities for improvement (not as indication of reviewer malevolence). Colleagues will be more willing to do reviews of your work if you do the same for them. Oh, by the way, the most useful advice on how to do revision work, that I know of, can be found in Booth *et al.* (1995) or later editions.

I think that a neglected part of research training is the review work, which is a significant part of the work of any qualified researcher. It should be properly included in all doctoral programs.

On rule 6 – "work on your English"

The language problem is a great problem to most of us. Just consider how often you are seduced by trivialities expressed in forceful English. English speakers are often unaware of the language problem, especially in England where the language is such an important indicator of status. The heart of the language problem is that you are likely not to be heard properly if your language is faulty. It is not only the grammar! It is the structure and the rhythm, which gets fuddled by the structures of your first language. For Scandinavians it is quite difficult to write good English. We tend to think in the structures our languages provide us with. Therefore, even a grammatically correct English text by a Scandinavian (or a German) author can be quite amusing reading for a native speaker. The cure for this is, besides visits to English speaking countries, to write English regularly and get feedback on the language, but also to read effective English, which I think is best found in the international news magazines like *Time, Newsweek*, etc. Textbooks with their pedagogic aims may not be the right medicine, even if the language is intended to be clear, since they deal with content that is all too known to article writers.

And we do have to write in English! The trend is very obvious. There is concentration going on in the scientific journal industry and the language will continue to be English. The *Scandinavian Journal of Management* is published in Oxford even if the publishing company has been taken over by a Dutch publisher!

A further piece of advice concerning English is the following: my experience is that it is better that I write the manuscript in bad English and have it corrected than if I write it in Swedish and have somebody translate it. Translators may be good in English but they are usually not as knowledgeable about the research topic as I am. This makes them uncertain, and therefore, they tend to be bound by my text and the result is often not a good readable English text. I know that there are some people who insist on writing in their own language and have texts translated, but that requires very good translators who take the time to discuss manuscripts with the author. Such translators are rare and expensive and research grants are not cut out for such extravagance.

On rule 7 – "use references to indicate what ball park you are in"

The first thing that you think about after defending your thesis is that now it is over this terrible ordeal of revising chapters and responding to unreasonable requests from tutors. Well it is not! After a few days vacation you have to start collecting items for your CV. You discover that there is a large number of new PhDs this year and they all want to have the best academic jobs. There used to be plenty of opportunities for a young PhD, and there still is, but things will never be as good as they were when your current professors were in the same situation. (Do not believe them! They did not have to struggle to get ahead as you will have to!). You have to publish to show that you are trying to improve yourself all the time.

So what have you got to start with? Which conversation (Huff, 1999) do you want to be part of? You have your thesis work and there are plenty of things that were not said in the thesis or which could have been said differently. If we assume that your thesis is a monograph (a book) it is desirable to present the results in article form to a wider audience. Please, do not try to make one article to summarize the whole thesis! It almost never works. Instead select a theme and make an article of that. Then select another theme . . . The reason for articles summarizing theses usually are rejected is that the author tries to say too much in those few pages since she or he wants to be true to the thesis. The purpose of the article is different from that of the monograph. One could say that the book tries to build a self-contained argument, with all the premises included in the first few chapters, while the purpose of the article is to contribute to the existing body of knowledge, or, as Huff (1999) would say, to participate in one particular conversation. I think the idea about participating in a conversation is a sobering one for article writers – you do not want to repeat unnecessarily what has already been said, but you want to align (dovetail as it were) your contribution with that conversation. There is no point then in demonstrating in an article that you have read classical authors like March, Simon or Hofstede (they may be your reviewers!) in a survey of the literature, in the way you may have to do in your thesis to earn the doctorate. I remember in my days as an editor getting a manuscript on international management from a young scholar who relied heavily on Geert Hofstede to make his or her case; Hofstede has said this and Hofstede has said that. I decided to send the manuscript to Geert for review and in his comments to the author he broke the blind review custom by starting out with "I might as well tell you that my name is Geert Hofstede and . . .!" In an article, you use literature references to indicate in what area (conversation) your contribution aims for (rather than build your case on one authority) and, consequently, on what basis it expects to be evaluated. If you want to criticize some earlier work, or demonstrate where earlier research went wrong, it is another matter. Because then it is very important to show that you are giving the previous writers a fair reading.

This means that all the work that you put into the chapter on earlier research will be reduced to a few lines in the beginning of the article manuscript, and furthermore, you will have to delete most of the thesis literature since it is common background knowledge for your article readers. Literature references thus are much more focused on the problem at hand in an article. Also it is usually not necessary to state your philosophy of science explicitly, since your reader will recognize that in the first few lines anyway. If your article is about methodology the situation may be different, but you are not likely to have much to add to the literature on methodology at the time you have just made it to the doctoral level.

On rule 8 – "beginning – middle – end"

The article you are about to write on the basis of your thesis work, thus needs to be much more narrow than the thesis. It keeps to one problem and it goes deeper into it. Usually, you cannot say more than one thing in a good article, so you should be careful to build it up to demonstrate your point. This is made easier if the structure is right. The most important structural aspect of an article is the idea of beginning – middle – end.

Everything you write should have a beginning, a middle, and an end. The first reaction to such advice is Of course! Tell me something not so trivial! Well, in my long life as editor I estimate that 90 per cent of the rejected manuscripts were rejected because they broke this fundamental rule. Usually, bad manuscripts consist of middle parts while lacking beginning and end. This is probably because authors are so fascinated by their findings that they forget to introduce their problem and since they have no problem there is no way to stop talking about the findings. The end will just mark where the author ran out of steam. The point of having a proper problem formulation is that it automatically offers a nice way to end the article by giving an answer to that problem! In doctoral theses, there is always a section in the concluding chapter on future research and sometimes this feature is carried over into articles making them open-ended and non-conclusive. What we want from an article is a clear statement of what the contribution is. If it is a good article readers will start to generate ideas about future research by themselves. Let them enjoy that pleasure by themselves! You will probably not do the future research you suggest anyway, will you?

Stating the problem is probably the most important part of the article writing. If you get that right, the rest will follow automatically. Go back to the formulation of the problem many times during revisions and see if you cannot make it clearer and more aligned to your findings. The simpler the better! The ideal problem formulation is "Now I will find out whether X is black or white" which gives the obvious ending "It was black!" (or white). To have a simple problem statement does not preclude complicated model work or analysis of data in the middle part of the article!

There are, of course, standard structures for articles, which may serve as reminders when you build your argument. One should remember, though, that slight variations may serve as "bait" for editors who have seen it all too many times already. When they see something slightly different (and good) they may take an interest in helping get the text ready for publication. That standard structure is:

- statement of the problem;
- earlier research relevant to that problem;
- focused statement of immediate research purpose (e.g., hypotheses);
- statement of what data are needed for that purpose, and a justified choice of method to collect and analyze such data (note justification refers to the problem, not that you happen to know how to do, e.g. DEA).
- specification of how data were collected and results obtained – presentation; and
- discussion – implications.

In the individual case the emphasis may vary, but the structure is usually the same. No doubt that the beginning, and to some extent the end, sets the stage for the middle part. No matter how well done the middle part is, a dull beginning can reduce its value to almost nothing.

On rule 9 – "when submitting follow the instructions"

Preparing your manuscript for submission is also part of the research process. All journals have instructions to authors, usually on the inside of the back cover, and those instructions are there because authors are supposed to follow them. The instructions are there because manuscripts go through a complicated production process where the link between the author and the editor is only a tiny little line up in the left hand corner of a very complex flow chart. Many people are involved and work has to be standardised because the production controllers get promoted and are replaced by newcomers, etc. It is not possible to treat every author with the respect he or she deserves, and have a unique procedure for the individual manuscript. These are mass production systems and you must comply! The large publishing houses have several hundred journals in all kinds of areas, and they are working with electronic editions, etc. They have all the arguments for standardisation (although we see an increase in variation as, e.g. the willingness to include pictures in articles increases). If the instructions say double-spaced it is because you are supposed to provide a double spaced manuscript, if it says three copies don't send two. Abstract, references, figures, questionnaires ... follow the instructions! Do not submit to more than one journal at a time! Submitting a manuscript to a journal means that you reserve the copyright for that journal while it is reviewing it. You can stop the process and take the manuscript back at any time but you cannot reserve the copyright to more than one publisher at a time. Once the first journal has rejected the manuscript you are free to submit it to another one, but then note that the instructions may be different. Between editors one may sometimes hear bragging about rejection rates as indicators of quality. It is true that the A journals in many social sciences maintain a rejection rate disturbingly close to 100 per cent. Authors should take this to mean that one should not give up after one rejection or two. Keep revising and submitting! Some day you will find the right journal for your text. Electronic submissions dominate nowadays but the same advice remains: do not make it more difficult for your manuscript to get through the review process by creating unnecessary extra work for the editors.

On rule 10 – "check that your manuscript fits the profile of the journal"

The editor will look through the manuscript and, if it looks ready for review, select the reviewers. Usually, two reviewers are selected and usually they accept, but sometimes reviewers are too popular among editors and manuscripts pile up on reviewers' desks. They may conclude that they cannot accept more work, or they may not respond at all. Then, later then intended, the manuscript may be returned to the editor and it will take some time before the review report is produced. Reviews are blind in scientific journals. That means that the author's name is taken away (see to it that you name is only on the cover page of the manuscript) and it is not good manners to refer conspicuously to your own work. If you do you should write "Cohen (1996) found that ... " Rather than "In my excellent study from 1996 (Cohen, 1996) I found that ... " It also means that the reviewers are anonymous. The only exception I remember is the Hofstede incident I mentioned earlier. It is another matter that part of the thrill in being a reviewer is to try to identify the author.

When the editor gets the reviewer reports (consisting of one page with an evaluation and a recommendation for the editor only, and one part with comments directed to the author) the editor writes a decision letter which is usually several pages long and has the structure:

- reviewer 1 recommends xx on this or that basis;
- reviewer 2 recommends yy;
- my own opinion is this or that; and
- my editorial decision is "revise and resubmit"...

"Comments to the author" from the reviewers are enclosed, and the reviewers usually get copies of the other reviewer's comments together with the copy of the decision (author's name undisclosed). In this way, reviewers can see how their judgement compares with that of the others. Also the reviewers are well prepared to evaluate the revised version of the manuscript when it arrives, because if the decision is "revise and resubmit" the same reviewers will look at the revised manuscript and that calls for two conclusions. The first is that the author should pay close attention to every point the reviewers have made (even if it is to state that you disagree), because if you do not care about what they say why should they spend part of their life (even if it is only a day) reading your manuscript (again). The other conclusion is that the feedback of review comments among regular reviewers will tend to give the journal its specific profile – at least for journals where the editor sits for a longer period. Then the journal can be said to maintain a specific conversation that you want to be part of. You want to avoid unwarranted discord with that conversation, so you pay attention to reviewer comments.

Are there any tricks to get a better deal from the reviewers, one might ask? The obvious trick is that journals have profiles if you examine their content over the last few years. It may have to do with the choice of editor. In some journals the editor is shifted every three years or so, and that might occasion shifts in direction, but usually frequent shifts of editor will make the journal more mainstream, because it takes some time to shift a profile. As to the choice of topic there are fashions, which can be detected by scrutiny of the last few issues. Therefore, it is wise to check the profile of the journal intended for submission to see if the manuscript fits. It is not much point in sending a manuscript based in one school of thought to a journal which favours another school. There are also some journal editors who look favourably at references to earlier work in their own journal (citation indices are used to classify journals into the A, B, etc. categories, and an author who does not refer to an article which is obviously relevant in the targeted journal is not improving prospects of acceptance). The editor knows what has been published in her/his own journal! Even if the editor is appointed for the short periods of three years it might be a goal to improve the standing of the journal by way of citations, so one should as a minimum not miss relevant citations to the journal one submits to.

On rule 11 – "revision is the core of academic writing"

When the author gets a decision to revise, which is the second most frequent decision, (during my more than ten years as editor I have only accepted a manuscript without revision twice!) it is time to plan how to respond to all points raised. If you choose not to change something, that must also be justified.

The first thing to note in this situation is that the decision "revise and resubmit" establishes a new bond between author and editor (and reviewers) because there is a common wish toward improvement. In spite of all the critical points raised the revise decision puts you in a very good position. Do not waste it by interpreting reviewer comments as hostile! I remember that during my first year as Editor of the *Scandinavian Journal of Management* I got a submission from a well known American researcher in a particular area that I, myself, was quite familiar with. The manuscript had not been given the finish one could expect from such a distinguished person, but I sent it out for review without delay (short backlog). The reviewers were quite frank about the deficiencies, but I could find enough positive comments to justify a revise decision, but I feared that the negative comments by the reviewers would turn the author off. To my relief I got a revised version back quite quickly and the author expressed his gratitude for the useful comments (close to insulting if you had asked me!). This illustrated very clearly how professional academic work needs to be able to turn critique (even less diplomatic forms) into something positive. The point is this bond that emerges between author and reviewers in the process. A consequence of this mutual interest in a successful outcome is that there is usually great improvement in the revised versions of the articles. Many are accepted at this stage, a few may have to revise again. I believe that the "stereo" – effect of having two reviewer comments makes the author see a clearer picture of what the manuscript is all about. The conclusion then is that reviewer comments should be taken very seriously and the author should consider both comments at the same time. By reconsidering the manuscript from both points of view the author will get a healthy distance to her/his own views and expressions and start to see the manuscript as the artefact with many authors that it really is. The author will find himself/herself giving it its own life. Sending it on its journey into the literature.

As noted above the manuscript will have gone through several revisions already before submission so the author may feel boredom taking upper hand in relation to the manuscript. It might be a good idea sometimes to put the manuscript to rest for a while, but in most cases I would guess that the author will make a better job of it by striking when the iron is hot. Again, revision work can be done well as routine work (Booth *et al.*, 1995).

On rule 12 – "the editor has power, but it is limited"

Editors are powerful persons in the life of academics, especially in those journals, which have as high rejection rates (which is usually the sign of an A-journal) as those in our area have. (It should be noted that journals in the sciences like Physics, Chemistry and Medicine have much lower rejection rates!) But there are limitations to the power of the editor. If I as editor were to go against the opinion of the reviewers too often I would soon have no reviewers left. Also every editor wants to edit a respected journal. Therefore, the editor cannot grant personal favours to authors. Especially, I found it very annoying when authors sent manuscripts to me and wanted me to, in a sense, co-author the article. "Do you think this manuscript could be turned into a publishable article and what do you think should be done with it?" I guess most editors (even for B and C journals) have quite enough of work with the ordinary submissions, which will take a large part of their time, not to appreciate this kind of invitation. The response, if any, will probably be rather rude. Even having to write a letter saying that the editor is not interested in helping the author write a proper manuscript will take time. Just imagine how much correspondence is involved in the editor's work? I remember the situation when about halfway through my stint as editor I thought it a good idea to clear some memory in my computer by putting my correspondence letters to authors on a diskette or something (this was the time before CDs) and the message came up that there was not enough room on the medium I was transferring the letter file to. It made me reflect upon the work and time put into these letters, and upon how the editor is caught up in a web of relations and dependencies. All these arguments, and other ones, point to the fact that all kinds of capacity problems prevent an editor from deviating too much from common (researcher) sense in the decisions. It is not a coincident that most respected journals are quite mainstream! This underlines another fact of life. In those great journals the editor can manage to hold the editorship for only a few (usually three) years. Out of these three years the first is easily lost as the editor struggles to get the hang of it, and adapt the rest of her/his life to the editorship calling. The second year can be productive as the flow of manuscripts starts to appear manageable, and the third year is devoted to find somebody willing to take over and organising the transition. Exercising power is more like an unintended consequence of the efforts to deal with the workload than anything else.

On rule 13 – "work – finish – publish"

Academic writing is a frustrating process, which most of us are tempted to avoid now and then, but it is very educational to take the abuse and do the revisions and admit that what you did at first can be improved. It is through the elimination of mistakes of all kinds (remember Popper!)[4] that we make progress. It is satisfying to add another title to your CV and it is nice to see that others refer to your work. It is well worth the effort in many ways. It might even be profitable! (The editors of the *Journal of Accounting & Economics* calculated that an article published in their journal was worth 30.000 USD to the author in terms of increased life income.) The crucial thing, however, is that the academic work cycle ends with publication. Your work is not done until you have reported it in a journal. That is why I would like to finish with the slogan one can find on Lars Engwall's[5] office wall. Work, Finish, Publish! It serves to remind us that we need work discipline to do what is required for successful publication. Others usually cannot see when we work and when we do something else, but they see your output in the list of publications. (Did you hear about the colleague at one of the UK universities, who, for some time, went to work in the morning, put his jacket on the support for the back of his office chair, and then went home again?). So you have to discipline yourself, and keep at it.

We work in projects. Research grants are given for projects, and we are supposed to finish these projects, and publish the results. But somehow, in real life, important results tend to be neglected since we never finish the projects properly since we are so busy getting started with the next one. We have to apply for another grant in good time before the current one is consumed. And since we are now on our way towards new exciting discoveries we postpone the publishing of current reports to a later date. In the meantime, we store interesting data in binders for later use. Oh, how much interesting, but unexploited material I have in binders on my shelves! Better start writing that article today!

Can one live by these rules and have fun?

The pleasure of seeing ones own text in a reputed journal is what keeps the academic going. There are disappointments more often than one would like, but by and large one can count on the review process of those journals as the best way we know of to sort out the best texts. Beside the fun in having success we can also count on the new stress on publication to make it worth our while to try harder. One of the accounting journals (Accounting and Economics) claimed that it was worth $30.000 in increased life income to publish an article in that journal. They had econometric evidence to prove it. Well worth the effort to test if it is true in Europe too!

Notes

1. Book writing is a different matter since it is oriented towards "the market" in a different way. In a mature area of research, results will come in the journals first and then they will be presented in (text-) books 15 years later. This is increasingly the case in management studies.
2. A leading organization theorist, William Starbuck served as editor of Administrative Science Quarterly.
3. Prominent scholars in Informatics and organization theory.
4. Karl Popper is probably the most well-known philosopher of science of the last century.
5. A Swedish colleague of Uppsala University.

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Further reading

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About the author

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