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## On academic writing

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## Abstract

Are there any useful tricks of the trade, as it were, to sustain a productive routine of writing for publication in academic journals?<sup>1</sup> Nowadays the drive towards international publication is stronger than ever in Europe (poor Americans, who have to publish in domestic journals). 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 cannot refer to any huge list of publications (only some 30 articles), but I have been in the business for some time (about 40 years), 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.

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<sup>1</sup> 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.



## 13 rules on the topic of academic writing

Aaron Wildavsky wrote a small book some 20 years ago 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 don't 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 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 researchers along in the 1970ies. I myself decided when the children were small that I would not work at home and that has become a habit, which I seem unable to break now that they are no longer there.

### *The first rule follows:*

Make academic writing a habit. Quality comes with regularity rather than with inspiration.

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 give 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 favorite quotation and the person behind it. Have you met the author? Do you have an image of her or him? My favorite 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 10 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

*Second rule:*

Academic work is writing. Whatever the enjoyment of the day it is what you write that counts! Write down your argument! Articulate in writing!

Karl Weick was able to set the stage for the workshop we attended 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 discussion. He was working with forest fires and fire fighters and the problem that he presented to us, which we could not get out of our minds, was Drop your tools! They have found that in recent forest fires in the USA several fire fighters have died with heavy tools in their hands. They could have saved their life if they had dropped the tools and run to safety, but instead they kept a 35 kilo chain saw in their hands and died. There even was a foreman who ordered his six men to drop their tools and run, but came after them and picked up the tools and died! Why don't they drop their tools? (Why don't 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 or masculinity or ....? Is there something in the word? - and I saw him make a note afterwards.

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 at the same workshop. The Clinton administration had invested in different improvement project for 6 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 doctors 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 seen 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 and he 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!”

*Third rule:*

You are more productive if you chose problems that interest also others than yourself. Present your problem to others and they will come up with good ideas!

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. 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 to approach wider by finding a writing partner in another discipline.

But it is true that it is frustrating to wait for the review process to produce results. The best way to beat that feeling is to find something to do while you wait. 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! 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. The best piece of advice here 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. 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.

*Fourth rule:*

Make it a habit to work on several manuscripts at a time. Do not stop working because you have sent a manuscript for review!

When you get the comments from reviewers you may see them as 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 % of the cases the fault lies 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 don't 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.

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 don't 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! Now I remember another advice 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 of preparing 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, don't dismiss them as misunderstandings or products of malevolent competitors, who want to stop your career and steal your ideas. 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.

### *Rule 5:*

Love your reviewers; Try to get as many of them as you can and respect their comments! Do review work yourself!

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.

The language problem is a great problem to most of us. If you consider how often you are seduced by trivialities expressed in forceful English, which you don't exactly understand but you don't want to look in the dictionary. The English speakers are often not aware of the language problem, especially in England where the language is such an important indicator of social status, 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, because we tend to think in the structures that our languages provide us with. Therefore even a grammatically correct English text by a Scandinavian 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. 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 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 extravaganza.

### *Rule 6:*

Work on your English! Find out what effective English is like!

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 isn't! After a few days of 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. (Don't 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? You have your thesis work and there is 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, don't try to make an 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 Anne 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 don't 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 readers!) in a survey of the literature, in the way you may have to do to earn the doctorate. In an article you use literature references to indicate in what area (conversation) your contribution aim for and expects an evaluation to be based in. 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.

### *Rule 7:*

Use literature references in an article to indicate what ball park you are playing in. (Don't tell us what famous researchers said in classical articles, because we know that!)

The article you are about to write on the basis of your thesis work, thus is 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 more than 10 year life as editor I estimate that 90 % of the rejected manuscripts were rejected because they broke this 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 point of having a proper problem formulation is that it automatically offers a nice way to end the article by giving an answer to the problem! In doctoral theses there is always a section in the concluding chapter on future research and sometimes this 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. That 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.
- 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 the middle part is done a dull beginning can reduce its value to almost nothing.

### *Rule 8:*

Beginning - Middle - End.

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 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 the willingness to include pictures in articles). 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. Electronic submissions dominate nowadays but the same advice remains: don't make it more difficult for your manuscript to get through the review process by creating unnecessary extra work for the editors.

### *Rule 9:*

When submitting a manuscript follow the instructions!

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. Then, later than 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 your 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 when Hofstede was reviewer of a manuscript that used his work in the wrong way and he chose to state at the beginning of his comments that "My name is Hofstede." When the editor gets the reviewer reports (consisting of one page with an evaluation and a recommendation to the editor, 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; 1) Reviewer 1 recommends xx on this and that basis, 2) reviewer 2 recommends yy on this basis; 3) my own opinion is this or that; my editorial decision is "revise and resubmit"..... Comments to the author from the reviewer are enclosed and the author is asked to revise and write an accompanying letter specifying how she or he has responded to the points made by the reviewers. All reviewers get the comments by the other reviewer and the decision letter. 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. 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.

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 influence the content, 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 to see if the manuscript fits. It isn't 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 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 3 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.

### *Rule 10:*

Check that your manuscript fits the profile of the journal!

When the author gets a decision to revise, which is the second most frequent decision, (during my 10 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. Don't 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 can assume its own line of reasoning. (But please note the stupidity in the conclusion: “The reviewers have not understood the point so I will not change the argument!”).

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 (see Booth et al, 1995).

### *Rule 11:*

Revision is the core of academic writing!

Editors are powerful persons in the life of academics, especially in those in 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 diskett 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 file to. It made me reflect upon the work and time put into these letters, and upon how the editor is caught in a web of relations and dependencies. All these arguments, and other ones, point to the fact that all kind of capacity problems prevents 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 3) years. Of these 3 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 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.

*Rule 12:*

The editor has power, but it is limited.

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!) 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 in a journal. That is why I would like to finish with the slogan one can find on Lars Engwall's office wall. It serves to remind us that we need work discipline to do what is required for successful publication. Others can not see when we work and when we do something else (Did you here about the colleague at one of the UK universities that 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. We work in projects, research grants are given for projects, and we are supposed to finish these projects and publish the results. Somehow the most important results tend to be neglected, we never finish the projects properly since we are so busy getting started with the next one. And since we are now on our way towards new exciting discoveries we postpone the publishing of current reports to a later date. Oh, how much interesting, but unexploited material I have in binders on my shelves! Better start writing that article today!

*Rule 13:*

Work, Finish, Publish.

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