

## **Some Modest Advice for Graduate Students < <http://www.eeb.yale.edu/stearns/advice.htm>>**

by Stephen C. Stearns

### **Always Prepare for the Worst.**

Some of the greatest catastrophes in graduate education could have been avoided by a little intelligent foresight. Be cynical. Assume that your proposed research might not work, and that one of your faculty advisers might become unsupportive - or even hostile. Plan for alternatives.

### **Nobody cares about you.**

In fact, some professors care about you and some don't. Most probably do, but all are busy, which means in practice they cannot care about you because they don't have the time. You are on your own, and you had better get used to it. This has a lot of implications. Here are two important ones:

1. You had better decide early on that you are in charge of your program. The degree you get is yours to create. Your major professor can advise you and protect you to a certain extent from bureaucratic and financial demons, but he should not tell you what to do. That is up to you. If you need advice, ask for it: that's his job.
2. If you want to pick somebody's brains, you'll have to go to him or her, because they won't be coming to you.

### **You Must Know Why Your Work is Important.**

When you first arrive, read and think widely and exhaustively for a year. Assume that everything you read is bullshit until the author manages to convince you that it isn't. If you do not understand something, don't feel bad - it's not your fault, it's the author's. He didn't write clearly enough.

If some authority figure tells you that you aren't accomplishing anything because you aren't taking courses and you aren't gathering data, tell him what you're up to. If he persists, tell him to bug off, because you know what you're doing, dammit.

This is a hard stage to get through because you will feel guilty about not getting going on your own research. You will continually be asking yourself, "What am I doing here?" Be patient. This stage is critical to your personal development and to maintaining the flow of new ideas into science. Here you decide what constitutes an important problem. You must arrive at this decision independently for two reasons. First, if someone hands you a problem, you won't feel that it is yours, you won't have that possessiveness that makes you want to work on it, defend it, fight for it, and make it come out beautifully. Secondly, your PhD work will shape your future. It is your choice of a field in which to carry out a life's work. It is also important to the dynamic of science that your entry be well thought out. This is one point where you can start a whole new area of research. Remember, what sense does it make to start gathering data if you don't know - and I mean really know - why you're doing it?

### **Psychological Problems are the Biggest Barrier.**

You must establish a firm psychological stance early in your graduate career to keep from being buffeted by the many demands that will be made on your time. If you don't watch out, the pressures of course work, teaching, language requirements and who knows what else will push you around like a large, docile molecule in Brownian motion. Here are a few things to watch out for:

1. The initiation-rite nature of the PhD and its power to convince you that your value as a person is being judged. No matter how hard you try, you won't be able to avoid this one. No one does. It stems from the open-ended nature of the thesis problem. You have to decide what a "good" thesis is. A thesis can always

be made better, which gets you into an infinite regress of possible improvements.

Recognize that you cannot produce a "perfect" thesis. There are going to be flaws in it, as there are in everything. Settle down to make it as good as you can within the limits of time, money, energy, encouragement and thought at your disposal.

You can alleviate this problem by jumping all the explicit hurdles early in the game. Get all of your course requirements and examinations out of the way as soon as possible. Not only do you thereby clear the decks for your thesis, but you also convince yourself, by successfully jumping each hurdle, that you probably are good enough after all.

2. Nothing elicits dominant behavior like subservient behavior. Expect and demand to be treated like a colleague. The paper requirements are the explicit hurdle you will have to jump, but the implicit hurdle is attaining the status of a colleague. Act like one and you'll be treated like one.

3. Graduate school is only one of the tools that you have at hand for shaping your own development. Be prepared to quit for awhile if something better comes up. There are three good reasons to do this.

First, a real opportunity could arise that is more productive and challenging than anything you could do in graduate school and that involves a long enough block of time to justify dropping out. Examples include fieldwork in Africa on a project not directly related to your PhD work, a contract for software development, an opportunity to work as an aide in the nation's capital in the formulation of science policy, or an internship at a major newspaper or magazine as a science journalist.

Secondly, only by keeping this option open can you function with true independence as a graduate student. If you perceive graduate school as your only option, you will be psychologically labile, inclined to get a bit desperate and insecure, and you will not be able to give your best.

Thirdly, if things really are not working out for you, then you are only hurting yourself and denying resources to others by staying in graduate school. There are a lot of interesting things to do in life besides being a scientist, and in some the job market is a lot better. If science is not turning you on, perhaps you should try something else. However, do not go off half-cocked. This is a serious decision. Be sure to talk to fellow graduate students and sympathetic faculty before making up your mind.

### **Avoid Taking Lectures - They're Usually Inefficient.**

If you already have a good background in your field, then minimize the number of additional courses you take. This recommendation may seem counterintuitive, but it has a sound basis. Right now, you need to learn how to think for yourself. This requires active engagement, not passive listening and regurgitation.

To learn to think, you need two things: large blocks of time, and as much one-on-one interaction as you can get with someone who thinks more clearly than you do.

Courses just get in the way, and if you are well motivated, then reading and discussion is much more efficient and broadening than lectures. It is often a good idea to get together with a few colleagues, organize a seminar on a subject of interest, and invite a few faculty to take part. They'll probably be delighted. After all, it will be interesting for them, they'll love your initiative - and it will give them credit for teaching a course for which they don't have to do any work. How can you lose?

These comments of course do not apply to courses that teach specific skills: e.g., electron microscopy, histological technique, scuba diving.

## Write a Proposal and Get It Criticized.

A research proposal serves many functions.

1. By summarizing your year's thinking and reading, it ensures that you have gotten something out of it.
2. It makes it possible for you to defend your independence by providing a concrete demonstration that you used your time well.
3. It literally makes it possible for others to help you. What you have in mind is too complex to be communicated verbally - too subtle, and in too many parts. It must be put down in a well-organized, clearly and concisely written document that can be circulated to a few good minds. Only with a proposal before them can they give you constructive criticism.
4. You need practice writing. We all do.
5. Having located your problem and satisfied yourself that it is important, you will have to convince your colleagues that you are not totally demented and, in fact, deserve support. One way to organize a proposal to accomplish this goal is:
  - a. A brief statement of what you propose, couched as a question or hypothesis.
  - b. Why it is important scientifically, not why it is important to you personally, and how it fits into the broader scheme of ideas in your field.
  - c. A literature review that substantiates (b).
  - d. Describe your problem as a series of subproblems that can each be attacked in a series of small steps. Devise experiments, observations or analyses that will permit you to exclude alternatives at each stage. Line them up and start knocking them down. By transforming the big problem into a series of smaller ones, you always know what to do next, you lower the energy threshold to begin work, you identify the part that will take the longest or cause the most problems, and you have available a list of things to do when something doesn't work out.
6. Write down a list of the major problems that could arise and ruin the whole project. Then write down a list of alternatives that you will do if things actually do go wrong.
7. It is not a bad idea to design two or three projects and start them in parallel to see which one has the best practical chance of succeeding. There could be two or three model systems that all seem to have equally good chances on paper of providing appropriate tests for your ideas, but in fact practical problems may exclude some of them. It is much more efficient to discover this at the start than to design and execute two or three projects in succession after the first fail for practical reasons.
8. Pick a date for the presentation of your thesis and work backwards in constructing a schedule of how you are going to use your time. You can expect a stab of terror at this point. Don't worry - it goes on like this for awhile, then it gradually gets worse.
9. Spend two to three weeks writing the proposal after you've finished your reading, then give it to as many good critics as you can find. Hope that their comments are tough, and respond as constructively as you can.
10. Get at it. You already have the introduction to your thesis written, and you have only been here 12 to 18 months.

## **Manage Your Advisors.**

Keep your advisors aware of what you are doing, but do not bother them. Be an interesting presence, not a pest. At least once a year, submit a written progress report 1-2 pages long on your own initiative. They will appreciate it and be impressed.

Anticipate and work to avoid personality problems. If you do not get along with your professors, change advisors early on. Be very careful about choosing your advisors in the first place. Most important is their interest in your interests.

## **Types of Theses.**

Never elaborate a baroque excrescence on top of existing but shaky ideas. Go right to the foundations and test the implicit but unexamined assumptions of an important body of work, or lay the foundations for a new research thrust. There are, of course, other types of theses:

1. The classical thesis involves the formulation of a deductive model that makes novel and surprising predictions which you then test objectively and confirm under conditions unfavorable to the hypothesis. Rarely done and highly prized.
2. A critique of the foundations of an important body of research. Again, rare and valuable and a sure winner if properly executed.
3. The purely theoretical thesis. This takes courage, especially in a department loaded with bedrock empiricists, but can be pulled off if you are genuinely good at math and logic.
4. Gather data that someone else can synthesize. This is the worst kind of thesis, but in a pinch it will get you through. To certain kinds of people lots of data, even if they don't test a hypothesis, will always be impressive. At least the results show that you worked hard, a fact with which you can blackmail your committee into giving you the doctorate.

There are really as many kinds of theses as there are graduate students. The four types listed serve as limiting cases of the good, the bad, and the ugly. Doctoral work is a chance for you to try your hand at a number of different research styles and to discover which suits you best: theory, field work, or lab work. Ideally, you will balance all three and become the rare person who can translate the theory for the empiricists and the real world for the theoreticians.

## **Start Publishing Early.**

Don't kid yourself. You may have gotten into this game out of your love for plants and animals, your curiosity about nature, and your drive to know the truth, but you won't be able to get a job and stay in it unless you publish. You need to publish substantial articles in internationally recognized, refereed journals. Without them, you can forget a career in science. This sounds brutal, but there are good reasons for it, and it can be a joyful challenge and fulfillment. Science is shared knowledge. Until the results are effectively communicated, they in effect do not exist. Publishing is part of the job, and until it is done, the work is not complete. You must master the skill of writing clear, concise, well-organized scientific papers. Here are some tips about getting into the publishing game.

1. Co-author a paper with someone who has more experience. Approach a professor who is working on an interesting project and offer your services in return for a junior authorship. He'll appreciate the help and will give you lots of good comments on the paper because his name will be on it.
2. Do not expect your first paper to be world-shattering. A lot of eminent people began with a minor piece of work. The amount of information reported in the average scientific paper may be less than you think.

Work up to the major journals by publishing one or two short - but competent - papers in less well-recognized journals. You will quickly discover that no matter what the reputation of the journal, all editorial boards defend the quality of their product with jealous pride - and they should!

3. If it is good enough, publish your research proposal as a critical review paper. If it is publishable, you've probably chosen the right field to work in.
4. Do not write your thesis as a monograph. Write it as a series of publishable manuscripts, and submit them early enough so that at least one or two chapters of your thesis can be presented as reprints of published articles.
5. Buy and use a copy of Strunk and White's Elements of Style. Read it before you sit down to write your first paper, then read it again at least once a year for the next three or four years. Day's book, How to Write a Scientific Paper, is also excellent.
6. Get your work reviewed before you submit it to the journal by someone who has the time to criticize your writing as well as your ideas and organization.

### **Don't Look Down on a Master's Thesis.**

The only reason not to do a master's is to fulfill the generally false conceit that you're too good for that sort of thing. The master's has a number of advantages.

1. It gives you a natural way of changing schools if you want to. You can use this to broaden your background. Moreover, your ideas on what constitutes an important problem will probably be changing rapidly at this stage of your development. Your knowledge of who is doing what, and where, will be expanding rapidly. If you decide to change universities, this is the best way to do it. You leave behind people satisfied with your performance and in a position to provide well-informed letters of recommendation. You arrive with most of your PhD requirements satisfied.
2. You get much-needed experience in research and writing in a context less threatening than doctoral research. You break yourself in gradually. In research, you learn the size of a soluble problem. People who have done master's work usually have a much easier time with the PhD.
3. You get a publication.
4. What's your hurry? If you enter the job market too quickly, you won't be well prepared. Better to go a bit more slowly, build up a substantial background, and present yourself a bit later as a person with more and broader experience.

### **Publish Regularly, But Not Too Much.**

The pressure to publish has corroded the quality of journals and the quality of intellectual life. It is far better to have published a few papers of high quality that are widely read than it is to have published a long string of minor articles that are quickly forgotten. You do have to be realistic. You will need publications to get a post-doc, and you will need more to get a faculty position and then tenure. However, to the extent that you can gather your work together in substantial packages of real quality, you will be doing both yourself and your field a favor.

Most people publish only a few papers that make any difference. Most papers are cited little or not at all. About 10% of the articles published receive 90% of the citations. A paper that is not cited is time and effort wasted. Go for quality, not for quantity. This will take courage and stubbornness, but you won't regret it. If you are publishing one or two carefully considered, substantial papers in good, refereed journals each year, you're doing very well - and you've taken time to do the job right.

Acknowledgements Thanks to Frank Pitelka for providing an opportunity, to Ray Huey for being a co-conspirator and sounding board and for providing a number of the comments presented here, to the various unknown graduate students who kept these ideas in circulation, and to Pete Morin for suggesting that I write them up for publication.

### **Some Useful References.**

Day, R.A. 1983. How to write and publish a scientific paper. 2nd ed. iSi Press, Philadelphia. 181 pp. wise and witty.

Smith, R.V. 1984. Graduate research - a guide for students in the sciences. iSi Press, Philadelphia. 182 pp. complete and practical.

Strunk, W. Jr, and E.B. White. 1979. The elements of style. 3rd Ed. Macmillan, New York. 92 pp. the paradigm of concision.

*Stephen C. Stearns*  
*Professor of Zoology*  
*Zoologisches Institut der Universität Basel*  
*Rheinsprung 9*  
*CH-4051 Basel, Switzerland*