SOME MODEST ADVICE FOR GRADUATE STUDENTS

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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 advisors might become unsupportive - or even hostile. Plan for alternatives.

Nobody Cares About You

In fact, some professor 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 hogwash until the author managed 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 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 on your own research. You will continually be asking yourself, "What and 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 Ph.D. 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 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 Barriers

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 know 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 Ph.D. and it's 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 your 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 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 field work in Africa on a project not directly related to your Ph.D. 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 be 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 counter-intuitive, 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 the 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 fails 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 or 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 interest.

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 limited cases of the good, the bad and the ugly. Doctoral work is a chance for you to try you had 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 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, referred 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 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 project 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 the 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 and Publish 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 a 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 Ph.D. 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 Ph.D.

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.

Postscript

This comment was originally entitled "Cynical aids towards getting a graduate degree, or psychological and practical tools to use in acquiring and maintaining control over your own life." It originated as a handout for the Ecolunch Seminar in the Department of Zoology, University of California, Berkeley, on a Monday in the spring of 1976. Ecolunch was, and is, a Berkeley institution, a forum where graduate students present their work in progress and receive constructive criticism. At the start of the semester, however, no one is ready to talk. This was such a time.

On Friday morning at Museum Coffee, Frank Pitelka, who was in charge of Ecolunch for that semester, asked me to make the presentation on the following Monday. "Asked" is probably a misleading representation of Frank's style that morning. Frank bullied me into it. I had just given a departmental seminar on the Ph.D. work I had done at British Columbia, and did not have much new to say about biology. Frank's style brought out the rebel in me. I agreed on the condition that I had complete freedom to say whatever I wanted to, and that the theme would be advice to graduate students. Frank agreed without apparent qualms. Then I charged upstairs to Ray Huey's office to plot the attack.

I whipped out an outline, Ray responded with a more optimistic and complementary version (see the following Commentary article), and I wrote a draft at white heat that afternoon. We felt like plotters. We were plotters. There were acts of self-definition in the air. On Monday, I recall that I made a pretty aggressive presentation in which, to emphasize how busy faculty members were, I kept looking at my watch. Near the end I glanced at my watch one last time, said I had to rush off to an appointment, left the room suddenly without taking questions, and slammed the door. They waited. I never came back, but Ray took over and presented his alternative view. Ray told me later that Bill Lidicker turned to him and said, "You mean he's not coming back?" I wasn't. Fortunately, they took it well. They were and are a group of real gentlemen.

I mention these things to explain the tone of our pieces. We would not write them that way now, having been professors ourselves for some years. We never intended to publish them, having regarded the presentations as a one-time skit, but our notes were xeroxed and passed around, and eventually they spread around the United States. In the fall of 1986 I got a letter from Pete Morin at Rutgers suggesting that we publish the notes. Its survival for ten years in the graduate student grapevine convinced me that there might actually be a demand for them. I had lost my original, and Pete kindly sent me a copy, which turned to be a nth generation version with marginal notes by a number of different graduate students. On rereading it, I find that I agree with the basic message as much as ever, but that many of the details do not apply outside the context of large American universities.

Ten years later, I have one after-thought.

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, then 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 enough time to do the job right.

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Some Useful References

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