ON BEING A SUCCESSFUL GRADUATE STUDENT

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[I do not remember where I got this!]

The competition for jobs in the environmental sciences is intense. The jobs are there, but you have to stand out from the crowd. On any given year, there may be 10-20 new positions in academic institutions for your particular subdiscipline, and you can be certain that there are plenty of graduate students and post-docs around the country who have spent the past 5-8 years working day and night to show that they have the drive, imagination, and expertise to compete for these few positions. In addition, there are assistant professors that are looking to change jobs and against whom you must also compete. For each of those jobs there will be 75-250 applications, depending upon how specific the search committee made the position description.

If you want to be in the group that is called for interviews, you must set up work habits early in your graduate career that will put you in a position to be competitive. Hard, consistent work will not guarantee that you will get an interview, but lazy, inconsistent work will just about guarantee that you will not get an interview in today's academic job market. If you want to spend your life doing research and teaching, you need to demonstrate that you are very good at it.

What follows is a set of recommendations for what I mean by a successful graduate student. These guidelines cannot make up for a lack of imagination in posing research questions and designing experiments to answer the questions. The guidelines simply indicate what you need besides a fertile imagination to be a successful graduate student with some hope of attaining a position in a major university or a major research organization. I don't mean to imply that everyone seeking a graduate degree should have their sights set on a university position. There are many other alternative, productive lives, and the simple fact is that only a small subset of graduate students will eventually ever get the opportunity to work in a major research, or research and teaching, environment. But many graduate students view a combined research and teaching job as at least one possible way of how they may like to spend their lives after they leave graduate school. These guidelines are written with that in mind, and I wouldn't change any of them if I were writing them for any of the other possible alternative lives.

Set Goals.

Set long-term goals, monthly goals, weekly goals, and daily goals. If you do not, then time will just slip away. Each month evaluate your progress toward the goals you have set. If you are falling behind in reaching those goals, ask yourself why, then do something about it. Learn self-discipline.

One of the clearest differences between successful and unsuccessful professionals in all fields is self-discipline. Set a schedule for yourself and stick to it. As a graduate student you must learn about your field of study in depth, set up a plan of research, carry out experiments, analyze the data, write manuscripts based upon the results, and participate in seminars and scientific meetings. To accomplish all this successfully, you must set up a schedule. Set a specific time that you will devote each week to reading new articles in journals. Set up specific times that you will work on experiments or analyze data. Set a specific time that you will devote each day to writing (5-6 days each week), except during the peak weeks of your research and data analysis each year. Having a specific writing schedule will become especially important after your first or second year in graduate school, by which time you will continually have proposals and manuscripts that need attention.

Never catch yourself saying, I have not had time to set up the experiments (or read that important new paper, or analyze the data, or work on the manuscript), because these other things got in the way. You must set your priorities so that it is only the "other things" that don't get done on some weeks. Anything else is simply procrastination and excuses.

The problem of writing deserves special mention. Few scientists (or anyone for that matter) find writing easy. But there is only one way to get it done, and virtually every major writer who has commented on the problem has said the same thing: set aside a block of time each day and let nothing, absolutely nothing, interfere with that time. Some days, you may produce no more than a few sentences during several hours. Other days will be better. The important thing is to avoid the temptation to get up after half an hour of producing nothing and go to the departmental office for some coffee or pick up something to read. Do not let yourself succumb to the easiest cop out of all: I just do not have it today; I will try again tomorrow. Sit there and fight it out today, then do the same tomorrow, and the day after. Eventually you will win. Plan on long work weeks, but keep them productive.

There is no substitute for long hours if you are to accumulate the knowledge and skills necessary for doing innovative research, analyzing results, and writing papers. Some weeks (e.g., peak of field season, or experiments that require almost continual monitoring) may require 70 or more hours. During most other times, you should set a weekly schedule for yourself that guarantees you will make good progress each week. <u>You will not be able to treat graduate school like a 40 hour a week job.</u> It will take much more. The important thing, however, is not to just 'put in hours'. Work hard and concentrate hard, and enjoy the work and concentration. Then set aside some time for exercising and socializing. Read broadly and critically. Understand the broader context of your research. It is not enough to know the 100 papers that are most closely related to your dissertation topic. To do successful graduate work in environmental science, you will want to have some familiarity with the wide range of subdisciplines that make up these fields of research. To gain that familiarity requires more than taking some graduate courses. The best way to do this as a graduate student is to take the major journals (e.g., Science, Environmental Chemistry and Toxicology, Geochimica) and work through at least a majority of the abstracts and introductions of every issue. If you spend two hours on each issue of the several major journals in your field, you will be well on your way to getting the broad perspective you need.

Reading regularly just a few journals, however, is not enough. You will want to regularly thumb through a large subset of the following journals (the exact subset depends upon the research questions you are trying to answer): Ecology, Limnology and Oceanography, Journal of Ecology, Oecologia, Chemical Geology, Journal of Analytical and Atomic Spectromentry, Genetics, Theoretical Population Biology. This list is not meant to be exhaustive. There are journals, some taxonomically restricted (e.g., American Journal of Botany, Entomologica Experimentalis et Applicata, Auk), that you will also need to check regularly, depending upon your research. You will also need to look through Nature and Science <u>each week</u>. Checking Current Contents (Agriculture, Geology, Biology, & Environmental Sciences) every week is an excellent way to check quickly for useful papers in still other journals. Online data search routines are getting better all the time, and you should make use of these as well. You will also want to read major new books as they appear.

Attend national meetings of one or more major scientific societies.

The papers presented at the national meetings of major scientific societies include the results that are currently in press or submitted to the major journals. By going to these meetings, you get to hear the newest results and you get a chance to talk with other researchers doing similar work. Initially, you will have nothing of your own to present. Go anyway, so that you can hear what others are doing and talk to them about their research. Design and carry out your research in a professional way that will help to minimize the chance of having your manuscripts rejected by major journals. Science is a marvelously creative process: the posing of interesting questions, the design of experiments, the analysis of data, the interpretation and arrangement of results in tables and graphs, and the presentation of these questions, methods, results, and conclusions in the text are all part of the process. Every part of the process is important. Skimp at any stage and you are setting yourself up for not getting clear answers to the questions you posed. Moreover, you are setting yourself up for a rejection when you submit your work for publication. Be prepared to have some of your manuscripts rejected. You will almost certainly have some disappointments when you begin to submit manuscripts based upon your research (unless you submit them only to obscure journals). The competition for space in the major journals is fierce. Nature and Science reject about 90% of submissions. Major journals such as Ecology and Geochimca reject at least 66-70%. Remember those percentages at every stage of your research. Every time you think about settling on a more mundane question to answer, or reducing your sample size, or skipping an experiment that would strengthen your interpretation, remember that reviewers and editors of the major journals are looking for the small minority of papers that stand out from the rest. Editors of major journals search carefully for originality in questions, novelty in approach, thoroughness in carrying through on observations and experiments, and, finally, clarity and economy in presentation of the results. Continually ask yourself if you as self-critic find this method, this experimental design, this analysis, and this interpretation justified and convincing. Check and recheck your data.

At every step of data collection, analysis, and writing, make sure your numbers are correct. You will make mistakes in recording numbers. The important thing is to find them - every last one of them.

1. Think about the numbers as they go into your notebook or onto your data sheets.
2. Check them, then recheck them, after you type them as data files into the computer.
3. Proof your data by printing out the typed data file and checking it against your notebook. Do not attempt to proof the data just by looking at the numbers on your computer screen after you have entered them. I have never found anyone who can proof data that way.
4. If you find mistakes, correct them and then print out another copy of the data file and recheck the whole data set again. It is very common to introduce new errors into a data file while correcting old ones, no matter how careful you are.
5. Repeat this process of proofing on paper, correcting the data file on the computer screen, and re-proofing on paper until you find no errors. The alternative to this entire procedure is to enter all the data twice into the computer and then use a proofing program to catch mismatches. If you use this method, correct the mismatches and then run the proofing program again to make certain that all mismatches have been corrected and that you have not introduced any new errors while making corrections.
6. The next step is to choose the subset of data that you want to analyze. Check each printout carefully. Just because you think you wrote the program to eliminate all plants weighing less

than 60g, do not simply assume you did it right. Print out the data and make certain that you are using only the subset you want to include in the analysis.

7. Now you are finally ready to run your statistical analyses. Check each statistical analysis carefully. Is this really the ANOVA model that you thought you were writing in SAS? Or, is this really the analysis you thought you were choosing when you finished pointing and clicking all the boxes on your computer screen?
8. Check the numbers that you transfer from your printout sheet to the manuscript.
9. Check them again after you have finished the final draft of the manuscript. With all the deletions and insertions you have made while typing the manuscript, anything could have happened.

Go through this nine-step sequence with every analysis you perform. Remember, you will make mistakes and you must find them. If the numbers are wrong at any stage leading to the final manuscript, you are no longer doing science and you are wasting your and everyone else's time analyzing data with wrong numbers. You are doing research to get answers to scientific questions. You must make certain that the numbers are right.

Regularly ask yourself if you are asking important research questions or trivial questions.

It is easy to get caught up in little side questions that are personally fun to explore but are simply trivial. Every few months, sit for a few hours and think very hard about the direction of your research. Ask yourself, so what? The answer to the question "What do you work on?" is not "I work on species x" or "I work on interactions between x and y".

How many times have you asked someone what they work on, only to have them name a species or some higher taxon or some particular interaction between two taxa as their reply? When you ask yourself that question, or answer it for others, you should be able to state clearly the major question in the environmental sciences that you want to confront. The particular species you are working on is an important tool to that end. You will need to understand in detail the natural history of the species you work on, if you are to construct meaningful experiments. But the species is a means to a larger end, not the end in itself. Your research should be structured in a way that allows you to use species to ask major questions. What you should be striving for in your research is an understanding of fundamental patterns and processes that transcend the particulars of any one species or any one interaction between species.

To do that you must do two things. First make sure that your questions are phrased in ways that are not limited to a particular taxon or element or field site. Second, make sure that you develop a good knowledge of at least two unrelated variables. For example, knowing one species well is the beginning, and too few graduate students take the time to know even one species very well. But you need more than one species as your touchstone for developing hypotheses about fundamental ecological and evolutionary processes. You need an understanding of the pattern of diversification within the lineage of that species, in order to get a feel for the relationship between history and adaptation. And then you need to get a feel for the same relationship in at least one other, unrelated group to act as a foil against the biases you develop about pattern and process from studying only one lineage. Too much theory has been developed either from scientists generalizing broadly from the one thing they have studied or, at the other extreme, from models that are not based on what we know about real situations. Successful hypotheses come from the careful interplay of the particular and the general, constantly testing one against the other. You must always have in your mind a range of ideas and a range of particulars to help mold your thinking. "Because it is poorly known" is not an adequate reason for choosing a dissertation project.

There is an almost infinite number of things that are poorly known. You must have a clear reason in your mind why, among the many poorly known phenomena in this universe, you have chosen a particular one for your research. Why is it a fundamental question? Work on expressing ideas and results to colleagues and students.

You will spend much of the rest of your life trying to explain concepts, hypotheses, and results to both colleagues and students. The ability to do so will not develop miraculously. You must learn from experience how to get your point across both in research seminars and in classrooms. If you want to convince colleagues that you have something important to say, you better be able to keep them awake and interested during a seminar. Think about how often you have been bored and disgusted by having to listen to a speaker who wastes an hour of your time as he or she mumbles or reads to you a disjointed talk that makes no important or interesting point. The same applies to giving lectures to students. With so many capable scientists competing for jobs, universities should be able to keep only those faculty who are both good researchers and good teachers. With the keen competition for jobs that now occurs, that is what will happen more often in the future.

So get all the experience you can get and learn from your mistakes. Watch carefully how others give seminars and lectures. Take the best from what you see in them and work out which of those techniques will work well for you. Finally, never read a talk to an audience. As Janzen (1980, Bull. Brit. Ecol. Soc.) once wrote, "If you, the person who knows more about it than anyone else, cannot remember something for 30 minutes, how do you expect me to remember it more than 30 minutes after the end of your talk?" When teaching a class you will need some notes. But when giving a research seminar, you will have your slides to prompt you. In addition to your slides, use either no notes or at most a one page outline.

Remember that science is a social enterprise.

You cannot make much progress as a scientist unless you are willing to seek the help of others and, in return, give help whenever you can. The major questions in science demand expertise in ideas and technical skills greater than any one person can garner in a lifetime. You have to be willing to work with others if you want to get answers to anything more than the most mundane scientific questions. You cannot work in isolation. Take a look sometime at the collected series of Darwin's letters (published by Cambridge University Press and expected to reach twenty of so volumes at completion). You will find that Darwin was constantly writing hundreds of letters to colleagues requesting help and information, and offering it when asked.

You are part of a laboratory.

Your first responsibility as a graduate student is to get to know very well the research being conducted by others in your laboratory. You should begin by reading all the recent papers of your advisor and a good representation of the major older papers. You should then make certain you know what everyone else in your laboratory is doing and why they are doing it. After all, you have chosen to work with your advisor and the others in that laboratory because the research is closest to your own interests.

Do not waste your time writing short notes for obscure journals.

Concentrate on finishing your major experiments, observations, or models and write them up as major papers for major journals. There will be plenty of time later, if you want to collect together a few small notes that will be of interest to only a few other specialists. Search committees are not fooled by a CV that includes half a dozen short notes in obscure journals but no major papers. It is crucial for you to publish papers; unpublished research is just the same as research not done. But what you want to publish are major papers that represent a solid body of work. Once you have given your advisor a draft of a manuscript, assume that it will take at least several more months before you will be able to submit it for publication (or to include it in your dissertation).

Do not give your advisor a first draft of a manuscript that is missing figures, tables, and sections of text. Be professional about it. Hand in a complete manuscript that is actually the third or fourth draft you have written and represents the best you think you can do with the paper. That doesn't mean that you shouldn't ask questions of your advisor while you are writing, or go over some trial paragraphs of the Introduction. You should. Moreover, you two should have gone over the major figures and tables and their interpretation before you started writing. But after that, take the advice and your own deliberations and put it together into a full preliminary manuscript so that you can both see the full flow of argument. Remember, you are making an impression on others every time you ask them to look at a piece of your work. The impression you make is up to you. The draft of the manuscript that is finally submitted may have little resemblance to the one you first hand your advisor, but it is much easier for the two of you to move from one specific draft to another specific draft than it is to go from a nebulous, incomplete draft to a complete draft.

Do not give the other members of your dissertation committee a draft until you and your advisor have agreed that the manuscript is now in sufficiently good shape for the other committee members to read.

Do not assume that you can hand in a draft and get a response two days later. The faculty on your committee have dozens of commitments. It may take at least a couple of weeks to get a response to each draft you hand in. If you ask for a hurried response, you will get back either no comments or a few superficial comments. Moreover, you will have left an impression that you wait until the last minute to get things done and do not really care about getting their thoughtful comments.

By the time you and your advisor have been through several drafts, and your committee has reviewed a draft, it may have been several months from the time you first handed your advisor the manuscript. Plan accordingly. If you plan to defend your dissertation in April or May, your advisor will need to have seen initial drafts of all parts of your dissertation by January (yes, January) and most parts of it earlier than that. That will allow sufficient time for the two of you to go over several drafts before handing the manuscripts to your committee. Yes, I know that it doesn't always work out that way. But that doesn't matter. What I am suggesting here is the process that will help you to hone your dissertation so that it stands out from the crowd.

Under no circumstances should you simply hand your committee members all the chapters of your dissertation for the first time a month before your final defense. They may ask you for additional statistical analyses or they may suggest major changes in interpretation. You must allow time to make the changes or to sort out differences in interpretation. Begin exploring possibilities for postdoctoral positions at least 1 1/2 years before you finish your dissertation.

Most positions in major universities now state in their advertisements that postdoctoral experience is preferred. Even if the job announcements do not state such a preference, someone with postdoctoral experience will have a competitive edge. The problem is that postdoctoral money is hard to come by. If you are lucky, someone may have a position available on a new grant and have no one specifically yet in mind for the position. But researchers often either have someone particular in mind when they submit proposals that include a postdoctoral position, or they have had at least a short list of potential postdocs in mind based upon conversations they have had and letters they have received over the past year or so. You will want to make sure you are on that list.

Some other postdoctoral fellowships are available through NSF, NIH, and NATO, but you will have to convince someone to be your sponsor, and you will have to write the proposal. The proposal will take time to develop, and you must allow enough time to go through several drafts with your sponsor before the proposal is submitted. Do not expect to contact someone suddenly in October and get much cooperation in submitting a proposal for a December 1 deadline. The kind of person with whom you will want to work as a post-doc is already busy, and you will have to allow sufficient time to get responses.

The basic unit of correspondence is three.

When you write to others for advise or ask one or more colleagues to read a manuscript, always thank them after they have responded. The basic unit is three: you write (or ask), they respond, and you write or call back. This is not just part of being a professional. It is part of being a decent person. You may agree or disagree with their comments, or their advice may not have solved your problem, but you have a responsibility to let them know and to thank them for their comments. Imagine how you would feel if someone wrote to you asking them to spend several hours reviewing a manuscript. You devote precious time to this favor, send back your comments, and wonder what the person thinks about what you have written. But, instead, you hear back nothing. You feel used. Would you ever agree to spend your precious time helping out that person again? Remember that the purpose of doing research is to get answers to interesting and important questions about how the world works.

In the process of worrying about all the things I have recommended, remember why you are doing them. If the answer is simply to get a degree that will get you a job that looks attractive, then you will not be able to maintain the schedule that is necessary both now and once you obtain a position. If you do not enjoy the process, you are setting yourself up for a most unsatisfying life. Just putting in time and trying to follow these guidelines as a formula is not enough. You can maintain this time-demanding schedule only if you absolutely enjoy the full process of posing scientific questions, designing experiments, analyzing results, getting some answers, writing up the results for other scientists, and discussing both your results and theirs. You must want in your bones to know the answers.

Appendix: Some Useful Books and Articles

The following general books and articles may be useful to you as you begin graduate work. Some of these readings have nothing to do directly with the specifics of conducting research in ecology and evolutionary biology. They are writings on the process of science and approaches to science. You can't work effectively as a scientist unless you have some clear view about how you think the process works and what it tells us about how the world is put together. A couple of the readings are a bit more philosophical (and pedantic) than others, but they all say some useful things about research. You will not agree with everything that is said in all of them, but together they will help you to think about various approaches to asking scientific questions. And they will help you consider which kinds of questions are worth worrying about. Finally, the book by Strunk and White and the article by Mack will be invaluable to you as an aid to writing with greater precision, clarity, and economy.

Bronowski, J. 1978. The common sense of science. Harvard University Press, Cambridge.

Darwin, C. 1859. On the origin of species. (Read the first edition with an eye to the clarity of Darwin's thought and the forthright and systematic way in which he builds his argument.)

Ghiselin, M. T. 1969. The triumph of the Darwinian method. University of California Press, Berkeley.

Kingland, S. 1985. Modeling nature: episodes in the history of population ecology. University of Chicago Press, Chicago. (This book is primarily on the rise of the use of mathematical models in ecology, but it also gives an excellent overview of the developing relationship between evolution and ecology throughout the twentieth century and different approaches to science taken by ecologists.)

Kuhn, T. S. 1962 (1970). The structure of scientific revolutions. Second edition. University of Chicago Press, Chicago.

Mack, R. N. 1986. Writing with precision, clarity, and economy. Bulletin of the Ecological Society of America 67:31-35. (This very useful paper summarizes an editor's thoughts on the most common problems he has found in manuscripts submitted to Ecology.)

Medawar, P. B. 1979. Advice to a young scientist. Harper and Row, New York.

Platt, J. R. 1964. Strong inference. Science 146: 347-353.

Strunk, W. J., and E. B. White. 1979. The elements of style. Third edition. Macmillan, New York. (Without question, the most important book on writing clear English. This book will be of immense help as you try to present your ideas and results to others.)

Watson, J. D. 1993. Succeeding in science: some rules of thumb. Science 261:1812-1813.