Fribourg Graduate School of Life Sciences (FGLS)

The Graduate Student Reading Package

with useful advice for PhD students, e.g. on how to pick an advisor, how to identify a promising research topic, how to write papers, how to give seminars, how to land a postdoc, etc.

September 2019

compiled by Thomas Flatt & Simon Sprecher

Four golden lessons

Scientist

Advice to students at the start of their scientific careers.

Steven Weinberg

When I received my undergraduate degree — about a hundred years ago — the physics literature seemed to me a vast, unexplored ocean, every part of which I had to chart before beginning any research of my own. How could I do anything without knowing everything that had already been done? Fortunately, in my first year of graduate school, I had the good luck to fall into the hands of senior physicists who insisted, over my anxious objections, that I must start doing research, and pick up what I needed to know as I went along. It was sink or swim. To my surprise, I found that this works. I managed to get a quick PhD — though when I got it I knew almost nothing about physics. But I did learn one big thing: that no one knows everything, and you don't have to.

Another lesson to be learned, to continue using my oceanographic metaphor, is that while you are swimming and not sinking you should aim for rough water. When I was teaching at the Massachusetts Institute of Technology in the late 1960s, a student told me that he wanted to go into general relativity rather than the area I was working on, elementary particle physics, because the principles of the former were well known, while the latter seemed like a mess to him. It struck me that he had just given a perfectly good reason for doing the opposite. Particle physics was an area where creative work could still be done. It really was a mess in the 1960s, but since that time the work of many theoretical and experimental physicists has been able to sort it out, and put everything (well, almost everything) together in a beautiful theory known as the standard model. My advice is to go for the messes — that's where the action is.

My third piece of advice is probably the hardest to take. It is to forgive yourself for wasting time. Students are only asked to solve problems that their professors (unless unusually cruel) know to be solvable. In addition, it doesn't matter if the problems are scientifically important — they have to be solved to pass the course. But in the real world, it's very hard to know which problems are important, and you never know whether at a given moment in history a problem is solvable. At the beginning of the twentieth century, several leading physicists, including Lorentz and Abraham, were trying to work out a theory of the electron. This was partly in order to understand why all attempts to detect effects of Earth's motion through the ether had failed. We now know that they were working on the wrong problem. At that time, no one could have developed a successful theory of the electron, because quantum mechanics had not yet been discovered. It took the genius of Albert Einstein in 1905 to realize that the right problem on which to work was the effect of motion on measurements of space and time. This led him to the special theory of relativity. As you will never be sure which are the right problems to work on, most of the time that you spend in the laboratory or at your desk will be wasted. If you want to be creative, then you will have to get used to spending most of your time not being creative, to being becalmed on the ocean of scientific knowledge.

Finally, learn something about the history of science, or at a minimum the history of your own branch of science. The least important reason for this is that the history may actually be of some use to you in your own scientific work. For instance, now and then scientists are hampered by believing one of the oversimplified models of science that have been proposed by philosophers from Francis Bacon to Thomas Kuhn and Karl Popper. The best antidote to the philosophy of science is a knowledge of the history of science.

More importantly, the history of science can make your work seem more worthwhile to you. As a scientist, you're probably not going to get rich. Your friends and relatives probably won't understand what you're doing. And if you work in a field like elementary particle physics, you won't even have the satisfaction of doing something that is immediately useful. But you can get great satisfaction by recognizing that your work in science is a part of history.

Look back 100 years, to 1903. How important is it now who was Prime Minister of Great Britain in 1903, or President of the United States? What stands out as really important is that at McGill University, Ernest Rutherford and Frederick Soddy were working out the nature of radioactivity. This work (of course!) had practical applications, but much more important were its cultural implications. The understanding of radioactivity allowed physicists to explain how the Sun and Earth's cores could still be hot after millions of years. In this way, it removed the last scientific objection to what many geologists and paleontologists thought was the great age of the Earth and the Sun. After this, Christians and Jews either had to give up belief in the literal truth of the Bible or resign themselves to intellectual irrelevance. This was just one step in a sequence of steps from Galileo through Newton and Darwin to the present that, time after time, has weakened the hold of religious dogmatism. Reading any newspaper nowadays is enough to show you that this work is not yet complete. But it is civilizing work, of which scientists are able to feel proud. ■

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Dive right in: exploring the unclear, uncharted areas of science can lead to creative work.

Cell PRESS

How to Pick a Graduate Advisor

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http://dx.doi.org/10.1016/j.neuron.2013.10.005

In this NeuroView, I provide a guide for young scientists on how to select a graduate advisor or postdoctoral advisor. Good mentorship is not only pivotal for career success, but it is pivotal for driving innovation and for the health of our universities. Universities need to do much more to teach faculty how to mentor and to ensure mentoring quality. I propose an M-index to measure mentoring quality. I also call here for better studies of what great mentorship entails, better reward for great mentors, and more consideration of mentoring quality when awarding prizes and grants.

Introduction

When I was a student, I often imagined what fun it would be to someday have my own lab. There I would be able to follow my curiosity, studying whatever questions happened to interest me. By great good fortune, this dream was fulfilled and I have been able to study the mysterious roles of glial cells in health and disease in my own lab at Stanford for the past 20 years. I cannot tell you how rewarding this quest has been and how incredibly lucky I feel to have had this opportunity. I never imagined as a student, however, that it would be just as much fun and just as rewarding to mentor students as to do experiments myself. It has been a tremendous privilege to mentor so many talented graduate students and postdoctoral fellows. But it seems to me that we don't talk a lot about what being a great mentor entails. That's what I'd like to talk about here. What is a good mentor and how can you find one?

As a student, I loved to read books with advice to young scientists (Ramón y Cajal, 1897; Medawar, 1979). These wonderful books focused on how to do excellent science but did not talk much, if at all, about the importance of selecting an excellent mentor. The importance of mentorship has sometimes been written about (Kanige, 1993; Lee et al., 2007), though this did not occur to me when I was young. Now that I am older, I often reflect on my good fortune to have been one of the half of the entering students in my PhD class at Harvard who was successful in science. I now realize that all of us selected our graduate mentors amateurishly, almost randomly, and certainly not wisely. Through sheer dumb luck, I happened to pick a wonderful mentor. It is in that spirit that I write this guide about how to pick a graduate advisor. It is the guide that I wish someone had handed to me the day I entered graduate school. I write this with some trepidation, as I am certainly not a Nobel Laureate as were Medawar and Ramón y Cajal. But, as I always tell my students, the real Prize is enjoying doing science. This is a Prize that I have won. I want my students—and every aspiring young scientist—to win it too.

So why do some talented students succeed as scientists whereas others do not? This is a question that has long intrigued me. I see it around me every day. Students who have always loved science from a young age enter graduate school, but some of these students leave not enabled to be a successful scientist and/or demoralized, having somehow lost their passion for science. I will argue here that for most students, selecting a good research mentor is the key. To be sure, many students realize in graduate school that another career choice appeals more to them and happily divert to a new goal. But here I address my comments to the large group of graduate students whose goal is to be a successful researcher, whether in academia or in industry or another setting.

First, let me mention what a student should never ever do. An advisor should not be selected solely because he or she is the one researcher at your university that happens to work on the precise focused topic that you think you are most interested in (usually whatever you worked on in an undergraduate lab). In my experience, this is exactly what nearly every graduate student does! Keep in mind that if you like solving puzzles, as all scientists do, there will be many different puzzles that you will find equally rewarding to work on. Although I study the brain, I am certain that I would be just as happy working on the kidney (some would argue that glia are the kidneys of the brain). Begin your search for an advisor by casting as broad of a net as possible. Neuroscience these days spans many areas from molecular, cellular, and developmental neurobiology, to physiology and biophysics, to systems, behavioral, and computational neurobiology. Try lab rotations in different areas, which is increasingly important in an interdisciplinary world. So as your first step in finding a good mentor, create a list of possible advisors in your general field of interest, broadly defined rather than focused on a highly specific research topic.

If not based on exact research topic, then how else can one select a good mentor? There are only two criteria of any importance: scientific ability and mentorship ability. If your advisor does not know how to be a good scientist or does not know how to train you to be a good scientist, you are unlikely to become a good scientist. Perhaps I would add passion for science to that list as well. I was lucky enough to be an undergraduate at MIT (back in the good old days when they selected 50% of applicants). It has been 37 years since I graduated, and I have long forgotten all of thermodynamics, physics, calculus, and almost

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everything else they taught me. What remains are memories of the incredible passion for science that nearly all of my professors exuded, including that of Professor Hans Lukas-Teuber, whose powerful course diverted me from my interests in chemistry and computer science to neurobiology and medicine.

Pick an Advisor Who Is a Good Scientist

First, how can you identify advisors who are good scientists? Okay, here is where I am going to start to get into some touchy opinions, and no doubt this is why practical advice articles are rare to come by. But let me proceed with honesty into a field of land mines. First and very importantly, never assume just because a faculty member has a job at a good university that he or she is therefore a good scientist. For one thing, many faculty members that appeal most to young graduate students are assistant professors. That is, they do not have tenure yet and only some of them will make it to tenure. As I will discuss later, however, young faculty are often superb choices for graduate mentors. Second, many faculty are not tenure track. This does not mean that they are not good scientists, but it does add to the risk. Third, some faculty who are not good scientists make it to tenure anyway. Tenure is by no means a perfect process, and there are good scientists who are not tenured and vice versa. Fortunately, every single university has many great scientists who are also great mentors. Your job is to pick one of them.

So how can you, a mere first year graduate student, possibly decide which advisors are good scientists? After all, the whole point of earning a PhD is to learn the difference between good and bad science and you haven't learned how to do that yet! Fortunately, there are some simple things that a first year graduate student can and should do. The hallmark of a good scientist is generally that he or she asks important questions and makes mechanistic or conceptual steps forward in answering them. Because most students are not yet prepared at the start of their PhD study to evaluate the quality of a scientist's research, a simple thing that a student can do is a PubMed search and make sure that their potential advisor is publishing research papers in good to top journals. Even though you are just beginning your training, you should read some of these papers to see if they are well written, rigorous, and interesting to you. Care should be taken to distinguish research papers from reviews, which although important are not signs by themselves of research accomplishment. Although quality of the research papers is paramount, number is also important, keeping in mind that large labs should obviously be publishing more papers per year than a small lab, so some normalization for that factor is important. If your prospective advisor has not published a good research paper in over 5 years, this is a serious warning sign (what is the chance you will just happen to be the one student in that lab to publish?).

Another measure of the overall productivity and impact of a scientist's work as a whole is known as the H-index, which is a single number that rates a scientist's most cited papers and the number of citations that they have received (http://en.wikipedia.org/wiki/H-index). Any scientist's H-index can be found at the Web of Science (http://thomsonreuters.com/web-of-science). Keep in mind that older scientists will have higher H-indexes than younger scientists. Second, a student can learn much about a potential advisor's research productivity and accomplishments by simply reading the advisor's curriculum vitae. You should not be shy to ask for a prospective advisor's CV. This does not reflect poorly on you but rather shows unusual maturity and that you are being careful about how you select your thesis advisor. In some cases, the candidate advisor may be a Nobel Laureate, National Academy member, HHMI investigator, or have won some other distinguished scientific award or prize, such as an NIH Pioneer Award, which is generally an excellent sign that they are a good scientist. Most good scientists, however, lack these awards and this should not be considered a negative factor. Indeed, working with a young faculty member who is skilled in the latest techniques, still has a small lab, and therefore much time to mentor you, can often be an excellent choice.

Another objective measure of the quality of science a lab is doing is whether they have established National Institutes of Health (NIH) (or other) grant support. If this information is not listed on his or her CV, it can easily be checked by going to the NIH grant database (http://www.report.nih.gov). Unless your prospective advisor is in his first several years of starting his or her own lab, lack of NIH support in the form of one or more R01 grants would be a sign that he or she has not been sufficiently productive to merit further support. That said, without doubt obtaining grant funding is highly competitive these days, and this means that many good scientists may sometimes fail to obtain or renew a highly deserving grant application. Nonetheless, it is important for your training that you select an advisor who has sufficient funds to support your graduate research.

When in doubt, a very important source of helpful information is to ask senior faculty, such as your graduate program advisor or your undergraduate thesis advisor, for their candid thoughts about particular faculty members of interest. A student would do well to listen carefully to the responses, as a senior faculty member is unlikely to torch another faculty member (after all, they have to work with them for the rest of their careers) but might make gentle comments meant to steer you away from one candidate in favor of others.

Doing all this research to select a good advisor may seem over the top, but as selecting a good advisor is one of the most important factors in determining whether you will be successful in your career, I think it goes without saying that you should carefully research what lab you will train in at least as thoroughly as you research what cell phone or car to buy (or in my case what espresso machine).

Pick an Advisor Who Is Also a Good Mentor

Selecting an advisor based on scientific abilities alone is not sufficient. Having narrowed your list of potential advisors to those that are good scientists, next you must determine which are also good mentors. One of the most important tasks of an advisor is to help his or her student to formulate a good and tractable question and then to gently guide a student to formulate good experiments to address this question while encouraging the student to be increasingly independent over time. A good mentor does not put his student on a scientifically trivial question. If a student does not address an important question and take it a step forward during their thesis or fellowship years, they will not have the confidence that they can do this in their own lab, and likely they never will.

Good mentors spend enormous amounts of time with each of their students discussing science, how to design good experiments and interpret and analyze data, how to write research papers and grants, how to review papers for journals, practicing talks, and providing career guidance. They also allow and encourage their trainees to take time away from their research to do other activities that will enhance their training such as TAing graduate courses, attending conferences, and taking special summer courses. Sometimes trainees will need some time away from lab for parental leave. A good mentor will be supportive of this for male as well as female trainees; a few months away are irrelevant in the lifetime of a typical multiyear project.

So how can a student tell whether a prospective advisor is a good mentor? First, talk with some of his or her current and previous trainees. Ask them whether this faculty member is a good mentor in terms of spending sufficient time with each student. Ask these trainees whether they enjoyed being in that lab, and especially whether there is a team spirit in the lab, with everyone helping each other rather than being pitted against each other. Are lab meetings group discussions in which everyone contributes their thoughts and ideas, or is it primarily a time where the faculty member dictates to presenters what they should do next? (Helpful suggestions are one thing; micromanagement is another.) Second, determine what percentage of trainees in the lab are postdocs versus graduate and undergraduate students. A lab that is nearly all postdoctoral fellows may suggest that the lab head does not enjoy, or wishes to minimize, time spent mentoring. Good mentoring takes much time and devotion. Therefore, graduate students should be very cautious about selecting unusually large labs. Your lab rotation will give you an additional chance to assess all these questions.

Lastly, and most importantly, it is critical that you determine the faculty member's track record of mentoring success. One way to begin to address this question is to obtain a copy of his or her "trainees list" (this will of course not be helpful in vetting junior faculty who do not yet have a long track record of training). This trainees list, which is required to be submitted for each faculty participating in an NIH training grant, is a simple list of all of the graduate students and postdoctoral fellows a faculty member has ever had and what job they are doing today. Asking potential advisors for their trainees list might be a tad awkward, so graduate program offices should keep up-to-date copies of these lists on file for their students, and I believe that the information contained in these trainees lists is so important that the NIH should post this information electronically in a publically accessible database. It is not uncommon when looking at trainees lists for all of the faculty in the same department or program to find widely varying "success" rates, with some mentors having 70% of their students attain academic positions and others sometimes only 10% or even fewer. Not every student ends up having their own lab, whether because of choice or ability, and so even the very best advisors rarely have more than 50% of their graduates going on to have their own labs. But if only a very small percentage of trainees go on to have their own labs (whether in academia, industry, or government), this is a warning sign that little successful mentoring is happening. Some scientists are simply better mentors than others (just as some models of cars and espresso machines are better than others). Some don't enjoy mentoring, some don't want to be bothered, and some plain don't know how. The output of a truly great lab is not measured only in Nobel prizes and research articles but just as importantly in how many successful scientists it trains. I certainly do not mean to discount in any way the value and importance of training young scientists to go into other excellent science careers including teaching, science writing, scientific journals, consulting, etc. In any case, quality mentoring will of course greatly enable your performance in all of these alternative careers as well.

I have previously written about the challenges that talented women still all too often face in their careers (Barres, 2006). Sometimes, female graduate students preferentially seek out female graduate advisors in order to obtain a role model for how to balance career and family. While this is understandable, increasingly male faculty also serve as important role models for work-life balance. I would strongly suggest to women students that as they evaluate potential graduate advisors, male or female, they examine to what extent prospective mentors have a good track record of having trained successful women scientists.

As you gauge the mentoring environment of a prospective lab, make sure to ask whether the students are generally happy. If not, this is a warning sign. I strongly believe that when a talented student is in the right lab, with a good mentor, that going to lab every day should feel almost like being in summer camp. Someone once told me with great sincerity that he felt that you had not done a real PhD until you hated your advisor and he or she hated you. This is a tragic way of thinking! I have heard of many cases in which a student has been told that they are not working long enough hours in a lab and that the advisor expects the student to work 60+ hours per week. In 20 years, I have never said or implied such a thing to any student. I feel that the advisor's job is to provide a fun and exciting environment, to set a good example, and the rest must come from the heart of a student. Henry Ford once said, "Hire good people, and then get the hell out of their way." What great advice! If all is well, doing science will feel like play, and students will freely choose to work long hours because it is fun and exciting (that does not mean there will be frustrating times when your experiments are not working, of course). Moreover, if trained well, there should be no problem being successful in science while leading a happy and balanced life (okay, I am not a great example of this—but most of my previous students have accomplished a balanced life in their own labs despite my poor example. And I am living the life I love, just as I hope for my students.)

Here are some signs that a prospective advisor is thinking more about his own career and less about your career: he (or she) never mentions his students' names when he presents their work in a talk or only mentions them in a long list in small print at the end of the talk, he does not practice the students' talks with them, he puts two students in the lab on the same project so that they must compete with each other, he tells you what experiments you must do, he insists on writing the research papers rather than allowing the student to write it and then editing it with the student, he allows the students' papers to sit on his desk (sometimes for years, sometimes never even submitting them), and he refuses to allow students to take their projects or reagents with them (or fails to make sure they have lots of good starting points for projects in their own labs). Although most faculty do not behave this way, I have seen these things happen to many students over the years. Most students who fall victim to these kinds of harmful, selfish practices do not survive in science as a result. This is among the reasons why I believe it is vital that measures be taken to better identify great mentors and to reward scientists as much for mentoring ability as for scientific accomplishments.

If the day arrives when you are in graduate school when you wake up and do not wish to jump out of bed and head off to lab, it is time to consider whether it is time to switch to another lab. I have encountered many students who realized midway during their PhD that they were not happy in their lab, only to decide to stick it out rather than discuss the situation with their advisors and try to resolve the problem. My advice is to have a heart-to-heart chat with your advisor, giving him or her a chance to help you resolve the issue. If your advisor is not sympathetic, then it is time for you to switch to another lab. If you cannot find a lab that you are happy in, then it is possible that science is not the right career for you. But all too often, the problem is simply poor mentoring or a mismatched lab for whatever reason. I have seen all too many students feel that they must please their advisors and complete their projects. But always remember that your PhD training is about YOU and your success. Most productivity occurs in the last 1 or 2 years of a PhD thesis and usually switching to a new lab, even after a few years in the wrong lab, does not delay a student's graduation. Just think of your time in the first lab as a long rotation that beneficially added to your training.

Once you have selected a great lab, it is time to get to work. How to be successful in that lab is the subject of another essay. But I would advise you to remember a few things. First, do pick an important question but don't pick the same topic that everyone else is working on. It will be more fun and less competitive to go your own way. For every trendy topic now, there are 100 other topics just as important and hardly studied yet. Second, there is no need to write more than one paper; just make it a good one. It probably will take you about 6 years (counting course work). If you can work on an important question as a PhD student (or postdoc) and take it a step forward, you will have the confidence and enthusiasm to do this for the rest of your life. And students, please, do not skip your postdoctoral fellowship no matter how successful your PhD thesis work has been. It seems to be all the rage these days to shorten training time. NIH is even providing special fellowships for those who want to move directly to independent positions after their PhD training. But I have noticed that people who skip their postdoc may do okay in their own labs, but they generally fail to broaden as scientists or to achieve the versatility and fearlessness to enter new fields that they might otherwise have achieved. That is a large price to pay for skipping what could otherwise be a marvelously fun and rewarding final period of training.

Some Challenges of Mentorship and the Path Forward

Anyone who has had a lab knows that by having great trainees with diverse backgrounds and perspectives immersed in an environment of genuine respect for their thoughts, creative new ideas are constantly bubbling forth in lab discussions—ideas that the lab head would never have had by himself or herself. I have heard scientists talk about the pleasure of scientific discovery—that moment when you know something amazing that no one else in the world knows. But there is no moment more mind blowing to me than when one of my students makes the leap to thinking like a real scientist.

Mentorship is a tremendous responsibility. Great mentorship does not end when a student leaves the lab. For instance, a good mentor must make sure the student selects a good next lab or job (and not compete with him on the same set of experiments), allow him to take his project, reagents, and mice with him, write strong letters of recommendation for fellowship applications and jobs, suggest his previous students as speakers for meetings and authoring review articles, and he should actively credit his student fairly for his accomplishments when giving seminars and bring his student's name to the attention of appropriate job searches. A great mentor is very generous and gives till it hurts.

I am concerned that as competition for funding increases in science, some good mentoring practices will increasingly be put into jeopardy. In the rush to make sure that they are successful in renewing their grant funding, lab heads may commit the cardinal sin of becoming micromanagers, dictating to their students exactly what experiments to do. Young scientists who are not allowed to be independent as students and fellows are generally not able to successfully achieve this in their own labs. Often these days, talented young scientists observe the stress that their highly accomplished PhD advisors experience after a failed grant application and become concerned, quite reasonably, that they will not be able to successfully compete for grants when they have their own labs. It is fortunate that NIH has put measures into place to make sure that a fair percentage of young scientists get funded.

It's a tremendous art to keep a lab highly productive while at the same time optimally nurturing one's trainees. How can we better recognize who the great mentors actually are? The H-index is an established tool for quickly evaluating a scientist's impact. To be sure, it is not perfect, but it is simple and widely felt to be pretty good. I propose that we consider developing an M-index to provide a similar measure of mentoring ability. The M-index would simply consist of an average of the H-indexes of a given scientist's mentees, that is of their average scientific productivity and impact. Because both H- and M-indexes become more meaningful later in a career, they would not be helpful in evaluating young scientists. The M-index could be calculated from data already on PubMed by including only first authors of the mentor's papers in the analysis and assuming that these first authors are the graduate students and postdocs. Because excellent mentors often beget scientists who themselves are excellent mentors, when evaluating a young scientist, it would make sense to take a look at the M-indexes of his or her mentors.

But identifying great mentors is only a first step. Whenever I meet a great mentor, I always ask them what they do that has the highest training impact. I rarely get the same answer, yet everyone thinks they know what matters. I have made some guesses in this essay, but data are lacking. We need to investigate what practices great mentors have that have the most impact in training successful young scientists. Recently, it has been increasingly realized that the teaching ability of K–12 public school teachers varies dramatically. The Gates Foundation funded the "Measures of Effective Teaching (MET)" project, designed to determine how to best identify and promote great teaching. The project demonstrated that it is possible to identify great teaching by combining classroom observations, student surveys, and student achievement gains (http://www.gatesfoundation.org/media-center/press-releases/2013/01/measures-of-effective-teaching-project-releases-final-research-report). They are now doing detailed studies to identify what practices underlie the most effective teaching. Perhaps academic science should do the same to understand what great mentorship consists of. Then we could start to actually teach this to our students.

I have argued that the greatness of a university may well depend on high quality of mentoring; happy and well-mentored trainees to a large extent drive great innovation. Effective mentoring should be an expectation that is not only talked about but actually ensured. Universities have an obligation to better track the experiences of trainees in each laboratory, so that pertinent data can be collected (in a confidential system that protects trainees' careers). I suspect that some mentors might well be surprised to learn that their trainees are unhappy and would be grateful for and responsive to any feedback. If, despite counseling, a faculty member continues to routinely take advantage of their graduate students, harass them, or fail to mentor them effectively, then I strongly believe that privilege should be revoked.

Once we can identify great mentorship, we should much better reward it. This is more important than ever. When awarding prizes, let us not consider only those who made a great discovery but rather those who made a great discovery while at the same time effectively mentoring their students. Doing great science should be necessary but not sufficient. The honor of top prizes can only be enhanced by giving them to great scientists who are also great human beings. Honoring one's commitment to our young, and treating them generously and fairly, is an important sign of our integrity as scientists. So let's create more awards for great mentoring. And let's take mentoring effectiveness into consideration, when considering promotions and even in awarding NIH grants. After all, much of NIH grant funding is used to support the salaries of trainees to create the next generation of scientists. If we do all this, then we will be affirming as a community that quality mentorship really matters and is vital to the sustained success of science.

ACKNOWLEDGMENTS

B.A.B. gratefully acknowledges that he was most fortunate to have had the world's very best mentors for his graduate and postdoctoral training: David P. Corey and Martin C. Raff. David and Martin spent countless hours training and advising me, allowed me to be as independent as possible, providing gentle guidance when needed, always exhibited the highest integrity, and both helped me to love science even more than I ever imagined possible. Many thanks also to my current and previous trainees for their many helpful comments on this manuscript.

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SOME MODEST ADVICE FOR GRADUATE STUDENTS

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 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.

Acknowledgments

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 during the last decade, and to Pete Morin for suggesting that we write them for publication.

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REPLY TO STEARNS: SOME ACYNICAL ADVICE FOR GRADUATE STUDENTS

Raymond B. Huey

Preface

When Steve showed me the preliminary outline for his talk, my first response was to say, "Steve, this is really cynical, even by your standards! You can't possibly present such a negative view of graduate education." My second response was to draft an alternative outline, which I intended as a direct challenge to Steve's, and which I presented after Steve so rashly stormed out of Ecolunch.

A decade has passed since we performed that amusing skit. In transcribing our old outlines into text, Steve and I have tried to preserve the intentionally argumentative, point-counterpoint format and flavor of our original presentations. We do so, not because we remain convinced that our old views are necessarily correct (I am pleased to note that Steve now recants his views, at least in part), but because we want to emphasize a diversity of views of how to be a graduate student.

Our main point is this: there is no one way to be a graduate student. Each of us is an individual - each of us has individual needs, goals, capacities, and experiences. Advice that is productive for one student may be disastrous for another. So think about these and other views, but don't accept them without question.

Initial Premise

Graduate school provides an opportunity for you to change from being someone who reads to someone who is read. That is a major metamorphosis, indeed. Not surprisingly, it presents challenges as well as opportunities.

Always Expect the Best

If you anticipate the worst, you are likely to experience it. Instead, develop a positive attitude, decide what you want (T.A. position, research funds, etc.), and then get it. Go outside your university whenever possible for advice and for funds. Don't merely rely on your major professor. In short, be active and independent, not passive and dependent.

Some People Do Care

People are more likely to care about you if you act like a professional (see below) and if you make yourself valuable. Obtain a skill (multivariate statistics, electrophoresis) that you can share (and of course yourself). Avoid being used, however.

Seek out and collaborate with fellow graduate students, especially ones who are doing interesting work and who are enjoying it. You are likely to learn far more from graduate students than from your advisor, if only because you have more in common and spend more time with them. In short, use these interactions as an opportunity to be introduced to different viewpoints and techniques and to become excited about your career.

Seek out emeritus or near-emeritus professors, at least ones who are still active. They have a wealth of knowledge and experience, and often have the time and interest to share it. Moreover, they can give you a personal appreciation for the history of your field. Science is an historical activity, and progress in science is often enhanced by an understanding of the past.

On "Exhaustive" Thinking

Thinking "widely and exhaustively" can be mentally exhausting if you aren't academically and emotionally prepared. You may instead make better use of your first year by making up deficiencies in your course background (do so as quickly as possible!). Moreover, some people simply need time before they are ready to think independently. That maturation process can sometimes be accelerated by starting your research with a problem that your advisor "hands you."

Ultimately, however, you must begin to think and do research independently, and you must understand why you are doing a particular project.

On Psychological Problems

Expect them. Everyone will go through periods of intellectual insecurity or stress, most likely in the firs year or two. You can often minimize these problems with some simple tricks.

1. Get requirements out of the way as soon as possible. You will be surprised at how much your attitude toward graduate school and your research will improve once you pass all language requirements and qualifying exams. Keep in mind that faculty are inevitably impressed by students who aren't intimidated or slowed down by academic hurdles.

2. Some people simply need time to mature academically. So, fight directives and pressure to complete your Ph.D. in 4 years. You may need to take some extra time or even take a leave of absence. Changing schools or advisors sometimes helps, especially if you can first obtain a Master's degree.

Becoming a Professional

Think of yourself as a professional, someone who will be a biologist for the rest of your life. Start to accumulate a library and reprint collection, develop a computerized list of references and addresses, attend meetings, meet with visiting seminar speakers, correspond with people working on related problems, send out copies of your articles as they are published, etc.

Treat each project (even a literature review) as if it is potentially publishable.

Faculty are more likely to treat you as a professional if you act like one. They are a good source of suggestions in this regard. Ask their advice on efficient ways to organize your reprints and reference files, or ask them to recommend key papers (their own, or those of others) that influenced their thinking and careers. Read those papers, then go back and discuss them with the professor. (Note: Many graduate students have not read most of their advisor's papers, or those of other relevant faculty in their department.)

Despite your best efforts (and theirs), the faculty may have a difficult time treating you as a colleague rather than as a student. Therefore, develop contacts outside of the department and the university, thereby gaining a new perspective on biology and on your own work. Go on a tour of other universities, meet with faculty and students working in your area, volunteer (if appropriate) to give an informal seminar of your thesis work. If possible, spend a term and take courses at another university (or a field station), especially if a course is special and especially if you are spending your graduate career at one university. These outside contact not only broaden your perspectives but may also increase you chances for a collaborative research project, a postdoc, or even a job.

Join appropriate scientific societies, attend their yearly meetings, give papers or posters, get to know your future colleagues. Meetings can be exciting and a chance to find out what is new. Moreover, you get practice at speaking in front of a "foreign" (e.g., nonsympathetic audience).

On Courses

Never pass up a lecture course from a great professor, even if it is somewhat outside your main area. Seek courses that challenge you to think rather than to memorize. Auditing courses can often be an efficient way to get an overview of a field, at least if you are self-disciplined.

Take short courses that can save you time over the years. Many libraries give instruction on efficient literature searches (see also Smith's book, cited by Steve); and most universities offer introductions to computers, statistical packages, etc. If you don't know these critical skills already, immediately learn speed typing and word-processing.

On Proposals and Grants

Grant writing is a key skill. Ask professors for copies of their successful grant proposals (perhaps ask for unsuccessful ones as well). In other words, find out what makes a good proposal before your start writing; don't waste time "reinventing the wheel."

Be a scholar. Showing that you know and understand the literature makes a good impression, and it gives you an awareness of the key issues in your field.

Use the working proposal Steve describes as a basis for a real grand proposal. Many societies, government agencies (NSF), and organizations give grants to graduate students - ask your major professor and other graduate students for the names of such organizations. Prod your department or advisor to start a permanent file on such grants.

Getting your own grant has important benefits beyond simply funding your research. (1) It gives you something to add to your C.V. (2) It helps establish your independence from your advisor and your department. (3) It really impresses your advisor and your committee!

Interactions with Your Advisors

(Tangent. Even after a decade, I can still hear Steve pontificating the first sentence in this section. His expression, "a baroque excrescence," is my fondest auditory memory of Berkeley.)

Onward. A thesis shouldn't be a culmination of your research career, but its beginning. You probably never really had your creativity challenged as an undergraduate. Here is your opportunity. Push yourself - you'll respect yourself more than if you are too cautious and try a no-risk project.

Remember that your future research directions need not be constrained by the topic of your thesis. In fact, your thesis experiences may convince you that your interests and talents are elsewhere. Use a Master's-to-Ph.D. switch or a postdoc to change directions, if appropriate.

Publishing

Contrary to widespread opinion, writing and publishing can be fun. More important, the process of writing is a positive learning experience - my understanding of my own research is invariably enhanced while developing a paper or grant proposal.

Writing and publishing aren't always fun, of course, but you can minimize problems by being careful, by organizing your thoughts before you write, by taking pride in crafting sentences carefully, and by having people critically review your papers before you submit them for publication. This review process should be sequential: First, give it at an "Ecolunch." Second, write a draft and have your fellow graduate students and advisor review it critically. Third (optional, but advised), send it to one or a few experts in the field. Fourth, submit the manuscript.

(Having now been an editor of several journals and books, I would add several caveats. Make certain you follow the "Instructions to Authors" for the journal: If you use the wrong format, the editor will suspect that (1) your paper was previously rejected by another journal, or that (2) your work style is casual and not necessarily to be trusted. Also, carefully check the citations in the text against the literature cited section. Check text, tables, and figures for accuracy and neatness. (A paper that is neat and well designed is easy to read.) If you are writing an invited chapter for a book, do your very best to meet all deadlines. Editors cherish contributors who actually meet deadlines and follow instructions.)

Publishing is an important responsibility - you share your insights with others. It is also essential. People occasionally get good jobs or a grant despite of a weak or nonexistent list of publications, but the odds of this happening are slim, indeed.

Although over-publishing is a mistake (as Steve notes) don't be embarrassed by writing one or a few minor papers - ample precedent exist. Moreover, we are often our own worst judge of what is truly significant (see Bartholomew 1982). (After gaining the benefits of the experience, you can eventually obscure any truly trivial publications by using the following widely used technique - simply change your official "List of Publications" to a "Selected List of Publications" or to a "List of Publications since 19xx"!)

Miscellaneous

Watch for and take advantage of opportunities. If someone is organizing a special field trip, ask if you can go along and help. If there is a job search in your department, look through the applications and learn first hand what makes a good C.V. and what makes a clear statement of research and teaching interests. (Note: Not all departments permit graduate students to read application files.) Find out your advisor's opinion of the candidates' job seminars. Thus when you start applying for jobs, you will have some idea of what works and what doesn't.

Concluding Remarks

Appearances to the contrary, graduate students need not be oppressed. You actually have as much freedom as you ever have (except perhaps as a postdoc or during a precious sabbatical). Be positive, not cynical.

Postscript

"Ten years later," I wish to emphasize one comment and then make one addition. First, do spend time around students and faculty who are doing significant research and who are excited about their careers. In short, surround yourself with good people. Enthusiasm is contagious. Second, learn to respect and to practice the art of being organized. Thus, be efficient and don't waste time. This will almost certainly enhance your productivity and your enthusiasm for your career.

Acknowledgments

I am, of course, grateful to Steve Sterns, whose outrageous views prompted this reply. T. Garland, Jr. made useful comments on a draft.

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ISRAEL JOURNAL OF ECOLOGY & EVOLUTION, Vol. 57, 2011, pp. 293–307
10.1560/IJEE.57.4.293

ON BECOMING A BETTER SCIENTIST

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ABSTRACT

Good scientific research yields insights that are important and general. But the process of learning to do good science is far from simple, and the inherent challenges are often more motivational than scientific. I review various ways that may help scientists (especially young ones) to do better research. Perhaps the most important is to spend time with people who are smart, productive, and enjoy what they are doing: motivation and success are infectious. Trying some risky projects, for which success is not guaranteed, can enhance motivation. Before tackling risky projects, however, seek advice from those with experience; but make your own decision. Always be as self-directed as possible (and as political): actively seek opportunities and don't wait for them to come to you. If you have to learn a skill that is challenging or unpleasant, try to convince yourself that you look forward to learning it. Similarly, develop a high tolerance for repetitive tasks, which are inevitable components of science. In particular, learn to communicate well both in writing and in speaking: treat communication as a vital apprenticeship to be learned. Conflict is inevitable in science, but collaboration with opponents can be a positive way to resolve and grow beyond conflict. Staying fresh becomes a challenge as scientists age, but changing fields, continuing to go to seminars and meetings, and interacting with students and new colleagues can minimize one's personal fossilization.

Keywords: good science, communication, motivation

INTRODUCTION

Trying to write an invited article on good science is daunting, even though I've been a working scientist for over four decades. I take some solace in knowing that others who have trod this path before me have had similar concerns. Consider the wry comment made by the great physiological ecologist, George Bartholomew (1982), at the beginning of an invited lecture on scientific creativity:

> To undertake to lecture on innovation and creativity to an audience of research scientists requires that one be ignorant, or conceited, or foolhardy, or senile, or some combination thereof. I have given you my credentials…

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Received 5 September 2011; accepted 13 February 2012.

Although I feel uncomfortable pontificating on *good* science or on how to become a *good scientist*, I am comfortable writing about how to become a *better scientist*. Striving to become better is a feasible and necessary goal not only for a beginning scientist, but also for those of us now "long in the tooth."

I suspect I was invited to participate in this special feature because of seminars that Stephen Stearns and I gave back in 1976 when we were Miller Postdoctoral Fellows at the University of California, Berkeley. Professor Frank Pitelka asked Steve to give a seminar to an ecology lunch group, and Steve proposed giving one on how to be a graduate student. Before the seminar Steve and I got together and talked about our own experiences. We had rather different perspectives, and so we ended up giving back-to-back (point-counter-point) talks and distributed outlines of our main points. For us the experience was both fun and interesting, but was intended as a "one-time skit" (Stearns, 1987).

These presentations have had a surprisingly long life. Our outlines were widely distributed in the graduate student network, even in those pre-internet days. We were eventually asked to publish our talks (Huey, 1987; Stearns, 1987). Steve's article was titled *Some Modest Advice for Graduate Students*; and mine was *Reply to Stearns: Some Acynical Advice for Graduate Students*. As Steve recently wrote (Stearns, 2009), these are "our most widely read and least cited papers." They are now reprinted on scores of websites. Our presentations were strikingly different. That contrast highlights a critical message that may explain the longevity of our papers: namely, there is no one way to be a graduate student or scientist. Each of us is an individual, and so each of us needs to find a path that fits and that works for us individually.

Steve and I are now long past the graduate student or postdoc stage, but much of the advice we gave should still be relevant, even though the academic world has evolved. I won't reiterate "Stearns and Huey"; rather, I will build on those articles and try to add some new ideas and suggestions, or sometimes merely offer a deeper perspective on old suggestions. I will start with some general comments on good science, and then turn to the goal of becoming a better scientist. I will focus more on motivation than in Huey (1987), since I now better appreciate the fundamental importance of motivation and commitment to science. In addition, I add some practical advice on "jump-starting" a career.

A PERSONAL VIEW OF "GOOD SCIENCE"

The science that I myself like (and thus is "good" science to me) makes me aware of some issue for the first time, or changes the way I look at a familiar issue, or reinforces the way I look at a familiar issue. The operative concept here is impact.A good paper or a good talk somehow adds to, changes, or reinforces my view and understanding of science.

This concept is hardly novel. Sir Peter Medawar (1979) noted that:

> …any scientist of any age who wants to make important discoveries must study important problems. Dull or piffling problems yield dull or piffling answers. It's

> not enough to know that a problem should be "interesting"—almost any problem is interesting if it is studied in sufficient depth.

Similarly, George Bartholomew (1987) noted that "two of the salient characteristics of 'good' science are originality of conception and generality of application."

Fine, but how does one find problems that are both original and general? That is the real challenge, because it requires that one already appreciate which topics are currently important and exciting, and also know the "state of the art" in that field. Only then can one think deeply and creatively about where to go next (Stearns, 1987). Bartholomew (1987) did offer practical suggestions for organismal biologists:

> All successful animals must remain functionally integrated. All must obtain materials from their environments and process and release energy from these materials. All must reproduce. All must differentiate and grow. By focusing questions on these obligatory and universal capacities, one can ensure that one's research will not be trivial and will have some chance of achieving general significance.

CAN TECHNICALLY INSUFFICIENT SCIENCE STILL BE GOOD SCIENCE?

It is important to distinguish between science that is *technically sound* (i.e., that meets standards of replication, randomization, control, etc.) and science that has *an impact*. Ideally, a study is both sound and impactful (though no study is perfect). Sometimes, however, technical soundness is impossible to achieve. Can a project that is technically flawed still be good science that is worth doing? This is an old debate in science. I side with Max Planck (1949, p. 139), who noted:

> ...I must take exception to the view (a very popular one these days and certainly a very plausible one on the face of it) that a problem in physics merits examination only if it is established in advance that a definite answer to it can be obtained.

To argue my point, I'll give an example of an experimentally flawed—but I think still useful—study of my own. Xavier Eguskitza and I wanted to determine whether the use of supplemental oxygen promoted survival of mountaineers on Everest and K2. In an ideal world, we would have designed and executed an experiment in which we randomly assigned use of supplemental O_2 (or control canisters filled with normal air) to mountaineers, who would be "blind" as to whether they had supplemental O_2 vs. air. Then we'd compare survival rates.

This study will never be done. It would never pass human subjects review. No mountaineer would participate, and all climbers could immediately discern whether they were breathing supplemental O_2.

Eguskitza and I knew that a proper experiment wouldn't be feasible. Nevertheless, we chose to proceed because quantitative data on risks (even if not definitive) would be vitally important to climbers trying to decide whether to use supplemental O_2.

We compiled and compared survival rates of climbers using (or not) supplemental O_2. Note that the climbers themselves chose whether to use supplemental O_2. We were concerned with this self-selection because the two groups were not equally skilled: the only climbers who would choose to climb without supplemental O_2 would likely be the best and most experienced climbers in the world. So all else equal, non-O_2 climbers would be expected to have lower death rates than would O_2 climbers.

We found that non-O_2 climbers actually had higher death rates than did supplemental-O_2 climbers (Huey and Eguskitza, 2000). This result was counter to the bias induced by differences in relative experience (above), suggesting that climbing high peaks without supplemental O_2 is especially dangerous. When we subsequently shared our results with the mountaineering community (Eguskitza and Huey, 2000), we explained our study's limitations, so that climbers could decide whether our conclusions were reliable.

One should always aim for technical soundness, but a working scientist knows that soundness isn't always possible. I've known several people who are brilliant but who seem paralyzed and unable to do research, simply because they have technical standards that are unreachable by mortals. A study's goal should be to advance our knowledge, and that can sometimes be achieved even by technically flawed studies.

DO WE KNOW GOOD SCIENCE WHEN WE DO IT?

One might imagine that we always know when we're doing good science. I usually think I know whether a project I'm doing will be of general interest, and peer-review soon establishes whether my intuition was right. Even so, I know several world-class scientists who viewed certain of their most famous projects as obvious and trivial. In other words, what is obvious to some is not necessarily to others. Bartholomew (1982) had an interesting perspective here.

> One is often a poor judge of the relative value of his own creative efforts….One's supply of reprints for a pot-boiler is rapidly exhausted, while a major monograph that is one's pride and joy goes unnoticed.

Cowles and Bogert (1944) serves as an instructive example. This monograph introduced the concept of behavioral temperature regulation and is probably the most influential paper ever written in herpetology. When I was a graduate student, I interviewed Cowles, who was in his late 70s at the time. Cowles told me he couldn't understand why people found that monograph interesting, as it was all so obvious to him. He was hurt that people had ignored his truly important work on why the dinosaurs went extinct. Then he proceeded to lecture me on why the dinosaurs went extinct.

Do reviewers always know good science? I'm sure anyone who has had a paper or grant rejected will answer emphatically "No!" An amusing example concerns one reviewer's comments on Joe Felsenstein's (1985) classic paper on phylogenies and the comparative method:

> This paper addresses a complex and important issue, and provides a solution to part of the problem—a very unsatisfactory solution, as the author is well aware, given the degree to which our data will usually fall short of the quality required by the method he proposes….Nevertheless, as far as I can tell the method does what is claimed, and it is *probably worth publishing* (emphasis added)….

In the quarter of a century since that review, Felsenstein's paper has been cited over 3600 times and is the second most cited paper in the history of *The American Naturalist*! The reviewer obviously underestimated its impact.

My point here is not to criticize Bartholomew or Cowles, or to poke fun at the reviewer of Felsenstein's paper. Rather I want to highlight that the long-term impact of a project is not always immediately obvious, either to the doer or to the reader. Thus scientists may need to be patient and hope that their findings are eventually discovered and recognized, though I realize this long-term perspective may not be reassuring to a beginning scientist attempting to establish a career.

Incidentally, Bartholomew (1982) drew a practical lesson from this issue. He suggested that the "…strategy of choice is to increase the odds favoring creativity by being productive." In other words, when a project is finished, publish it, hope for the best, and keep moving. This is critical advice for both beginning and established scientists.

HOW TO BECOME A BETTER SCIENTIST

I will assume that most readers of this article will be young scientists trying to establish their careers. This stage of one's academic ontogeny is exciting—if sometimes terrifying. One is transitioning from being a student (someone who reads) to a researcher (someone who is read). Everyone finds that metamorphosis to be challenging. One needs to learn to do and publish research, to obtain research funds, to teach and mentor effectively, and to make a name for oneself, all in just a few years. There's much to learn and not much time to learn it.

The obvious question this is how can one jump-start a career? How can one learn all this and establish a reputation? I have no easy answers, but I can make some general suggestions.

First, and most importantly, spend time with people who are excited about what they are doing and who are productive. This is key because excitement and productivity are infectious. As a corollary, avoid people who are depressed, complaining, and unproductive. Our associates inevitably influence our achievements, at any stage of our careers. Associate wisely.

Second, pick your graduate and postdoctoral program carefully. Make certain that the department, advisor, and lab you choose are active and supportive. To find out, interview grad students and postdocs: they will usually be candid.

Third, actively seek and create opportunities: do not expect that they will miraculously land on your doorstop. In other words, become an "actively foraging" researcher, not a "sit-and-wait" one. For example, if your department doesn't have a course on some topic of importance to you, start a study group and encourage students and faculty to join you. You will learn what you need to learn, and impress everyone in the process.

Fourth, make your career as fun as you can because fun is motivational. The great mountaineer Alex Lowe often said, "The best climber in the world is the one having the most fun." I'm not a climber, but I do appreciate Lowe's insight: I always do my best work when I'm totally immersed in a project, because nothing else matters.

[Note: Of course, not all science is fun (see below). Moreover, some scientists are motivated by factors other than pleasure. When I started graduate school, a professor (later a National Academy member) told me that liking organisms was the worst possible reason for becoming a biologist. I disagreed with him then, and I still do. He and I are different animals.]

GETTING STARTED QUICKLY

Getting started in research is challenging. In some fields, students are handed a project. This obviously makes getting started easier, but won't be as satisfying as evolving your own project. Moreover, your career will be short if you depend on others for ideas. But even if handed a project, one must take ownership of it. Stearns (1987) proposed that the best way was to "read and think widely and exhaustively for a year." This is sound advice if you already know what topics are important. [Though this won't be feasible in universities with very short Ph.D. programs.] But when I started grad school, I had only very general ideas about I wanted to do and only began to focus while during fieldwork in the deserts of Peru and the Kalahari. I spent months walking around those deserts, just watching animals. Eventually I began to see patterns; and I then began to ask questions. Those deserts were my equivalent of Steve's library. A year in the Kalahari gave me the chance to "read" nature and think widely and exhaustively. Thus what matters is that you think hard and independently, not whether you do so in a library, a lab, or the field.

Graduate students often sample a series of projects before finding one that fits and is satisfying. As a beginning master's student, I tried several projects that had potential; but I soon discovered that they didn't fit my personality, skills, or interests. At times I became discouraged (would I ever find a good project?), but I kept moving forward. I supplemented my own experiences by helping fellow graduate students with their research. In so doing I was able to sample additional fields and to establish some good friendships at the same time.

In any case, the longer one is in science, the easier it is to develop new projects. When I now come up with a new idea for a project, I jump in quickly and see if has traction. I use the Internet to search for published data that will enable a quick-and-dirty test of my idea. Sometimes my ideas don't hold water (or will prove impractical), but sometimes they do. Whichever the results, I always find the experience invigorating: for me, few aspects of science are more exciting than the initial chase after a new idea or hypothesis.

Many ideas in science fail, and one must learn to accept and even appreciate being wrong. Biology is more complex than we can possibly imagine, and thus our expectations will often prove wrong or oversimplified. Many beginning students (and even old professors) are disappointed when their working hypotheses aren't supported: perhaps they interpret this as a sign that their scientific intuition is poor. Others see being wrong as an opportunity: *My idea seemed good, factually sound, and logically tight. So if it is wrong, then something interesting must going on here. I'll dig to find out what that is.*

A close friend and colleague (and a great scientist) once told me that only about half of his a priori expectations were supported. I remember being surprised, because I expected that he would be right most of the time. But when I later thought about his comment, I realized that might explain why he is such a successful biologist: when he is wrong, he finds out why.

RISK IS AN "ESSENTIAL DIETARY CONSTITUENT" FOR SCIENTISTS

Bartholomew and Medawar (quoted above) both note that good science necessarily focuses on important and general problems that are fundamental. But finding and selecting an important and general problem is only a first step. One needs to figure out how to convert an idea into a feasible research project and then to execute it and carry it through to completion. That requires skills and knowledge, which are field-specific; but it also requires motivation and commitment, which are universal.

Motivation to start and finish a project can be generated in a variety of ways (threat from advisors, greed, etc.), but motivation can also emerge from the project itself. The amount of motivation so generated depends in part on the degree of risk (in other words, the uncertainty of success) associated with that project. Some projects are sure bets, whereas others could easily fail. Sure-bet projects may be comfortable but are unlikely to yield major dividends; after all, if a project is important but also easy and safe to do, then someone will probably have already done it. In any case, sure-bet projects just aren't exciting to do and thus can't generate significant motivation or personal satisfaction (Stearns, 2009).

High-risk projects are inherently exciting. You are by definition trying something that is bold and for which success is by no means certain. That uncertainty generates the motivation and commitment needed to start and to finish a difficult project. As Tom Hornbein (1991) aptly wrote, "…risk is a necessary dietary constituent in medicine..." [Note: some people find uncertainty paralyzing: they probably should not be scientists.]

The uncertainty associated with risk taking is also a major motivator in extreme sports such as mountaineering (Tejada-Flores, 1967; Hornbein, 1991). As mountaineers become more skilled, they tackle increasingly difficult routes so that the outcome (summiting, surviving) remains uncertain and thus that experience of climbing remains satisfying. Scientists should do the same.

High-risk projects may be exciting, but they can easily fail. Bertrand Russell (1949) expressed this challenge: "A life without adventure is likely to be unsatisfying, but a life in which adventure is allowed to take whatever form it will is likely to be short." Even so, learning to accept failure is important. As Stearns (2009) wrote recently, students "…must learn that it is all right to make mistakes and not to fear them, for we all need practice in recovering from failure. Life is going to throw a lot of it at us."

So what's the optimal strategy here for a scientist? This is a serious question, especially for young scientists trying to make their mark. Personally, I think some risk taking is necessary to be competitive on the job and grant market. In any case I'll parasitize a strategic approach borrowed from Modern Portfolio Theory (MPT), an investment strategy designed to maximize the expected return from investments for a given amount of risk (Markowitz, 1952). MPT proposes that investing in a diversified portfolio of uncorrelated investments will maximize return:risk. Perhaps, then, a parallel strategy for young scientists is to start multiple, independent projects, each with varying degrees of return:risk. Thus if the high-risk project fails, one still has backup (uncorrelated) projects in the pipeline. If a high-risk project succeeds, you can increase your investment in it and go on to riches and glory.

I fully appreciate that each project requires an investment of start-up time, and there are significant time costs in starting several projects. However, one way to reduce cumulative start-up time is to collaborate with experts on some projects.

Advisors and friends may try to discourage you from trying high-risk projects. They may do this with the best of intentions, and of course they will sometimes (perhaps often) be right: some projects are just not feasible or practical. I'll have more to say about such advice in the next section.

SEEK ADVICE, BUT TAKE IT SELECTIVELY

Learning by doing is important, but is not always the most productive (or safest) way to proceed. Advice from an experienced scientist will usually help you get up and running quickly and also can help you avoid disasters. However, always evaluate advice and be prepared to reject it if you're convinced that is not right for you.

Be especially careful when someone discourages you from pursuing a new idea, stating that *it can't be done* or *it will never work*. Such negative advice sometimes says more about the limited vision of the advisor than about the feasibility or importance of the project.

Of course, negative advice is generally given in good faith, and an example concerns negative advice I gave to Barry Sinervo when he was a graduate student of mine. He and Larry McEdward had pioneered a way to manipulate egg size in sea urchins, and they used their technique to investigate the developmental (allometric) consequences of differences in egg size (Sinervo and McEdward, 1988).

One day Barry told me that he wanted to study the consequences of reduced egg size in lizards. He was going to stick a syringe needle into a lizard egg and suck out some yolk. I thought this was a clever idea, but I knew it would never work. I said, "Barry, lizard eggs are too sensitive. If you merely 'show' an egg a syringe (you don't even have to puncture the shell), the egg will roll over and die. Clever idea, Barry, but stay with your sea urchin system, which is elegant and which works." I was genuinely trying to save him from wasting time on a manipulation that I was convinced would fail.

Like all creative scientists, Barry followed his intuition and tried his luck. Several weeks later he brought in box of lizard eggs, the smallest of which was ½ the size of the largest. All were from the same clutch, but the small eggs had had some yolk removed. When I held the smallest egg up to the light, I saw a developing embryo inside. When I looked over at Barry, I saw one of the biggest and brightest grins I've ever seen. He had ignored my advice, tried and pulled off a high-risk experiment, and is so doing earned a classic series of papers, including three in *Science* (Sinervo and Huey, 1990; Sinervo and Licht, 1991; Sinervo et al., 1992). By ignoring my advice, he jump-started a successful career.

I don't mean to imply that all advice (or even mine!) is bad. Advice is usually given with the best of intentions, and is often the result of hard-won experience. Thus constructive advice is always worth considering and usually worth following. But my point is that if you really want to try something, but are advised against it, carefully consider the advisor's reasons and perspective. Then make your own decision. Of course, if you decide to ignore someone's advice, and if your project flops, then you'll have some bridges to repair. Conversely, if your project succeeds, you may need to figure out a way to save face for the advisor, who might be embarrassed by having given you "bad" advice.

Be skeptical of people (e.g., Horgan, 1996) who advise you not to enter a field because everything important is already known about it. Perhaps they are right, but perhaps they are merely blind to open opportunities. A classic example concerns Philipp von Jolly (a Munich physics professor) who told a young Max Planck not to go into physics because "in this field, almost everything is already discovered, and all that remains is to fill a few unimportant holes" (Lightman, 2005). Von Jolly did not live to see Planck win the Nobel Prize for developing quantum physics.

Biologists are not immune to negative attitudes. Professor Louis Agassiz was the founding Director of the Museum of Comparative Zoology, Harvard University. En route to a field expedition to Brazil in 1865, Agassiz lectured the ship's crew on biological topics. In one lecture he noted (Agassiz and Agassiz, 1868):

> The time for great discoveries is passed. No student of nature goes out now expecting to find a new world, or looks to the heavens for any new theory of the solar system. The work of the naturalist, in our day, is to explore worlds the existence of which is already known: to investigate, not to discover.

What an astonishingly negative statement for 1865! Given Agassiz's worldview of the contemporary nature of science, it is perhaps not surprising that he never accepted Darwin's views on evolution.

Several times I've been told not to do something because so much was already known. When I was a beginning Ph.D. student about to head to Puerto Rico to study *Anolis* lizards, a famous physiological ecologist told me not to bother because everything interesting about the lizards there was already known. After two weeks of field work in Puerto Rico, I had the data for papers in *Science* (Huey, 1974) and *Ecology* (Huey and Webster, 1976). Much was known, but not everything. Consider advice carefully, but make your own decisions.

LEARN TO LIKE WHAT YOU DON'T LIKE TO DO

Science is not all fame, fortune, and glory. The process of doing science is often boring and repetitious. Moreover, some aspects (data collection, data analysis, writing, speaking) can be challenging or even unnerving. As a result, many scientists often put off doing those things, or never learn to do them efficiently; for that reason, they inevitably become less successful than they could be.

A good survival rule-of-thumb is this: if some aspect of science is critical for success but is unpleasant or difficult for you, then "reprogram" your attitude so that you actually like to do that task. In other words, turn a dread into a delight. Your enjoyment of doing science—and the quality of your science—should improve dramatically.

I admit that reprogramming (or perhaps "self-deception") isn't always easy, nor can I tell you how to do it. I do know it is an important ability to master. For example, early in my career I disliked writing and was a terrible writer. But I knew that writing well was a basic prerequisite for a sustainable career. So I decided to start thinking of writing as a craft that I could not only learn to do, but also learn to enjoy doing. Fortunately, I had thoughtful advisors (Carl Koford, Eric Pianka) who valued good writing and who took the time to edit my papers. I now find that writing papers and even grant proposals (well, sometimes) has become for me one of the most enjoyable parts of doing science. But even decades later, I still study the craft. One should always try to get better.

A related issue of reprogramming involves the tedious, dull, and repetitive aspects of sciences. Learning to tolerate such tedium is a key survival skill. Linda Partridge views fly pushing (that is, counting and sexing thousands of *Drosophila*) as meditation (I definitely don't!). Others find that music in the background provides a useful distraction. Sometimes, one just needs to grit one's teeth and push relentlessly through a task until it is finished. Alternatively, one can break up a tedious task into bits and do them at intervals, but this often ends up taking more time and energy than just plugging away from start to finish. In any case, the associated tedium will eventually be forgotten (or at least buffered), especially if the project becomes a success.

TURN A DISADVANTAGE INTO AN ADVANTAGE

Graduate students often face hurdles en route to the Ph.D., and they frequently put them off as long as possible (Stearns, 1987). I put off taking my qualifying and thesis exam as long as I could; as a result I wasted a lot of time just worrying about that future exam. In retrospect, I wish I had taken that exam as soon as possible and gotten on with my research. The strategy here of jumping over rather than avoiding hurdles (Huey, 1987; Stearns, 1987) not only minimizes your cumulative anxiety, but also impresses your advisors.

At many institutions, the first chapter of a thesis is supposed to be an overview of the field. Students often object that such overviews will never be read and are thus a waste of time and effort. I felt the same and so put off writing the overview chapter until my last semester as a grad student. However, as I began to write, I was sidetracked by two new projects that were much more exciting to me than the overview. I told my Ph.D. advisor that I wanted to "trade" two new chapters for the overview. He agreed, as long as I would give a lecture on them for his biology class! I in turn agreed, as long as his artist would draw the figures. We were both happy with our bargain. I published one of the new projects (Huey, 1978), and I eventually did write an overview when a suitable venue became available (Huey, 1982).

Perhaps the optimal solution to the dreaded first chapter is to publish it as a review paper as well as include it in a thesis. Eric Pianka (1966) published the first chapter of his thesis on species diversity in *The American Naturalist*, and Steve Stearns (1976) published the first chapter of his thesis on life history evolution in *The Quarterly Review of Biology*. Both papers became Citation Classics. Both helped define fields. Both helped jump-start careers.

LEARN TO COMMUNICATE WELL

If I learn something exciting while doing a project, I want to (and am obliged to) share that information with an audience, and of course to subject it to peer review. Sharing requires communication, both verbal and written. A sustainable career in science requires effective communication skills, and beginning scientists must master those skills quickly.

Good writing is the key component of successful communication. A poorly written paper will force your readers to work hard to figure out what you've done and whether it is important. They will remember a badly written paper and so may avoid your papers in the future. Thus, if your paper loses your readers, it fails, even if the science you are reporting is fundamentally sound.

How does one learn to write well? One simple way is learn by observing: every time you finish reading a paper, ask whether you enjoyed reading it and whether you could easily understand it. If so, ask "why?" If not, ask "why not?" You'll soon discover what works and what does not, and thus find good templates for your own writing.

Learning to write is like learning to play an instrument or to play a sport. Practice, practice, practice. Get into a regular routine—write for at least one hour each day, every day, without fail. Work and rework papers until they work. Take pride as you see improvement.

Learn to make effective graphs, because they are the best way to convey patterns in data. Graphs can even help explain a complex theoretical idea (see, for example, the classic "morphology, performance, fitness" graph in Arnold, 1983). Graphs can make the difference between acceptance and rejection of a grant proposal or paper.

To learn graphical design, learn by observing. Study graphs in papers or in seminars. Ask whether they work. Pay special attention to papers and presentations by graphical masters, and read Tufte's books on graphical design (e.g., Tufte, 2001). The R Graph Gallery is an eye-opening introduction to diverse kinds of graphics (http://addictedtor.free.fr/graphiques/). Graphical design and communication are evolving very rapidly, so keep up with those advances. As publishing becomes ever more electronic, the opportunities for innovative graphics will only increase.

In the years after Stearns and I were grad students and postdocs, the Internet has of course opened up revolutionary way of communicating one's research and interests. Websites, Facebook, twitter, Skype, and other venues enable scientist to "sell" their work and to network with fellow scientists around the world. If your advisor is behind the times, volunteer to him or her set up a lab website; and make sure it features students and postdocs in the lab.

COMPETITION, CONFLICT, OR COLLABORATION?

During a long career, one will inevitably have conflicts with competitors or opponents. Sometimes those interactions are exciting, but sometimes they become unpleasant and nasty. Unfortunately, not all scientists are diplomatic or polite. In fact, some appear to thrive on conflict and go out of their way to provoke it.

Learning to deal with conflict is thus an important survival skill. One option is to fight back, but that will trigger escalation. Alternatively, one can design a new project to evaluate whether you are right. One of my own favorite projects was thusly motivated. I gave a seminar at a major university when I was a graduate student. In the question period, a professor made statements that were (in my view) not only wrong, but also rude and unprofessional. I was unable to convince him at the time that he was wrong. But some years later, I saw an opportunity to design a research project that would challenge the relative merits of our differing views. That project soon became fascinating in its own right and was done with some great friends, and fortunately supported my perspective (Huey et al., 1989).

Another way to deal with conflict is to propose that you and your opponent collaborate and try to resolve your differences. Chances are that both of you are partially right, and partially wrong. By sitting down and working through each other's assumptions and data, you two may reveal unexpected complexities. As a bonus, you may end up being colleagues rather than combatants. [N.B. This strategy works only if you and your opponent are both reasonable.]

For me, collaborating with colleagues has always been among the most enjoyable and productive parts of my career. I often do my best work working with someone. I've been lucky to work with people who are interested in the same problems, who are very smart but who have slightly different perspectives, so our views are complementary. As a result, we learn from each other, we motivate each other to do our share, and we push each other to carry the project through to completion. A little within-team competition is productive. Plus I've gained many vintage friends through these research collaborations.

ON SCIENTIFIC ONTOGENY—THE END GAME

Although this paper is largely intended for young investigators, I myself am long past that stage of my academic ontogeny. My own thoughts now have less to do with how to get started in biology, but rather with how to stay fresh and active. After all, with age may come administrative duties, declining health (personal or family), and even boredom. Not surprisingly, scientific productivity and creativity often decline with age. Thus a challenge to established scientists is how grow *older* without growing *old*. I won't claim to know the answers, but I am testing several.

The most obvious suggestion is to stay in environments that encourage growth and change, and where other people are growing and changing. Universities and research facilities are usually good venues for this.

Continuing to interact with students and postdocs via teaching or collaboration, as well as continuing to go to scientific meetings, should help, too. We seniors have long ago learned how our long-term colleagues think, and so we are unlikely to be surprised by a new paper they've written. But we won't yet have learned how young investigators think, and thus their papers are much more likely to excite us with novel ideas or to force us to question our long-held beliefs. Students learn from teachers, but teachers learn from students.

Changing fields (or organisms) can be invigorating. I've changed research directions several times and always with positive results. Sometimes I switched because I had become bored with a field (or because marginal returns were declining), or sometimes I was captivated by a new approach. In either case, the switch exposed me to a new literature, to new types of experiments and analyses, and to a new group of scientists. Of course, switching fields increased uncertainty and risk, thus amplifying my motivation and commitment (Hornbein, 1991). I will admit that getting funding to pursue a new direction can be challenging!

Ultimately, however, some ideas become so much a part of our very being that we can reject challenges to them. Ernst Mayr, one of the greatest evolutionary biologists in the last century, seemingly admitted this around 1976. Mayr had come to Berkeley to give a seminar. He was in his early 70s at the time, and I was a postdoc. At the departmental coffee before Mayr's seminar, his host asked if anyone could take Mayr to the airport after his seminar. No one volunteered, which I found surprising because opportunities to talk with someone of Ernst Mayr's stature are rare. I volunteered but immediately realized that I had put myself into a difficult spot. At the time I was an ecologist and knew little about evolution. I began to wonder what Mayr and I could talk about on the long drive to the airport.

I decided to ask, "How does one stay fresh in science throughout a long career?" Mayr's comments were interesting. They were candid.

First, he said, attend as many seminars as you can. He admitted that he usually didn't learn much at most of them, but some exposed him to a new idea or enabled him to see an old idea from a fresh perspective. So his primary advice was, go to seminars.

Then he described a biography that he was reading about Max Planck. The book summarized an interview with Planck when he was old. The reporter asked Planck what other physicists thought about quantum mechanics when he first published his ideas. Planck replied that they felt his ideas were dead wrong. The reporter then asked how today's physicists view his ideas. Planck responded that they see his ideas as dogma. When asked what accounted for the change in the way his work was viewed, Planck replied that he had outlived all of his detractors.

This view of science revolutions has become known as Planck's Principle (Planck, 1949, p. 33):

> A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it.

Fortunately, Planck's Principle is an exaggeration; but it conveys two important lessons. First, it assures younger scientists that being hit by heavy criticism from established (perhaps deadwood) scientists does not necessarily mean that they are wrong. Second, it reminds established scientists to fight to stay open to new ways of thinking so that we ourselves do not become the opponents in Planck's Principle. When Mayr told me this story as we drove to the airport, I was convinced that he was not only giving me advice for my future, but also that he was confessing that he had reached that stage of his career when he could no longer change some of his views. It was a poignant moment and a learning moment.

ACKNOWLEDGMENTS

I sincerely thank Inbal Brickner-Braun, Oded Berger-Tal, Keren Embar, and Avi Braun, and other graduate students at the Jacob Blaustein Institute for Desert Research. They were great hosts, I had a superb time, and I learned a lot. My advisors, friends, and colleagues have improved the way I do biology. I owe a special debt to Eric Pianka, Paul Hertz, Steve Stearns, Dave Wake, Al Bennett, Steve Arnold, Joel Kingsolver, Joe Felsenstein, Barry Sinervo, and the late George Bartholomew. I thank Tom Hornbein for introducing me to the idea of uncertainty and motivation, for his constructive and cogent feedback prior to my visit to Midreshet Ben-Gurion, and for allowing me to recycle several quotes from his 1991 paper. I thank Mike Kaspari and anonymous reviewers for constructive suggestions.

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ON BEING A SUCCESSFUL GRADUATE STUDENT IN THE SCIENCES

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Version 8.4
(Revised March 2013)

The competition for jobs in academia and in top companies 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 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 who are looking to change jobs and against whom you must also compete. For each of those jobs there may be 75-250 applicants, 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 and a critical mind 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 or a research position within a non-profit organization, a government agency, or an industry. There are many 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

[1] This treatise was developed as a supplement to the discussions I have with all new graduate students in my laboratory. It began as a few pages in the 1980s and has expanded in length over the years. Although I never intended it to spread beyond my own lab, it has taken on a life of its own, spreading over time by hand and email among colleagues in a number of countries. I continue to revise it from time to time as graduate life continues to change. The newest version is always on my lab website: http://bio.research.ucsc.edu/people/thompson

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 or models, 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 analysis of 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 non-essential 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. If you are having trouble with the Introduction, then try working on the Methods section. Or think of one crucial sentence that you want to place in the Discussion. Keep at it. 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. You will not be able to treat graduate school like a 40 hour a week job. 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 time to exercise and socialize.

Regard yourself and present yourself as a professional.

Don’t choose average graduate students and postdocs as your role models. Most of them will not end up with the kind of position you are hoping to attain. Aim higher, but do so with humility and respect for others.

Read broadly and critically.

Understand the broader context of your research. It is not enough to know the 100 papers most closely related to your dissertation topic. To do successful graduate work, you will want to have some familiarity with the wide range of subdisciplines that make up your field 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 read a majority of the abstracts and introductions of every issue of the major journals in your subdiscipline. If you spend and hour or two on each issue of the several major journals in your field of study, you will be well on your way to getting the broad perspective you need. Place yourself on the eTOCs of the major journals.

Reading regularly through just a few journals is not enough. You will want to regularly thumb through other related journals and books to look for fresh ideas or approaches that could help make your research novel. Online data search routines are getting better all the time, and you should make use of these as well. Keep an eye out for major new books in your discipline.

Attend national meetings of one or more major scientific societies, and join those 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. Talk with them about their research.

Join several of the major scientific societies in your discipline. It is part of being a professional. Scientific societies are more than the journals they produce. They are the voices for your scientific discipline. If we want scientists to have a say in the future of science, then scientific societies, and the meetings and outreach efforts they organize, are our best hope.

Learn how to write grant proposals.

Proposal writing is a fact of life in almost all major research positions. Early on, take every opportunity you can to read successful proposals (i.e., those that were funded) written by others. Ask yourself, what makes this a good proposal? Do the same with proposals that were rejected. Ask yourself, just what is it about this proposal that kept it from being funded. Help with the proposals being written in your own research group. Never try to write just a good proposal. Aiming for good is not good enough. At major granting agencies, often only 10-20% of proposals receive funding, and the percentages have continued to fall in recent years. You must use solid arguments to convince reviewers that this is a proposal that falls in the very top group. That group includes proposals that test major hypotheses, use up-to-date methods, show careful thought on experimental design, and provide a convincing case that the work can actually be accomplished during the funding period.

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 models and 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 more than 90% of submissions. Many major journals within specific disciplines 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 making corrections, 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.

3-5 (alternative): The better modern alternative to this entire procedure is to enter all the data twice into the computer and then write an algorithm to catch mismatches. If you use this method, correct the mismatches and then run the algorithm 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. Check to 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 analysis carefully. Is this really the ANOVA model that you thought you were choosing after you finished pointing and clicking through 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 he or she works on, only to have that person name a species, some higher taxon, some particular interaction between two taxa, or some small detail of a biological, chemical, or physical process as the reply? When you ask yourself that question, or answer it for others, you should be able to state clearly the major scientific question that you want to answer.

"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?

Later on when you begin to write papers based on your research, remember that "because it is poorly known" is the least convincing justification for scientific study. Even so, it is probably the most common justification given in the introduction of scientific papers. If you have thought deeply about your research as you have worked on your dissertation, you will be able to write justifications for your work that go well beyond that very weak justification. You will be able to explain clearly how your work addresses a major scientific hypothesis, resolves alternative hypotheses, explains conflicting results found in previous studies, or unifies past results that seemed to be caused by separate processes.

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 others. The ability to do so will not develop miraculously. You must learn from experience how to get your point across in research seminars, in classrooms, and in meetings with people outside your discipline. If you want to convince colleagues that you have something important to say, you need to be able to keep them awake and interested during a seminar or a discussion. Think about how often you have been bored by having to listen to a speaker who wastes an hour of your time as he or she mumbles or reads to you—slide after slide—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. The structure of a good talk is completely different from the structure of a scientific paper. Your goal should be not only to convey information on your recent work but also to put that information into the kind of broader context that is not possible in a scientific paper. The most boring talks are those are nothing more than a description of the methods and an endless series of tables and graphs. Your audience deserves more than these details, as important as they are. The audience deserves to hear from you what these results mean in a broader sense and why they should care.

Finally, never read a talk to an audience. As Daniel 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 may need some notes in addition to your slides. But when giving a research seminar, you will have your slides to prompt you. Use either no notes or at most a one-page outline. And don't cheat by piling so many words onto your slides that you are essentially reading the talk to the audience. No member of your audience wants to read slide after slide of bulleted text.

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 letters of Charles Darwin (published by Cambridge University Press and expected to reach at least twenty volumes at completion). You will find that Darwin was constantly writing letters to colleagues requesting help and information, and offering it when asked.

Learn how to introduce others, and learn how to introduce speakers.

You will often need to introduce colleagues to other colleagues. Learn how to do it effectively with several brief sentences, so that they can immediately see where they may be some common ground for conversation. Learn also how to introduce speakers by listening carefully to introductions by others and developing a style of your own. Speak clearly and briefly about that person's work and accomplishments. Don't bother giving a list of the universities that the speaker attended as student. It is irrelevant information and shows a lack of preparation of a real introduction. The audience wants to know why it might be worthwhile to listen to this particular speaker talk on this particular topic.

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 that 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 their 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 papers for major journals. There will be plenty of time later, if you want to collect together several 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 the same as research not done. But focus on publishing 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 include it in your dissertation.

Do not give your advisor a first full 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 versions of the Introduction and the Methods. 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 someone 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 gave 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 a week 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 initial 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 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 in mind when they submit proposals that include a postdoctoral position, or they have at least a short list of potential postdocs in mind based on 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, NERC, ERC, and other agencies associated with various research councils worldwide. And be sure to look for funding opportunities offered by private foundations. In most cases, 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 work 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 should 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 you to spend a couple of 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 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 doing 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 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 deeply 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.

ESSAY

How to succeed in science: a concise guide for young biomedical scientists. Part I: taking the plunge

Jonathan W. Yewdell

Abstract | Biomedical research has never been more intellectually exciting or practically important to society. Ironically, pursuing a career as a biomedical scientist has never been more difficult. Here I provide unvarnished advice for young biomedical scientists on the difficulties that lie ahead and on how to find the right laboratories for training in the skills that you will need to succeed. Although my advice is geared towards succeeding in the United States, many aspects apply to other countries.

If you are contemplating pursuing a career in the life sciences, or have already embarked on one, you need to give some thought to your career prospects. So, take a study break, grab a cup of coffee and read on.

Unfortunately, I need to begin with some depressing facts. First, only a small minority of Ph.D. students will ever have opportunities to become principal investigators (PI) in academic settings and direct their own independent research programmes (FIG. 1). Second, even if you are among this elite group, the odds are that you will be well down the path towards retirement by the time you receive your first research project grant (R01) (the average age is 43) from the National Institutes of Health (NIH), the principal source of funding for biomedical research in the United States. Third, for your entire career as a PI, you will put inordinate efforts into writing grants. If you should ever lose funding, you will be at the mercy of your institution for your continued employment. Fourth, if you do achieve the 'Holy Grail' of full professorship then you will not be poor, but you will be far worse off financially than nearly all of your peers who have similar levels of talent, energy and dedication, but who chose other careers.

Your professors might tell you that this is the way it has always been, but this simply isn't true. Twenty-five years ago the situation was much rosier. Scientists gained independence a decade earlier and funding, although never easy, was more reliable and accessible. Universities were more humane institutions where accountants had less influence over institutional priorities and decisions. Our current lamentable situation is fixable, and will have to improve significantly if the United States is to maintain its position as a leader in science and technology. A positive outcome is not guaranteed, however, and fixing the current mess will require the concerted efforts of scientists, university presidents and politicians to save the biomedical goose that has laid golden eggs for US biotechnology and health care for the past 50 years.

Science rocks

But there is good news too. Society desperately needs your talents. The future health, wealth and even survival of *Homo sapiens* depend on a deeper understanding of the laws and mechanisms of nature and on using this information to develop new technologies and therapies. For rationally thinking people with an altruistic bent, life can be no more rewarding than when practising the scientific method for the benefit of all of the denizens of this fragile planet. As a budding scientist, you are trained to expertly use the scientific method. That is, you learn how to wield the body of techniques that are used to identify and investigate natural phenomena by formulating and rigorously testing hypotheses. The origins of the scientific method date back at least 1,000 years, and it is arguably the most important invention of civilized man. Armed with the scientific method, we can explore and understand nature to the limits of our intelligence. As a high priest of 'Scientific Methodism', you will be equipped for success not only in science and its allied occupations, but in virtually any career that requires rational decision making (and in some, such as politics, that ought to).

More good news: for individuals with a hunger for knowledge and an insatiable curiosity about how things work, science offers a constant challenge and, best of all, the intense thrill of discovery. What can match being the first person who has ever lived to know something new about nature? And not just the big, infrequent, paradigm-making (or breaking) discoveries, but the small, incremental discoveries that occur on a daily or weekly basis too. If this doesn't give you goosebumps and if you are not in a rush to get to the laboratory in the morning to find the results of yesterday's experiment, then you should seriously consider a non-laboratory career. Making discoveries is the core reward for the myriad of difficulties you will face in your scientific career (see Part II, in which I discuss making discoveries[1]). Although it is possible to succeed in science even if you lack this passion for discovery, you will almost certainly be miserable and make your colleagues, friends and family wretched too.

Science has other perks. Contemporary science is one of the most communal activities ever pursued by humanity, and is among the most international careers possible. You will probably be interacting on a daily basis with scientists from all over the world, both in your laboratory and over the internet. Once established in your career, you can fly to dozens of cities across the globe and be greeted by a colleague that you either know personally or through reading each other's publications. You might even train a generation of researchers in your laboratory who will disperse around the globe to pass the torch of the scientific method to the next generation of their nation.

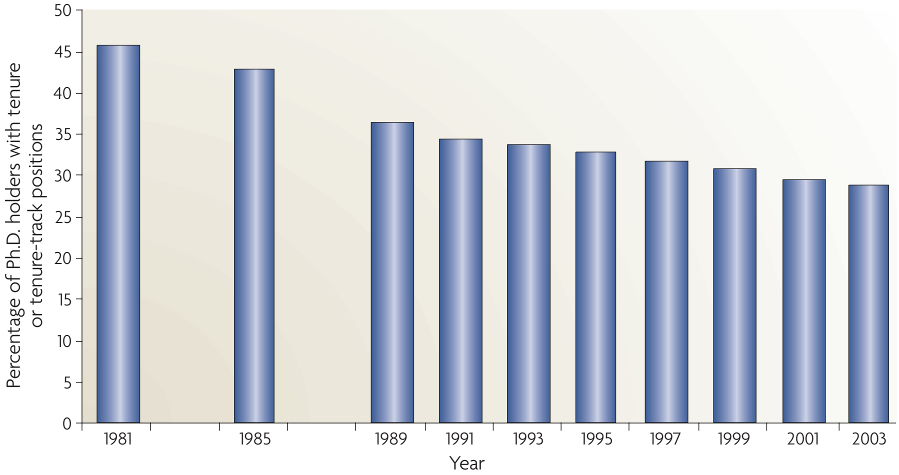

Figure 1 | **The tenure track derails.** The number of doctorate degrees awarded per year in the United States in the life sciences has increased more than threefold since 1966, whereas the number of tenured scientists has decreased slightly from a peak in 1981 (according to National Science Foundation data[3]). Consequently, in the past 25 years the fraction of Ph.D. holders with academic independent investigator positions has decreased steadily. The fraction of Ph.D. holders with tenure or tenure-track position is now ~30%. Graph reproduced from REF. 3 © (2007) FASEB.

This generational transfer of Scientific Methodism is, in fact, the most important and tangible achievement of a scientist. Discoveries are the joy and stock of our trade, but when your career is over (and probably well before this moment), few people will remember your brilliant papers. If you are successful (and lucky), you will have contributed a few lines to text books that future students will resent having to memorize. Through no fault of your own, and for reasons that you could not have anticipated, your discoveries might prove to be the artefacts that led your field in the completely wrong direction. You will be happiest in science if you are content with pursuing the truth to the best of your abilities and in passing the skills and insights you have developed to the next generation. Scientists who pursue fame are destined to be forgotten and forever dissatisfied with their achievements. In practical terms, peer recognition is needed only to maintain funding and to attract talented individuals to your laboratory who will make your daily laboratory life more productive and enjoyable. Beyond this, chasing fame is a waste of time that could be better spent on science itself, or on enjoying life outside the laboratory.

Getting started: graduate school

Choosing a graduate programme. Choosing a graduate school in which to pursue your Ph.D. should be largely based on the field that you would like to enter. Obviously, you should choose a programme that has a well-respected faculty. Size provides a large number of advantages, including a larger number of potential mentors to choose from, more students and post-doctoral fellows who can become lifelong friends and colleagues, better chances for collaboration, greater access to reagents, techniques and specialist equipment, and a more exciting intellectual environment. To minimize the insanely long 'training' period of your career, you should find a programme that takes pride in expeditiously awarding Ph.D. degrees . It should take 4 or 5 years for a decent student to finish a Ph.D., with an absolute upper limit of 6 years. Any longer than this and the student is either not suited for science or is being exploited by the mentor. Also, choose a department where the current Ph.D. students are treated as junior colleagues, with an eye towards their career development, and are not just exploited as inexpensive labour (small departments can be better in this respect).

Choosing a laboratory. Once you have chosen a school (or vice versa) to work in, your most important decision will be to choose a laboratory. The decision can be based either on the topic of research or on the mentor. I would strongly recommend the latter (BOX 1). Good scientists work on interesting and important topics, so a good mentor has this covered. Your goal as a graduate student is to become an expert in wielding the scientific method, and this can be achieved pursuing any project. The topic matters most in the types of experiments it entails. A good project will enable you to design, perform and analyse experiments on a routine basis,

Box 1 | **On the innate superiority of rabbits over wolves**

A rabbit is happily grazing one day when it is ambushed by a wolf.

"Please don't eat me Mr Wolf," pleads the rabbit, "I haven't completed my Ph.D.!"

The wolf spits out the rabbit and laughs until he almost chokes.

"Yeah right! A rabbit? Doing a Ph.D.? What about? Carrots? Duracell batteries? I just gotta hear this one!"

The rabbit clears its throat and intones: "On the innate superiority of rabbits over wolves."

"That's a crock for a start," scoffs the wolf.

"But I can prove it," says the rabbit. "Come to my hole and I'll show you my results, and if you still don't believe me, then you can eat me. Deal?"

"Sure. Can I have fries with that?" says the wolf, following the rabbit down the hole.

But only the rabbit comes out.

Months later the rabbit is grazing contentedly again when it meets another rabbit.

"How's tricks?" asks the friend.

"Wonderful," says our hero, "I've just submitted my Ph.D. dissertation."

"Congratulations! What's it called?"

"It's called 'On the innate superiority of rabbits over wolves'."

"Unbelievable — I mean, literally. Are you sure?"

"Yes, I thought it was crazy at first too. But I've tested the model rigorously and that's the result I get."

"Wow…"

"Look, if you don't believe me, why not come to my hole and I can show you the results?"

"Of course, I'd love to!"

So the two rabbits scurry down the burrow. In the first chamber is a workstation, covered with and surrounded by piles of books, papers, printouts and half-eaten carrots. In the second chamber are boxes and boxes of wolf bones, all catalogued and annotated. And in the final chamber, in a rocking chair, is a large and very satisfied looking bear.

Moral: do your Ph.D. on any subject you like, provided you have a good supervisor.
Posted on the *Nature* Network.

Figure 2 | **The nine types of principal investigator.** This cartoon was kindly provided by Alexander Dent, http://dentcartoons.blogspot.com.

ideally several per week, if not daily. This provides the best training and, importantly, is also the most fun. This will also develop your abilities to conceive the crucial controls that are needed to interpret the data in a meaningful way. 'Control creativity' is a central part of your scientific IQ; it comes only from the experience of designing and interpreting experiments. You should avoid projects that are largely based on using a single technique to develop a reagent or collect data (for example, generating a transgenic mouse).

Choosing a mentor. Although there is tremendous subjectivity in choosing a compatible mentor, there are a number of objective criteria (FIG. 2). Are the people in the laboratory happy and enthusiastic about their research? Have former students gone on to productive careers? Does the mentor treat students as junior colleagues and not as employees? Generally speaking, you should run from laboratories where a PI is referred to as Doctor X and not by his or her first name.

Frequently, you will have to choose between a small laboratory with a new investigator versus a large laboratory with a well-established scientist. Newly minted assistant professors will not have much of a track record as mentors; you might even be the first student they train. Still, you should seriously consider joining such a laboratory if the chemistry seems right. Although this has its obvious risks, you are a much more valuable commodity to a small laboratory, the survival of which could well depend on your personal success. Consequently, you will get more intense mentoring and will probably be working side-by-side with the PI. The best situation is to be the first Ph.D. student of a rising star, for you will be maximally productive, will generate well-developed ties to your field and will have an influential champion for years to come (although because academic 'star' formation is an inexact science, this often takes some luck).

Skills, not papers. Contrary to what you might have heard, it is not critical to have a spectacular publication record from your Ph.D. When the time comes to apply for a tenure-track job, the selection committee will focus on the productivity and promise you displayed during your postdoctoral fellowship. Furthermore, a solid Ph.D. with one good first-author paper that is based largely on your own work is all that is usually required to obtain the postdoctoral position of your dreams, particularly for citizens of the United States, who are in short supply at this level. Your focus as a graduate student should be to develop all of the skills you will need to be an independent scientist.

At some point as a graduate student you will need to take responsibility for all aspects of your career and develop the skills of an independent scientist. You need to develop confidence in your ability to make discoveries and learn new techniques, so that you will not be limited later in your career when your findings lead you to new and unexpected areas (see Part II (REF 1)). You need to do the background reading to place your results in their proper context and determine the next step in the project. You need to learn how to present a seminar in which you convey not only the data and conclusions, but also your depth of knowledge and enthusiasm for your field of research. Such public-speaking skills are critical for peer recognition of the impact of your research, for recruiting students and fellows to your laboratory, and for effective teaching. Most importantly, you need to learn how to write concisely and lucidly[2], for without this skill, you will not be able to raise grant money or place your papers in high-impact journals.

Step two: postdoctoral fellowship

In many ways the most important decision on the PI career path is where you do your postdoctoral fellowship. It should be in a field in which you envisage starting your independent career, the success of which will be almost entirely dependent on your ability to attract funding. As a newly independent scientist, study sections will be loath to fund you to embark on a project that is not a direct continuation of your postdoctoral studies. This also means that you will need access to the reagents you developed as a postdoctoral fellow. You will also need the blessings of your mentor and, optimally, your mentor should actively support your nascent career. So, in choosing your postdoctoral mentor, it is critical to determine whether a mentor enthusiastically supports, both materially and psychologically, the careers of their fledglings. This is easier to determine if the mentor is an established scientist with a pedigree. Established scientists will also be able to offer laboratories with a greater variety of expertise, reagents and greater financial resources, all of which will help you establish an independent line of research for you to parlay into an independent career.

It is essential to visit the laboratories that interest you to gauge the productivity, independence and happiness of the students and postdoctoral fellows. It is a good idea to contact scientists who have left the laboratory to obtain their honest opinion of their experience (in laboratories headed by evil mentors, this might be the only way to ascertain their pathology, as the current laboratory members may be too intimidated to express negative opinions). If the laboratory won't pay your travel expenses, then this does not augur well, as it indicates either limited financial resources or stinginess. All things being equal, it is advantageous to work at larger, wealthier institutions where there will be better access to expensive, state-of-the-art instruments and core facilities, greater overall intellectual ferment, more laboratories for collaboration and a better chance to impress other established scientists, who can write the crucial recommendation letters for getting your tenure-track application into the interview round. Sometimes, however, all things are not equal, and if the best mentor is at a smaller institution, this will do just fine.

What is it going to take?

Perspiration. Success in science will require a major commitment of your body and soul. As a graduate student, you should be spending a minimum of 40 hours per week actually designing, performing or interpreting experiments. As there are many other necessary things to do during the day (for example, reading the literature, attending seminars and journal club, talking to colleagues both formally and informally, and common laboratory jobs), this means you will be spending 60 or more hours per week in science-associated activities. The key to success and happiness is that most of this should not seem like work. If the laboratory is not the place you'd most like to be, then a career as a PI is probably not for you. At the postdoctoral level you will have to work at least as hard, but your most intense effort will actually begin as a tenure-track faculty member, when you are expected to fund your research (and at least some of your salary too), teach undergraduates as well as graduate and professional students, serve on committees and run your laboratory, which itself entails learning an entirely new set of skills (such as accounting, diplomacy and psychology). Ironically, you will have more to learn as a fledgling professor than as a postdoctoral fellow. Until you are well into your career, there will be time in your life for just one additional significant activity (family, active social life with friends, a sport or a hobby), but probably not for much more than that.

Talent. Enthusiasm and effort are necessary but not sufficient for a successful scientific career. Talent is a key part of the equation, and at some point in your career (not necessarily as a graduate student), you will need to objectively assess your skills and potential relative to your peers. The inexorable weight of the scientific career pyramid squeezes out all but the most talented from getting the tenure-track job that will offer you the chance of establishing your own laboratory. Furthermore, the insanely competitive funding situation is making the previously safe transition between tenure-track and tenured professor a far dicier proposition. Scientific talent is not a single parameter, but a complex mix of innate and learned skills and abilities. Deficiencies in one area can be offset by strengths in another. Some scientists achieve success by their experimental skills or insights, others by their management or political skills. There is no one path to success and each successful scientist has unique combinations of strengths (and weaknesses).

If, for whatever reason, you decide that you are better suited for life outside the laboratory, there are numerous career alternatives. Neither you nor your mentor should consider this outcome a failure. It is unfair, and even irresponsible for mentors to expect trainees to emulate their own career paths. Each mentor has only to train a single replacement to maintain the PI population at equilibrium. Even with robust growth in NIH-funded biomedical research (which is unlikely in the foreseeable future), the current investigator-to-trainee ratio dictates that most trainees will pursue careers that differ fundamentally from those of their mentors.

Networking plays a key part in providing information about potential alternative careers and in landing such jobs. Alumni of the laboratories and departments you have worked in are the most proximal source of networking partners. E-mail has opened a great portal into the academic community for initiating contacts that can be deepened by follow-up telephone conversations. It can be difficult to penetrate the corporate world by this path, but conferences provide ideal circumstances for meeting scientists out of the academic mainstream who can provide insight, advice and even job opportunities. It might be possible during your postdoctoral fellowship to develop your skills and attractiveness to potential employers by moonlighting or volunteering in the career path you are contemplating.

Final thoughts

So, your cup of coffee should be finished by now. Please don't be discouraged, but give some thought to your career path. If you are talented and passionate, you will have a good chance of becoming a PI; particularly in the United States, which still provides great opportunities for truly independent entry-level positions. If the trials and tribulations of being a PI aren't for you, there are many other ways to use your scientific training to make a decent living and a valuable contribution to society. Now get back to work.

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doi:10.1038/nrm2389
published online 10 April 2008

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Acknowledgements
The author is grateful to the many junior and senior scientists who shared their insights into scientific success. B. Dolan, K. Grebe, S. Hensley and J. Ishizuka made valuable suggestions for improvements to the manuscript.

FURTHER INFORMATION
Jonathan W. Yewdell's homepage:
http://www3.niaid.nih.gov/labs/aboutlabs/lvd/cellBiologyAndViralImmunologySection/BenninkYewdell.htm
Graduate student resources on the web:
http://www-personal.umich.edu/~danhorn/graduate.html
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ESSAY

How to succeed in science: a concise guide for young biomedical scientists. Part II: making discoveries

Jonathan W. Yewdell

Abstract | Making discoveries is the most important part of being a scientist, and also the most fun. Young scientists need to develop the experimental and mental skill sets that enable them to make discoveries, including how to recognize and exploit serendipity when it strikes. Here, I provide practical advice to young scientists on choosing a research topic, designing, performing and interpreting experiments and, last but not least, on maintaining your sanity in the process.

You're back for more advice, despite my best efforts in Part I (REF. 1) to paint the bleakest possible picture of career prospects in biomedical research? Well, I am delighted you haven't enlisted in the French Foreign Legion just yet. In fact, it's a great pleasure to welcome you as a fellow practitioner of 'Scientific Methodism'. Your mission now is to discover something completely unexpected about how cells or animals work. You might think that such surprises top nearly every scientist's 'to do' list, but this is not the case. The present culture in biomedical research favours conservative science, which essentially entails refining accepted models.

Swim against this current. Your mission as a scientist is to discover how current models are wrong, not right, and to create new paradigms. When you succeed, you will have to fight to publish and fund your research. However, if you persist (and are actually right) then the world will eventually come around to your point of view. At this point, your mission will be to expose the flaws in your new paradigm, and so on. The best part of your newly chosen career is that you will never have to worry about running out of things to discover.

Choosing a project

Experience counts. To make a discovery you'll first need to choose a research project. As a graduate student, it is wise for the principal investigator (PI) to choose the initial project, or at least play a major part in choosing the project. You simply don't have the experience and judgment at this point to choose an interesting project with a significant chance of success. At a postdoctoral level, the decision is more conditional. If you are continuing in the field of your Ph.D. studies, you should be capable of choosing a good project. If it is a new field, however, your advisor will need to provide guidance as to what is feasible and interesting.

Make the most of your surroundings. In choosing a project, it is crucial to exploit the intellectual and physical resources of your immediate surroundings. This does not just mean that you should plough the same furrow that the laboratory has already seeded and harvested. Introducing new techniques and approaches to your laboratory provides many advantages. For example, you will gain confidence in your ability to follow up your findings wherever they lead. It is much easier, however, when you can learn from the expertise of neighbouring laboratories. Imagine, for example, that your institution has a first-rate confocal microscope facility, but that confocal microscopy has never been applied to the major research interest of your own laboratory, even though it has a number of obvious applications. Should you take advantage of the situation? Of course! An extreme example to be sure, but many projects have foundered before they started because of the sheer impossibility of gaining access to the requisite technology or reagents.

> “The best part of your newly chosen career is that you will never have to worry about running out of things to discover.”

Basic or applied research? There is an important dichotomy between applied and basic research. Funding agencies put a tremendous emphasis on applied research, which is clearly important, as it is the sole means of translating discoveries into therapies. However, applied research is based on the knowledge at hand, regardless of whether it is sufficiently sophisticated to have a reasonable chance of improving existing therapies. Furthermore, applied research is far less likely than basic research to lead to serendipitous findings that will provide novel insights into unexpected quarters. The nature of applied research is such that if a clinical trial does not work, the project is usually kaput. By contrast, biology is such a complex tapestry woven from a myriad of components and pathways that, with some patience, properly performed basic research will always lead to interesting discoveries. The problem is that translating these discoveries into therapies is often indirect, and invariably requires decades. This requires a level of patience from funding agencies that is difficult to maintain in the face of political pressure to provide immediate therapies and cures.

Big or little questions? Although it is a good idea to avoid following the herd, don't shy away from pursuing important questions, which by their very nature will attract the attention of other laboratories. It is usually no more difficult to work on something interesting and important than it is to work on something of limited interest that will be difficult to publish and fund. Ideally, you will be far ahead of the pack and won't have to worry about direct competition until you spill the beans about your great findings.

Figure 1 | **Another reason for small experiments.** This cartoon was kindly provided by Alexander Dent, http://dentcartoons.blogspot.com.

Having such a lead isn't always possible, but you should always aim to have a novel approach to your research question, even if your approach is a bit oblique.

Designing experiments

Ideas: they don't come from storks. Most graduate students have had minimal independent research experience and will depend heavily on their advisors (or on the postdoctoral fellows that they are teamed up with) to get a feel for designing experiments. Within their first year full time at the bench, however, students should be designing their own experiments. Experimental design encompasses many parameters. The most important, of course, is the hypothesis the experiment is designed to test. For this you need to have an original idea. But where do ideas come from?

Although really good ideas seem to come from nowhere (at the same time, they also seem obvious after the discovery), they are seeded by information from external sources. The key concept is cross-pollination. Talk to your fellow students and more senior scientists in your department and at meetings. Discuss your (and their) research. Commonly, ideas and techniques that are standard in one field are novel in another, and their application can lead to breakthroughs. Read widely, but not necessarily deeply. Scan the major journals; if the title is interesting then read the abstract. Still intrigued? Read the discussion. Only if the paper seems relevant should you actually look at the data and then carefully read all of the sections. While on this topic, reading the methods sections of irrelevant papers can give you good ideas about how to improve your experimental protocols or can suggest novel strategies to attack your problem. You should also attend seminars in other disciplines, but sit near the back and beat a strategic retreat if the talk turns out to be of little interest.

Growing your wings. There is nothing like enthusiastic naiveté to seed a discovery. Knowing too much about a topic can actually be a barrier to discovery. Experiments that experts know won't work sometimes do, because either the experts' assumptions are wrong, or new reagents or technologies became available that allow nature to be queried in a new way. Imagine you have just read the latest issue of *Nature Reviews Molecular Cell Biology* and are struck with a stupendous idea. You excitedly barge into the office of your PI and propose your killer experiment. She spends the next 30 minutes explaining in excruciating detail, with impeccable logic, why the experiment not only can't possibly work, but will be uninterpretable if it does. Dejected, you stumble from the office in a haze of self-recrimination and doubt. But then, while cycling home, you regain your bravura and decide that you are going to do the experiment anyway.

This is exactly the right attitude that you should have. It is crucial during your training that you develop confidence in your insight and learn to think independently of your mentor (in the wise words of my first mentor, "the outcome of the perfect training experience is that you leave the laboratory thinking that your mentor is a good person, but a bit dumb"). So you do the experiment, and 95 times out of 100 the experiment doesn't work. Don't freak out. Here's a secret from the PI world: if you don't tell us, we won't know that you even did the experiment. When I walk through my laboratory, I have no idea what the postdoctoral

Box 1 | **On fraud**

Science always has been, and always will be, tarnished by fraud. Scientific fraud is ultimately self-correcting, but it wastes precious human and material resources. Fraud harms or even kills people when it involves clinical research. Fraud undermines society's faith in the integrity of science, and threatens public support of science and the scientific method.

Extreme competition for funding brings out the worst in human nature. When scientists' careers are on the chopping block with each paper or grant rejection, even good people can succumb to temptation. Fraud encompasses much more than pure black-and-white fabrication: it includes fudging data and cherry picking experiments to support the most convenient conclusion (this topic is treated wonderfully in the novel *Intuition*, by Allegra Goodman[3]).

Sooner or later in your career, you will suspect the legitimacy of a colleague's data. You are obliged to expose fraudulent activities, but you must do so in a careful, considered and deliberate manner. Being unable to reproduce the findings of others does not necessarily mean that the findings are fraudulent. Some experimental systems are exquisitely finicky. Some scientists are more skilled than others.

If you are convinced that fraudulent activity has occurred in your laboratory or in another laboratory, the first step is raise the matter with your principal investigator (PI). If you are not satisfied with the response of your PI, you should approach a different, sympathetic PI in your department, and ultimately the Chair. Still not satisfied? Contact the Office of Scientific Integrity or the responsible Dean or administrator.

You should be aware that with each step of the process, the stakes for everyone involved (including you, the whistle-blower) are magnified, and scientific careers can be destroyed.

fellows are doing. I know what experiments they've done recently, and what we discussed they should probably do next (it's their decision), but on a day-to-day basis, I really don't know. Just watching them pipetting something or looking into a microscope, whatever the purpose, puts a smile on my face — they might discover something today!

So when the experiment doesn't work, put the data in your notebook (the failure will probably be useful down the road) and don't tell your PI. On the rare occasion when the experiment gives you a glorious result, you will have the great pleasure of strolling into the PI's office with a broad grin on your face and asking (magnanimously, of course) whether they would care to see the data from the 'experiment that would never work'. Only a control freak PI (see figure 2 in Part I (REF. 1)) could fail to share your joy and excitement. In fact, when you are a PI yourself be careful when discouraging your mentees from performing experiments, no matter how spectacularly flawed they might seem. There is simply no substitute for enthusiasm in science, and you douse it both at your own peril and at the peril of those whose careers are your responsibility.

Size matters. Having a good idea (or even a bad idea, sometimes any idea will do, as they can all lead to serendipity) is only the start. Designing experiments is an art that you will continue to improve for as long as you work at the bench or supervise those who do. The size of the experiment is crucial (FIG. 1). It should be just large enough to have a sufficient number of repeat samples and positive and negative controls for you to interpret the results with confidence. Small experiments are much more likely to work than big ones, as there is less to go wrong. Furthermore, no matter how much thought you give to the experiment, the crucial controls will occur to you after doing the experiment, typically only after many repetitions, if at all. Rare is the scientist who has not been confronted with an essential control when the work is presented in a seminar or for publication. By doing a series of small experiments with constant modifications based on each preceding experiment, you will progress much more rapidly than by performing larger experiments that try to anticipate all of the problems and possible outcomes. An important psychological advantage of small, rapid experiments is that failure (the typical fate of new experiments) is much less depressing than after spending huge amounts of time and energy in a much larger but equally unsuccessful effort.

Doing experiments

Golden eyes. Every well-established laboratory has a 'Hall of Fame' of legendary alumni with 'golden hands'. Golden hands? Golden eyes is closer to the mark. Experimental science does not demand the dexterity of neurosurgery, but it does demand the neurosurgeon's focus on the task at hand. The key to being a good experimentalist is obsessive attention to detail. They are constantly thinking about the matter at hand (and not about dinner, their next work-out or the cute student in the next laboratory). They constantly use their eyes to monitor every relevant detail. For example, is the water bath too hot? Is the CO_2 setting in the incubator correct? Is the buffer cloudy or off-colour? In cell-based experiments, the golden eyed pay close attention to the cells. They have a feel for how cultured cells look when they are thriving and for how to keep cells in tip-top shape for each experiment. They are constantly scrutinizing the cells during the experiment, even using the microscope when convenient to monitor cell happiness (and to make the odd discovery based on the macro-behaviour of cells). They notice the size, colour and texture of the cell pellets and how they disperse. Details, details, details!

Good experimenters understand every part of an experiment (including buffer and detergent selection) and quickly learn to recognize which are the most important aspects of an experiment and which steps can be shortened or even discarded. While doing the experiment they are already planning how each step could be improved or done more efficiently (doing things more quickly allows more samples to be included or more experiments to be performed, and can be crucial for making discoveries).

Although the repetition of experiments is an essential step to gain confidence in a finding, it is a poor experimenter who does not frequently make at least minor changes to their protocol. In fact, making the same finding after modifying an experiment bolsters the validity of the finding. Above all, as an experimental scientist, you must be certain that your observations are reproducible (BOX 1).

Laboratory notebook: the scientist's best friend. An essential part of each experiment is to record accurate and appropriately detailed notes. Start each experiment entry with a statement regarding the hypothesis you are testing. In describing your actions, make sure you include all of the unique details of the experiment that you will need in order to repeat it. Those who don't heed this advice are fated to make an incredibly exciting finding that they will never be able to repeat. Believe me, this really hurts.

Record the important events that occurred that will help you interpret your findings (such as when the centrifuge tube cap flew off in the centrifuge and (Argh!) weird material collected in your cell pellet). Neatly write or tape data into your notebook. After careful thought, force yourself to write a conclusion: what went right, what went wrong, how does your hypothesis look now and what is the next step. Writing the conclusion is important — it is all too easy to fall into the trap of working hard without thinking hard. If you are going to be an independent scientist, you must do both.

There is an element of luck behind most great discoveries. Your luck will be proportional, however, to the number of well-conceived and expertly performed experiments that you execute and on how prepared your mind is to process unexpected findings. As famously attributed to Louis Pasteur, one of the greatest experimentalists of all time, "Dans les champs de l'observation, le hasard ne favorise que les esprits préparés" (in the fields of observation, chance favours only the prepared mind).

> Box 2 | **Conclusions are conditional**
>
> A mathematician, a biologist and a physicist are sitting in a street café watching people going in and coming out of the house on the other side of the street.
>
> First they see two people going into the house. Time passes. After a while, they notice three people coming out of the house.
>
> "The measurement wasn't accurate," says the physicist.
>
> "No, no," says the biologist, "they have reproduced."
>
> "I don't think so," says the mathematician. "If exactly one person now enters the house, then it will be empty again."
>
> This joke was posted on Profession jokes.

Interpreting experiments

Think big. Discoveries are not physical entities, but the products of cogitation. Making discoveries is the best part of science: it hooks you as a student and never lets you go. Some discoveries hit you like a frying pan and don't require a huge amount of thought. These are a real kick, so enjoy the initial glow because sooner or later doubts will tarnish your bright, shiny, discovery as you carefully consider its implications. Other discoveries are more subtle, at least given our mindset, which is hobbled by existing paradigms. To break

Box 3 | **Staying happy and sane in the laboratory**

- Your default opinion of others should be that they, like you, are sincere, well meaning individuals. Assume that dust-ups stem from an easily resolved misunderstanding. Wait before confronting. Most problems solve themselves in a few days. If not, patiently plan a conservative course of action. For advice, consult senior members of the laboratory or department. Involve your principal investigator (PI) only when absolutely necessary. Why? Whatever the issue, it will probably not reflect well on you, regardless of your innocence. Having the PI intervene will permanently mar your relationship with the other laboratory member and negatively affect laboratory esprit.
- Never write an emotional e-mail: have your confrontations on the phone or, better still, in person, as you will have the benefit of visual clues that will allow you to determine the effect of your words on your antagonist. Because of the imperfection of memory, spoken words (unlike written words) remain shrouded in the mists of uncertainty.
- Your career will be much easier if you develop a thick skin. You should embrace valid criticism, because it can improve your science and qualities as a scientist and a person. Of course, not all criticism is valid. With time you will develop a sense for legitimate criticism that needs to be addressed, and other criticism that should be ignored (with no malice to the source).
- If possible, avoid intra-laboratory romances, which typically lead to awkward break ups. Yes, only another scientist will truly understand you, but try to find your soul mate in another laboratory!
- Daily exercise will enhance your energy levels and improve your mood. No matter how brief or easy the workout, it is better than no workout at all. Vacations are essential to maintain your mental health and enthusiasm for science. Get as far away from laboratory life as possible and stay away from your e-mail!

the shackles of convention, the first thing you should do with fresh data is to come up with the most interesting possible interpretation of the results. This has several benefits. First, occasionally, you will actually be right. A surprising number of great discoveries were missed by previous investigators who made the same findings but never made the intellectual leap. Go to enough meetings and you will hear somebody lament "Oh, we saw that too, but didn't make anything of it". Second, even when the most interesting interpretation is wrong, thinking creatively will help you to place your findings in their proper context and will pay large dividends in designing and interpreting future experiments. Third, it is fun, particularly if it leads to brain storming with your mentor and other members of the research team.

Repetition trumps p values. Experiments have two general outcomes. Either they are interesting or they aren't. If they are interesting, you need to repeat them to the point where you are sure they are correct. It is far better to repeat a given phenomenon in a series of slightly imperfect experiments than to rely on a single experiment with perfect replicates that yield impeccable p values. Although statistics are important, don't be blinded by them — they are only as good as the assumptions they are based on. Statistically significant differences between samples only mean that something was different between the samples. The something might be the thing you were testing, or it might be something you didn't consider, like the temporal or spatial order in which you set up the samples.

Yes you can! You've done a superb experiment and your brilliant and subtle interpretation has led to an important discovery. This step actually trips up many young scientists, who lack the confidence to believe that their own two hands and brain could achieve such a thing. You need to get over this attitude immediately. Although oversized egos are as big a problem in science as in any profession, you need a healthy ego to be successful in science. You have got to believe that you have good ideas and can make an important contribution to your field (and don't fret, it's really true).

Embrace serendipity. What if your great discovery is not on the list of specific aims? Frequently, the best discoveries are serendipitous. Serendipity is easiest to embrace if it provides insight into your question of interest, but it often leads you into other fields. You should seriously consider pursuing these leads, but the final decision will have to be made by your PI. After all, it is your PI who is paying the bills. When you are a PI, these will be some of your more difficult scientific decisions. When you are in this position, remember that an excursion into a new field need not be permanent, but can be an exploratory expedition that may or may not lead to a permanent shift in direction.

Avoid the P-word. Without going off the philosophical deep end, it is useful to occasionally step away from the trenches of day-to-day research and contemplate the nature of discoveries. Observations are statistical phenomena that can be verified beyond a shadow of doubt. For example, a dead mouse is really and truly dead. By contrast, conclusions are the product of human thought based on an existing theoretical framework that is imposed on a system (that is, nature) that is inchoate and therefore essentially unknowable — for inspiration, see Huxley's translation of Goethe's view of nature (the system), which is the opening essay in the very first issue of *Nature* (the journal)[2]. Conclusions, therefore, are conditional; they are always wrong or incomplete in some manner, it's just a question of the degree to which they are incomplete (BOX 2). Do not fall into the all too common habit of stating that your findings 'prove' a given conclusion. They don't, and thinking this way closes your mind to alternative explanations and future discoveries.

Remember — science should be fun

Well, that's about it. Here's one last bit of advice — science is much more enjoyable and productive when it's fun (BOX 3). Maintain your sense of humour, particularly about yourself. Above all, pass on the joy of science to the next generation.

Now go and discover something that shocks everybody and makes your mother proud.

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doi:10:1038/nrm2390
Published online 10 April 2008

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Acknowledgements
The author is grateful to the many junior and senior scientists who shared their insights into scientific success. B. Dolan, K. Grebe, S. Hensley and J. Ishizuka made valuable suggestions for improvements to the manuscript.

FURTHER INFORMATION
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http://www3.niaid.nih.gov/labs/aboutlabs/lvd/cellBiologyAndViralImmunologySection/BenninkYewdell.htm
Profession Jokes:
http://www.workjoke.com/projoke22.htm
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TURNING POINT Biochemist's high-risk research direction pays off **p.131**

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COLUMN

Turbocharge your writing today

Before you can tackle the overwhelming task of huge writing projects, you must first put aside some widely held myths, say **Maria Gardiner** and **Hugh Kearns**.

As a graduate student, you might find yourself well on the way with your education and 'ABD' (all but dissertation). Day after day, you tell yourself that you really, really intend to start writing your paper. After all, you've collected all the data, analysed them many times and entered them into tables.

But then you start thinking that maybe you need just a few more data. Perhaps, too, you should try a different analysis technique. And what if the tables you used aren't the right ones, or need to be formatted differently?

Many of the thousands of researchers we have worked with are constantly being tripped up by finicky, niggling details that keep them from writing up their research. Every day, they mean to start, but every day, something gets in their way or seems more important — and this can go on for years. Some very common obstacles get in the way of high-quality, high-quantity scholarly writing, but powerful, evidence-based techniques can help researchers to overcome repetitive and unhelpful habits and get moving (see 'How to get out of a dissertation-writing rut').

WRITING MYTHS

The biggest impediments to scholarly writing are long-held myths that seem to get passed down through the academic ranks like precious but unhelpful ancient wisdom. The first is the Readiness Myth — "I should write when I feel ready, and I don't feel ready yet". The secret to high output is that you have to write before you feel ready, because you might never reach that point. Researchers read endlessly and conduct countless experiments in the belief that it will eventually make them feel ready to write — we call these habits readitis and experimentitis. But ironically, all that reading and experimenting often makes them less likely to write, and more confused. So the first way to speed up your writing is to stop waiting, stop reading and experimenting, and start writing. You won't feel ready, but you have to do it anyway.

This brings us to the second myth, the Clarity Myth — "I should get it all clear in my head first, and then write it down". This isn't how writing works in practice. You have probably had the experience in which you were sure about how a paper would go until you started to write it. Then you discovered that there were inconsistencies, or it didn't flow well or the links didn't make sense. This tells you that it wasn't all that coherent in your head, after all. In fact, writing clarifies your thinking. Writing is not recording — you don't just take ▶

EDUCATION

Better teaching needed

The United States must boost the number of people pursuing degrees and careers in science, technology, engineering and maths (STEM), says a 23 June report from the National Academies. The nation should foster better education in schools, said the report, *Successful K–12 STEM Education: Identifying Effective Approaches in Science, Technology, Engineering, and Mathematics*. The authors also recommend improving STEM literacy to fill STEM-related jobs that do not require advanced degrees, such as science teacher or energy technician. The US Bureau of Labor Statistics says that only 4 of the 16 STEM-related jobs with the largest projected growth by 2018 need an advanced degree.

JOB-HUNTING TOOLS

Inside information

An online forum aims to give job seekers inside information about employers. CareerBliss (www.careerbliss.com/company-questions) in Irvine, California, matches applicants with current employees who can answer queries. The forum has respondents for about 500 companies, universities and organizations in the United States including biopharmaceutical firms such as Pfizer and Genentech, says spokeswoman Alia Henson. Questions can be on any topic, including research funding or grant opportunities.

FAMILIES

Women want flexibility

Female early-career researchers with newborn babies are most likely to want to keep their jobs if their employers provide security and flexibility, including the right to leave work to care for an ill child, a study finds. Published on 23 May in the bi-monthly *Journal of Applied Psychology* (D. S. Carlson *et al. J. Appl. Psychol.* doi:10.1037/a0023964; 2011), the study reports better job retention for new mothers who stay physically and mentally healthy as a result of accommodations. Lead author Dawn Carlson, a professor of management at Baylor University in Waco, Texas, says that scientists should check how a prospective employer handles the needs of families before accepting an offer. To retain female staff, universities should allow maximum flexibility. "Whether extending the tenure clock or some other measure, the organization has to figure out a way to support these people if they want to reduce turnover," says Carlson.

▶ a photocopy of what is in your head and put it on the page. It is a far more creative and interactive process. As you write, you develop your thoughts. Writing is, in fact, rigorous thinking. So the second way to turbocharge your writing and improve its quality is to get the words down on the page — no matter how bad you think they look or sound at first.

SNACK WRITING

Once researchers get beyond the myths that stop them writing, they often declare that they can't possibly write anything eloquent, insightful or clever unless they have a whole day or week to do it in. And because they don't have that amount of time, they conclude that there is no point in starting. We call this 'binge writing'. Binge writing isn't inherently wrong; it's just that, for busy people, it can greatly reduce the amount of writing they do. The alternative is 'snack writing'. This means short — but regular — writing sessions. We suggest about 1–2 hours a day for graduate students who are writing a dissertation, and about 45–90 minutes a day for researchers trying to increase their publication output.

"Get the words down on the page — no matter how bad you think they look or sound at first."

Many researchers tell us that they couldn't possibly get anything useful written in that amount of time. The good news is that studies (which we have replicated many times in practice) show that academics who write for 30 minutes a day produce, on average, more peer-reviewed publications than academics who write for big blocks of time. But the 'snacks' have to be regular — 45 minutes once a week doesn't work, but 45 minutes a day 5 days a week does wonders. When possible, try snack writing first thing in the morning. Our experience suggests that this increases the chances of success by minimizing distractions and ensuring that you have sufficient energy to write clever things. However, for snack writing to lead to really high-quality results, you also need to write in a very specific way.

WHAT IS WRITING?

Before we tell you what writing is, we should tell you what it isn't, at least for the purposes of snack writing.

Writing isn't editing: you should not spend your brief snack-writing time trying to find the perfect word or getting your grammar right. Writing isn't reading journal articles for research: write first and read afterwards, so that your writing shows you what you need to read. Writing isn't referencing: when you make that killer argument and want to reference Smith and Brown (2006; or maybe it was 2007?), don't stop and look it up. Write "Smith & Brown (200??)" and keep going. You can look up the reference later. Furthermore, writing is not formatting, literature searching, photocopying, e-mailing or nosing around on Facebook. Writing — at least for your snack-writing sessions — means putting new words on the page or substantially rewriting existing words.

So, you might ask, when do you do all the editing, reading and other associated tasks? The answer is, any time in the other 23 hours and 15 minutes of the day — just not during your snack-writing time.

So stop waiting to feel ready. Get started with some short and regular writing snacks. What you write won't be perfect at first, but you will be on your way to becoming a prolific academic writer. ■

Maria Gardiner *and* **Hugh Kearns** *lecture and research in psychology at Flinders University in Adelaide, Australia, and run workshops for graduate students and advisers (see ithinkwell.com.au).*

TOP TIPS

How to get out of a dissertation-writing rut

- Write before you feel ready — because you might never feel ready. It's amazing how people magically feel ready when there is an imminent deadline.
- Don't wait to have a clear picture of the paper. As you start putting down your ideas, you may actually clarify them.
- Snack write — work in short, frequent bursts instead of waiting to sit down for big blocks of time. Those blocks hardly ever come, and when they do, they don't usually get used very productively.
- Set specific times in your schedule for writing — don't leave it to chance, because chances are it won't happen.
- Writing means putting new words on the page or substantially rewriting old words. It does not mean editing, reading, referencing or formatting — and it definitely does not mean composing e-mails.
- If you refrain from writing because you worry that what you write won't be good enough, try noting the adage that to write well, you first have to write.
- To really increase the quality and quantity of your writing, get feedback from mentors and colleagues — it can be painful, but it works. M.G. and H.K.

Jeff's view

How (not) to give a seminar

Sometimes I wonder how many seminars I have sat through. My brain tells me 'several thousand', but my gut says 'zillions'. Let's see: progress reports, journal clubs, faculty seminars, job seminars, The Harvey S. Benefactor Distinguished Lecturer Award, acceptance speeches for prizes, the list goes on and on. When advising large institutions I often heard forty or more seminars in a few long days. Yes, 'zillions' sounds about right. We spend an inordinate part of our life in seminars. And here is the bottom line: Most seminars are bad. *Real* bad.

Yet seminars *are* important. As a postdoctoral fellow, I published my results in the best possible journal and then thought of the next experiment. This habit must have died out in the late Paleolithic. Now the scientific literature is exploding and nobody even tries to keep up with it any more. Today you must go out and sell your stuff. To be at science's forefront, you must head for the storefront.

I would not even dream of telling you how to give a seminar. Three children, fifteen PhD students, and 84 postdocs have taught me that raising a finger is just as bad as raising your voice. My postdoctoral mentor left me a little wooden plaque that says, 'He who always agrees with you cannot be very bright'. Yes, it's sexist, but that's how they did things in those days. The plaque adorned my office and greatly impressed my students and postdocs. When I told them what to do, they thought of the plaque and did the opposite. That's how they discovered great things. So here is how (not) to give a seminar.

Let's start with the basics. Your seminar should not inform, but impress. And don't call it 'seminar', for God's sake. That word is a clunker. Today it's *Roadshow*.

As with any show, the title matters. It must be flashy and get the adrenalin flowing. '*Signal transduction in the inflammatory response*' is precise, scholarly – and, well, scholarly. '*TNF R1, RIP, TRAF2 and FADD in NF-kappa B activation*' is more like it. 'This guy is hot stuff, a real deep thinker' your colleagues will suspect, and flock to your lecture. A hip title is also OK: '*Sex, drugs and yeast mass mating*' should catch their attention in Europe and at most major centers in the US, but do check things out before you speak at the Pontifical Academy in Rome or in the US Bible Belt.

Don't bother with introductions. General background, biological significance, earlier work by others – that's for the birds. The presence is now, so get right down to business. The opener 'When Jack, Mary and I did Westerns with RIP monoclonals, it was *me* who noticed some strange bands' will immediately grab their attention. Showing these bands on screen will also let you kill the room lights early on and then keep them off for the rest of your talk. Let your listeners relax, particularly if your seminar is right after lunch. There is nothing wrong with an innocent postprandial nap.

There are still people out there who project glass-mounted slides – through things called *projectors*! Ughh! Today you *beam Powerpoints*. Don't check out the electronics beforehand – do it while you speak. They never work right away, so you can show how great you are with computers. While you take your time fiddling with the knobs, your audience can enjoy the Microsoft® logo on screen and Bill Gates gets a little free publicity. Even he deserves a break once in a while.

Your hosts have paid through the nose for their high-resolution beamer, so you owe it to them to squeeze the last little pixel out of it. When slides ruled the earth, a diagram's complexity was limited by the skill and the patience of the draftspeople. But now we are talking twenty-first century, and the sky is the limit. Fill the screen with all you got – preferably raw data straight out of your lab notebook. Let the audience feel the pulse of discovery. There used to be a rule that said: 'No more than one slide every two minutes'. Baloney! Today's generation was reared on TV and video games and is hooked on images. So keep those pictures coming.

Ages ago, lecturers used wooden sticks to point to things on the screen. They don't sell such contraptions any more, because everyone is into lasers. *Star Wars* stuff. They are cool gadgets, so use them. Keep them on, and keep them moving back and forth until the heads of your audience make you think of a tennis match. If the battery dies, keep on pointing. This will keep your listeners alert, because they must now search for a dot they cannot see on an image they do not understand.

Don't ever look into the audience. Keep your eyes on the action – the screen. If it happens to be blank, your lecture notes will also do. Once a friend of mine did look into the audience and saw so many people dozing that he was marked for life and never lectured again.

Never talk without lecture notes. Leave that to actors, politicians, and other frivolous folk. You are a scientist, an *intellectual*. So act like one and read your talk in the time-honored meter of scholarship – the monotone. If you cannot do without some spontaneity, follow this simple three-step protocol: (a) don't staple the pages of your notes together; (b) drop them on the way to the podium; (c) use them the way you picked them up. Your talk will be remembered for its startling connections, sure signs of a creative mind.

Your talk should focus on a single point – YOU. Nobody expects you to be a talking edition of Annual Review of Biochemistry. All those great ideas – you had them first. It was you who foisted them on your unbelieving collaborators who then did the obvious experiments. If you cannot avoid mentioning ideas of others, explain why they are wrong. Your talk can be elliptical, as long as you occupy both focal points. It wouldn't hurt to throw in a little chauvinism. Competitors from your own country always have full names. Competitors from elsewhere can be taken care of by collective epithets such as 'a couple of Japanese' or 'a bunch of Dutchmen'. If you are British, 'Work by Sir X at Oxford and by some Europeans' will please those from Great Albion. If you are American, refer to most others as 'people from overseas'. And if you are privileged to work in California, it's simply 'The Coast'. We all know there is no other one.

Stay away from simple language. Simple words spell simple

doi:10.1016/S0014-5793(02)03786-9

minds. Even the international language of science, Bad English, loves New Speak. No wonder, the two are close cousins. You never *read journals*; you *keep abreast of the literature*. You don't *do good science*; you are *at its cutting edge*. Your postdocs are not simply *good*; they are *the brightest and the best*. You never *work hard*; you *seek aggressively*. And experiments are never *unfinished, inconclusive* or *a failure*, but *in press*.

Half-way through the talk, your time is usually up. Now is the moment to think of a scientist's three most important goals: (a) the Nobel Prize, (b) unlimited research funds, and (c) unlimited speaking time. To get (a) and (b), you must have brains; to get (c), you must have guts. So don't skip anything – say it faster. Give the audience a rousing coda – they know that the coda is always the fastest part of a piece. No matter how much longer you still want to go on, keep saying 'now, in closing' or 'in these last few diagrams'. That's a great way to keep people from leaving.

When you have finished, do not summarize what you have said. Who wants to hear things twice? Get ready for the discussion, because that's where things might get tricky. Without those beamed diagrams you are left out in the cold. And some listeners may turn cranky, because the lights wake up the old geezers in the front row, and switching off the beamer sends the young ones into Acute Visual Deprivation Shock. It's wartime. Take every question as an excuse to continue your talk. Don't answer to the point, and make discussants feel guilty for their inane question. If you are cornered, don't say 'I do not know' or 'you are right', but tell them that your many *papers in press* will answer everything. And if you cannot be right, be wrong at the top of your voice.

On leaving the lecture room, face the usual hand-shakes and small talk in good humor. This can be quite a challenge, particularly if you remember faces as badly as I do. If somebody who looks vaguely familiar traps you with outstretched arms and a familiar grin, try a generic opener such as 'How was the trip?' That's a safe one, because biologists get around. Stay away from inquiring about the spouse; as I just said, biologists get around. Relax and enjoy your drink while your intimate stranger gives you his horror story about the canceled flight. But then it is time to go. Do not stay for the official Dean's Reception and the dinner. Mumble something about next day's lecture at a famous place, and head for the airport. Being a hit as a lecturer gets your career off the ground; a hit – and – run lecturer has arrived. Besides, you can get home early and write those *papers in press.* Try to keep *abreast of the literature*.

Thanks to my friend Stuart J. Edelstein for his comments.

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The importance of stupidity in scientific research

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Accepted 9 April 2008
Journal of Cell Science 121, 1771 Published by The Company of Biologists 2008
doi:10.1242/jcs.033340

I recently saw an old friend for the first time in many years. We had been Ph.D. students at the same time, both studying science, although in different areas. She later dropped out of graduate school, went to Harvard Law School and is now a senior lawyer for a major environmental organization. At some point, the conversation turned to why she had left graduate school. To my utter astonishment, she said it was because it made her feel stupid. After a couple of years of feeling stupid every day, she was ready to do something else.

I had thought of her as one of the brightest people I knew and her subsequent career supports that view. What she said bothered me. I kept thinking about it; sometime the next day, it hit me. Science makes me feel stupid too. It's just that I've gotten used to it. So used to it, in fact, that I actively seek out new opportunities to feel stupid. I wouldn't know what to do without that feeling. I even think it's supposed to be this way. Let me explain.

For almost all of us, one of the reasons that we liked science in high school and college is that we were good at it. That can't be the only reason – fascination with understanding the physical world and an emotional need to discover new things has to enter into it too. But high-school and college science means taking courses, and doing well in courses means getting the right answers on tests. If you know those answers, you do well and get to feel smart.

A Ph.D., in which you have to do a research project, is a whole different thing. For me, it was a daunting task. How could I possibly frame the questions that would lead to significant discoveries; design and interpret an experiment so that the conclusions were absolutely convincing; foresee difficulties and see ways around them, or, failing that, solve them when they occurred? My Ph.D. project was somewhat interdisciplinary and, for a while, whenever I ran into a problem, I pestered the faculty in my department who were experts in the various disciplines that I needed. I remember the day when Henry Taube (who won the Nobel Prize two years later) told me he didn't know how to solve the problem I was having in his area. I was a third-year graduate student and I figured that Taube knew about 1000 times more than I did (conservative estimate). If he didn't have the answer, nobody did.

That's when it hit me: nobody did. That's why it was a research problem. And being *my* research problem, it was up to me to solve. Once I faced that fact, I solved the problem in a couple of days. (It wasn't really very hard; I just had to try a few things.) The crucial lesson was that the scope of things I didn't know wasn't merely vast; it was, for all practical purposes, infinite. That realization, instead of being discouraging, was liberating. If our ignorance is infinite, the only possible course of action is to muddle through as best we can.

I'd like to suggest that our Ph.D. programs often do students a disservice in two ways. First, I don't think students are made to understand how hard it is to do research. And how very, very hard it is to do important research. It's a lot harder than taking even very demanding courses. What makes it difficult is that research is immersion in the unknown. We just don't know what we're doing. We can't be sure whether we're asking the right question or doing the right experiment until we get the answer or the result. Admittedly, science is made harder by competition for grants and space in top journals. But apart from all of that, doing significant research is intrinsically hard and changing departmental, institutional or national policies will not succeed in lessening its intrinsic difficulty.

Second, we don't do a good enough job of teaching our students how to be productively stupid – that is, if we don't feel stupid it means we're not really trying. I'm not talking about 'relative stupidity', in which the other students in the class actually read the material, think about it and ace the exam, whereas you don't. I'm also not talking about bright people who might be working in areas that don't match their talents. Science involves confronting our 'absolute stupidity'. That kind of stupidity is an existential fact, inherent in our efforts to push our way into the unknown. Preliminary and thesis exams have the right idea when the faculty committee pushes until the student starts getting the answers wrong or gives up and says, 'I don't know'. The point of the exam isn't to see if the student gets all the answers right. If they do, it's the faculty who failed the exam. The point is to identify the student's weaknesses, partly to see where they need to invest some effort and partly to see whether the student's knowledge fails at a sufficiently high level that they are ready to take on a research project.

Productive stupidity means being ignorant by choice. Focusing on important questions puts us in the awkward position of being ignorant. One of the beautiful things about science is that it allows us to bumble along, getting it wrong time after time, and feel perfectly fine as long as we learn something each time. No doubt, this can be difficult for students who are accustomed to getting the answers right. No doubt, reasonable levels of confidence and emotional resilience help, but I think scientific education might do more to ease what is a very big transition: from learning what other people once discovered to making your own discoveries. The more comfortable we become with being stupid, the deeper we will wade into the unknown and the more likely we are to make big discoveries.

(Chris Comer, of the Texas Education Agency) who are hounded out of their jobs because of their support of evolution in school curricula, and have initial nods toward sanctioning the Institute for Creation Research (now located in Dallas) by the Texas Higher Education Coordinating Board, the same state agency that certifies the school I work at, the University of Texas at Austin. That said, I recently was invited to serve on a panel to review the ICR's graduate program, and I was extremely impressed by the professionalism and commitment of the other educators that were invited and by the staff of the agency. This wasn't exactly a revelation, but it was greatly reassuring. I was once privileged to hear Stephen Gould speak on his experiences with court cases involving Creationism, and he talked about sitting down and drinking lemonade with people who disagreed with him, and how they were all quite civil about their disagreements. I think my experiences are somewhat similar, in that while both sides are quite passionate about their interests, dealing with the people involved, the civil network we're all part of, makes it somewhat easier to put the disagreements in perspective.

Well, I'm glad you remain low key about these issues. Are you always so neutral? I would say I'm an equal opportunity curmudgeon. I also find the attitudes of many of my own colleagues to be moderately bewildering (and vice versa). In particular, while we like to talk about how biology is the study of life, we actually have no decent scientific definition as to what life is. In recent years, I've come to believe that this is because there is no such thing, that the term 'life' is more useful to poets than to scientists. We classify a large set of replicators as 'life' based on our experience. In so doing, we also assume that the classification has a fundamental meaning in and of itself, beyond its utility. This is the problem. We tacitly assume the very same notions that the lay public does in talking about life. In my view, many biologists are closet vitalists.

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My word

Women in science — passion and prejudice

Christiane Nüsslein-Volhard

Scientific research requires special talents, just as much as intelligence, passion and diligence. I do not know a single successful scientist who is really lazy, and only very few who are able to pursue at the same time other interests with intensity and success. Reaching a leading position in scientific research is very demanding and requires early independence and perseverance. These truths universally acknowledged hold for both men and women. However, measured by their scientific potential, women, whose intelligence is fortunately no longer disputed, were and still are underrepresented in science, in particular in terms of professorships or leading research positions.

I love being a researcher: it is a great pleasure to discover new things about life, to be able to run a large lab and to support talented young people in their careers. I used to work long hours in the lab while pursuing my own ideas and observations, but I also have come to enjoy having some power, being involved in decisions in scientific organisations or as an advisor in science policy. I am convinced that I would be unhappy without my science. Therefore, I often think about women of similar passion and personality, but facing circumstances that make it extremely hard or impossible to be successful as a scientist. Where are the problems, what can be done to solve them?

Presently, there is general consensus that efforts should be made to increase female contribution to modern science, not least because our society cannot afford to lose so many highly trained talents. After all, not all the males in leading positions are better than all the females in non-leading positions. In Germany, for instance, only about 11% of full professors are women. In the Max-Planck-Society, the leading German research institution, the fraction of female directors is even smaller, about 7%. When I was elected as a scientific member and director of the Max-Planck society, I was one of only two women, and the only one in natural sciences. Ten years later, in 1995, the society was able to boast that 25% of their female directors had received a Nobel prize. Now, there are 19 female Max-Planck directors among a total of 266. Life as an exception, as a role model has not always been particularly comfortable, but with an increasing number of female colleagues and a general awareness of gender issues, open discrimination is now rarely encountered as a serious problem. It has not always been like that. In my early days, as representative of a small minority, I felt quite awkward, unprotected and often overlooked.

I grew up in Frankfurt in a liberal family. With my family I shared a cultural interest in arts and music, whereas my early passion for animals and plants was not shared by the others. It was nevertheless much supported by my parents, who allowed me to keep pets and bought the right books for me. At the age of twelve or so I knew that I wanted to become a biologist. I went to an excellent girls' high school with devoted teachers and a focus on science. At this school, I never had the feeling of not being taken seriously in my attempts at understanding science; moreover, gender differences and competition with males weren't an issue at that time. Such single sex schools hardly exist anymore, which is probably a mistake as for me this environment was very important and provided a strong support for my early determination to pursue a scientific career. Also, later as a university student, I do not remember having encountered gender problems, and as an ambitious and enthusiastic graduate student I felt generally well respected and appreciated.

My first significant experience with discrimination as a woman in science came while publishing the results of my thesis: The project had been started by a rather fortuneless male graduate student and I had finished it producing all of the data. However, on the three-author letter to Nature, which I had written, I was made only second author. The graduate student, a good friend of mine, had a family — "he needs his career" was the comforting explanation. At the time, however, curiously enough, I even agreed to this! Such things as social considerations exerting an influence in assessing scientific contributions probably do not or at least should not happen any more.

I first encountered open prejudice as a postdoc: My supervisor had the attitude of giving women a chance, but at the same time was expecting them to fail. This made me very angry. It was no fun to work under a boss who openly declared that women in principle cannot do great science — "there is no female Einstein" — but could excel in other professions, such as pottery. At the same time, this made me even more determined to 'show them'. My boss was glad when I moved on, and so was I. At the EMBL in Heidelberg, I was offered a group leader position, but only after it was clear that a younger male colleague would share the lab with me. A woman alone would have not been entrusted with her own lab.

This, however, turned out well, because the male colleague with whom for three years I shared a discussion microscope and a tiny laboratory was Eric Wieschaus. The fact that we were thrown together to work with one technician made us embark on a fantastically interesting and challenging project which fifteen years later won us the Nobel prize.

When I was appointed a director at the Max-Planck Society in 1984, I regarded this as a great success, until I found out that never before or after had a new director got as little funding and space as I had. But soon fate changed: Owing to very good working conditions and excellent students and postdocs my lab was very successful. Recognition came, which encouraged me to ask the president for an upgrade, and finally I was granted what my male colleagues had received without special merits.

Looking around now, I think the situation for women in science has changed considerably, and the types of open discrimination I experienced are becoming rare. By contrast, in many countries enormous political pressure is being put on universities and research institutions to increase the fraction of female scientists in high level positions — even though some disciplines, such as chemistry and physics, do not seem to attract many women. This raises the question of what the aims of the policy towards women in science should be. Should there be equality in all respects? Should 50% of all high level positions in all fields be filled with women? Is this aim reasonable, and if so, how can we approach it?

I confess that I do not think that this particular aim is reasonable. I have observed that while many women may admire me for my success, they admit that they "would not want my job". Men and women are different by nature, not only because of their education or the roles traditionally ascribed to them by society. Of course, I do not think that women are in any way less intelligent than men or do not have the capacity to do excellent science in principle. It is not a matter of skills or talent, but according to my observations the strengths, aims and interests of women differ from those of many of their male contemporaries, at least on average. I know many women who share my disgust for the personal pride, vanity and narrow focus of some successful male colleagues and in turn appreciate the more considerate, broad-minded way some female colleagues do their science. I understand women who hate to push themselves forward, or who are not willing to narrow down their spectrum of interests, including family and friends. I have often experienced that women in my family — much more so than men — have a hard time understanding my passion for science, while they are more interested in social issues, art and music.

Finally, for many women, a leading position is simply not attractive, because it means directing other people's activities and involves the necessity to exert power, which includes making unpopular decisions. This, in a nutshell, is what leadership means in science: acquiring the power to let other people work for you to support your individual scientific projects, and not those of a supervisor. In many universities and research institutes in Europe, the only independent positions are leading positions, associated with considerable resources and administrative tasks. Lean independent research positions with few responsibilities outside the running of the research project, which might be more attractive for many women, are rare — or reserved for cases with dual career problems.

Personally, I have pursued broad interests while at school and as a student, but necessarily had to focus considerably during my scientific life. I have no family, which helps avoid a lot of possible conflicts of interest. In my scientific career I have been fortunate and more successful than one is entitled to expect. Nevertheless, not all women trained as scientists would like to be in my position. This has to be respected. However, it is obvious that in our society many gifted women with great potential and ambition do not succeed at a career in science because of a complex set of unfortunate circumstances.

I have already mentioned several obvious discriminatory situations hoping that they belong to the past. Most important of all, the lack of confidence and trust by supervisors or deans of faculty, as I have experienced it, can be very inhibitory. At the same time, I am convinced that care must be taken to not shield women from just and fair criticism — the kind of pressure and challenge that every scientist needs in order to successfully develop her or his career. Well intended protection, which also often means taking away important opportunities to build up your profile, can be as harmful as open hostility. A good rule of practice is to mentally go through a given case and ask if the same expectations and questions would also be applicable to a male scientist.

Frequently, it is the women themselves who lack confidence and are too timid and modest. Also, women often present themselves less convincingly than their male colleagues with equal qualifications. Many men are unable to recognise scientific talent in the disguise of a female phenotype. I have often experienced that women do not have as much of a problem admitting they made a mistake, but this is often held against them. Mistakes and failures are tolerated less than those of male colleagues, who are shielded by a network of loyalty in which women often are not included. Although this probably reflects a minority issue rather than a gender issue, it may affect all women as they are 'tainted by association'. On the other hand, women displaying attributes that are generally regarded as more masculine, such as a loud voice, dominant, aggressive behavior and an open display of self-confidence are also not appreciated in our society. In addition, a woman singled out as a successful scientist is often sensed as a threat, and awe-inspiring by her contemporaries, both male and female. In our society, features of attractive women traditionally concern beauty or social skills rather than intellectual achievements. In retrospect, I realise that I intuitively shielded my success from my colleagues and friends as much as possible in order to avoid provoking them. It has to be considered that for many men it is much harder to accept the superiority of a female than that of a male colleague.

Career problems that arise when both partners are doing science, such as restrictions in mobility or the difficulty to find equally attractive job opportunities, often affect women more severely than men and frequently lead to the woman working for her male partner. Although this might be suitable in many cases, for the woman it often means giving up an independent career. The problem to combine a family life with a high-level career affects mainly women. Even if the husband does his share of household tasks, the woman will bear the children and will generally be more involved in their care. As a consequence, many women scientists decide not to have children. In other cases, they adopt less ambitious and more dependent positions, often after desperate attempts to combine doing science and having a family. However, positions in science administration, writing or industry, even if well paid and interesting, often provide a painful and difficult compromise for a passionate scientist. Therefore, in our societies we should do all we can to enable talented and ambitious women scientists to pursue a successful, independent scientific career even with a family. The prejudice of some male scientists against women collaborators with children probably is because they simply cannot imagine how they themselves would have made a career without the steady support of their wives. This is why some successful women hide the fact that they have children. However, ample examples of great woman scientists who have managed to combine family with a successful career have demonstrated that this is possible, provided support and fortunate circumstances.

There are a number of characteristic 'career traps' for women both with and without children: Some women take long maternity leaves and often return on part-time positions. This frequently ends in a 'drop-out' from an independent career in science; in the meantime, the interesting projects may have been taken over by others, because they would take much longer, causing difficulties for lab mates and supervisors. It is very difficult to catch-up lost time, and new investments are required to update the qualification and produce scientific discoveries enabling a career step. Talent, skills and qualifications do not automatically guarantee a scientific career, but to do so, they must lead to the production of some original scientific contributions in the form of publications. This does take time and energy, there is no way out. Concessions may be made to women with children with respect to their age, but not with respect to the quality and impact of their publications.

Women sometimes have great emotional difficulty to hand over parts of the education and caretaking of their children to other people, even if these are professionals. In many European countries, the society's influence leads to the mothers suffering from the situation much more than necessary, causing bad conscience that they do not spend enough time with their children. Provided the day care is of high quality, however, most children actually do enjoy it, and in the company of other children they may get in fact an excellent education. For instance, the campus at my institute hosts a day-care center supported by the Max-Planck-Society, which provides ideal solutions for mothers and small children.

Some women — especially those who have grown up in Austria, Switzerland or Germany — even refuse to accept domestic help in their household. Women scientists should not hesitate to ask for (and pay for) any possible support in household chores to gain time to spend with family or in the lab, rather than having to do laundry. In particular, for women with children household support will be immensely helpful. Obviously, for women at the beginning of their career such help is too costly. To overcome this problem, I am running a foundation (www.cnv-stiftung.de) together with my colleague Maria Leptin, which supports talented young women with children with individual grants for household help. We are still at the beginning, but our first impressions are positive, not the least because of the encouragement and moral support we can give these women.

One other problem concerning women more than men is their readiness to perform what others request of them in terms of organisational matters in their institutions. In addition, because women still represent a minority in science, they tend to be overwhelmed with proposals for memberships in committees, panels and other professional tasks. Too many such duties can easily ruin a promising career. Women must say no to such requests more frequently than men, and they have to endure not being always loved for this. Men should become more aware of gender issues, which would render the obligatory female participant as an observer in commissions unnecessary.

It is probably safe to say that the prospects for woman scientists were never better than they are now, but we are not yet at a stage where women have the same opportunities as men to turn their passion for science into a successful career. I hope that all the efforts that are underway will soon lead to a situation that the topic of women in leading positions in science is no longer an issue that needs to be discussed constantly.

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Marine Pollution Bulletin 50 (2005) 1457–1458
MARINE POLLUTION BULLETIN
www.elsevier.com/locate/marpolbul

Editorial

The "So What?" factor

The "So What?" factor is, quite simply, the most important test that any scientist can put his or her work to, before starting, during execution, and following completion prior to presentation and/or publication. It is the ultimate judge of the worthiness of any scientific (or other) endeavour, and is too often failed by the studies that are currently being presented at conferences and/or published in the peer reviewed literature.

This Editorial originated with a conversation at CICTA Iberoamerican Congress of Environmental Contamination and Toxicology 2005 in Cadiz, Spain (25–28 September, 2005). One of us (PMC) is an older researcher, who was a keynote speaker at that conference. The other (LMG) is a student, completing his Bachelor's degree and attending the conference to learn as much as possible.

The conversation began with some simple questions regarding biomarkers, measurements of whole organisms or specific tissues, and the validity of studies whose overall applicability and purpose were not immediately obvious. By this time the conversation was no longer simple; it was touching on key issues regarding why we do science and what we should and should not do when doing science. It now dealt with the "So What?" question.

Towards the end of the conversation the student asked the older researcher if he had ever published guidance to students and researchers regarding the "So What?" question. He had not, but it was a wonderful idea for an editorial, and an even better idea was for a joint editorial, comprising two key points of view—a student beginning his career, and an older researcher with more years behind than ahead in his career.

Hence this Editorial, which provides the two points of view, and then provides joint advice to researchers of all ages and level of experience. We begin with the viewpoint of the older researcher and continue with that of the student, finishing with our joint advice to any and all researchers.

I (PMC) have noted too often that researchers' reports, posters, presentations, and publications fail to follow what we were taught in University: set up testable hypotheses; attempt to falsify them; then report the results in terms of those hypotheses. I have also noted that too many studies are, to say the least, not as useful as they could/should be. Specifically, there are too many studies that might as well not have been done: for instance, testing the toxicity of a chemical to yet another organism without any plan other than to conduct and report such testing; developing a new index of some sort where there are already too many indices of doubtful utility; applying established tools to yet another location with nothing really new such that the study may be of local interest but is hardly of global interest (yet it may still be published in an international journal). When the "So What?" test is applied to such studies there is no clear answer as to why they were done, what overall purpose they serve, nor how they fit into what should be our main focus as environmental scientists: determining pollution; assessing pollution; and providing decision-makers with the necessary information to address pollution that is adversely affecting the environment in which we live.

In the beginning of a work/project I (LMG) usually have a question in my mind: what is the purpose of this work/project? Sometimes, when I don't understand or don't have the answer, the work/project does not make sense to me. So I ask myself: regarding this situation what should I do? Write the work even though I don't understand its purpose or not write it? This was one of the points of my conversation with PMC, to whom I referred my doubts about the validity of a work of mine: I didn't achieve my initial objectives, which were making it a good environmental managing tool. Should I publish it? It's a fact that I need to improve my résumé, but if it is not a good work it is of no use for my personal satisfaction and reputation neither for the overall scientific work. His answer was "only a So What? test can solve it". At this point John Lennon's words came to my mind: "There's something else I'm going to do, only I don't know what it is, but I do know this isn't for me" (The Daily Telegraph, Wednesday, 5 October, 2005, p. 23).

Based on our joint experience, we propose that both new and not-so-new researchers heed the advice given to Alice

0025-326X/$ - see front matter
doi:10.1016/j.marpolbul.2005.10.010

by the Cheshire Cat in the children's book "Alice In Wonderland", written by Lewis Carroll. Those with children will recall that Alice asked the Cat "Would you tell me please which way I ought to go from here?", and the Cat, sitting on a branch just above her and slowly disappearing except for its grin, replied "That depends a good deal on where you want to go".

As scientists, we need to choose where we go by, in order:

1. Beginning with one or more questions.
2. Ensuring that those questions are worthwhile and address pollution issues in a "big-picture" context—be sure of why you want to answer those questions, and that the time and money (usually not your own) spent will be well spent. In this regard, ask yourself why anyone would be interested in the answers to the questions you are asking, and how many would really be interested/benefit from this information. Be honest with yourself. Answer the "So What?" question.
3. Setting up testable hypotheses based on those questions. Be prepared to find those hypotheses falsified and to obtain results that are not what you expected and that may not even make sense. This is far from an atypical situation in science—we often find that the answers we get do not match the question we asked, and now we have to determine the question(s) being answered. Finding those answers can provide more useful information than the questions themselves—particularly if the answer turns out to be "42" (Douglas Adams, The Hitchhiker's Guide to the Galaxy, The Ballantine Publishing Group, June 1997).
4. Conducting the study, and being flexible as you proceed, relative to item 3, above. Continue asking yourself the "So What?" question.
5. Writing up the study, presenting and/or publishing it. Ensure you again answer the "So What?" question, and again answer it honestly. Based on the answer to that question, make a final decision regarding presentation or publication. Remember, it is better not to present or publish a study that will not add to your reputation and may even detract from it, than to proceed irregardless.
6. Being proud of yourself for following all of these steps and truly contributing to the environmental sciences and thus, in a small but meaningful way, to the well-being of our planet.

The "So What?" question is also known as the "Laugh Test" and, as such, has been applied in a variety of human endeavours, ranging from aircraft design to political campaigns. It is the ultimate test to which we should apply both our work and ourselves. To do less is to betray both ourselves and our profession.

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