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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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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.

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