How To Have a Career in Pasteur's Quadrant

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Context

I closed the book with a chapter on career advice, in particular how to have a career in which one is motivated by important applied problems and simultaneously seeks fundamental understanding. Donald Stokes (Stokes, 1997) called this Pasteur's Quadrant (explained below). The editor at Princeton University Press suggested that even if Princeton were to publish the book, I remove this chapter and publish it as a stand alone paper; this is what got me thinking about making all the book into stand alone papers rather than trying to rewrite. For obvious reasons, I have written the chapter with ecologists in mind, and the references are heavy with applications of State Dependent Life History Theory (SDLHT) implemented by Stochastic Dynamic Programming (SDP).

The topics covered are these:

- An Epigram from Phil Levin.

- Introduction (a description of Pasteur's Quadrant and examples of the use of SDLHT in Pasteur's Quadrant).
- Working Across Disciplinary Boundaries.
- Professional Preparation (undergraduate, graduate, post-doctoral studies; working hours and work life balance; finding mentors at every point your career).
- Communicating Science (writing and speaking from the short talk to the job interview; using practice to improve your speaking skills).
- The Far Beyond (The impostor syndrome, the importance of stupidity in science, and post-decision regret; academic or non-academic positions and leaving and returning to academia, becoming part of a (not too big) research group, and ethical issues in funding).

Epigram from Phil Levin

Phil Levin's reconstruction of a poster he did on Louis Pasteur in elementary school (ca.1972; see http://www.washingtonnature.org/fieldnotes/2017-science-what-inspires-me):

> *Louis Pasteur discovered that the growth of microbes was responsible for spoiling milk*
> ***And***
> *he invented a process of heating milk to kill the microbes and prevent it from spoiling.*
>
> *Louis Pasteur discovered that a disease caused by microbes was killing silkworms*
> ***And***
> *he developed a process to eliminate the microbes, protect the silkworms and ultimately save the French silk business.*
>
> *Louis Pasteur discovered that he could artificially weaken disease-causing microbes*
> ***And***
> *he created some of the earliest vaccines, exposing people to the weakened microbe to foster immunity to the stronger form.*

Introduction

Levin continued "It was the 'ands' that stuck with me. Some of Pasteur's peers were solely on a quest for fundamental understanding. Others, were less interested in knowledge for knowledge's sake – but rather sought to practically apply existing knowledge. Pasteur was different. He pushed the frontiers of knowledge, but did so because he saw a real-world need. Pasteur sought to uncover nature's secrets - and use this wisdom to improve the human condition...We conservation scientists must follow in Pasteur's footsteps – steadfast in our learning and resolute in its application. By conducting such use-inspired research, our scientists have the best hope of developing novel, practical, applicable and scalable solutions to the wicked problems our ecosystems face." Most of this chapter is about how (rather than why) to have a career in Pasteur's Quadrant.

Ludwig et al (2001) address the why: "More than any previous generation, the current generation of ecologists is concerned about applications of their work to society's problems. Since the first Earth Day about 30 years ago there has been a change in the public perception of environmental problems and a corresponding growth in the sensitivity of ecologists to issues of public policy" and reiterated the question asked by Roszak in 1972: "Which will ecology be: the last of the old sciences or the first of the new?" (Roszak 1972, pg 404). We'd like the answer to be that ecology is the first of the new sciences.

Since 1980 – that is, since I began my academic career – ecologists and evolutionary biologists working in universities slowly but inexorably shifted from disdaining work that had application to seeking it. In part this is due to changed funding opportunities, but it is also due to the demands of our students that work be both scientifically first rate and important.

Simply put: we must do science that matters and science that matters will be motivated by an important applied problem and will cross disciplinary boundaries. Today, of course, everyone – including funders – is talking about work that spans disciplinary boundaries; indeed *Nature* had an entire special feature on interdisciplinary work in 2015 (e.g. Brown et al 2015, Ledord 2015, Pettitt 2015, Rylance 2015, van Noorden 2015).

Pasteur's Quadrant

In his book *Pastuer's Quadrant* Donald Stokes (1997) argued that a single axis between basic and applied science is the wrong way to think about things. Rather, one must focus attention in a plane. One axis of assessment of work is whether it is motivated by a consideration of use and the other is whether there is a quest for fundamental understanding. The plane can then be divided into the four quadrants (Figure 1) 1) No consideration of use and quest for fundamental understanding; 2) Consideration of use and a quest for fundamental understanding; 3) Consideration of use and no quest for fundamental understanding; and 4) No consideration of use and no quest for fundamental understanding.

Stokes called the quadrant 'not motivated by consideration of use and search for funda-

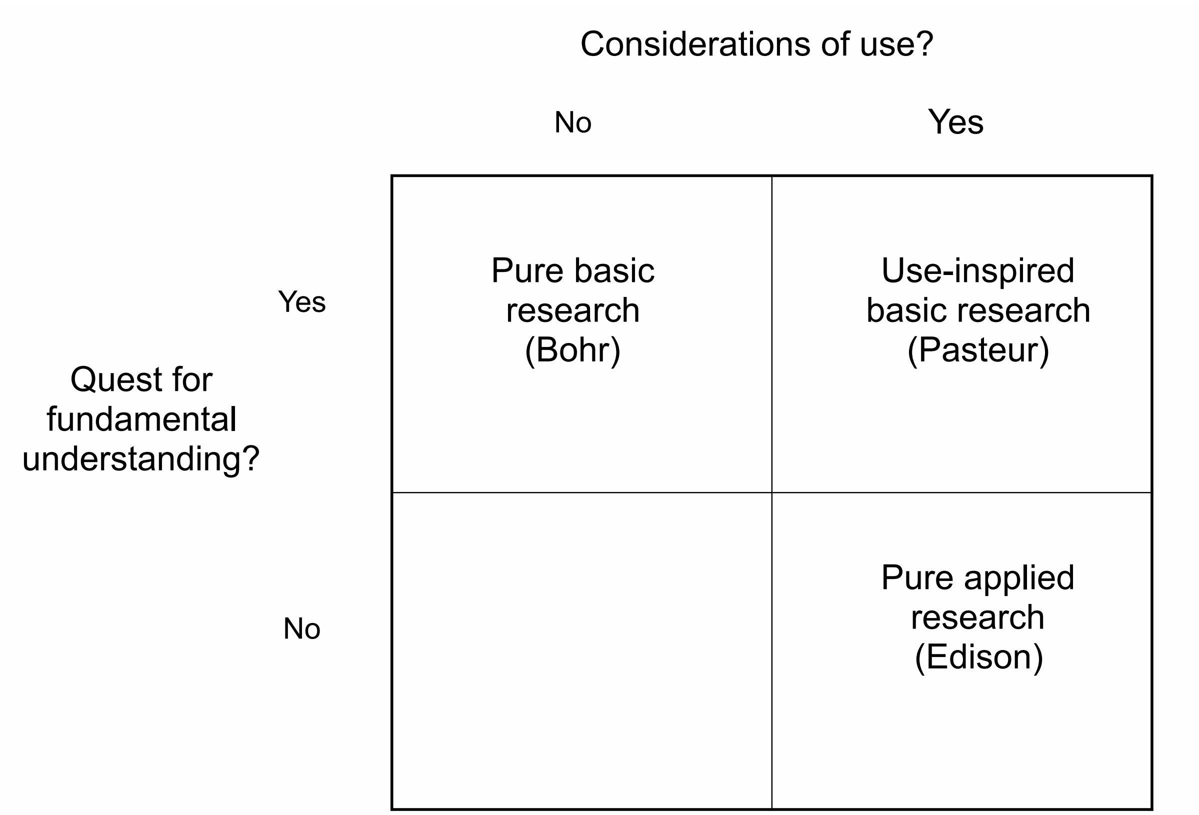

Figure 1: Stokes's (1997) vision of science is that the dichotomy between basic and applied science is a false one. Rather in every scientific endeavor we may ask about application – is there consideration of use motivating this work – and whether or not there is a quest for fundamental understanding. Niels Bohr provides the canonical example of an individual whose work was not motivated by consideration of use but involved the deep search for fundamental understanding and Thomas Edison one whose work was motivated by consideration of use but did not search for fundamental understanding. Louis Pasteur – whose work from the time of his PhD was motivated by an important applied problem (Debré 1994) while he simultaneously sought fundamental understanding in his work.

mental understanding' Bohr's Quadrant, the one 'motivated by consideration of use and no search for fundamental understanding' Edison's Quadrant and the quadrant 'motivated by consideration of use and search for fundamental understanding' Pasteur's Quadrant since Pasteur's entire career involved work that was motivated by an important applied problem but simultaneously sought fundamental understanding (Debré 1994). Pasteur himself said "There is no such thing as a special category of science called applied science; there is science and there are its applications, which are related to one another as the fruit is related to the tree that has borne it (Debré 1994)".

There are already many examples of the use of SDP models for work in Pasteur's Quadrant. In Table 10.1, I show a smattering of papers more or less randomly chosen to illustrate some of the applications (also see Chapters 7 (on discarding in fisheries), 8 (on conservation biology), and 9 (on agroecology) in Clark and Mangel (2000)).

Table 10.1. Some Examples of the Use of State Dependent Behavioral or Life History Theory in Pasteur's Quadrant Since 2000

Authors	What They Did
Babcock and Pikitch (2000)	Modeled the targeting decisions of bottom trawlers to understand the consequences of management actions and market forces on the fishery and the fish.
Shea and Possingham (2000)	Used in SDP in a metapopulation context to determine optimal number and size of releases of biological control agents.
Westphal et al (2003)	Used a SDP model to determine optimal restoration policies for the metapopulation of an endangered Australian bird.
Costello and Polasky (2004)	Developed a SDP for choosing sites to include in a network of marine reserves, accounting for the inability to protect all sites immediately, and the benefits to biodiversity from different sites
Meir et al (2004)	Used SDP to assess biodiversity consequences when a network of reserves cannot be implemented all at once but rather over time and derived simple rules of thumb.
Rout et al (2007)	Used a SDP model to determine optimal translocation strategies for threatened species, comparing minimizing long-term loss and maximizing short-term gain.

Table 10.1 (continued) Some Examples of the Use of State Dependent Behavioral or Life History Theory in Pasteur's Quadrant Since 2000

Authors	What They Did
Bogich and Shea (2008)	Used SDP models to determine the most efficient management strategy for gypsy moth (*Lymantria dispar*) and developed a rule of thumb that, depending on the state of the system, the optimal action was eradicating medium-density infestations, reducing large-density infestation or reducing the colonization rate from the main infestation.
Bauer et al (2010)	Explored potential explanations for observations that Red Knots use a diverse array of migration routes, with potential application to habitat protection.
Baruch-Mordo et al (2013)	Used the basic patch selection model of Mangel and Clark (1988) to develop a decision support tool to evaluate mitigation strategies in human-wildlife conflict.
Hackett and Bonsall (2016)	Used SDP to determine optimal rotation strategies between treatments and refuges to delay development of resistant in *Bacillus thuringiensis* (Bt) treated crops.

Table 10.1 (concluded) Some Examples of the Use of State Dependent Behavioral or Life History Theory in Pasteur's Quadrant Since 2000

Authors	What They Did
Kling et al (2017)	Using SDP with information as a state variable in a bioeconomic model of lionfish invasion and control.
Visscher and Merrill (2018)	Used SDP to explore how interpatch distances on a landscape affected ruminants moving across that landscape.

This chapter is a 'lessons learned' from my career for young scientists who want to work in Pasteur's Quadrant (also see Mangel 2017[1]). To do so, we will first discuss work that crosses disciplinary boundaries, and then turn to the relevant professional preparation. In that section, we will go forward in time – considering undergraduate school, graduate school, postdoctoral work, and beyond (including both academic and nonacademic work). We will then turn to communicating science. We close this chapter with advice from Pasteur himself.

Working Across Discipinary Boundaries

The words "interdisciplinarity" and "transdiciplinarity" are quite the rage, but there are problems for both defining the kind of work and achieving successful outcomes (Ledford 2015). For example, does interdisciplinary mean crossing between physiology and behavioral ecology? I believe that interdisciplinary should mean something more than crossing silos in the biological science.

In the recent book edited by Phil Levin and Melissa Poe (Levin and Poe 2017) on marine conservation in the anthropocene many chapters use the word transdisciplinary. For example, Arkema and Ruckelshaus (2017) wrote: "Transdisciplinarity is the youngest of the trio of terms that also includes multidisciplinarity and interdisciplinarity (Choi and Pak, 2006). Although definitions differ across fields, in general, multidisciplinary research

[1] This paper is one of a series in the "Food for Thought" section of the *ICES Journal on Marine Science* in which senior scientists write brief autobiographies in which they look back and provide advice for young scientists. They are open access and can be found at https://academic.oup.com/icesjms/pages/luminaries

draws on knowledge from different disciplines but stays within individual disciplinary boundaries. Interdisciplinary research links two or more disciplines by integrating theoretical frameworks, methodology, and the perspectives and skills from several disciplines. Transdisciplinary research creates new conceptual, theoretical, methodological, and translational innovations to societal and related scientific problems concurrently by integrating various societal and scientific bodies of knowledge". I have provided their citation to Choi and Pak (2006), and another (Lang et al 2012) for your convenience.

Lang et al (2012) focus on sustainability science, rather than medical science as in Choi and Pak (2006) They describe three phases of the work.

- *Phase A: Collaborative work to frame the problem and build the research team.* This phase includes identifying and describing the problem to be solved, agreeing on the research objectives, which includes collaborative formulation of the strategy (broad objectives) and tactics (specific research questions), and designing the conceptual framework to be used and ways that knowledge across disciplines will be integrated. The key here is to find the match between complexity of the problem and the science that is to be done. No science – experimental, field, or theoretical – can ever be 'realistic', by the very nature of science. See the Appendix to Chapter 2 of Mangel and Clark (1988) or Chapter 4 in Clark and Mangel (2000) about why. It is not unsurprising, but good to know, that collaborative development of a model increases the trust in it and its use (Ulibarri 2018).

- *Phase B: Using collaborative work to create new and transferrable knowledge.* This is when the research gets done, which requires agreeing on a set of methods, developing them as needed, and applying them to the problem. If the collaboration is effective, different disciplinary skills will be brought to bear on the focal problem. It is key here to situate the problem and to identify the particular contributions to the solution of the problem, and how the disciplinary approaches will be integrated. The earlier one involves stakeholders in the process, the more likely one is to get buy-in on their part.

  For example, Benson and Stephenson (2017) argue that interdisciplinary work in fisheries is done in the space of scale of the problem, nature of the application, and the type of advice required for fisheries management. Current approaches provide tactical, prescriptive advice at the scale of fishery and address a small fraction of the decision space required for holistic management. The types of questions that need to be addressed include i) *Management Context*: Here one asks what are the management objectives and how will one evaluate the success or failure of management. ii) *Inventory the Methods*: Valiela (2001, pg 6) noted "[i]t is a waste of time, of course, to worry about the density of angels on any surface, let alone the head of a pin, unless we have a working seraphometer available." That is, unless one has an appropriate method – or can develop one – a variety of management objectives may remain unattainable. For example, in the case of fisheries, Benson and Stephenson (2017) evaluate the following methods: ecological risk assessment, Management Strategy Evaluation (MSE), multispecies and ecosystem models, dynamic multispecies models, aggregate (biomass production)species models, full ecosystem models that range from Minimum Realistic models (MRM) and Models of Intermediate Complexity for ecosystem assessments (MICE) trhough Ecosim with Ecopath to Atlantis, and Bayesian Belief Networks.

- *Phase C: Integrating and applying the new knowledge.* In this phase, the results of the research are applied and implemented. Lang et al (2012) believe that this needs to be done differently than the usual rubric of 'from science to practice'. They see it as the integration (or re-integration) of the results into societal practice (e.g., implementing the evidence based strategies and actions developed during the research) and scientific practice (e.g., publishing the results in peer-reviewed scientific journals).

Lang et al (2012) consider that in addition to the tangible products of the research (new approaches to the problem, new papers in journals), a properly designed and implemented transdisciplinary process may lead to less tangible products such as high self-efficacy (cf Bandura 1997) of the decision-makers or an intense learning process.

Lang et al (2012) also offer design principles. For example in Phase A, one needs to build a collaborative research team that has a joint understanding of the problem to be addressed so that it is possible to define the research project, specific research questions, and decide how one knows when success has occurred. In Phase B, the team leaders must assign the appropriate roles to team members, who then have to adjust and integrate their disciplinary research methods for the problem at hand. In Phase C, one wants to assess the success of the research, develop products for the end-users, and evaluate the overall social and economic impact of the project.

Throughout all this, one must be cognizant of the possible modification of objectives

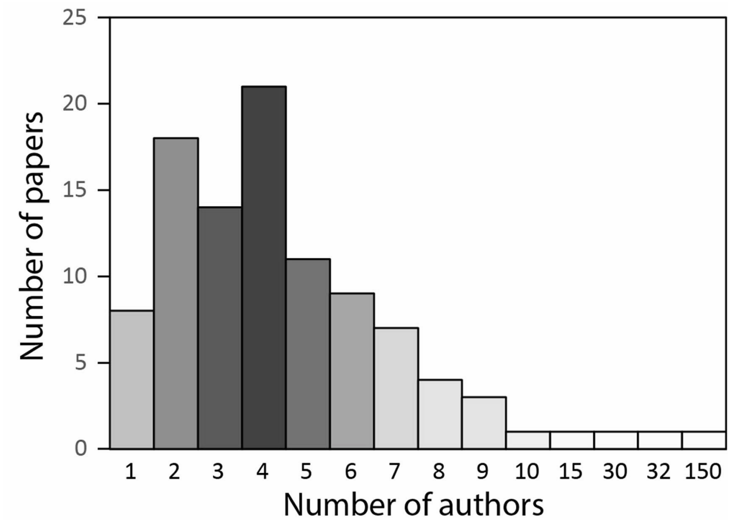

Figure 2: When doing interdisciplinary work, you should plan to become part of a research team Caldera (2016) reviewed multiauthored papers in *Environmental Research Letters* and found that 61 of the top 100 papers had four authors or less. More on the size of research groups below.

(Eisenhower once said something like "I have found that in war plans are useless but planning is essential"), mitigate conflicts between members of the team, and sustain participation in the project. Those modifications include what you think you can achieve and how involved you are – learning how to collaborate in interdisciplinary work is more like a damped spring than a switch. Brown et al (2015) advise researchers (pg 316) "Build stamina, patience and self-awareness to manage the long journey of establishing a productive interdisciplinary team. Put your best ideas forward even if they are unfinished, and be open to alternative perspectives from other disciplines, policymakers, industry practitioners and community members. Prioritize depth early on, and embrace breadth by building relationships with those from other fields and practices." They envision damped oscillations toward constructive interdisiciplinary conversation as a disciplinary scientist learns to find the golden mean between dominating conversations and passively listening. Thus, achieving the ability to do interdisciplinary work is not a switch that you turn on one day, but more like a damped oscillator that overshoots a steady state for some time but then settles into a regular pattern. Furthermore, you should expect to be part of a publishing team (Figure 2), but aim for one that is not too large to be effective (below we will discuss the size of research groups).

Palmer et al (2016) describe how the National Socio-Environmental Synthesis Center (SESYNC) facilitates interdisciplinary research. Their process includes collaborative project development, project planning, the leads of the interdisciplinary teams having a workshop to plan the project, design and facilitation of teams, and check-in meetings. SESNYC also provides computational support.

Hoffmann et al (2017) discuss transciplinary integration rather than interdisciplinary work, focusing on the challenges to researchers who want to integrate across disciplines. In this case, the different kinds of knowledge that one can generate, the different participants in the collaboration, and the different levels of involvement by the participants are all crucial. They conclude that interdisciplinary research requires a warm-up stage in which leaders of the project conceptualize what is to be done and what kind of knowledge needs to be integrated, a middle stage in which all the relevant actors are involved in the details of the work, and a final stage of synthesis, which includes the different groups of participants agreeing on the results and communication with the target audience.

In my experience, a good estimate for timing is that the first and last stages of the project will take about 10% of the total time and the middle stage will take the remaining 80%. Of course, it is worthwhile to remember what my buddy from graduate school, Davis Cope (retired from the Department of Mathematics at North Dakota State University in 2017) said he learned about doing a Phd: "One must keep in mind that the first 90% of writing a dissertation takes the first 90% of the time and the last 10% of the dissertation takes the other 90% of the time". Hoffmann et al (2017) argue that transdisciplinary integration requires strong disciplinary skills for the participants, good interpersonal and management skills of the leaders, and enough time for the integration to occur. I concur with all of these points.

Benson and Stephenson (2017, pg 13) found "that two of seven proposed tools to support decision making in the management system can provide tactical advice, but only one (MSE) provides advice that is consistent with our criteria for generation, transmission, and use of scientific information in management advisory processes. MSE ranks similarly well for strategic con- siderations, as it is an approach that meshes tactical, annual decision making with longer-term strategic objectives and planning...The EREAF performs similarly well against our criteria as a strategic planning tool. Both MSE and EREAF were developed within the functionalist management science paradigm, which seeks technical (and cost) efficiency in management.. In contrast, the only interpretive management science method that has high applicability in the current management context is BBN. These models help to clarify and define problems using messy, in- consistent, and potentially contentious data and objectives...We suggest that BBN could prove very useful as a direction- setting tool for highly complex fisheries problems."

Based on their experiences with water management strategies, Brown et al (2015) suggest that they keys to interdisciplinary work are to

- *Forge a shared mission* Collaboration requires a common mission between the collaborators; otherwise one is simply bringing in collaborators for specific technical skills and disciplines are not likely to be crossed in substantial ways A shared mission provides an overall goal (and desired outcome) and must be broad enough to involve all of the disciplinary researchers in a meaningful way. This is really important because there will be bumps in the road and failures (we need failures in order to learn); shared goals allow researchers to keep the investment of time and effort in the project.

- *Develop 'T-shaped' researchers.* The idea of T-shaped researcher comes from an article in the *Harvard Business Review* (Hansen and von Oetinger 2001). Such researchers are able to succeed in their own discipline but also able to look beyond it. That is, interdisciplinary work requires both breadth and depth (and is more difficult than disciplinary work, for that reason). A T-shaped researcher is highly regarded for contributions in his or her own field of research but also actively engages with other disciplines to understand how they work. In my opinion *to be truly interdisciplinary one must both be a serious disciplinary scientist and master the core skills in one or more of the collaborative disciplines.* Brown et al (2015) note that interdisciplinary work is not just for senior people and that many of their collaborators maintained high disciplinary publication rates while being part of a team. On the other hand, it took nearly five years for them to begin publishing their interdisciplinary work in "high impact (i.e. tabloid)" journals[2].

- *Nurture constructive dialogue.* To which you might respond "Duh", but it is true. Via trial and error, Brown et al (2015) found that it was important to create an environment and behavioral rules in which researchers across all disciplines – some of which are vastly different in their particular norms, technical language, and means of communication – could communicate with each other. This included commitments to use plain English (it is wise for you to buy, read, and re-read Strunk and White

[2]This is my experience with State Dependent Life History Theory implemented by Stochastic Dynamic Programming – Colin Clark and I, and Alasdair Houston and John McNamara had published many papers in specialized journals before our paper in *Nature* (Houston et al 1988).

1979, Dreyer 2019), to respect the norms and standards of other disciplines, and to spend time reflecting on what is working and what is not working in the collaborations across disciplines. All of these things take practice, and almost nobody who is new to interdisciplinary work will be good at it from the outset. For example, new collaborators tend to try to dominate discussion and assert the primacy of their discipline; they need to be reined in (and hopefully learn quickly how to behave).

- *Build bridges between research, policy and practice.* Brown et al (2015) emphasize that establishing connections between researchers, policy-makers, and industry stakeholders is key to developing interdisciplinary collaborations. Contrary to what most academics think, industry rarely thinks in disciplinary silos. Indeed, unlike academia where the individual CV is foremost, in the non-academic setting, the success of teams is crucial. Engaging policy-makers and industrial partners in the design of research program from the outset allows policy and industry partners to provide feedback on the scientific approach and presentation of results.

Professional Preparation

One can begin a career in Pasteur's Quadrant at any age – Thomas (2000), Dill (2017), and Ommer (2018) provide interesting and contrasting a personal histories of their development for work in Pasteur's Quadrant[3]. Dill (2017) and concludes that functional approaches, such as those used in behavioral ecology (and State Dependent Life History Theory implemented by Stochastic Dynamic Programming) allow predictions from first principles of the range of possible responses to environmental change outside the range of conditions that organisms have experienced in their evolutionary history or ontogeny. He also notes that models have the strength in that they can be generalized across species and ecosystems. Most of us cannot be the 19th century explorer Richard Burton, who was a one-man interdisciplinary team (Pettitt 2015). But we can get ready for work in Pasteur's Quadrant.

In Figure 3, I show the problem-solving plane that I developed in discussion with Phil E. DePoy when he was president of the Center for Naval Analyses and I was a consultant there. [4]. In this plane, the x-axis represents the degree of difficulty in solving a problem,

[3]The papers of Dill and Ommer appear the *ICES Journal of Marine Science* "Food for Thought: Luminaries Collection" that I mentioned earlier.

[4]I thank Spiro Stefano, who was my second PhD student, for the visual here. It works much better in a talk!

which ranges from low to high. The y-axis represents the degree of structure in the problem, which includes such issues as how clearly is the problem formulated, are there ready methods for solving the problem, and is it clear which of those methods to use? This also ranges from low to high. Beginning in the upper left hand quadrant (Figure 3a), a problem with a high degree of structure and a low degree of difficulty is simply boring. Similarly, a problem with a low degree of structure and a low degree of difficulty is irrelevant. In the academic world, we tend to give students problems with varying degrees of difficulty but moderate to high structure – the upper right quadrant in the problem solving plane (Figure 3b). But problems in the actual world usually don't have structure, so they occupy the lower right quadrant of the problem-solving plane (Figure 3c). Thus, the applied research world – Pasteur's and Edison's Quadrants – spans the first and fourth quadrants of the problem solving plane, with the vast majority of the problems involving moderate to high degrees of difficulty and moderate to low degrees of structure (Figure 3d). How does one get ready to solve those kinds of problems? The issue is no longer how to solve the problem but how to find the problem in the first place (Mangel 1982, which is hard to find, so I have included it as an Appendix).

Undergraduate and Graduate School

Although it would be ideal to start preparing for a career in Pasteur's Quadrant as an undergraduate, most of us do not. Even if you are already a graduate student, post-doc, or faculty member, I encourage you to read the section on undergraduate preparation.

Undergraduate Preparation

- Master Your Disciplinary Major Don't pick a major because it has the fewest number of required courses, but rather because you are willing to delve deeply into it. Most likely your major will have some blend of empirical and theoretical/conceptual traditions (physics is the best in this regard). Learn both sides of that even if you are 'not good in the lab' or 'not good at math' (see below).

- Get Out Into the Field The best starting point for our kind of work is through an observation that leads you to ask the question "Why is nature like this?". Thus, you should get out into the field. If there are formal field courses, take some. If there are not, create your own by getting out into nature, making an observation, and proposing a hypothesis and asking how you would test it. On my web site, you can

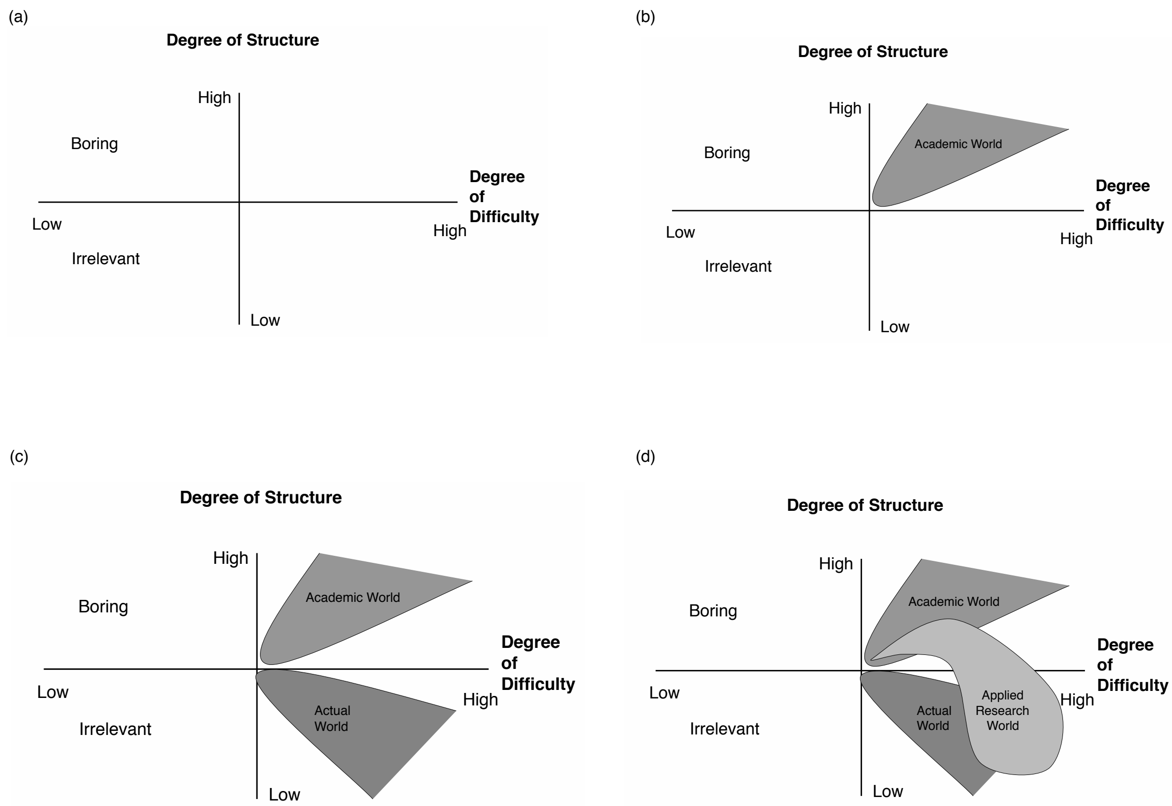

Figure 3: The Problem Solving Plane. a) In this plane, the x-axis represents the degree of difficulty in solving a problem, which ranges from low to high. The y-axis represents the degree of structure in the problem, which includes such issues as how clearly is the problem formulated, are there ready methods for solving the problem, and is it clear which of those methods to use. This also ranges from low to high. problem with a high degree of structure and a low degree of difficulty is simply boring. Similarly, a problem with a low degree of structure and a low degree of difficulty is irrelevant. b) In the academic world, we tend to give students problems with varying degrees of difficulty but moderate to high structure. c) Problems in the actual world usually don't have structure, so they occupy the fourth quadrant of the problem-solving plane. d) The applied research world spans the first and fourth quadrants of the problem solving plane, with the vast majority of the problems involving moderate to high degrees of difficulty and moderate to low degrees of structure. All this begs the question: how does one get ready to solve those kinds of problems?

find a field project book that I prepared as a complement to my undergraduate ecology course that had no laboratory (https://people.ucsc.edu/ msmangel/field.pdf). Think it as a set of examples of field projects that you could construct for yourself based on what you have learned in the classroom.

- Choose Electives to Build Your Interdisciplinary Skills. Among the electives that I believe are most crucial are

  - A course in resource economics, since understanding how costs and benefits are balanced and assessed is crucial. A good example is the new book by Larry Karp on natural resources as capital (Karp 2017).
  - Courses in the social sciences that will help you understand how people make decisions (Kahneman and Tversky 1979, Kahneman 2011, Lewis 2017, Ballard et al 2016, Boyd 2016), how societies function (Converse 2006) and how policy and science advice are passed to governments (Ludwig et al 2001, Hutchings and Stenseth 2016, Milner and Boyd 2017, Murphy and Weiland 2016, Donnelly et al 2018). The best departments for these courses will depend upon the characteristics of the social sciences at your particular college or university.
  - A course in political science, since understanding how government works and policy is made is crucial to what we want to do.
  - A course in art, preferably one that involves active drawing.

  Why art? There are two reasons. First, artists and scientists have the same problem: to look at a complicated situation in the natural world, decide what aspect of the situation to analyze and communicate, and abstract it for purposes of analysis and communication. Even the best representational art is not, like the best science, 'realistic' since the artist, like the scientist, has to decide what to include and what not to include when communicating the idea. Second, the essence of art – as in science – is perception (Edwards 2012) and by enhancing our perception through drawing, we develop a transferrable skill of intuition about other matters.

- Participate in Research Research is a skill and one develops skills by practice. Colvin (2008), in his book titled *Talent is Overrated*, argued that to be at the top of anything requires 10,000 hours of practice – so get started early and do it consistently. Find ways to participate in research projects as early as you can in your studies. If you can do an undergraduate thesis or capstone project for your major, do it. And try to come up with the idea yourself.

- Master Your Skills in Modeling and Statistics. Wilson (2013) has very strong opinions about whether one has to master quantitative skills. He wrote an essay with the title "Great scientist $\neq$ Good at Math". Of course, he also published seminal books in mathematical biology (MacArthur and Wilson 1967, Oster and Wilson 1978). Wilson gives two principles: Principle Number 1 is "It is far easier for scientists to acquire needed collaboration from mathematicians and statisticians than it is for mathematicians and statisticians to find scientists able to make use of their equations." Principle Number 2 is "For every scientist, there exists a discipline for which his or her level of mathematical competence is enough to achieve excellence."

  On the other hand, Bangham and Asquith (1991, pg 992) wrote "It is a widespread fallacy that what mathematics contributes to biology is quantification of an otherwise innumerate science. But experimental biologists have long been expert at measuring and quantifying. The real contribution of mathematics lies in a precise qualitative framework of reasoning. The rate-limiting step in the advance of biology is usually experiment, not theory. (One of the very few notable exceptions was the theory of evolution by natural selection.) Experiment, however, is in no sense superior to theory, nor vice versa: both are necessary ingredients of a proper understanding of nature. An experiment done with no theoretical framework to analyze or interpret the results (let alone a hypothesis) is meaningless; theory in the absence of experiment remains mere theory."

  Ellenberg (2014) makes the case that mastering quantitative skills is the easiest way not to be wrong about lots of things. However, you are still likely to make plenty of mistakes if you are working at a level that challenges your abilities; this is where you want to be.

  I agree with Wilson, Bangham and Asquith, and Ellenberg! I encourage you to master quantitative skills as much as possible. Here's the reason: with mathematical skills you internalize one level when they are tools at the next level . Putting aside the insipid reason that calculus should be a gate-keeper for medical school, the reason for requiring calculus of biology students is so that they really master the skills of pre-calculus such as knowing what a function is or how to instanty identify the slope and intercept of the line $y = 7x + 5$. These are things that you have to do constantly in research. Imagine if everytime somebody told you that the growth rate of a population is 5% of its current size, you had to write down $dN/ds = 0.05N$ and then laboriously solve it, rather than by saying "That means the population grows exponentially according to $N(s) = N(0)e^{0.05s}$" because you know this in your gut.

There is no doubt that learning quantitative skills is difficult. In another "Food for Thought Piece", Moore (2020) writes: "Barrier to personal progress in science can also arise from undeveloped skills. 'Take math courses until you get a C' is what G. Evelyn Hutchinson, the eminent ecologist (Hutchinson, 2011), told me and other graduate students at Dartmouth College when he visited our small group in the mid-1980s. I have recalled his wise advice numerous times throughout my career, and I passed it on to my students"[5].

Graduate Preparation

The recommendations that I have about coursework in undergraduate school apply here as well. It is likely to be more difficult to take a variety of courses outside of one's discipline in graduate school, but I encourage you to find a course on resource economics and one on policy (at least seriously audit them if you cannot take them). Thus, you will have to take more courses to become an interdisciplinary scientist than you would to just become a disciplinary scientist.

Similarly, you need to attend meetings in your disciplinary field but at the same time look for opportunities to extend yourself by finding interdisciplinary meetings. When going to meetings, especially the latter ones, remember the Spanish proverb that we have one mouth and two ears to remind of us that we should listen more than talk.

You also need to read more widely than you would if you were doing only a disciplinary degree. Avoid the trap of the impact factor and the high profile journals. By its very nature, work in Pasteur's Quadrant requires more space for the full development of ideas than does work in some of the other quadrants. Thus, you may not find the right kinds of papers in the high profile/high impact factor journals. And if you do, the important material will often be buried in Appendices that are very hard to decipher (that is, it will be difficult to impossible to reproduce the work described in these appendices).

[5] I have not looked up the Hutchinson citation, it is: Hutchinson, G. E. 2011. in *The Art of Ecology: Writings of G. Evelyn Hutchinson.* D. K. Skelly, D. M. Post, and M. D. Smith (editors). Yale University Press, New Haven, Connecticut, USA. 356 pp.

Working Hours and Work Life Balance

Although this section could sit with the previous one, I give its own heading because of the importance of the topic of work-life balance.

Over the last few years, when visiting universities to give talks, I have done mentoring sessions with graduate students and post-docs. One question that consistently comes up, especially from those interested in a career in academia, is whether they need to regularly work 70 hour weeks in order to succeed.

There are a number of comments I make. First, it is really hard to *work* a 70 hour week. I have done it on occasion but consider that few – if any of us – can sustain that for very long and continue to be productive.

Second, there is a threshold-asymptotic relationship between the level of effort that you put in and the quality of the product (Figure 4). That is, there is a minimal level of effort for there to be any quality in the product. Above this threshold we might imagine two models for quality as a function of effort. In the first model, there is a linear relationship between effort and quality until a certain level of effort is reached after which quality remains constant. In the second model, quality is an asymptotic function of effort – you may never reach maximum quality and part of the challenge here is to decide at which point to consider sufficient effort to be put into a product. I give three examples in Figure 4– you could stop at about 60%, 85% (by increasing the effort by fifty-percent over that giving 62%) or 92% (by doubling the effort of that giving 62%) of maximum quality. Deciding where to stop depends on what happens next with the product. For example, for a first draft of a paper with collaborators you might want to stop at 70% of maximum quality (thus getting the draft to them sooner rather than later, and allowing real opportunity for them to improve the paper), with a thesis chapter that you expect serious comments from your committee perhaps 85% of maximum is better, and with a paper that you are about to submit, 90-95% is a good stopping point. Now, if you want to guarantee that you both overwork and rarely get things completed, reason like this: "I am at 92% of maximum possible quality, but if I just put in the same amount of effort as I have already, then I can claim the next 5% of quality."

One of the tricks to both not working 70 hour weeks and getting things out is learning which tasks just need to be done, which need to be done well enough, and which need to be done really well (but not to 100% of maximum quality– which may take you into the asymptotic time sink). For example, a letter to the campus registrar about the poor quality whiteboard does not need to be the best possible letter of complaint, rather it just

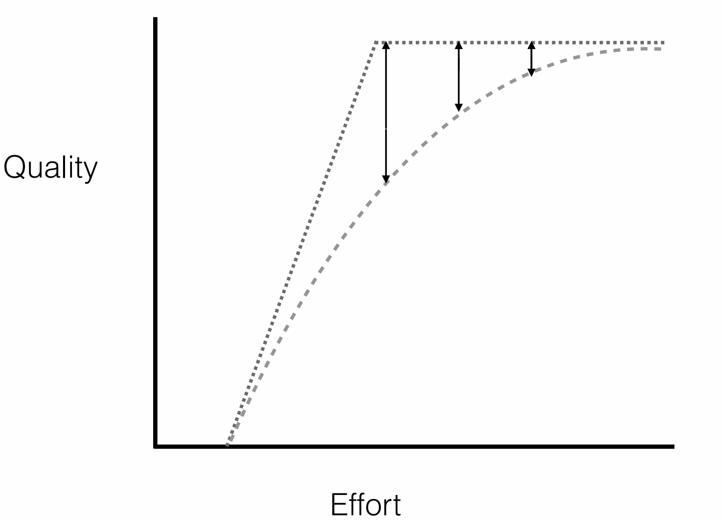

Figure 4: A theory of effort and quality of research and writing. Without some minimum level of effort, there is no quality. After that threshold is passed, quality increases with additional effort. But quality always has an limit (whether that is approached linearly or asymptotically is depends upon the situation, but my opinion is that it is almost always asymptotic). One aspect of having a successful work-life balance is to learn to be satisfied with quality that is close to but below the asymptote.

needs to go out. A grant proposal needs to be done really well.

Third, as your career progresses, the quality per unit of effort will increase. Now since quality per effort is a ratio, it can increase in one of two ways. First, it could be that quality remains the same, but the effort needed to produce that quality declines with experience. When I was a graduate student, the western novel writer Louis L′Amour (check spelling) was interviewed on the news show *Sixty Minutes* and he said that he could write a novel sitting in the middle of the LA freeway – demonstrating his ability to concentrate (and his fearlessness) and the kind of formula that he had for writing. I'd say there that quality remained constant but effort declined.

The other way that quality per unit effort increases is that the struggle is always present, but the quality for the same effort increases. In 1991, John Updike published his collected non-fiction from the 1980s (Updike, 1991). In the preface to this collection, Updike noted how hard it was for him to write some of these pieces. When John Updike finds it a struggle to write, we should all take solace and just keep working.

Finally, work-life balance is important if you are to be both a successful scientist and a content person. A 70 hour work week does not allow for any kind of balance. I will leave you with a homework problem: who impresses you more – the person who works 10 hours a day seven days a week and succeeds or the person who works 6 hours a day 5 days a week and succeeds? Which kind of person do you want to be?

Postdoctoral Work

Hans Krebs considered the postdoctoral period to generally be the most important and formative for one's career. He wrote "To attain distinction in scientific research you must 1) get a post-doctoral Fellowship, which will give you the time and opportunity to test yourself; 2) attach yourself to a centre of excellence; 3 work hard and make the fullest use of the time and facilities that the Fellowship affords; and from time to time, search your heart critically, with the help of objective critics, to find out whether you really possess the right mixture of qualities – urge, commitment, imagination, humility– which are the roots of creativity in science." (Krebs 1981, pg 179). In a festschrift honoring the John Bachall, William Press (whose article was is about using Bayesian methods with data on the Hubble constant) wrote that Bachalls's advice to young scientists was to choose important problems, get involved with real data, and bring the most powerful analytic tools to bear on the problem at hand (Press 1997, pg 59).

So how do we find the important problem? Is it the one that is trendy right now? Maybe, but perhaps not.

Richard Feynman's Advice to Koichi Mano on Picking Problems in Science

After Richard Feynman received the Nobel Prize in physics, he received a congratulatory note from Koichi Mano, who had been at Cal Tech when Feyman was doing the work for which he won the prize but who was back in Japan. In his note to Feynman, Mano wrote that he was a nameless man working on unimportant problems. Feyman's poignant response is this:

"The worthwhile problems are the ones you can really solve or help solve, the ones you can really contribute something to. A problem is grand in science if it lies before us unsolved and we see some way for us to make a little headway into it. I would advise you to take even simpler, or as you say, humbler, problems until you find some you can really solve easily, no matter how trivial. You will get the pleasure of success, and of helping your fellow man, even if it is only to answer a question in the mind of a colleague less able than you. You must
501 not take away from yourself these pleasures because you have some erroneous idea of what is worthwhile.

"You met me at the peak of my career when I seemed to you to be concerned with problems close to the gods. But at the same time, I had another Ph.D. student (Albert Hibbs) whose thesis was on how it is that the winds build up waves blowing over water in the sea. I accepted him as a student because he came to me with the problem he wanted to solve.

"...No problem is too small or too trivial if we can really do something about it. You say you are a nameless man. You are not to your wife and to your child. You will not long remain so to your immediate colleagues if you can answer their simple questions when they come into your office. You are not nameless to me. Do not remain nameless to yourself – it is too sad a way to be. Know your place in the world and evaluate yourself fairly, not in terms of ideals of your own youth, nor in terms of what you erroneously imagine your teacher's ideals are." (Feynman 2005, pg 198-199).

When looking for a post-doctoral position, seek one in which you will be allowed to develop your own ideas, rather than just work on those of your supervisor. For example, ask if you can have one day a week for your own scientific development that will give you momentum going into your next position.

Finally, use the post-doc to continue to develop your skill set (get that course in resource economics now if you have not done so and always learn more statistics and modeling); for example audit one course a semester/quarter.

Find Mentors at Every Point in Your Career

We are not in this alone (Obama 2018). As an undergraduate student you will likely be assigned an advisor. Treat your advisor as a mentor, ask questions of them and seek advice. If your advisor seems uninterested in you, then seek a different advisor. At every level, if you mesh well with a particular instructor, seek them out as a mentor as well. My very first scientific mentor was William Hay, who taught an upper division oceanography course that I took (you can read more about Bill in the *Homage to My Mentors*, to be found on my web site.

As a graduate student and post-doctoral scholar you have a supervisor. If the world were perfect, every graduate or post-doctoral supervisor would be a terrific mentor. Sadly, the world is not perfect. Thus, it is on your shoulders to once again find mentors, by meeting faculty other than your supervisor, by asking to attend group meetings of other faculty, and by simply observing how people behave. When you are starting out as as a graduate student, more senior graduate students and post-docs can also be your mentor.

This process continues once you start a permanent job. Whether in academic or non-academic work, discover the people whom you admire and treat them as your mentor. Of course, as you get older, it may happen that younger people can serve as mentors too. For example, a number of my junior colleagues are my mentors with R and I am happy to have their mentorship.

We are truly not alone.

Communicating Science

> It wasn't by accident that the Gettysburg address was so short. The laws of prose writing are as immutable as those of flight, of mathematics, or physics – Ernest Hemingway to Maxwell Perkins
>
> If a lecture is not as alluring to the audience as a bride to her groom, you had better not deliver it at all – Simeon Ben Lakish
>
> If I am such a natural athlete, how come I have to practice 11 hours a day – Bill Russell
>
> It usually takes more than three weeks to prepare a good impromptu speech – Mark Twain

Writing and speaking are also skills. As with all skills, these require some talent and lots of hard work. The ability to communicate science to the non-expert is about as important and pressing as it has ever been. For that reason, I will emphasize speaking more than writing. When writing the first draft of this chapter, I was reading Hope Jahren's *Lab Girl* (Jahren 2016). There is much to recommend about a book that begins "THERE IS NOTHING in the world more perfect than a slide rule" (Jahren, 2016, pg 7)[6]. Jahren has been described as a rock-star of science communication, and although we all cannot be like her, we all can improve our ability to communicate science. One of the things that is also apparent very early in Jahren's book is how hard she has worked and works. Writing is an essential part of communicating science (publish or perish happens pretty much everywhere in some form, not just in academia), pretty much almost everyone getting a PhD should understand the importance of written communication. However, verbal communication is equally important but often not given appropriate attention in graduate programs.

[6]Younger readers may not know what a slide rule is or how it is used and Jahren's book really does not do justice to slide rules. I encourage you to put away your phones and calculators, seek out a slide rule, and have some fun with it.

In this section, we will explore two points First: No matter how good you are technically, if you cannot tell your story in a compelling manner, it will be lost. Perhaps we need short and pithy advice columns on how to do this rather than whole books; for example in the 22 Nov 2016 issue of *Forbes* magazine, Marshall Shepherd, wrote a column "9 Tips For Communicating Science to People Who Are Not Scientists". His tips are consistent with much of this chapter, as is the advice of Martinez-Conde and Macknik (2017).

The second point comes from former Mangel Lab member Kristen Honey (who is now doing wonderful things concerning Lyme disease – visit her site at Honey2Healing.org). Kristen reminded me that for our group meetings the tradition was everyone – even brand new students – talked about their research (ideas, results, problems) and that my mantra to the group was "Practice, practice, practice".

Kristen went into more detail and wrote to me "What I remember you sharing with us was: to say 'Yes' to every speaking opportunity that came along – whether teaching a class/seminar, giving a course lecture, speaking at a conference, a departmental brownbag lunch/seminar, a lab talk, etc. Apply for any and all conferences and opportunities to publicly share your research while in graduate school. Practice speaking in public in front of audiences big or small. Public speaking is a teachable skill, which can be honed and developed as one's experiences and time practicing add up over the years, and, eventually, decades... to become an excellent speaker in your own style/voice. Practice. Practice. Practice."

I do not recall saying this, but also do not doubt that I did. Kristen also wrote to me that the advice about speaking is "analogous advice to how you used to say 'Math is not a spectator sport. Think of it like going to the gym to build muscle strength with sustained commitment and applied effort. Keep doing math – your own math – not just watching others (nobody ever got strong by watching other people work out in the gym) – and build your math muscle.' The common theme about both math and speaking: Practice. Practice. Practice. Both are teachable skills that improve with time and commitment."

Writing

Skills improve more rapidly with effective coaching and consistent training. For writing, this means both sharing what you have written and trying to learn about how to be a better writer. To do the latter, it is helpful to have a collection of books on writing. Here are some of mine:

- Alley (1987) *The Craft of Scientific Writing*
- Atwood (2002) *Negotiating With the Dead. A Writer on Writing*
- Day (1998) *How To Write and Publish A Scientific Paper. 5th Edition*
- Dreyer (2019) *Dreyer's English*
- Elbow (1998) *Writing With Power*
- Evans (2017) *Do I Make Myself Clear? Why Writing Well Matters*
- Freed (2005) *Reading, Writing, and Leaving Home. Life on the Page*
- Friedland and Folt (2000) *Writing Successful Science Proposals*
- George (2004) *Write Away*
- Greene (2013) *Writing Science in Plain English*
- Higham (1998) *Handbook of Writing for the Mathematical Sciences*
- Pinker (2014) *The Sense of Style. The Thinking Person's Guide to Writing in the 21st Century*
- Prose (2006) *Reading Like a Writer*
- Rabiner and Fortunato (2002) *Thinking Like Your Editor*
- Schimel, J. *Writing Science. How to Write Papers that Get cited and Proposals that Get Funded*
- Silva, P.J. *How To Write a Lot*
- Strunk and White (1979) *The Elements of Style*
- Truss (2003) *Eats, Shoots and Leaves*

You should not rush out to buy these; I have collected them over a long period of time. One of these authors writes fiction; you may ask what can you – a scientist – learn from them? Plenty. For example, Elizabeth George writes fiction about a British detective named Thomas Lynley. Her book about the writing life is roughly 50% about how to write fiction and 50% about the habits of writing, which is why it is on this list. She also makes the point that one needs to ask if he or she wants to be an author – somebody who gets up every day and goes to the desk to write – or an Author – someone who goes on TV shows, is photographed for profiles magazines, and does all the other things that prevent one from writing. Similarly, you might ask if you want to be a scientist – somebody who does science every day – or a Scientist – somebody who traipses around the world speaking about the work that other people do for him or her.

Gopen and Swan (1990), Plaxco (2010), Heard (2014), Yossa (2014), Endler (2015), Grossman (2017), and Mensh and Kording (2017) are papers about writing papers.

Among these books and papers, which are all worth reading and re-reading, I take special note of Strunk and White (1979), which you definitely should buy, read and re-read regularly. Pinker (2014) and Dreyer (2019) each offer an update of and homage to Strunk and White.

Tim Smith told me that when he began to write his wonderful book on the history of fishery science (Smith 1994), a friend of his who is a writer told him that the author of a book is in a struggle with the reader, because the reader can always decide to put the book down and never open it again. The challenge of the writer is to keep them reading. Similarly, even if it is only for five minutes, the challenge for the speaker is to keep the audience engaged, rather than wandering off in their daydreams.

Think that you are building a relationship with your reader. It is conversation, but does not happen in real time.

The most important message of these books and articles on writing is this: Get yourself into the writing habit. One can read books about writing forever and not do it. Here's a simple but important calculation. Imagine that you write for one hour a day, five days a week, forty weeks a year so that you give yourself nearly an entire quarter off from writing. If you can write one page in that hour you will write $1 \cdot 5 \cdot 40 = 200$ pages a year. Certainly then, especially early in your career, you will then run out of things to write about long before you run out of time for writing. So where is the difficulty? Finding that 1 hour. Different people will resolve it in different ways. I like to write first thing in the morning, when my mind is freshest and least encumbered. Other people like to write at the end of the day, when they have had time to synthesize. Some people prefer to do their 5 hours in one block or two 2.5 hour blocks, although my opinion is that energy fizzles with such long effort. Find what is right for you and then stick to it – this means writing at least every work day (a friend of mine from undergraduate school who is science fiction author writes 6 or 7 days a week). In his Food for Thought piece, Cury (2018) recommends writing every day; he does so for two hours with no distractions. Try it out.

The sticking to it is very important. Many Hemingway wanna-bees forget that no matter what the great writer did the night before, the next morning he was at his table in the cafe, pencils and yellow pad in hand.

Speaking: The Role of Deliberate Practice

There are also literally hundreds of full books written about communicating science. Two nice recent ones are Dean (2009) and Walker (2010)). Dean is a science journalist and on the faculty at Harvard. This is a wide ranging book for scientists about communicating with the public and covers the landscape in which such interactions may occur. The second chapter is "Know Your Audience". This is a very handy book once one has mastered the art of the short talk, a good reference when a journalist is calling, or when one acts as an expert witness.

Walker runs a series of companies on media training. This book goes into much more detail about technique, but pretty much begins at the time of the presentation, so that there is little about deliberate practice.

We will begin with strategy and tactics for making a short a success. We will work towards deliberate practice for communicating. The rubric some of us learned in high school speech class –"Tell them what you are going to tell them, tell them, and tell them what you told them" – is a good start, but insufficient. We should seek to identify the laws that Hemingway wrote about to Maxwell Perkins, and modify those laws for communicating with non-experts?

Since speaking is a skill, each one of us is born with a different level of ability to give talks, but without deliberate practice one does not hone the skill; talent is once more overrated (Colvin 2008). Colvin is a senior editor for *Fortune* magazine and the subtitle of his book is "What Really Separates World-Class Performers from Everybody Else" and the answer is deliberate practice; he explains how to conduct deliberate practice in great detail. One can spend all of one's time reading about how to prepare short talks without actually preparing any; this will not do you much good. Regardless of whether you are having a 5 minute talk about what you do at work with your grandmother or giving a 50 minute talk in a job interview, you must tell a story. So let's get going.

The Short Talk: Elevator Speeches and 5 minute Talks

By a short talk, I mean one of 5-10 minutes given by one person to a few to many other people. Almost *everyone*, not just scientists, does this in the course of their work. Short talks include contractors explaining to home owners the details of a potential remodel; ministers, rabbis, and imams speaking to their congregations; entrepreneurs speaking to venture capitalists either formally or in an "elevator speech" (if you – the entrepreneur – have not captured the venture capitalist in three to five minutes, go back and work on your pitch some more). Even much of teaching consists of short talks – in which questions are answered for small groups of students who are attentive at that moment.

Intentionally being redundant: Giving such talks is a skill, which means that no matter how much native talent one has, the quality of the talk can be improved through deliberate practice. In general, all talks involve asymmetric information, since the the speaker knows much more about the subject than the audience; the great challenge is how to communicate the key ideas most effectively.

You should aim to communicate: **W**hat you are working on, **W**hy you are doing it, **W**hich tools you are using, and **W**isdom you have gained. Think of these as $\mathbf{W}^4$.

Know Your Subject: One cannot give a good and meaningful short talk without knowing the subject matter. This is more than accumulating a collection of facts, but collecting the facts and organizing them in a way that is consistent with the goal of the talk. As you read, it is key to allow sufficient lead time for subconscious work along with conscious work.

Know Your Audience: In most cases we have an idea of who the audience is and need to think carefully about the limitations of their knowledge. Imagine a contractor talking to another contractor rather than to a client who who has never built anything. Imagine explaining "where babies come from" to a 5 yr old, 10 yr old or 15 yr old.

Learn More and Use Less – Selecting Your Knowledge to Match Your Audience: The real secret to the short talk is figuring out how to reach your audience without overwhelming them. In almost every case, this means using less information than you have at hand. Thus, we need to winnow information to create the optimal match between the needs of the audience and the knowledge of the speaker. The focus is finding a nugget of information that they can hold onto after the talk ends.

Technique Matters

Even a good match between the audience and your knowledge base is not sufficient, technique is important. Important aspects of technique are:

- Speak clearly and slowly: We all know how annoying it is to try to follow a speaker who speaks too quickly, mumbles, is not articulate, or does not project. Don't be one of those people. If this means that you need to use a microphone where somebody else might not, use the microphone. Experiment with its placement so that you get to know it before you have to start to use it. We all tend to speak too quickly when we are excited about a topic or nervous in front of an audience, so be sure to slow down. In fact, if you use written notes, at the top of them it is a good idea to write "SLOW DOWN!!"

- Move while talking in a way that adds rather than distracts: Speaking is somewhat like acting, with the important difference that an actor does the same thing for a different audience every night, but we (especially when teaching) do different things for the same audience. Actors learn is how to move their bodies in a way that the visual effect of movement enhances the aural input. Think about this as you imagine your body motions during a talk.

- Address and engage the audience: The best way to do this is to look at the people in the audience. Scan the audience, to assess if people are following you, and sometimes directly look at an individual (or two or three) for a more detailed sense of how you are being followed.

- With great Power(point) comes great responsibility: One might say that "Powerpoint does not bore people, people bore people" but that is only partially true. When using Powerpoint think about the information content per slide, number of slides per minute, and how are you going to use the slides. Are you going to face the slides and read them (do not do that), point out salient features, or say in different words the information that is on the slides (thus providing both aural and visual sources for the same information).

- Chalk or White Boards: It is unlikely (but not impossible) that you will face using a chalk board (some are still around), but you are likely to use white boards especially when speaking extemporaneously. Writing on a board is another skill – which means you need to practice how big the letters are, figuring out the best density of information per board, and how to simultaneously write on the board and how to talk to the audience. For the latter, turn to face the audience frequently – saying, whenever possible, what you have just written but in a different way, once again giving visual and aural input for the same information

- Notes: Most of us will not talk with tele-prompters, but we may use notes that range from from notecards with key words to a few pages with key ideas to notes on your PowerPoint or Keynote file to a fully written talk that you read. What you use is a matter of personal choice – but you have to experiment (i.e., practice) to figure out which works best for you and how not to bury yourself in the notes.

- How You Dress Is Important: The way that you dress gives the audience a sense of your respect for them. Thus, aim to mirror the dress standard of the audience (even if it means asking in advance). It is better to overdress than underdress. Nobody is going to fault you for wearing a tie to a job interview, even if none of the audience (except possibly the Dean) do, but if you show up in a tee shirt and torn jeans (thinking that this will show what a great scientist you are) you have a battle from the start – people will be asking about why you dressed that way, rather than listening to what you are saying.

- Don't be Arrogant. It is easy for highly trained people to become arrogant and play 'gotcha' with an audience, to show off to the audience. Here's an example, which I used both in *The Ecological Detective* (Hilborn and Mangel 1997) and *The Theoretical Biologist's Toolbox* (Mangel 2006) because it tells the story so perfectly. It comes from Hogarth (1980).

  Consider these two series:
  Series A: 45, 32, 12, 23, 26, 27, 39
  Series B: 1040, 1027, 1007, 1018, 1021 , 1022, 1034

  When asked which sequence is more variable, most people will say that sequence A is more variable than sequence B. However, sequence B is sequence A plus 995, so that the variance of these two sequences is exactly the same. Hogarth writes: "Which series exhibits more variability? Most people answer series A. However, the statistical measure of variance - which indicates the amount of irregular variations from the mean of a series of numbers - is the same for both series. Series B is simply series A plus a constant. However, intuitive judgments of variability are usually influenced by the size or context of the series or objects. That is subjectively relative variability is more salient than variability per se" (Hogarth 1980, pg 44).

However, variance or standard deviation by itself, lacking units, is nearly meaningless. Is a standard deviation of 5 large? Who knows? Compared to what? Without units we cannot assess the meaningfulness of the standard deviation. The coefficient of variation, standard deviation divided by the mean, has no dimensions, so if we have a coefficient of variation that is 5, we then can conclude it is large, since this is telling us that the standard deviation is 5 times the mean. The coefficient of variation of sequence B is much smaller than that of A. You should (i) verify that this is true by computation, and (ii) understand the reason for this being true.

In *The Theoretical Biologist's Toolbox*, I added a series and made it into an exercise that is both easy and fun (Mangel 2006, pg 88). I repeat it here. We now have

Series A 45, 32, 12, 23, 26, 27, 39
Series B: 1401, 1388, 1368, 1379, 1382, 1383, 1395
Series C: 225, 160, 50, 115, 130, 135, 195

Ask at least two of your friends to, by inspection, identify the most variable and least variable series. Also ask them why they gave the answer that they did. Now compute the mean, variance, and coefficient of variation of each series. How do the results of these calculations shed light on the responses?

- <u>Spend Time Thinking About Different Ways of Presenting the Same Information</u>. Those of you who have taught know that much of being a good teacher is anticipating the different ways that students may not understand the material, and coming up with ideas that will help overcome the barriers. That is why teaching takes so much subconscious time (which is also the time spent on research – one of the reasons that we are often less productive in the midst of teaching).

  This same problem arises when we give talks, and it is worthwhile for you to spend time in advance thinking about different ways of presenting the same information. I will give two examples.

  From 2013-2016, I served on the Scientific Review Board of the International Pacific

Halibut Commission (you can find out about us here http://www.iphc.info/srb). The SRB's role is to meet with the Commission scientists to provide feedback about both the science that they do and communication of the science to the Commissioners.

One thing we discovered is that people understand a phrase such as "6 times out of 100" more readily and fully than simply saying 6% of the time. I am not certain why this is so, but it could be that when we say 6 times out of 100, it is easy to envision 100 coin flips, or bottles into which we are dropping marbles (or your favorite visualization) than the more abstract 6%.

We also found that concepts about probability are more effectively communicated as cumulative distributions rather than probability densities. Imagine trying to communicate the probability density of spawning biomass. First, we have to explain the notion of a continuous random variable X and that the probability density $f(b)$ is interpreted to mean that the chance that biomass B falls between b and $b + db$ is approximately (remember those pesky $o(\Delta b)$ terms when defining a density (Mangel 2006)) $f(b)\Delta b$. Thus, we would illustrate that the chance of the biomass being about the specific value b_1 and a bit more than that Δb is $f(b_1)\Delta b$. To describe a confidence interval, we need to implicitly discuss the notion of integrating the probability density.

If instead that we use the cumulative distribution function, we explain that this function $F(b)$ represents the chance that B is less than b – a much simpler concept to convey, with a plot that only rises in the b-$F(b)$ plane. Furthermore, we can easily illustrate the ideas of the 5% and 95% confidence interval by drawing horizontal lines from 0.05 and 0.95 and seeing where they intersect the curve. The point here is not that you should use cumulative distributions rather than probability densities or that you should report 6 times out of 100 rather than 6%. In summary, you should continuously think about better ways of communicating quantitative concepts and think of communicating as an experiment – try things out, assess what works and does not work, and modify accordingly.

Using Practice to Improve: The NW to SE Assessment of Skills

We now come to the process of deliberate practice, ranging from actual practice talks to thinking hard about the talk one is about to give, and its role in making your short talks excellent.

To the old saw “Practice makes perfect” one can respond “Practice makes permanent” – that is simply practicing may reinforce your bad habits. The way to get around this is through feedback.

Here’s a feedback sheet that I used in my lab group meetings when students, post-docs, or I practiced talks, and in our team taught course for graduate students in their first quarter. The compass headings NW and SE indicate Needs Work and Skilled and Effective respectively.

Name of Speaker:

General skills

Voice:

Clarity NW == == == == == == == == SE

Pace NW == == == == == == == == SE

Volume NW == == == == == == == == SE

Carriage:

Confidence NW == == == == == == == == SE

Mannerisms NW == == == == == == == == SE

Engagement:

Eye contact NW == == == == == == == == SE

Use of repetitive phrases NW == == == == == == == == SE

(eg. Um, like, so, you know)

Kept within time limits NW == == == == == == == == SE

Scientific skills:

Motivated the question: NW == == == == == == == == SE

Math Presentation: Level of detail NW == == == == == == == == SE

Math Presentation: Clarity NW == == == == == == == == SE

Information Flow:

Sequencing NW == == == == == == == == SE

Major Topic Emphasis NW == == == == == == == == SE

Slides:

Number NW == == == == == == == == SE

Detail NW == == == == == == == == SE

Interest NW == == == == == == == == SE

Content:

Introduction: Clear and concise NW == == == == == == == == SE

Body: Expanded introduction NW == == == == == == == == SE

Conclusion: Repetition of main themes NW == == == == == == == == SE

General comments:

An important point about using a sheet like this: it is best done in a situation in which over the course of the afternoon, weekly group meeting, or formal course, everyone speaks. This helps to temper comments – when writing comments on others, one knows that his or her turn is coming and that tends to make everyone better behaved to know that they too will talk at some point.

The 12-15 Minute Talk

Although there are instances of 5 minute talks in professional settings, most of the short talks that we give as scientists occur in society meetings with 12 or 15 minutes allocated for your talk, questions, and transitions (so think of these as 9 to 12 minute talks).

One can view giving a talk as a research problem. Construct hypotheses about how to effectively present the information and then try them out. Regardless of how it goes, analyze the result. If you were successful, you want to know why and if the talk bombed you also want to know why. Trust me – we all give talks that bomb.

Here there are a number of other things to consider

- Detail versus flow and clarity: The essence of dynamic programming is thinking backwards in time and you can do much the same with planning your talks. That is, ask yourself "what do I want people to remember when I finish the talk?" and then ask how you can get there. Doing so will allow you to construct the flow of your talk from the targeted end point to the very beginning. In practice, you might even begin by writing your conclusions slide. When doing this, every time you add a detail you can check whether or not it will contribute to the flow to the conclusions and the clarity of what you are doing. If it does not, cut that detail. Once you have reached the introductory slide, you can now iterate forward – working on clarity and focus – to get the detail of flow and clarity as best as possible. This same kind of backward and forward iteration is used to solve dynamic state variable games (Alonzo and Mangel 2001, Alonzo et al 2003).

- Short Term Memory and the Aurual/Visual Focus: Psychologists tell us that people can keep no more than 7 items in short term memory (Miller 1956), although Marshall Shepherd, whom I mentioned above, says that it is closer to 3 items. The point is that your audience will only remember a small amount of what you are saying (and if you are in a session with 4-6 other talks, only a small amount of what was said in total). Keep that in mind as you trim. Also, as we have discussed above, keep working on using your slides so that the aural and visual reinforce each other – use your spoken words to enhance the visual of the slide and vice versa. Never read the slide to the audience.

- Dealing with Technical Questions: We are all absorbed, and often motivated, by the details of our work. And once in a while, you will get a similarly interested person asking a question following a talk. Resist the temptation to get into the details when you are standing up and everyone is watching – rather suggest that you meet after the talk (e.g. at the end of the session for a coffee) to discuss those details.

- Saying "I don't know": At other times you will be asked a hard question to which you do not know the answer. When that happens, do not try to bluster your way through it – most of the audience will see what you are doing and think less of you rather than more for trying this. Sometimes, in fact, a question to which you do not know the answer may lead to a publication. This happened to me at a workshop on extinction risk for west coast salmonids in 1996, when I was asked a question from somebody way back in the audience and did not know the answer. I replied "I do not know – we should write a paper on that", which we did (Cooper and Mangel, 1999).

- Making Equations Interesting: Even in a short talk, there are times when you will need to show equations. The path bifurcates very quickly. If the equations are too difficult to explain in a small amount of time (which is all that you have in a short talk), then simply show them and provide a reference to the equation. The idea here

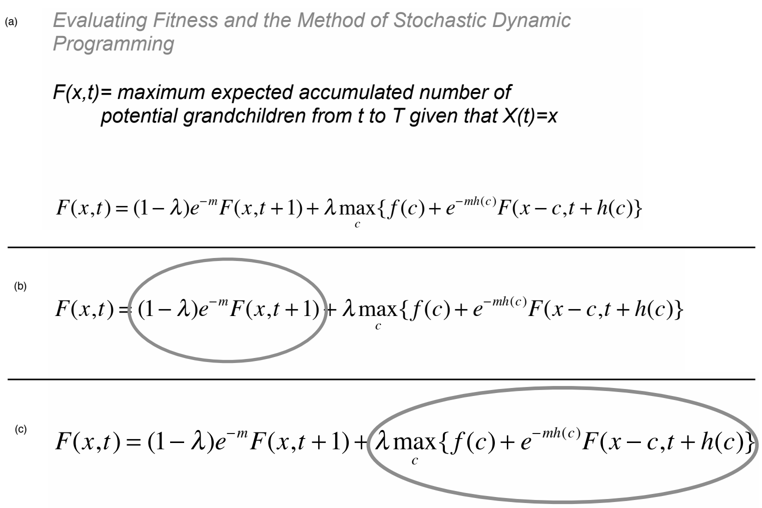

Figure 5: One approach to presenting equations in a short talk, taken from a talk that I give on the evolutionary ecology of offspring size and investment. a) I show the dynamic programming equation for the fitness of a pro-ovigenic parasitoid. b) I then circle [in a bright color] one term on the right hand side of the equation – corresponding to the parasitoid not finding a host in period t and explain each of the terms in the product. c) I then circle the second term on the right hand side of the equation – corresponding to the parasitoid encountering a host in period t and then ovipositing a clutch of size c – and explain each of these terms. The visual and aural input thus complement each other.

is that those who already know the equation will immediately grab it and those who do not will have a source to follow up on the ideas.

If the equations are not too difficult to explain – by which I mean that you can derive them while speaking – then you need to think carefully about how to present them so that the listeners do not get lost in the weeds. In this case, I have found two approaches to be successful. One can show the equation and then step through it, circling various pieces of it (visual) while describing the meaning of those pieces (aural); I give an example of this approach in Figure 5.

Alternatively, even in a short talk, we may choose to derive an equation, particularly if you believe that the audience has the mathematical chops to follow it. For example, if we were deriving the Holling disk equation, we would first introduce the variables T, λ, and h as the time interval during which foraging occurs, the rate of encounter with the prey items, and the handling time of the prey items, with the goal of discovering how encounter rate and handling time determine the number of prey items encountered between 0 and T. We then introduce S and H as the total search and handling times, so that the analysis proceeds as follows

$$\begin{aligned} S + H &= T \\ H &= \lambda S h \\ S + \lambda S h &= T \\ S &= \frac{T}{1+\lambda h} \\ \text{Number of prey encountered in } (0,T) = \lambda S &= \frac{\lambda T}{1+\lambda h} \end{aligned}$$

The associated verbal cues would be something like this:
"When we sum the search and handling times we get the total time."
"The handing time is the number of prey encountered (λ S) times the handling time (h) per prey item."
"We use that in the first equation."
"We now solve for S."
"The number of prey items encountered is then λ times S".

Depending on the next steps, one might point out that the rate of prey encounters overall is this result divided by T or that the rate of energy accumulation is this result divided by T times the energy obtained per prey item.

When making a set of slides with this derivation, my recommendation is that the equations appear one at a time and that only the most recent equation appears in black, with the others in grey (so that they are visible but it is clear which equation dominates focus). This allows the audience to focus on your most current aural cues while having weighted visual cues (black being more important than grey).

Remember, of course, that there will be some people for whom mathematics can never be interesting, but only sterile; it is their problem, not yours.

The 50 Minute Talk

We now come to the longest talk that most of us will give (except possibly for teaching a 2 hour block). Pretty much everything that we have discussed until now applies, but there are a few other considerations

- More time does not release you from considering time constraints: When I started interviewing for jobs towards the completion of my PhD, our friend Chris Friedrichs, a professor of history at the University of British Columbia, gave me advice that I have continued to pass on. It is: *Do not run over*. If you are told that the 50 minute talk includes 7 minutes for questions, then hone your talk to be 43 minutes, not 47 minutes. There is no better way of losing your audience, and possibly the job, especially with an afternoon talk, than going over the time limit.

- <u>You can get into detail</u>: A 50 minute talk allows you to get into detail, but a few experiences will show you that 50 minutes is way too short a time to allow you to get into all of the details that you want. Thus you must still think carefully about which details are essential to your presentation and which you can simply mention (aural cue) and then show (visual cue) a citation for the relevant work. Knowing into which categories details fall depends on knowing your audience. If you are giving a talk involving DNA and mention the Central Dogma, you will treat this differently with an audience of molecular biologists, applied mathematicians , or general public.

The Far Beyond

Wenow briefly touch on the choice of kind of career (academic versus non-academic being the first point of bifurcation), the size of the research group you find yourself in, and funding.

The Impostor Syndrome, The Importance of Stupidity in Science, and Post-Decision Regret

All of us remember the time when we had started calling ourselves by our scientific discipline, as in "I am an ecologist" or "I am a mathematical biologist", but thinking that we were really not and somebody was "going to find us out". This is called the impostor syndrome, a term coined in 1978 by two psychologists (Clance and Imes 1978). Avoiding a technical, psychological definition, it goes something like this: even though you have been successful by the usual metrics, you somehow are convinced that you don't belong here (wherever here is), that you are some kind of a fake or fraud. You may even find ways of explaining your success as good luck or people not really paying attention.

When we first encounter the impostor syndrome, we think that with time, it will pass and we will feel more comfortable as a scientific professional. But it does not; more importantly *it should not*. To advance in science as individuals we must push to work just below the limits of our abilities (this is why I dissected “good at math”) – Peter Medawar called this the art of the soluble (Medawar 1982). The idea is that nobody gets rewarded for trying something and consistently failing, even though it is almost always true that some failing is essential for the road to success.

Sometimes, we will jump over that limit and try something that does not work out. So get used to it – it means that you are doing the right thing. Schwartz (2008) emphasizes the same when he points out the importance of stupidity in science. The great physicist Paul Dirac is reputed to have said that there are four stages following a discovery. First, you think it is wrong. Second, you think it is right but everyone knows it. Third, you discover that everyone does not know it and you have made a contribution. Fourth, you berate yourself for all the wrong turns you took and all the mistakes you made on the way to the discovery.

In summary, if one works around the limit of one’s abilities, then the impostor syndrome will never go away, and that’s the way you want it to be.

This is as good as place as any to discuss another psychological phenomenon called post-decision regret, which is also called cognitive dissonance theory (Festinger 1962). Again, with as little technical jargon as possible, this means that whenever you make a decision – no matter how carefully you have thought through it – after you make the decision you will feel that you have made the wrong choice. Whatever the deeper reasons, it seems that the proximate cause is clear: in making a decision, a choice, you have closed off one path in favor of another, and one will always wonder about the other path. Robert Frost summarized it beautifully in his poem (https://www.poetryfoundation.org/poems/44272/the-road-not-taken).

The Road Not Taken
by Robert Frost

Two roads diverged in a yellow wood,
And sorry I could not travel both
And be one traveler, long I stood
And looked down one as far as I could
To where it bent in the undergrowth;

Then took the other, as just as fair,
And having perhaps the better claim,
Because it was grassy and wanted wear;
Though as for that the passing there
Had worn them really about the same,

And both that morning equally lay
In leaves no step had trodden black.
Oh, I kept the first for another day!
Yet knowing how way leads on to way,
I doubted if I should ever come back.

I shall be telling this with a sigh
Somewhere ages and ages hence:
Two roads diverged in a wood, and I?
I took the one less traveled by,
And that has made all the difference.

Academic or Non-academic Positions?

The Science Council (http://sciencecouncil.org/about-us/) is a broad-based organization of professional bodies and scientific societies. One of their web posts (http://sciencecouncil.org/about-science/10-types-of-scientist/) lists 10 kinds of scientists: 1) the business scientist, 2) the communicator scientist, 3) the developer scientist, 4) the entrepreneur scientist, 5) the explorer scientist, 6) the investigator scientist, 7) the policy scientist, 8) the regulator scientist, 9) the teacher scientist, and 10) the technician scientist. I list these without going into detailed descriptions, which can be found on the web site, to remind you that there are many different pathways for one's career.

A career in Pasteur's Quadrant can be consistent with any of the kinds of scientist identified by the Science Council. One should start a career in Pasteur's Quadrant as soon as possible. That is, don't say "Once I have achieved - - [fill in the dashes], I will start doing important applied work". Rather find an important applied problem early in your career, build you disciplinary skills while seeking fundamental understanding when solving this problem, and implement the solution in an interdisciplinary manner.

Should one choose an academic career? I believe that there is only one compelling answer to that question: because you feel that you have to teach. This does not mean i) you don't mind teaching, ii) like teaching, or iii) that you want to supervise research students. Rather it means that part of you will feel empty if you are not teaching formal courses, i.e. that teaching formal undergraduate courses is part of the essence of who you are.

Although I knew this about myself at a very early age, I only figured out how to articulate the point about teaching being part of my essence towards the end of my career. All of us have been in a classroom when the instructor did not want to be there, and know how quickly the students sense this. Similarly, many (perhaps not all) of us have had very demanding instructors who clearly cared about the class; the students generally accepted the demands with good cheer. I chaired three departments in the University of California and sometimes had colleagues tell me that they received poor teaching evaluations because they were so demanding; however they clearly emanated a sense of not wanting to be in the classroom at al. Being demanding was simply an excuse that they used.

If you feel that you do not have to teach, or work on a very specific organism or system, then work outside of academia has the promise to be as exciting and satisfying as within academia and much less stressful, with higher pay, and a potentially more collegial environment than within academia. Indeed, since most faculty do not have experience outside of academia, they think that outside of academia one has "no freedom" and it is a cutthroat world. But, as one of my post-docs, whose partner worked outside of academia, noted to me many years ago, in academia the focus is the individual CV, whereas in non-academic setting the focus is the team. That is, in academia one advances or not according to the individual CV, whereas in non-academic settings, one advances according to whether or not the team succeeds.

The freedom to choose a particular research project is surely greater in academia, especially if one does not seek external funding. But once you have received grant or contract for a project, you are constrained to work more or less on that topic – at least for the length of the grant or contract. Thus, the nature of academic freedom is perhaps over-stated. Of course, if you really want to work on mating strategies in preying mantids, then a job at a fisheries agency is probably not a good match; on the other hand if what excites you is mating behavior and its effect on population dynamics, then a job at a fisheries agency could be a very good fit.

Doing interdisciplinary work still faces more challenges in academic settings than non-academic ones in large part because the structure of universities silos disciplines (regardless of the amount of talk about interdisciplinary work). Cronin (2015) noted that his university enabled "rewarding transdisciplinary work", where rewarding is used as an adjective, but that it was still resistant to rewarding transdisciplinary work, where rewarding is used as a verb. As he noted, this remains problematic for those doing interdisciplinary work.

Leaving and Returning to Academia

There is great virtue is obtaining experience outside of academia. Thus even if you are certain that you do need to teach, consider spending time outside of academia. This raises the question of how one returns to academia. You will almost surely take a cut in salary to leave a permanent non-academic position to an academic one (my salary went down by nearly 50% when I left the Operations Evaluation Group of the Center for Naval Analyses Mangel (1982a) for UC Davis in 1980).

As of this writing, the gold currency in academia is still peer-reviewed publications, so if you think that you want to return to academia keep publishing even in your non-academic position. Everyone will understand if you do not publish as much, but there is a big difference between 1 or 2 papers a year and 0 papers a year. Be sure that the work from your PhD thesis is published.

You do not need to focus on the highest profile, splashiest journals, but focus on the top disciplinary journals. I was the opponent for the PhD defense of Nic Jonzen at the University of Lund in 2001. While there, I learned that whenever a person had a paper accepted in *Science, PNAS* or *Nature* at Lund they celebrated with a bottle of champagne. But the same happened with papers in *The American Naturalist, Ecological Applications*, or *Ecology.*

Continue to go to scientific meetings, even if you do not present your non-academic work. As in all things in life, networks matter and it is important that you maintain contacts.

Become Part of a Group – But Not Too Big of One

Caldera (2016) reviewed multi-authored papers in *Environmental Research Letters* and found that 61 of the top 100 papers had four authors or less (Figure 2). He concluded that that “small groups of authors often produce the work with the greatest impact, even in an inter-disciplinary setting. This suggests that it may be wise to institute policy changes that discourage inflation of author lists and that encourage the funding of research conducted by single investigators and small groups of researchers.”

von Tunzelmann et al (2003) found that in many scientific fields per-capita productivity peaks at group size of 6-8 individuals, after which there is little per-capita gain. They noted that the threshold for the optimal productivity varies with discipline (e.g. lower in the humanities, arts, and pure mathematics and higher in clinical medicine).

Other researchers have found that there is little relationship between the size of a research group and its per-capita output (Cohen 1980, 1981, 1991). For example, Cohen (1980, 1981) found in research groups ranging from 1 to about 50 scientists, the addition of one scientist to a group increased expected annual production by 1.1 papers and that a 0-truncated negative binomial distribution, which is the steady state frequency distribution for stochastic models of freely forming primate societies, fit the data very well. Cohen concluded that there is no reliable evidence for an optimal group size to maximize per capita research production.

Thus, you have to find your own way in selecting group size. One helpful thing is to do the Myers-Briggs personality test (https://www.myersbriggs.org/my-mbti-personality-type/mbti-basics/home.htm?bhcp=1). Knowing your personality type provides a clue to the kind of group (and mentor) that will allow you to flourish. Also learn how authorship is assigned in the group (Duffy 2017) – for example, will the group leader on every paper as senior author even if his or her contribution is minimal and you have your own post-doctoral money (“but you were sitting in my lab”)? Finally learn how much time you will have to explore your own ideas versus working on those of the leader of the group.

Ethical Issues in Funding

Here's a headline from the *New York Times* on 31 Dec 2016 "Scientists Loved and Loathed by an Agrochemical Giant" with subtitle "With corporate funding of research, 'There's no scientist who comes out of this unscathed.' "

The article describes how the agrochemical company Syngenta funded research by James Cresswell at the University of Exeter to study the increasing rate of mortality of colonies of bees, and whether this was due to a the mite *Varroa destructor* or to pesticides (that is, natural or anthropogenic mortality). According to the article, Cresswell had been skeptical about the pesticides (perhaps why the company approached him), but his research kept pushing him away from the mites as the cause. The result of this was that the company pressed him to consider new data and a different approach.

About these interactions, Cresswell noted that Syngenta had its own agenda, which was that he was supposed to show that the problem was the mite, not the chemicals. The article concludes "A review of Syngenta's strategy shows that Dr. Cresswell's experience fits in with practices used by American competitors like Monsanto and across the agrochemical industry. Scientists deliver outcomes favorable to companies, while university research departments court corporate support. Universities and regulators sacrifice full autonomy by signing confidentiality agreements. And academics sometimes double as paid consultants."

If you work in Pasteur's Quadrant, this ethical issue may arise and you need to be prepared before difficulties occur to face it. Remember that the closer an issue is to fundamental human goals and aspirations, the more difficult it is to separate scientific conclusions from other influences (Ludwig et al 2001).

Similar questions arise in taking funding from the military or fossil fuel industries – not necessarily to work on problems of direct application to the military. For example, a considerable amount of the research concerning marine mammals and noise is funded by the US Office of Naval Research (and another large chunk by oil companies such as Shell and Mobile).

It is incumbent upon each of us who works in Pasteur's Quadrant to wrestle with these issues and to determine a self-consistent approach for funding.

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