



Food for Thought

Twelve easy steps to embrace or avoid scientific petrification: lessons learned from a career in otolith research[†]

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Campana, S. E. Twelve easy steps to embrace or avoid scientific petrification: lessons learned from a career in otolith research. – ICES Journal of Marine Science, 75: 22–29.

Received 12 July 2017; revised 19 July 2017; accepted 23 July 2017; advance access publication 31 August 2017.

Prior experience is as much an eye opener in science as it is in life, and often results in increased efficiency, greater productivity and reduced stress. While some actions and behaviours must be experienced first-hand to be appreciated, there are some behavioural patterns that can be readily absorbed from others, allowing the learning curve to be shortened and the professional career enhanced. After nearly 40 years of scientific research in otolith and shark science, it is clear that some strategies and tactics worked well at advancing my career, while others were ineffective or even counterproductive. Targeted mainly at graduate students and early-to-mid-career scientists, this somewhat philosophical essay identifies 12 easily adopted scientific behaviour patterns that would have had a hugely positive effect on my career, if only I had known about them early on. My hope is that early-career scientists can take advantage of some of the hard lessons that I have learned along the way, thus avoiding needless mistakes in the process of becoming the best scientist that they can be.

Keywords: career lessons, media, scientific writing, writing strategies.

Science is fun and challenging, but it can also be more time consuming and more demanding of a “heart and soul” investment than some other careers. To many scientists, the scientific lifestyle can be all-consuming, in the same way that some business people can completely lose themselves in their companies, and artists can immerse themselves in their music. Indeed, a balanced lifestyle can be difficult to maintain; although some scientists manage to reserve significant portions of their day for non-scientific activities, many others choose (or feel obligated) to totally immerse themselves in their work. Interestingly though, an informal survey of about 100 scientific colleagues showed that those considered most “successful” (based on publications and reputation, not

personal satisfaction) were not necessarily those who spent the most time at it. In other words, success (however it was defined) was not a linear function of the amount of time invested. Ideally, I would have introduced intrinsic intellect and creativity as factors into this impromptu analysis, and thus been able to determine the residual contribution of clever planning and behaviour to career success. But that will have to wait for another time.

After nearly 40 years of scientific research, most of which was spent at Canada’s largest government marine science institute (the Bedford Institute of Oceanography), I have the luxury of looking back at my career in otolith and shark science and assessing the strategies and tactics that worked best at advancing my

[†]Food for Thought articles are essays in which the author provides their perspective on a research area, topic, or issue. They are intended to provide contributors with a forum through which to air their own views and experiences, with few of the constraints that govern standard research articles. This Food for Thought article is one in a series solicited from leading figures in the fisheries and aquatic sciences community. The objective is to offer lessons and insights from their careers in an accessible and pedagogical form from which the community, and particularly early career scientists, will benefit.

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career, vs. those that didn't. Few of the strategies would have been effective, or even appropriate, during my undergraduate years (1970s) at Dalhousie University in Halifax. In contrast, some of my most effective behaviours started early in my graduate studies at the University of British Columbia (Canada) in 1978, largely thanks to my PhD supervisor Norman Wilimovsky. Wilimovsky was not the easiest person to work with or study under, nor was he especially prominent as a researcher, but his lessons in work strategy have stood the test of time, and I still consider him to be the single best mentor I've ever had. Other than those scientific behaviour patterns that I developed to survive Wilimovsky's grueling fisheries biology course, most of my effective scientific strategies were developed later as the product of insight and/or trial and error throughout my professional life. I firmly believe that most would have had an even more positive effect on my post-graduate career, if only I had known about them early on.

This somewhat philosophical, but very practical essay, is targeted mainly at graduate students and early-to-mid-career scientists, although I would be very interested in hearing from more senior scientists about their views on the subject. My hope is that the scientists still developing their career can take advantage of some of the hard lessons that I have learned along the way, and thus avoid the needless mistakes that I (and many others) have made, in the process of becoming the best scientist that they can be.

Read, read, read

Every Friday afternoon at the Bedford Institute of Oceanography, I would grab a cup of coffee and head up to the library and spend a couple of hours in the reading room. There, I settled back into a comfy chair, and started scanning the week's new offering of journals. The Friday afternoon time slot became a regular routine for me, not because I had nothing else to do on Fridays, but because otherwise it was too easy to forget about the new literature in the continuous buzz of work demands on my time. Over the years, I heard the snide comments of a couple of the technical staff: "Oh look—there's Campana taking Friday afternoon to sit back and relax again, while we have to work!" And it was sometimes easy to understand their error in perception; how could one ignore the occasional emeritus scientist relaxing in a chair a few seats over from me, journal in his lap, eyes tightly closed, lightly snoring? But for me, scanning the journals was an interesting and integral part of getting to, and remaining, on top of my field. There was no fixed order for my reading; I would simply start with the nearest journal or listing in my Current Contents list and scan the table of contents of any journal that lay even remotely in my field of interest. If a title caught my eye, then I would go to the paper and read its abstract. Seldom would I read the entire paper right then and there; rather I would note the contact email of the author or download the pdf of the paper and store it for reading at a later time, usually at home. Fisheries and marine science journals were of particular interest of course, given my research field. But I paid almost as much attention to those journals that intersected tangentially with my own field. Geochemical journals, trace element chemistry and instrumentation, terrestrial population biology, statistics journals, even astronomical journals—all were fair game for a quick scan of the table of contents. The generalist, high-profile journals such as *Nature* and *Science* were particularly valuable for this purpose. In the early stages of my career, the scans only occasionally led to a full-scale browse of an abstract; but when they did, they had the potential to lead to something very exciting. I've realized over the years that the best scientific advances often occur at the interface between

disparate fields, in part because this is the road less travelled. Most fisheries biologists will scan the fish and fisheries literature and glean the useful but incremental advances from them. Those are the backbone of science, and lead to its gradual progression. But it is the topics largely unfamiliar to the mainstream scientists in a discipline which can lead to the "breakthroughs," whether it is the conceptualization of a new theory of gravity (not so likely) or the adaptation of concepts and approaches used in other fields that can lead to new insights in your own field (much more likely). This happened surprisingly often, and I attribute a significant part of my innovative contributions to science to my regular perusal of journals in seemingly unconnected fields. Case in point: astronomical journals. I have a long-standing interest in astrophotography, not for its science, but as a hobby. But it was while scanning specialist journals for the latest astronomical discoveries that I increasingly appreciated the stunning imagery being obtained by NASA and other agencies. Reading further, I soon realized that many of the galaxies and nebulae being photographed were not particularly small; rather, they were very faint, making the images very noisy. So the impressive part of the NASA imagery was not so much the hugely expensive telescopes being used for the photographs, but the after-the-fact image enhancements which were used to bring out the information in the image. Why, I asked myself, could I not apply some of the same image enhancement methods to reveal the hidden information in otolith growth sequences? Otolith age readers often remark on the remarkably low contrast between the opaque and translucent zones; indeed, one of the characteristics of an experienced age reader is their ability to see and interpret exceedingly faint growth bands. So why not let modern technology—in the form of digital video cameras—capture the image, and modern image enhancement software—now present in the form of Photoshop, Image J, and others—highlight the structures of interest? It all seems very routine now (i.e. Campana *et al.*, 2016). But I can assure you that it was all greeted with considerable skepticism when the approach was first broached to the fisheries world in the 1980s (Campana, 1987).

Otolith are rocks, not bones. Otoliths contain none of the cells, blood vessels and connective tissue characteristic of bone, which makes the entire otolith (other than the growing outer layer) acellular and metabolically inert. Once deposited, otolith material is not resorbed, even if the fish itself is starving. And the otolith continues to grow even if the fish does not, due to the fact that 97% of the otolith material comes from inorganic elements in the water passing over the gills, not from the diet. These are the two features of the otolith which make it so incredibly useful to fisheries science, not just for the age determination of old fishes, but for the entire field of otolith chemistry. So if the otolith is a rock, why would a scan of geological and geochemical literature not be relevant? Again, this seems so obvious now, but back in the 1980s when otolith chemistry was but a gleam in my eye (and that of John Kalish, then doing his PhD at the University of Tasmania), it was not so obvious. Yet it was the reading of the geochemical and tracer journals that provided the seed thoughts for the entire discipline of otolith chemistry, both for me and (I suspect) for Kalish. There have been over 1500 scientific articles published on otolith chemistry since that time, making it a mainstream discipline used in stock identification, temperature reconstruction, migration studies, mass marking, and recruitment studies (Campana, 1999). And it all evolved as a result of reading journals "outside" of one's own field.

Ideas are the life blood of science. It is relatively easy to take an existing approach or concept from one's own field, and then

apply it to the specifics of your own research interest. I suspect that 95% of all science is done in this way; it is a valid approach, provides the system-to-system replication that science requires, and allows for slow, incremental growth in a field. But the big steps in science—the conceptual or methodological jumps that can create or transform a discipline—they require thinking outside of the box. And scanning the literature outside of one's field may be the single best way to achieve those innovative breakthroughs.

To be applied, or not to be applied: that is the question

Picking a research problem becomes increasingly easy as your career progresses. Not only does your skill set expand and your experience broaden, thus allowing you to tackle increasingly complex questions, but the research and funding opportunities increase as other scientists become aware of your expertise. Late in your career, you can basically let the research opportunities come to you, rather than having to seek them out. But early on in your career, selecting the right research question can be more challenging; you have to balance your interest in the problem, with your skill set, with your likelihood of funding. Some scientists would argue that these are the only issues that need to be considered. But I firmly believe that there is a 4th criterion that must be considered before you commit another few years of your life to a research study. And that is the question of the eventual impact of the research.

The holy grail of scientific research is a methodological or conceptual breakthrough that revolutionizes science. Most of us will never reach that pinnacle. The other extreme is what is often disdainfully referred to as “applied science;” research pursued to answer a routine question, usually in support of business, and without a conceptual advance. Both are valid, and appeal to different personality types (idealistic vs practical). But even within the avenue of basic research, I would argue that serious thought needs to be given to the end product of the research, and that preference be given to research which is likely to have an applied or conceptual impact. Indeed, all else being equal, why would you not select a research question that has a higher probability of leading to broad-scale theoretical acceptance or application? How could you possibly go wrong by selecting a research question that might lead to application by other scientists or industry, vs. an equally interesting question in a system that no one else will ever care about?

For example, I was faced with two equally interesting research questions as I left my MSc research to move into a PhD. One question involved the search for a causative agent behind a lethal skin tumour in certain species of flounder, while the other question was a search for the causative factors behind the newly discovered otolith daily growth increments (daily rings) in young fish. Both questions were very appealing from a scientific point of view, in that both would involve descriptive and experimental science, and both would lead to conceptual advance. However, the flounder tumour issue seemed constrained to only certain species and estuaries, and thus seemed unlikely to lead to a broader understanding of, say, cancer. Nor did it seem likely to be a significant factor in fish population dynamics. Similarly, the handful of daily growth increment papers that had been published to that point (Pannella, 1971; Brothers *et al.*, 1976) were still very descriptive and speculative, and it was by no means certain that

they would turn out to be valid daily age indicators in any fish species, let alone all species. Nevertheless, they had the potential to be applicable to many species of fish. And if they turned out to be useful age indicators, I knew from my population dynamics classes that they could be used in a broad range of vital rate calculations, including growth and mortality. Thus they had serious application potential. And that's why I selected that question for further study. Was it applied research? No—not as it's usually defined. But it was research that was likely to lead to application, and I would argue that that should be one of the key criteria in selecting a research question.

There are two corollaries to the premise that one should prefer to do research on questions where the results are more likely to have an impact. One of those corollaries is that the research is likely to be much more media-friendly. And as I will discuss later, media-friendly is good when it comes to advancing your research. The second corollary is more of a question: are you more likely to produce a high-impact result by pursuing incremental science in a well-studied system, or by taking the road less travelled? In general, I would suggest that it will be more difficult to make significant progress pursuing a conventional question in a well-studied system. However, going where few have gone before always involves more risk, and high risk is not necessarily a good idea for a brand new scientist. More on that later . . .

Write, write, write

It was once suggested to me that an unpublished study is no better than one that was never done. Strictly speaking, that's not really true, since a scientist will often use unpublished research as a springboard to further research, which is then published. But in many cases, unpublished research is essentially lost to science; if there is no record of it having been done, then there is nothing for other scientists to learn from, or build upon. Even conference presentations tend to be too transient in memory for others to build upon. To my mind, a study is merely hobby research until it has been published in some form. The challenge, of course, is to write up a study while it is fresh in your mind, while not totally succumbing to the siren call of new projects beckoning with the prospect of far more entertaining work.

There is some truth to the old adage “Publish or Perish,” but its meaning is often over-interpreted. Taken at its extreme, it's certainly true that failure to publish anything over a period of years is tantamount to career suicide. However, even a very modest publication rate (once every 2 years) can sustain a successful career if the papers have a high impact. And at the other end of the spectrum, we all know scientists who have published a large number of mediocre papers, or split a study into too many marginally distinct papers, to the point where they are given little credence as serious scientists. So quality surely triumphs over quantity. But if the quality is there, more is definitely better than less. So how then does one maximize the number of “optimal publishable units”?

I believe that one of the key contributors to my career has been an ability to write up most of the scientific studies that I've completed. Is that because I find paper writing to be any more entertaining than anyone else? I doubt it. Writing is hard work, and at least for me, not much fun, even if it is satisfying to get it done and finished. There is no question that the finished product leads to an enhanced scientific reputation and thus greater reward, at least over the long term. But working towards a reward in the undefined future is far more difficult than working towards

immediate gratification. So I've adopted a number of motivators to keep me on track with my writing, and to keep my output high. Foremost of those is my personal reward system: I reward myself for writing.

My writing reward system starts at the beginning; sometimes the hardest part is just getting started. So I have to set the stage. First of all, I carve out a block of time in advance (ideally a full day, but otherwise a minimum of 2 h) where I have no other commitments. Second, I almost never write a paper in my office; there are simply too many distractions. So I'll head up to the library, or find a quiet corner on campus, or even out to a coffee shop; anywhere that is away from usual surroundings and people I know. The email and phone both get turned off, or at a minimum, the texting functions disabled. Anyone who thinks that they can split their focus between a paper and a text conversation with friends over Facebook is either a fool or deliberately reducing their writing output by >50%. After that is all done, I start the actual writing. My goal is usually very modest, something on the order of one paragraph for each half day, but it is a must-meet goal. Why so low? Because this is an achievable goal, even if I have writer's block. And as ridiculous as it sounds, it satisfies my goal-oriented personality, making me feel good when I meet it. If, as often happens, I find myself completing the paragraph well before the end of the day, I just keep going and write as much as I can through the day. On those delightful but rarer days when the writing is going smoothly and easily, I take advantage of it as long as I can, and keep going late into the evening, or even into the following days. The productivity of those good-writing days can easily match the output of an entire week of average days. But if it's just not coming easily, I can always stop after meeting my one-paragraph goal, and have the satisfaction of having done something useful. This is what I call my Stage 1 writing goal.

My Stage 2 writing goal is more ambitious, has a more lucrative reward, and is only invoked once or twice during the preparation of a paper. The goal might be something like: "write the entire Methods section of this paper today" or "prepare all of the figures for the paper today and tomorrow." The reward for meeting this loftier goal is something more than just mere satisfaction. I might treat myself to a dinner out at a good restaurant. Or I might buy myself a book or gadget that I've had my eye on. The key, though, is that I don't allow myself to claim the prize unless I actually meet the planned goal in the planned time. It's remarkable how motivating that can be when you're at the halfway point of the writing goal, and beginning to slow.

My Stage 3 writing goals are usually reserved for particularly challenging papers, such as crafting a manuscript for *Nature*, or rewriting a major grant application for a collaborator. Here, my goal is usually completion of a draft, or at least the major components of it, over a period of several days. The twist is that I leave town to do it. Sometimes that will mean going to a friend's cottage in the mountains—with or without the friend—and split my time between writing, hiking, and fishing, with clear writing goals each day. Other times, it might mean travelling to another city, putting myself up in a nice hotel, writing intensely during the day, running or playing squash in late afternoon, and wining and dining myself (again, with or without friends) in the evening. Whatever the environment, the intention is to mix business with pleasure—intensive writing with pure entertainment. This type of approach has a monetary cost of course, although the cost need not be great. But for me, the satisfaction of being able to write

major components of a paper in only a few days makes the investment very worthwhile. Work hard, play hard—more on that later.

A final note on writing—although many scientists procrastinate in writing up their science, it's not solely due to a difficulty in writing. It can also stem from the sense of perfectionism inherent in many scientists: "This study really needs a few more observations," or "I should do another experiment to back up the results sometime." Admittedly, it's sometimes hard to draw the line on when to end the data collection phase of a study, since more is almost always better. But a line must be drawn eventually. One rule of thumb I've sometimes found helpful is to ask myself the question: "Will the addition of these new observations (or experiment) make the difference between this paper being accepted or rejected by my target journal?" If the answer is no, then I would tend to air on the side of publishing sooner rather than later, and using the additional time to carry out other studies. There is simply too much risk that the wait for additional data will result in the study never being published at all. And that is a huge loss.

Grab opportunities, not baubles

Graduate students and young scientists are like crows—they are easily attracted to, and distracted by, bright shiny things in the form of opportunities. I was no different at that stage. As a newly minted PhD in 1983 armed with the expertise to reconstruct the life of young fishes using otolith daily growth increments, I soon found myself faced with a bewildering array of research opportunities. Even better (so I thought), I began to field calls from established scientists asking me to collaborate, and to bring my new skills with me. For awhile, this was both intellectually refreshing and great for the ego—I was actually in demand! But it wasn't until I continually found myself over-committed in research collaborations, that I realized that not all opportunities were golden, and that some weren't opportunities at all—they were merely bright shiny baubles which served to further dilute my time with no appreciable impact on my reputation or career. Time, I realized, was the major constraint on my professional life, even more so than funding. So I had to become much more discerning about differentiating between golden opportunities and the tin variety. Sometimes it was easy to tell—when a big name contacts you and asks you to collaborate on a study which will require one week of your time and will almost certainly end up in *Nature* or *Science*, it doesn't take too much thought to put aside the other studies that you're trying to finish and jump into the new initiative. But what about the continual contacts to collaborate on projects producing routine age and growth papers? Are they really worth diluting your effort on your other projects? So I started asking myself: if I start this new project, will it leave me further ahead in 5 years time compared to where I would be if I didn't do it, and instead focused on finishing existing projects? If the answer was no, or the endpoint was similar, the new project wasn't worth doing. With a fixed amount of time available for research, and assuming that you're already working at full capacity, diluting your efforts with yet more projects will not increase your productivity. That was a frustrating lesson to learn, and one that many scientists seem to have trouble with.

There is a flip side to opportunities: they may arrive not in the form of a present, but in the form of a catastrophe. Unexpected disasters are inevitable in any scientific career; cooling units break down, killing off all the fish in a long-running experiment; ship-borne surveys fail to find the target species; or 3 weeks of

unprecedented winter blizzards destroy an entire field season. In many cases, it's merely a case of grin and bear it. But in all cases, and particularly if the event is particularly catastrophic, it's worth taking the time to search for the opportunity that is buried in the debris. And that opportunity is almost always there, albeit not necessarily easy to find. Take for example my destroyed PhD plans. After careful searching and planning, I had secured an opening to do my PhD under Arthur Myrberg Jr., a world-class shark researcher at the prestigious University of Miami in the USA. The opening came with the university's top fellowship. All was golden, with the exception of having to leave my girlfriend behind in Canada. But with just 3 weeks remaining before I started, I received a late night call from my supervisor. He had just had a close call with another graduate student, who was almost bitten by a shark during a field trip. It was the last straw for Myrberg; he was giving up all shark research and taking up research on sound production in damselfish. He hoped it wouldn't affect my plans at all . . . Well of course, I was devastated! I wanted to study sharks! So I cancelled my plans to work under that scientist, turned down my large fellowship, and then found myself adrift. But after a short period of feeling sorry for myself, I started looking for the opportunities, and realized that I could get a great education in fisheries biology much closer to home, and thus acquire critical skills that I could later apply to sharks, if I were ever given the chance. Sure enough, 19 years later, my Director at the Bedford Institute of Oceanography approached me and asked me to start up a new program on any class of exploited but un-assessed fish species. So I picked sharks, and started up the Canadian Shark Research Laboratory in 1997. Armed with the expertise I had acquired in doing fish stock assessments and otolith research, I was soon able to tackle the most pressing issues in Canadian shark ecology and management with skills I almost certainly never would have acquired otherwise. I was way further ahead than I would have been if I had done a PhD on sharks. And it all came from turning adversity into opportunity. This has happened on a number of occasions since, and in each case, there were opportunities lying buried in the carnage of apparent catastrophes. So the take-home message is: when disaster strikes (as it will), go ahead and mourn, but then start digging for the gold . . .

Speak and network

Conferences are a great place to find others with similar research interests, and to make contacts and forge alliances that may not bear fruit for many years. They can be especially important for the shy and introverted. For me, networking is the primary value of conferences; the presentations are strictly secondary, and I seldom spend more than half of my time listening to talks. That being said, I will always give a presentation (preferably a talk, but otherwise a poster) at any conference I attend. My goal is not so much to share research results, but to make myself visible to possible collaborators. Presentations are easy for me now—fun even—having given >200 to date. But that was certainly not the case in the early days of my career. I rehearsed for days prior to my first major conference presentation (at the Canadian Conference for Fisheries Research in Ottawa in 1982), and had detailed written notes to back me up if my memory failed. Despite being incredibly nervous, I felt that I was totally prepared, and that nothing could go wrong . . . so when I stood up in front of the 200+ scientists and started to speak, I was alarmed to realize that my brain had vacated my body, and I barely remembered

a word of my memorized talk. Luckily though, I had my notes. That is, until my shaking hands dropped them, and the pages got mixed up on the floor. After what felt like the most terrifying 10 min of my life, as I tried to find where I had left off, I managed to resume my talk and finish in the allotted time. To my mind, it was a total disaster: everyone must have seen my nervous shaking, and the extended silence when I lost my place and dropped my notes was hugely embarrassing. At the first coffee break afterwards, when people started to approach me and say that they enjoyed my talk, and asked some follow-up questions, I just assumed that they felt sorry for me and were being polite. It was only later in the day, as established scientists continued to approach me and ask questions, that I learned that no one had noticed my hands shaking during the talk. And the 10-min interval of silence when I got lost? It was actually 10 s—and no one noticed. And even if they did, they didn't think anything of it. But what surprised me the most was the number of people who approached me to talk to me, far outstripping the number I had worked up the nerve to talk to in the days before my presentation. Suddenly, the networking for that conference was easy, in that I didn't have to do anything—people came to me. And it was all from giving a talk, and making myself visible. Even mundane talks are effective that way. So one of the few prime directives I have for myself and anyone else is: make a presentation at every conference you attend!

So what is the big deal about networking anyways? As a young scientist, I was far more comfortable working by myself or with a small group of my close colleagues, than by stressing myself by trying to talk to total strangers, or worse yet, famous total strangers. But I soon realized that even brief exposures to new colleagues can yield huge benefits down the road. Months or years later, they probably won't remember any details about you, but they'll probably remember your general area of expertise . . . especially when they realize that their project requires that type of expertise. And then they'll contact you. Or vice versa when you're the one leading the project. Collaborators are good. Not only do they bring needed skills into your project, but collaborations quickly form a positive feedback loop, in that they lead to even more collaborations, and more papers. It's a win-win arrangement for everyone. As for increasing the number of authors on the paper, I don't care at all. As long as I'm senior author, having more coauthors can actually look better for you. And if I'm not the senior author, the number of coauthors doesn't matter much, and is more than compensated for by additional papers that you will later be asked to join. As a result, I look forward to bringing collaborators into many of my projects, as long as they can make the resulting study better, or reduce my workload. Indeed, I will collaborate with anyone useful, even those I dislike, as long as I respect their abilities. But if they prove to be unethical or unreliable, then I will never ask them to work with me again. Ever.

Work hard, play hard

I don't think too many people make it to the stage of being a scientist without having worked hard. Nor do I think you have much control in terms of how hard you work; some people are intrinsically more driven than others. But what can be controlled is the balance between work and non-work, and how they interact. Early on in my career, I realized that I needed to integrate some non-work outlets into my weekly schedule; otherwise science would consume me and I would risk becoming one-dimensional. So I started a daily exercise/sports regime to manage

stress, and various hobbies to diversify my interests. Nothing unusual there. A more important realization was that I could often combine work and pleasure in the same package. Work trips abroad, or even just to the next city, became wine and dine opportunities during the evening, and chances to explore after the meeting. Work travel became an opportunity during which I could totally immerse myself in science without home responsibilities, which was immensely satisfying, and the days tacked on afterwards became a vacation. Conversely, vacations away with my family became the chance to spend the occasional hour reviewing papers or hatching new ideas in brilliant sunshine and exotic surroundings. Is there a colleague coming to town for some reason? Sure, we could arrange to talk in my office. But how much more fun it is to go to a local coffee shop or outdoor café or pub and talk in new surroundings. I can't tell you how many times I've come up with research plans with colleagues over a beer, and then written them down on cocktail napkins. I'd say 80% of those plans were later implemented and published. There's nothing wrong with having fun with your work.

I reserve the best work/play combinations for the most important issues. For example, I once decided to tack a week onto the end of the Australian Society for Fish Biology conference in 1995. After the conference, I booked a couple of rooms at a jungle resort in the heart of the Daintree forest, and invited John Kalish to join my wife and I there. We spent the next 4 days exploring the jungle, searching for possums and cassowaries at night, and engaged in serious wining and dining over dinner. We also spent hours each day around the pool in beautiful sunshine planning joint research programs and a review paper on otolith chemistry. Sounds like a trumped-up excuse for a luxury vacation, doesn't it? But that one trip led directly to my 1999 review of the field of otolith chemistry (Campana, 1999, with >1500 citations to date) and the now-popular subdiscipline of bomb radiocarbon dating (Campana, 2001). Not a bad result from a modest investment in money, wouldn't you say?

Follow ethics, not rules

I consider myself to be a very ethical person. It would never even occur to me to cheat someone, and I have done my best to never intentionally block or steamroll over a scientific competitor as my career progressed. But I'm no fan of meaningless rules, and I do not hesitate to disregard them whenever encountered. A few years ago, at the peak of scientific muzzling by a science-hating Canadian government, various arcane rules were put in place to stymie any reasonable effort by federal government scientists to attend scientific meetings or conferences. The 2014 Sharks International conference in South Africa promised to bring together the largest collection of shark biologists in the world, and it would have damaged my career not to attend. So I got approval for a week of vacation, used my personal money to buy a plane ticket and pay all expenses, and then registered for the conference without naming my government affiliation. The conference was great, and it led to an unplanned publication and two new collaborative research projects. However, once I got back, I was hauled into the Director's office and formally disciplined for attending the conference. In his view, I was not allowed to travel to a scientific meeting on my vacation using my own money. It was unethical, he said. No, I responded: your actions are unethical, and I stand by my actions. One year later, the science-hating government was out, and the travel rules were rescinded. The ethics of the situation never changed though.

Use the media to your advantage

Many scientists avoid media publicity, fearing that they might be quoted out of context, or that the resulting publicity might cheapen their science. This is a mistake. Although reporters are not out to be your friend, nor are they trying to be your enemy. They have their own agenda to fill, which is to interest their readers/viewers. So if they are going to "use" you to meet their goals, there is nothing wrong for you to "use" them to suit your own agenda. I like to think of it as mutual parasitism (rather than mutual predation). So how can the media help you? A media interview on your research is usually targeted at the public, and in many instances involving scientific stories, the educated public. The educated public, in turn, influences politicians and senior managers, many of whom will have seen the media report already. This influence is often casual and by word of mouth, but it's there. And although there may not be an immediate cause and effect, politicians and senior managers who see positive stories about their staff will often favour them (consciously or unconsciously) over the long term, especially when it comes to funding. So releasing a media story about your research actually increases your own long-term viability in science.

Admittedly, some stories and topics are easier to sell to the media than others. While leading the shark research program in Canada, it was absurdly easy to publicize our research findings. Almost everyone loves sharks, whales and dinosaurs. But long before I started shark research, I made it a practice to prepare media-friendly news releases (always including photographs) on any and all of my significant research findings, on any topic, such as those involving otoliths. In most cases, media outlets love such releases, since a well-written, easy to understand media release takes little time for them to edit and publish. And virtually any science story can be made interesting to the general public, even if the emphasis isn't on the major scientific conclusion. For example, I would often include photographs of otolith annuli, or daily growth increments, in any otolith story. These photos can be gorgeous, and they often catch the eye of the reader/viewer enough for them to pay attention to why they're being featured. Were the growth rings necessarily the main subject of the story? Maybe not. But as long as they were the tool used to do the research, their use was fair game. While reporting on my research on newly settled cod (Campana, 1996), I would sometimes include a photo of a 5-cm cod in my hand. Even fishermen have seldom seen such small cod, and they would be intrigued. And what if you're doing research on a more challenging topic; say, enzyme production in lump sucker livers? There's nothing wrong in headlining the article with a parallel to liver function in alcoholics. I firmly believe that any research finding can be packaged in such a way as to interest the public. More importantly, if someone can't explain some aspect of their research in such a way as to interest the public, then I think they are hurting themselves.

If all of your research is successful, you're not taking enough chances

Any competent scientist can do routine science. And indeed, routine incremental research is the backbone of science. But bigger advances usually involve risk, since they almost always take you into an area where no one else has paved the way. In general, the larger the potential payoff, the bigger the risk. High risk is not necessarily a good idea for those at a very early stage of their career, such as a graduate student, since failure to produce a result

could be hugely detrimental. But in general, the top-rate scientists—those that have made breakthroughs—have also suffered a number of major failures. In their case, failure is a badge of honour, since it shows that they were willing to take risks in their attempt to open up a new area of science. Of course, most scientists don't publicize their failures, but if the risk had worked, you would certainly be reading about it in *Nature* or *Science*. For example, molecular biologist Paul Bentzen (head of the Marine Gene Probe Laboratory at Dalhousie University, Canada) and I put some effort into trying to extract DNA from the interior of an otolith shortly after the movie *Jurassic Park* aired. Fossil otoliths would have been comparable to amber in DNA conservation potential, and hugely superior to fossil bone. Success could have meant a source of less-degraded DNA stretching back millennia. However, the attempt was a complete failure. Bronwyn Gillanders (marine scientist and otolith expert at the University of Adelaide) and I poured considerable time and money into a comprehensive series of experiments on the incorporation of rare earth elements into otoliths. The goal? A search for new and easy-to-apply mass marking agents. We found nothing (or at least, nothing exciting). In another study, Simon Thorrold (Woods Hole Oceanographic Institution), Hans Høie (University of Bergen) and I dropped everything in a race with global physical chemists to explore temperature fractionation in stable calcium isotopes. Can you imagine how perfect otoliths would be for such a temperature reconstruction? Not only are they metabolically inert with a 38% calcium content, but the fixed environmental isotope ratio would mean that any temperature reconstruction would be unhindered by the environmental water composition variability that complicates the use of stable oxygen isotopes. It would have been a game changer in science. But instead it led nowhere.

My failures leave me with no regrets. I've realized that if I don't have some failures along the way, I'm not taking enough risks.

You make your own luck

I've sometimes heard the comment, "Wow, he was lucky to get that award," or "She was lucky to be there at the right time to make that discovery," or something similar. The comments are always tinged with envy, a bit of bitterness, and never come from very successful scientists. And admittedly, the precipitating event was often one in which there was a random element of chance involved. But in fact, it was never totally random; in almost every circumstance, the scientist involved had loaded the dice such that their probability of success was elevated over that of others. How did they do that? By thinking ahead and recognizing opportunities when they saw them. She wasn't lucky to find that new genus or uncover that new process—she organized the cruises, she went out of her way to get new funding, or she put extra care into selecting her cadre of capable graduate students. I am a firm believer in the adage that good luck comes from within, and often reflects past choices.

Don't try to win: try to win-win

Science has always been competitive. Even when working in a sparsely populated field, there is usually the desire to compare favourably with one's peers. However, the element of competition appears to have taken a sharp turn upwards since the 1980s, as the number of peers (and potential competitors) has increased exponentially. Perhaps more importantly, competition for limited research funds has become more severe, with many prominent

national funding agencies boasting rejection rates of >90%. As a result, it has become increasingly difficult to avoid competition, even if you want to. However, there are a number of scientific interactions where there don't have to be losers; everyone can walk away a winner. For example, a recent funding call seemed to be well suited to my research interests, and I started preparing a funding proposal. As I started preparing the proposal though, I heard that two separate sets of European colleagues were preparing proposals along the same lines as me, against the same funding call. Although I respected the work of these colleagues, I did not know them; they were not friends. So one option would be for each of the groups to prepare separate proposals, with almost complete certainty that only one would be funded. There would be one winner, and two losers. The other option was for me to contact the other groups and see if we could collaborate on a single, joint proposal. The total budget would be much larger, albeit not quite the sum of the three individual budgets. But our probability for getting funded was also much larger than for any single proposal, so our net cost-benefit was actually larger than if all of us had submitted independently. We ended up getting funded, and our joint proposal was much stronger than it would have been otherwise. Nor were there any losers in the game. I call these win-win interactions, and whenever possible, I reach for them. Win-lose dealings may make you feel superior for a fleeting moment, but win-win arrangements almost always lead to better relationships down the road, not to mention more collaborative research opportunities. I don't consider myself to be much of a people person, but there is no question that it is both more productive and more pleasant to work in an environment where your less-successful competitors are not trying to stab you in the back.

Stay fresh with your enthusiasm for science

Many scientists seem to fall into one of two research camps: focus on one issue for most of a career, becoming increasingly expert in that one field of enquiry; or follow one's curiosity, jumping periodically from field to field, wherever the curiosity may lead. As with most things in life, it's not strictly black and white; there is a continuum. Personally, my research strategy lies in between the two extremes. But whatever strategy is pursued, I consider it very important to remain interested and excited in whatever it is that you're researching. Through much of my career, I looked forward to going to work, or at least the research side of my work. But once a particular project had been finished, and the obvious spin-offs in terms of papers and funding had been completed, I always asked myself the question, "What next?." This was not always an easy question to answer, since like most scientists, I have multiple projects on the go at any given time. And it is very seldom that a question is fully answered, or an issue fully resolved. There are always loose ends. And those loose ends can often be turned into more papers and more funding. Some scientists continue to tie up those loose ends as long as they can, for many years. But for me, it reaches a point where I start to get bored. And I hate being bored. So I periodically find myself switching disciplines, at roughly 10-year intervals. First it was otolith microstructure. Then shape analysis. Then otolith chemistry. Then bomb radiocarbon. But wait, you say: those are all targeted at otoliths. And you're right. But they are very different disciplines! The otolith chemistry that forms the basis for many stock identification and migration tracking applications is true trace element chemistry, and requires a completely different skill set, theory,

instrumentation and literature background than does, say, otolith microstructure. Similarly, the skills, applications, instrumentation and theory associated with bomb radiocarbon age validation is based on global carbon sinks, oceanic circulation and metabolic pathways, and thus is completely different again. The end result (for me) was a switch to a totally new field of enquiry, with new challenges, problems and potential goals. And that is exciting! Yes—it's gratifying to get to know a sub-discipline of science so well that others around the world turn to you for advice and collaboration. But if you're not enjoying what you're doing, it's time to switch.

So what about your shark research, you ask? Sharks don't even have otoliths! Well, the shark work is a perfect example of what I discussed above, and is not the aberration that it may appear. In 1997, 17 years into my research career, things were going well. But it was all about otoliths, even though I had fingers in every diverse aspect of otoliths that could be imagined: daily increments, annual growth patterns, bomb validation, otolith chemistry, growth models, shape analysis, fossils, fish hearing, species identification, diet reconstruction and others. It was all good science, both challenging and fun. But also intellectually intense. So I was ready for something totally different—something that was completely unconnected with otoliths, in fish that didn't even contain otoliths, that was not based in the lab, and less demanding intellectually. Something fun and exciting. Like sharks! So I jumped into the shark world purely as a diversion; a side branch to periodically relax my mind. Little did I realize that there were so many interesting research questions to be answered there, and that I would be drawn into that scientific world so completely. Why, much of the shark age literature to that point seemed to be out of the Dark Ages compared to the otolith world! The end result was a completely new—and exciting—research direction for me, and one that completely refreshed me in all aspects of my work.

My most recent step to maintain my scientific enthusiasm wasn't really a change of scientific direction, but more a change of lifestyle: I moved. Until 2015, my entire scientific career had been based out of a single location, the Bedford Institute of Oceanography, which in this day and age, is somewhat unusual. Despite the global nature of my science and collaborators, residing in one location for so long simplified my work, since my non-scientific network of friends, colleagues and contacts (i.e. in the

fishing industry, government, and media) extended throughout Canada. But the combination of the previous science-hating government and the increasingly routine nature of my federal government work led me to abandon all things familiar, and move to a completely new environment: Iceland. The shift was not just geographic; I also shifted from being a government scientist to a university professor designing and teaching new marine science programs and fisheries courses. It was a huge upheaval in my life, and just what I needed to regenerate my enthusiasm in life and career.

Variety is indeed the spice of life, so my message to anyone who will listen is: don't let your science get stale. If you're beginning to get bored or if your work is getting routine (even though successful), it is probably worth diversifying.

Acknowledgements

I thank Howard Browman for inviting me to write this essay, and giving me licence to focus on the broader issues that have helped me shape my career as a scientist. It is not often that you get to give serious thought to such matters, and have such fun doing it!

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Handling editor: Howard Browman