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Food for Thought

More than a fair share of good luck

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This article deals with reflections on a career as a fish biologist that has spanned 40 + years. I provide insights into lessons I have learned over the years, describe some research successes and failures, and end by drawing the conclusion that variety in teaching and research is the spice of academic life. The lessons I have learned might be a guide for those at an early stage of their scientific career:

You should try to recognize where your talents lie and what your weaknesses are: play to your strengths, but do not try to hide your weaknesses.

Collaboration with perceptive colleagues and receptive associates is the foundation upon which an academic career is built.

Research should be designed to address a problem, not apply a technique; use the technology, and do not submit to the temptation of letting it use you.

The most rewarding pieces of research are often those that produce unexpected results; you learn more from having your preconceived ideas challenged than by having them confirmed.

It is important to know who your readers are, and to select the most appropriate channel for publication of your work. It is not necessary to publish in high impact factor journals to get your work read and cited.

You are likely to shift your focus many times during your career; you must continue to grow to remain fresh and enthusiastic.

Your students are your scientific legacy; nurture them well because there is nothing more satisfying than to see them succeed.

I offer a final piece of advice: Should teaching and research cease to be fascinating and fun, move on; do not risk becoming a square peg in a round hole.

Keywords: aquaculture, bioenergetics, eco-physiology, fish biology, growth, marine fish, salmonids.

Introduction

This article is a personal reflection of my 40 + years as a fish biologist with a career in teaching and research. The narrative provides some insights into the lessons I have learned over those years. Although I am sure that each of us has a unique story to tell about how and why we entered academia, we may agree that there are a few universal rules that can guide young scientists.

Let me start this retrospection with a couple of confessions. Scientific research has neither been my main interest nor the major driving force of my academic career. That is not to say that I have considered research to be a necessary evil. I reserve the term necessary evil for the administrative duties that occupy an increasing amount of time as academia becomes more regimented and bureaucratic, and as I have climbed the academic ladder. Research has been, and continues to be, fun; but in my psyche research plays second fiddle to teaching. As a result, I think of my book contributions as being my most satisfying and rewarding academic works (e.g. Jobling, 1994, 1995; Houlihan

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.

et al., 2001; Le Francois *et al.*, 2010; Huntingford *et al.*, 2012). It is, however, important to realize that teaching and research are not separate entities. I think of them as Siamese twins, sharing much in common and with a need for synergy between the two. My second confession is that very little in my academic career has been planned. In writing this I do not admit to having been a gadabout, it is just that so much in my career has been serendipity.

Early inspiration, cold feet, and U-turns

When I started secondary school on Tees-side, in the industrial heartland of the north–east of England, I was fortunate to be taught biology and chemistry by two young and dynamic teachers who did not play things according to the book; both were enthusiastic expounders of "learning by doing". In chemistry classes there was a focus on the running of simple experiments that were written up as short scientific articles in the IMRAD format, and biology was an observational science, both indoors and out. Inspired, my career course seemed set; I looked to follow in the footsteps of these two early mentors and become a secondary school teacher. In senior school, I was given access to the teaching laboratories and could work on small projects: looking at the metabolic rates of insects, measuring enzyme activities and trying out a few histological techniques, for example. All of this stood me in good stead for the transition from school to university.

At the University of Hull, I took a B.Sc. degree in Botany and Zoology, and followed this with a year of teacher training. This, to prepare me for what appeared to be my destiny. I was soon to receive a rude awakening; a full-time teaching post in an inner-city secondary school on Tees-side was neither Utopia nor Shangri-La, far from it. In my first, and only, secondary school teaching post I was timetabled to teach more mathematics and English than biology and chemistry, and many of the students were difficult to motivate. In less than a month I had become disillusioned and was looking to escape. In desperation, I contacted my former tutor at the University of Hull for both solace and advice; with hindsight this is one of the best moves I have ever made. He had his Ph.D. in marine biology from the University College of North Wales, Bangor, and had friends and colleagues who were still at the Marine Laboratories in Menai Bridge. By chance he had been told that somebody had withdrawn from the M.Sc. course in marine biology at the last minute, leaving a vacancy; did he know of anybody who might be interested? Although my leanings were mostly botanical, my undergraduate studies had also re-awakened a childhood interest in marine animal life, so I applied and was offered the place on the course. As a result, I resigned from my school-teaching position after less than two months, much to the chagrin of the head of school, and set off for Bangor.

Seeing the light and becoming an ichthyophile

Little did I realize when I started my M.Sc. studies in Bangor that the experience would lead to both a life-long passion and open the door to a career in academia. When I arrived at the Marine Laboratories in Menai Bridge, I intended to take my M.Sc. thesis project on macroalgae. By Christmas all had changed; I had become fascinated by fish, and had become an ichthyophile. This is a calling I have followed to the present day.

How did the transition come about? A case of serendipity. Following a hockey match the team adjourned to the local pub for a post-match pint or two of beer. By chance, I was sitting at the same table as Dave Grove, who was later to supervise my thesis work and become an inspirational scientific sparring partner during the formative years of my career. Thus began my conversion from plant ecologist to fish biologist. Within weeks, I was spending quite a lot of time in the Nuffield Fish Lab, and had been introduced to the fish physiologists Ragnar Fänge, Steffan Nilsson, and Sue Holmgren, who were on a visit from the University of Gothenburg, Sweden. The atmosphere in the Fish Lab was very relaxed, with everybody being allowed to voice an opinion, be heard and be treated as an equal. There was also practical joking and I was the butt of one of Dave's. My knowledge about fish was extremely rudimentary so I asked Dave to suggest something I could read as a starter. When Dave gave the quick reply "Go and look at Fish Physiology by Hoar and Randall; you'll find it in the main University library in Bangor" I heard a couple of the doctoral students snigger, but I didn't know why they found the remark funny. I trotted off to the library to discover that Fish Physiology was a six volume treatise; I felt like the young apprentice sent to the factory stores by the foreman for "a pot of striped paint" or a "long stand". Having overcome my embarrassment, and suppressed my irritation, I started to browse through the tables of contents, and discovered that the books were a goldmine: I was particularly attracted to the chapters by Phillips (1969), Fry (1971), Gleitman and Rozin (1971) and Hasler (1971) and read these over the ensuing few weeks. One of the first things I did when I was awarded tenure, and was given a budget, was to buy a personal copy of this series of books. By this time, the number of volumes had increased from six to eight, and included the volume on bioenergetics and growth (Hoar *et al.*, 1979) that was my "scientific bible" for many years.

To cut a long story short, I did my M.Sc. project, not on macroalgal ecology, but on feeding and gastro-intestinal function in the dab, *Limanda limanda*. With the benefit of hindsight I can say that the project was too ambitious and the aims unrealistic. We were interested in searching for possible relationships between rates of gastric emptying and appetite return, using X-radiography to study gastric emptying and self-feeders to look at feeding rhythms. At the time both techniques were novel, and there were few studies in which fish had been the experimental animals. We had some background upon which to base the X-ray studies (Molnar *et al.*, 1967; Edwards, 1971; Goddard, 1974) and self-feeders had been used to examine the regulation of food intake and learning abilities of goldfish, *Carassius auratus* (Rozin and Mayer, 1961, 1964) and rainbow trout, *Oncorhynchus mykiss* (Adron *et al.*, 1973). The sub-project involving X-radiography went smoothly, but the study of feeding rhythms of a bottom-living marine fish using surface-operated self-feeders was fraught with difficulties. First we had to wean the wild-caught dabs to accept a dry pellet feed, and once weaned we had to train the fish to operate a trigger to obtain food. This took weeks; much exasperation and frustration, but our patience was rewarded in the end. The self-feeders were more like the contraptions drawn by the English cartoonist William Heath Robinson (1872–1944) than scientific equipment; they were made from spring-loaded tea dispensers, reed switches, rubber stop-bars cut from erasers and small permanent magnets attached to thin, swinging metal triggers. Flimsy wiring connected the self-feeders to event recorders wrapped in polythene sheets to protect them from the salt-spray that would have caused short-circuiting and corrosion. Self-feeder designs and reliability have improved substantially since then, and they have been used to study feeding rhythms and behaviour, and diet selection in fish, as well as being adopted as a feeding system in the commercial farming of some fish species (Jobling *et al.*, 1995; Houlihan *et al.*, 2001).

Needless to say, our first-generation self-feeders often malfunctioned, and it was not easy to obtain records of feeding activity over prolonged periods of time. Although this part of my thesis project fell flat on its face, it somehow aroused a passion in me for asking questions about what fish do, how they do it and why. The X-ray work from my thesis gave me my first scientific article (Jobling *et al.*, 1977). Dave and Ragnar used two of the figures from the article in their chapter on fish digestion published in the Fish Physiology book series (Fänge and Grove, 1979); as proof of ownership, my finger- and thumb-prints are on the X-radiographs used as Fig. 5 in the chapter. I had been too eager and clumsy when developing the plates and handled them before they were dry, thereby leaving an indelible impression. Such clumsiness when carrying out laboratory work has dogged me throughout my career, and I have never considered hands-on experimental work to be my forte.

Learning to know one's scientific self

Fortunately, scientific research is as much a mental activity as a manual one, and a key to success is to recognize where your talents lie and where you have weaknesses. I have always enjoyed writing, and prefer presenting data to collecting them. These are useful attributes to have when editing and reviewing manuscripts; activities that have occupied increasing proportions of my time in recent years (https://publons.com/author/18696/malcolm-jobling#profile). Over the years, I have edited a few books, conference proceedings and special issues of journals (e.g. Houlihan *et al.*, 2001; Le Francois *et al.*, 2010; Huntingford *et al.*, 2012) and have been involved in shaping the development of fisheries and aquaculture journals. It is a privilege to be able to use one's skills to serve the international scientific community in this capacity. I am grateful to the journal editors, particularly Alwyne Wheeler and John Blaxter, who took the time to help me hone my writing skills as an early career scientist. I hope that I am now paying back some of this debt. I have never considered myself to be a good administrator and organizer, although I have occasionally heard colleagues express the opposite view. This means that I have never actively sought administrative office, and it is only rarely that I volunteer to take the lead role on a scientific panel; nor will I accept the invitation to join a committee unless I feel that I can make an important contribution.

As time has gone by, I feel that I have developed into a whole-animal biologist who has a knack for tying together loose threads and weaving tapestries. More specifically, I think of myself as a fish eco-physiologist; I would define fish eco-physiology as being the branch of ecology concerned with the study of how the physiology of fishes is influenced by environmental changes. The principles and techniques of fish eco-physiology can be applied to both wild fish and those held in captivity as farmed species. I feel that I am more a synthesizer, than one who is able to conjure up new and original ideas. This means that mulling over the ideas of others, along with reading their works and discussing science with my peers and students has been a major stimulus for me throughout my career. More than a fair share of good luck has allowed me to carve out the academic career of my choice.

Formative years and becoming established

I was in Bangor for only one year, and then moved to the University of Glasgow to continue work on fish for my doctorate; looking at feeding and metabolism of the plaice, *Pleuronectes platessa*. As a doctoral student I was given a lot of freedom to formulate my research and pursue my whims and fancies. It was a relief not to be constrained by a rigid project description and the need to meet the strict reporting deadlines that is the lot of most doctoral students to-day. I enjoyed my academic freedom and independence immensely, and I am sure that I developed a wide range of scientific interests as a consequence.

My starting point for building upon my basic background in energetics and metabolism gleaned from reading relevant chapters in Fish Physiology (Phillips, 1969; Fry, 1971) was to turn to the classic works by Krogh (1916), Winberg (1956) and Kleiber (1961). I also continued my interest in gastro-intestinal function, and the medical journals Gut and Gastroenterology became regular reading; providing me with ideas, methods and models that I could apply to my fish studies (Jobling and Davies, 1979; Jobling, 1986). It is my experience that, to use modern parlance, "going outside the box" is stimulating, broadens horizons and opens up new opportunities for research. In parallel with the fish gut work, I started to run respirometry studies. A chance encounter with Ashworth's (1969) article on malnourished children set me on a track that led me to look at specific dynamic action (SDA) and possible relationships between protein metabolism, metabolic rates, and growth (Jobling and Davies, 1980; Jobling, 1981, 1985). There were also forays into the applied animal nutrition literature to obtain information about principles and analytical techniques (Maynard and Loosli, 1969; Halver, 1972). This stood me in good stead some years later when my research started to shift towards, and eventually become dominated by, studies with application to fish farming (Jobling, 1988, 2004, 2016). Following a stay of just over four years at the University of Glasgow I had a chance to move to Tromsø, in the north of Norway, and have been an incumbent at the university ever since. It was not my intention to carve out a career at the University of Tromsø. I had planned to stay for no more than three-to-four years before seeking employment in Canada; I considered Canadian universities and research institutes to be the centres at which the most exciting research in fish eco-physiology was being carried out.

Work during the early years in Tromsø involved developing courses in aquatic ecology, fish biology, physiology and aquaculture for students of fisheries science, writing up the manuscripts from my doctoral thesis and getting them published, and conducting research that underwent a shift in a more applied direction. After three years as a temporary member of staff, I was offered tenure, which I accepted, and three years later I was awarded a professorship; the post I currently hold. Some of the articles resulting from my doctoral work were well-received and resulted in invitations to hold conference talks and contribute chapters to proceedings and books (e.g. Jobling, 1985, 1986). My course teaching also developed over time. Teaching compendia were written, revised and improved to incorporate recent research findings, and this eventually resulted in invitations to write textbooks (Jobling, 1994, 1995). This was hard work, but I found it very rewarding; the necessity to focus on communication and explanation increased my awareness and resulted in me learning a lot. I think it unfortunate that many seem to feel that there is little incentive to write textbooks to-day because the tangible rewards are so few. The monetary rewards are not great, and in the competition for appointments and academic advancement the greatest emphasis is placed on research output. I still teach fish biology, physiology and aquaculture to students of fisheries science; I wonder if I may hold some sort of endurance record here? Course contents have been revised and updated, but the teaching philosophy has remained the same; a throwback to the lessons I learned from my earliest mentors in secondary school—"learning by doing". My experiences with reviewing and editing have given me insights into the publication process, and I try to communicate these to postgraduate students when giving a course in scientific writing; once again "learning by doing" are the watchwords.

Over the years, changes in my personal research interests, along with research funding trends and fashions, have led me into a variety of basic and applied research areas. Those studies have covered areas as diverse as nutritional requirements, biorhythms, fish metabolism and growth, fish as food, endocrine regulatory mechanisms, and gene–environment interactions (https://scholar.google.no/citations?user=NFoaMx4AAAAJ&hl=no&oi=ao; accessed 3 January 2017.). Such a diversity in research during a career spanning 40+ years is probably not unusual. It is essential to blossom and grow during your career, remain fresh and enthusiastic, and make sure that research continues to be both fascinating and fun.

No man is an island

When I look at my scientific production, it comes as no surprise to me that it is review articles and book chapters that top the list of works with most citations (https://scholar.google.no/citations?user=NFoaMx4AAAAJ&hl=no&oi=ao). My name is the only one on many of these publications, and this may give the false impression that I have been an academic hermit, working alone in an isolation cell of my own construction. All of you know that interactions with knowledgeable colleagues and enthusiastic students are an essential part of academic life; these interactions are your most important stimuli and drivers. I have been fortunate to work with students and colleagues more dexterous than myself in the laboratory, and these collaborations have resulted in the publication of several articles that have been highly-cited. It is particularly pleasing that some of these arose from thesis projects carried out by M.Sc. students. The works that have proved popular cover diverse themes, and it has been a surprise to me that some of these publications have been so widely-cited. I put this down to having a canny knack of being in the right place at the right time; serendipity.

One oft-cited publication that came as a surprise arose as a spin-off from the thesis work of my first M.Sc. student, Anne Breiby. Her project involved the investigation of the dietary habits of the squid, *Todarodes sagittatus*, in which sagittal otoliths present in the stomach contents were used to identify fish prey. The pH of the squid stomach is only very slightly acidic. This meant that recovered fish otoliths were rarely eroded, identification of prey was easy and otolith lengths could be used to estimate the size of the fish prey. As part of the project we had made a compilation of the literature covering the use of fish otoliths in dietary studies. When looking through this literature, I became concerned about possible sources of error when scat analysis was used for assessing predation on fish by marine mammals. Did fish otoliths isolated from scats present a realistic picture of the fish species consumed; the entire fish would need to be consumed for the otolith to appear in the scat? Did erosion of the otoliths in the acidic mammalian stomach introduce serious errors for back-calculation of prey size from otolith size? Could the data be used for a realistic examination of the bioenergetics of predation? We decided to run a very simple *in vitro* otolith erosion test by exposing fish otoliths to solutions that differed in pH and then measure their lengths at regular intervals.

We ran the test over the Easter weekend holiday period, and I presented the data in a short talk given at a meeting of Norwegian marine biologists during the following autumn. Following my talk I was approached by Torleiv Brattegard, who was the editor of the Norwegian journal Sarsia (now Marine Biology Research). He asked me if I would be interested in writing a manuscript for submission to the journal. Sarsia, named after the marine scientist Michael Sars (1805–1869), was produced by the Marine Biological Station, University of Bergen, and was a journal with a limited circulation and readership. That this article (Jobling and Breiby, 1986) has become so widely-cited serves as an indication that even the results of simple experiments published in relatively obscure, parochial journals have the potential to make an impact.

During the course of another M.Sc. project, in which Ilona Miglavs was looking at RNA:DNA as a biomarker of recent growth history, we became interested in the growth responses shown by fish following periods of reduced feeding; the compensatory growth phenomenon (Miglavs and Jobling, 1989). The results of the work did not convince us that monitoring RNA:DNA would serve our purposes as a biomarker of growth so we did not pursue this further, but we have returned to studies of compensatory growth on a number of occasions in the intervening years. For example, we looked at how compensatory growth could be exploited in capture-based aquaculture of Atlantic cod, *Gadus morhua* (Jobling *et al.*, 1994) and also used the compensatory growth response to examine the mechanisms that regulate body growth and composition in Atlantic salmon, *Salmo salar* (Johansen *et al.*, 2001). Both of these projects involved the participation of, and substantial input from, postgraduate students.

I was familiar with the use of X-radiography from my M.Sc. studies on gut physiology of the dab, *L. limanda*, but the introduction of particulate markers to replace dispersed contrast medium added an extra dimension to the technique (Talbot and Higgins, 1983). When combined with individual recognition of fish by tagging there were new opportunities to use X-radiography in studies of fish feeding and growth (Jobling *et al.*, 1995; Houlihan *et al.*, 2001). We were quick to adopt and apply this technique, and it became a cornerstone of our studies for almost two decades; these studies included our work on the effects of exercise training on salmonids. This work attracted a wide audience and resulted in practical applications to the fish farming industry. It is likely that most work that attracts attention and recognition is the result of researchers being fortunate enough to be in a certain place at the time an opportunity arises, being able to see that opportunity and then to pursue it with as much creativity as they possess.

During the course of his thesis research Jørgen Schou Christiansen had seen that Arctic charr, *Salvelinus alpinus*, exposed to flowing water seemed to grow slightly faster than those held in static water. At the time, the prevailing idea was that slow tangential water flows in fish tanks should be used to facilitate self-cleaning, but that higher flows should be avoided because they were detrimental to growth:

> "Farmed fish kept in raceways or tanks with a concentric flow must swim to maintain station. In so doing they are

> using up energy sources which would otherwise have been stored in growing body tissues. Farmed salmon do better in terms of food conversion and growth rate in static water where there is no directional flow. . . ." (Sedgwick, 1988).

I was, understandably, skeptical about Jørgen's claims and asked him to check his data. If the exercised fish were growing better this needed to be examined in more detail. When we carried out a full-scale study the results revealed that Arctic charr exposed to flowing water reduced haphazard swimming and aggressive behaviour, orientated against the current and formed schools, increased their growth rates and showed improved feed conversion efficiency (Christiansen and Jobling, 1990). Some of the most satisfying research is when your preconceptions are shown to be incorrect. Subsequently, follow-up studies were carried out on both Arctic charr and Atlantic salmon, *Salmo salar* (Jobling *et al.*, 1993), and we were able to make a contribution to the earliest work on the welfare of farmed fish. We recently returned to some of these data when preparing an overview of environmental requirements for farming of Arctic charr (Sæther *et al.*, 2016). Our work on exercise training of salmonids has been cited regularly over the years, but my feeling is that it has been somewhat neglected by recent recruits to the ranks of those who investigate fish welfare. If I have a criticism of early-career scientists, it is that many of them do not devote sufficient time and effort to exploring the early work that is the foundation upon which recent studies have been built. Perhaps this is the result of a pressure to produce rather than reflect, combined with the disturbing trend of some reviewers and journal editors to shun manuscripts that contain citation of articles that are of venerable age.

As a final example of an oft-cited article that owes more to chance than good planning and management, I will recount the tale of a piece of work I did with my Finnish colleague Juha Koskela (Jobling and Koskela, 1996). Juha and I were invited to teach on a practical course "Experimental methods in fish biology" for Finnish postgraduate students, and were asked to demonstrate the use of the X-radiographic technique. We opted to be ambitious; not only to demonstrate the technique but also to get the students to collect data that could show some of its applications. Prior to the course, Juha set up tanks with individually-tagged (PIT-tagged) rainbow trout, *O. mykiss*, some of which were fed in excess and others that were given a restricted ration. The aim was to use this design to collect data to illustrate the effects of restricted rations on hierarchy development and inter-individual variations in feed intake. We hoped to demonstrate this using X-radiographic measurements of feed intake collected on day 1 of the course. We planned to repeat the exercise on day 2 to demonstrate the negative effects of handling stress on the fish, using feed intake as the metric. The measurements made on day 1 gave results that met our expectations in full, but those from day 2 gave us a surprise; the fish had not reduced their feed intake despite being handled and X-ray photographed the previous day. Fortunately, we had not told the students about our expectations beforehand, so Juha and I did not lose face. We were quick to see that the unexpected result offered us the opportunity to run an interesting small-scale feeding trial, incorporating an investigation of compensatory growth. Juha had the chance to retain the tanks of marked trout for a number of weeks, so the same evening we mapped out an experimental design over a couple of beers in Juha's summerhouse. This was a piece of research work that was fun to do, but I must confess to being surprised that the resulting article (Jobling and Koskela, 1996) has been cited so often. Admittedly, our approach was novel and we grasped our opportunity when it came along, but the popularity of the resulting article still came as a surprise.

Mulling and musing

I will start these concluding comments by taking the risk of being accused of stating the obvious: Research should be driven by curiosity, the posing of questions and the desire to solve problems, rather than by the wish to apply a particular technology or technique. Techniques and technologies are there to help answer questions, not to determine the types of experiments performed and shape their designs. Nonetheless, it can be tempting to follow trends, jump aboard the latest technological bandwagon and try to find experiments that can fit a technique. For example, when I started my career molecular biology was a relatively new area of scientific specialization, and very few marine scientists and fish biologists were using molecular techniques in their research. All the molecular biologists I met were technique-orientated, and few seemed to know much about the animals they were studying. This was an anathema to me as a whole-animal biologist. It is probably also the main reason for me being slow to open up to molecular biology, grasp the opportunities offered by the techniques and incorporate them into my research. Although the situation has improved, I can still often detect cases in the published literature where technique seems to have determined problem, rather than the reverse.

As researchers, we all like a puzzle, and many of us also have a masochistic streak. We often find that research is more fun when the findings indicate that our working hypothesis is probably incorrect, and a return to the drawing board is called for. I have almost certainly learned more from being wrong than from being right; our work on the effects of exercise on salmonids is a prime example (Jobling *et al.*, 1993). Unexpected findings and outliers in the data should not be ignored, and hidden from sight by being swept under an intellectual carpet; they are most likely to be the inspiration for new experiments and the source of your next research grant application.

Throughout my career as a faculty member at a university I have been expected to perform teaching, research and administrative duties, but I have not been either equally motivated to perform, or equally proficient at, each type of duty. Administration has been my Achilles' heel and in recent years I have become increasingly aware that I am not particularly adept at playing the role of the bureaucrat. These days it seems as though senior faculty are expected to master not only the administrative duties required for the day-to-day running of their academic department, but also have business acumen and accountancy skills. They are expected to be adept at project and personnel management, be industrial entrepreneurs with knowledge about proprietary rights and patenting regulations, and to display their social and political awareness via membership of national and international scientific committees. There are also increasing pressures to popularize science through outreach activities, scientific journalism, blogging and other forms of social media, and open chat-lines with members of the public. More-and-more, I feel as though I am being coerced into becoming a jack-of-all-trades, with a severe risk of ending my career as a master of none.

These recent changes in academic life mean that it is an unfortunate fact that the pressures on early career scientists are much greater today than when I started my academic career. These days, young researchers are often constrained within the rigid framework of a project proposal, are expected to publish frequently in top-ranking scientific journals, must meet strict reporting deadlines, are expected to be scientific entertainers and social media gurus, and are all-to-often faced with demands to prove their worth. In my opinion, there are aspects of academic stewardship that are questionable, and should be challenged and debated. Is academia being steered on a safe and secure course, or can we expect to see wreckage on the rocks? Those of us who are nearing the end of productive academic careers must make sure that we provide adequate encouragement to the early career scientists who are to be our successors; we must mentor them well. Even when days are dark and little seems to be going right we must instill them with a sense of self-belief and convince them that their academic activities have far more positives than negatives. We must ensure that they are given sufficient time for intellectual reflection, we must strive to prevent them from succumbing to the temptation of resorting to dubious research and publication practices and we must make sure that they do not burn-out or become disillusioned before they turn 40.

As a Parthian shot; Variety in teaching and research is the spice of a life in science. Should you no longer find that teaching and research are fun you should leave the game and search for pastures new; do not risk becoming a square peg in a round hole.

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Thanks to Howard Broman for inviting me to write this short memoir. Such invitations are, however, a double-edged sword; both an honour and an indication that your days are probably numbered. Thanks to my close colleagues Per-Arne Amundsen and Jørgen Schou Christiansen for providing feedback on my draft manuscript; as ever, your comments served to hold my ego in check and impose controls on my idiosyncratic and quirky character. Thanks also to my mentors who saw something in a young, obstreperous biologist (= yob) that I was unable to see in myself. Final, a big thank-you to the unsung heroes and heroines, the *et alia*, whose important efforts and contributions are rarely fully recognized. I hope that this short memoir can serve as a small token of my appreciation.

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