ICES Journal of
Marine Science

ICES Journal of Marine Science (2017), doi:10.1093/icesjms/fsw228

Food for Thought

Know your organism, know your data†

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Mangel, M. Know your organism, know your data. – ICES Journal of Marine Science, doi:10.1093/icesjms/fsw228.

Received 23 October 2016; revised 18 November 2016; accepted 21 November 2016.

I review my career in marine science chronologically forward from the time that I decided to become a scientist to the present. Among other themes, I illustrate how much of my career was the result of recognizing good opportunities rather than specific plans, the role that search problems have played in my career, and the power of mathematical methods to allow us to find commonalities in systems appears totally different. I discuss in detail my involvement in the International Court of Justice between Australia and Japan concerning special permit whaling in the Antarctic and conclude with my current activities—showing that surprises can happen at any point in a career.

Keywords: behavioural ecology, fisheries management, international law, life history theory, search theory, stochastic dynamic programming.

Introduction

A retrospective such as this—regardless of one's stage in life—forces one to understand patterns and processes that shaped a career. For example, I was struck by how much of my career was the result of recognizing good opportunities rather than specific plans.

Although Stochastic Dynamic Programming (SDP—see below) teaches us that to understand life, we need to move backwards in time, I have written this essay going forward, from the event in middle school that caused me to become a scientist to my current activities. I have separated sections according to my education and early, middle, and late career. I discuss in detail my involvement in the case in the International Court of Justice between Australia and Japan in which the Court determined that Japanese special permit whaling in the Antarctic contravened the International Convention on the Regulation of Whaling. I close with a description of my current activities, which show that even late in our career we can be surprised by the directions research takes us if we are receptive to opportunities.

Search problems have been a theme through my entire career—but emerging in very different ways in different fields of application. Similarly, the methods of applied mathematics—particularly simple mathematics used in mature ways—have been important, since they show us commonalities in questions where one might otherwise only see differences.

In a short essay, many details—and people—have to be left out; I apologize in advance to the students, post-docs, and colleagues who helped shape my career for not mentioning them by name.

New York, Chicago, Urbana, and Jerusalem, to 1974

One's ideas must be as broad as Nature if they are to interpret Nature—Sherlock Holmes in A Study in Scarlet.

I was born in New York, but my family moved to Illinois when I was young and I grew up around Chicago. From an early age,

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I was interested in physics, chemistry (like many kids growing up in the 1950s and 1960s, I had a "lab" in our basement and did "experiments"), and biology (I loved growing and taking measurements on green beans and spent many hours walking along the shoreline of Lake Michigan). I was never "good at math" but recognized that mathematics was a terrific tool for science.

Sunday evening was an important family television time in America then and shaped many careers (e.g. Springsteen, 2016). When I was 11 years old, an episode of the television show *Bonanza* involved a young Albert Michelson attempting to measure the speeds of sound and light (http://www.imdb.com/title/tt0529603). This struck me as a great life: thinking about how nature works, being outside, and using mathematics (in the case of the TV episode, geometry). Indeed, after the episode, I borrowed one of my father's books on geometry and went to school the next day asking classmates if they had ever heard of the subject.

I graduated from high school in 1968 and went to the University of Illinois as a physics major, but committed to learning chemistry and oceanography, hoping to one day find a career in the marine sciences. I did not know about mathematical biology. The years of university and graduate school are filled with self-discovery; the most important event in 1968–1969 was meeting the girl, Susan Milke, who agreed to become my wife and has been my support, critic, and teacher since then.

In the fall semester 1969, I took an upper division course in Oceanography, taught by WW Hay, who is a pre-eminent paleo-oceanographer. His book *Experimenting on a Small Planet* (Hay, 2016) is a gem. I did not realize it then, but Bill was quite a radical—he taught us plate tectonics as if it were already well accepted, even though Takeuchi's book had appeared only 2 years before (Takeuchi *et al.*, 1967; Oreskes, 1999). For that course, we had to do a term paper and I focused on complex ions in seawater (complex ions had been one of my favourite topics in high school chemistry). With Bill's support and tutelage, this became my first paper (Mangel, 1971). Like many first papers (Holmes, 1991), this one had minor significance for science but was essential for my development as a scientist: I saw how one could use simple mathematical methods and existing data to obtain new insights about nature. I kept in touch with Bill over the years and consider him to be one of my most important scientific mentors, in addition to the earliest. Most recently, he, Don Marszalek, and I have been discussing the functional role of the test in foraminifera, something that they worked on together many years ago (Marszalek *et al.*, 1969; Marszalek, 1982).

In fall 1969, I discovered the books of Nikolai Rashevsky (Rashevsky, 1969). These books are still useful (Mangel, 2015) and the phrase "mathematical biophysics" captured all the things that I was interested in. We had a department of physiology and biophysics at the University of Illinois, so I wandered over there hoping to do an independent study on membranes, which I had become interested in during the oceanography course studying phyto- and zooplankton. I often wonder how my career path would have differed had I found instead the classic on island biogeography by MacArthur and Wilson (1967), visited the Zoology department instead and thus arrived at mathematical biology much earlier than I did. I found Rashevsky's books in a university bookstore, but might have easily just as encountered them in a library; there is great value in visiting libraries and bookstores and perusing titles and reading (or at least skimming) the entire issue of a journal—because we never know where the next good idea will come from.

I completed MSc in Physiology and Biophysics working on thin lipid membranes (artificial reconstruction of cell membranes to allow better control in experiments) and in 1971, went to my first scientific meeting—that of the Biophysical Society in New Orleans. There, I met an Israeli scientist, Asher Ilani, who also worked on membranes at the Hebrew University in Jerusalem. The 1970s were the first nadir of the job market for PhDs and I saw many of my friends completing PhDs and not finding jobs, so decided that I would use my graduate education as an opportunity to travel and asked Asher about doing a PhD with him.

We arrived in Jerusalem in July 1972 and I got right down to the work of learning Hebrew in the mornings and developing experiments in the afternoon. Three important things happened in the first 15-18 months there. First, I had a problem with my experimental system and could not get it to work for about a 6-week period. Second, I had already done some experiments that were qualitatively but not quantitatively reproducible, and wanting to do theory again, I made a visit to the Mathematics Library. I saw a book called *Brownian Motion and Diffusion* (Friedman, 1971), and thought that this would be perfect for modelling my experiments. The book begins: "**(1)Definition.** Normalized Brownian motion *B is a stochastic process* $\{B(t) : 0 \leq t < \infty\}$ *on a probability triple* (Ω, F, P)...". I realized that I was not likely to be able to teach myself the mathematics that I required. Third, in September 1973, I attended a meeting on warm lakes in Tiberias and this rekindled my interest in working in marine/limnological systems. Keeping to my desire of using graduate school for travel, I decided to leave the studies in Israel and pursue a PhD in Applied Mathematics and Statistics at the University of British Columbia.

Vancouver 1974–1978

Life is complicated, but not uninteresting—Jerzy Neyman (ca. 1980)

When I arrived in Vancouver in August 1974, I was interested fluctuations in systems with oscillatory dynamics. Donald Ludwig arrived at UBC at the same time, recruited to UBC from the Courant Institute of Mathematical Sciences at NYU.

I approached Don about doing a PhD with him, working on fluctuations in systems with limit cycles. He said we could work together for a while on a problem that interested him and to see how we got along before he would take me as a student. This problem involved fluctuations in systems with a separatrix (Mangel and Ludwig, 1977; Mangel, 1994), motivated by Thomas Park's magnificent study of competition between two species of flour beetles (Park, 1954)—something every undergraduate ecologist learns about today—and which had been worked on by every luminary in mathematical biology in the 1950s and 1960s (see the cites in Mangel and Ludwig, 1977; Mangel, 1994), including Jerzy Neyman (Neyman *et al.*, 1956), one of the founders of frequentist statistics. At Don's insistence, I took the course in Bayesian statistics that Jim Zidek offered; this became very important in my career.

Working with Don was a fantastic experience and I continue to try to pay forward the support and goodness that he showed to me. (I am skipping the story of failing my first qualifying examination and a variety of other tribulations!) And, as often happens with things we encounter early in life, I continue to be enthused by flour beetles—and for many years had them in my laboratory.

I ultimately wrote my paper on fluctuations in systems with multiple limit cycles (Mangel, 1980) and did work with Dan Gillespie (Gillespie and Mangel, 1981) on such systems before he became famous for his algorithms solving problems in stochastic chemical kinetics small volumes (e.g. Gillespie, 2007).

Although Don had research money from Canada's Natural Science and Engineering Research Council (NSERC) regulations then prohibited such funds from being used to support American students (presumably because of the large influx of Americans during the Viet Nam war). The early 1970s were a period of high inflation and what had seemed like a generous TA salary ($400 a month) when I applied to UBC simply did not go very far, especially since our first daughter was born shortly after I started graduate school and the second one was on the way in 1977. I discussed this problem with Don a few times and one day he told me that Colin Clark, who had written a very insightful paper (Clark, 1974) on schooling, had been approached by NOAA Fisheries to do some work on tuna-porpoise fisheries.

I had taken a course in optimal control theory from Colin in my first term at UBC and was sufficiently desperate that I convinced him to hire me as a research assistant on the 1-year grant. I also petitioned the Graduate Dean at UBC to allow me to work both as a Graduate Research Assistant and as a Teaching Assistant, to thus keep my family housed, clothed, and fed.

Working with Colin was another fantastic experience, and as with Don, I have tried to pay forward the debt that I owe to him from those days. We ended up with a very nice paper on the relationship between overall abundance of tuna and catch rate of tuna at dolphin-associated schools (Clark and Mangel, 1979); I wrote another paper on a stochastic version of those models (Mangel, 1982a). There were, of, course, difficulties: in the first 6 months of our work, we could not find the right kind of model and the stochastic version of the models was soundly rejected by *The American Naturalist*. In the course of this work, we learned that Jerzy Neyman—motivated by the collapse of the California sardine fishery—had worked on estimating the number of fish schools (Neyman, 1949) and that tuna purse seine vessels spent the majority of their time searching for schools of dolphin and much less time actually setting tuna. It was my introduction to search theory.

I started graduate school in 1974, the year that Ray Hilborn and Carl Walters were at IIASA (see Hilborn, 2016), and when they returned Ray was already on a rapidly accelerating trajectory and I was struggling graduate student. So I knew of him, but really did not know him. In August 1977, Colin organized a special session on Natural Resource Management at a meeting of the American Mathematical Society in Seattle. At that meeting, I spoke about our tuna work and met John Beddington; this becomes important later in my story.

The job market had not improved by the late 1970s. For example, a famous applied mathematician—Fritz Oberhettinger (well known to many physical oceanographers)—retired at Oregon State and the department received two jobs back for his one. They received 500 applicants per position. This does not sound unusual today, but it was then. I unsuccessfully sought a permanent faculty position, turned down one post-doc because the faculty mentor had no intention of letting me develop my own ideas (from which I decided that if I ever had post-docs, I would let them spend one day a week working on their own ideas, and have done so), and accepted another with Joel Keizer, a theoretical chemist, at UC Davis (at a salary of $12 000 a year—$11 000 as a post-doc and $1000 for teaching a one quarter course). In one of the bleaker moments of searching for jobs, I had also started applying for non-academic jobs, and one of those applications bore fruit: I was offered a permanent position in the Operations Evaluation Group (OEG) of the Center for Naval Analyses (CNA), at a salary of $24 000. Joel was very gracious in when I declined after having accepted the post-doc. I was anxious to move on and almost surely made mistake of not staying in Vancouver through my formal graduation in May 1978, but instead took the job in OEG, starting in November 1977.

Washington, DC and Oak Harbor, Washington 1977–1980

If, instead of sending the observations of seamen to able mathematicians at land, the land would send mathematicians to sea, it would signify much more to the improvement of navigation and safety of men's lives and estates upon that element—Isaac Newton

The Operations Evaluation Group started in 1942 when the US Navy approached Phillip Morse at MIT about providing scientific help to deal with the German submarines and other forces operating off the US east coast (see Little, 2002; Tidman, 1984, and Morse, 1977 for a history of OEG). He recruited Bernard Koopman (an applied mathematician) and George Kimball (a theoretical chemist); together they created the field of operations research in the US while PMS Blackett was doing the same with operations analysis in the UK (Nye, 2004). My boss while I worked there, Phillip DePoy, recently gave an interview that describes both the history of OEG and his role at the time I was there (Sheldon, 2016).

When I was hired at CNA, OEG was the main group that sent scientists to work at operational navy commands and had a tradition of staff spending one day a week on their disciplinary research, since operations research is a synthetic field. Most readers of this article indirectly know about OEG and CNA, because my colleague Christine Fox was the role model for Kelly McGillis's character in *Top Gun* (http://www.imdb.com/title/tt0092099/). When I met Christine, she was an analyst in DC, before going to the fighter wing in San Diego; she later became Director of OEG and President of CNA. Subsequently, she served as Acting Deputy Secretary of Defense and in that role Acting Secretary of Defense when the Secretary was out of the country, becoming the first woman ever to do so. As of this writing, Christine is Assistant Director for Policy and Analysis of the Johns Hopkins University Applied Physics Laboratory.

During WWII, Koopman developed the theory of search (e.g. Koopman, 1956a, b, 1957) and in the late 1970s, as turning his papers into a magnificent book (Koopman, 1980). While I was waiting for my security clearance to come through (which took a while because I had lived abroad), Phil DePoy suggested that I work through a draft of the book. The final chapter left open the question of how to deal with moving targets and I realized that the methods I had used in my PhD thesis could apply to search problems. For example, if the target is moving along a course characterized by mean vector $(b_1(x, y), b_2(x, y))$ subject to random fluctuations independent of location that can be characterized as a diffusion process with variance σ^2, and if $\psi(x, y, S)dt$ is approximately the probability that the target is detected in the next little bit of time dt when it is at (x, y) and the search plan is S, then the probability $f(x, y, t, S)$ that the target is at location (x, y) at time t and still not detected satisfies

$$\frac{\partial f}{\partial t} = \frac{\sigma^2}{2}\left[\frac{\partial^2 f}{\partial x^2} + 2\frac{\partial^2 f}{\partial x \partial y} + \frac{\partial^2 f}{\partial y^2}\right] - \frac{\partial}{\partial x}(b_1(x, y)f) - \frac{\partial}{\partial y}(b_2(x, y)f) - \psi(x, y, S)f \tag{1}$$

I knew how to obtain approximate solutions of such equations σ^2 was small compared with the deterministic component of motion (e.g. Mangel, 1981a) and so once more became involved in search problems.

Thus, I continued to work on search problems while in OEG. When I went to the field at Whidbey Island Naval Air Station, Oak Harbor, WA, I developed a method for locating a radio transmitter when there are biases in the angle measured by the receiver and the true angle (Mangel, 1981b). That paper received the Koopman Paper Prize from the Operations Research Society of America in 1982. I describe some of my other work while in OEG in Mangel (1982b). My time in OEG reinforced the power that comes from using simple mathematics in mature and sophisticated ways.

Because of this interest in search theory, Phil also suggested that I attend a NATO Advanced Research Workshop (ARW) on search theory, held in Faro, Portugal, in March 1979 (Haley and Stone, 1980). ARWs were wonderful meetings, bringing together colleagues from around the world to spend a week away from distractions discussing common intellectual themes. Ray Hilborn was also there and, as he explains (Hilborn, 2016), we connected again but this time more as equals. Because Ray had a house on Whidbey Island, we began regular contact. Since he has told the story of the *The Ecological Detective* (Hilborn and Mangel, 1997) and its authorship in his essay in this series, I will not repeat it here, save to mention that had I known how successful the book would be (https://dynamicecology.wordpress.com/2016/02/17/what-is-your-all-time-favorite-ecology-book/), I would have traded authorship order and bought the banjo myself. In an analysis of the Princeton *Monographs in Population Biology*—of which *The Ecological Detective* is a part—Elworthy (2007) notes that there are things which cannot be done effectively in a series of articles but can be done in a book. Introducing modern statistical methods to biologists, as Ray and I did, was one of them and making SDP accessible to biologists, as Colin Clark and I did (see below) is another. However, writing a book requires much more time (think of it as 10–12 papers none of which appears until the final one is done) and the full impact may not be seen for many years.

Although not required to do so, while in OEG, I kept finding ways to teach—I offered a course on search theory at CNA and taught introductory math for Embry Riddell Aeronautical University while in Oak Harbor. I now understand that I have a personality that requires me to teach and when my students and post-docs are looking for jobs, I ask them "Do you feel that you *have to* teach formal classes, rather than don't mind teaching formal classes?" If not, there are plenty of other wonderfully satisfying jobs outside of academia.

The Department of Mathematics at UC Davis advertised three positions in applied mathematics in spring 1979. Joel Keizer encouraged me to apply and if offered one to delay coming until 1980 so I could finish my commitment to OEG at Whidbey Island. This is exactly what happened. In September 1980, we moved to Davis, CA, where I planned to do OEG-like work in fisheries and agriculture. By OEG-like work, I mean bringing the scientific approach to problems for which the fundamental laws governing the processes are either unknown or too complex to derive from first principles, or for which we know the fundamental laws but do not know the value of the parameters associated with them—thus merging applied mathematics and statistics.

One of the last things that I did for CNA was to write for publication the technical reports of Abraham Wald on aircraft survivability (Mangel and Samaniego, 1984), which was the *JASA Applications Invited Paper* in 1983. Wald's thinking still intrigues people (http://www.ams.org/samplings/feature-column/fc-2016-06#mangel) and Jordan Ellenberg begins his book (Ellenberg, 2014) with Wald's work.

UC Davis, 1980–1996

All the business of war, and indeed all the business of life, is to find out what you don't know by what you do; that's what I called "guessing what was on the other side of the hill"—Sir Arthur Wellesley, The Duke of Wellington

Since this is a journal of marine science, I will only spend a few sentences on my work in agriculture, making three points. First, the ideas of search had natural application in agricultural pest control, and very nearly from the instant of my arrival at Davis I was applying them (e.g. Mangel *et al.*, 1984; Stefanou *et al.*, 1986). Second, problems involving insects led to two of my longest and most pleasurable collaborations—with Bernie Roitberg at Simon Fraser University and Jay Rosenheim at UCD. Third, mathematical methods allow us to see commonalities in situations in which others only see differences. For example, the inestimable Paul Smith enthused about the use of the Negative Binomial Distribution (Mangel, 2006) for describing the patchiness of fish eggs (Smith, 1973, 1978) and in the early 1980s, we began working together using it (Mangel, 1987; Mangel and Smith, 1990). However, exactly the same methods can be used to describe the aggregation of insect eggs or larva (e.g. Southwood and Henderson, 2009). Indeed, my most recent paper with Bernie Roitberg is "species-independent" and deals with the effects of relaxation of the thermal performance curve on individual growth (Roitberg and Mangel, 2016). Paul Smith also convinced me to study the diversity and longevity of the rockfish *Sebastes*, which lead to a rich collaboration with Mike Bonsall, first at Imperial College and then at Oxford (e.g. Bonsall and Mangel, 2004; Mangel and Bonsall, 2004, Mangel *et al.*, 2007; Bonsall and Mangel, 2009).

Once I started at UCD, my goal was to bring the ideas of search theory—which is fundamentally a Bayesian construct since we are seeking the location of a target conditioned on unsuccessful search—into fisheries (and agriculture) in a modern way. This lead to one of the first applications of Bayesian analysis in stock assessment (Mangel and Beder, 1985). In summer 1981, Colin Clark invited me back to UBC to collaborate with him and we began our work on informational problems in fisheries and natural resource management (Mangel and Clark, 1983; Mangel, 1985; Mangel and Plant 1985; Mangel and Clark, 1986a, b). While working on those informational problems in fisheries, we realized that the behaviour of fishing vessels is a special case of the general problem that almost all foraging organisms face: trading off gaining information about a foraging situation with actual gains in that situation. This work lead us to more general cases in behavioural ecology (Clark and Mangel, 1984, 1986); Clark and Mangel (1986) will shortly be published by Edward Elgar in a collection *Biological Economics* edited by Andrew Lo and Ruixin Zhang at MIT. According to Elgar: Our book is designed to bring together the most important and influential material in the subject area as facsimile reprints, to supplement the research resources of newly founded libraries around the world.

In the early 1980s, one of the problems in behavioural ecology was the crisis of the common currency: behavioural ecologists recognized that animals faced the problem of avoiding predation while acquiring food to avoid starvation. The units of predation risk are 1/time and the units of food acquisition are energy/time, so that the risks were non-commensurate. One afternoon in fall 1984, the day before I was planning to visit Colin at UBC, I left my office and walked towards the student union for a coffee. About halfway there, I realized that if we simply thought about overall survival—combining starvation and predation risk—we could use approaches similar to those in search theory, with SDP for optimizing behaviour, to resolve the crisis of the common currency. My recollection is that Colin had a stomach virus during my visit over the next few days, so it was a bit of time until he saw the power of this method—but not long after that we were on the path towards a unified theory of foraging (Mangel and Clark, 1986a, b). As it happened, Alisdair Houston and John McNamara at Oxford had virtually the same idea at the same time (McNamara and Houston, 1986). Over the last 30 years, we have jointly (Houston *et al.*, 1988), pairwise (Mangel and Clark, 1988; Houston and McNamara, 1999; Clark and Mangel, 2000), and separately (Mangel and Ludwig, 1992) worked to make this method a standard tool for life scientists. I provide a bit more history and some simple examples of how state dependent life history theory, implemented by SDP, is developed and applied in Mangel (2015).

In spring 1985, Bob Fridley—who ran the aquaculture and fisheries program at UCD—approached me about being involved in a project to develop genetic methods for identifying natal streams of returning chinook salmon. The project was funded by the fishing industry, which had a management zone closed on the basis of the then current method, only to have a very strong run of salmon return to the closed zone. Bob matched Graeme Gall, a top-notch geneticist in the Department of Animal Science, and me as the PIs. I recruited graduate students John Brodziak and Richard Gomukiewicz to work with me and we were very successful in using allozyme methods to determine stream of origin (Gomulkiewicz *et al.*, 1990; Bartley *et al.*, 1992; Brodziak *et al.*, 1992). That project began 30 years of me working on *Oncorhynchus* species.

In spring 1986, I received an unexpected letter from John Beddington on behalf of the Scientific Committee of the Commission for the Conservation of Antarctic Marine Living Resources (SC-CCAMLR). The CCAMLR Convention had been in force for just a few years and the Scientific Committee was wrestling with how to use fishery dependent data to estimate abundance of southern ocean krill, *Euphausia superba*. The difficulty was that vessels spent much of their time searching for swarms of krill and only a small amount of time harvesting, so that standard proxies for abundance such as Catch Per Unit Effort were not useful. John asked if I would be interested in serving as one of the first two invited experts to SC-CCAMLR; the other would be Doug Butterworth from the University of Capetown who would model the Japanese krill fleet while I would model the Russian fleet (Mangel, 1989, 1990). I attended the meeting of SC-CCAMLR in 1988 as an invited expert, and after my contract with CCAMLR ended continued to work on krill (e.g. Mangel and Switzer, 1998) so attended meetings of the krill working group and the SC-CCAMLR meeting in 1991, where I met George Watters, who now directs the Antarctic Ecosystem Research Division of NOAA Fisheries and is a longtime collaborator.

After the SC-CCAMLR meeting in 1988, I was asked to join the Committee of Scientific Advisors of the US Marine Mammal Commission, on which I served from 1990 to 1996. Among other things for the MMC, I lead the development of principles for the conservation of wild living resources (Mangel, 1996). In retrospect, the causal chain was tuna models to search theory to krill fishery to the MMC, a clear set of links that can only be seen looking backwards.

I had a sabbatical in 1987–1988, spending November–January at the Hebrew University in Jerusalem and February–August in Oxford with support from the Guggenheim Foundation for the entire period (to work on the development of unified foraging theory) and the Fulbright Foundation for the time in the UK. This was an important year in many ways in addition to the regeneration that a sabbatical provides. Before leaving Davis, I had decided that on my return I would try to move to a biological sciences department—I was ready to be surrounded again by biologists and have coffee with mathematical scientists rather than the reverse. UCD was a big enough campus, and one with a tradition of interdisciplinary work, to allow this to be feasible. As it happened, Robert May left the Department of Biology at Princeton that year to move to Oxford as Royal Society Professor. I applied for the job and was offered it; the retention offer from Davis included moving my appointment to the Department of Zoology (which later became the Department of Evolution and Ecology) and formation of the Center for Population Biology (http://www.cpb.ucdavis.edu) of which I was the founding director. Founding the Center for Population Biology is my greatest administrative accomplishment. Princeton ultimately hired Simon Levin for that position; it is still a perfect match.

In Oxford, I was hosted jointly by the Department of Zoology and Centre for Mathematical Biology. Jim Murray, director of the Centre, circulated a list of talks that visitors would be happy to give and I was invited by Felicity Hungtingford to visit Glasgow and Pitlochry. In Glasgow, I learned of the work of Felicity, Neil Metcalfe, and John Thorpe on how size going into the summer and feeding over the summer affected whether a juvenile Atlantic salmon would smolt the next spring or not (Metcalfe *et al.*, 1989). They had developed a statistical predictor based on the size and the growth rate and I realized that this was a perfect situation for applying state dependent life history theory, which we did—although it took us nearly a decade of work to get agreement on all the details (Bull *et al.*, 1996; Mangel, 1996; Thorpe *et al.*, 1998); already a senior Professor, I had the luxury of waiting until the right intellectual structure emerged, rather than rushing to print sooner. In the next section, I discuss application of these ideas to steelhead trout in California.

In 1990, I was invited to give a plenary lecture at the meeting of the International Society for Behavioural Ecology (ISBE) in Lund, Sweden. After that, Alasdair Houston and I co-taught a Doctor of Science course organized by Jan Ekman (who unexpected and sadly passed away in August 2016) at the field station of Stockholm University. Such courses were intended for graduate students and post-docs from all Scandinavian countries. There were wild rose bushes on the property, so in addition to lecturing the students on the work that I had done on insect oviposition, we could do field trips! At this course, I meet Jarl Giske, who had just completed his PhD at the University of Bergen. I told Jarl that one day he would have a sabbatical and he should come and spend it with me. And indeed he did—twice (2000–2001; 2010–2011)—as well as sending students and post-docs from Bergen to visit me in California and having me regularly visit Bergen. Since 2010, I have been Adjunct Professor in the Theoretical Ecology Group in Bergen and I continue to collaborate with colleagues there (Eliassen *et al.*, 2009; Giske *et al.*, 2013, 2014; Jorgensen *et al.*, 2016). Jarl also organized two Doctor of Science courses in Bergen that I co-taught with Tony Pitcher (1994) and Colin Clark and Paul Hart (1996), so I have had a 20+ year association with Bergen.

Throughout these years, fisheries management had been a theme of my research and I believed that to successfully train students to work in fisheries management, they had to learn social science as well as natural science so that my students took courses such as resource economics as electives. In 1994, a new PhD program Environmental Studies began at UC Santa Cruz and a senior position in that department became available when Michael Soule retired. I moved to UCSC after spending 16 years at Davis, where I could have spent the rest of my career there, but looked forward to new challenges and opportunities.

UC Santa Cruz, 1996–

The purpose of models is not to fit the data but to sharpen the questions—Sam Karlin (1983)

Susan and I moved to Santa Cruz in June 1996, so I have just completed 20 years at UCSC. In August 1996, I travelled to Bergen for a meeting of a SC-CCAMLR working group, a week holiday, and then taught the second Doctor of Science course. I had been working on krill for about a decade and went to the working group intending that this would be my swansong with krill. However, while there I learned that Reuben Lasker—one of my scientific heroes—had worked on the north Pacific congener *Euphausia pacifica* (Lasker and Theilacker, 1965; Lasker, 1966). I decided to return to Santa Cruz and start to work on individual variation in North Pacific krill (Marinovic and Mangel, 1999) and to continue working on southern ocean krill, with a focus on going from life histories to fisheries. The former project was supported by my start-up funds and the latter by NSF. I also agreed to organize the Second International Symposium on Krill (Mangel and Nicol, 2000).

When I could not find a graduate student in the new PhD program in Environmental Studies interested in krill, I used the salary to hire Suzanne Alonzo as a post-doc. We wrote a series of papers (Alonzo and Mangel, 2001; Alonzo *et al.*, 2003a, b) using individual behaviour to predict the effect of krill fisheries on penguin foraging success. When that NSF grant ended, I thought that I was done with working on krill, but this was not to be, as I explain below. Suzanne left Santa Cruz to a faculty position at Yale, but happily for me she returned to UCSC in 2014 as Professor in Ecology and Evolutionary Biology. We continue to interact and I serve on the PhD thesis committee of one of her students who is working on rockfish.

Around 2000, Bill Fox, then Senior Scientist at NOAA Fisheries recognized that a very large number of quantitatively trained NOAA scientists would be retiring in the next decade and that NOAA Fisheries needed to be proactive about filling the gap.

The Fisheries Ecology Division (FED) had recently moved from Tiburon to Santa Cruz and Churchill Grimes (Director of the lab), Alec MacCall, and I developed a training program between the FED and UCSC. Our idea was to use the funds that NOAA provided as a foundation for training students and post-docs in the quantitative population biology needed to sustain fisheries, thereby increasing the pool of quantitatively trained people who could be hired by NOAA Fisheries.

We called our program the Center for Stock Assessment Research (CSTAR) and sought students and post-docs who were committed to learning the relevant quantitative population biology and interacting with NMFS colleagues. For example, CSTAR students and post-docs generally participated in a stock assessment. Our focus was to train them so that they could develop the new quantitative methods that we will require in the 21st century. We attracted UCSC students from programs in Anthropology, Applied Mathematics and Statistics, Ecology and Evolutionary Biology, Environmental Studies, and Ocean Sciences and post-docs from a similar wide range of PhD programs. The discretionary funding for CSTAR was never considerable—alone it could not even support a graduate student and post-doc—but it provided a base that allowed us to send students to meetings or to work with colleagues, to have a robust seminar series, and provide summer support and some research support. This discretionary funding was supplemented by funding that NMFS colleagues or I obtained, or funding that the students themselves obtained. Of the last seven CSTAR students, five had NSF Graduate Research Fellowships and one had a NMFS-Sea Grant Population Dynamics Fellowship; of the last four CSTAR post-docs, two had NSF Postdoctoral Fellowships and one a Marie Curie International Outgoing Fellowship.

The first two post-docs appointed to CSTAR were Melissa Snover (whom I met on a visit to the Duke Marine Lab in 2001) and Steve Munch (with whom I had worked when he was a graduate student, Munch *et al.*, 2003) and they set a very high bar for the research by CSTAR students and post-docs for both breadth and depth of investigation and publication outlet (e.g. Munch *et al.*, 2005a, b, Snover *et al.*, 2005, 2006). After CSTAR, Melissa joined NOAA Fisheries Protected Species Division in Hawaii; she is now at the US Fish and Wildlife Service in Corvallis. After CSTAR, Steve joined the faculty at Stonybrook University, where he received tenure and promotion but even so we were able to lure him back to the FED in 2010 and he and I continue to collaborate (e.g. Salinas *et al.*, 2012, Boettiger *et al.*, 2015).

CSTAR also had a musical component: Alec MacCall, Steve Ralston, Sarah Newkirk (Steve Munch's partner), and I played music together from about 2002-2006. One night before music started, Alec asked me if the Fokker–Planck equation might be used to compute a prior for steepness (the fraction of unfished recruitment when the spawning biomass is 20% of its unfished size). I thought about and decided no, but that the Kolmogorov backward equation (e.g. Mangel, 2006) could be. This lead to a series of papers about steepness and its interpretation (He *et al.*, 2006; Enberg *et al.*, 2010; Mangel *et al.*, 2010a, 2013).

CSTAR was its biggest from about 2006 to 2011, in part because I was able to bring three very big grants to support the discretionary funding. Susan Sogard and I received a CalFed grant and embarked on a multipronged project concerning steelhead *Oncorhynchus mykiss* that involved field work, laboratory work, and modelling, with the goal of applying the ideas that I had developed with Atlantic salmon [Mangel and Satterthwaite 2008; Mangel and Satterthwaite 2008, Satterthwaite *et al.*, 2009, 2010, 2012a (selected as best publication for 2009 in *The Transactions of the American Fisheries Society*); Beakes *et al.*, 2010; Sogard *et al.*, 2012]. Will Satterthwaite continues to work on salmon, and followed up our genetic stock identification work on chinook salmon with a paper (Satterthwaite *et al.*, 2014) that won the Stevan R. Phelps award for best genetics paper in an American Fisheries Society Journal (2015).

Margaret Bowman, the program manager for the then newly formed Lenfest Ocean Program, convinced me to work on southern ocean krill once more (I tried to convince her to fund work on longevity and diversity in the *Sebastes*), to think about krill and their predators, krill fisheries, and climate change. Thus, although I had thought I was done with krill when Suzanne and I finished our work, krill came back into my life and I have given up trying to not work on krill (my current PhD student Ryan Driscoll is working on krill in winter). For this project, I advertised widely and assembled a large and productive team (Wiedenmann *et al.*, 2008; Cresswell *et al.*, 2009; Wiedenmann *et al.*, 2009; Mangel *et al.*, 2010b; Wiedenmann *et al.*, 2011; Cresswell *et al.*, 2012; Shelton *et al.*, 2013; Richerson *et al.*, 2015, 2016), with the goal of doing fundamental work that could feed into the Krill Predator Fisheries Model (KPFM) developed by George Watters and his colleagues at US AMLR (Watters *et al.*, 2013). Finally, I was part of the Bering Sea Integrated Ecosystem Research Program (BSIERP), for which the Secretary of Commerce awarded the Department's Gold Medal in 2015. The objective of BSIERP was to go from physical forcing through zooplankton and fish to top predators; our role was to predict how a changing environment would influence fur seals and kittiwakes (e.g. Satterthwaite *et al.*, 2010; Satterthwaite and Mangel, 2012; Satterthwaite *et al.*, 2012b; Vincenzi and Mangel, 2013; Vincenzi *et al.*, 2013; Vincenzi and Mangel, 2014). Each of these grants represented an opportunity; our choice is to seize the opportunity or not.

The whaling case in the International Court of Justice

In August 2010, I was unexpectedly contacted by the office of the Attorney General of Australia, asking if I would serve as the Independent Scientific Expert witness in a case in the International Court of Justice (ICJ) concerning the Japanese Whale Research Programs Under Special Permit in the Antarctica (JARPA II). This was the second phase of a very controversial program of lethal take that had begun in 1986, just as the commercial moratorium on whaling went into effect.

The ICJ is the principal judicial organ of the United Nations and all member states of the UN are automatically members of the Court's statute. The Court's role is to settle legal disputes submitted to them by States, so most scientists (including me before 2010) do not know about it. In general, the International Criminal Court (ICC) is in the news; the ICJ has only been so recently because of the dispute in the South China Sea.

I was asked by the government of Australia to develop criteria for a program for purposes of scientific research in the context of the conservation and management of whales in the southern ocean, and to assess JARPA II against those criteria. In order to assure my independence as a witness, I was not informed of Australia's legal strategy and only saw it unfold during oral proceedings.

Thus, my task involved considering science as a process, and communicating these ideas along with my assessment of JARPA II to the Judges. My full analysis is included as Appendix 2 to the Australian written submission (http://www.icj-cij.org/docket/files/148/17382.pdf). My determination was that a program for purposes of scientific research in the context of conservation and management of whales required (i) a conceptual framework leading to testable predictions, which is almost a definition of modern science; (ii) a process for setting sample sizes of lethal take based on solid statistical reasoning and analyses of the accuracy required to meet objectives; (iii) regular peer-review of research proposals and results; and (iv) design to avoid adverse effects on the stocks being studied. Assessing JARPA II against these criteria, I concluded that JARPA II is an activity that collects data in the southern ocean. However, it is not a program for purposes of scientific research.

Ultimately, using different reasoning, the Court concluded that JARPA II contravened the International Convention for the Regulation of Whaling and ordered it halted; Japan complied and 2014–2015 was the first time in 100 years that whales were not taken in the Southern ocean. Excellent analyses of the case in general can be found in Fitzmaurice (2016) and Fitzmaurice and Tamada (2016). Analysis of the case from the perspective of scientists can be found in de la Mare *et al* (2016), Mangel (2016), and Press (2016).

It was an amazing and nearly unique experience (the Court had not had an invited expert witnesses for more than 20 years and never really had a scientist). Although I have long had an interest in policy (e.g. service on the Committee of Scientific Advisers of the MMC), and written on policy for scientists (Ludwig *et al.*, 2001), this was my first real foray into international environmental law. The team of lawyers from Australia, lead by William Campbell, and all of the external counsel were stellar colleagues. I worked particularly closely with Philippe Sands, an environmental and human rights lawyer, whose books on environmental law (Sands *et al.*, 2012) and human rights (Sands, 2016) are amazing, and James Crawford, who was elected a Judge of the Court by the UN in fall 2014; his book of Hague lectures (Crawford, 2014) is titled *Chance, Order, Change*—something that every good Darwinian relates to! At one point, I asked the lead lawyer for Australia how they chose me and he said that they asked a variety of people and my name kept coming up; after looking at some of my talks posted on the web they decided I would be an effective witness. Once again, I could draw a causal chain from tuna in graduate school to whales in the ICJ, with each link representing a new opportunity but for which I had to make an active choice. For example, to do the work that the whaling case demanded, I put aside a project that I had planned to do on modelling metabolism and will probably never return to it.

Into 'retirement'

By July 2013, I had served the University of California for more than 33 years and decided that was time to step away from the constraints of formal teaching and professional and university service to concentrate on my research, much of which involved travel. I found it difficult to travel when teaching formal courses, especially to undergraduates, because of my sense of responsibility to the students. I was 62 years old at the time and followed my mentors who had retired at ages 61 (Don Ludwig) and 63 (Colin Clark). I consider that I had then finished the first two-thirds of my career (1971–2013).

The University of California reserves the title Research Professor for emeriti who have returned to active service to continue doing research. A one-month break is required between retirement and return to active service. Thus, on 1 August 2013, I returned as Research Professor.

As Research Professor, I am allowed to supplement my pension with research grants. When I retired, I expected that in 2016 I would have support to work on linking life history theory to stock assessments and on aspects of Ecosystem Based Fishery Management (Mangel and Dowling, 2016). I could not find funding to investigate those ideas, but I have had continued good fortune of being supported by NSF. I hold an OPUS (Opportunities for Understanding through Synthesis) grant that supports writing a book on ectotherms in changing environments, in which I will show how state dependent life history theory interweaves with other methods for studying how organisms respond to environmental change. I also hold a NSF-NERC grant with Holly Kindsvater (Rutgers) and Jason Mathiouplous (Glasgow) in which we are developing methods to use life history information to replace missing data for data poor species, motivated by tunas and groupers. This collaboration arose as a result of me telling Holly that she should meet Jason when she went to the International Marine Conservation Congress in Glasgow. As usual, the NSF grants are ones that I had to apply for (and the OPUS more than once), but unexpectedly I was invited to collaborate with Lisa Schwarz, Elizbeth McHuron and Dan Costa at UCSC to use state dependent life history theory to predict the population consequences of disturbance on marine mammals. We have already developed a general framework paper (McHuron *et al.*, 2016) and are in the process of applying the methods to California sea lions, blue whales in the California Current Ecosystem and the western Pacific population of gray whales.

Colin once described his retirement as a 10-year long sabbatical, and this is feeling true. I continue an active research program, my appointment at the University of Bergen, and very limited service to the profession (I am a member of the Scientific Review Board of the International Pacific Halibut Commission; http://www.iphc.info/srb).

What's been left out

I have not discussed teaching in much detail, but I taught at all levels—from introductory courses to specialty graduate courses; I considered my teaching successful if I could profoundly affect the life of one student each term and almost always did so. Undergraduates in introductory or intermediate courses became undergraduate thesis students, but I generally encouraged them to go elsewhere for graduate studies in order to obtain a different worldview. Some have come back to work with me as post-docs. I still love to teach, and in some sense still *have to*: in winter 2015, I gave a series of lectures at the FED on quantitative fisheries science. It could not be called a course because I had not been returned to active service to teach. I have not discussed editorial work nor university administration, but did too much of that and should have taught more. As mentioned above, I consider the Center for Population Biology at UCD my greatest administrative achievement.

Conclusion

Jon Schnute and Laura Richards visited CSTAR to give seminars, and Jon told me about Donald Stokes's book *Pasteur's Quadrant* (Stokes, 1997). Stokes argued that Pasteur was always motivated by an important applied problem (like Edison, but not Bohr) and sought fundamental understanding (like Bohr, but not Edison). I thus had the great fortune of a career in Pasteur's Quadrant.

That career began in a shaky way—not being able to find a permanent academic position but the three years in OEG turned out to be instrumental in the entire rest of my career. Search theory continues to develop (Stone *et al.*, 2016) and search algorithms are a bridge between organisms, evolution, and ecology (Nolting *et al.*, 2015; Barbier and Watson, 2016; Hein *et al.*, 2016). Because I kept publishing when outside of academia, I was able to return to it. The University of California has been a wonderful and almost always supportive place to work. Thus, virtually from the outset, my career was guided by a love of science and a recognition of serendipitous opportunities and seizing them, rather than a careful plan.

I constructed the title for this essay from a GLOBEC essay (Mangel, 1993) and the title of Chapter 3 in *The Ecological Detective*, which together summarize my advice to young scientists embarking on a career. First, truly understand the system in which you are working—make knowledge of your system so deep that it is part of your gut intuition. Second, when applying quantitative methods, be certain that you are choosing the ones that match your system, rather than using something that comes off the electronic or physical shelf. Also make these methods part of your gut intuition. And if a method is lacking for what you need to do, create it because that is the way science advances.

Acknowledgements

I thank Howard Browman for inviting me to write this essay. Over a long career, I have had support from NOAA Fisheries, NSF, Sea Grant, and USDA and the Lenfest Ocean program; I thank them all. Similarly, I thank the members of my group not mentioned or cited in here for helping create the rich intellectual life that I have enjoyed in UC for almost 40 years. Although I have not published a paper with these colleagues, I thank them for friendship and support over the years: Nancy Reid (since 1974), Simon Levin (since 1977, and an able squash partner for nearly 20 years), John Gillespie and Michael Turelli (at Davis), Joe Travis (since 1996), and John Thompson (at Santa Cruz). I thank Susan Milke Mangel, my partner in this adventure of ideas, an anonymous referee and Howard Browman for comments on a previous version of the manuscript.

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Handling editor: Howard Browman