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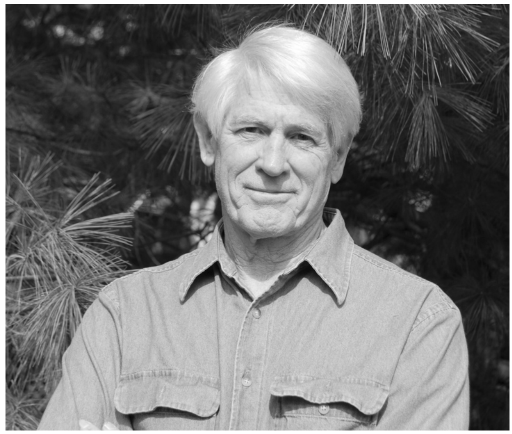
Historical Perspectives

Roger Lee Gentry

(born 19 March 1938)

Short Biography



Roger Gentry spent 34 years doing original research on marine mammals: three years in the laboratory investigating hearing and vision, and 31 years in the field studying the dynamics of social behavior and behavioral ecology. His field work involved observation and experiments on many aspects of behavior. He helped G. L. Kooyman in the development and use of the first time-depth recorders for marine animals and later incorporated them into his own field research. His field work focused on fur seals and sea lions (seven species at multiple field sites) with ancillary work on phocids, penguins, and the gray whale. His main study site was St. George Island, Alaska, where he studied northern fur seals for 19 seasons. In 1998, he and P. G. H. Evans managed the scientific program of the World Marine Mammal Science Conference in Monaco. He has published 55 journal articles and two books about his research.

In the second phase of his career, starting in 1998, he spent ten years advising regulators and interacting with Congress and many government agencies in Washington, DC, on problems involving underwater noise of human origin and its effects on marine animals. There he created a new acoustics program for the National Oceanic and Atmospheric Administration (NOAA) while acting as a liaison with the U.S. Navy and the oil industry. As an administrator, he dealt with 20 separate issues on underwater noise and explosions that were resolved but not published.

In 2006, he became a private consultant on noise issues. He is now an advisor to the Joint Industry Programme, a private funding group that supports original research on the effects of noise on marine animals. His present goal is to help coordinate the research efforts of industry and government agencies globally on the subject of anthropogenic noise and its effects on marine animals.

He received his Master's degree from San Francisco State College in 1966, his Ph.D. degree from the University of California–Santa Cruz in 1970, and held a Post-Doctoral Fellowship at the University of Adelaide, South Australia, in 1971. Although an academic at heart, he always worked as a government scientist and rarely taught. Outside of science, he alternates between writing and restoring old houses.

Marine Mammal Research Then and Now

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Introduction

The inspiration for this article was the 18th Biennial Conference of the Society for Marine Mammalogy (SMM) in Quebec, Canada, in October 2009. There, multitudes of young people, and rarely someone I knew, raced among three concurrent sessions in auditoriums so large the speakers could only be seen on giant video screens. The papers were presented to a relentless drum beat, with occasionally a perfunctory question but no in-depth discussions. Posters made up 75% of the almost 1,100 presentations, yet the setting was so chaotic that reading them was nearly impossible. Cold winds kept attendees inside during the breaks where they cued for access to the Internet or hotly thumbed their smart phones. On the positive side, some of the science reported was simply astounding. One group directly measured the stiffness of the basilar membrane in cetacean inner ears. Another was developing an acoustic means of assessing squid populations at hundreds of meters of depth where deep diving odontocetes forage. A keynote speaker reported the ability to identify frog species by analyzing DNA in pond water. Any newcomer would wonder how these researchers ever came to ask such questions and what tools they used to answer them.

Personally, I was overwhelmed at what these meetings had become, good and bad, since the first one in 1964. For me, the Quebec meeting had too many people, too many presentations, and too little opportunity to think and assimilate. Had I been a new student trying to enter this field, I would have felt deeply discouraged by the pace of the meeting, the large number of competing students, and the difficulty of the science being reported. I would have wondered how the field ever got to be this way. So, when I was asked to write this article, I decided to address those questions and summarize some of the interwoven factors that have brought our community to its present state. I also briefly summarize my own career to show how chance used to be a major factor in the direction of one's career but no longer may be.

This is not an exhaustive review backed up by facts. These are my personal impressions of changes in the field, mainly in the U.S., since 1963, with a few sample references available to me without a research library. It also is not a reminiscence of the "good old days." Frankly, they weren't that good. The take-home message is positive. The research being done today in the face of impediments like increasing competition, decreased funding, increased regulation, and other factors is far more important and intellectually satisfying than the research we elders were able to do earlier when we had virtually total research freedom. This field has become hard to enter and even harder to succeed in as a functioning scientist. But, I advise young people not to give up trying. The science being done is fully worth the effort.

The History of the Biennial Meetings

The first meeting of what later became the SMM biennials was called the Conference on Biological Sonar and Diving Mammals. It was held in the summer of 1964 by Thomas Poulter at the Bio-Sonar Laboratory in Coyote Hills near Newark, California. Poulter had established the laboratory in 1963 to investigate whether, like dolphins, pinnipeds used echolocation (they didn't). The laboratory was in a bucolic setting, backed by hills and overlooking an extensive marshland far from traffic. The facility had formerly been a base from which the military was prepared to fire Nike missiles from hardened silos atop a nearby hill.[1] Poulter had built cement pools for five species of pinniped among the stolid military brick buildings where the meeting was held. Most of the marine mammal researchers of the U.S. and Canada, all 25 of them, occupied metal folding chairs in a dismal classroom. Attendees included John C. Lilly, then trying to sell his dolphin communication laboratory in the Virgin Islands, and Victor B. Scheffer (then U.S. Fish and Wildlife Service), the reigning pinniped biologist at the time (Scheffer, 1958). Gerry Kooyman, then at Arizona State University, and I were the only two students. Winthrop Kellogg, a dolphin echolocation researcher who had helped Poulter establish the laboratory, was notably absent. Some of the attendees knew each other from meetings of the American Society of Mammalogists, but no one

[1]For reasons that pass understanding, Bruce Mate, whale biologist from Oregon State University, once bought those abandoned silos!

seemed to know everyone. There was no sense of "community." The audience was mostly older men working in isolation from one another and discussing their work in hope of finding some commonality. There were about 11 unhurried talks followed by good question-and-answer sessions. There were no posters. Between sessions, people conversed around the anechoic tank where Poulter recorded seal vocalizations, fed the seals, or went bird watching in balmy weather.

Poulter held similar meetings over the next ten years, sometimes with an emphasis on student papers due to a lack of new results by professionals. The second meeting in 1965 had about the same number of attendees and only 12 presentations. By the fifth meeting, in 1968, 102 people attended. From that year onward, meetings were held at the Stanford Research Institute, the Poulter lab's parent organization, in Palo Alto. In 1975, Poulter retired and turned over his conference organizational materials to Ken Norris, newly arrived at the University of California–Santa Cruz from Hawaii. Norris renamed the meeting the Conference on the Biology and Conservation of Marine Mammals. For some reason, the next meeting was not held until 1977. Then "Conservation" was dropped from the title, and poster sessions were held for the first time, but concurrent sessions had not become necessary. The audience was so large that for the first time it was not possible to know or meet everyone present. I have always wondered why, as the audience grew and presentations soared from 11 to 1,100, no return was made to the original schedule of annual meetings. Surely with the present inflow of data, annual meetings would better serve the community than continued biennial meetings.

The early meetings were the only way people then could discuss research underway. Also, there were no college courses on marine mammals and no textbooks. People learned from reading the original literature, written in several languages, and worked from reprint collections since few libraries carried all the pertinent journals. News of new papers or techniques spread by word of mouth, and there tended to be few papers on any one topic because there were so few researchers.

Career Planning

Everyone I knew in the 1960s got into marine mammal research by chance. No one I knew back then planned to enter the field or had influential friends pulling strings. If one was in the right place at the right time and had an interest in the right subject, opportunities came their way. In fact, most of the changes in my career directions came about this way as the following summary shows. I'm not sure when it became so difficult to enter this field that influence and/or years of unpaid volunteerism were necessary to get a start. It became noticeable in the 1970s and now seems endemic.

When I started graduate school in 1963, I wanted to study ornithology, but there were no opportunities to do this because the ornithologist at San Francisco State College had too many students, which forced me to explore other fields. After reading *Listening in the Dark* (Griffin, 1958), my interest shifted to bat echolocation. I wanted to develop that theme in a graduate seminar, but another student was given that topic so I picked marine mammals on a whim. As a boy, I had seen a recently drowned California sea lion (*Zalophus californianus*), and it left me wondering how a mammal with the face of a dog ever came to live at sea. Nearly the first article I read for my seminar claimed that this species produced dolphin-like clicks and, like them, probably used echolocation (Poulter, 1963). I called Poulter to ask some questions about his paper. Luckily, he was at the Bio-Sonar Lab only about 32 km (20 miles) from my school, so he invited me down for a talk. After a few hours of conversation, he unexpectedly offered me a job and the chance to do a master's thesis on underwater directional hearing in *Zalophus*. This ability was essential to echolocation but had not yet been demonstrated for this species. And that's how I got into science. I basically walked in off the street with no special skills, background, or connections. I was just generally interested, available, and cheap—the very combination Poulter was looking for. My interests soon crystallized around the evolution of terrestrial sensory systems for an aquatic existence.

My first experimental design to measure directional hearing failed, but luckily Ron Schusterman had arrived at the Bio-Sonar Lab a few months earlier and was setting up to measure sea lion vision. He coached me through training animals for psychophysical testing, experimental design to establish a minimum audible angle, signal presentation, data collection, and eventually publication (Gentry, 1966). He was not my academic advisor, just a very interested and generous spirit.[2]

During a 1965 visit to nearby Año Nuevo Island, where the lab got its study animals, I spent a day among breeding Steller sea lions (*Eumetopias jubatus*) from a blind in their midst. It was like an epiphany. I realized that sensory systems were but a small part of the suite of adaptations required for mammals to make a living at sea and that the place to see those adaptations was in the field, not the laboratory. I found and read the few papers then

[2]The day before writing this paragraph, I was notified that Ron had just passed away. Those of us whose careers he influenced mourn his passing.

available on pinniped behavior. Writing my thesis and the resultant paper convinced me that I was actually more interested in behavioral ecology than in sensory abilities, so I decided to switch fields for my doctorate.

Academic advisors who knew pinnipeds were rare at that time; there were only two in California: George Bartholomew (University of California–Los Angeles [UCLA]) was a physiologist, and Carl Hubbs (Scripps Institution of Oceanography) who mostly worked on fish. Luckily, Richard Peterson, then a rising young specialist in otariid behavior at Oxford University, got a faculty position at the newly opened University of California–Santa Cruz. He took me on as a graduate student in 1966 because of my interest in his field and my familiarity with Año Nuevo Island where he intended to work. Together, we took over research there because Poulter's work was ending. We had our pick of the four species that used the island because there were no other faculty or students interested in seals. Dick took northern elephant seals (*Mirounga angustirostris*), and I took Steller sea lions (Figure 1). We also went on several expeditions to islands in Mexico and southern California, and we did a short study on visual acuity in the sea otter (*Enhydra lutris*) (Figure 2). Dick died before I finished my degree, and Burney LeBoeuf, who arrived at Santa Cruz in 1967, took over the elephant seal work and guided me through to graduation in 1970.

Figure 1. The author and Edward H. Miller (*right*) doing behavioral research on Steller sea lions, Año Nuevo Island, California, 1968

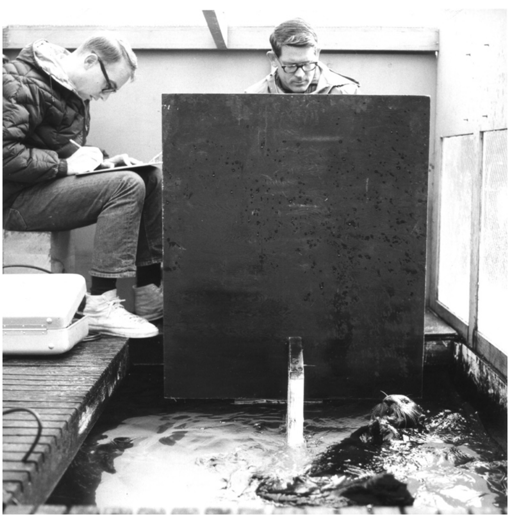

Figure 2. Richard S. Peterson (*left*) and the author measuring visual acuity in Gus, a sea otter, held at the Point Defiance Zoo and Aquarium, Tacoma, Washington, in March 1967

While writing my doctoral dissertation, I had started corresponding with Ian Stirling, then studying New Zealand fur seals (*Arctocephalus forsteri*) on the South Neptune Islands in South Australia (Figure 3). Halfway through his project, he accepted a position studying polar bears (*Ursus maritimus*) for the Canadian Wildlife Service for which he is now so well-known. He kindly offered me the remainder of his fur seal project, and I took it so I could begin comparing the behavior of different otariid species instead of just describing them (Figure 4).

When that project ended in early 1971, I returned to Santa Cruz as an unpaid research associate and began writing grant proposals. I was not very successful and quickly ran out of money. In 1972, Gerry Kooyman, who had moved to Scripps Institution of Oceanography, saw my plight and took me to Palmer Station, Antarctica, to study penguin metabolic rates (Figure 5). On that trip, I learned about the details of his dive research and that his major obstacle was retrieving instruments. In 1973 and 1974, Ken Norris also helped by

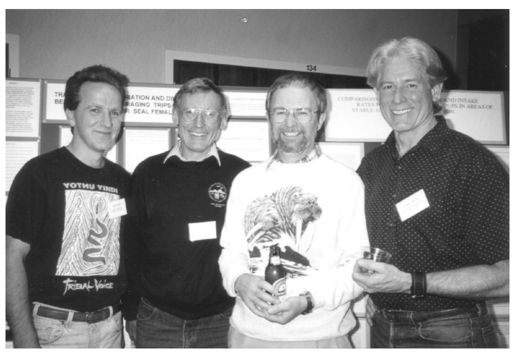

Figure 3. Ian Stirling (*second from right*), who started the research station at the South Neptune Islands, and three of the four researchers who followed him there. *Left to right:* Nick Gales, Peter Shaughnessy, Ian Stirling, and the author (photographed at the 17th Biennial SMM Conference, Cape Town, December 2007)

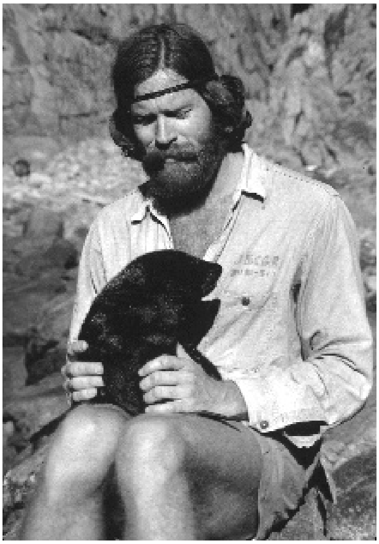

Figure 4. Author with a New Zealand fur seal (*Arctocephalus forsteri*) pup on the South Neptune Islands, January 1971

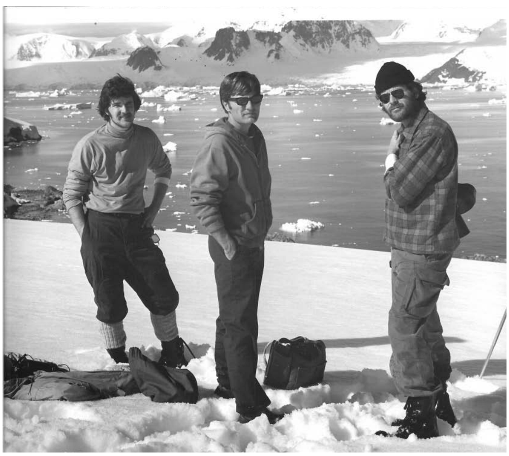

Figure 5. Adelie penguin research at Palmer, Antarctica, with Phil Bergman (*left*) and Gerry Kooyman (*center*), January 1972

hiring me to manage his NASA project, capturing, harnessing (Figure 6), and putting instruments on gray whale (*Eschrictius roustus*) calves (Norris & Gentry, 1974). Both of these remarkable opportunities came from my being somewhat experienced but mainly available, not to mention in need.

In 1973, George Harry, National Marine Fisheries Service, invited me to apply for a position NOAA had available to study the behavior of northern fur seals (*Callorhinus ursinus*). I applied and was selected, again because I had the right background and I was available for a position I had not sought out. I spent the years 1974 to 1998

Figure 6. With Ken Norris (*right*) on board the *Louson,* holding an expandable harness used to attach instruments to gray whale calves (Northern Magdalena Bay, Baja California Sur, January 1973)

working on the Pribilof Islands with the fur seal team. Other opportunities came my way in those same years. I found that fur seal females moved around more predictably than the Weddell seals (*Leptonychotes weddelli*) Kooyman had studied in the past and would likely improve his chances of retrieving instruments. I suggested he devise a dive recorder that would run as long as a fur seal foraging trip. He did; we deployed the first units in 1975; and they were a success (Kooyman et al., 1976). In 1977, we deployed the new recorder on Cape fur seals (*Arctocepalus pusillus pusillus*), hosted by Peter Best then at the South African Museum in Cape Town. In 1978, we went to the Antarctic to put instruments on Ross seals (*Ommatophoca rossi*). These two projects did not occur by chance; we found grant funds for their support. Thereafter, foraging ecology was a major theme in my field work (Figure 7). I spent the 1986 and 1987 austral summers studying diving and behavior of the Hooker's sea lion (*Phocarctos hookeri*) on the Auckland Islands after a chance meeting at a workshop with Martin Cawthorn, then with the Ministry of Agriculture and Fisheries in Wellington, New Zealand (Figure 8). In 1990, I worked with Valeryi Vladimirov, then with the Russian Federal Research Institute of Fisheries and Oceanography in Moscow, on diving in northern fur seals at Mednyi (Copper) Island in the Commander Island Group, Russia, where the foraging habitat differed from that at the Pribilof Islands (Figure 9). We had planned this trip for several years, so again chance was not involved.

In 1995, underwater noise of human origin became a societal issue. Because of my background in acoustics, I was recruited to help NOAA regulators deal with the ATOC (Acoustic Thermometry of Ocean Climate) project, the HESS (High Energy Seismic Survey) committee, low-frequency naval sonar, and other acoustic problems until my fur seal work was finished

Figure 7. Capturing a female northern fur seal for deployment of a time-depth recorder; Vivian Casanas "discourages" the male while Bernard Walton (BBC film director) observes (St. George Island, 1988).

Figure 8. The field team studying Hooker's sea lions at Enderby Island, Auckland Islands, New Zealand, in 1986. *Left to right:* Bob Warnecke, Chris Thomas, author, and project leader Martin Cawthorn.

(Gentry, 1998). At that point, I moved to NOAA headquarters to work full-time on developing a new acoustics program. There, I liaised with the Navy on mid-frequency sonar and beaked whale issues (published as Evans & England, 2001), briefed Congress about underwater noise, testified in federal court cases, and helped prepare U.S. responses to treaty resolutions on noise. I convened panels to write noise exposure criteria for marine mammals (Southall et al., 2007) and fish, and started efforts to address noise from the shipping and oil industries.

In late 2005, I retired from federal service. Three months later, a consortium of oil and gas companies invited me to join their Joint Industry Programme (JIP) as its program manager, helping fund research on the effects of industry noise on marine animals (see www.soundandmarinelife.org). I had the background in acoustics, marine mammal research, and government regulations for which they were looking. Again, the combination of availability and background provided another career change.

I am unsure whether modern researchers can benefit from the kinds of unsolicited opportunities that were so critical in my own career. Judging from the number of young people at the Quebec conference, and the small number of opportunities in this field, I suspect that careful planning is replacing opportunism as a basis of careers.

Maturation of Marine Mammal Science

The field of marine mammal research has followed the same developmental stages as other scientific disciplines, namely description/correlation, comparison, experimentation, and synthesis. In the 1960s and earlier, most researchers were engaged in descriptive work. At that period, very little was known about marine mammals, including their evolution, taxonomic affinities, bio-geography, and which species were still extant. Brief expeditions of discovery to remote islands were a source of this information (Peterson et al., 1968a, 1968b). Parasitologists described the parasites of marine mammals, acousticians described hearing, and behaviorists described social behavior (Peterson, 1965; Peterson & Bartholomew, 1967).

As the number of single descriptions grew, it became possible to compare species in detail. The comparison allowed one to identify broad patterns that could not be seen on a single-species basis. Physiologists were the first to use comparison extensively because they had available data on terrestrial mammals. Physiologists were also the first to use experimentation, which to me was the most satisfying approach because it could reveal the underlying causes of phenomena, whereas description/correlation and comparison could not. For these reasons,

Figure 9. Valeryi Vladimirov (*left*), Mark Pierson (*background*), and the author restraining a female northern fur seal to apply a time-depth recorder (Mednyi Island, Commander Islands, Russia, July 1990)

papers on physiology were always my favorites at the early marine mammal conferences even though that was not my field. Recently, it has become possible to compare experimental results across species to identify phylum-wide patterns (e.g., Williams, 1999). Finally, by synthesizing data from many different disciplines over long periods of time, it is becoming possible to consider the ecological connections associated with large scale, regional population declines (e.g., Williams et al., 1998).

The trend in most branches of marine mammal research has been toward reductionism, working at progressively lower levels of organization. Until the middle of the 20th century, field biologists focused on the species, its relations with other species, and its geographic distribution. Then the emphasis switched to following the population dynamics of local stocks of a given species over time. Behavioral studies initially focused on large seasonal patterns like reproduction, migration, and foraging. They then shifted to the analysis of social behavior as it relates to population dynamics, and after the introduction of dive recorders, foraging behavior of local stocks. More recently, behavioral research has started to focus on persistent behavioral characteristics of individuals within local stocks (Twiss & Franklin, 2010). Starting in the 19th century, laboratory work on marine mammals focused on describing gross anatomy in great detail. Over time, laboratory work shifted to the properties of tissues and cells. The latest emphasis is on subcellular fields like genomics, proteomics, and metabolomics. Hopefully, the next phase of marine mammal research will be the integration of the individual, at all levels or organization, back into their ecosystems. This synthesis will be much more difficult than the reductionism that has characterized the field to date.

Some individual careers have followed parts of the above pattern. My initial work on Steller sea lions focused on description and correlation, and my post-doctoral work (Gentry, 1973, 1974a, 1974b) and our work with the first time-depth recorder focused on comparison (Gentry & Kooyman, 1986; also see Figure 10). Much of my later work on northern fur seals involved behavioral experiments (estrus, philopatry, etc.). To me, description was so unsatisfying that I never tried to publish my dissertation.

Funding Sources

Marine mammal research underwent another change that had nothing to do with maturing as a science but everything to do with the source of research funds. Because of this change, the field shifted from doing mostly basic research under grants to doing applied research under contracts. The post-war years saw a quest for new information in all scientific fields. The 1950s and early 1960s were the golden age of federal grants when new information was desirable without regard to its applicability; knowledge alone was the accepted goal of research. Through the late 1960s and into the 1970s, applicability of results became desirable, so projects on basic science began to produce some applied products. Later still, the need for answers to specific questions became so paramount that basic science came to be done as an adjunct (often unreported) to an applied project that was performed under contract.

Research on marine mammal sensory systems exemplifies the above shift. In the 1960s, hearing, echolocation, and vision in marine mammals were studied under grants solely to describe the evolutionary changes necessary for an aquatic existence. The Bio-Sonar Laboratory took advantage of this trend. Later, the U.S. Navy became interested in possibly improving sonar systems by studying echolocation in dolphins. A substantial research program was set up that had an applied objective but that nevertheless produced much excellent basic science (Au, 1993). From the mid-1990s onward, research on marine mammal hearing has been driven by the specter of harmful (and, by statute, illegal) impacts of sound from sonar, pile driving, oil exploration, and shipping. Most of this research is applied in that it tests for animal responses to specific sounds that humans produce (e.g., Finneran et al., 2005).

Another reason for the shift away from basic science was that society changed its attitude toward science in general. Encouraged by people like Senator William Proxmire of Maryland, basic

Figure 10. Seven authors designing (note the matrix on the blackboard) the first book on comparative foraging ecology in marine mammals, *Fur Seals: Maternal Strategies on Land and at Sea*. *Left to right:* T. Seamus McCann, John Croxall, the author, Randall Davis, Fritz Trillmich, Dan Costa, and Gerry Kooyman, who conceived the book and convened this meeting at Scripps Institution of Oceanography in June 1983.

research came to be seen as a frivolous waste of taxpayer money by many. From 1975 to 1988, Proxmire annually announced what he called Golden Fleece Awards in which he derided government-funded projects by focusing on their methods and ignoring the scientific principles under study. This was like saying golf involves swatting little balls into distant holes using a stick but ignoring the physics and finesse involved. This movement away from basic science turned out to be penny wise and pound foolish. The U.S., which used to lead in basic science, has now failed to develop some key industries that spun off from advances in basic science. Those of us trained in the 1960s still feel that basic science is the only way to get a well-rounded answer to applied questions. Those who fund science projects need to learn that undirected quests for scientific knowledge are not a luxury but are the source of all that is new.

Finding research funds these days is like shooting at a moving target as far as applicants are concerned. Research topics go in and out of vogue in an unpredictable pattern. Sometimes the reason is clear; research on the effects of global warming on endangered species comes to mind. Sometimes the reasons are political. Once when I was helping on biodiversity issues in Washington, DC, we were forbidden to call for research on animal conservation, not because conservation was no longer necessary, but because the previous administration had called for conservation research and they were from the opposite political party. Funding directions may change with the winds of politics.

Collaboration

In the early 1960s, people usually did research alone or with a partner, but no one worked in the large teams that characterize modern research. The closest I saw to a team was Bill Schevill, his wife Barbara Lawrence, and Bill Watkins from Woods Hole working on whale acoustics. In those days, teams were not really necessary because so little was known about the animals that researchers were trying to simply describe what they saw, and description did not benefit from teamwork. Those of us who were students in the 1960s mostly saw single- or double-authored papers. We considered working alone a mark of professionalism because it demonstrated the depth and range of a person's abilities. Over time, teams became more common in marine mammal science, in part because the topics became so complex that no single worker had the breadth of knowledge required. It was also a response to the funding agencies, which, in the mid-1970s, began calling for a few large, collaborative studies instead of the many smaller individual efforts they had previously supported. Some modern field studies resemble military operations. The JIP and the U.S. Bureau of Ocean Energy are funding a study on the effects of seismic airguns on migrating humpback whales in Australia. This year it involves about 60 people, 40 of whom are unpaid volunteers.

Publication

Over time, some scientific journals have changed the kinds of papers they publish. In the 1960s, journals like *Science* and *Nature* would publish any paper about marine mammals that described novel results, even fairly unimportant ones (e.g., Gentry & Peterson, 1967). This showed that their target audience then was scientists. But over time, the high profile journals shifted to targeting the broad lay media, publishing scientific results that were considered newsworthy to society instead of informative to scientists. Now if a marine mammal paper gets accepted in such a journal it usually has multiple authors and is stripped of essential details to fit the shrinking space limitations. Years ago, papers on marine mammals were considered "soft science" because of their focus on description and their inability to report large sample sizes. Scientific advances like those seen at Quebec most likely will remedy that situation.

The pressure to publish has also changed. In 1965, one of my professors had on his office wall a pair of opened shark jaws with a card in the center saying "Publish." So, pressure to publish has existed for some time. But the kind of pressure applied and responses to that pressure have changed over time. Formerly, researchers were judged by the number of papers they published. Carl Hubbs published nearly 1,000 papers in his career and did it by writing many papers that were short on details like range extensions of a given species. Presently, researchers are being judged more by the so-called "impact factors" of their papers than by sheer numbers. Journals are numerically rated according to who likely reads them, and by a multiplication process, a résumé gets reduced to a single number. This process tries to derive an objective score for a phenomenon (namely "influence") that is intrinsically subjective. One could take this as some colossal joke were it not for the unjust pressure it places on researchers. This process clearly favors laboratory scientists over field workers because of the time required to finish a project. This difference could influence the career directions of incoming marine mammal researchers.

Tools

In the 1960s, field research required as little equipment as binoculars, a gun (or fishing line), and a notebook. We called it "rifle biology" because specimens were collected in the field and analyzed in the laboratory where all the thinking occurred. Collecting specimens was a major purpose of the early expeditions of discovery. I went on several of these to observe behavior while others made collections. It was frustrating because proper observing takes more time than merely collecting. It was like observing behavior by walking past cages on a zoo visit.

In the 1970s, several marine mammal projects began that eventually lasted 20 years or more. Examples of field projects are the polar bear work by Ian Stirling, Jim Estes' sea otter work funded by the U.S. Department of the Interior, Randy Wells' Sarasota Dolphin Research Program, and my own work on northern fur seals. Also, Ron Schusterman, Sam Ridgway, and Gerry Kooyman carried out long-term laboratory studies on perception and cognition, medicine, and physiology. In addition to providing a great deal of new data about a few species, these projects all developed new tools and methods. Their long-term, stable funding was a key factor as was the fact that researchers moved from one research topic to another over time. The main tool that came out of my fur seal project was the time-depth recorder that Gerry Kooyman developed (Kooyman et al., 1975). It basically gave scientists access to the pelagic lives of larger marine animals (Kooyman & Kooyman, 2009). Over time, electronic models replaced mechanical ones; new sensors were added; and software was developed to analyze complex data files. One of the most amazing modern instruments is the Dtag that for the first time can record the echoes used in echolocation (Johnson & Tyack, 2003). Instruments have become the norm for research on all large marine vertebrates (see www.topp.org/about_topp) and are the basis of a small industry.

Marine mammal research also benefited from the methods that were developed in other disciplines. Some examples are estimating field metabolic rates using isotope dilution, analysis of DNA from very small samples, assessing prey consumed by using stable isotope ratios and fatty acids in tissue, and visualizing internal structures using CT and MRI scans. The list goes on and on. Early work in our field was limited by our inability to observe animals in any way other than by eye.

Another great change over time has been in the use of models. They used to be held in very low esteem. In the 1970s, a popular expression was, "No one believes a model except the person who wrote it; everyone believes a data point except the person who made the measurement." Or more pointedly, as one professor used to say to students, "Write a model, go to jail." Today, models are the only way to provide point estimates that can be tested empirically.

At least in behavioral research, there has been a shift away from piecing together theories from empirical observations. Increasingly, researchers start with a notional theory and look for confirmatory or contradictory trends in the field. In the 1960s, notional theories in behavioral research were based on results from birds or lab rats and did not fit the behavior of marine mammals in the wild. Therefore, almost all the early behavioral marine mammal research began with empirical observation. Over time, notional theories like group selection, kinship, altruism, reciprocal altruism, female mate choice, and others came and went but increasingly seem to guide research activities.

The impact of computers goes without saying. Unfortunately, they were not field ready by 1992 when my fur seal work ended. After hand collecting and digitizing 15 large boxes of data sheets, we found, disappointingly, that the files easily fit onto the jump drive of a modern computer. Computers are now essential to many field projects. For example, sound propagation models that are run on portable computers are used to modify the source output in sound playback experiments to control the received level at a distant animal as its range from the source varies. If young people entering the field have any advantage over the older people who advise them it is facility in the use of computers.

The Changing Role of the Principal Investigator (PI)

The changes in marine mammal science described above have driven up the cost of doing science. Some dive instruments cost between $5,000 and $10,000 each, for example. But these so-called direct costs are less important to the PI than the fees their institutions charge for their participation. In the 1960s, these indirect costs were about 10% of the actual costs. Now the rate exceeds 200% at some institutions; the institution gets $2 for every $1 the researcher needs. This suggests that some institutions now view their researchers in a different light than before. Forty-five years ago, they saw researchers as a minor drain on a university's budget; indirect costs were repayment for what the researcher actually cost the institution in terms of telephone, lights, and the like. But the phenomenal rise in indirect costs suggests that institutions now use researchers as cash cows to bring in outside funds to pay for costs that are unrelated to research such as undergraduate education. As a result of these fees, researchers get priced out of applying to small funding agencies and are pressured into proposing, as their first priority, research topics that will attract money. Therefore, it is now much harder for some PIs to fund their research than it was 45 years ago, and their choice of research topics is forced by need. Increasingly, PIs are managers of other people's research instead of front-line researchers in their own right.

Attitudes Toward Marine Mammals

A major influence on marine mammal research has been society's changing attitudes toward these animals. To people of my generation, marine mammals were thought of as commodities. Newsreels of the 1950s showed factory ships firing exploding harpoons into large whales with as much compassion as if they were felling trees. The wearing of fur seal skins signified wealth and fashion, not the violent killing by clubs that it is. Basically, marine mammals were there to be used. In 1963, when my career started, marine mammals were still just meat. A few anatomists visited the then-active whaling station at Richmond, California, but students generally had no interest in them. The absence of other interested students was largely the reason I could walk in off the street and be offered a job at the Bio-Sonar Lab.

In the mid- to late-1960s, as part of the social movement that rejected conformity and touted individuality, the "hippie" culture arose along with popular opposition to the war in Vietnam. Probably as an offshoot of the times, society's attitudes toward animals changed as well. To many, marine mammals ceased to be commodities and became objects of intrinsic value in need of conservation. The U.S. Congress reacted accordingly. In 1969, it passed the interim and toothless Endangered Species Preservation and Conservation Act; in 1972, it passed the Marine Mammal Protection Act (MMPA); and in 1973, it passed the Endangered Species Act (ESA) to replace the 1969 interim version.

By 1975, the extreme vision of cetaceans (not pinnipeds) was that they were icons, imbued with mystical or spiritual properties unlike those of any other animal. They were often portrayed with rainbows and crystals. Douglas Adams spoofed this attitude in his 1984 novel, *So Long and Thanks for all the Fish*, in which dolphins were space aliens here to exploit humans of lower intelligence.

The iconic view of marine mammals has had a noticeable effect on the research community. The animals came to be considered so precious and fragile that federal regulators began requiring a permit just to observe them through binoculars. At one point, researchers were not allowed to drizzle marking bleach on the hair of Hawaiian monk seals (*Monachus schauinslandi*), sleeping like logs, for fear of "harassing" them. Avoiding trivial disturbance now seems of greater concern to the public than conservation research. Acting through the regulatory process, this attitude has had a debilitating effect on research activities.

Regulation

Before 1972, marine mammal research was not subject to regulation in any country of which I am aware. Researchers were free to use whatever methods were commonly used in their professions. Because everyone in those days focused on animal populations, researchers believed they could do anything they wanted with individuals because their populations would not be harmed. Some of the methods they used were admittedly horrific and would be considered unethical today; however, they were right that their activities had much smaller effects on animal populations than whaling, sealing, or commercial fishing.

When the earliest regulations were passed, the intent was to protect individual animals from all forms of human interference and to make every effort to restore endangered species to their original population levels. Regulators and the regulated struggled to understand the new prohibitions because they were cloaked in legal jargon like "take," which covered all the effects between minor disturbance and death.

Laws never remain the same over time; they always change due to pressure from varying sources. In 1989, when commercial fishers thought the prohibitions on "taking" marine mammals were interfering with their livelihood, they lobbied for and received an exemption from the MMPA for marine mammal death and injury incidental to fishing operations. The exemption was not quite total. Congress required that the take rates be reduced over time, and it established target levels and dates, and the oversight to ensure it. It never required that the take rates reach zero, however, which was the original intent of the act. The congressional decision to go easy on fishing is mystifying because worldwide fishing has been shown to be the primary threat to marine mammal populations, killing hundreds of thousands of animals per year (Read et al., 2005). The process Congress put into place was flawed because, for most species, the target levels for mortality and serious injury, and the dates on which they were to be reached, have mostly been missed. In 2007, the U.S. Department of Defense was granted a two-year exemption from the MMPA for its operations involving mid-frequency sonar, again with caveats. It seems that the U.S. Congress is willing to weaken the intent of the MMPA and ESA to foster certain kinds of human activities at sea. However, marine mammal researchers should not expect to receive a similar exemption. Researchers have no lobbyists, contribute nothing to the gross national product, and provide no national defense. They only provide the data that are essential for marine mammal conservation that federal laws require.

Regulation is also on the rise at the institutional level. The proliferation of Institutional Animal Care and Use Committees (IACUCs) and national and professional standards for the use of live animals in experimentation are now prescribing acceptable methods that researchers may use. The paperwork involved in getting research permits and passing IACUC scrutiny is becoming a time-consuming impediment to researchers.

Distractions in Field Research

A final trend in marine mammal research that concerns me is the deterioration of working conditions in the field. An absence of distraction in the field has always been essential to careful thought and data collection. I worry about modern researchers going to the field with cell phones, satellite phones, e-mail, faxes, DVDs, and MP3 players that interrupt their thought processes. The need to file progress reports or write blogs from the field, be actors for visiting documentary film crews, or show visitors around puts increasing demands on especially those who work on charismatic wildlife. Increasingly, field work on marine mammals seems like doing research in a fish bowl with the world watching. Field work offers the gift of solitude, but it is a fragile gift that needs protection.

Summary and Conclusions

In the 1960s, marine mammals attracted few researchers. Funds for research were relatively easy to obtain in those days because the expensive tools and teams of modern research did not exist, and because society valued science for its own sake. No regulatory controls existed then, and the small number of active researchers meant that almost anyone could find a species and do whatever research they liked with no competition. However, the descriptive research being done then to provide the most basic information about species had little intellectual appeal. It could not explain causes, answer the "why" questions that scientists ask, or identify cross-species trends. Just as descriptions became numerous enough to permit comparisons, more students discovered the field and began seeking positions. Society's attitudes toward basic research in general, and toward marine mammals in particular, then changed. What followed was an increase in regulatory control and a shift in the focus of funding agencies toward more applied topics. Today, competition to enter the field is keen. PIs are under increasing pressure to bring in more funds on research that has more "impact" on society, but to do it in an atmosphere of increasing regulatory control. All in all, the impediments to entering the field and to later carry out a successful career are quite daunting compared to 45 years ago. But those that succeed in it can now produce results that would have been inconceivable to earlier researchers working in a climate of few impediments. There clearly has been a tradeoff over time between the amount of research freedom available and the intellectual satisfaction with the resultant data.

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