CHAPTER 3

ENCOUNTERING DISCIPLINARY (i.e., TRIBAL) AND IDEOLOGICAL SANCTIONS

In his superb book *Naturalist*, Wilson (1994) attributes his early, strong relationship with natural systems to his father's changing professional positions frequently, invariably requiring a move to a new location. As a consequence, establishing long-term relationships with those of his age was quite difficult. He found natural systems everywhere he lived, and, thus, a long-term interest in nature became firmly established in Wilson's life.

My own isolation occurred at a much later age and was also inadvertent, despite the fact that it was in science rather than in a social context. Although I did not realize it at the time, four isolating mechanisms were immediately operative: (1) I effectively had a graduate research assistantship that was paying for the gathering of my thesis data; most others students did not, (2) I was working on a team when all other graduate students were "lone wolf" research investigators, (3) since the work involved pollution of aquatic ecosystems, I was involved in "applied research," although the river survey teams were investigating the effects of pollution stress on the structure of aquatic communities, and (4) I was working under the direction of a female scientist, which was extremely rare in those days (the extramural funding that supported my research was acquired by Dr. Ruth Patrick). All these factors were much less tolerated by others than they were by me.

Of course, having a salary as a graduate student was splendid for a person with a wife and child (initially $3,600 per year, later reduced for budgetary reasons for all crew members to $3,000 per year for nearly a year). Some faculty members were being paid only slightly more in 1948 for nine-month appointments in the less well-paid academic institutions. In addition, I had money for travel, living expenses when out of town, equipment, and the like. Unquestionably, a gulf exists between the academic "haves" and "have nots," just as between rich and poor. Disraeli et al. (1845) remarks that neither intercourse nor sympathy exists between these two groups, as if they were inhabitants of different planets and not governed by the same laws. In academe, the "haves" are, naturally, intensely focused on sources of extramural funding and competition for these funds. The "haves" are generally more mobile within academic institutions that are dependent on extramural funding. The "have nots" move less frequently and spend far less time on the acquisition of extramural funding. In the United States, institutions may be ranked according to their total amount of extramural funding. Most academic institutions have individuals in each of the "have" and "have nots" categories, but the proportions vary. Individuals may go from "haves" to "have nots" and vice versa. This transition is often accompanied by an attitude change.

Of course, I was unaware of this situation initially, but I certainly would have felt an impact had I known of the gap. Obviously, a better perception of isolating mechanisms and ways of coping with them would have helped. However, my experiences were dramatically different from my graduate student colleagues. Despite the common perception that individuals within a team lose independence, I had a greater awareness of a group in which each individual has a different specialty and exchanges information. I felt that working on a team was enriching and no less challenging than lone-wolf research.

Although Dr. Mary Gojdics helped me initially with difficult identifications and the like, by the end of the first summer I needed relatively little help. I had done nothing but identify protozoans for over three months, seven days a week, often working twelve hours per day. This experience certainly fixed different species firmly in my mind. In actual fact, I felt I was better off than lone wolf researchers because I still had the challenge of doing my own work but had available, with modest effort on my part, the detailed water chemistry at each collecting site and data on all of the other major groups of organisms associated with the protozoans. In my MS thesis, I was able to diagnose most pollution effects without information about the other organisms, and I did so because the entire thesis would be complete in itself. I was reassured that confirming biological/chemical/physical information supported the conclusions I had already drawn. The reason my evidence was available first was not any intellectual skill on my part, but rather due to the highly perishable nature of protozoans. I had to do my analysis immediately and could not preserve the specimens for more leisurely examination. The most important factor in the minds of some of my fellow graduate students appears to have been that I was doing research with practical value—community response to anthropogenic stress was definitely low status to them, although of considerable theoretical interest to me and many others.

These encounters are almost as vivid today as they were the day they happened, in some cases approximately 57 years ago. Naively, I viewed academe as an intellectual community interested in a variety of things and was eager to share knowledge. This picture is given to the outside world, and an uncritical person might be forgiven for buying it lock, stock, and barrel. My utopian vision, though battered, has never been shattered, because there are wonderful people in the system who truly help others (both applied and theoretical) unstintingly and effectively. As a caveat, these people may often help by being extremely critical of a manuscript, research data, hypotheses, and the like. Arguably, even utopia needs a quality control system to ensure the maintenance of perfect conditions.

Today, the idea is incredible that anyone working to preserve natural systems should be chastised for doing so. Yet, criticism is still at work, although sometimes muted and restricted to particular venues. In the 21st century, the primary, often virulent, opposition to the protection of natural systems and sustainable use of them (i.e., use without abuse) comes not from mainstream science but rather from political ideologies that feel threatened by scientific evidence. Although a major producer of anthropogenic greenhouse gases, especially carbon dioxide, the United States rejected the mandatory emissions restrictions that are a key element of the Kyoto Protocol. This pact commits the three-dozen industrialized countries taking part to cut, by 2012, combined emissions of greenhouse gases to at least 5% below levels measured in 1990. Although the *New York Times* (13 December 2004, "Cheers and Concern for the New Climate Pact") reports that many scientists feel the pact is deeply flawed, it is a beginning toward a decades-long shift toward limiting greenhouse gases. Economic globalization may induce the United States, with a great deal of production and sales outside the country, to move closer to the position of other industrialized countries. A second major facet of this tribal bickering is the belief by one faction that economic growth will soon encounter limits to resource availability on a finite planet. Proponents of perpetual economic growth depict this situation as a choice between preserving natural capital and preserving and accumulating man-made capital. Major catastrophes with substantive economic impact may be necessary to reduce the denigration of science and force a rapprochement between science and economic and political ideology.

For younger readers, it is worth mentioning that this attitude was more defensible in the middle of the 20th century. For example, at the 1939 World's Fair, which I attended, gleaming exhibits showed the world of tomorrow, including partially automated kitchens, transportation, communication, and the like. Machines would do all "dirty work," including waste disposal (although such appliances were definitely not highlighted at the 1939 World's Fair). The belief that new technology will be developed to solve all problems created by old technology is still alive and well despite considerable evidence to the contrary. Nearly three-fourths of a century after the 1939 World's Fair, we have realized that technology has created many problems not yet resolved, such as traffic jams and road rage, polluted ecosystems, or extreme distortion of the hydrologic cycle and water quality. That the dichotomy between theoretical and applied science still exists was demonstrated vividly when a former post-doctoral fellow, with whom I had carried out research and published, visited me in July 1998 and described an adversarial situation very similar to the one I encountered for the first time half a century before.

Theoretical Redux

In the mid-1960s, during a summer at the University of Michigan Biological Station, I decided to have a go at purely theoretical research. Robert MacArthur had given a seminar at the Academy of Natural Sciences on the equilibrium model he had developed with E. O. Wilson, and I decided to see if the theory held true for protozoans. It did. The results were published in *The*

American Naturalist (Cairns et al., 1969), definitely not an applied journal. I should have felt elation at moving from one caste in publications to another, but I did not. Just labeling the research theoretical produced neither more nor less satisfaction than applied research. Further, although I felt I had moved from one caste to another, critics of applied research did not. Apparently the caste system is not easy to escape.

I have a strong desire to carry out "useful" research that also has theoretical value. My research on protozoan colonization processes in freshwater ecosystems had little practical value initially, but was extremely enjoyable. No feelings of guilt about applied research occurred during the many hours I spent identifying species, analyzing data, and writing articles. Additionally, very few scientists in North America were interested in this research in 1948, although considerable interest existed in countries where large numbers of protozoologists were capable of identifying free-living freshwater species. The only justification for mentioning colonization research at this point is that, when it was undertaken, it was purely theoretical and had no obvious immediate applied value. At the few national meetings involving both theoretical and applied sections, my research would have been relegated to the latter, although it would arguably not have been of much interest to the former. The more important issue here becomes: Is the problem of determining the effects of human society on natural systems any less interesting than any other factor affecting natural systems? The scientific process applies equally well to each, and a well-designed applied research program should have both theoretical and applied value. Both theoretical and applied research, when published, can be either boring or fascinating, as evidenced by the number of theoretical articles never appearing once in the *Science Citation Index*.

The most valid objection to applied research is that it is proprietary and often subject to removal of evidence, perhaps damaging to the sponsor, before being submitted for publication. However, proposals for extramural funding can be written so that the right to publish cannot be challenged, except by the established peer-review practice of professional journals. While many investigators accept proprietary research grants when the right to publish at one's own discretion is not included, it is not essential to do so. I find that research divisions and offices of sponsored programs in comprehensive universities frequently have no hesitation in adding "right to publish" clauses—many require them and are extremely hesitant to agree to any grant for which the principal investigator does not have full control of the data. Furthermore, one can persuade potential industrial sources of grant funding that the credibility of the research is increased markedly if both parties approve such clauses at the outset and the principal investigator consistently does not accept proprietary research and is known for inserting such "right to publish" clauses. Despite all these caveats, industrial money is often labeled "dirty money" by colleagues who feel that unbridled funding is the most important criterion for determining the quality of the research. Although the situation has improved dramatically during my professional career, some still believe that anyone who accepts money from sources other than the National Science Foundation and similar organizations is somehow contaminated. However, this attitude is becoming increasingly problematic as NSF funds continue to be cut. In this regard, it is worth noting that President Abraham Lincoln established the US National Academy of Sciences (NAS) so that distinguished scientists could assist in solving societal problems. The NAS (through the National Research Council) still spends much of its institutional efforts toward this end. Nevertheless, NAS members are elected primarily on theoretical contributions to one or more areas of science.

Arguably, the rapid development of the Internet has vastly increased environmental literacy globally (although the Internet has faults). This development is indeed timely because of the increased efforts to denigrate scientific evidence when it conflicts with political agendas. Another potent force is the appearance of a significant number of transdisciplinary journals. Wilson's *Consilience: The Unity of Knowledge* (1998) pleads that humankind save its common home Earth by seeking a common system of knowledge. This challenge is dramatically different from reductionist science, which was an isolating approach that dominated 20th century science.

Another isolating mechanism in my career was encountering bias toward women. Women scientists, especially team leaders, were not particularly common immediately after World War II, although they were not unknown. Most commonly, they worked in laboratories and the names of the most prominent are well known. However, women scientists who worked in polluted water caused by sewer and industrial waste outfalls were extremely rare. Despite the fact that women for ages had changed diapers and cleaned up innumerable disgusting messes in households, their working with societal wastes, both sewage and industrial, and, worse yet, talking about them, was simply not acceptable to many in the scientific community.

In contrast to the struggles women endured to be accepted in science, and many other professions, those women who worked in applied and transdisciplinary science had an even more difficult time. My principal mentor Ruth Patrick had to overcome both obstacles. She was an inspiring example to those of us who had only one obstacle to surmount.

I began working for Ruth Patrick because she felt that what she was doing was exciting. Such excitement is catching! The exhilaration almost certainly was intensified by the fact that both field teams shared this excitement to a large degree. The two teams worked together only for the summer of 1948, and the number soon dwindled to four staff members working under Patrick's direction. This research was enough to finish my MS thesis.

I can easily reconstruct events during this period. Some fellow students in more traditional areas of research would often make derogatory comments about interdisciplinary activities, especially research outside academia. My own students have had to face many of the same pressures in more recent times, and I never found a satisfactory way of guiding them through these difficulties. Most people want approval from their peers, and being a contrarian is always socially awkward. As a consequence, I hedged my bets for the PhD dissertation by doing studies on transfaunation of protozoan parasites from frogs and salamanders to a variety of hosts and vice versa; I even included transfers from some warm-blooded animals. Conveniently, "trichomonads" parasitized a wide variety of organisms. During most of my PhD candidacy, I continued to work with Patrick on pollution problems, even though I did take a substantial part of a year off to finish my dissertation research. Completing the dissertation during weekends, evenings, and holidays was time intensive since I had to get restarted each time (regain the mind-set that I had when I stopped earlier work) after gaps of days or weeks.

However, I so enjoyed the challenge of pollution problems that I continued the work while completing my dissertation on host/parasite relationships. When the dissertation was completed and published, I published two additional short articles on host/parasite relationships. I then left behind such studies completely, concentrating for the rest of my career on stressed ecosystems, including ecological restoration and what is now called ecotoxicology.

In making this career choice, I knew that I would face the problems of anyone who strays outside one's home discipline and yet remains in it because of the way universities and research organizations are structured. I found the host/parasite relationships exceedingly fascinating, and parasites are often quite beautiful when viewed under a microscope, rather than being experienced in one's body. Also, given my penchant for seeing connections outside of any specialty, I would have undoubtedly become involved with public health officials and other disciplines, as Henry van der Schalie did in studying schistosomiasis for the World Health Organization. At some point in my career, probably in the 1960s, I began to view the disciplinary sanctions the way I viewed overhanging brush when fishing a small trout stream—aggravating, sometimes infuriating, always present, but a necessary price for fishing superb areas. Even the occasional hook in the thumb when I snap-cast to avoid the overhanging brush was still not an excessive price to pay and rewarded me for using barbless hooks.

Disbelievers and Marginalizers

The petty academic warfare just described is certainly not admirable and can sometimes be career threatening. Such activity is almost certainly not as harmful to society as a whole as are the individuals and organizations who assert that no serious environmental problems exist; that biotic impoverishment is not really occurring, and, if it were, it is not important; that global warming is a myth with no scientific support; and that human populations and economic growth as now understood can continue indefinitely into the future. In some cases, an admission is made that the evidence may be correct, but "people are more important than fish." Placing the whole environmental argument in a jobs-for-humans or environment context misleads the public into believing that a healthy environment and a thriving economy are incompatible. There is no recognition that human health in an unhealthy environment is an oxymoron. Many who call for "sound science" cannot be convinced by any evidence contrary to their beliefs and cannot, or will not, acknowledge the existence of contrary evidence, even in peer-reviewed, professional journals. Anyone interested in betrayals of science should read Ehrlich and Ehrlich (1990) or the debate between exemptionalists and environmentalists (Myers and Simon, 1994; Hawken, 1993). Exemptionalists believe that human ingenuity and technology exempt humankind from the universal laws of nature that affect other species.

Another group, the diverters, typically try to redirect the discussion by making such statements as “oh, yes, pollution is important but we have to solve human society’s problems of homelessness, malnutrition, disease, and poverty before addressing environmental problems." Anyone wishing to follow the discussion on this topic will find Bartlett (1998) interesting. Finally, ecological denial that any problems exist also persists (e.g., Orr and Ehrenfeld, 1995).

Anyone choosing environmental research will have these fun folks to contend with, in addition to colleagues defending disciplinary purity. To paraphrase former American President Harry Truman—if you can’t stand the heat, don't go into the kitchen!

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