Writing a book is a horrible, exhausting struggle, like a long bout of some painful illness. One would never undertake such a thing if one were not driven on by some demon whom one can neither resist nor understand.

George Orwell, 1946

If one does not look into the abyss, one is being wishful by simply not confronting the truth . . . On the other hand it is imperative that one not get stuck in the abyss.

Robert Jay Lifton

FOREWORD

The idea for this book arose during a small dinner party held following a visiting scholar seminar by Eugene P. (Gene) Odum entitled "Balance of Nature: Myth or Reality?" at my home institution on September 5, 1996. Along with my wife Jean and me, Marlene and Fred Benfield and Sandy and Jack Webster (both Fred and Jack have worked for years on the University of Georgia's Coweta project) were in attendance. During a lull in the conversation, Odum turned to me and said, "What people expect from you now is a book containing illustrative examples that mark important developmental stages in your career, each preceded by a brief discussion of why you made the shift and its importance to you and environmental problem solving in general." Odum pointed out that I had, throughout my career, published in a variety of journals, and colleagues would unlikely have followed my career path the way they could had I restricted my publications to a particular subdiscipline or to one or two journals. Neither Odum nor I can remember the precise wording, but the content of the message was clear.

Certain experiences in my life have had a profound effect upon my professional career, such as growing up in a mill town, the US Great Depression, admission to Swarthmore College, and obtaining a professional position with Dr. Ruth Patrick. Connections to these defining moments appear in a variety of chapters in this book. I believe these connections are more important than the inevitable redundancies that result from some repetition, which I hope does not adversely affect the flow of the entire book.

During the Great Depression in the United States, most people were focused on short-term survival, i.e., in many cases, day-by-day living. However, even then, ordinary citizens valued an education because they understood the long-term benefits. Scientists and teachers were respected, regardless of their income. In those days, before television and high consumption of material goods ("one or more automobiles in every garage"), social interaction was at a much higher level. Radio was important because listening to "The Lone Ranger," "The Shadow," and so on required a vivid imagination. However, my listening was limited to one hour after dinner, and the rest of the time was spent studying and reading. By today's standards, Conshohocken, Pennsylvania, was a small community. I could walk anywhere in town in 10 or 15 minutes. Most homes did not have telephones (cell phones had not been developed), so conversations were "face to face."

At that time, I saw no major problems in how close in proximity naturalistic areas were to industries. A trout stream (Wissahicken Creek) was within the Philadelphia city limits. Potts Quarry, where I fished almost daily all summer, was only 10 minutes by bicycle from my home. The Schuylkill River, on one edge of town, was polluted and contained only carp and catfish. At that time, I had no appreciation for what the river could have been. While a teenager, I was able to fish in Yellowstone Park (my aunt and uncle, Dr. and Mrs. Walter Latshaw, lived in Salt Lake City, Utah). That experience was my first indication of what truly natural systems were like. For two weeks each summer, I also was able to fish in salt water at Somer's Point, New Jersey. Fishing pressure on nearby salt water systems was comparatively low, and the waters were relatively unpolluted.

In retrospect, I believe I had a sense of place that included both natural systems and humanized environments. In my youth, my sense of place was small geographically and limited ecologically. I still try to find a humanized environment in close proximity to a naturalistic environment. (I define a naturalistic environment as one in which human artifacts are minimal or, preferably, seldom visible. The environments I find are not truly wild systems. A humanized environment is one in which human artifacts are designed so that other life forms are treated with respect.)

I met Jean at Grange Women's Dormitory in 1941. I can still vividly visualize meeting Jean 64 years later. Our courtship was long by today's standards – nearly three years. We did

not engage in long discussions, which now are labeled communication; we just did things together. Of course, we discussed what hikes we would take the next weekend, but being together was our form of communication. Neither of us placed a high value on material possessions. I remember going to Penn State with a large suitcase and a winter overcoat over my arm. The US Postal Service had a special rate that permitted laundry to be shipped home for washing and ironing. When I see students arriving at Virginia Tech, usually with a rental van and the entire family carrying possessions into the dormitories, I marvel at how things have changed in 60 years. Although our material possessions increased as our family grew and we become more affluent, they never came close to those of our peer group. Living simply became a blessing when we spent summers at field stations where a simple life with few material possessions was the norm. Another lasting impact of the Great Depression was thrift. Our frugality paid off in a variety of ways, most notably when Jean had to enter a nursing home in June 2001 and remained there until she died on 21 February 2005.

Fortunately, Jean and I shared the same value system. The retirement community where I now live, Warm Hearth Village, has over 50% of the land in forests and one is less than 200 feet from all the windows of my Showalter assisted-living apartment. When Jean was in the nursing home there, the view from her bedroom window was of a forest. On my thrice daily visits to see Jean, I saw forests on both sides of the road that connects the two facilities. Many paths, both paved and unpaved within the property, offer space for communing with nature.

Jean and I spent 33 summers at the Rocky Mountain Biological Laboratory in Colorado and the University of Michigan Biological Station near the junction of Great Lakes Michigan and Huron. Both of us enjoyed a sense of place in these ecologically quite different ecosystems. I have spent 37 of my 57-year professional career in Blacksburg, Virginia, where our home was surrounded by 8.5 acres of woods. Jean and I could leave the back door of our home, walk for miles, and only see a power line and some distant houses from the end of our two favorite trails. Two national parks are nearby, as is the Appalachian Trail (a 2,050-mile trail that goes from Georgia to Maine). If I did not have significant daily contact with naturalistic systems, I would feel deprived. I am indeed fortunate that, at 82 years of age, some of this contact is still possible.

As undergraduates, Jean was always on dean's list or close to it. On the other hand, my grades were modest. Jean knew of my less than stellar grades, but she never mentioned them. When we married during World War II, Jean had her degree in biochemistry and I had not even graduated. After I was discharged from the Navy, I enrolled at Swarthmore College and improved my grades. With a wife and daughter dependent upon my performance, I simply could not return to marginal academic performance. While getting my grades to a level that would give me a chance to attend graduate school, I found great enjoyment in the academic life and never left it. Without Jean's faith in me, I suspect this turnaround would not have happened. A relatively recent happening that relates to this period of my life warmed my heart and brought tears to my eyes. I was scheduled to graduate from Penn State in biochemistry in 1944. Also, I was scheduled to be, but never was, inducted into my fraternity Alpha Zeta (Morrill Chapter – both honorary and social [with a fraternity house]). Instead, I served in the Navy and graduated from Swarthmore College in 1947. A group of present Alpha Zetas from Penn State combined with Virginia Tech's Alpha Zetas to induct me as a member 58 years later (in 2002).

The early 1940s were turbulent, and my recollection of dates, friends, and colleagues decreases every year. I have no trouble remembering the sequence of events, but the precise dates of events over half a century ago in my personal life are often not easy to determine. The situation is quite different for my professional life since I have always maintained a detailed and frequently updated *curriculum vita*. Even though some of the details are waning, the defining moments of both my professional and personal life remain clear, even vivid. Looking into the past has brought both moments of joy and distress – the latter when I am embarrassingly reminded that I could have done much better than I did.

I graduated from high school in 1940 and worked that summer in a paper mill. Even then, I could maintain my association with natural systems. In fall 1940, I entered Pennsylvania State University, then a small institution surrounded by natural systems. The following summer, I worked for the US Department of Agriculture Laboratory in Wyndmoor, Pennsylvania, as a sub-professional 6 (the lowest rank). This work was my first opportunity to observe research; it fascinated me, although it seemed far beyond my grasp at that time. Moreover, I never dreamed that one day I would view these quite different activities just described as part of a larger system.

During my professional career, I have benefited enormously from Odum's suggestions about what I should do next, although such comments have been exceedingly rare. As soon as I heard his suggestion, about the autobiography, I knew the project had to be done! I cannot hope to reach the elegance of E. O. Wilson's *Naturalist* or the series of Aldo Leopold's essays and seminars titled *The Mother of the River of God* (published after Leopold's death by his colleagues Flader and Callicott, 1991). Odum's difficulties in getting ecology accepted as a bona fide field are well known to those of us who witnessed the field's astonishing development, but these difficulties tend to be forgotten by new students and faculty who take the existence of ecology for granted.

I can easily select the major transitional periods in my career even though the changes occurred incrementally. Even though I could list and document the origin and development of each new area of interest, most scientists are remembered for recent work or vignettes of what colleagues remember from earlier work.

In her book *Composing a Life*, Mary Catherine Bateson (1989) states: "We take advantage of slim opportunities, swerve ever so slightly to avoid obstacles, and the next thing we know we are on a new life course." This statement describes my own career path perfectly. However, I have known a number of systematic and orderly people who envisioned their entire professional career while still a teenager (or before), and actually followed through and succeeded, some brilliantly. My own career was academically unsatisfactory until after World War II, which interrupted my college education for some years. After the war, my focus and motivation had increased and my career continued to change dramatically at fairly frequent intervals, partly because of my poor judgment at times, but mostly because of opportunities that seemed too good to ignore. Obstacles were aplenty, ranging from a department-level administrator (who felt that environmental science was a fad) to purchasing agents (who felt that "going by the books," even if it involved a delay of months, was more important than dealing with a spill of toxic chemicals). Most people were extremely helpful, and I am grateful for both their help and the psychological boost their interest provided.

Since 1948, when I began writing my MS thesis, writing has been the integrating theme for me professionally. Since writing for scientific journals requires substantive knowledge of the field, research and writing have been the foundation for my teaching. Writing for professional journals is often, but not always, based on hard data that is gathered at some considerable sacrifice to one's personal life. The November 1977 issue of the journal *Environmental Health Perspectives* contains my 1,300th publication. I had gathered no "hard" data for that commentary ("Defining Goals and Conditions for a Sustainable World"). I felt no joy in reaching that particular number of publications and, in fact, have only once celebrated a numerical threshold (the appearance of my 1,000th publication) with a few colleagues. However, I did feel joy in the article itself, not because it was perfect but because it represented an incremental advance for me personally in trying to understand the possibility of sustainable use of the planet with, of course, the recognition that nothing goes on forever.

Writing this autobiography has caused me to revisit the past far more frequently than I normally do. Recalling events would have been a worthwhile experience even if the book were never published because I have realized indebtedness to others more vividly than I did at the time the indebtedness occurred. The joy of writing for me, and I suspect for most others so engaged, is what Peter Drucker termed "future facing" or "solving the future" (Beatty, 1998). In

my present writing, I am preoccupied with how human society can balance the development and utilization of technological systems so as not to impair the health of the planet's biospheric life support system. My earlier writings were based on pollution assessment, with the goal of managing societal waste better in the future; ecotoxicology, with the goal of predicting the effects of potential toxicants before they were released into ecosystems; and ecological restoration, with the goal of determining how to repair ecosystems for future benefits after they had been damaged by societal activities. All my writings have been based on the hope that human society will develop a more harmonious relationship with natural systems and will leave a habitable planet for future generations. In short, humankind will learn to use natural systems without abusing them.

At age 82, I realize that the future becomes the present with alarming rapidity, and time spans of 10 or 15 years, which once appeared substantial, are now shrinking with enormous speed. However, time is often the healer of damaged ecosystems. Nature has survived major episodic extinctions before in history, and probably can again. I have less faith, however, in the ability of human society to survive a major cataclysm, including one induced by ecological destruction. The present is indeed an exciting time because society is witnessing an epic struggle to use its biospheric life support system in a sustainable manner, but may fail to do so.

This concern about future events does not diminish my joy in research! When an article appears in print, I experience a certain sense of satisfaction for having moved incrementally along a continuum toward what I hope is enlightenment. To view any publication as completion is entirely fallacious. If the article receives attention, invariably some responses will be favorable, some unfavorable; some communications in the latter category will call attention to omissions in the literature citations, and some will mention yet unpublished relevant research. Some of my publications may initially be ignored entirely, or what appears to be entirely. I have received letters as long as 20 years after the publication date that request some details not given in the article.

I also have moments of pleasure when I find, as I did early in December 1997, that a former honors undergraduate (Andrew Heaton, then a graduate student at the University of Georgia), who worked in my laboratory for several years, was given, along with all other new graduate students, a copy of my 1989 paper “Speaking at Length” from *BioScience*. To learn that a publication, nearly a decade old, is still useful is very gratifying! When this recognition is coupled with the pleasure of receiving a substantial letter from a former student, it is doubly gratifying.

As a member of a number of editorial boards of professional journals, I am often asked to solicit manuscripts for the journals from distinguished scholars. When I do so, I always request an article on something that interests the potential contributor at present. Almost certainly, the resultant article will involve some recently perceived issue, likely to be critical in the future. Moreover, the issue is one on which the invitee is intensely focused, and, therefore, the contribution is quite likely to be exceptional. Furthermore, as Luciano Pavarotti said in an interview with David Frost on Public Television, “ I am famous because I take risks.” World-class scholars are risk takers and usually produce notable writings as a consequence.

I began my professional career as a freshwater protozoologist working on a field team, which required long hours of sitting on a chair bent over a microscope and studying highly perishable samples. In January 1995, I suffered serious blood clots in the deep veins of my right calf and was cautioned by my physician to avoid sitting for long periods of time on planes, in committee meetings, and the like. Furthermore, my eyes can no longer take the 12 or 14 hours/day of microscope work that they once endured. If I viewed myself as only a freshwater protozoologist, my career would have been over at least a decade ago, if not before. Since I viewed myself as a person who determined how ecosystems are stressed and how to restore stressed ecosystems to some semblance of predisturbance condition as a step toward sustainable use of the planet, my career opportunities were not eliminated by medical problems—they were merely

redirected. Early and frequent experience with the need for redirection in one's career is surely one of the key components of continuing professional productivity.

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DEDICATION

I dedicate this book to my parents, John and Eunice Cairns; to Jean Ogden Cairns, my friend and companion for over 63 years; to our children: Karen Jean Cairns, Stefan Hugh Cairns, Duncan Jay Cairns, and Heather Cairns-Chambers; and to our grandchildren: Hannah Cairns, Shannon Huecker, Moria Cairns-Chambers, Laura Cairns-Chambers, and Joel Cairns-Chambers with hope that our children and grandchildren will live in a sustainable world. My graduate students were a joy to work with; they entered as students and left as colleagues.

ACKNOWLEDGMENTS

I am deeply indebted to John Tanton for funds to transcribe a significant portion of the dictated first draft of this book. I thanked Gene Odum in the Foreword, but I feel it appropriate to again state my gratitude—had his suggestion not been made, I would likely have continued devoting all my professional time to journal articles, book chapters, or other books

The dictated, first draft of this volume was transcribed by Teresa Moody and Eva Call, and Darla Donald and Karen Cairns transcribed more recent, handwritten drafts.

This book probably would not have been possible had it not been for the retirement contract I signed with Virginia Tech in late 1994 and early 1995 (effective 1 June 1995). The contract permitted me to retain my office and necessary laboratory space for myself, my senior technician B. R. Niederlehner, and my editorial assistant Darla Donald, as well as for my remaining graduate students, the last of whom was John Heckman. Since I am a University Distinguished Professor of Environmental Biology, I reported, at that time, to President Paul E. Torgersen; Senior Vice-President and Provost Peggy S. Meszaros; Dean Robert C. Bates, College of Arts and Sciences; and Joe R. Cowles, Head, Department of Biology. At present, I am indebted to President Charles W. Steger; Provost M. G. McNamee; Dean Lay Nam Chang, College of Arts and Sciences; and Robert H. Jones, Head, Department of Biology. These individuals enabled me to continue my professional career with editorial assistant Darla Donald at this institution.

I am also indebted to former students Kenneth L. Dickson, William T. Waller, and Richard E. Sparks for remembering some events from our first years at this institution; to David Orvos, John P. Slocomb, Anthony F. Maciorowski, and James R. Pratt for recollections of events in the middle period; and Karen Holl and John Heckman for recalling events in the final years. David Hart, Director of the Patrick Center (at its inception, the Limnology Department) of the Academy of Natural Sciences of Philadelphia, helped with many historic events regarding that department. John Paul Cherry, Director of the Northeastern Regional Research Center, US Department of Agriculture Laboratory at Wyndmoore, PA, furnished information about persons for whom I worked during my employment there.

Also, much needed assistance was provided by Ruthanne L. Krauss (Swarthmore College) and Thomas Dolan IV, Stuart S. Bamforth, and Herbert Levi (original river survey team members). Roger Kaesler and Hampton Shirer (both University of Kansas) have gone well beyond the bounds of professional courtesy in helping me construct events of the past.

In addition, I am indebted to the many people with whom I had the honor of working and without whom my investigations would have been much more difficult and less exciting.

- Robert K. Enders, my advisor at Swarthmore College and throughout my career until his death
- Everett C. Hunt, Dean, Swarthmore College, who admitted me as a student after World War II, despite my unimpressive academic record and my truthful but unflattering admission to him that I chose Swarthmore because I could live inexpensively with my mother-in-law, whose house was within commuting distance
- David H. Wenrich, my advisor for both the MS and PhD degrees, who also recommended me to Ruth Patrick as protozoologist for one of the river survey teams
- Ruth Patrick, who provided my first professional position and with whom I had the pleasure of working from 1948 through 1966 and on whom I have depended for advice and counsel since then
- The 74 graduate students whose committees I chaired or co-chaired and with whom many of my papers were published
- The post-doctoral fellows who were responsible for invaluable assistance on numerous research grants and projects

Special thanks to Kenneth L. Dickson, Jeanne Ruthven, Richard E. Sparks, and William T. Waller, who moved to Virginia Polytechnic Institute and State University with me from the University of Kansas and the University of Michigan Biological Station and without whom neither the Aquatic Ecology Program nor the University Center for Environmental and Hazardous

Materials Studies (UCEHMS) could have been established. Dickson was particularly helpful in the latter because he stayed after graduation for many years as assistant director and a professor in the Department of Biology. Later, both James R. Pratt and David Orvos served as Assistant to the Director, UCEHMS.

Heartfelt thanks and appreciation to Darla Donald, who started working with me in 1975 and who has been of enormous assistance on all publications since then, including this one, and to B. R. Niederlehner, who began working with me in 1974 and who has been a tower of strength in both basic research and publications since then. Both Darla Donald and B. R. Niederlehner continued working with me when I officially retired in 1995. Without them, my productivity in publications and research would have been substantially reduced.

Last, but not least, I express appreciation to a long line of persons who transcribed my dictation—starting with my spouse Jean, who suffered through my first attempts at dictation. In order to produce a book for the American Institute of Biological Sciences Patterns of Life series, I had no other choice but to dictate in the evenings and on weekends and holidays. Each one in the succession of people since then is acknowledged in appropriate publications, including this book, transcribed by Teresa Moody, Eva M. Call, Darla Donald, and Karen Cairns.

Three people, with whom I have published for extended periods, deserve special mention: Arthur Scheier, through most of my years at the Academy of Natural Sciences; Roger L. Kaesler, through the years at the University of Kansas and many thereafter; and Donald S. Cherry, through practically all of my years at Virginia Tech.

CHAPTER 1

THE BEGINNING OF A LIFELONG INVOLVEMENT WITH POLLUTION: A MAJOR THREAT TO SUSTAINABILITY

Early Encounters with Polluted Ecosystems

The Schuylkill River that borders Conshohocken, Pennsylvania, was badly polluted as early as the 1930s from coalmines upstream and a number of industries. I was well aware, as one with a lifelong interest in fishing, that I could catch only carp and catfish in the river but the fish were big. Occasionally, I caught some fish other than these, but even the carp and catfish often had surface lesions. Nearby, within a 5-10 minute bicycle ride or a 15-minute walk from my house, was a disused stone quarry that had filled with water. Although the sides were extremely steep and the biological productivity must have been minuscule, I caught plenty of sunfish and an occasional bass. These fish were healthy, robust, and alert fish; a professional scientist was not needed to see the difference between polluted and unpolluted water.

Although the river near my home was polluted, I was well aware that some stretches of the river, both above and below Conshohocken, were less polluted. These areas were beyond the daily reach of a boy using a bicycle or walking as the two major forms of transportation, unless a lot of time was spent in traveling. Even so, I could roam relatively wild places on land and enjoy decent forests, meadows, and the like. In those days, when significant green space still existed between Conshohocken and Philadelphia, people who lived in an industrial town had access to relatively natural systems without having to travel by automobile to a more distant area. Later, of course, when I became old enough to drive a car (or even when I was younger and could persuade an older acquaintance to drive me somewhere), I could trout fish on Wissahickon Creek, which was, ironically, within the Philadelphia city limits. Wissahickon Creek was then stocked, but some trout were carryovers. Valley Forge was not too far away either, and, at that time, trout and chubs were still living in the little creek that flowed through the park. Most places that I enjoyed as a child are now either gone or are under intense pressure, which has caused substantive change in them ecologically.

I mention these details because, although in my childhood I recognized pollution, I still felt that industry and natural systems could coexist because uninhabited places remained that were relatively untouched. Even later, when I purchased property in a wooded area across the river from Conshohocken, foxes, herons, deer, pheasants, and other wildlife were present. Many areas that I had enjoyed as a boy were changed beyond recognition by then, but patches of relatively untouched natural systems existed, mostly in private hands.

Motivation

My first full-time job was in the Hamilton Paper Company at Miquon, Pennsylvania, where I stood at the end of a machine that produced very high quality paper and removed sheets that were defective and tossed them into a large wheeled bin behind me. Very shortly, I worked my way up to oiler, not because of any particular innate ability, although I was fairly large and moderately strong for a teenager, but because the accident rate was fairly high. At that time, the mill was in continuous operation because it was expensive to shut it down and start it up again. Three rotating shifts worked daily. As a consequence, one week I worked from midnight until 8:00 a.m.; one week had normal working hours; and the other was essentially from late afternoon until midnight. Every third Sunday as the shifts swung around, I worked 16 hours straight. The mill was very hot (dispensers with salt tablets were located at strategic intervals) and very noisy. I virtually had no social life because my free time varied each week and working those long hours was fairly tiring. As a consequence, I spent most spare time fishing and reading and, of course, saved exceptional amounts of money that proved useful for a college education.

Working in a paper mill was an extremely valuable experience. It gave me an understanding of the industrial workforce and management personnel that I otherwise would never have acquired.

Equally important, I saw, as a consumer, the system from which the products I purchased emerged. Finally, this experience enabled me to work regularly with industries later in my career on solving environmental problems because I had observed some of the difficulties they face. Almost certainly, however, this experience was the imperative to do something useful (resulting from my childhood background) and influenced me to include industry as part of the larger ecological system rather than avoiding industry's influence and studying pristine ecosystems, as did many of my colleagues.

At the outset of my career, I focused primarily on point source discharges (that is, the end of an industrial or municipal waste discharge pipe when it entered a river). Unless the pipes were widely spaced and relatively harmless, an aggregate effect became obvious. Further, most pollution abatement conferences I attended were organized by engineers who were concerned with the effectiveness of waste treatment systems. At that time, I was convinced that, if point source discharges were properly dealt with individually and kept within the assimilative capacity of the ecosystem into which they were discharging, environmental pollution effects could be minimized.

Then, Carson's *Silent Spring* (1962) hit the environmental world like a bombshell! Not only were pesticides and many other chemicals exposed as persistent, but also they traveled through the ecosystem in ways not appreciated before then. Despite the many attacks on her, Carson was really quite conservative in her approach, and the errors of fact were infrequent, trivial, and irrelevant to the main theme. The book definitely helped human society toward a reappraisal of existing policies and practices. The President's Science Advisory Committee issued a report in 1963 that was a fairly thoroughgoing vindication of Carson's thesis. Since I had frequent interactions with the industrial community at that time, I immediately observed that objective, detached science often evoked severe penalties when economic interests were threatened. One chemical company is reputed to have tried to persuade Houghton Mifflin not to publish Carson's book and threatened to withdraw advertisements in publications sympathetic to her point of view. Anyone contemplating an environmental career would be well advised to study the response to *Silent Spring* and more recent events, such as endocrine disrupters and global warming, before deciding to go public with any view of the danger to the human condition.

I have always felt that creating a new model that might cause a societal paradigm shift would be the most effective long-range strategy for protecting the environment. However, if publicity is the goal, battles are sure to attract more media attention. A systematic, orderly, reasoned approach is a sure path to anonymity! A few scientists have been recognized during their lifetimes at a national meeting of their discipline. However, recognition drops dramatically once one leaves one's discipline. Great communicators—who use printed words, such as Isaac Asimov; who use television and the printed word, such as Carl Sagan; who can demonstrate a devastating wit understood by a large viewing audience, such as Paul Ehrlich on the "Johnny Carson Show" and its contemporary equivalents; who can communicate the beauty, splendor, personal joy, and wonder about science to a vast array of people, such as Edward O. Wilson—are a tremendous inspiration to anyone interested in how the world works and mankind's place in it. Very, very few have caused major paradigm shifts or are remembered for them.

Darwin's *The Origin of Species* is arguably the most notable for a paradigm shift in the biological world and, of course, Carson's *Silent Spring* for the environment. Causing a major paradigm shift often inflicts severe penalties on the originator. Darwin hesitated publishing for years, anticipating quite well the consequences; his ideas are still attacked vigorously today. Distracters who perceived *Silent Spring* as having severe adverse economic effects attacked Carson personally, often viciously.

Most scientists sneak up on a problem incrementally, rather than making a great leap forward, such as the one represented by the double helix, which won the Nobel Prize for Watson and Crick. Except for anyone with similar interests, trends in thought patterns represented by a sequence of publications are likely to go unrecognized, particularly with the compartmentalization of disciplines by journal, department, annual meetings, tribal language, and the like. Hawken (1993) noted that scientists often argue interminably about particular leaves on a tree without referring to the tree with any notable frequency. Such an accusation was partly the result of

publication requirements of particular journals and the rarity with which broad overview articles are published. The general public is unlikely to have a clue as to what a particular, individual scientist is doing unless, as was the case with Theo Colborn, the path leads to a finding that shocks both the scientific community and human society (e.g., Colborn and Clement, 1992). Not surprisingly, Colborn has paid, and is still paying, a severe price as attacks on her views go well beyond normal scientific questioning characteristic of the peer-review process.

Perhaps this scrutiny is all well and good! The word *scholar* is not often used at present in an age of specialization. However, G. Evelyn Hutchinson indeed truly deserved recognition as a person of broad interests—interests that he integrated so beautifully that what appeared impossible seemed almost platitudinous. In his later years, he noted that young academicians knew he was a famous person, but were not quite sure what he had done. Although he and I had quite different backgrounds and I sometimes had difficulty communicating with him, his tutelage of the limnological survey crew at the Academy of Natural Sciences of Philadelphia changed my entire academic career. The association with a person of such talents must suffice for most scientists; it was certainly enough for me.

I am astonished that the major paradigm of my generation is exponential growth on a finite planet. Huge numbers of people contend that affluence is a "right" acquired when born. Civility and responsibility have dramatically diminished, and such abominations as "road rage" are of increasing social concern, as is violence in schools and society as a whole. In the United States, arguably the world's leading nation in technology, technology is worshipped, but the education that produced the science and technology is denigrated. Literate people are called names such as “nerd” or worse. I have always been optimistic about what could be done to leave a habitable planet for future generations, but I am in a state of barely controlled panic about what human society will do. However, a strident approach is even less effective than the systematic and orderly presentation of evidence. Perhaps consequences are the best, albeit the most expensive, paradigm shifters. Historic evidence shows that the universe does not accept ignorance as an excuse for inappropriate action.

The globalization of the world economy, the rapid emergence of persistent chemicals with adverse biological effects, the enormous consumption of energy, the burgeoning growth of the human population, the growth of affluence despite continued large-scale poverty, and the mass emigration from countries that are extremely poor or destabilized by war to such countries as the United States, Canada, and Australia may destabilize over decades even the most robust and stable societies. The amount of information available overwhelms even those of us who have devoted our entire professional careers to environmental problem solving; the volume of information surely is beyond the grasp of the average person in academe not focusing on environmental problems and is even more intimidating for the average citizen. Even those of us who have a reasonably comprehensive grasp of the major environmental trends still have trouble communicating them.

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APPENDIX 1*

PERSONNEL OF BIOLOGICAL SURVEY
Director: H. Radclyffe Roberts
Limnologist in Charge: Ruth Patrick

Scientists

Algologists		
John L. Blum, Professor of Botany, Cassin College		June 15–Sept. 15
John H. Wallace, University of Pennsylvania		June 1 –Sept. 15
Bacteriologists		
Donald Reihard, Jr., Pennsylvania State College	(half time)	June 15–Dec. 31
Raymond L. Smith, Pennsylvania State College	(half time)	June 15–Sept. 15
Entomologist		
John W. H. Rehn, Cornell University		June 15–Sept. 15
Invertebrate Zoologists		
Thomas Dolan, 4th, Cornell University		June 10–Dec. 31
Herbert W. Levi, University of Wisconsin		June 10–Sept. 15
Charles B. Wurtz, University of Pennsylvania		June 1–Dec. 31
Protozoologists		
John Cairns, Jr., University of Pennsylvania		June 10–Dec. 31
Dr. Mary Gojdics, Professor of Zoology, Beret College		June 15–Sept. 10
Vertebrate Zoologist		
James A. Jones, University of Minnesota		June 14–Sept. 15
Water Chemist		
John M. Ward, Rutgers University		June 4–Sept. 15
	(part time)	Sept. 15–Dec. 31

Scientific Consultants

Algologists
Dr. Francis Drouet, Curator of Cryptogamic Botany, Chicago Museum of Natural History
Dr. Gerald W. Prescott, Professor of Botany, State University of Michigan

Bacteriologist
Dr. Robert W. Stone, Pennsylvania State College

Chemists
Mr. Joseph Demann, Atlantic Refining Company
Dr. Roy Weston, Atlantic Refining Company

Entomologists
Dr. Philip P. Calvert, Research Fellow, Academy of Natural Sciences of Philadelphia
Dr. Herbert H. Ross, University of Illinois
Dr. Henry K. Townes, U. S. Department of Agriculture
Dr. Jay R. Travers, University of Massachusetts

Botanist

Dr. Norman C. Fassett, Professor of Botany, University of Wisconsin

Geologist

Dr. Jack B. Graham, District Geologist of the U. S. Geological Survey

Invertebrate Zoologists

Mr. Leonard M. Bennetch, Research Associate, Academy of Natural Sciences of Philadelphia
Dr. Libby Hyman, American Museum of Natural History
Dr. J. Percy Moore, Research Fellow, Academy of Natural Sciences of Philadelphia
Dr. Henry A. Pilsbry, Academy of Natural Sciences of Philadelphia

Limnologists

Dr. Arthur D. Hasler, University of Wisconsin
Dr. G. Evelyn Hutchinson, Yale University

Mycologist

Dr. Fred K. Sparrow, University of Michigan

Statistician

Dr. Ralph O. Erickson, University of Pennsylvania

Vertebrate Zoologists

Mr. Henry Fowler, Curator of Fish, Academy of Natural Sciences of Philadelphia
Dr. Ernest A. Lachner, Pennsylvania State College

Technicians and Assistants

Stuart S. Bamforth, Laboratory Assistant		June 15–Sept. 15
	(part time)	Sept. 15–Dec. 31
Hazel D. Barner, Laboratory Assistant		June 10–Sept. 15
	(part time)	Sept. 15–Dec. 31
James F. Bergseng, Field Assistant		June 15–Dec. 31
Edward Haldeman, Laboratory Assistant	(part time)	Sept. 15–Dec. 31
Robert L. Kane, Field Assistant		June 15–Sept. 15
Mariana F. Spangler, Secretary		Oct. 15–Dec. 31
Dorothy L. Wright, Laboratory Assistant		June 1–Dec. 31

*Taken from Appendix 5 (pp. 340-341) in Patrick, R. 1949. A proposed biological measure of stream conditions based on a survey of Conestoga Basin, Lancaster County, Pennsylvania. Proceedings of the Academy Natural Sciences Philadelphia 101:271-341.

CHAPTER 2

COPING WITH REJECTED COLLEGE ADMISSIONS APPLICATIONS: GETTING INTO SWARTHMORE COLLEGE

Had I not been admitted to Swarthmore College after my service in the Navy, the possibility of an academic career might have ended for me. After graduation from high school, I entered Pennsylvania State University and majored in biochemistry. An uncle, Walter Latshaw, had a PhD in biochemistry and left Kansas State University when the chemistry building burned to conduct air pollution studies for a mining company in Utah. His career choice was the only academic field I knew, so I chose biochemistry also. In the middle of my undergraduate career at Penn State, the Japanese attacked Pearl Harbor. My academic career was interrupted, as was virtually everyone's life at that time.

I met my spouse Jean at Penn State. She completed her degree in biochemistry, and we were married during the war. When the war ended, we had our daughter Karen.

I could have been readmitted to Penn State, but housing was scarce. Even had housing been available for married students with children, the GI Bill money I was relying on would almost certainly not have been adequate to cover living costs. My mother-in-law, Eleanor Ogden, owned a house where Jean and Karen were already occupying one room. The three of us could live there if I enrolled in a Philadelphia-area college. So, I immediately began making the rounds of the colleges in the larger Philadelphia area within reasonable, and even unreasonable, commuting distance. The first five institutions quickly turned me down. Since many veterans were attempting to enroll, the decision of these colleges was quite rational, but very unfortunate for me.

Finally, I approached Swarthmore College; I knew nothing of its stature or high admissions standards. My first interview was with Dean Everett Hunt. This interview immediately alerted me to the fact that Swarthmore College was indeed unique. My encounters at the previous institutions had been with staff members in the admissions offices, and I had not even encountered a faculty member, let alone a dean. I did initially encounter staff members at Swarthmore also, but they immediately sent me to Dean Hunt's office. Regrettably, I cannot remember my conversation with him in detail, but I did discuss my past academic record, why I thought I would do better, and what my hopes were for the future. I had refocused my academic interest on biology, as a result of reading while I was in the Navy and of talking with some of the persons with whom I served. One part of the interview, however, I do remember vividly. Dean Hunt asked me how I had chosen Swarthmore. I described how I had taken a map and, with a compass, had drawn a circle of a certain diameter around my mother-in-law's house, where my family and I could live. I related how I was hoping I could find a college within the circle, which represented a reasonable commuting distance, willing to accept me. Dean Hunt then had what appeared to be an uncontrollable coughing fit, which only weeks later did I realize was almost certainly an attempt to cover his uproarious laughter. He then wiped his eyes and, to the best of my recollection, said: "that may be the most honest answer I have ever heard." He sent me to interview with Robert K. Enders, then acting head of biology, because Laurence Irving, the chair, was in Alaska or some other part of the world.

When I entered Professor Enders office, I noticed that he was informally dressed, had his shoe resting on his desk, and was gluing on a rubber sole, repairing ones' own shoes was a common practice in those days. I remember a series of questions that made me aware that Dean Hunt had phoned Enders and given a thumbnail sketch of our discussion. One question I remember in particular: if there were freshwater sharks in Central America and most sharks were found in salt water, how could I explain this? Such questioning and probing were not done just during interviews. Months later I remember talking with Professor Enders in the laboratory while he was obviously keeping one hand behind his back. Suddenly, without any warning, he threw a small skull into a container filled with cotton. The cotton was so plentiful that the skull disappeared from view. Enders then asked me to comment on the skull based on what I had briefly seen. Fortunately,

airplane recognition in the Navy had helped me develop some immediate recognition skills. I was able at least to guess that it was the skull of a bird and made some other comments about it.

After my initial interview with Enders, I was asked to furnish letters of reference. One letter of recommendation was from Dr. "Pappy" Willits, for whom I had worked just before the war at the US Department of Agriculture Research Laboratory on Mermaid Lane in Wyndmoore, Pennsylvania. Even though I was probably the lowest rank possible in the employment hierarchy, I was assigned to a research project extracting rubber from North American plants found in arid climates. Since the extraction was a continuous process, I was placed, after a brief period of training, on the midnight to 8:00 a.m. shift. My task was fairly straightforward. I had to change samples from one extraction process to another, weigh dried residue, and the like. Periods elapsed when I could read or whatever I chose while waiting on completion of processes. I indicated that I would prefer washing laboratory glassware or doing any other work that I could do with relatively minor instructions and without supervision. Apparently, the fact that I could work well without supervision and was more interested in helping in the laboratory than in filling in time left an impression that was not quickly forgotten. Consequently, I had one good reference from a Swarthmore graduate, although I was not aware at that time that Willits was an alumnus.

The Swarthmore faculty members I remember best are Ruth McClung Jones, Walter J. Scott, and C. Brooke Worth; all frequently advised me. I majored in biology and minored in chemistry and physics, both of which were extremely helpful later in my career.

I could not afford to eat in the Swarthmore dining room, so I took sandwiches, which I ate in my car or in an empty classroom. As a consequence, I have a very dim memory of classmates, and they probably have no recollection whatsoever of me. My anonymity is not at all surprising, since most people lived on campus, ate in the dining room, and socialized together. At times when the car did not work, I commuted to the 69th Street bus terminal from Havertown, Pennsylvania, and then to Swarthmore. Counting waiting time between buses and so on, this commute took at least an hour each way—another reason for not socializing. I do recall having trouble with German at Swarthmore and was approached by another undergraduate proficient in German who offered to tutor me at no cost. Clearly, Professor Enders or one of the other faculty members had a hand in this assistance. So, classmates helped when I needed tutoring; but, when I currently read the alumni news, I regret missing the strong bond that has lasted for years between other Swarthmore graduates.

Actually, I spent so much time on academic courses that I did not interact to any degree with anyone outside the classroom. Jean and other family members were extremely tolerant because I spent practically all my time either in class, commuting, or studying, including weekends. I was determined to get high grades; I worried that the years without attending classes would keep me from doing well.

After a few weeks of attending class, I became aware of Swarthmore's reputation and the fact that everyone around me was academically successful. Further, most students had not spent time in the armed forces. It was daunting to find that the standards were far higher than I had thought. Nevertheless, my grades were satisfactory, due in part to the time I invested, which almost certainly was more than most other students. My classmates, all younger than I, went to dances, concerts, and the like—I studied.

I graduated in 1947 after completing an advanced chemistry course at the University of Pennsylvania during the summer following completion of the Swarthmore courses. My diploma was delayed until satisfactory completion of the summer chemistry class. Then, suddenly, my options increased, and several graduate schools were available to me. However, graduate student support was scarce in those days, and my tolerant mother-in-law continued to house us while I went to graduate school at the University of Pennsylvania. Fortunately, for her and for us, at the completion of my first academic year at the University of Pennsylvania, I was offered a full-time summer position at the Academy of Natural Sciences in the newly formed Limnology Department under the direction of limnologist Ruth Patrick. This job was extended to a permanent position at the end of summer, which took care of my financial problems. Even so, I still had a time management

problem—how to finish a MS and then a PhD while holding a full-time position that often included out-of-town trips for two or more weeks. Nevertheless, from the time I graduated from Swarthmore, I always had several career options available.

Although I officially retired from my last professional position on June 1, 1995, I still mentored students, especially probable "late bloomers" who faced similar problems to those of mine after World War II. As a consequence of my own experience, I have always tried to emphasize persistence when advising both my assigned advisees and anyone else who asked for help. Since becoming a faculty member myself, I have often spent hours discussing career options with students that I had never seen before and who were not formally assigned to me. When they thank me for taking so much time, I always tell them it is a partial payment on an old debt. In fact, it often helps them to hear my story if they are late bloomers.

A major lesson from my Swarthmore experience is not to become discouraged, even when things look very grim indeed. Fortunately, I did not focus on what would happen if I did not get into Swarthmore, probably because, despite all the setbacks, I still had confidence in my abilities. Since then, my career has had a few extremely crucial points when the situation seemed, if not hopeless, extremely discouraging. I never lost confidence that the future would not be bleak. Undoubtedly, the experience at Swarthmore was responsible for this belief, and it has been a blessing for over five decades.

Another major lesson from the Swarthmore experience is to always seek out people who are not afraid to make professional judgments and who use institutional criteria and standards as guidelines rather than rigid standards. In retrospect, until I interviewed with Dean Hunt, not a single admissions person had asked me how military service during World War II had changed my attitude towards education. No one before Dean Hunt attempted to determine whether my motivation had changed or whether I had achieved a better focus on career aspirations and the like. However, it is a mistake to assume that only persons holding high rank can exercise judgment—the secretary who sent me to see Dean Hunt had the same evidence that resulted in rejection elsewhere.

In the process of cleaning my desk at home a few years ago, I uncovered Swarthmore College's *Halcyon II* from 1997, which was produced to commemorate the 50th reunion of the class of 1947—my class. An amazing number of people had submitted fairly extensive summaries of their careers since graduation, undoubtedly due in large part to the persistence and enthusiasm of Cliff and Mildred Gillam, editors of the volume. These classmates of mine were, and are, interesting people; some were truly exceptional! I was saddened to realize that I did not know a single person well and only recalled a few names of people who sat beside me in histology laboratory or something of that sort. Out of the entire class, I did not have a single friend with sufficient common experiences so that we kept in touch in any meaningful way. Due to the circumstances I described earlier, this lack of camaraderie was easily rationalized, but was not grounds for satisfaction. I could have rectified this lack of knowledge of others, to a modest degree, if I had attended class reunions, which, from descriptions in the *Swarthmore Alumni News*, were memorable and nostalgic for participants. However, I was usually in the field working during reunion times. In 1995, when I stopped going into the field, I was faced with travel restrictions for health reasons, which is why I stopped fieldwork in distant places. Our daughter Heather attended Swarthmore for only one year, even less time than my short experience, and had to leave because of asthma. Heather lived in a dorm, ate meals in the dining room, and, I am sure, was a typical Swarthmore student; I was an atypical commuter who could not even afford lunch in the dining room. Heather can remember individuals extremely well, even their eccentricities, values, ethics, etc. If that much rapport was established in only one school year, it is quite clear that the level achieved for someone attending for four years must be truly exceptional. In contrast, during my first college experience at Penn State, I was a very social creature. I can remember the names of many of my classmates, particularly those in my fraternity Alpha Zeta, and I can picture many of them in vivid detail and even still correspond with some of them. It was at Penn State that I met and courted Jean, who somehow managed both good grades and a social life. Recently, I wrote to a fellow student from the Penn State days who had lost his spouse and to another fighting a major battle with prostate cancer. I do not know a single,

former Swarthmore student to whom I could even come close to writing a letter of that sort. In essence, my vivid memories of Penn State all involve fellow students, and my vivid memories of Swarthmore involve faculty. My father had the gift of being able to approach a total stranger and walk away with a condensed version of the person's life history. He could learn more about a person in less than an hour than most people could during years of acquaintance. I have never had this gift, but I do think that the Penn State period demonstrates clearly that I am not inherently antisocial.

World War II certainly matured all those who experienced it more rapidly than would otherwise have been the case. However, World War II and the associated maturation are only partial explanations of the differences in my two college experiences. The major difference was the realization, however poorly articulated, that I could not achieve the professional goals that would provide enduring satisfaction without considerable focus. This degree of focus diminished both the depth and number of purely social relationships. I have only recently reestablished communication with some of my cousins after a lapse of many decades. On the other hand, the number of professional relationships has grown enormously and covers many areas of the world. Naturally, these professional relationships also have a social side, often a very satisfying one. Some of my Russian colleagues have even honored me with poems, and I regularly get birthday cards, Christmas cards, and emails from China, India, Indonesia, Europe, and Australia. In many cases, I have never met these colleagues, nor is it likely that I will ever do so. Yet we exchange photographs and other items quite peripheral to our professional relationship, although very satisfying.

In the United States, the present rage is "having it all," that is, a successful professional career, enjoyable leisure time, exciting vacations, a loving family, and a large circle of friends. I have the good fortune to know multidimensional people who actually have succeeded in this endeavor, but they are exceedingly rare. Most people are fortunate to manage exceptional success in one area and moderate to poor success in others. When I returned from World War II, I realized that I had to make enormous sacrifices to regain lost intellectual momentum and to go well beyond my initial performance. I never dreamed I could "have it all," but I realized that my focus would have to be intense to achieve professional success. While I do regret the lost social interactions, I realize that everyone has limited energy and time. I find great satisfaction in my professional career, which would not have been possible if I had given it less attention.

In retrospect, I count three main lessons from my Swarthmore experience: (1) even though I had an unexceptional beginning to my academic career, an institution was willing to give me a second chance if I was sufficiently persistent and was willing to follow through with performance when given the chance; (2) unless my failures were truly spectacular, they were likely to be forgotten if followed by some degree of success; (3) problems that seemed intractable seemed less so after modest applications of reason.

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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CHAPTER 4

THE PERPETUAL BALANCING ACT: ECONOMICS, TECHNOLOGY, AND ECOLOGICAL SUPPORT SYSTEMS

Before the agricultural and industrial revolutions, the life support system for humans was entirely ecological. However, present population size, distribution (highly urban), and level of affluence have made humans dependent on both a technological and biospheric life support system. The perception of close ties between human well being and the health of the technological/economic system has diminished the concern for the health of ecosystems that furnish services upon which human survival depends. Many people feel that those attempting to preserve and restore ecosystem integrity are more interested in the well being of owls, lemurs, and wolves than other humans. Perhaps some are, but this fact does not lessen human dependence on the planet's biospheric life support system.

Some economists and others believe that technology has made obsolete such concepts as carrying capacity and limits to growth. Technological innovations such as fertilizer, irrigation, highly mechanized agriculture, elevators (permitting more people on a finite amount of the planet's surface) and air conditioning have permitted an expansion of both population and affluence that did not seem possible earlier. However, these innovations have come at an enormous price—the diminution of natural capital (e.g., topsoil, old growth forests, clean air and water, and so on). Such rapid growth is unsustainable. Furthermore, technology has been used to circumvent or delay policy development. For example, a US political decision on limiting greenhouse gases that produce global warming has been based more on technology than reducing dependence upon fossil fuels. Reducing resource consumption and the size of ecological footprints have not been given serious attention in the US, which has a large ecological footprint and consumes 25% of the world's resources, although the US represents only 4% of the planet's human population.

The US Great Depression resulted when the economic system suffered severe reverses, and, with far less money to buy its products, the technological system also declined. By comparison with many other people of my age, the Depression had comparatively little effect on me personally because my father remained employed. As a freight solicitor, his salary was almost certainly adversely affected because the economy was devastated and, therefore, the shipments, such as steel, lumber, and grain were definitely reduced. Nevertheless, I was always adequately fed, had adequate medical care for those times, and remained in our house with sufficient heat. My father worked in an office in Philadelphia, but he traveled a good deal to where the shipments were. As a consequence, my family was better situated than many other people. The parents of many of my contemporaries were unemployed or laid off from time to time from their businesses, such as clothing shops, barbershops, and food markets, and had relatively little money.

Economic matters of the time, such as who was working and who was not, and whether things would improve or continue to deteriorate economically were openly and exhaustively discussed. These topics were matters of considerable interest to me because continuously I could see the consequences to the people whom I knew well. Entire families listened to US President Franklin D. Roosevelt's "fireside" chats on the radio. These programs were discussed in considerable detail and were often accompanied by heated disagreements and arguments for days afterwards. This situation was the "real world," not what was shown on the movie screen on Saturday. The valuable lessons for the children of that era were that all opinions were not equally valid and that unpleasant consequences often resulted from errors in judgment. However, the biospheric life support system was never discussed.

One of the lessons I gleaned from the adult conversations was that things were pretty good between World War I and the Depression and that *the latter arrived unexpectedly for everyone*. From that time on, one was to assume that, no matter how good times were, things could deteriorate quickly and good sense dictated being prepared for bad conditions. Preparation for bad times could be accomplished by not accumulating too much debt; by always saving a portion of one's income,

however meager; and by sacrificing high income to seek employment that was comparatively resistant to bad times, such as education or delivery of essential services. In contrast, others believed in spending money as fast as it arrived, because depositing it in a bank was risky since banks had failed. An extension of this view was that, if one lived for the moment and did not save for bad times, the government would eventually take care of those in need. The current extraordinary level of credit card debt in the 21st century seems to support this view.

A second memorable event for many people in my generation, particularly those on the East Coast of the United States, was the New York World's Fair of 1939. The message of the World's Fair was unmistakable—technology would solve all problems! It would provide endless comforts and jobs, while simultaneously reducing human physical labor. Instead of smoky, grimy industrial cities, people would live in sparkling new dwellings with glamorous transportation readily available, and, most important, all these advancements would be available to ordinary citizens. We high school students visiting the Fair were ecstatic that the future held all these utopian vistas for us; who were we to doubt the appearance of these technological marvels after seeing them at the Fair? The long-term lesson from this glimpse of the future was unmistakable—do not place too much faith in unproven technology.

Along the same lines, World War II boosted faith in technology enormously. A relatively large portion of the American Pacific fleet was sunk at Pearl Harbor on December 7, 1941. Just a few years later, armadas that stretched from horizon to horizon retook the islands taken by the Japanese after Pearl Harbor. These enormous armadas represented only a portion of the resources available, since the war in Europe took priority. Suddenly, everyone was working, either producing or using the products of a technological system. Moreover, American technology was the best and produced not only enough for Americans but for the Allies as well. Nowhere was this more evident than in the Pacific theater where small islands, such as Iwo Jima and Tarawa, were literally surrounded in depth by American technology.

This awe of technology continued after World War II when quality, low-cost housing (although often in homogenous tracts) became more generally available for more people than ever before. Practically every family had at least one automobile, and food was abundant at relatively inexpensive prices.

With these events from history as a background, I was shocked when I joined the river survey team at the National Academy of Sciences and saw, first hand, the downside of technology (i.e., pollution). I was suddenly confronted with the effluvia of industrial and municipal systems at a huge number of locations in an entire drainage basin not all that distant from where I grew up. I realized quickly that the technological system, which had given so many people "the good life," could also destroy, or seriously abuse, ecological systems. I had felt an affinity for natural systems my entire life, and my educational experiences had enabled me to appreciate them even more. The systems were far more complex than I ever imagined and far more vulnerable than I had ever dreamed. Although many years passed before I first encountered Aldo Leopold's statement that to be an ecologist was to live in a world of wounds, my academic experience had heightened my awareness of ecological damage to a far greater degree than that of my fellow citizens. This dichotomy of the co-existence of natural systems and technology exists to this day. Some years ago, a colleague, who has a deep respect for the interdependent web of life, gestured toward the view of a typical upper middle class neighborhood from a building where we were meeting. New, well cared for homes were each on separate plots of land and surrounded by well kept lawns and various ornamental vegetation, mostly non-indigenous species. In the distance were mountains consisting of second- or third-growth forests. My friend commented, looking at this view, that one would never guess there were environmental problems. I told him that, to the contrary, what I saw was an enormous increase in impervious surfaces (roads, parking lots, roofs, and driveways), lawns that required fertilizers and pesticides to maintain the monoculture, and enormous expenditures of energy to keep the grass trimmed. Furthermore, per capita energy use was high for air conditioners, heating, transportation, and grass cutting, not to mention development of roads, sewer lines, and other development needs. My viewpoint was the result of observing ecological damage elsewhere

that first produced and then maintained this misleadingly idyllic setting. Not long after that conversation, I heard him speaking against increasing the size of a parking lot that would add to the impervious surfaces of the area. He, too, now lives in a world of wounds, and I sometimes wonder if I should not have spoken. Neither of us will likely live long enough to witness environmental protection and repair that exceeds environmental destruction and damage.

Fortunately, my spouse Jean shared my love of nature, so we lived (until 2000) on an 8.5-acre hillside tract covered almost entirely by trees. Our 22-foot x 44-foot two-story house was surrounded by trees (with no lawn) and was reached by a narrow gravel driveway. The hillside was so steep and the trees so close that a view from second-floor windows gave the impression of being in a tree house, which our children frequently noted. Trees fell, hit the house, and damaged the roof. A friend removed trees that fell on the driveway and used the wood for fuel in his stove. Otherwise, the dead trees stood for our woodpecker friends, and trees felled by ice storms and high winds stayed on the ground to provide habitat for other creatures. In terms of space, all but a tiny portion of our 8.5 acres was for all other creatures. If it were not for the very noisy truck traffic on a nearby bypass and the lights of the other houses at night, we could imagine nature to be dominant. Since my wife and I were then both 77 years old and since ice storms had left us without power for as much as a week, our time in such continuous, close contact with nature was nearly over. However, we remained as long as we could because it gave the illusion of living in a different world than the one we actually inhabited.

I am convinced that humankind's present addiction to technology and exponential growth is suicidal for human society. Technology helps control my blood pressure and asthma and provides many amenities; however, it is simultaneously both a danger and an opportunity. I share Speth's (2004) belief that, if Americans grasped the full dimensions of the global environmental crisis, they might rise to the challenge and first diminish and then reverse the present rate of environmental damage, which they have caused even far beyond their own borders. Both US political parties give only "lip service" to the environmental crisis, so effective remedial action is minimal. Humankind is acting as if it is immune to the laws of nature that affect all species on the planet. Sustainable use of the planet will require extraordinary changes in civilian, corporate, and governmental behavior and practices. Human society is still enamored of perpetual economic/technological growth on a finite planet. The decades of robust scientific evidence on the deleterious effects of unrestrained economic/technological growth are largely ignored. I can only hope that this disregard will change.

I entered the field of environmental biology over half a century ago. Although many success stories can be noted, the condition of the global biospheric life support system has dramatically worsened. New, major concerns have emerged, such as global climate change. In addition, the human population is expected to increase by 3 billion in the first half of the 21st century. In 1948, science was respected; at present, it is being distorted to conform to political ideologies. Scientists, including many Nobel laureates and members of the US National Academy of Sciences, have protested this misuse of science without substantively diminishing the misuse. At least one major environmental catastrophe may be needed to cause a major change in human behavior. Individuals who flagrantly misuse science must be publicly discredited in order to avoid future problems resulting from misuse of science. Only then will living sustainably have at least a chance.

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CHAPTER 5

THE JOYS OF BEING ON A TEAM OF SCIENTISTS

For most of my tenure at the Academy of National Sciences Philadelphia (ANSP), I had administrative charge of the river survey crew on field trips. Ruth Patrick designed the sampling programs, picked station locations, oversaw the overall design and extramural funding, and, especially in the early days, frequently collected diatoms and other algae. I made arrangements with motels, shipped equipment, and the like. Of course, I did not have the detailed taxonomic and ecological knowledge of each of the groups that the specialists on the team had. Typically, the team consisted of an algologist, an entomologist, an invertebrate zoologist, a protozoologist (generally me), an ichthyologist, and a water chemist, who also did minimal bacterial counts such as total counts, coliform counts, etc. A field assistant also helped set fishnets and other sampling devices for fish and collected fish. The entire crew collaborated on dredging, which was a labor-intensive activity. During most of my time at ANSP, I identified protozoans; in the 1950s and early 1960s, I also collected fish when no ichthyologist was available. The camaraderie of the team was exceptional, and no one showered or changed from field clothes until the entire team had returned to the motel. Since the chemist and I worked with highly perishable samples, we were always the first to finish in the field. Sometimes I checked the fishnets and other sampling devices, such as hoop nets, before collecting protozoan samples. The entire field crew would collaborate on seining and other fish collecting activities that required substantial numbers of people.

The aquatic toxicology program was continuous since Arthur (Art) Scheier, a chemist, was never in the field and could work continuously on toxicity testing. Art and I worked together for many years and published numerous articles together. The collaboration was perfect, even though Art was only a half-time employee in the beginning. He also had a degree in optometry but was just getting a practice started. Even after he became established, we continued to work together because we enjoyed the research. When I left the Academy in 1966, Art took over the toxicity program. Before Art died, I had the pleasure of seeing him again when his son married the daughter of our friends in Blacksburg.

Although toxicity tests could be carried out by one person when the tests involved only one species and short time spans in containers low in environmental realism, we worked as a team as the number of species gradually increased to communities and both the complexity and time span increased. Our major interest was primarily in complex systems, which are best studied by a team. The river surveys were conducted in different parts of the contiguous United States, in Canada, and occasionally elsewhere, such as the Amazon River in South America. Generally, these surveys were pre-construction surveys for industrial plant sites. One early employer of the river survey team was E. I. du Pont de Nemours & Company. Just after the completion of the Conestoga and Brandywine River surveys (which brought me to ANSP), the two field crews, totaling some 35 persons, were "downsized" (to use current jargon) to just a few of the original crew members: Charles Wurtz, John Wallace, Jackson Ward, Thomas Dolan V, and me.

At that time, Crawford Greenwaldt was chief executive officer of du Pont and was also a member of the board of trustees of ANSP. His hobby was photographing and studying hummingbirds, which is undoubtedly at least one of the reasons why a captain of a major industry served on a board of an essentially biological organization. When Patrick told Greenwaldt what the Limnology Department was planning, he decided in favor of a baseline pre-operational river survey before each new du Pont plant began operating. The biological conditions, as well as the water chemistry conditions, would be known before the du Pont plant had an opportunity to affect them. The design of the surveys included reference or control stations upstream of the proposed plant site, a second sampling station just below the area where the engineers predicted the waste discharge would be fairly well mixed with the river water, a third sampling station downstream in an area where an oxygen sag might be expected to occur if organic waste were involved, and, finally, one or more delimiting stations well below the area where any effects were likely to occur. Generally, a

simulated plant waste was used for toxicity testing, which, together with other evidence on predictive rates of waste transformation and the like, could be used to set the delimiting stations. This experience in river surveys heightened my awareness of the connection between laboratory predictive models and the response of complex natural systems. The survey team returned after the plant became operational to decide if the stations had been set properly. The team also observed any effects that might be attributable to events other than the plant's operations, such as channel dredging. For very important sites, such as the Savannah River, Sam Roback and I often did several "mini-surveys" a year to determine if either insects or protozoans showed any indication of deleterious effects.

What worried me about the sampling were the long intervals between surveys, even if mini-surveys decreased the intervals. Patrick reduced this concern by developing the Catherwood Diatometer, which was placed permanently in the river at various sampling stations. Slides were collected and shipped to ANSP at weekly or greater intervals to investigate any appreciable changes in diatom community structure. This design was a vast improvement. However, lag time was still significant because shipping took a few days, even by air, and the analysis of the slide could take several days to a week. Although river surveys done in the late 1940s and early 1950s were predominantly for plants under construction or to be built, surveys inevitably began to be conducted on a significant number of existing plants.

I treasure the field period and realize it was a necessary precursor to my most productive research years as measured by both quality and quantity of publications. The most valuable part was sharing ideas with and learning from other team members, which constitutes an incredibly valuable experience—an experience not easily obtained in any other way. Although I benefited enormously from the skillful and patient guidance of Robert Enders while I was an undergraduate, I did not have the opportunity to compare my performance in depth with others in my peer group due to lack of interaction with them (discussed earlier). My first opportunity to make a comparison of my performance to others was service on one of Patrick's two limnological field teams while I was still a MS candidate. Within my field team, and compared with most members of the other field team, I felt I was doing an acceptable job. I had less experience than virtually all the other taxonomists, especially Dr. Mary Gojdics, the protozoologist on the other field team. Consequently, I was fixed on my responsibilities. Later correspondence with Herbert W. Levi, Professor Emeritus at Harvard University, has really startled me. He remembered that evenings and weekends were free for the crew to play tennis, to go home weekends if they lived in Philadelphia, or to explore the Lancaster County area. I hasten to add that Patrick, with responsibilities as a professional algologist and also head of the entire operation, was in the laboratory at least as much as I was on the evenings and weekends. It is probably fortunate that I did not realize how much I was playing catch up with the rest of the crew, despite the fact that I was working with perishable samples and their strict time deadline. I gained confidence in my academic ability that I had never had before and probably could not have acquired easily under other circumstances, even in graduate school. Equally important was the fact that I had managed the transition from an isolated individual to a member of a team.

Some people worked with highly perishable material (Mary Gojdics, the bacteriologist, the chemist, and me), and some with material that could be stored for enormous periods of time before identification was carried out (especially diatoms used by Patrick and John Wallace). Some people had to sort and preserve their collections, and even make preliminary identifications. Consequently, a gradient of time was allocated for both collection and identification, which depended on the individual group. I knew that I had to start new collections within 48 hours. If I did not, some specimens would have died or reproduced, thus distorting the community sample. Some species I worked with were very fragile and low in numbers. Preservation would have eliminated valuable characteristics, such as a contractile vacuole, so I had to use speed identification with drawings when the keys did not provide enlightenment. In some cases, I had to be satisfied with the generic name without identifying to species because the community structure was of major importance to my investigation. I needed the number of different kinds of species, with some attention to abundance, rather than extremely precise identifications for each component species. Fortunately,

training in rapid aircraft identification during World War II prepared me to gather multiple attributes or characters simultaneously rather than sequentially, as one is tempted to do with a dichotomous key. Nevertheless, team needs had a major influence on the way I approached problem solving.

The team was studying a large system and, even though I was only studying a component of the large system, the work still provided a system perspective. The entire project was financed with extramural funding, which gave me my first exposure in depth to that important area of my future professional life. Also beneficial was my being able to participate in the completion of a final report suitable in condensed form for publication in a professional journal (e.g., *The Proceedings of the Academy of Natural Sciences of Philadelphia*), an experience that was to benefit me greatly throughout my career. In a very real sense, being on a team provided a means of self-evaluation that would have otherwise not been easily possible while, at the same time, enlarging my perspective enormously. I learned a great deal through observing Patrick lead two teams at one time.

Money has always been important to most scientists for purchasing equipment; providing salaries for technicians, graduate students, and hourly employees; and ensuring that one had some degree of control over personal time management (Isaac Asimov is reported to have replied, when asked to define academic freedom, "extramural funding"). The ability to see the connection between one's own area of interest and other parts of mainstream science, as well as society, increases substantially the probability of acquiring extramural funding. For example, Patrick saw the connection between kinds and abundance of diatoms and other species of aquatic organisms to water pollution, convinced others (both academics and corporate executives) of its utility as an index of pollution, and was able to obtain substantial extramural finding for a long and productive academic career. Additionally, she was able to give others, including me, a start on academic careers with this funding. She also saw the connection between applied and theoretical research—primarily that a good research project design could include elements equally useful in both areas. Funding not available to individual specialists was available to teams of specialists focused on a unifying theme. Ironically, to learn the factors leading to a significant degree of academic independence required sacrificing that independence temporarily.

One also sees quite a variety of personality types on field teams. Some members like to sleep late (in terms of field teams, to 7 or 8 a.m.). These same individuals may cheerfully work till 1 or 2 a.m. on a regular basis. On the other hand, some field crew members like to get out on the river soon after sunup, especially in summer when this time is the coolest part of the day. These individuals generally retire early. Some people are not particularly sensitive to local customs. Since the crew often depended on local citizens for information, supplies, help with repairing motors, etc., individualism had to be redirected sometimes so that it did not have deleterious effects on team acceptance locally. I remember one situation where a male crew member wore purple toreador pants to a restaurant in Allendale, South Carolina, which in the early 1950s was a sleepy town on US 301. Although Patrick directed almost every operation at the outset of the Conestoga River Survey, I gradually acquired, as the department grew, other non-academic responsibilities for making arrangements on field surveys for such things as motel rooms. None of us on the team could replace any others in the areas of specialty, but some things I did could have been done by anyone on the team who was willing to do so. Somehow I gradually acquired more and more administrative responsibilities. This management duty expanded my perspective, but I also incurred a price in loss of time for academic matters, such as preparing manuscripts for publication, etc.

The downside of serving on a field team is quite apparent: (1) the pace and direction are set by the group, (2) the perception of others that field team members cannot function well as individuals, (3) time away from home is too much for many individuals (being part of team is not the only way to be stressed in this way; research projects near or far can take an individual away from home), (4) the level of physical stamina is difficult, but not impossible, to maintain in the later part of one's professional career (individual field work can also be very demanding, but one may have more options to reduce stress when working alone that one has as a team member; mid-course adjustments are always easier for an individual than a team), (5) the opportunity to attend many

important professional meetings is reduced because of conflict with team schedules, and (6) more dependence on the performance of others than does "so called" independent research (which still generally involves others). Some of my colleagues participated in team investigations for their entire professional careers, but they were notable exceptions. It is possible to do individual research and also be on a team, but it is backbreaking work in terms of the effort required. One also hopes to have a satisfactory family life as well. It was clear that this was possible since I observed Patrick managing this tricky time management problem. The toll for even the successful individual was clearly a cause for concern. I managed to maintain this team schedule for 19 years (1948–1966). Did I spend too much time on this balancing act? I'll never be certain!

A major problem of working in teams is keeping the balance. Small teams of two or three people, who work together reasonably harmoniously, are in an entirely different category than teams of 14 or more. As the group increases in size, responsibility to it diminishes for many individuals, and vulnerability that results from individual negligence or lack of responsibility increases dramatically. On the other hand, the cash flow that maintains research momentum is definitely enhanced by team funding, which permits activities to continue that otherwise might be interrupted. Everything considered, it seems to me that being on a team increases the diversity of experiences and capabilities of an individual and, thus, leads to the sort of stability in extramural funding and associated activities that diversification in a stock index does. This association also ensures that one cannot achieve the excellence that comes from totally independent research that is continuously funded.

Today, a scientific team is often a collection of individuals who are temporarily assembled for a specific task. Possibly the best known scientific team is the one that assembled the atom bomb during World War II. However, the teams most familiar to American citizens are baseball and football teams and, in other countries, teams such as cricket, basketball, and soccer. When I was a boy in the 1920s and 1930s, sports teams were relatively stable because players remained on one team for perhaps their entire careers. Teams of that era also had a loyal following and, presumably, were also loyal to the area that supported them. Today, players have enormous mobility, and entire teams disregard loyalty to the area and change geographic locations depending on facilities and contracts offered. I raise these issues, despite the fact that I am not a knowledgeable sports fan, because some of them parallel situations in the academic world. I have struggled with some of these issues (with modest success) throughout my career, but remain ambivalent to this day, as the following brief discussion shows.

When I joined the two river survey teams in summer of 1948, both were led by a single person (Ruth Patrick) and shared a chemist (Jackson Ward) and a bacteriologist (Raymond Smith). Although composed primarily of specialists in various subdivisions of limnology and aquatic biology (see Appendix 1 in Chapter 1), the teams had a common objective. Their individual efforts, while important in their own area of specialization, were only notable in terms of the grant if the results could be integrated effectively and conclusions drawn from a synthesis of all the individual efforts. The teams suffered from turnover of personnel. Patrick and I were the only two members still employed from the original 1948 survey crews when I left ANSP in 1966. However, many original field team members remained my colleagues for a decade or longer. Even so, substantive personal and professional adjustments were necessary each time a new team member arrived. A team with extended relationships among members has a much better chance of achieving information synthesis than a recently assembled team. The danger is that the team may become too satisfied with a particular approach—too rigid both individually and collectively. The dangers of a standardized approach are dramatically increased if competition introduces "efficiency" (i.e., cutting costs) as a major factor. Teams financed with "soft money" are particularly vulnerable to this danger.

During my last year at ANSP, the pollution problems being assessed required a much greater mix of disciplines than were available on a fixed composition team. Furthermore, each problem was sufficiently unique to require a somewhat different array of team members. Teams

became more diverse in disciplinary composition and the research teams were newly structured for each problem.

In the early 1960s, both Dr. Charles Reimer (a field team member) and I were offered faculty positions by a midwestern university of intermediate national ranking. We were then both full curators (comparable to full professors) with tenure at ANSP. We were both offered assistant professorships without tenure by a full professor with tenure whose publication record was markedly inferior to each of ours. The rationale was twofold: (1) service in a university was so different from that in a research organization that we would be starting over and (2) our presumed rapid promotion would enable larger salary increases later than would be possible for an immediate appointment as a full professor. We both immediately declined! Soon after, I was offered a superb position as a department chair by a university enamored of interdisciplinary activities. This offer came because of my administrative experience with interdisciplinary teams. The salary was nearly half again as much as I was making, and the fringe benefits were even more impressive. I enjoyed administration, and, although it may be immodest to say so, I did it well. However, I could not envision giving up research entirely. Had my involvement with team research become a trap with the only exit labeled "administration" or "starting over" as if I had just obtained the PhD? Fortunately, it had not, but only my many hours of individual research had saved me from the stereotype of a team person.

When I went to the University of Kansas in 1966, I began working with H. W. Shirer, who had both a MD and a PhD in electrical engineering—just the person I needed to develop computer-interfaced biological early warning systems. I also worked extensively with Roger Kaesler, a paleontologist in the Geology Department, who studied community structure in ways that were of great interest to me. The three of us, together with our graduate students, formed a team whose scope was broader than the teams I was accustomed to working with, but whose total number was smaller. However, when I came to Virginia Polytechnic Institute and State University at the invitation of Robert A. Paterson, then head of the Department of Biology, my specific assignment was to form an aquatic ecology group. Since I was hired in a research position, I had time to devote to first identifying research problems and then acquiring the funding to investigate them. As might be expected for that era, most members of the aquatic ecology group were in their first professional position after acquiring the PhD. Extramural grants provided research assistantships for graduate students and money for technicians, equipment, travel, and the like. The system was quite successful for about a decade, with over 60 graduate students and 7 faculty members at its peak, although one faculty member flatly refused to work on teams. The aquatic ecology group was understandably modeled after the ANSP group, but redesigned to fit a university situation with graduate students. The aquatic ecology group was a team only in the sense that the group was able to acquire funds not readily available to young PhDs. Problems I was called upon to consider required a more diverse group of disciplines than I had been part of earlier, and the mix of disciplines was different for each particular problem. In order to compete for this extramural funding, I quickly started adding members of other disciplines such as engineers, statisticians, geologists, and even psychologists and historians. This diversity necessitated approaching not only other departments and colleges but also sometimes other institutions.

A comprehensive university with substantial extramural funding cannot escape some administrative problems, such as distribution of overhead monies generated by team research. Therefore, after two years at Virginia Polytechnic Institute and State University, Leslie Malpass (Academic Vice-president) approached me about forming an interdisciplinary center for environmental studies. As director, I would have the authority to negotiate directly with faculty members and their department heads and could escape the time consuming chain-of-command procedures. The tremendous advantage of this system was that I could gather the mix of specialists needed to solve a particular problem. One disadvantage was that many of the individual faculty members had probably never worked on a multidimensional team before and were unaccustomed to exchanging information with other disciplines. Most had never faced the problem of synthesizing information from a variety of disciplines. Another disadvantage was the availability of faculty

members to their home departments. If the individual were participating on a team and not fully involved in the department, this divided loyalty was often viewed with great suspicion by some department heads and colleagues. Funding for research and graduate students and equipment for research helped dispel or dampen some fears. Also, team members had access to a huge data pool, which would not have been available to a lone investigator. Increased data and information strengthened many publications. Nevertheless, time management problems still existed since many team members were usually carrying a full department load in addition to the team activities. Also, some teams grew rather large and unwieldy, the largest being 51 faculty members from 14 departments in 3 colleges, plus associated graduate and undergraduate students. Even the small teams required tracking for accountability because some sponsors required monthly reports and all required quarterly reports. This aspect of accountability would not have been possible had I not had Kenneth L. Dickson as my assistant director; he excelled in this type of activity.

Multidimensional individuals are now carrying out many of the problem solving activities that once required a small team; regrettably, many universities have gotten on the interdisciplinary bandwagon when the parade was nearly over. Transdisciplinarity is now increasingly common. Both top-down and bottom-up strategies are essential to resolve global and other large systems issues (Cairns 2003). My primary point is that universities and other institutions should consider interdisciplinary activities as an essential step toward transdisciplinarity rather than as an endpoint. I started with a modest mixture of disciplines because interdisciplinary activities were then quite rare; then I moved beyond that level. Even that level of complexity was a major challenge to a novice. At present, over a half century later, I am still seeking transdisciplinarity. Even my former students will probably spend their professional careers on this quest.

In the late 1980s and early 1990s, state-supported universities came under fire because faculty members were perceived to be ignoring teaching responsibilities to ensure research success. Since most of the money I acquired in extramural funding supported student research, I have always felt that my research was part of teaching. The research experience was, in fact, an invaluable experience for the transition of many students to both academic and non-academic positions. However, this evaluation of the co-existence of research and teaching is not a general perception among legislators and the public. This misperception is quite understandable since universities did not make as great an effort to demonstrate the benefits of this co-existence as, in retrospect, they should have. My own belief is that the interdisciplinary team, which is becoming increasingly unwieldy and difficult to operate in a university, should be replaced, to some degree, by consulting firms and research organizations or by multidimensional people with some experience in more than one of what used to be classic, isolated disciplines. The Worldwatch Organization, founded by Lester Brown, is a good example of individuals carrying out a synthesis on such problems as transportation, feeding China, and other issues. The word *interdisciplinary* implies that the classical disciplines will still dominate, despite the fact that they often are hampered by jargon only understood by a relatively few people. Some disciplines still rely on artificial constraints on the boundaries established, which are the construct of the discipline for quality control and are not necessarily easy to surmount for solving the complex, multivariate problems that the world faces today. Therefore, multidimensional people are needed to replace the old construct of interdisciplinary teams, and then some of the problems associated with an aggregation of individuals not accustomed to working together will be resolved. However, both faculty and students need to get their training somewhere. Naturally, part of that will be outside of educational institutions, but, ideally, the most important learning experience will be in them.

Experiencing An Institutional Paradigm Shift

Institutions also experience paradigm shifts. Before Patrick formed the limnological survey team, the ANSP research staff was entirely world-class systematists. They were specialists who published almost entirely within their specialty. This research continued uninterrupted, and both groups benefited from the presence of the other. Predictably, frictions arose between the two

groups, but the institutional paradigm shift of adding a new approach while maintaining the well established research activities succeeded.

Thomas Dolan, an original Conestoga/Brandywine survey crew member, has been most helpful in checking my recollection of the early team period. Others, now deceased, were John M. Ward (chemist, was part-time) and Charles B. Wurtz (malacologist). Donald Reihard, Jr. was a bacteriologist and was half time. John H. Wallace (algologist) continued to be associated with the group but was pursuing a graduate degree at the University of Pennsylvania full time. Stuart S. Bamforth (laboratory assistant), Hazel D. Barner (laboratory assistant), Edward Haldeman (laboratory assistant), and James F. Bergseng (field assistant) were also employed until December 31, 1948, although all but Jim Bergseng were part-time. The two crews carrying out the Conestoga/Brandywine river surveys were headquartered at Franklin and Marshall College in Lancaster, Pennsylvania. Neither Tom Dolan nor I recall having any substantive interaction with personnel at ANSP during that summer, nor does Herbert Levi. We, of course, knew what ANSP was like, to some degree, but it was not until we went to work there the following fall that we realized how different we were from the other curatorial staff. We worked as a team despite our individual specialties; other ANSP curators worked primarily alone. We were involved with pollution problems; other staff members sought areas where there were none. They were supported by long-term endowments and the like; we were on "soft money." In terms of our activities on the river survey group, we were all taxonomists, although our use of the information was quite different from the other curatorial staff at ANSP who named new species and constructed new taxonomic hierarchies. Our information was used in ways that affected industrial and societal decisions or, at least, was intended to.

Many of the curatorial staff at ANSP historically had other professions (e.g., Joseph Leidy), and many who were employed when I first went to work there were in endowed chairs or independently wealthy. The source of funding for the river surveys during the summer of 1948 was the Sanitary Water Board of the Commonwealth of Pennsylvania, definitely a unique source of funds in ANSP's history. The dichotomy in both professional activities and sources of funding became even greater when the grant from the Commonwealth of Pennsylvania ended and we began carrying out investigations for industrial firms, such as E. I. du Pont de Nemours & Company and the Commonwealth of Virginia. Had the extramural funding for the Limnology Department ceased, we would have been without salaries because ANSP did not have reserves for this purpose. My recollections of that period do not include the deep concern about funding that would have been quite reasonable because this was a new undertaking to which even industry was unaccustomed. At that time, the idea that biologists had a role in pollution investigations, although not new in the professional literature, was quite rare for industry where pollution studies were the realm of chemists and sanitary engineers (now environmental engineers). In one period, directly after the Commonwealth of Pennsylvania's funding ended, salaries were reduced from $3,600 per year to $3,000 per year—something I remember vividly because my wife and I had just purchased our first house. Fortunately, my mother, who died rather young, had left me enough money for a substantial down payment, and the blow was not as severe as it otherwise would have been. Additionally, extramural funding increased over the next year; our original salaries were reinstated and then subsequently increased. Clearly, financial security was not the primary concern of team members.

However, the uncertainty about the source of funding, the need to prepare quarterly and sometimes even monthly reports, and the need to send out competitive grant proposals certainly caused a great dichotomy in outlook between the well established curatorial staff and us. The other curatorial staff had a much greater level of security than we did, were much more isolated from competition for money, and, thus, could unhesitatingly embark on long-range studies while we could not. However, the enormous benefit to this situation was that, while I was still completing my graduate degrees, I was exposed to the intricacies of obtaining extramural funding, which has benefited me for my entire career.

The Guadalupe River survey was the Limnology Department's first industrial survey, and it was a thrill to the entire team! Tom Dolan remembers it vividly, despite the half century that has

elapsed since then. The river survey team was applying Patrick's then well validated methodology in a very common situation during the rapid industrial development that followed World War II. The Guadalupe River flows into the Gulf of Mexico, and the team was to survey the river before completion of a new du Pont synthetic fibers plant to be located near Victoria, Texas. The team was examining a river ecologically quite different from the ones we had studied, and we were a substantial distance from the large library in Philadelphia. Field work was simpler in those days, and the survey equipment for the entire team fit in one of the then-common station wagons of that era (when I left ANSP in 1966, we were carrying tons of equipment in large trucks). Such things as boats and even outboard motors were rented, a pattern that continued for some years.

The team stayed in the Hotel Victoria in 1949, which had a lobby and some adjacent rooms that resembled a museum. We worked long hours, but fortunately in Texas, especially when construction crews were in the area, our muddy appearance did not cause any sensation when we walked through the lobby at the height of the cocktail hour. Our post-field collection investigations were carried out in a metal Quonset hut, which boomed every time the metal expanded or contracted in response to the heat of the sun. Since the Guadalupe River flowed into the Gulf of Mexico not too distant from the proposed plant site, we used a shrimp boat crew for baseline shrimp and oyster samplings. It was the beginning of my many encounters with other cultures on a working basis, which made life very interesting. I learned that local people were willing to help if I took the time to get acquainted with them. For example, our food allowance went much further because the crew of the boat boiled freshly caught shrimp on a Primus stove in a red powder called "shrimp boil," which to this day I have never found an equal for flavor, even in the most expensive restaurants.

This survey also launched a lifetime for me of explaining biodiversity, aquatic community structure, and other esoteric matters to industrial executives, engineers, regulatory personnel, and local citizens. Had I not been on a team, it is unlikely that these experiences would have been open to me.

I also had my first experience of completing toxicity tests on the simulated industrial waste streams in the ANSP laboratories in Philadelphia. I also observed and worked on the river where the effluents would be discharged. The tests were the crude, single species, short-term laboratory tests in containers low in environmental realism, but they were state-of-the-art for that period. Most important, I was forced to make the laboratory-field connection so uncommon for toxicologists and ecologists, which became a major focus of my entire career.

Quite different teams replaced the ones I first experienced. I speculate that multidimensional people will replace teams in many areas in the future. The teams on which I started my career were groups of people gathering information, at a fixed time and a fixed location, and meant to be assembled for a specific purpose. We were primarily focused on point source discharge pollution coming from an industrial or municipal pipe. We primarily counted "critters," with chemical/physical information as a background. Although we occasionally looked at entire drainage basins, only a segment of a drainage basin was usually surveyed, with five or six points of reference, including a control. This operation was expensive (despite our relatively low salaries) because of housing, per diem, and transportation in the area of the survey.

Multidimensional people, instead of interdisciplinary teams, will enjoy a number of advantages: (1) communication problems will be reduced, (2) as much time as necessary can be spent at a particular locus rather than the arbitrary time generally characteristic of teams, (3) the direction can be quickly changed by new information, and (4) exhaustive detail, when necessary, can be obtained from a specialist. The synthesis in the report is more likely to be understood by non-specialists since the person writing the report is a generalist who is knowledgeable, to a certain degree, about all the areas and, most important, cognizant of how the information will be used. While the teams on which I served were aware of how the information would be used, they did not commonly meet with the persons who would use the information. A spokesperson, generally Patrick or sometimes me, would convey the information, which is essentially what a multidimensional person would do if substituting for a team.

In this scenario, the camaraderie of a team would be gone, although a different type of camaraderie may develop between the decision makers and the persons furnishing the detailed information. While the detailed information gathered by the specialist would not be shared in the same way, a different kind of professional growth, likely more enduring, would result, since the multidimensional person could easily incorporate the information selected for the diagnosis than when it was predetermined by the specialist.

These dynamic, continuous changes have kept my interest intense for over a half century.

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CHAPTER 6

WORKING FOR A WOMAN SCIENTIST IN THE LATE 1940s

Since Ruth Patrick has been my mentor from 1948 to the present, I had first-hand exposure to what women scientists endured during the late 1940s. When Ruth gave a series of seminars at Virginia Polytechnic Institute and State University in the early 1990s, I noticed that the graduate students were very interested in her contrasting the experiences in her career with events they were experiencing. Karen Holl, a graduate student at that time, described the interactions as one of the highlights of her time in graduate school and commented on how much she appreciated Ruth's candid discussion of experiences she had as a female scientist in the middle of the 20th century.

The undergraduate students in American colleges and universities are approximately 60% women at present. This figure does not astonish the media, legislators, and young faculty members because they have forgotten or are unaware of the miniscule number of women in science after World War II. I am not surprised, but then I had the advantage of working for a woman scientist, which convinced me that the stereotypes of the mid-century were wrong. I was extremely proud that, when the Association for Women in Science celebrated its 25th anniversary in 1996, I was selected to be one of the 25 fellows of that organization. How gratifying to have recognition from a group that provided evidence that scientific ability is not gender related. Working for and with Ruth had a profound effect upon both my career and attitude.

Working for a woman scientist in an era when women scientists were, regrettably, a rarity was an interesting experience. On the plus side, I learned not to be concerned if I had a minority opinion or because my research was different from that of my colleagues, because a woman working on pollution was then definitely in a minority. When I accompanied Ruth to industrial meetings, we often met with chief executive officers. These industry heads were always male and were very interested in seeing and talking with a woman who worked near sewage and industrial waste discharge pipes. During these occasions, I saw their curiosity change to respect and then admiration when these men realized that Ruth was a first-rate scientist with all the necessary management skills to supervise a team of scientists and to analyze a series of complex issues. I also learned that one could overcome formidable obstacles if one had enough energy, commitment, and, above all, creativity.

An important skill I acquired while working with Ruth was how to obtain extramural funding, because science without money is extremely difficult (arguably impossible). In the Limnology Department at the Academy of Natural Sciences (ANSP), salaries were then entirely dependent on grants and contracts. If the cash flow stopped, so did salaries. It is a tribute to Ruth's fundraising abilities that salaries were only reduced once (from $3,600 to $3,000 per year), and they never were reduced again while I was there.

Had it not been for comments I heard outside the survey team, I would never have thought anything was unusual about working for a woman scientist. I once worked briefly as a sub-professional for the US Department of Agriculture research laboratory, and my immediate supervisor (Dr. Willits was the senior supervisor) there was a woman. I worked the midnight to 8:00 a.m. shift on extractions that had to be done around the clock. This position was my first and only semi-professional employment, and I felt honored to be trusted to work alone for eight hours each day without supervision. Since I was a sub-professional 6 (the lowest category in existence in those days), no one questioned me much about my position, so my reporting there to a woman scientist caused no comment.

I viewed Ruth as a scientist from whom I could learn much and who was launching a very exciting research project. She never asked anyone to work longer hours than she did herself. In addition to supervising two field teams during summer 1948, she also collected and identified diatoms. She worked long hours at what could be very tiring activities—this type of research involved rather arduous field work on occasions and was combined with attention to minute detail once one returned to the laboratory.

No one on either team, to my knowledge, thought about gender differences affecting professional activities. Some situations did develop for which no prior experience had prepared me. For example, when the team was using boats, Ruth did the same work as the other crew members and carried moderately heavy objects from the station wagon, van, or truck to the boats. However, in the evening when we were going to dinner (especially in the South in the late 1940s and early 1950s), one would be considered as lacking manners for not opening doors for a woman. Should I also show such cultural courtesies when we were colleagues in the field and sharing work equally? By trial and error, I finally decided that, when we were doing professional work, we were colleagues; but, on social and semi-social occasions, the civility thought appropriate in the culture of that time was mandatory. For those who view this as an example of latent male chauvinism, I should note that, while Mary Gojdics and I were each a protozoologist for one of the two field teams, she was so far senior to me academically that I would have opened doors and carried materials for her even if she were a man, because these courtesies would be my way of showing respect for a senior scientist. Why not do so as well for a woman who was both professionally and administratively senior? Gender had nothing to do with the allocation of respect! Showing respect and civility are not signs of servility! Respect is an acknowledgment of accomplishments one admires. Civility is merely an acknowledgment of the worth and dignity of another individual. The decline of both respect and civility in modern society is saddening to me and a cause for concern.

Ruth rarely talked about her childhood—she was too busy living in the present. Several times she told me that her father, an attorney in Topeka, Kansas, held her on his knee so she could use his microscope. This activity was probably the origin of her life-long passion for diatoms. Clearly, her father believed that females, even as little girls, could take an interest in science.

After many years of a working relationship, I was able to call Ruth by her first name. However, I never did so for my major professor David Wenrich because we were not colleagues after I obtained my PhD as I was with Ruth. In those days, respect for senior scientists was always demonstrated by using a professional title. This regime did not diminish either affection or cordiality, since respect is in another category.

Compensation (especially fringe benefits) was much less at ANSP than obtainable elsewhere in academe at that time, but I do not regret for a moment the years I spent there. We thought nothing of unpaid overtime work, although Ruth tells me that overtime is now paid in the department. Since I worked for a brief time when I was younger as an oiler in a paper mill and was paid not only overtime but also time and a half and sometimes double time, I learned the major lesson that professionals worked well beyond normal limits, sometimes double the expected work hours, without additional compensation. The crucial focus was obtaining adequate scientific evidence, not time spent. Perhaps this mind-set is the best description of the atmosphere at the time that Ruth first employed me and for which she was the role model. We considered ourselves privileged to be working on exciting research projects, and putting compensation before quality would have diminished the experience. Of course, one must care for family and personal needs of food and shelter. If one's occupation provides satisfaction and enjoyment, why spend time fighting for more money? I hasten to add that my job at the paper mill was not in this category, but many other activities (many not in science or research) I think would be equally rewarding for me. My first professional position gave me the opportunity to develop a set of guiding beliefs that has been modified, but not dramatically changed, during my entire career.

My recollection of field work with Ruth is still vivid. When she and others on the team were tired, the coffee was cold, and some of the sandwiches were in water in the bottom of the boat, she still wanted to collect "just one more sample." That night, everyone worked on samples. This attention to detail, to me, is the essence of scientific investigation—have confidence that the data base is adequate and relevant.

In a period when women were not regarded as the primary role models in science, my primary role model was a woman. Years later, when Ruth has been deluged with honors and awards, the choice of her for all these honors seemed obvious. I am grateful that I had a great opportunity to observe the process by which she earned such honors for over half a century.

CHAPTER 7

GOING FROM REACTIVE TO PREDICTIVE POLLUTION ASSESSMENT

Working with perishable organisms has some advantages. When I returned from a field survey (Cairns 1948, 1949) for the Academy of Natural Sciences Philadelphia, the work was essentially finished. The species lists were complete, and alterations could not be made because the material was perishable. I could identify a few additional organisms from sketches I had made or, in later years, from photographs, but this activity was a relatively minor time commitment compared with the large numbers of specimens still to be identified by the other field team members. After a few years (but before acquiring the PhD), I was given some administrative responsibilities as field crew chief (Ruth Patrick designed the surveys) and for organizing the final report, meeting deadlines, and such activities.

Nevertheless, I had "free" time academically. I had minored in chemistry and physics as an undergraduate at Swarthmore College and had taken a number of graduate physiology courses. As a consequence, when Patrick was asked by various corporate executives to do toxicity tests as well as river surveys, she felt I was a prime candidate for this assignment. Money was not available at that time to hire a full-time environmental toxicologist, even had one been available. I was even more fortunate in that W. B. Hart, with his colleagues P. Doudoroff and J. Greenbank, had produced a toxicity testing manual in 1945 that used blue gill sunfish as the test species. This manual was, arguably, the first standardized methodology in this field. More important to my career was the fact that W. B. Hart regularly advised Patrick on promising areas of research.

As I reflect on the Hart, Doudoroff, and Greenbank (1945) publication from the vantage point of many years later, I can hardly grasp that this toxicity testing methodology was developed and published when it was. World War II was over in 1945, but the troops were still coming home. Of course, providing housing for returning soldiers and meeting the pent-up demand for automobiles and other items that had been scarce during the war were occupying everyone's time. More astonishing is that the research for the toxicity testing manual (as I recall, about 300 pages) was carried out while the war was in progress. Hart had the vision for such methodology and was sufficiently persuasive with the Atlantic Refining Company to gain support for the research. Had the research not been published when it was and had I not inherited the equipment used in the research, doubtless my career would have been greatly different. Belatedly, I am honoring Hart for his vision and Doudoroff and Greenbank for their essential contributions to the publication. My copy of the report was almost worn out when it was lost during my move to the University of Kansas. Had I realized how precious it would eventually become as a historic document, I would have made every effort to preserve it.

Moreover, Hart was available to show me how toxicity tests were carried out, and I received all the equipment that had been used at the Atlantic Refining Research Laboratory to carry out this pioneering toxicological study. Toxicity research, with only single species to start, was beneficial because it focused my attention on developing a predictive capability (the river surveys were reactive) for pollution control and on an important source of stress on riverine communities. Another benefit, of course, was to determine that an industrial waste or compound was toxic before it entered natural systems (predictive) rather than after harm had been done (reactive). The research also enabled toxicologists to determine at what levels no-observable deleterious effects occurred. Testing also provided a rare link between laboratory and field studies. All these features and benefits were useful to my career and also were an unexpected source of much aggravation (e.g., spills of toxic substances into natural systems often occurred on weekends and holidays).

I believed that a strong conceptual link existed between toxicity testing of industrial waste effluents before they entered an aquatic ecosystem and the pollution effects that I was observing in the system itself. Professionals who were most interested in the concepts that interested me (e.g., pollution effects in both laboratory and field) were predominantly engineers (then called sanitary engineers; now called environmental engineers) and water chemists. What I regarded as an

interrelated, unified spectrum of information was fragmented into laboratory and field studies in those days. They were rarely connected and, worse yet, were studied by professionals far removed from my discipline of biology. Thus, I was faced with three aggravating situations: (1) my interests drew me away from the discipline of biology into associations with engineers and chemists because most biologists were not interested in pollution effects at that time, (2) even for biologists interested in pollution effects, a dichotomy existed between laboratory and field investigators, which still exists to some degree today, and (3) speaking out about the pollution effects upon both society and natural systems had severe penalties, as fictionalized in Ibsen's 1882 play "The Enemy of the People" (Fjelde 1965) and in real life by the saga of Carson's going public with information on the consequences of pollution trends that had mostly been restricted previously to professional journals.

This lesson (speaking out) was quite astonishing to a young investigator in environmental pollution. Being a contrarian could damage one's personal life and end a career in a nightmare. Even if one were correct, one could die before being vindicated! Although I never had the honor of meeting Carson, I may have a better understanding near the end of my own career of how she felt. Worse than the personal abuse and criticism is the feeling that perhaps I remained comparatively silent about issues that will affect both present and future generations. The academic community will always have "dissenters for hire," even to oppose ideas accepted by mainstream science. The general public often has difficulty distinguishing between genuine dissent (which strengthens science) and hired dissent (voiced by recipients of large consulting fees). However, professionals can make the distinction, and time will correct misrepresentations. Despair is in order only if young investigators begin to admire and emulate the hired dissenters.

In the 1950s, concern arose over heated wastewater discharges from steam electric power plants. The nuclear power age had not yet begun, but substantial amounts of water were used to cool the partially spent steam generated by fossil fuels from turbines that generated electric power. The question surfaced about what this thermal addition (some industrial people referred to it as thermal enrichment) would do to natural systems. As an avid "catch and release" flycaster, I could not resist the opportunity to research this question since fish are sensitive to temperature changes.

Both toxicity testing and field work with thermal discharges (Cairns 1966a) had a dramatic effect on my career because both were of interest to engineers. I received an invitation from Don Bloodgood, who was then heading the annual, prestigious Purdue University Industrial Waste Conferences, to present a plenary session address at the Tenth Industrial Waste Conference. The address was subsequently printed in much more detail (Cairns 1956a,b). This invitation was a blessing because I become acquainted with the engineers who were interested in these problems and I gained a publication outlet of many years. At that time, the classical biology journals were not interested in either toxicity testing or thermal discharge research. When I submitted articles, I was generally informed that they did not fit the interest of the readers of the biology journal. If the manuscript reached the review stage of the journal, the reviewers were generally not competent in either of these areas. As a consequence, I attended the Purdue Industrial Waste Conference every year for over a decade because my research was accepted there. The toxicity testing research was also, ironically, published in *Notulae Naturae* (Nature Notes) of the Academy of Natural Sciences of Philadelphia where I was then employed. My papers were enormously different from the almost entirely systematic or taxonomic articles that appeared in *Notulae Naturae*. However, I was an assistant curator of limnology and a staff member and, therefore, was entitled to publish in the journal. Since most engineers and others interested in industrial wastes did not read *Notulae Naturae*, I purchased large numbers of reprints and mailed them to colleagues with similar interests. This distribution was moderately effective, given the small numbers of professionals carrying out water pollution research in those days. Although the conclusions were a shock to those enamored of simplicity, the results were no surprise to anyone who had actually done some research in this area. Both regulators and industry favored tests with absolute values determined by widely accepted standardized methods that were understood and accepted by all. The legal system wanted (and still does want) such research to be simple and easily understood.

As a child, I was taught to revere the judicial system and the rule of law. As an adult, I became fascinated with the process of science. I was shocked when I began to realize how alien the thought processes of each system were to each other. Scientists, accustomed to probabilistic determinations based on evidence that can usually be validated in some way were in contrast to the justice system, where uncertainty was often used as a shield to protect the guilty. Innocent until proven guilty beyond a reasonable doubt is certainly appropriate for individuals! However, this concept should not apply to persistent, potentially toxic chemicals that may adversely affect ecosystems. As Homer remarked in the *Iliad*, "Once harm has been done, even a fool can understand it!"

The inadequacy of the judicial system to protect transitional zones, such as wetlands, has been particularly apparent. The extraordinary ecological value of wetlands as part of the hydrologic cycle has been recognized by mainstream science for decades. The judicial system still views wetlands primarily as isolated fragments of property. This discrepancy is why, in the last part of my career, I became enamored of ethics as superior to the formal law in coping with environmental problems. The impeachment trial of American President Clinton showed clearly how disenchanted the American public was with hair-splitting lawyers. Is it time for the public to take control in order to protect the planet's life support system?

Despite the misgivings just expressed, some means of determining the reliability of scientific evidence is needed. Since ecological situations differ, often dramatically, from one location to another, an effective way of establishing confidence limits for professionals and organizations is needed. An important first step in this process is to have an unknown sample analyzed by specific standard methods by an individual or organization. Steps must be taken to ensure that the sample is not "farmed out." So, despite my misgivings about prescriptive approaches to environmental problem solving, "standard methods" did appear to have some utility. As so often happens, things did not work out as I expected.

The Hart, Doudoroff, and Greenbank (1945) methodology was converted to a standard test method for the American Society for Testing and Materials (ASTM) by Roy F. Weston, Howard Baker, and me. A standardized test has the purpose of dictating precise details for any testing procedure. Such prescriptive methods always reduce environmental realism dramatically. However, these methods also increase the credibility of the results in courts and hearings. Not only are test fish confined in a small container, with no mud, plants, or other organisms, but the environmental conditions, such as dissolved oxygen concentration and temperature, are maintained at constant values. Standard methods are in stark contrast to natural systems where variations are often dramatic. As a consequence, I began to wonder what would happen to toxicity testing results using fish if numerous characteristics, such as dissolved oxygen concentration, temperature, water hardness, and bluegill sunfish body size, deviated from those in the standard methods. Also, since the standard test was 96 hours in length, I considered dramatically increasing the time period. What then would be the effects on the response to toxicants? Illustrative examples of such changes and resulting effects were published in the 1950s and 1960s (Cairns 1957a,b, Cairns and Scheier 1957, 1958a,b, 1959, 1962, 1963).

During the period just mentioned, I also considered the response of other organisms to potential toxicants, such as to detergents, which were then a cause for great concern for a variety of reasons. Research (e.g., Cairns et al. 1963) showed clearly, and not unexpectedly, that different trophic levels had different responses to toxicants and that fish were not always the most sensitive species. However, at that time, I was still testing each species in isolation from others. The quest for replicability overrode my concern for lack of environmental realism initially.

In the first phases of this research, death was used as an end point for fish and invertebrates, and reduction in rate of reproduction was used for diatoms and other microorganisms. Obvious questions concerned what physiological changes might occur prior to death (Cairns and Scheier 1964a) and how exposures of different time lengths would affect the correspondence of short- and long-term exposure (Cairns and Scheier 1964b). Evidence had already been gathered that detergents had effects on both tissues and behavior, as well as other attributes,

at concentrations well below those that were considered safe (Foster et al. 1966, Scheier and Cairns 1966). Despite my pride in the fact that biologists now had a role in water pollution assessment, I had serious misgivings about the reliability of the predictions based on short-term laboratory tests low in environmental realism, especially standardized tests (Cairns 1967a,b,c).

Following these new perceptions, inevitable steps forward were to focus on water quality standards and the management of aquatic ecosystems. Both were major steps in enlarging my perspective in terms of information and in terms of the scale of the problem as well (Cairns 1967c,d). Episodic events, such as heavy suspended solid loads caused by floods or even from industrial discharges, were also an interesting consideration (Cairns 1965a,b, 1968). Instead of assessing long-term effects, research in this area focused on how much a species could endure during intense but short-term exposures.

In the mid-1960s, the government and some professional societies became concerned about water pollution and the survival requirements of organisms inhabiting natural systems. Also, concerns were expressed about the question of communication between industry and regulators, between scientists and practitioners, and between scientists and the general public. All these areas became a matter of concern to me, and I ventured into what one might call the underlying rationale for water pollution assessment, as well as the problems of communicating among disciplines and professions (Cairns 1965c,d,e,f). Venturing into the public arena with articles in the *Scientist and Citizen* (Cairns 1967b), the regulatory arena (Cairns 1965c), and the interaction of disciplines, such as biology and engineering (Cairns 1966b), was a major step for me. I was amazed at how well the engineers received a biologist and how tolerant they were of my initial lack of understanding of their field. Possibly, my just making the attempt at that time was enough. I read their articles in various journals and their books, attended their meetings, and listened to engineering presentations. At the same time, I became aware that the pressures on regulatory professionals were quite different than the pressures on the industrial personnel and academicians. Regulatory people could, and frequently did, end up in court defending their views. Many of them were not inclined to take risks that might threaten their jobs and income. This stance was quite understandable since the same situation is true of many in the academic community.

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CHAPTER 8

THE FIELD STATION ERA

In 1960, as I approached 40, I became concerned about: (1) my total dependence on extramural funding; obtaining funds often took time from synthesis, and the funding did not always correspond with my target research areas, (2) whether considerable field work would be as appealing in my middle age as in my youth, (3) my increasing responsibility to educate four children when my livelihood depended on extramural funding, (4) whether I could survive in academe with arduous university demands, (5) the timing of a transition to academe, and (6) whether some transitional experience was possible. A transitional opportunity came at that time from my advisor at Swarthmore College, Robert K. Enders (who was then also director of Rocky Mountain Biological Laboratory [RMBL]), who invited me to teach a course in comparative limnology at RMBL during summer of 1961. I had taught a course for high school teachers at Temple University on Saturdays for an entire academic year, but this summer opportunity would be my first teaching experience at a field station. Furthermore, RMBL had a mixture of undergraduate and graduate students from a wide variety of institutions. The field station did not pay salaries but offered board for two, plus a free cabin. I recall receiving $100 for roundtrip travel expenses for the approximately 1700 miles from Philadelphia to Gothic, Colorado. The summer months were the peak field season at the Academy of Natural Sciences Philadelphia (ANSP), and I would be unavailable to act as field crew orchestrator. On the other hand, I had never had a full two-week vacation with the entire family because summers, when the children were free from school, were the height of the field season. I saw the invitation to teach as a test of whether I could perform adequately in a university when I became older and the work on a field team became less appealing. Considerable resistance was voiced to the possibility of my being absent during summer at the ANSP, but, as I recall, relatively little of the resistance was from Patrick, who would probably suffer the most from my six-week absence.

The time at the field station reinvigorated my interest in research, and, as usual, students often asked penetrating questions that people who do the same thing for years had somehow forgotten to ask. Interacting with the students, especially since they were from a wide variety of institutions in both North America and abroad, was extraordinarily energizing and increased, rather than decreased, my research productivity. I partially offset the loss of my services to ANSP during the first summer by doing a preliminary site visit on the Green River near Moab, Utah.

During the early 1960s, faculty usually taught only three summers at RMBL since the facilities were small and the course offerings had to be rotated to provide opportunities for repeat enrollment. During summer 1963, Professor Alfred H. Stockard (then Director of the University of Michigan Biological Station [UMBS] headquartered at Douglas Lake, Michigan, near the straits of Mackinaw between Lake Michigan and Lake Huron) contacted Director Enders to determine whether RMBL would be upset if UMBS offered me a position with its summer faculty to teach protozoan ecology. Enders told Stockard about the three-year, unwritten rule and apparently gave high recommendations for my field capabilities. UMBS had previously offered a course in protozoan parasitology and wanted to refocus on field ecology of the protozoans of local lakes and streams, particularly their community structure, an interest that continues at the Station to this day with Rex Lowe's algae course.

UMBS is located on Douglas Lake and has a wide variety of lakes, swamps, bogs, fens, wetlands, and streams within a short drive. At RMBL, I was required to transport my equipment from Virginia to Colorado for teaching each course; at UMBS, all equipment was supplied. In addition, exceptionally good water chemistry laboratory facilities (at least for a field station) literally abutted the lake, so that I could get samples from lake to laboratory in less than five minutes. My non-teaching days, including weekends, were devoted to research. Furthermore, I was provided with a teaching assistant (William Yongue, Jr.) who was also interested in protozoans.

Having two of us in the laboratory offered the opportunity for replication—an extremely difficult task for one individual working with perishable/changeable material.

For my entire tenure at ANSP, I had worked almost entirely on streams and rivers and only rarely studied reservoirs. Lakes, bogs, fens, and swamps, the common habitats at UMBS, were new to me, and I spent the first few summers (in what ultimately became a 19-year relationship) familiarizing myself with the protozoan species in these new habitats. This research was the beginning of my study of colonization dynamics, which was to take much of my attention in summers, even when my relationship with UMBS ended in 1983.

In the mid-1960s, UMBS bought new microscopes, some of which were allotted to my class. I found that the microscopes were packed in polyurethane foam (essentially limber, sponge-like material). My research with protozoans in streams and rivers previously focused entirely on species associated with natural substrates. I continued this study design at UMBS and added artificial, uncolonized substrates. In fact, many species inhabiting both types of substrates were old friends living in quiet sections of rivers and streams and on the substrates in lakes, ponds, etc. I had the good fortune to hear Robert MacArthur discuss the equilibrium model that he and E. O. Wilson had developed. I speculated that, since substrate-associated protozoans were usually quite different from the truly planktonic species, a substrate anchored in the lake would be an ecological island. As a consequence, I cut the polyurethane packing from the microscope shipment into small cubes, tied them between two floats, and anchored the floats to the bottom of the lake by cement blocks—much like an aquatic clothesline. The colonization dynamics of the lake matched those hypothesized by MacArthur and Wilson quite well (Cairns et al. 1969).

Five summers at RMBL and UMBS were enough to convince me I could handle the rigors of university life. I had lecture notes for two courses (limnology and protozoology) and nearly enough for two more (stressed ecosystems and aquatic toxicology). As it turned out, all this preparation was entirely unnecessary except to build my confidence. I had badly overestimated the performance level needed to survive in academe. However, not until I joined William Argersinger's Graduate School Committee at the University of Kansas was I exposed to faculty curricula vitae in large numbers and was then able to judge my own academic performance in the proper perspective. I could have slowed down my research without endangering my position, but to what end?

Although I had originally regarded my field station experiences as aids in a transitional career period, they rapidly became a passion. I spent every summer from 1961–1994 at a field station. When I ended my summers at field stations in 1995 for medical reasons, Jean and I both felt a sense of loss. Nothing will ever replace the field station experience, and I am thankful to have enjoyed it for so many years!

One reviewer questioned why I left UMBS after summer 1983 and never returned. This information was omitted because that experience was painful, but the reviewer deserves a reply. At the conclusion of the 1983 summer session, Director David Gates informed me that my invitation to teach at UMBS would not be renewed in 1984. Some University of Michigan faculty thought my course “Stressed Ecosystems” was not suitable. I neither asked for a formal letter justifying this decision nor did I appeal. I knew that I was no longer wanted. The students in my class immediately began a “Save Stressed Ecosystems” effort and were joined by many former students. Although I was both touched and honored by their concern, I simply did not wish to stay where I was no longer welcome. David Gates generously informed me that extramural funding for my salary was available if I wished to continue teaching. When I thanked him but refused, he stated that Jean and I could continue to occupy the cabin we had been using if I would return as a research investigator. Again, I refused—neither Jean nor I wished to remain where my academic efforts were not welcome. We decided to return to RMBL where we had spent many delightful summers. I would be a research investigator and give up teaching at field stations entirely. However, soon after my arrival at RMBL, Enders asked me why I was no longer teaching and I gave him my reasons. He explained why I should continue teaching, and the next summer I resumed field station teaching and continued to do so until January 1995 when some blood clots in my legs changed my mind. It was

simply not fair to the students for me to teach when the blood clots might disrupt the course study. The picture of Jean and me standing on a Colorado mountain top in 1994 gives some evidence of our joy in what was to be our last field station summer. After my return to RMBL, I began to teach restoration ecology, and this location was a superb area for teaching such a course. Despite the conditions under which I left UMBS, my attachment to it and the many people I spent summers with remains strong.

Since I have always felt transdisciplinarity is essential to both understanding and developing sound policy for environmental problems, I encouraged students from a variety of disciplines to enroll in my courses. This upset some faculty who felt a substantial number of science prerequisites should be required for enrollment. Students from all over the world wanted to discuss humankind's ethical responsibility for other life forms and the interdependent web of life. One of the most interesting students was Tashi Wangchuck from Bhutan, a small country with an impressive diversity of wildlife. Moreover, Wangchuck was interested in living sustainably and ethically with these other life forms. Other students from both developed and third world countries had very similar interests, but were coping with dissimilar issues.

Naturally, I was interested in the reasons for the excitement generated by field stations. My first insight was the unique ambiance of field stations. Without question, living together reduced, but did not eliminate, the barriers between students and faculty. For those students who had been born, reared, and educated in cities, field stations were clearly a defining moment in their lives. However, actually standing in ecosystems, both damaged and pristine, had a profound effect upon the discussions. Field station experience is usually limited to a few disciplines, such as biology, forestry, fisheries, and geology. Fortunately, all our children shared this experience with Jean and me. Our oldest child Karen had only one full summer (1961) at a station. The other three children, Stefan, Duncan, and Heather, had many years at field stations. Individuals can develop a sense of eco-ethics in a variety of ways—both emotional and intellectual. Field stations are notable because they foster both kinds of eco-ethical maturation.

Invariably, I do ponder some "what ifs."

(1) What if a natural system was not easily accessible during my childhood? If I were growing up in the same area today, access to natural areas would be extremely limited. Other residents of the Warm Hearth retirement community where I now live indicate that my experience in the loss of the natural systems of my childhood was all too common.

(2) What if I had not had extensive field station experience? I did not initiate either of the RMBL and UMBS experiences. My initial expectation of a field station experience was to increase my teaching abilities, which it did. The bonus, which became more and more important as the years passed, was development of a sense of eco-ethics.

(3) What if my family had not shared my love of field stations?

(4) What if population growth and the consequent explosive growth of recreational facilities (e.g., ski resorts) had not encroached upon field stations, thereby diminishing their ambiance?

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CHAPTER 9

BIOLOGICAL MONITORING RESEARCH

In the mid-1960s, I became convinced that, if human society had "real time" information about the condition of the planet's biospheric life support system, it would take immediate corrective action when the health and condition of these systems were threatened. I defined the term *biological monitoring* as surveillance undertaken to ensure that previously established quality control conditions were being met. To get the information rapidly, computer-interfaced systems were used. Of course, the system has been vastly improved over the years, but it represented a significant improvement over the methodology used at that time. However, human society only minimally recognized both its dependence upon the biospheric life support system and its ethical obligation to other life forms. Over four decades later, this condition still persists.

I toured many industrial plants while doing field surveys and collecting industrial waste effluent for toxicity tests. Most industrial quality control systems were impressive. When any entity pushed a part of the system beyond acceptable boundaries, sensors detected the deviation from normal limits; alarms went off, and corrective action was taken immediately. I wondered if a comparable, rapid information system could be designed for environmental quality with near real-time feedback of information and a concomitant means of either shunting the unacceptable waste to a holding pond (instead of discharging it into a river), recycling it for additional treatment, or, in the ultimate emergency, shutting down production until the problem was corrected. Such a system would require direct computer interfacing (or what passed for computers in the 1960s) and a whole set of early warning parameters that could immediately signal the appearance of a deleterious level of toxicity in the waste stream before it reached the river. The Academy of Natural Sciences Philadelphia (ANSP) did not have staff with the technical competence I required for this undertaking. However, when I left ANSP in 1966 for the University of Kansas, I had to rethink my entire research program: the field team research approach was definitely out; aquatic toxicity testing could continue, but should I do this type of research?

I decided to launch an entirely new rapid biological information system to operate as a quality control unit. Early attempts are described in some of my publications from the 1960s (Cairns 1966, 1967). In my search to find help with the technical aspects of a computer interfaced system, I had the good fortune to encounter H. W. (Tony) Shirer, a Professor of Electrical Engineering at the University of Kansas who also happened to have a medical degree. About the same time, I acquired my first PhD candidate, William T. (Tom) Waller, who was extremely enthusiastic about rapid, biological information systems. Our first apparatus (the electronic components were built almost entirely by Tony Shirer and the equipment to test fishes was built predominantly by Tom Waller) was from a design that I developed and was funded by a water resources research grant obtained in a national competition through the University of Kansas Water Resources Research Center. The first publication on this apparatus appeared in 1968 (Shirer et al. 1968). I also acquired my first MS candidate, Richard E. (Rip) Sparks; however, the space was insufficient in the tiny research "closet" allocated to me by the Zoology Department of the University of Kansas for more than one student. Although Sparks had finished his MS thesis on aquatic toxicity testing when I left Kansas, he and Waller both came with me to Virginia Polytechnic Institute and State University (VA Tech) to work on rapid, biological monitoring systems.

Fortunately, Alan Heath, then a young assistant professor at VA Tech, was measuring electrical signals from fish heart and opercular muscles for his physiological research. We quickly added this methodology to our research base, using equipment initially borrowed from Heath. The basic idea was not new—the King's wine taster and the canary in the coal mine are well known examples of early warning systems. The objective was to detect deleterious concentrations of chemicals before they left the factory and entered the stream since pollutants are generally most easily treated in their most concentrated form. Water quality frequently has pronounced effects on the expression of toxicity. Here was an opportunity to assure that the assimilative capacity (non-

harmful loading) of the stream or river was not exceeded. Instead of analyzing complex waste components individually, their biological effects could be measured in the aggregate. Of course, discovering the particular cause of the problem still required chemical analyses. Furthermore, knowing the precise concentration of each individual chemical in an effluent did not provide a reliable estimate of the combined effect. The investigation worked quite well for short-term episodic events (e.g., spills), but not for chronic, long-term toxicity problems. The cause of the warning signal was usually immediately apparent, but sometimes I had to visit the purchasing department of the industry to check for any new chemicals that had been acquired. Often, a new chemical was being used that was not a part of the production process (e.g., a new cleaning agent) and was not on the list of chemicals for which regulatory compliance analyses were required.

As has often been the case throughout my career, large foundations did not generally fund this area of research. However, the Federal Water Quality Administration (the forerunner of USEPA) did. Preliminary papers on this research were published in 1970 (Cairns et al. 1970a,b). By the time the apparatus was ready to be field tested, its size had been reduced due to miniaturization of computers. The Manufacturing Chemists Association (MCA, now the Chemical Manufacturers Association) not only sponsored the trials, but also found a nearby industry (Celanese Corporation) for the actual work. Later, even more extensive trials were carried out at the Radford Army Ammunition Plant (RAAP), which was nearer the VA Tech campus. At the latter site, the entire apparatus was housed in a mobile trailer. A postdoctoral fellow (David Gruber), later an adjunct faculty member, spent many years entirely on the RAAP project. Both MCA and RAAP agreed at the outset to unrestricted publication of the results—what an advantage to find a sponsor willing to forgo censorship before publication! Fortunately, many organizations now realize that the promise of unrestricted publication vastly increases credibility, despite some evidence that public relations personnel regard such publications as unfavorable.

The graduate students involved in this extended research, listed in order of graduation, were: 1971 – Richard E. Sparks, PhD; William T. Waller, PhD; 1973 – Eric Morgan, PhD; 1975 – Anthony F. Maciorowski, PhD; I. Prather, MS; W. B. Wrenn, PhD; 1977 – William van der Schalie, PhD; 1979 – Kenneth S. Lubinski, PhD; 1984 – Thomas R. Doane, PhD; and 1985 – S. I. Hartwell, PhD. Postdoctoral fellows were David Gruber, Kenneth W. Thompson, and Gary F. Westlake. Their contributions were essential to the completion of this complex research.

Industrial effluents have a way of co-mingling in receiving waters, and the cumulative impact often differs significantly from individual effects. In addition, a community of indigenous organisms could be used as sensors, and the analysis could be automated so that results could be quickly compared with the single species "in-plant" monitoring system. Diatoms were selected as the indigenous organisms because Patrick's diatometers, which were colonized by diatoms, had a two-decade history of being effective in a variety of conditions.

After a number of false starts, both within and outside the university, Silverio P. Almeida, an optical physicist on campus, agreed to work with biologists to address this problem. Naturally, large foundations rejected the grant proposals as not on their priority list or as too visionary, unworkable, etc. Again, the MCA came through with a small sum of money that enabled the construction and testing of a small working model. We then approached the National Science Foundation, Research Applied to National Needs Program and got multi-year funding. The sum seemed large to me as a biologist, but to a physicist, it was barely adequate. I enjoyed working with Almeida and the people he selected to participate in the project. I found the system intimidating and felt very insecure until Almeida ventured the opinion that diatoms were very complicated. We were each strangers in an alien discipline, but eventually, everyone functioned as a cohesive group.

Publishing this research was as great a challenge as obtaining extramural funding. Eventually, the first manuscripts were published; within a few years, we even published in one or two journals that had rejected the pioneering manuscripts. I still have warm feelings for the *Transactions of the Kansas Academy of Sciences* and *Archiv für Microbiologie* for having the courage to publish the first manuscripts (Almeida et al. 1972, 1978; Cairns et al. 1972). Later publications include Cairns et al. (1976, 1982) and Case et al. (1978).

None of the monitoring systems from our laboratory were widely used, probably because automated environmental quality control was still not a high priority item in industrial settings. Still, all the systems were sufficiently tested elsewhere to demonstrate their utility. I remain confident that the years spent on an automated monitoring system provided evidence that system-level, automated, environmental quality control is possible and that, in the future, society will realize the benefits of monitoring the health of the biospheric life support system. However, until humankind recognizes its dependence upon the planet's biospheric life support system and its ethical responsibility for other life forms, the likelihood of extensive use of any monitoring system that might markedly change the behavior of human society is unlikely.

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CHAPTER 10

THE GRADUATE STUDENT ERA: 1966-1997

The differences in the dynamics of a research organization (at least the one in which I spent the first 18 years of my professional career) and a research university are more numerous than a casual observer might think. In the former, at least in the part in which I was employed, extramural funding was essential to survival, and any major disruption of cash flow meant no money for salaries. This situation definitely produced a sense of community in the group. Even though the organization was large enough that money seemed to be available somewhere, one never knew when transition funding between grants would be necessary. Therefore, helping others in time of need increased the probability of being helped in times of need. Of course, Ruth Patrick was chair of the Limnology Department of Academy of Natural Sciences Philadelphia (ANSP) and had the final decision on all money allocations, but she had a strong incentive to maintain the group intact. Patrick had a few graduate students over the years I was there, but other members of the group had none. The technical staff, laboratory technicians, etc. outnumbered the curatorial staff (faculty) by a much greater margin than in most research universities. They stayed longer than graduate students; had no other distractions, such as course work; and could easily be moved from one grant to another. The staff worked 35 hours per week—not the long hours of graduate students. The staff was, for the most part, solid and dependable workers, but they did not share the excitement of publication as do graduate students. The staff did realize that publications were a discrete form of academic advertising that helped maintain the cash flow and increased job security. They were not risk takers with new ideas as are first-rate graduate students. Arguably, the most important difference was that they did not make presentations or present papers at professional meetings and were not usually as sensitive to potential weaknesses as those who were exposed directly.

An equally important difference between research organizations and research universities was the vulnerability that purchasing personnel felt financially if equipment purchases were delayed, which subsequently had adverse effects on completing the work for the grant. Accountability was very high in the research organization and purchasing was not impeded by the complex rules (bids, etc.) that were common to research universities. On the other hand, overhead charges in research organizations were much higher because no endowment or other support was available to subsidize salaries for maintenance, purchasing agents, secretaries, and the like. Even though Patrick bore the primary responsibility in the research organization for keeping people employed, I was very aware that "inappropriate" time management on my part would cause financial hardship for others.

At ANSP, I filled in daily time sheets at the half-hour level to show what grants my time had been charged to that day. After detailing working time for approximately 18 years, I was somewhat disoriented when I went to the University of Kansas and did not have to account for how I spent my time. Although tracking working time at the ANSP did tend to compartmentalize my thought patterns, it was not as bad as it sounds. Effects were probably diminished because official work time was 35 hours per week, but the time actually spent was far greater.

The discussion on money in this chapter may have too much emphasis; however, even today in retirement, I cannot do everything I wish to do professionally without funding. During my more active years, even the graduate students who had NSF predoctoral fellowships needed money for equipment, travel, and journal page charges. Isaac Asimov is reputed to have defined academic freedom as "extramural funding." With extramural funding, publications, and a clear view of future research goals, the number of options for obtaining funding is vastly increased over those faculty with good teaching records alone. Although my emphasis on funding may offend some people, it has been central to my career. The worst periods of my career have been the two times when the cash flow temporarily went below a critical level.

As I consider my research career, I realize the growth and importance of my relationships with graduate students. I did not have graduate students at ANSP (despite the institution's

affiliation with the University of Pennsylvania) because I was in the field too much and had no space for a graduate student to work. However, when I left ANSP for the University of Kansas, I acquired my first two graduate students, Richard (Rip) E. Sparks and William T. (Tom) Waller. Graduate students who worked with me during the three different stages of my career were, in one sense, working with a quite different person.

Period I—Buildup Period

Although I went to the University of Kansas as a full professor with immediate tenure, I had no experience chairing graduate committees, had left all my equipment behind, and had no grants or contracts that were transferable (although I quickly acquired new extramural funding). Furthermore, I was about to embark on a very risky research operation—the idea that biological early warning systems could be developed that would produce nearly real-time information on ecological stress with the use of computers.

I was only at the University of Kansas for two years (1966-1968), but Rip Sparks acquired his MS and Tom Waller, who already had an MS from Pittsburgh State University in Kansas, had a two-year start on the PhD. Sparks was intensely interested in the computer-interfaced, biological early warning system that I was then developing; the system was also the focus of Waller's dissertation research. Since research space at Kansas was inadequate for the project, I was delighted when Virginia Polytechnic Institute and State University (VA Tech) offered me a free hand in developing my research program and offered to transfer Waller's and Sparks' academic credits. The Chancellor and the Provost at the University of Kansas very kindly telephoned then President of VA Tech, T. Marshall Hahn, to apprise him of my research space problems; this kindness further facilitated the transfer of the research project. I am deeply indebted to all three.

Despite my brief stay at the University of Kansas (brief because of the poor quality of the research space assigned to me), I had valuable experiences: (1) I was successful in carrying out research in a new setting; (2) the research was of an entirely different character than my past research; (3) I had acquired the ability to interact productively with graduate students; (4) I had become acutely aware of the need to negotiate explicit conditions when moving to a new institution; and (5) I had learned how much one could depend on quality graduate students.

While at the University of Kansas, I gave seminars at North Texas State University (now University of North Texas) in Denton, Texas, and Central College in Pella, Iowa. I gained two graduate students from these visits. At the University of North Texas, I met Ken Dickson, who had obtained his MS with J. K. G. Silvey. At Central College, I met Jeanne Ruthven, who was interested in protozoan colonization of artificial substrates. David E. Wilson of the Biology Department at Central College provided superb help during Ruthven's entire senior year in getting her ready for rapid protozoan community identifications. I met Jeanne Ruthven because Don Huffman, then Biology Department Chair at Central College, had worked with Alexander Smith at the University of Michigan Biological Station where I taught for many summers.

Although I appreciated all them at the time, I realize in retrospect that I was blessed with four capable, independent graduate students at a time of my greatest need. These four graduate students were the key to a successful outcome. Had I not worked with all four before moving to VA Tech, I could not have distributed my energies and time the way I did. Had this division of energy and time not been possible, a successful outcome would almost certainly have been in serious doubt.

The summer before I was to begin the academic year at VA Tech, Jeanne Ruthven and Ken Dickson joined me at the University of Michigan Biological Station on Douglas Lake where I had been teaching since 1964. At that time, I was teaching protozoan ecology. Jeanne Ruthven became my teaching assistant, which required an ability to identify protozoans. Ken Dickson and I started research on the sequential comparison index for which I had already published one paper (Cairns et al. 1968), but for which researchers wanted more detailed directions. I was able to know these students in a far different set of circumstances than is customary in a research university setting.

Waller transported the computer-interfaced monitoring equipment from the University of Kansas, but it was not set up until space in Derring Hall on the VA Tech campus became available

the following academic year. Sparks was able to work on electrical signals from fish opercular muscles and the heart in Alan Heath's laboratory. Sparks, Waller, and I spent much of our time in Price Hall writing grant proposals on rapid biological monitoring systems. In addition, Waller and Sparks completed much of their coursework. Dickson was to work on rapid biological information systems of stream and river communities. Ruthven was to carry on my research program in freshwater protozoan colonization dynamics. Heath was enormously helpful with establishing my program in monitoring at VA Tech and served on both Sparks' and Waller's advisory committees. All this assistance made it possible to reestablish my research program in a very difficult period.

Period II—Expansion (or Growth) Period

I was initially employed at VA Tech in the Department of Biology in a research position, which required no teaching. However, since I had not regularly had the enjoyment of teaching in a university setting, I decided to teach limnology and protozoology the first year. In addition, I was charged with building an aquatic ecology program, particularly at the graduate level. The research in this program was to be supported entirely by extramural funding. All four of my graduate students were PhD candidates who had a large stake in the success of the research.

Since Derring Hall was not completed on schedule, Bob Paterson, the department head, offered me a large room in Price Hall that would serve as office space for me and the graduate students; the space also had microscope benches for protozoology and aquatic insect identifications. Even this temporary space was over four times larger than my space at the University of Kansas. I was cautioned by some of the faculty, who knew that I had little experience with PhD candidates, that sharing office space with them was not a good idea. However, this set up was the only choice for the situation. I had to place enormous burdens on the graduate students, well beyond what should normally have been expected of them. Some of my graduate students have commented recently that this philosophy of graduate student training prepared them for the real world and has proved highly successful in their careers. The fact that they accepted additional responsibilities with enthusiastic consent did not lessen my apprehension because, if this effort failed, I might easily have lost faith in the entire research undertaking, which many of my colleagues felt was too visionary and impractical. In a very real sense, the small group functioned almost immediately as colleagues, although I was still their mentor. The graduate students helped prepare grant proposals, selected some field sites (which I did not have enough time to visit initially), worked together as a team that helped each other, and, most heartening of all, developed the writing skills necessary for publication of parts of their research before completion of the dissertations.

Before the middle of the first academic year, two federal monitoring proposals and two field studies were funded. The little group in aquatic ecology went from poverty to abundance literally almost overnight, and the problem became how to complete all the work without diminishing quality. The graduate students immediately switched to extensive research. Since we had been functioning as a group, obviously successfully, the team spirit never diminished. If someone needed help, one of us volunteered.

When we moved into more spacious quarters in Derring Hall, we kept in close touch with each other as a matter of course. At the end of the first year, inquiries came from other prospective graduate students. I asked my present graduate students for help in determining which applicants seemed best suited to the research program that required a high degree of individuality coupled with a team spirit. This team spirit continued throughout the remainder of my professional career, although, not surprisingly, some individuals preferred the "lone wolf" approach much more than others. However, since the nature of our research required interactions with other disciplines and other components of society outside the academic world, everyone shared the view that a multidimensional approach to environmental problem solving, though demanding, was also exciting.

Unlike my employment at ANSP, my salary would be constant in the university setting even if I failed to obtain grants. However, the technicians, graduate students, and the hourly employees, including undergraduates, would have no income if extramural funding were not available. I could not calculate exactly the right amount of money to obtain because students kept coming and going

and projects changed. I settled on two solutions: (1) obtain a little too much money and have everyone agree to share the extra work load for the benefit of increased security and (2) place my consulting and speaking fees in a special account to be used in emergencies. The group shared the extra work load, and this team approach contributed to the community spirit. However, the amount of funding that I was able to sequester at that time, although significant, was not sufficient to provide the degree of security everyone wanted. Fortunately, the students knew that, when I was absent for consulting or speaking, much of the money I earned went into the kitty and, therefore, benefited them. They knew I was not neglecting them through my absence, but I was actually fostering their professional careers.

Lest this description of community spirit appears too utopian, I must note that many problems surfaced when the students interacted with more than one department. The interactions were absolutely necessary for the graduate students since they needed the help of electrical engineers and the engineering faculty (particularly Cliff Randall in Civil Engineering) with equipment construction (most equipment construction had occurred at the University of Kansas, but continuous remodeling was necessary at VA Tech). Faculty in other departments helped with computer interfacing (Ray Dessey in the Department of Chemistry was particularly helpful in the early stages of development) and statistics (J. C. Arnold in the Department of Statistics was particularly helpful in the early stages and Eric Smith, in the same department, in the later stages). Another problem surfaced when the graduate students published in journals that, at that time, were not familiar to most biologists (my graduate students "home" discipline). Although this challenge ultimately helped their careers, it did isolate them from the faculty in the local biological community, some of whom felt that biology entailed too much for any one person to learn, so why tackle other disciplines. The answer is, of course, that the isolation of the disciplines from each other is an academic artifact that impedes recognition of the connections that exist in the natural world. This isolation of the program and students at that time worsened as the Aquatic Ecology Program expanded to the point that it was recognized as an individual entity. This misunderstanding worsened further with the formation of the University Center for Environmental Studies (UCES), which gave the program an administrative impetus. A variety of interactions under this construct mandated fewer interactions within other members of the biological community.

I served as director of the UCES for roughly two and a half decades. Its purpose was to engage in environmental research that transcended the capabilities of a single discipline and was almost entirely supported by extramural funding. Dickson finished his degree with me when the UCES was launched, and he became my assistant director.

Quality control became a real concern in this program that grew so rapidly and was so different from other departmental activities and whose faculty was young and untenured. As a consequence, in addition to each student having an advisory committee, each was required to report individually once a month to the entire aquatic ecology faculty. Many students regarded this extra requirement as an imposition, not endured by graduate students in other academic units. Dick Pratt (personal communication) remarked that graduate students came to refer to this requirement as "The Inquisition." However, Pratt admits this individual meeting with the faculty was one of the most important aspects of quality control ever practiced. He also feels that it was ultimately good for students—even the ones who hyperventilated. Furthermore, this requirement was necessary for a number of reasons. Many grants at that time were block grants with a number of components assigned to individual faculty members and, hence, to particular students. The student's advisory committee was not particularly intent on ensuring that the conditions of the grant were fulfilled, especially when many of the faculty on these committees were specialists in other areas of biology than stressed ecosystems and might have been inclined to encourage the students to pursue directions not covered by the grant. However, if one or more components of the block grant had notable failures, then the entire block of funding was seriously threatened. Many industrial grants required monthly progress reports (and a few weekly). This reporting procedure was not a well understood situation by either the new aquatic ecology faculty or faculty members outside it. Funding in the Department of Biology at VA Tech was not robust when the aquatic ecology group

was formed, and many students outside the aquatic ecology program were carrying out research while being supported by teaching assistantships or their own money. They were not under the pressure of reporting at two meetings or writing monthly (or even weekly) reports. This difference led to a certain degree of isolation of each group of students from the other.

This funding regime created another dichotomy—the "haves" and the "have-nots." Graduate students whose major professors had money could spend a huge amount of time on their research, particularly when funding during summer was included. Money for equipment, for travel to meetings to present papers, for hourly help and technicians, and the like made research easier. At VA Tech, all graduate students were required to teach a course, a lab/lecture session in general biology (a good idea, since many of them would be teaching general biology in their first professional position). After this requirement was met, those with extramural funding could concentrate on their research. Those without extramural funding were on teaching assistantships, which took a substantial amount of their time. Naturally, some loss of camaraderie was inevitable when some graduate students had what appeared to be endless amounts of money and equipment and others were scraping along on a shoestring. The same was true to some degree among the faculty. Even some of the sources of money (sometimes labeled "dirty money") were from non-traditional sources, such as industry, municipalities, and the like; "clean money" was from the National Science Foundation and other similar organizations. Government grants from organizations such as the USEPA were usually regarded somewhere in between, but more on the side of the foundations. Of course, in more prestigious institutions, the "haves" far outnumber the "have-nots." In institutions devoted primarily to teaching, the "haves" are a minority. The institutions between these two groups have varying proportions and, therefore, a significant dichotomy. This situation is a foretaste of professional life, not only in academic institutions but elsewhere where research funding is scarce.

Funding I obtained for graduate students also allowed them to reallocate time to writing manuscripts and publications. Everyone who contributed conceptually to a project was included in the list of authors. Initially, I was senior author because I had developed the research plan and, in some cases due to the novelty of the approach, extensive interactions were necessary with journal reviewers, and so on. Furthermore, until the graduate students had a bit more experience so that they were reasonably well acquainted with the professional risks involved in publication, I felt students should not be senior authors. As soon as they were well aware of the risks and had made a major conceptual contribution, senior authorship for them seemed an obvious choice. I followed this practice throughout my career and have often been the last author on a publication. The progress from junior author to senior author showed the students that I had sufficient faith in their research to take the risks of publication with them.

I have always guaranteed my students that I would pay for page charges, purchase a reasonable number of reprints, pay extra charges for figures and photographs, and nearly always give money to them for travel to professional meetings to present papers. This assistance was not entirely altruistic. When one presents a paper at a professional meeting, one's attitude towards research is never the same. The same is true after one sees reviewer comments on what the author felt was a perfect manuscript. Almost invariably, one gathers the extra bit of data to make the manuscript even more persuasive and looks at it with more critical eyes after the first professional publication. Furthermore, this process turns all research activities into collaborative partnerships since the partners are attempting to persuade others that the research is sound and are working energetically to minimize criticism. This relationship is entirely different from the one that exists if the dissertation or thesis is being reviewed internally by a clearly identified group of faculty. The "proof of the pudding" of the effectiveness of this relationship is that many of my former students and I continued to publish together long after they have obtained their PhDs.

I employed Darla Donald as an editorial assistant in 1975 and paid her from consulting fees, book royalties, and money from a variety of sources. Graduate students were encouraged to give her their manuscripts when they had reached a certain stage of development. She styled the manuscript for the requirements of a particular journal and edited the writing. Having such a person available

during one's graduate career could make one dependent on an editorial assistant. In fact, however, the training in preparing manuscripts that they received from Darla has been invaluable to practically every graduate student in the program. Even the ones who did not go into academia were still required to write reports for industry or government agencies. The students' acquiring manuscript preparation skills was as important as learning how to write grant proposals (Darla also helped in this regard), prepare budgets, and the like.

Another advantage to the graduate students (and undergraduate students working on research projects under my supervision) was the availability, from the beginning, of a manuscript typist. Initially, the cost of this position was covered by extramural funding but, when I became a University Distinguished Professor, a full-time secretarial position was created in the Department of Biology by the President and Academic Vice-President to cover this function.

In 1970, the President and Academic Vice-President (now Provost) asked me to organize a University Center for Environmental Studies (UCES) as an administratively independent unit within the Research Division so that any or all colleges could be involved whenever appropriate. All my students were already working with people in other disciplines, so the transition was both easy and natural for them. Today, involvement with other disciplines is more common, but, nearly three decades ago, it was remarkable and caused much discussion and comment ranging from admiration to hostility. Some years ago, some civil engineers wanted to form a hazardous materials center and, when difficulties were encountered, permission was granted to extend the designation of the existing Center to University Center for Environmental and Hazardous Materials Studies (UCEHMS). Although the UCEHMS still exists on paper at present, no buildings, equipment, nor faculty are intimately associated with it. Apparently, no one was anxious to take on the responsibility of an organization so dependent on extramural funding. However, the model developed at VA Tech has been emulated by many other institutions, and some have even employed students from the original UCEHMS program. The University of North Texas Institute of Applied Sciences is the most visible example. The Environmental Studies Program at Arkansas State University is developing in a manner similar to UCEHMS. Other programs, such as those at Mississippi State and Clemson University, could be viewed as equivalent models still functioning robustly.

As faculty members in the aquatic ecology group attained tenure and additional seniority, they naturally wanted to explore their personal interests and engaged less in group activities. Since the group never had formal administrative approval, its dissolution was a matter of mutual agreement. Some of the aquatic ecology faculty, such as Ken Dickson and Albert Hendricks, chose to remain with the UCES, and, therefore, the dissolution of the aquatic ecology group did not destroy the group's spirit. The exposure to a multidimensional view of environmental problems, including statistics, economics, engineering, chemistry, and physics, was useful to the students who interacted with these other disciplines. However, the depth of interaction with one's disciplinary peers is necessarily weakened by developing relationships with other disciplines.

A comfort often comes from belonging to some sort of "tribal" group professionally, where a relatively comparable level of literacy on a particular subject exists, problems are common, mutual assistance is offered, equipment is shared, and, perhaps most important, a shared terminology exists. Those graduate students coping with hypotheses and problems that require significant input from other disciplines generally do not share the same needs and are not part of a tribal unit. When graduate students in these circumstances compare their lot with the camaraderie of the more closely knit subdisciplinary groups, the situation is often unsettling. However, such circumstances may be a foretaste of the highly probable circumstances in their future careers. Fortunately, multidimensional approaches to environmental problems are now becoming the norm, so we were on the right track despite the problems that the cross discipline approach caused my graduate students and, of course, me as well.

Period III—Equilibrium Period

As is the case for all rapid but incremental growth in any transdisciplinary group, no abrupt thresholds were crossed, but, rather, a series of breakpoints and thresholds occurred. However, the middle period had a number of distinguishing characteristics: (1) diminished contact with each individual, even my own students; (2) diminished time for my own research despite a 60+-hour work week and no vacations; (3) diminished interactions with the Department of Biology due to involvement with the UCEHMS and the sheer volume of work; (4) delegated to an administrative assistant and a business manager matters I once handled personally; (5) increased attention to persons outside the university, primarily because of chances of extramural funding, which supported the two groups for which I was responsible; (6) caught between two worlds of administration and faculty. In this middle period, I felt I had accomplished the two charges given me when I first arrived at VA Tech: (1) establish a major research program that would receive national and international attention and (2) fund this organization almost entirely with extramural funding.

From the standpoint of both graduate and undergraduate students, the program appears to have been successful despite the issues already mentioned. The students now have challenging positions in the field in which they obtained their degrees. All the presidents, provosts and deans of the research division, deans of the college, and at least two biology department heads were pleased that so many graduate students were supported by extramural funding. I also felt a personal satisfaction to have fulfilled the charge given to me by the university when I was employed.

However, I had no trouble relinquishing the leadership of the Aquatic Ecology Program when the members of that faculty acquired tenure and wished to pursue other ventures. Although I had much help in acquiring extramural funding, I was held personally responsible, both internally and externally, for ensuring that the conditions of each grant were met. Mercifully, the amount of paper work on grants, both internally and externally, was far less in those days. Many years later, I still have a good relationship with students from that period despite the reduced amount of my time available for them individually. This group of students includes some whose committees were chaired by others. Some students from this period have commented recently that I provided evidence that one could do both administration and research (for which Ruth Patrick was my model). Clearly, neither task was done as well as each could have been if each had been done alone. On the other hand, had I devoted all of my energy to administration, my professional career would probably have ended at age 65. I simply cannot imagine working six days a week on administration solely and feeling the same zest I would feel if it were research.

Since neither the Aquatic Ecology Program nor the UCEHMS has survived in its earlier form (the former is a loosely affiliated group of individual investigators, and the latter is a paper organization), I wonder if my efforts were wasted. Gene Odum left a thriving Institute of Ecology at the University of Georgia. Ruth Patrick left a solid organization at ANSP, and other organizations have gotten their starts there. Clearly, the Aquatic Ecology Program and UCEHMS might both have survived if all my energy had been expended on one or the other and on either administration solely or research solely. Still, I learned valuable lessons that might not have been possible any other way. For example, interdisciplinary teams were a good transitional stage for resolving complex problems. However, most team members retained their disciplinary bias, which made communication and, thus, synthesis difficult. The emergence of multidimensional individuals signaled the doom of interdisciplinary teams, although most organizations seem not to have realized this change. After fighting interdisciplinary teams for decades, academic institutions now espouse such teams. These teams produce much information, but rarely is it used effectively. Having to acquire large amounts of extramural finding also sharpened my foraging behavior. I had grants until 2001, even in my retirement years. Arguably, the most important learning experience for me in the middle period was the development of a systems-level approach (top-down is the current jargon), which probably would have developed slowly in other circumstances. The optimism and zest of the graduate students persuaded me to explore new areas that I might not have considered.

The Middle Period was good for the students and for me. Some of the students are saddened from that time when they return to VA Tech and find that both programs have dramatically

declined from an organizational perspective. My view is that programs are just collections of individuals; the concepts have been nurtured by former students and are flourishing elsewhere.

Period IV—Closure Period

Students who worked with me from the time that funding for state institutions in Virginia dramatically decreased in the early 1990s had an entirely different relationship with me than earlier students. We were disassembling an organization that still required much of my time if it were to be done without hurting any of the people involved with it and if the organization's reputation was to be maintained throughout this process. When Karen Holl and I were discussing the possibility of my chairing her PhD committee, I mentioned my perception of trying times ahead. I was confident that, even though I was eligible for retirement with full benefits, I would be working long enough for her to get her PhD and I would have enough resources to support her research. John Heckman, my final graduate student, saw the disappearance of the entire UCEHMS program, the dispersal of the equipment, and the like. I was able, fortunately, to continue a professional working relationship with B. R. Niederlehner and Darla Donald. I retired two years before John Heckman's graduation and was in the process of dismantling the UCEHMS for the first half of his graduate career.

Having students during the closure period was quite different from the earlier periods. During this time, students were working on all the topics that I worked on throughout my career, but the faculty teams that could have aided in this undertaking were gone. While Karen Holl was a candidate for the PhD, students were working on toxicity testing, genetically engineered microorganisms, and various topics in ecological restoration. The intellectual overlap with graduate students in other parts of the program was dramatically reduced. Still, my remaining graduate students interacted well with each other on a personal level and helped each other. Senior technician B. R. Niederlehner was especially important during this period as was editorial assistant Darla Donald. The students were extraordinarily resourceful, which benefited them greatly after graduation. Another major difference was that I was much more available to the students due to greatly reduced field work, less grant money to keep track of, fewer major reports, and far less travel. I also worked fewer hours, although, in the aggregate, they still exceeded 40/week.

Unquestionably, my relationship with my graduate students was different in each of the three periods of my professional career in a variety of qualitative ways. Students in all three eras learned much about the process of science, working with those in other disciplines, acquisition of extramural funding, the process of publication, preparation of quarterly and annual reports on extramural funding, and meeting with a variety of groups (e.g., regulatory, federal and state, industrial, citizens and environmental activists, recreational, and municipal). Students from each of the three eras have careers in quite different professional positions in academe, industrial, regulatory, and non-profit environmental organizations. Clearly, their career choices were based on their individual predilections rather than their relationship with me. Or, stated differently, the types of exposure just mentioned were the dominant factors in a career choice rather than the era in which they were graduate students. Last, but far from least, I have enjoyed an ongoing professional relationship with many of my former graduate students, in some cases, for decades after graduation. Again, these relationships have depended more on their individual personalities than on the era in which they were graduate students.

I cannot possibly indicate a preference for one of the periods. I found each exciting, challenging, and refreshingly different. The shared excitement in the undertaking seemed to override all other considerations. Interestingly, some of the most enduring, post-graduation relationships have been with students whose committees I neither chaired nor co-chaired. Students from all three eras meet together at particular professional meetings and rekindle a group identity that I would not have thought possible. Some years ago, I received a pennant from one such meeting (Pensacola Society for Environmental Toxicology and Chemistry meeting in 1987, which I regrettably missed), signed by well over half of my former graduate students. They indicated that I should not miss this particular meeting in the future because I would miss seeing them on an

annual basis. Also, when my former students celebrated my 70th birthday and my formal retirement (which occurred soon thereafter), considerable numbers from all three eras appeared and joined together harmoniously. At my 70th birthday, some of the then current graduate students were taken aback at the age of some of the earliest students, but then they were over two decades apart. In 1968, I shared an office/laboratory with my four graduate students. All indoor work (e.g., protozoan identification, examination of macroinvertebrate samples) was performed there. Eventually, we acquired a computer. At the end of the graduate student era, the graduate students shared office/laboratory space together, although each had a computer. Closing the administrative units was an activity not suited for graduate students, and one I hoped would never be necessary for them. Perhaps the explanation for the closeness is that one should always be undertaking something new and exciting, in which the graduate students can take part and be intellectually stimulated—the nature of the undertaking, as long as it is not disreputable, makes little or no difference.

Although my last graduate student finished in May 1997, I still served, until recently, on graduate committees, advised students, supported a few students on grants, and discussed issues with colleagues in various stages of their professional careers and in many areas of the world. What can I now pass on to students and colleagues? Success is difficult to define. Happiness, as defined by contemporary American culture, is too simplistic and materialistic for my taste. Enjoying one's life is my definition of happiness. As a caveat—no one is entitled to a "free ride" because of family, wealth, or power. Everyone should contribute to societal integrity and the health of the planet. This contribution should be a continuous process, not something postponed to a "convenient" time or special day (i.e., Earth Day). I always asked each new graduate student (and undergraduates seeking advice) how s/he envisioned life at 50 (for the young, a formidable task). Then I would ask—how do you feel about that image? If the feelings were not positive, then I pointed out that something is wrong! I used these questions on myself, which resulted in several career changes and a satisfactory transition to formal retirement but not a cessation of professional activities.

I always tried to help each student determine personal tolerance for risk. Achievements in any field are invariably preceded by various risks. So are major disasters! World-class achievers generally are so involved with their projects that the risks do not paralyze them. Awareness of the risks, as a result of doing their "homework," usually inspires them to exceptional efforts. For most, the price, in energy and time, is unthinkable. For a few, loss of an opportunity is unthinkable. These judgments are a life-long exercise. One can help students and those in early stages of career development to find their own tolerance levels by describing one's own choices, good and bad.

Another essential "leg of the stool" is to associate with people of like mind to the fullest extent possible. Excitement is contagious! Although personal needs undoubtedly vary, I find just one or two highly motivated people that one encounters professionally once or twice a week is satisfactory. In an era of rapid distance communication, trusted colleagues anywhere on the planet will serve very well indeed.

I cannot imagine my career without graduate students! Still, I do not regret their absence in the early part of my career. In retrospect, I was not fully prepared to chair committees due to lack of university experience. Most important, I was not sufficiently well established that students who would fit my research program would seek me out. I cannot choose one period of my professional career over another, but the dismantling of the programs that I had built over decades was a sad period. Easing the impact on students and staff made it bearable.

Although some faculty had more personal relationships with graduate students (e.g., going out with them at the end of the week for a few beers or having them to their home for dinner), I did not feel that such activities were appropriate behavior for me (I do not condemn it for others). I did occasionally go white water canoeing with a group that included one of my graduate students, or I rode with a graduate student to a trout stream where we went our separate ways for a day of solitary trout fishing. Without question, I missed many opportunities for a different relationship with students, but I always believed in limits to the degree to which my private and professional lives should overlap.

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Cairns, J., Jr., D. W. Albaugh, F. Busey, and M. D. Chanay. 1968. The sequential comparison index – a simplified method for non-biologists to estimate differences in biological diversity in stream pollution studies. J. Water Pollut. Control Fed. 40 (9):137-140.

APPENDIX 2

Graduate Committees Chaired or Co-chaired

1968	Sparks, R.E. (MS)	Some Effects of Neutral Mixture of Calcium Oxide and Sulfuric Acid on Channel Catfish Ictalurus punctatus (Rafinesque)
1970	Dickson, K.L. (PhD)	Development and Evaluation of Methodology for the Instream Assessment of the Effects of Water Pollution Upon Macroinvertebrate Organisms
1971	Lorton, E.U. (MS)	The Effects of Thermal Stress on Protozoan Community Structure
	Sparks, R.E. (PhD)	Using the Respiratory and Cardiac Responses of Bluegill Sunfish (Lepomis macrochirus) to Monitor Zinc Concentrations in Water
	Waller, W.T. (PhD	The Use of Fish Movement Patterns to Monitor Zinc Continuously in Water
1972	Lanza, G.R. (PhD)	Effects of Thermal Stress on Microorganisms
	Ruthven, J.A. (PhD)	The Response of Freshwater Protozoan Communities to Concentrations of Various Toxicants Particularly the Heavy Metals, Zinc and Copper
	Yongue, W.H., Jr. (PhD)	The Structure of Fresh-water Protozoan Communities
1973	Crossman, J.S. (PhD)	Recovery and Restoration of Damaged Ecosystems
	Hales, V.M. (MS)	Biological and Chemical Monitoring of Three Streams in the Area of Blacksburg, Virginia (Co-Chair E.F. Benfield)
	Herricks, E.E. (PhD)	The Recovery of Stream Macrobenthic Communities from the Effects of Acid Mine Drainage
	Morgan, E. (PhD)	The Effects of Stress on Fish Behavior
	Sullivan, G. (MS)	Acute Bioassays for Assessing the Toxicity of Six Heavy Metals and Factors Affecting Zinc Toxicity to the Rotifer, Philodina (near acuticornis) (Co-Chair A.L. Buikema, Jr.)
1974	Camp, F. A. (PhD)	The Application of Algal Growth Potential Techniques to Surfactant and Zinc Toxicity Studies (Co-Chair A.C. Hendricks)
	Dolan, J.M., III (PhD)	Comparative Studies of the Toxic Effects of Three Surfactants on a Fish (Lepomis macrochirus Rafinesque) and a Snail (Goniobasis Lea sp.)
	Hocutt, C. (PhD)	The Effects of Celanese Inc., Narrows, Virginia on Chemical and Biological Water Quality in the New River (Co-Chair K.L. Dickson)
	Smrchek, J.C. (PhD)	The Effects of Various Tertiary Treatment Nutrient Removal Schemes Upon the Productivity of Autotrophic (Periphyton) Communities in Model Laboratory Stream Ecosystems
1975	Gregg, B.C. (PhD)	Effects of Chlorine and Heat on Selected Stream Invertebrates (Co-Chair E.F. Benfield)
	Maciorowski, A.F. (PhD)	Heavy Metal Toxicity to Aquatic Insects (Co-Chair E.F. Benfield)
	Prather, I. (MS)	Computer Interfacing with Biological Systems
	Slocomb, J. (MS)	Laser Holography as a Pollution Monitor (Co-Chair K.L. Dickson)
	Stauffer, J. (PhD)	The Distribution of Fish in Relation to Thermal Outfalls (Co-Chair K.L. Dickson)
	Wrenn, W.B. (PhD)	Remote Monitoring of Fish in a Thermally Enriched Zone (Co-Chair K.L. Dickson)
1976	Lee, D.R. (PhD)	Development of an Invertebrate Bioassay to Screen Petroleum Refinery Effluents Discharged into Freshwater (Co-Chair A.L. Buikema, Jr.)
	Messenger, D.U. (MS)	Variability in Sensitivity in Invertebrates to Various Toxicants
	See, C. (PhD)	Effects of Heavy Metals on the Behavior of the Planarian, Dugesia tigrina (Co-Chair A.L. Buikema, Jr.)

	Trotter, D.M. (PhD)	The Development of an Algal Bioassay Procedure using Stigeoclonium subsecundum and the Demonstration of the Effect of Intermittent Chlorination on an Attached Filamentous Alga (Co-Chair A. C. Hendricks)
	Wright, J.R., Jr. (PhD)	Chemical Limnology, Algal Growth Potential, and Nutrient Limitation Factors of the Upper New River, Virginia, and Predictions Concerning Trophic Status for the Proposed Blue Ridge Reservoirs (Co-Chair E.F. Benfield)
1977	Clark, J. (MS)	Evaluation of Methods to Estimate Aufwuchs Biomass (Co-Chair K.L. Dickson)
	Klarberg, D.P. (PhD)	Investigations of the Macrobenthos and Physiochemistry of the Upper New River Basin
	Larrick, S.R. (MS)	Behavioral Avoidance by Fish of Residual Chlorine in Power Plant Discharges (Co-Chair D.S. Cherry)
	McGinniss, M. (MS)	Interactions of Acute Thermal Shock and Simulated Effluents to Daphnia pulex (Co-Chair A.L. Buikema, Jr.)
	Paul, R.W. (PhD)	Leaf Processing and the Effects of Thermal Perturbation on Leaf Degradation in the New River, Virginia (Co-Chair E.F. Benfield)
	Rodgers, J.H., Jr. (PhD)	Aufwuchs Communities of Lotic Systems: Nontaxonomic Structure and Function (Co-Chair K.L. Dickson)
	van der Schalie, W. (PhD)	Use of Minicomputers in Biological Monitoring
1978	King, C.L. (MS)	The Development of a System using the Ventilatory Activity of the Fathead Minnow, Pimephales promelas, to Detect and Predict the Presence of Toxicants (Co-Chair K.L. Dickson)
1979	Giattina, J.D. (MS)	Response of Fish to Chlorinated Effluents under Field and Laboratory Conditions as Determined by Behavioral and Electrophoretic Procedures (Co-Chair D.S. Cherry)
	Honig, R.A. (MS)	Effects of a Simulated Refinery Effluent on Periphyton Communities in Laboratory Streams (Co-Chair A.L. Buikema, Jr.)
	Kuhn, D. (PhD)	A Biological Characterization of Environmental Conditions
	Leslie, M. (MS)	The Use of Biotic Value Allocation in the Assessment of Heated Discharges
	Lubinski, K.S. (PhD)	Monitoring Bluegill Swimming Behavior and the Effects of Sublethal Ammonium Gradients
	Plafkin, J. (PhD)	The Colonization of Artificial Islands by Protozoa in Differing Habitats and Systems
1980	Boatin, H., Jr. (PhD)	Factors Affecting the Endogenous Regulation of Fresh-water Protozoan Communities (Co-Chair W.H. Yongue, Jr.)
	Clark, J.R. (PhD)	Effects of Selected Pollutants on Grazer Utilization of Aufwuchs (Co-Chair D.S. Cherry)
1981	Abbott, T. (PhD)	The Role of Macrobenthos Drifts in the Energetics of Rainbow Trout (Co-Chair A.L. Buikema, Jr.)
	Henebry, M.S. (PhD)	Protozoan Communities, Macrophyte Vegetation and Trophic Status of Northern Michigan Wetland Lakes (Co-Chair W.H. Yongue, Jr.)
1982	Hart, K. (MS)	Effects of Toxicants upon Microbial Colonization Processes
	Lechleitner, R. (MS)	The Resistance of Three Aquatic Insect Detritivores to Fly Ash Constituents (Co-Chair D.S. Cherry)
1984	Doane, T.R. (PhD)	Comparison of Biomonitoring Techniques for Evaluating Effects of Jet Fuel on Bluegill Sunfish (Lepomis macrochirus) (Co-Chair A.L. Buikema, Jr.)
	Peters, G.T. (MS)	Response of Isonychia bicolor to alkaline pH (Co-Chair D.S. Cherry)
	Pratt, J.R. (PhD)	Export of Species from Sources of Differing Maturity and Complexity

1985	Belanger, S.E. (PhD)	Functional and Pathological Responses of Selected Aquatic Organisms to Chrysotile Asbestos (Co-Chair D.S. Cherry)
	Hartwell, S.I. (PhD)	Validation of Laboratory Versus Field Avoidance Behavior of Schooling Fathead Minnows to Heavy Metal Blends Relative to Acute Toxicity During Long Term Exposure (Co-Chair D.S. Cherry)
	Stewart, P.M. (PhD)	Diatom and Protozoan Community Analysis and Colonization on Artificial Substrates in Lentic Habitats
1986	Doherty, F.G. (PhD)	Aquatic Ecology (Co-Chair D.S. Cherry)
	Farris, J. (PhD)	Assimilative Capacity (Co-Chair D.S. Cherry)
1987	Genter, R.B. (PhD)	Species Interactions in Microbial Communities
	McCormick, P.V. (MS)	Patterns of Microbial Community Development in Isolated Aquatic Systems
1988	Clements, W.H. (PhD)	Community Responses of Aquatic Macroinvertebrates to Heavy Metals in Laboratory and Outdoor Experimental Streams (Co-Chair D.S. Cherry)
	Pontasch, K.W. (PhD)	Multispecies Toxicity Tests Using Indigenous Organisms: Predicting the Effects of Hazardous Materials in Streams
1989	Orvos, D.R. (PhD)	Environmental Risk Assessment of Genetically-Engineered Microorganisms
1990	Comeaux, J. (MS)	Transfer of Genetic Information from GEMs to Indigenous Organisms
	Rifici, L.M. (MS)	Investigation into Three Potential Modifying Factors in Larval Fathead Minnow (Pimephales promelas) Growth and Survival (Co-Chair D.S. Cherry)
	Scanferlato, V.S. (PhD)	Environmental Risk Assessment for Toxic Chemicals and Genetically-Engineered Microorganisms: A Microcosm Approach
1991	Atkinson, R. (PhD)	Rehabilitation of Damaged Ecosystems
	Palmer, S. (MS)	Application of Molecular Biology Techniques of the Assessment of Microbial Community Responses to Environmental Perturbations
1993	Hill, S. (MS)	Evaluation of Seed and Seedling Response to Aid Revegetation of Hazardous Chemical Waste Sites
	Arnegard, M. (MS)	Functional Toxicity Tests in Microcosms
1994	Dobbs, M.P. (PhD)	Ecotoxicology (Co-chair D.S. Cherry)
	Holl, K.D. (PhD)	Vegetation and Lepidopteran Succession on Reclaimed Coal Surface Mines
	Sabre, M. (MS)	Restoration of Disturbed Ecosystems Using Wildflowers
1995	Jones, D. (MS)	Macroinvertebrate Richness and Abundance of Accidental Wetlands on Surface Mines
1996	Comeaux J. (PhD)	Effects of Copper on Benthic Communities in Artificial Microcosms
1997	Heckman, J. (PhD)	Restoring Functional Attributes of Damaged Ecosystems

Post-Doctoral Fellows and Research Associates

Fred Benfield	Dennis T. Burton	S. K. Case*	Donald S. Cherry
J. K. T. Eu*	J. M. Fournier*	H. Fujii*	David Gruber
P. F. Lai*	Kenneth W. Thompson	G. F. Westlake	J. R. Pratt
Kay H. Austin	David R. Orvos	Paul V. McCormick	Robert B. Atkinson

*On NSF grant on which S. P. Almeida was co-principal investigator

Sabbaticals

Rodger H. Green, University of Western Ontario
Roger L. Kaesler, University of Kansas
Jin Hongjun, Nanjing University, China
Rex L. Lowe, Bowling Green State University
Shen Yun-fen, National Academy of Sciences, China

APPENDIX 3
paper from former graduate students

The Graduate Student Era: 1985-1991

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The University Center for Environmental Studies was a somewhat different place in the late 1980s than earlier generations of students may have recalled. While University funding was far less and the total number of graduate students was less, the research conducted continued to span a diverse array of subject areas. Even though money was tight, students were still expected to present results at regional and national meetings. They did! Productivity per student never wavered. John ensured sufficient funding for his students as long as he observed reasonable progress in that student's data collection and analysis efforts. The University Center for Environmental Studies became the University Center for Environmental and Hazardous Materials Studies in 1988 to reflect the broadening of the Center's mission. Students continued to concentrate themselves on the first floor of Derring Hall with Center labs on the first and second floors. The "elder" students who left in 1985-1986 included Paul Stewart, Dick Pratt, Bob Genter, and Paul McCormick. Will Clements was remaining resident of Derring 1027 and was eventually joined by Kurt Pontasch, Keith Sappington, and David Orvos. As always, Bobbie Neiderlehner was there to keep us in line - both personally and professionally!

The graduate student research activities from the mid-1980s to 1991 centered on the examination of anthropogenic stresses to ecosystems. We classify the research into three main categories: environmental risk from release of genetically-engineered microorganisms (primary graduate students were Orvos, Scanferlato, Palmer, and Comeaux), macroinvertebrate responses to xenobiotics (Clements and Pontasch), and wetlands ecology and restoration (Atkinson). Protozoan and ecological impact studies also continued under Bobbie Neiderlehner, Paul McCormick, and Dick Pratt.

The examination of potential effects from genetically engineered microorganisms started in a rather unique location - the kitchen of Clete Sellers in Harrisonburg, Virginia. Sellers was Orvos' Master's advisor at James Madison University and had invited John to give a seminar in the Fall of 1984. After talking with Orvos at length about the promise of biotechnology as well as its potential risks, John proposed the idea of conducting research at Virginia Tech complete with drawings on the back of paper napkins. At the time, Orvos was finishing his fish physiology thesis with a strong interest in cellular and molecular biology but with little knowledge of applied ecology and no knowledge, per se, of risk assessment. However, the initial contact with John Cairns was such that Orvos soon initiated a dialogue that led to his coming to Blacksburg in August, 1985.

At the time, the Center was poorly equipped to initiate such a research program and, with the assistance of Dick Pratt, a Virginia Water Resources Research grant was obtained that provided some equipment and supplies for initial data generation. We soon found out that to conduct GEMs research, it would be useful to have in our possession a GEM! Fortuitously, Orvos' wife, Andrea, was enrolled in a Plant Pathology class whose teaching assistant worked for Dr. George Lacy of Tech's Plant Pathology, Physiology, and Weed Science department. Lacy was willing to work with the UCES and provided his GEM, *Erwinia carotovora*, to us. *Erwinia* is a pectinolytic organism that causes the familiar vegetable soft rot often seen when food is left in the refrigerator too long. Lacy's group had engineered the bacterium so as to remove that part of the genome that produced the pectinolytic proteins. Lacy's excellent track record in phytobacteriology and molecular biology proved to be advantageous to development of GEMs research at Tech. Orvos helped Lacy move into a new state-of-the-art molecular biology laboratory that enabled Orvos and others to pursue GEMs research from both molecular and organismal levels.

Preliminary GEMs research continued through 1986 both at Lacy's and UCES laboratories. Lacy, Cairns, and Orvos wrote several grants. Microcosms were developed using either Pandapos Pond for aquatic microcosms or farming soil for terrestrial microcosms. The fall of 1986 brought two events that would dramatically affect the GEMs research: the awarding of $298,000 in joint research funds by the NSF and EPA

and the awarding of the SETAC Pre-Doctoral Fellowship to Orvos. Funds became available to purchase equipment and supplies as well as fund graduate and undergraduate researchers. Two additional students were brought into George Lacy's lab and three students, Vjera Scanferlato, Sarah Palmer, and Jay Comeaux, eventually joined John's group during 1987-88. While parts of these students research addressed non-GEMs issues, so as to improve their subsequent marketability, all played critical roles at various phases of the project. Scanferlato adapted her microcosm studies to include heavy metal stresses and Palmer published work that examined use of DNA:DNA hybridization in pollutant stress quantification.

The GEMs project progressed very well but did upset many in the biology department and Provost's Office at Tech. "Space wars" and "overhead battles" were common and the administrative headaches of such an interdisciplinary grant often caused the loss of sleep. But, the project moved forward. A post-doctoral research associate, Kay Austin, came on board in the Spring of 1988; Orvos and Lacy traveled to the United Kingdom in 1988 to present a paper; Scanferlato and Orvos took a contingent of four undergraduates to the 1989 American Society for Microbiology meeting to present their results; a total of 13 undergraduates eventually worked on the project; and approximately ten manuscripts resulted from the research. However, the crowning achievement, in John's and our eyes, was when the Virginia Tech Research Office, after surveying all biotechnology-related activities on campus, mentioned that only the Center's research appeared to be seriously competitive with biotechnology-related research programs at other land grant institutions. This, of course, was at a time when Tech administrators continued to decrease the budget and belittle the interdisciplinary approach of the Center. These same administrators were reluctant to acknowledge the findings, but then did find $50,000 to purchase equipment that would benefit all of John's research programs.

Other research areas were as equally important. Macroinvertebrate responses to xenobiotics were investigated by Will Clements and Kurt Pontasch using both field and laboratory approaches. Clements, currently at Colorado State University, came to the Center from Florida State University and examined heavy metal effects on aquatic insects. Clements won the Cunningham Dissertation Fellowship and, after leaving Tech, was awarded the first SETAC Post-Doctoral Fellowship. Pontasch, presently at the University of Northern Iowa, came to Tech from the University of Idaho and examined macroinvertebrate response to complex industrial effluents using a novel paddle-wheel microcosm in his laboratory studies. Both Clements and Pontasch graduated in 1988 and continue to maintain aggressive research programs. Two of Clement's graduate students have been awarded SETAC Pre-Doctoral Fellowships in this decade.

Restoration ecology studies, discussed in more detail elsewhere in this book, took on new vigor at the Center in the Fall of 1988. Rob Atkinson, a friend of Orvos' from James Madison University, took little persuading to come to Tech after visiting in the Spring. Atkinson's long-standing interest in wetlands and desire to explore restoration techniques in non-tidal freshwater systems seemed a natural match to John's desire to once again conduct research in restoration ecology. His career at Tech proved quite successful as his dissertation developed new methods for evaluating created wetlands associated with Section 404 of the Clean Water Act and was the first assessment of nontidal wetland mitigation in Virginia (published in the journal *Wetlands*, a book chapter, and elsewhere). Before graduating, Rob wrote a successful proposal with John to the Department of the Interior to design wetland habitats to be left after surface mining. Atkinson stayed on for another four years conducting that research . He is presently at Christopher Newport University and was recently awarded $700,000 for a study of restoration of an endangered ecosystem: Atlantic White Cedar swamps.

Sarah Palmer is presently on the staff of the University of Arizona where she is responsible for assessing the impact of GEMs releases and rDNA safety issues. Vjera Scanferlato currently lives in Piossasco, Italy where she is involved with a research project investigating bio- and phytoremediation of sediments dredged from the Venice lagoon. David Orvos is at Sweet Briar College and is the founding chairman of the Department of Environmental Studies.

Many professors continue to conduct research in the same general area as they did when they themselves were graduate students. Not John Cairns. If nothing else can be garnished from the text of this chapter, it is John's willingness, actually insistence, on changing research directions "every five years or so" as he would often say that should be clear. We know of no other professor at any time that had the research diversity present in his laboratory as John Cairns did in the late 1980s. While some criticized him for this, those of us who experienced it believed that it strengthened each of us in different ways. All of the students learned from each other, got along well together, and still stay in contact with each other. Who, but John Cairns, would tackle a problem like GEMs or wetlands restoration with minimal equipment and facilities and reap immense success? Who would do it repeatedly throughout his career? How many other advisors put the amount of trust in their students as John did?

One of the superb advantages of working with John was the constant flow of scientists who visited Blacksburg or met with him privately at various national meetings. One of us, Orvos, fondly recalls the pleasure of meeting with and standing in awe of such scientists as Ruth Patrick, Linda Birnbaum, John Harte, Gene Odum, Peter Day, Larry Slobodkin, and Paul Erlich. We doubt if any other program provided so much opportunity for interaction.

Graduate students came and graduates students left Blacksburg throughout this period. The one point of consistency that prevailed was John's right arms: Darla Donald and Bobbie Neiderlehner. We both had the privilege of working with them and firmly believe that if Center students have had success after graduation it was due, in no small part, to the efforts of the both of them to improve our grammar, writing, statistical analysis, lab technique, experimental design, and personalities! We know many others share this belief.

John Cairns, Jr., passed on many things to his students over the years. While his pedagogy, beliefs, and work ethic were passed on to all generations, we both experienced his concern for problems of a personal nature. His insight and understanding into the troubles of Yugoslavia, Scanferlato's home, and the stress of having family there or the health of Orvos' elderly parents meant a great deal to the both of us. Many have seen the professional side of John, but we believe relatively few have seen the personal man.

In finishing this accounting of Center history, one of us, Orvos, reflected on two things. As a scientist for SmithKline Beecham, I was sent in 1995 to a weeklong academic/industrial conference in France which consisted largely of French and U.K. scientists. When they learned that I was educated at the Center, I was left with the distinct impression that the Center had the reputation of being a skyscraper-type structure with a staff of at least dozens, with productivity very appreciated by the Europeans. I did little to dispel the belief, but only wished that someday Virginia Tech would realize what, in fact, they had on that campus. Finally, one of the things that has always impressed me the most about John is his "extended" family. The students, spanning two generations, and now the students of those students. To borrow and modify something I read just this morning in *Chemical & Engineering News* (27 July 1998, page 62), 'As a biologist, one's legacy is not the science that is left behind. It is the students one has trained." If that is an acceptable criteria for success, then John Cairns, Jr., has indeed been among the most successful biologists.

CHAPTER 11

THE UNDERGRADUATE RESEARCH ERA: 1961-1997

My experience with undergraduate research covers a longer time span (36 years) than with graduate student research (31 years). Because research heightened my interest in science, I thought others might have a similar positive reaction. As a consequence, when I had the opportunity to teach a six-week course at Rocky Mountain Biological Laboratory (RMBL) in 1961, I had all 24 students in my course on comparative limnology do a research project. I was warned about the substantial increase in personal time that this requirement would take, particularly if I supervised four National Science Foundation (NSF) undergraduate research participants in addition to teaching the course. The students were all enthusiastic about their projects, even though they knew that their experiments would have a narrow focus and would require a great deal of work for them to finish within a six-week time frame. RMBL was then not well equipped for course research on this scale, particularly in the field of limnology. I borrowed equipment and supplies from the Academy of National Sciences Philadelphia. Fortunately, both Ruth Patrick and the institution were supportive of my effort.

I loaded my Volkswagen microbus for the trip from Philadelphia by placing a board with supports from the motor to the front seat and stuffing all the teaching materials and a tent (for camping on the trip to Colorado) under the board. Personal gear went on top the board and a small aluminum boat was placed on the roof. This set up would all be illegal today, because our youngest daughter Heather was then under 3 and had no restraining device on her seat. In fact, had the microbus turned over, hundreds of pounds of equipment would have landed on us, a thought I had constantly during the trip.

The students finished their research on time, and some presented the results at state academy meetings when they returned to their home institutions. Without exception, they were all more enthusiastic at the end of summer than they were at the beginning because, for practically all of them, this experience was the first research they had done. I did absolutely no personal research that summer. The people who told me how much time the undergraduate research would take outside of normal classroom hours were absolutely right. Also, the students scattered in many different directions, and I spent a great deal of time visiting each research site at least twice during the six weeks. I taught this undergraduate research class in the same manner through summers of 1961, 1962, and 1963.

In summer 1964, I moved to the University of Michigan Biological Station (UMBS) to teach freshwater protozoan ecology. This session was eight weeks long, and I intended to continue the practice of having a research component in the course. Fortunately, the first year's enrollment was under 10 students (my recollection is 8 or 9). Although the type of research was different and the UMBS had all the necessary equipment, I was still challenged because all but one of the students was foreign.

Years later, Dr. Frederick K. Sparrow (then director of UMBS) was able to obtain new microscopes for my class. I surmised that the protective polyurethane foam material that was used in the packing of the microscopes could be anchored near the surface in the lake to constitute an ecological island. The material was inert and provided a reasonably complex surface for colonizing microorganisms associated with substrates. This approach enabled students to observe microbial community dynamics. The research at UMBS also inspired me to follow the publications of Edward O. Wilson, an event that has benefited my professional career greatly (although our interests at that time might have been perceived as enormously different). Most important, the design showed the students that research could be carried out with relatively inexpensive materials—in this case, materials that would have otherwise been discarded. I found it satisfying that the packing material for the microscopes furnished the collecting device for microbial communities to be studied with the microscope. This serendipitous event resulted in many theses and dissertations and many undergraduate presentations at state and regional professional meetings.

Since I did my research in the laboratory where the class was taught and the students also did their research there, they could see the hours I invested, discuss problems with me whenever they chose, and the like. I obtained a better understanding of undergraduates than I would have had opportunity to do on a campus where faculty often appear unapproachable to students, especially outside normal classroom hours. Students told me that it was very important to them to see how I managed my time, although they may not have stated it to me in exactly those terms.

I continued to be involved in supervising undergraduate research for several years after I retired. In fact, my deep involvement with both undergraduate research and graduate student research only ended in 1997 when my last PhD candidate graduated and the last honors students, who had been carrying out research with me and my graduate student, left for graduate school.

Although I have discussed my experiences with undergraduate and graduate students in two separate chapters, both types of students were closely linked. I frequently involved graduate students in the supervision of undergraduate research, when both the undergraduate and the graduate student felt this coupling was a good idea; this extension of graduate student activity helped my professional relationships with them significantly. Issues that might not otherwise have been discussed were raised frequently; the graduate students had experiences they might not otherwise have acquired until their first professional position, and the undergraduates had a role model closer to their own age.

I estimate that, from the period of 1961 through 1997, I supervised, with or without help, approximately 630 undergraduate research projects. The peak number of students at field stations was usually 24 per summer for all but a few summers; and, during the academic year, I always had a minimum of 4 undergraduate honors research students (and once 12) or regular independent undergraduate research students. I have had undergraduates from the early 1960s, as well as in more recent years, tell me at professional meetings how much the research meant to them, although over three decades had passed in some cases. Also, a few former undergraduate research investigators have even sent money to the alumni fund to help support undergraduate research. All this research supervision was an enormous effort and did not qualify as teaching by those who consider stand-up lecturing in a classroom the only sort of teaching that occurs. In my opinion, such supervision is the most valuable sort of teaching, although the number of hours a researcher spends with each student would lead many to believe it is not cost effective. I pity both the faculty and students, both graduate and undergraduate, who cannot enjoy the exhilaration of these activities! The joy of students when their first research project works well, as it did in most cases, was as stimulating to me as good results in my own research. As E. O. Wilson (1998) notes, one can invest 40 hours in a professional position and probably function adequately. An additional 20 hours per week is necessary for some degree of success and a further 20 hours for noteworthy achievement. Dedicating hours does not guarantee any particular level of achievement, but even the most skilled and highly motivated people must work more than 40 hours weekly to pass these thresholds. Some researchers may be exceptions to this arduous work schedule, but not many.

In my opinion, research should begin with students in high school or earlier and, certainly at the very latest, in the undergraduate years. When students thank me for the hours I gave, I tell them that this involvement is a partial payment on an old debt to Professor Robert K. Enders, my advisor at Swarthmore; Professor David Wenrich, my major professor for both the MS and PhD degrees at the University of Pennsylvania; and Dr. Ruth Patrick, who became my mentor at the Academy of Natural Sciences Philadelphia. I hope that some of my students will have the opportunity to enjoy the same experiences I have had with students and that some academic institutions still permit this time-consuming work, regardless of budgetary constraints.

My own conviction is that the most effective form of teaching is solving problems together (e.g., Cairns 1998; see Appendix 4). In this situation of a student and teacher, the student, whether undergraduate or graduate, learns about the process of science, the synthesis of information, the ability to construct a testable hypothesis, and the commitment needed for an above average or exceptional level of professional achievement. This set up requires a significant effort from the teacher; Marston (1977) notes

> the knowledge a faculty member acquires in graduate school is similar to the principal in a bank account. If an individual draws on the principal but has no source of income, the principal is soon gone and bankruptcy follows. Research investigations prevent intellectual bankruptcy and basic research, as a consequence, makes possible competent teaching.

Every professional must engage in some form of continuing education! Physicians, who are burdened with HMO, Medicare, and other forms, as well as a heavy daily practice, must make time to read professional journals; this type of education is also necessary for engineers, geologists, economists, sociologists, statisticians, and the like. Professional, continuing education is absolutely essential for them to stay abreast of the rapidly evolving and expanding basic knowledge and techniques in their respective fields. The professional who does not regularly read the scholarly journals, recent books, and attend professional meetings will soon fall behind colleagues. All competent professionals are crucially dependent on this process of professional renewal. During my entire 57-year career, I have never been able to accomplish this in a 40-hour week, including summers. In fact, during the first two decades of my career, every professional article, book chapter, or book of mine was written evenings, weekends, and holidays.

Human society depends primarily on its universities to both generate new knowledge and to graduate creative, research-oriented students. Corporations have reduced basic research, as have state and federal governments. At the same time, governments in many American state universities have reduced the percentage of funds available to both students and research investigations—extramural funding (i.e., grants and contracts) has become increasingly important. *Intellectual properties* (i.e., patents) is a term now common at universities. Universities will have to adjust to this new, probably durable, shift in emphasis, and the adjustment must be done in a way that will enhance creativity. Many believe that a robust measure of the potential for increasing the number of jobs is related to the availability of venture capital. A country with a substantial debt load, such as the United States, will have less venture capital.

Another important point is that society has placed primary responsibility for the vital process of research and professional renewal on its major comprehensive research universities. These institutions are epicenters for the production and refinement of knowledge and the correction of faulty hypotheses. With all the problems facing the world today, society simply cannot afford to neglect research as it is now doing. Some research titles, even on nationally competitive grants, sound hilarious to the lay person and even to faculty in other areas of specialization. Members of the US Senate and House of Representatives have used these titles to good advantage when assigning such tongue-in-cheek awards as the “golden glove” and in criticizing how federal research money is spent and how some of their institutional support is being wasted. Without doubt, some faculty members waste research money, but then waste in government is not unknown. Surely, the research establishment has done as well as many government programs in terms of wastage. Even wasted research efforts, in the sense that they produce negative results, is not money lost because they identify unprofitable research areas or unprofitable approaches.

I am no longer being paid for carrying out research and am, in fact, using some of my own money to finance my writing, as I have done throughout my career. Passing the process of science to future generations is necessary for society to survive, given the world’s problems today and those likely to be faced in the future. Society cannot leave this problem solving knowledge to future generations without diverting a significant portion of its resources to this end. Accountability in science is exceedingly important, but judging should not come from managers, but from world-class scholars who are best able to identify other creative research investigators necessary for quality control. This responsibility is essential in the profession of science.

During my entire career, I have never taken a sabbatical leave (for the first 18 years, I was not entitled to do so). I have always feared losing research momentum and felt that the professional

renewal could be carried out in other ways. Nevertheless, large blocks of time at the professional's disposal are essential for research in a variety of ways, including reading the professional literature, discussing ideas and concepts with colleagues, and the like. Academic institutions may have to re-evaluate the sabbatical leave in this era of quick travel and electronic communication so that a set of mini-sabbaticals (of which I have taken many) are available; in some ways, these leaves are more effective than an entire year of renewal every seventh year. Absence for an entire year might be hard on one's students. Fortunately, many institutions encourage faculty to take sabbaticals of one or two quarters or one semester because it impacts students and teaching less. Although some members of faculties have been known to use sabbaticals as extended vacations, most are working as hard as normal but are freed from classroom teaching and committee service. Spending full time on research generates a momentum otherwise difficult to achieve.

Although I primarily worked with university undergraduates through 1997, I worked with high school students for my entire career and continue to work with high school students in the 21st century. The most enjoyable has been working with Charles Jervis' class at Auburn High School in Riner, Virginia, USA. I was impressed with their insights into complex problems involving both science and ethics, for example, to what extent are precautionary measures justified to reduce the probability of catastrophic climate change? Because all my travel has been restricted due to four spinal compression fractures, I never actually met either Jervis or his students. Still, we managed a very stimulating exchange of ideas via email and the Internet. For me, the major benefit is knowing that the next generation of citizens will have people like this class. Arrangements have been made to continue this relationship during the coming school year. I am reassured by the ability of the class to combine robust science with personal ethics and societal value judgments. I am still deeply concerned about the future of humankind and the planet's biospheric life support system, but I sleep more soundly because I had the good fortune to interact with these high school students and their teacher.

I find some problems with the current academic environment that are unsettling. Binge drinking by students, who are supported financially by their parents and society, seems irrational since the students are presumably in college to improve their minds. Additionally, life and career threatening behaviors are associated with drunkenness, such as drunken driving, AIDS, and inattention to dangerous situations. Not unimportant is the inability to attend or appreciate class the next day.

A close second to this primary concern is the increasingly common belief of students that class attendance is optional—as one student remarked, like buying a ticket for a football game. Faculty members are regarded by students as paid performers, not intellectual guides. This view denigrates both the structural and reasoning components of the educational process. Even if faculty members are merely performers, a listless audience is not likely to elicit the best in the teacher's performance.

Related to the first two concerns is a markedly diminished sense of student responsibility. Some years ago, a major donor provided money for academic fellowships with the condition that the awards be presented to the students at a ceremony—no student appearance, no fellowship (I am confident that illness or a death in the family, etc. would have been a mitigating circumstance). Some students viewed this requirement as unfair. As a student of another era, I would have viewed the award as a public acknowledgment that I would perform to the best of my ability, as the donor had the right to expect a fellowship recipient to do. My last grant provided funds for two graduate research assistantships, and a prospective graduate student asked, "Like, are there any responsibilities?" When informed that the sponsor expected quarterly reports showing significant efforts, the student was stunned. I was fortunate that practically all my students, both graduate and undergraduate, whose work depended on extramural funding had a well developed sense of responsibility. I believe that this attribute is not as widespread as one might reasonably hope it to be.

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APPENDIX 4
letter from undergraduate student

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6/29/98

Dr. John Cairns, Jr.
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Dr. Cairns:

Thank you very much for your letter. It was interesting to read the draft of your book chapter "The Undergraduate Research Era: 1961-1997." From the limited view that I have, I didn't see any inaccuracies or anything that was difficult to understand. I only have one additional idea. In my case it was not only applied science experience (i.e., lab and field work, etc.) that I gained from working with you and Heckman. Indeed, the more scientific aspects of research I had largely already learned from lab classes and previous commercial lab experience. What I did learn as a member of your lab that I feel like I could have never gotten from a book or class was in-depth training on the way that scientific interaction and scientific communication works.

Before entering your lab I had no understanding about the role of scientific journals, scientific meetings or grant funding to the progress of science. I had no idea whatsoever about what it really took to carry out science beyond the simple steps of putting reagents in test-tubes, balancing chemical equations or counting cells under a microscope. However, when I arrived in your lab, Heckman had me writing a grant proposal within the week. When I didn't win that one, I tried again, and as you know, by the time I left there I had personally won three grants and had helped John get money from the Debris Landfill to help pay me for the last summer I was there. Most new graduate students have not applied for any grants before they enter graduate school. Furthermore, the vital importance of the communication of scientific work (i.e., publications) was NEVER DISCUSSED in any of the classes that I took at VPI, Portland State or University of Georgia. Without your own advice on the subject, I might have learned the lesson about publications the hard way when I looked for my first job.

I hope that this is of some help. It is difficult for me to comment further because I was, as you mentioned, only in the lab during an atypical time period of your career of teaching undergraduates. Good luck with the book.

Sincerely,

signed

Andrew Heaton

CHAPTER 12

EMBRACING TRANSDISCIPLINARITY

When I went to work with Ruth Patrick in 1948, she had been criticized for three things: (1) going beyond disciplinary boundaries, (2) accepting problems defined by society rather than by the academic community, and (3) using a team approach to study system-level problems. The chief academic criticism was that human life was too short to master everything within a single discipline and that going beyond a single discipline was irresponsible until one had complete mastery of it. This line of reasoning is actually relatively recent; in Leonardo da Vinci's day, scholars were admired when their intellect was challenged by a diverse array of subjects. Furthermore, the normal atmosphere was for those with different academic interests to exchange ideas; in addition, a scholar not doing so would be regarded as overly specialized, provincial, and narrow. This situation, of course, was during a period when learned people were scarce and higher education was the privilege of a relatively few people. As education became more egalitarian and the numbers of professors grew, the tendency was to move away from being a generalist toward specialization because the number of people with whom one could communicate was limited by time, life span, etc. Inevitably, this wish to keep abreast of developments in one's own area of specialization and the sanctions that accompanied not doing so overrode the curiosity about other areas of interest. Rites of passage (i.e., acquisition of the PhD, promotion and tenure, acquisition of extramural funding, and acceptance of a manuscript by a professional journal, to mention a few of the isolating mechanisms) depended upon mastery of the tribal language and customs, and survival depended on sufficient specialization to take advantage of a resource less available to non-specialists. In this way, scientists are not different from Darwin's finches, i.e., competition for limited resources was reduced by specialization and the consequent resource partitioning.

When I arrived at Virginia Polytechnic Institute and State University in 1968, some faculty members boasted of being theoretical, despite the university's motto "that we may serve." I found it astonishing that criticism of "applied" research existed in a land-grant university. I was, of course, encountering the same resistance that Patrick had encountered when accepting funding for solving a societal problem (environmental pollution) because the problem was defined differently than it would have been had it originated within a discipline. Panels or committees within a discipline chose which research should be funded, and, generally, research that best fit the disciplinary paradigm then in vogue was successful. However, entities with urgent problems care very little about disciplinary boundaries; they are seeking solutions to their problems.

The paucity of transdisciplinarity will become a critical issue of the first half of the 21st century. All the world's major problems (e.g., global climate change, sustainable use of the planet, population stabilization, resource allocation, carrying capacity) transcend the capabilities of any single discipline. Arguably, all issues and problems associated with sustainable use of the planet require transdisciplinarity. System-level studies are commonly referred to as "top-down," i.e., the system-level strategy has a major influence on the selection of components for detailed study. In the "bottom-up" approach, a judgment is made on which components (i.e., disciplines and/or subdisciplines) are studied in detail, and then the connections that make up the system are given serious attention. Clearly, system-level research would be more effective if top-down or bottom-up strategies were orchestrated from the outset.

To reach this desirable state, the first step is to stop "thinking in a box," i.e., begin to think of the well being of the system instead of disciplinary boundaries. This approach will require an enormous expansion of both spatial and temporal views of the planet's biospheric life support system, including its carrying capacity for humankind, both present and future generations. Progress toward sustainable use of the planet requires shared knowledge, which is seriously impaired by the present paucity of exchange among the disciplines. Progress is also hampered by discrediting of science when the evidence conflicts with political ideologies (e.g., global warming and climate change). Multidimensional thinking should place humankind on the path toward

sustainability. I will probably not live long enough to see this change well underway, but I am persuaded that the human species is capable of adapting to the quest for sustainability.

The evolution of reductionist thinking and the reemergence of multidisciplinarity have followed identifiable lines: (1) multidimensional thinking – hunter/gatherer era, (2) concentration on technology – agricultural and industrial revolutions, (3) fragmentation into disciplines – reductionist science era, (4) appearance of multidisciplinary "teams" – traditional disciplines study a single problem with little or no interaction during the study, (5) appearance of interdisciplinary teams/focus on a single problem: interactions between and among disciplines facilitates mid-course corrections that improve results, (6) emergence of multidimensional, transdisciplinary individuals (e.g., Lester R. Brown) and organizations (e.g., Earth Policy Institute) – present challenge is to develop a multidimensional perspective on both of humankind's life support systems, ecological and technological.

At the beginning of the 21st century, the technological/economic system appears to be thriving, but at the expense of the ecological system. However, the technological/economic system cannot survive without the resources and services of the ecological system. This challenge is unique in human history and will require a rate of social evolution much more rapid than in recent times.

CHAPTER 13

LEAVING A HABITABLE PLANET FOR OUR DESCENDANTS: THE QUEST FOR SUSTAINABILITY

By the time I had finished my first major research project in 1948, my primary professional goal was to develop the means for protecting ecosystems from the damage I had viewed that summer and then rehabilitating them when they had been damaged. Although many of my colleagues over the years have viewed my research activities as disparate, I have viewed the study of stressed ecosystems as a unifying theme. The presumed discontinuity comes from the present structure of science, i.e., ecotoxicologists and traditional ecologists still rarely communicate regularly, although this lack of communication is changing. Even so, in the early years of my career, I had to publish in a variety of journals because no one journal was dedicated to publishing transdisciplinary work. My colleagues accused me of having no fixed objective in my research and believed that this variety suggested a grasshopper approach, leaping from one focal point to another. I have also been criticized for not having enough "hard science" publications—that is, those articles essentially preoccupied with data. However, over half of my journal articles are in the "hard data" category (over 250 journal articles). What apparently bothers others in science is the diversity of subjects in my publications: ecotoxicology, ecological restoration, microbial community structure and function, ethics, and sustainable use of the planet. I view these as interconnected by the theme "use without abuse of natural systems." However, the main problem may be that the articles are written so that non-specialists can understand them. Diamond's (1997) article on Carl Sagan emphasizes that Sagan lost potential membership in the US National Academy of Sciences not because he failed to produce sufficient important scientific research but because he had too much success as a popularizer of that research. As Diamond suggests, rejection of Sagan by other scientists appears incomprehensible because they themselves continually advocate that the public's understanding of science be improved. Yet, a scientist successful in improving these communications is paradoxically and frequently faced with hostility, penalties, and the like. Unquestionably, this situation is changing; for example, the Aldo Leopold Traineeship of the Ecological Society of America is focused on increasing communications abilities of ecological scientists and has been quite popular.

My awareness of the difficulties of moving from one disciplinary group to another was heightened when I served on a National Research Council committee in 1977 (Gloyna et al., 1977). The committee considered, among other things, the possibility of effectively re-educating individuals from other, somewhat related disciplines to serve as professionals in the water quality field. However, even professionals then unemployed were reluctant to make such a move because they feared being permanently excluded from their original field. After this experience, I wrote "Academic Blocks to Assessing Environmental Impact of Water Supply Alternatives" (Cairns, 1979) as a chapter in the *Thames/Potomac Seminars*. The next year (May 13, 1980), I spoke on this issue in the lecture "Suppression of Creativity in Academe" at "The Not Your Average Lectures Series" organized by the students of my own institution. I discussed the unfortunate consequences for young faculty members and students who ventured outside of their disciplines too early. I had another opportunity to draw attention to this subject when my home institution's president James McComas launched a series named "President's Symposium," which focused on concerns for large societal problems. *Environmental Literacy and Beyond* was produced from the symposium (Wallace et al., 1993). In my chapter in the volume ("Intellectual Electric Fence"), I noted that people who stayed within a discipline with a very strong specialization should not impede scientists who attempted to examine the larger system, a sine qua non for environmental problem solving. Another attempt to explore this issue is my article "Communication and Status: The Dilemma of an Environmental Scientist" (Cairns, 1993), which was reprinted by permission in two other sources after it first appeared in *Speculations in Science and Technology*. All these publications resulted in correspondence, telephone calls, and exchanges at professional meetings from people who feared the consequences of straying beyond disciplinary boundaries, but who felt powerless to correct the

situation. Diamond's article (1997) made a deep impression on me because it illustrated that, if someone as famous as Carl Sagan could be singled out, then what protection does an untenured assistant professor or a professional in industry or government at early stages in career development have? Wilson's (1998) volume, *Consilience: The Unity of Knowledge*, provides hope that disciplinary boundaries can be surmounted without damaging their quality control systems. Aldo Leopold (1966, p. 197) states:

> One of the penalties of an ecological education is that one lives alone in a world of wounds ... An ecologist must either harden his shell and make believe that the consequences of science are none of his business, or he must be the doctor who sees the marks of death in a community that believes itself well and does not want to be told otherwise.

The turning point for me in making the decision to devote much of the remainder of my career to sustainability issues occurred at the first Abel Wolman Distinguished Lecture given at the National Academy of Sciences. In a lecture entitled "Ethos, Equity, and the Water Resource," Luna Leopold (1990) discussed each agency acting as if "it were the only flower facing the sun" and deplored the compartmentalization in organizations and disciplines. I reasoned that, if a distinguished scientist and the son of Aldo Leopold could focus on these issues at that stage in his career, I was obligated to do what I could in another context that faces the same problems of compartmentalization and reductionism.

The dichotomy in thinking about sustainability is between those who believe in infinitely replaceable resources (e.g., Julian Simon in Myers and Simon, 1994) and those who believe in a large pool of recolonizing, globally distributed species.[1] The latter point of view assumes that humans are dependent on a biospheric life support system and that they cannot manipulate everything for their own needs. Sagan (1994) calls attention to humankind's curious belief that the universe was made especially for it. The most plausible explanation is that the self-esteem of humans is so fragile that only a custom-made universe will do. Humankind's actions support this view: (1) humans are taking essential habitat from a huge number of species, (2) humans co-opt a huge proportion of the planet's resources, leaving the 30+ million other species with less resources each year, (3) toxic chemical substances reduce the ecological value of habitats far distant from the place where they were used, (4) facilitating invasions of alien species without their biological controls causes great ecological disequilibrium. The evidence for infinitely replaceable resources, given sufficient economic incentives, fits the free market concept of willingness to pay, but ignores a number of issues I consider important. An illustrative list of these issues follows.

(1) Even if humankind could manipulate the planet and the solar system, and perhaps even the universe, so as to achieve sustainability by sequential substitution of resources, implicit in this belief is the right of humans to deprive other species of resources necessary for their existence. Thus, this idea is not, for me, a guiding belief; and, even if it were possible, I would reject it.

(2) If the present generation depletes resources during its lifetime, leaving the next generation to develop substitutions, does this action show a lack of compassion for future generations or does it show a blind faith in the ingenuity of future generations? One billion people go to bed hungry nightly, and two billion live on an income that would cause most American citizens to riot. These two facts should at least indicate that problems exist with carrying capacity and infinite substitutability of resources.

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[1] The MacArthur/Wilson (1963) equilibrium model demonstrates that, when a particular species disappears from a particular community or locale, it is replaced from a pool of species better suited to that particular habitat at that particular time. For species with a cosmopolitan distribution, a replacement species is always available. One might reasonably consider this a renewable resource situation. For species with a much more restricted distribution, the loss may well be permanent for that particular locale. Biological impoverishment will markedly reduce the number of potential colonizing species. The more of the latter that are available, the greater the resilience of natural systems. However, natural capital is finite on a finite planet and natural resources are not infinitely replaceable. This is the main obstacle to a rapprochement between economists and most ecologists.

Although I have published many articles based on the continued use of natural resources with the clear view that continued use would not be possible if natural resources were damaged, my most important article (Cairns, 1997) on this concept came about purely by chance. Peter Raven, Director of the Missouri Botanical Garden and then Home Secretary for the U.S. National Academy of Sciences, invited me to participate in a small workshop on sustainability in February 1997 in Wisconsin. Accompanying the invitation was a four-page, undated, publisher-unspecified description of the Natural Step Program, originated by Karl-Henrik Robèrt and his colleagues. I was horrified that I had not even been aware of this splendid development and was intrigued by the four conditions stated in that four-page leaflet. One important feature of the Natural Step Program was reaching a consensus on the conditions for sustainability. What, I wondered, would happen if someone produced a set of conditions likely to prove effective in preserving the biospheric life support system and its services upon which human society depends, even though a consensus at this time would be clearly unachievable? The four conditions seemed so self-evident that I would almost describe them as platitudinous to someone moderately literate on environmental problems. However, I was astonished that Robèrt and his colleagues had achieved a consensus even on these issues, given the anti-environmental backlash described by Ehrlich and Ehrlich (1996) or the ecological denial so beautifully described by Orr and Ehrenfeld (1995). Robèrt, and a number of others whom I would dearly have loved to discuss these matters with, would be present at the meeting. However, health problems (blood clots in deep veins of one leg and asthma that was precipitated by exposure to second-hand cigarette smoke) precluded any long-distance travel for me.

I pondered the concepts and decided that the consensus conditions, while a splendid start, were far short of what was needed. I immediately produced a list of eight conditions, including the existing four. I became almost obsessed with this task and by May had produced a list of illustrative goals and conditions that would have to be addressed if a sustainability strategy were to be developed. Fortunately, I had a number of invitations from nearby areas to teach and speak, which permitted me to test these ideas on a variety of interest groups. The first was an Elderhostel at The Mountain Conference Center in Highlands, North Carolina; followed by a regional Phi Beta Kappa luncheon talk; a banquet address for the local society of Sigma Xi; a convocation address at Roanoke College, Salem, Virginia; an address to over 200 high school students for a regional meeting sponsored by the Society for Sigma Xi; a lunchtime seminar talk for a group of businesspersons in Roanoke, Virginia, sponsored by my university's regional graduate school in that city; a seminar on campus in an urban and regional planning class; and a talk at an economics seminar. The response was reassuring, although most of these people had not even thought about problems of sustainable use of the planet, which were now my major preoccupation. The response of the high school students to the talk by a person worried about their future was touching. In fact, the 10-minute question period at the end of the talk was extended by 40 minutes because of school bus schedule changes. Even with this extended time, the students were still asking questions and discussing various issues when the buses arrived.

Service on an award committee for selecting a person who had achieved excellence in implementing sustainable practices in industry made me aware of the industrial ecology publications of Graedel and Allenby (1995), Graedel (1999) and Tibbs (1992). My exposure to the Natural Step Program made me belatedly aware of Hawken's superb book *The Ecology of Commerce* (Hawken, 1993).

My doubts that mainstream science, engineering, and economics could achieve transdisciplinarity were dispelled by service on a number of National Research Council (the operating arm of the U.S. National Academy of Sciences and Engineering) committees. The committees consisted of individuals who had achieved notable success in their specialties, but this success did not impede their ability to collaborate with other disciplines. I was fortunate to chair one 15-person committee that enthusiastically and skillfully connected science, technology, and public policy (National Research Council, 1992). The volume produced by the committee recommended that the rate of aquatic ecosystem restoration exceed the rate of damage. The target date of 2010 for the first stage of this process seemed quite reasonable in 1992. With only five years remaining, the target date will probably not be met. Since the publication provided both numerous case histories

and literature citations to document that the science and technology *of that time* were adequate to implement the recommendations, it is a great disappointment to me that a wealthy and scientifically and technologically advanced country such as the United States has done so little to repair ecological damage that humankind has perpetrated.

Arguably, the greatest threat to the security of humankind is an alteration in the functioning of the biospheric life support system so that conditions are less favorable, or even unfavorable, to the human species. Polls indicate that most American citizens want clean air and water and greatly reduced hazardous chemical substances in their environment. However, this aspiration has not markedly affected public policy. Earth's environment, at present so favorable to humans, will not remain so for the entire estimated 15 billion years until the sun dies. Consequently, continued serious damage to the biospheric life support system upon which humankind depends is stupid. If humans are unwilling to protect their life support system, then perhaps intelligence, as presently defined, does not have as much survival value as once thought. However daunting the obstacles, humans can still achieve long-term sustainable use of the planet if unsustainable practices cease.

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CHAPTER 14

MY GUARDIAN SPIRITS

My major social values originated with my parents, religion, and the great philosophers. Many professional mentors were and are very important to me. Ruth Patrick has been my continuing mentor for my entire professional career from 1948 to the present. Robert K. Enders of Swarthmore College encouraged an apprehensive returning veteran from World War II and continued to provide guidance throughout his life. David Wenrich chaired my MS and PhD committees and introduced me to Ruth Patrick. G. Evelyn Hutchinson introduced me to the joys of ecosystem-level thinking. Abel Wolman demonstrated the satisfaction that can be derived from combining theoretical and practical research. Otto Kinne encouraged my interest in eco-ethics and sustainability ethics, which is currently my major professional interest. I could name others, including my graduate students, who collectively comprise my guardian spirits. In life's defining moments and periods of severe stress, these people are a source of comfort and inspiration. I am well aware that my guardian spirits construct is almost certainly the result of the cumulative impact of each on my life. This realization does not diminish their value.

My spouse and companion Jean shared with me the peace that comes from association with natural (or naturalistic) systems. For over 60 years, we scheduled a daily association with nature. Even reviewers who made few comments urged that I say more about Jean. One reviewer suggested it would be especially important to give credit to those who made the careers of younger females possible, particularly since it was not typical for her to have her own career, even though, as a biochemist, she most certainly could have had one. I start with the dedication (reprinted with the kind permission of Professor Otto Kinne, Editor-in-Chief) to Jean in *Eco-Ethics and Sustainability Ethics* (Cairns 2003).

DEDICATION

This book is dedicated to Jean, the blithe spirit, who graces my life.

Jean enriched my life in so many ways that it would be difficult to enumerate them. However, one of the most important things is that she has helped me keep my sense of perspective. For 60 years she has called me 'Johnny,' but when she was about to give me an important message she prefaced it with 'Cairns.' If I happened to be complaining about the obstacles to the completion of a manuscript, she would say 'Cairns, stop whining and finish the manuscript!' When I related to her some environmental degradation that I found shocking and that perturbed me greatly, she would say 'Look at that hummingbird on that beautiful flower.'

An important help with my perspective was the continual reminder that resting on one's laurels at any age was simply not a good idea. In June 2001, our daughter Karen was telling Jean about an award that had been given to me. Even on medication to alleviate Alzheimers and Parkinsons, Jean rose to the occasion: 'Big deal,' she said. Her comments through the years were not said disparagingly, but lovingly, so that I would not be diverted from the research presently being carried out. It was acceptable to feel joy in awards and to express satisfaction that one's peers thought the work fit to receive recognition; it was not acceptable to stop or even markedly reduce the research that was a major source of joy.

In contrast, when Jean married me, and even when I was courting her, her support was unwavering, firm, and unconditional. My undergraduate grades were barely adequate, and I had no idea whatsoever of the type of career that would interest me on a sustained basis. Then WWII came, interrupted my education, and put me at some risk.

Jean's firm support and faith in me never wavered after the war when my applications to a long list of undergraduate academic institutions were rejected. When I was accepted at

Swarthmore College and had to commute to classes (usually a total of two or more hours daily), and then study until late at night, she never complained. Graduate school came next and further postponement of having our own house and the other amenities that most of our peers were enjoying. In short, when my academic prospects were dim, Jean supported me in every way.

When I became enthralled with studying ecotoxicology, and, ultimately, restoration of damaged ecosystems (which, in the middle of the last century, were both far from mainstream science), her comments were invariably supportive. Acquiring academic recognition took a long time, as it should, but Jean's stance remained constant throughout this period. When the awards and honors finally began to arrive, her stance immediately shifted so that I would not let them go to my head. Never once did I feel she was unappreciative or was neglecting any of my achievements, but, rather, she was furnishing the type of partnership I needed at each stage of my career. She never criticized or complained when some of my peers got recognition that I had not yet received, and, when recognition did come, she made sure that it did not distort my perspective.

When I thought it would be interesting to accept an unpaid summer position offered me at Rocky Mountain Biological Laboratory by my former advisor Robert Enders (food and lodging were provided), she was enthusiastic. She did not complain that the unpaid leave from the Academy of Natural Sciences reduced our annual income by nearly 20%.

In 1966 I resigned from a secure position at the Academy of Natural Sciences to venture into University teaching and research, a move which took us from family and friends in Philadelphia. Not only did Jean not complain, she encouraged me to do this. When research space failed to materialize at the University of Kansas, Jean neither reproached me for not demanding the space promised nor did she complain about leaving Pennsylvania for an unviable situation.

Another aspect of Jean's enrichment of my life deserves comparable attention. As is the case with most people who carry out research of any kind, I tended to get very intense, sometimes at what would have been unacceptable levels had Jean not diverted me. She is the one who made it attractive to exercise daily by walking in the woods, folk dancing, swimming, and the like. For 57 years, she ensured that we frequently got to classical music concerts, plays, lectures on subjects that I might easily have passed up, and a variety of other activities.

Jean enlarged my perspective on life. She connected me to the wider world and showed me its beauty. It is all too easy to become narrow in vision when one lives within academia, seeing only the geography and landscape of this specialized land, with its own culture, and speaking only its language. Jean has been my link to everything beyond this horizon: to people, other cultures, and other languages. Her interests covered a variety of topics—music, books, social and political activism, social justice issues. One of Jean’s greatest gifts was her perception of the natural world. For her, there was a language with which to converse with clouds, birds, animals, mosses, fungi, and even rocks. She shared with me her personal gift of direct experience with nature. The sense of being ‘completed’ by her is the loss I feel most deeply now.

Sometime in early 1998, I began to realize that her mental function was significantly impaired. Her memory loss had been noticeable during 1997 to our children and was cause for concern to them. However, because the changes were incremental, and possibly because of denial, I did not realize that these were more than usual for a normal aging process. When it was clear that Jean had dementia and concomitantly less energy, I naturally stayed with her all the time. For a while, I could leave her at our home on Bishop Road in Blacksburg, Virginia, where we had lived for approximately 30 years. Since the area was isolated and difficult to access, I eventually moved us to a townhouse in a retirement village. Shortly after the move, a neighbor reported that, when I went into my office on campus, Jean would wander around the parking lot looking for me. From that day on, in spring of 2000, I took her with me to the office and decreased the number of times that I went. Fortunately, my colleagues Darla Donald and Bobbie Niederlehner had worked with me for such a long time and had so much sympathy with my situation that the work flow continued unabated, although I would have sacrificed writing if Jean had needed more attention.

The writing provided peace of mind for me for a long time, because I could dictate or write while Jean dozed. Now that Jean is in the nursing home in the same retirement village, I have more time for writing. Jean and I have been together for so long (approximately 61 years) that I can accurately predict what she would say to me in almost every situation. In addition, the habits I acquired under her tutelage, such as music, plays, and the like, remain.

Writing has been a continual source of serenity. Such concentration, while not precisely the same as meditation, serves the same purpose. Writing was a solace available at all times and, together with the other forms of support, really essential to maintaining my emotional stability.

Sometimes the veil of Jean's dementia lifts fleetingly, and I glimpse the blithe spirit I love and cherish. With her eyes twinkling, Jean will deliver one sentence with her usual wry humor before the veil of dementia descends. The humor is always in context and both funny and loving. The effort that this touch with reality requires is known only to Jean. For me, seeing her as she was for most of our lives is an unexpected joy because it happens with no warning. Even when this return to me disappears completely, the bond between our spirits will be strong because it was forged over so many years. At present, for all too brief flashes, I glimpse what we shared and feel a surge of joy.

I am well aware that even the relationship we now have is temporary. But what a joy that Jean shared so much of her life with me!

I was able to give the book to Jean, although I am not clear whether she realized what it was. Jean died February 21, 2005, and six months later I am still adjusting to a world without her. For most of her life, she worked to improve the lives of others. In her eulogy for Jean, Reverend Christine Brownlie noted that, despite trappings of a husband, children, and home, Jean was definitely not the domestic type. She was an activist in many causes: civil rights, fair housing, integration, the League of Women Voters, Head Start, banning the bomb, and many others. Our houses were all small and easily cared for, and our meals were simple but nutritious. We had few material possessions, but lived almost entirely close to nature. We never took a typical vacation—how could we surpass the splendor of field stations? We took "mini-vacation" whenever the opportunity arose. We were partners in the sense that each of us encouraged the other to live a creative life. The children enjoyed the opportunities to live in small, academic communities with children their own ages, and, even when they were on their own, they often visited us at field stations.

In retrospect, the few material possessions, living for most of our lives in the woods with no grass to mow, and simple but cozy houses provided much time for creative activities that we enjoyed. Despite our personal interests, we spend much time together.

Looking at the world through Jean's eyes transformed the world into a magic place. She took delight in tiny alpine plants that one had to examine from a few inches away or using a magnifying glass. View from the tops of mountains far from most people was well worth the long strenuous hikes. I have a picture of Jean taking part in my Elderhostel class encirclement of a large tree in the mountains not far from Highlands, North Carolina—her face was alive with joy! This was not a rare occasion; this was the way Jean saw the world.

When Alzheimers had a major impact on Jean, I had looked at the world through her eyes and so I could point things out to her. Now that she is gone, I can see the world through her eyes, but not as vividly. I was blessed to have a companion and guardian spirit who filled my life with joy. Our children shared this joy. I am pained to witness the damage done to natural systems in the last half of the 20th century, which is increasing in the 21st century. I can barely control the panic I feel from humankind's assault on nature. Since humans are a part of nature and are not apart from it, severe consequences from the assault will be heaped on human society.

In the United States, the term *pro-life* invariably refers to protecting a human fetus. To me, pro-life refers to all life, not just one life of one species. Since we know that individual organisms die, it is the life force (i.e., evolutionary processes) that is sacred, although individual organisms deserve respect and compassion. Robust evidence indicates that the life force has survived five great extinctions, so it is likely to survive the sixth, although individual species (e.g., *Homo sapiens*) may not. Since anthropocentric alterations of the biosphere (e.g., greenhouse gases) are increasing at a rate unprecedented in human history, the probability is increasing that one or more ecological tipping points will be reached in the 21st century (e.g., a major shift in global climate). While humankind may aspire to live sustainably, no assurance is forthcoming that it will do so. I am confident that my guardian spirits would be saddened by the disappearance of *Homo sapiens*. I am equally confident that they would rejoice in the continuation of the life force. When I am confronted by evidence of severe human disturbance of and damage to other life forms and their habitat, my guardian spirits comfort me. Earth and its sun may last another 15 billion years, during which a diverse array of other life forms should emerge.

Not until the beginning of the 21st century did my environmental panic level increase dramatically. Many effects of anthropogenic biospheric degradation, such as melting of glaciers and ice packs, appear to be driving many systems toward ecological tipping points at a rapid pace. Gone is the comforting illusion that humankind has ample time for the transition from unsustainable to sustainable practices. A new ecological equilibrium favorable to *Homo* sapiens will probably not be reached in Earth's biospheric life support system in a time span favorable to human society. Worse yet, the new biospheric equilibrium is unlikely to be as well suited to humankind as the one in which the species evolved. The new state could even be a hostile environment with greatly reduced natural capital and the ecosystem services that it provides. Of course, both natural capital and ecosystem services will be useful to some species, but not necessarily *Homo sapiens*.

Biospheric disequilibrium may even occur in my lifetime. Still, I feel it is imperative to make a continuing personal contribution to humankind's quest for sustainable use of the planet, and I am persuaded that my guardian spirits agree.

Positive factors are present: the Internet and organizations such as Eco-Ethics International Union provide both the means for rapid information exchange and the opportunity for the development of a global set of values (e.g., eco-ethics and sustainability ethics). Neither of these entities is under substantive pressure to conform to a particular political ideology as are many of the major sources of news. Arguably, the most important step is to acknowledge that, on a finite planet, neither resource consumption nor human population can increase exponentially forever. The United States and other wealthy nations cannot continue disproportionate use of the world's resources if the world's least fortunate nation-states are to be persuaded to join the quest for sustainable use of the planet.

The assault on science by political ideology has intensified in the 21st century. One of the most virulent, and successful, efforts to denigrate science and reason by political ideologies is repetition of charges such as scientific uncertainty. No matter how much additional evidence supports a hypothesis (e.g., greenhouse gases cause global warming), the repetition and type of charges remain constant. The academic community responds by elaborating on the evidence already accepted by mainstream science. This technique worked well for Adolph Hitler in Germany and Senator Joseph McCarthy in the US. Even though this method worked quite well short term, history documents the long-term consequences.

Human history has seen many centers of science—some in locations that seem unlikely in present times. Scientific "renewal" is merely the continuation of a struggle between science and ideology that has been continuous globally. Those societies that permit ideology to dominate science will ultimately collapse, and others, which revere scientific freedom, will become ascendant. In most of the world, scientific freedom is a tenuous proposition, but it somehow manages to survive. I hope that the antiscience trend has gone as far as it can go.

To believe that humankind can achieve sustainable use of the planet requires belief in the essential goodness of the majority of humans. The present state of the world could easily discourage people from supporting this view. However, the prospect of leaving an uninhabitable planet is so appalling that the thought may bring out the best in people. At present, however, global environmental literacy is not adequate to appreciate the consequences of unsustainable practices. Only two alternatives exist: (1) do nothing and hope for the best and (2) do everything possible to avoid major ecological tipping points that will result in biospheric disequilibrium. I choose the second alternative, and I am confident that my guardian spirits support this decision.

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CHAPTER 15

ACADEMIC AWARDS: ARE THEY EVER TOO LATE?

In late January 2001, Bill Lewis, then President of the American Society for Limnology and Oceanography, informed me that I would be receiving the Society's Ruth Patrick Award on February 12, 2001, at the Society's annual meeting. Receiving an award from a prestigious professional society is always a highly emotional experience. Furthermore, to receive an award established to honor one's mentor is the ultimate such experience!

As I tried to sort out my feelings, I suddenly remembered a remark made by a younger colleague when I was elected to the U.S. National Academy of Sciences a decade earlier. I was then already 67 years old, two years past the date when I was eligible for full retirement benefits in the Commonwealth of Virginia's system for state employees, including faculty. The colleague remarked, "Too bad they didn't elect you to National Academy membership while you are still young enough for it to do you some good." Another colleague present at that time immediately remarked, "I would be delighted to be elected to the National Academy at any age!" My feeling precisely! When told that I was to receive the Ruth Patrick Award, I was rapidly approaching my 78th birthday and the news did me a world of good! So did the Sustained Achievement Award from the Renewable Natural Resources Foundation in November 2001 and election to Honorary Membership (the highest honor) in the World Innovation Foundation in July 2004. These three awards have special meaning to me.

Membership in the National Academy of Sciences and the American Philosophical Society were the most gratifying because these two, elite, professional societies had just begun to recognize environmental studies as a component of science and philosophy. In addition, although I have always felt joy in my chosen profession, I was comforted to know that these two organizations, which I have always revered, felt that my research and publications had value.

Since I had witnessed the problems faced by a woman scientist in the 1940s and 1950s on a daily basis, becoming a Fellow in the Association for Women in Science "for having demonstrated exemplary commitment to the achievement of equity for women in science and technology" brought tears to my eyes—I had made a small repayment on the huge indebtedness to my mentor.

Many of the honors and awards I have received are memorable because of the high regard I have had for colleagues who had already received the honor: (1) Research Associate, Academy of Natural Sciences, (2) Fellow, American Association for the Advancement of Science, (3) University Distinguished Professor of Environmental Biology, Virginia Polytechnic Institute and State University, (4) Superior Achievement Award, U.S. Environmental Protection Agency, (5) President, American Microscopical Society, (6) Founders' Award, Society for Environmental Toxicology and Chemistry, (7) Icko Iben Award for Interdisciplinary Research, American Water Resources Association, (8) Fellow, American Academy of Arts and Sciences, (9) Award of Excellence, American Fisheries Society, (10) Foreign Member, The Linnean Society of London, (11) Distinguished Service Award, American Institute of Biological Sciences, (12) Fellow and Founding Member, Eco-Ethics International Union, (13) Sustained Achievement Award, Renewable Natural Resources Foundation, (14) Member, International Ecology Institute, (15) Morrison Medal for Outstanding Accomplishments in the Environmental Sciences, (16) United Nations Environmental Programme Medal for unique and significant contributions to Environmental Restoration and Sustainability, (17) Life Achievement Award in Science from the Commonwealth of Virginia and the Science Museum of Virginia, (18) Charles B. Dudley Award for outstanding publication, American Society for Testing and Materials, (19) Fellow, Virginia Academy of Sciences, and (20) Twentieth Century Distinguished Service Award, International Statistical Institute.

One of the most heartwarming events of my life was the Festschrift arranged by my graduate students in honor of my 70th birthday in May 1993. Many graduate students contributed academic papers or reminiscences to the book and attended the Festschrift. Over a decade later, I still feel great joy when I think of that event.

When I retired in 1995, the Department of Biology honored me with a reception, which many of my former graduate students also attended. Colleague Bruce Parker was the master-of-ceremonies and organizer. This event was also a memorable experience.

Another heartwarming experience was being inducted as a member of the Morrill Chapter of Alpha Zeta fraternity on 13 March 2002. I had been a pledge at Penn State when the United States entered World War II. I met my spouse and companion Jean at Penn State, and we spent many happy weekends at the AZ house. Although I was not a member, I contributed over the years to the fraternity for sentimental reasons. I was touched when a group of AZ brothers from Penn State joined with the Virginia Tech chapter of AZ to induct me as a member; I had theoretically been a pledge for approximately 60 years—possibly a world record. The best thing about this induction was that I was connected to the past in a delightful way.

Various honors reassured me about the direction of my research. Although I have always felt my research was focused on the effects of anthropogenic stress upon natural systems and the rehabilitation of damaged ecosystems, many scientists felt that ecological restoration and ecotoxicology were different fields. The invitation to be the 1980 Nieuwland Lecturer at the University of Notre Dame reassured me that my research on computer interfaced biological monitoring systems was becoming accepted by mainstream science. Earlier invitations to be a discussion leader at the Gordon Research Conferences (Stream Sanitation, 1956; Environmental Sciences: Water, 1964; Hazardous Material, 1973) and to be Session Chairman of the 1966 conference on Environmental Sciences: Water persuaded me that the endpoints selected for the computer interfaced monitoring had merit. The invitation from the U.S. National Academy of Sciences to present the 1994 Abel Wolman Distinguished Lecture ("Eco-societal Restoration: Re-examining Human Society's Relationship with Natural Systems") confirmed both the importance of ecological restoration and humankind's ethical relationship with natural systems. Becoming a Fellow and Founding Member of the Eco-ethics International Union in 1998 convinced me that I should devote my retirement years to this important area.

Frequently, when I am attempting to determine what a thing is, I start by listing what it is not. An academic honor definitely should not be an opportunity to parade before one's colleagues and bask in the limelight! Although the Ruth Patrick Award was a major milestone in my professional career, I could not attend the ceremony because for some years my spouse Jean had suffered from Alzheimers and I was then her primary caregiver. Her sense of time was not good even then and, if I had been away for the time it would take to get to Albuquerque, New Mexico, for the awards ceremony, she could easily have assumed that I had disappeared permanently. Once, while our daughter Karen was visiting, I left Jean in her care for several hours while I went into my office at the university to complete a manuscript. Even in that short time, Jean was concerned that something had happened to me. I simply could not risk the emotional impact this absence might have on her. President Lewis was extremely understanding and sympathetic when I explained the situation, and a colleague of many years, Rex Lowe, accepted the award on my behalf. Naturally, I would have treasured being present at the award ceremony because it would have given me yet another opportunity to express publicly how much Ruth's mentoring has meant to my professional career and because not many people are still alive who worked with her during the early stages of her career. I was able to do this to some extent by writing a short statement on how much Ruth has meant to my career, including the fact that she probably saved my life when we were studying the upper Amazon River basin and I came down with appendicitis. Ruth found the best physician in Iquitos, Peru, and then arranged for the doctor and me to be at the operating room at the Hopital Santa Rosa simultaneously.

Receiving an academic honor is definitely not an excuse for resting on one's laurels, whatever they may be! One should do one's best in one's chosen profession or life's work without expectation of honors, awards, or recognition. The joy and zest of academic life is itself a great reward. I often marveled that I was paid a salary for doing something so pleasurable. Additionally, my awards and honors came late in life, as did Ruth's; and accidents of fate, such as the appendicitis attack in Peru,

could easily have ended my life before acquiring any honors and awards, but the pleasures of research and teaching would not have been diminished.

Almost certainly, the response to receiving honors and awards varies from one individual to another and I can only write about what it means to me personally. First of all, being awarded recognition is an indirect tribute to those persons who had faith in me. The presentations provide yet another opportunity to remember all of the people who have helped, and are still helping, and reminds me to thank again those still living.

Even when an honor was bestowed upon me, I had always been commonly supported by a team. Darla Donald, my editorial assistant at present and for over three decades, edits all my writing, checks galley proofs, and prepares final drafts of manuscripts to adhere to publisher's requirements. She has been indispensable to me by assuming multiple tasks previously handled by an executive secretary, production typists, and a bookkeeper. Even though she has continued the task of her editor's job, she keeps track of all administrative aspects of my campus office and professorship. Senior Technician B. R. (Bobbie) Niederlehner has always assisted me with locating references and has always been indispensable in my laboratory. She was also critical in training my graduate students during the closure period and in the period before that as the Aquatic Ecology Group faculty and their graduate students moved to individual research. During the closure period, she was essential in keeping the University Center for Environmental and Hazardous Materials Studies functioning. One of my former graduate students related that Bobbie was a more important teacher than many faculty members. Finally, it would not have been possible to maintain research in community-level toxicity testing and protozoan ecology without her assistance. In addition to these two very important people, many other colleagues also have read various drafts of my manuscripts critically and did not hesitate to point out deficiencies. Of course, I always remember former students who chose me as their major professor, faculty advisor, or independent research advisor, or who took one of my courses.

One aspect of an honor or award is that the people who receive them were generally contrarians for most of their careers—that is, they were engaged in activities that were not the norm in their discipline at that time. Honors and awards are one way of validating that these ideas and activities, once regarded as odd or even outrageous, either have become or are becoming part of mainstream science. For me, this recognition is by far the most pleasurable part, not primarily because I feel vindicated in taking the approaches that I did, but rather that the theories, hypotheses, and ideas I espoused have been validated to some degree. My pleasure is not diminished by the realization that probably all the ideas will be regarded as anachronistic in the not too distant future because science progresses. In the meantime, I can contemplate the awards and honors hanging on the walls of my den in Warm Hearth Retirement Village and take satisfaction in the fact that I played a brief role on the stage of the planetary theater.

CHAPTER 16

WRITING

During the first 25 years of my life, I never thought that writing would be even a minor part of my career. Ironically, my basic writing skills developed while completing industrial reports on river surveys, toxicity tests, industrial contracts, and academic grant proposals. Initially, Ruth Patrick did the lion's share of the writing, but, gradually over a period of years, I developed the necessary skills to assemble entire reports and proposals, which were nearly ideal circumstances for developing writing skills: (1) the incentive was strong to function adequately since the operation was entirely dependent on extramural funding, (2) both proposals and reports varied in size from short toxicity tests to major river surveys, (3) both proposals and reports had to be informative and understandable to a wide variety of professionals, and (4) meeting agreed upon deadlines was essential since the information was frequently used immediately.

In the 1940s, 1950s, and early 1960s, the Limnology Department of the Academy of Natural Sciences Philadelphia (ANSP) accepted some contracts to complete research that was not suitable for publication in peer-reviewed journals. Usually, these contracts served two purposes: (1) to obtain new equipment and (2) to carry out toxicity tests in a wide variety of circumstances with a wide variety of chemical substances. From the outset, all understood that the purpose of acquiring money was to generate information that would be useful to both science and society. Usefulness to science was met by publishing in peer-reviewed journals or books with established scientific publishers. Usefulness to society was met by a variety of means, including talks to citizen groups, articles in magazines intended for the general public, and occasionally an "open house" at a research site.

At ANSP, the staff consisted of curators, technicians, secretaries, and so on. Except for Ruth Patrick (also an adjunct professor at the University of Pennsylvania), none of us mentored graduate students. As soon as I left ANSP, I acquired graduate students, which was an additional important attribute of extramural funding involving students. Contracts and grants then had to be suitable for theses, dissertations, and other activities that would forward the careers of the graduate students. However, the purpose of acquiring money should always be to develop information useful to both science and society, not to support individuals or institutions. Obtaining money for use by individuals and for institutions can easily be rationalized, but this diversion takes the zest out of research!

The process leading to publication that worked best for me for 56+ years was to dictate the narrative, thereby getting my ideas on paper even if the sequence was not orderly. I then examined what I had dictated to make certain a unifying theme was evident. This illuminated areas of confusion and disorganization. For over three decades, Darla Donald has provided superb assistance in polishing my literary style, and B. R. Niederlehner helped me for almost two decades with the scientific portions. During this process, I filled in the gaps, attempted to tighten the logic, strengthened the discussion of important areas, and discarded distracting portions not clearly related to the central theme. I routinely let a manuscript sit unexamined for weeks or months if it had no deadline for completion. This absence definitely "made the heart grow fonder" because I was often distressed with sections that did not please, or at least satisfy, me earlier. I was blessed until recently with this method of writing by having skilled people for transcribing dictation, but I no longer have that assistance. Writing by hand or using a word processor simply does not allow me to maintain momentum that I once achieved by dictating. I considered using a voice-activated computer, but it was not satisfactory. Essentially, I "saw" manuscripts in my mind and simply read them into the dictaphone. I now write each manuscript by hand and then have it transferred to a word processing program. This process has not proved to be as onerous as I expected.

Naturally, publishing frequently for over half a century has produced some bizarre situations. Over two decades ago, one of my graduate students had his heart set on publishing in a particular journal. Since he was the first author on the paper, I reluctantly went along with the idea

and the manuscript was dispatched. In about two months, the manuscript was returned with favorable comments from reviewers and a letter from the editor to the graduate student stating that it was time that "John Cairns learned to write." This correspondence was curious since the editor was not known for joking in this way and both reviews were very favorable and recommended publication.

Every few years I got a really harsh criticism of a manuscript from both the editor and reviewers. In some cases, I discarded the manuscript. However, if I felt the criticisms were not persuasive, I sent the entire package, reviews and all, to another journal. Every one of these has been published, and one even won an award. Editors and reviewers usually help strengthen a manuscript, sometimes making useful comments and going well beyond what an author should reasonably expect. A few times, an editor has told me that a manuscript is unsuitable for the journal to which it has been submitted but has suggested an alternative journal. Two journals for which I have a high regard came to my attention this way. I was even appointed to the editorial board of one.

Twice I was invited to write an article on environmental ethics for a religious magazine. Neither gave me any indication of what was expected, but both indicated that a "highly regarded" person had recommended me. Both journals rejected the manuscripts without a reason or comment on deficiencies. Both articles were subsequently published by professional journals interested in environmental ethics.

Just after arriving at Virginia Tech, I sent a manuscript with over 20 hand-drawn, original figures to a European journal via airmail. I learned later that someone in the campus mailroom decided the package was too expensive to send via airmail and had changed the designation to printed matter mail. Regrettably, the package was lost and the figures had to be redrawn at considerable expense and time.

Every three or four years, I received an invitation to write on a specific subject for a specific publication. Occasionally, the person who requested the article ended up asking me to change the content so much that I had difficulty recognizing the manuscript as the one I had submitted. Large sections of my original manuscript had been deleted and replaced with equally long, sometimes longer, rewritten sections. Naturally, I always withdrew the manuscript since the thoughts were no longer mine. I always wondered why the invitation had been issued at the outset. The person who asked for the manuscript should have written it personally. One possibility is that the inviter wanted a particular message of personal importance, but under my name. I could speculate on other reasons for this occurrence, none of which seems rational to me.

Numerous requests have come over the years asking me to assess or evaluate the prospects for a new journal that is being contemplated. Some of the ones I recommended have been extremely successful; others have had only moderate success thus far. The successful journals may have thrived because traditional journals were not adequately meeting the needs of a significant number of research investigators and readers. Science evolves and so do many journals, but some needs still exist, which is why new journals succeed if the timing is right. Some of the most successful journals have been interdisciplinary, and my hope is that transdisciplinary journals will emerge and succeed in the 21st century.

I have been told that I have a tendency to write "run-on" sentences. This penchant is almost certainly due, in part, to my attempts to make connections between disciplines for which no widely accepted terminology exists at present. Fortunately, my editorial assistant Darla Donald is available to help me cope with this style of writing. However, even she, at times, can do little with my sentences that might cover five to six printed lines because no other grammatical structure is possible.

In recent years, I have published articles in international journals, some of which are available on the Internet. Access to some of them is even free. In some cases, traditional journals have permitted me to reprint articles from their publications in free, on-line books (e.g., two such books on sustainable use of the planet are available at www.esep.de). This set-up is a superb means of reaching people who do not have access to a large library and/or cannot afford traditional journals

or academic materials. Sustainable use of the planet will not be possible unless people who cannot afford literature have access to it.

Well operated, electronic journals are also able to reduce the time between submission of a manuscript and its availability to a large readership. Quality control is still dependent on qualified reviewers of manuscripts. Their contributions in the context of electronic journals require rapid turnaround of reviews. Transdisciplinarity journals may find that identification of qualified reviewers is even more difficult than for disciplinary journals. However, the world's major problems (e.g., global warming and population stabilization) require that this issue be resolved. I hope that persons starting their careers in transdisciplinary and interdisciplinary areas have more access to journals that I did half a century ago. Fortunately, I was carrying out research on freshwater protozoans for which a number of traditional outlets were available.

My 600+ journal articles have appeared in 168 different journals. This diversity has been an important learning experience for me. Before sending a manuscript to a journal in which I have never published, I always read at least one issue. Most times, the articles for journals in which I had not previously published were prepared following an invitation from the journal. In such cases, I was usually given explicit instructions on what should be in the article. Since the instructions to the author varied from one journal to another, the assistance of Darla Donald was extremely valuable. The greatest benefit from reading new journals for me was seeing the common knowledge base within each discipline, which enabled me to be more effective in framing my message. The reviewers and letters following publication have always been extremely helpful in making effective connections between and among disciplines.

CHAPTER 17

HAPPINESS IN AN UNSUSTAINABLE WORLD

Shakespeare is rumored to have said that all literature is about loss—a concept I struggled with and wanted to reject, upon first hearing. Preferring to write out of joy—indeed ideally, out of exaltation, bearing witness to the things I love during this brief life—it took me a while to realize that even the act of celebrating is an acknowledgement of loss, for it is the temporal nature of celebration—the awareness that a thing has not always been one certain way before, and may not always be thereafter—which most sharpens the poet's and reader's senses. Celebration and loss are shadows of one another in literature.

Rich Bass, *The Space Between*

I have been suspicious of the word *happiness* since post World War II when the term increasingly became associated with drinking a particular brand of beer, owning a particular car, or, even more incredibly, wearing a particular brand of shoe. During my childhood, this word was not commonly heard except at major benign life transitions, such as weddings, births, promotions, and winning a lottery. Even then, the word was used with caution lest the hubris associated with its use jeopardize or tarnish the event being celebrated. Conspicuous consumerism would have started the wagging of tongues at that time, especially if the buyer flaunted the acquisitions. Satisfaction came from a job well done regardless of its nature, and competency was acknowledged in any activity useful to human society. Competition existed, ranging from who could hang the wash out to dry first on Monday morning to the quality of jams and baked goods at church suppers. I remember my father introducing me to his garage mechanic when I could finally afford a car of my own after World War II. I realized quickly that just having the money to pay for repairs was not enough and I was clearly on temporary trial for being one of the mechanic's customers. A lesson exists in this previous way of life for those of us working on both theoretical and practical environmental problems. The personal satisfaction of doing something well is almost certainly the major reward; arguably, a close second is the recognition from our peers who are competent to judge our performance.

Years ago, I heard laughter as I passed one of the laboratories in Derring Hall (one of the science buildings) at Virginia Polytechnic Institute and State University. Looking in, I saw some of my students looking at pictures of early biomonitoring equipment. They could hardly believe how cumbersome, unwieldy, and mammoth were what passed for computers just a decade before. Even so, a certain satisfaction comes from looking back 30 (even 50) years and realizing that one was frequently on the right track, even if in a stumbling, awkward fashion.

My *Random House Dictionary* defines *happiness* as the state of being happy (synonyms: pleasure, joy, exhilaration, bliss, contentedness, delight, enjoyment, satisfaction). Gertner (2003) discusses how Daniel Gilbert (Department of Psychology, Harvard), Tim Wilson (Department of Psychology, University of Virginia), George Loewenstein (Economist, Carnegie-Mellon Institute), and Daniel Kahneman (Nobel Laureate in economics at Princeton) have studied the decision-making process that shapes the sense of well being: how does one predict what will make one happy or unhappy, and then how does one feel after the actual experience? They concluded that almost all actions are based on one's predictions of the emotional consequences of these events and concluded that humans falter when imagining how they will feel about events in the future. Many television advertisements for automobiles, cosmetics, and so on picture beautiful actors in exotic places that are quite unlike the urban environment. Almost always, the product is purportedly extremely attractive to the opposite sex. Rarely do these products deliver the implied delights—depending on

human artifacts for happiness is not a good idea. In short, the new "unicorn" automobile will not meet the expectations of making life perfect—traffic jams, huge trucks, drunken drivers, polluted air, and many other hazards will spoil the dream. Gilbert and his colleague Wilson (Gertner, 2003) have developed a means (called impact bias) for estimating both the intensity and duration of emotions to bias the tendency to err. Gilbert uses the term *miswanting* to describe how these mistakes of expectation can lead directly to mistakes in choosing what will give pleasure. How sad that many material possessions produce environmental damage without resulting in durable happiness.

People often ask me how I can be happy when I spend much of my life studying pollution, damaged ecosystems, hazardous substances, and species impoverishment (i.e., extinction and loss of large numbers of still surviving populations). This situation has even been exacerbated during the last half of the 20th century and the first few years of the 21st century. In the early part of the 21st century, significant evidence (e.g., enough to persuade mainstream science) indicates that anthropogenic environmental stress will push the biospheric life support system upon which humankind depends into disequilibrium (past ecological tipping points). Climate change will be more difficult and expensive to avoid. The "First Rule of Holes" advises: when one is in a hole, stop digging. If intelligence has survival value, humankind should be able to use Earth without abusing it. Many sustainable practices could replace unsustainable practices if human society had the will to do so.

Gould (1996) notes that *Homo sapiens* is not now, nor has it ever been, the dominant species on the planet. Bacteria among living creatures have always had dominance. As Durant and Durant (1968, p. 18) remark, the laws of biology are the fundamental lessons of history. They acknowledge that humans are subject to the processes and trials of evolution and to the struggle for existence. Possibly most important, if some individuals appear to be escaping the stresses, they are doing so because the group is protecting them either deliberately or inadvertently. Nevertheless, the group itself must meet the challenges necessary in a way leading to survival. Even though these highly regarded historians make these statements about survival, without any caveats or qualifications, human society, either by country or globally, is paying no attention to the tests that society must meet.

Durant and Durant (1968, p. 19) also remark that the second biological lesson of history is that life is selection. Presumably, some individuals and some societies are better equipped than others to meet various stresses. Inequality is not only natural and inborn, but it grows with the complexity of civilization. When inequality becomes too great, that is, the range between the richest and the poorest is the greatest, either a revolution or a governmental redistribution of wealth occurs. This redistribution also applies to, for example, the amount of energy consumed by *Homo sapiens* compared to the amount of energy remaining for the other 30+ million species on the planet.

Humankind evolved as a small group species in which survival required a high level of literacy about its habitat (i.e., environment). At present, much of humankind lives in enormous groups that are not closely associated with the natural systems that produce the resources upon which they depend. Kindness (Editors of Canari Press, 1996) was more likely to be reciprocated in tribal groups.

I remain hopeful about moving toward sustainability because some major obstacles can be markedly reduced, even eliminated, by modifying human behavior: (1) since sustainable use of the planet is essentially a social contract between human society and natural systems, the social and natural sciences need to work together far more closely than they now do, (2) eco-ethics must replace single issue compassion, which is incompatible with system-level decisions, (3) political leaders and the general public cannot dismiss the conclusions of mainstream science just because they are not congruent with their political ideology, (4) perpetual growth (except in such things as literacy) is simply not possible on a finite planet, (5) it is naïve to expect the world's impoverished peoples to support adequately the quest for sustainable use of the planet with the present extraordinary differences in both per capita ecological footprint size and nation-state footprint size, (6) both social and environmental scientists need to communicate their concepts to the general

public as well as economists do, (7) pro-life must mean all life forms, not just individuals of *Homo sapiens*.

Arguably, the most difficult obstacle to achieving sustainability is the failure of most people to realize they are a part of the natural environment and not apart from it. Humankind must acknowledge its dependence upon natural capital and the services it provides. If natural capital and its services are placed in disequilibrium, both the human species and its economy will be severely harmed, perhaps even extinguished. Schultz et al. (2004) use an implicit association test (IAT) to measure the degree to which people associate themselves with nature. They distinguish between egoistic concerns that focus on self and biospheric ones that focus on life forms. They believe that the type of concerns developed about environmental issues is associated with the degree to which each individual feels associated with natural systems. Not surprisingly, their study showed a moderately positive relationship between biospheric concerns and implicit connections with nature. A negative relationship exists between implicit connections with nature and egoistic concerns. They conclude that connectedness is fairly stable across time and question how malleable connectedness is. Both Schultz (2001) and Dunlap et al. (1993) have provided evidence of country-level differences in attitudes about environmental issues. Their and other publications on this subject indicate both the types of future research that are needed and what approaches might be useful in applying this research to increase biospheric concerns so essential to sustainable use of the planet. Such publications, if incorporated into a transdisciplinary framework, will facilitate the monitoring of global eco-ethics and sustainability ethics, especially when guided by reason and intelligence.

My response to the disquieting aspects of human society's relationship with the environment is to devote as much time as possible in the remaining part of my life to making a small contribution to arousing biospheric concerns in the general public with the hope of increasing the number of people who wish to leave a habitable planet to posterity, including the preservation and accumulation of natural capital. If enough people adopt this perspective, humankind will have an improved opportunity to achieve sustainability.

Of course, egoistic concerns may block the quest for sustainability. Species extinctions do occur, and the quest for sustainability is based on the assumption that such an extinction will not happen to *Homo sapiens*. However, until robust evidence indicates that plan A is working, a plan B is a must. My own plan is based on personal experience. My book *Eco-ethics and Sustainability Ethics Part 1* (Cairns, 2003) and *Part 2* (Cairns, 2004) is dedicated to my companion and spouse Jean, who had both Alzheimer's and Parkinson's afflictions. Jean's communications with me began declining in 1997. We were in a "long goodbye" (Jean died on February 21, 2005). Naturally, I did everything possible to see that medical treatments slowed her decline to the extent possible, and I visited her three times daily for about an hour to continue our companionship. Applied to the biosphere, this experience means preserving, to the best of my ability, the biospheric attributes and qualities that I love as long as I can. Moreover, I should make a major effort to maintain my connections and association with nature to the fullest extent possible. The possibility of loss (or in Jean's case, the certainty that our companionship has ended) should not eliminate pleasure, joy, exhilaration, delight, and enjoyment of what is still available to me. I believe the quest for sustainable use of the planet is basically an ethical issue.

Deep pain is frequently associated with deep love. At the individual level, the pain can result from one's cherished companion suffering from an irreversible affliction. On the environmental level, anyone who loves nature is pained by species extinction, global climate change, and a depressing array of other insults to the environment. In this instance, the pain results from both the damage to natural systems and the failure to implement remedial measures that are available (e.g., reducing greenhouse gases). We should feel joy that we were blessed to have had the wonderful experience of associating with a loved person or with a loved array of natural systems, some of which have disappeared (e.g., due to such things as urban sprawl) or have been badly damaged in our lifetimes. The depth of our love is reflected in the depth of our pain.

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CHAPTER 18

FACING MORTALITY IN THE RETIREMENT YEARS

This chapter on mortality is strongly influenced by three events. The first was the sudden and unexpected death of my only cousin on my father's side of the family. Howard Kent, MD, died in a traffic accident when two young males in a car crossed the median strip and hit the car driven by Howard, who was accompanied by his wife Peggy, head on. I learned of Howard's death on my 77th birthday through the kindness of his neighbor Mr. N. Thomas Sauer, who somehow found my telephone number. Peggy survived, which is fortunate since two children would have had to suffer the sudden loss of two parents. Howard was approximately eight years younger than me and was still seeing some patients, so he had not fully retired. Howard's death was particularly unsettling to me because the decision on how to spend the remainder of his life was taken from him by an irresponsible driver. Sudden death can happen to anyone, since similar head-on collisions that are the result of careless or drunken driving are all too frequent. Laws of life apply equally to all people; none are exempt—a reality that seems to elude many individuals. I feel reasonably certain that Howard had sufficient retirement funds to end his medical practice entirely and, therefore, I think he was serving some patients because of his desire to be of service to humanity, rather than to accumulate wealth. I suspect that, even had he known that his life would end shortly, this service would have continued. I have always been impressed that the famous physicist Richard Feynman (1989) made a conscious decision to serve in the now famous "O-ring" Congressional hearings on the spacecraft Challenger disaster, even though he was suffering a fatal disease. Feynman made the crucial point about the lack of resilience of the O-rings at low temperatures when he dipped one in a glass of ice water during the hearings and showed how much resilience the O-ring had lost. This demonstration was the defining moment of the hearings. Feynman could not have been absolutely confident that he would have any impact on the outcome of the hearings, but he chose to be of service anyway.

The second important event impacting my contemplation of mortality was my moving to a townhouse in Warm Hearth Village, a retirement community, in March 2000. This long-considered move occurred because my spouse Jean and I recognized our vulnerability to being isolated from the community by snow and ice storms, which might include a loss of electric power in a house that was heated and otherwise run by electricity, including the water supply. Although we signed a life lease on the townhouse, we did not own it and paid a monthly fee, which included maintenance of the dwelling itself, except for the washer and dryer, and maintenance of the grounds. Such a living arrangement meant no loss of independence but considerable reduction in responsibility. In addition, Warm Hearth Village also made available the transition to an assisted living center (me) and then to a full-scale nursing facility (Jean). Relatively few people in retirement continue their former professional activities at any level, although considerable tolerance and support can be found in a retirement community. In some cases, musicians may suffer loss of hearing, while other professionals may suffer diminished eyesight, and so on, which cruelly prevent them from continuing their former professional activities. Others, such as Wybe Kroontje (now deceased), whose vision for this retirement community (based on The Netherlands' model) was the impetus for the facility, still continued to be active in service to Warm Hearth Village and pursued other interests such as gardening. Some retirees are active in social groups, church groups, etc. Most seem to have one or more activities that are important to them and pursue them to the limit that their health permits. Jean and I initially accepted responsibility for the maintenance of a portion of the 7-mile hiking trail system, which we then used on a daily basis, weather permitting, and which we thoroughly enjoyed.

Another factor for considering retirement (and my own mortality) was a continuing series of invitations to give keynote addresses by means of electronic media (because I seldom traveled), to write chapters for books edited by others, to edit and write books, and, last but not lest, to produce manuscripts for professional journals. In terms of the development of the field of environmental

studies and my remaining professional career, I felt my location geographically made little difference.

After moving to Warm Hearth Village, I decided to cease public speaking entirely (I honored already scheduled commitments) so that Jean and I could have more time together and to avoid the aggravations of travel. As if fate were determined to check the sincerity of this decision, an invitation to give a keynote address for the 2000 Olympiad of the Odyssey of the Mind at Sorbonne University arrived. The invitation was very tempting, as well as being a great honor. However, the reasons for the basic decision still seemed persuasive; I sent a letter expressing deep regrets and explained the circumstances that led to the decision. This letter became a "point of no return" because I could not justify accepting any other invitations. Predictably, an invitation came to lecture in a colleague's class. The invitation involved no travel, but Jean's dizzy spells had increased and their occurrence was unpredictable. A fall for her would be dangerous. The end of my professional speaking era brought no pangs of regret or feelings of frustration! Presumably, since invitations were still coming, I was pleasing a wide variety of audiences right up to the end—much better to have stopped willingly than to cease because invitations stopped!

A bright side to these decisions was that I had better control of my time. Jean and I were able to take walks on the trails of the Village, or she could nap when she was fatigued and I could write. Getting better control of my time has not diminished my communication with colleagues. I have been better able to focus on writing. The joy of producing manuscripts has remained at about the same intensity for all but the beginning of my career.

I am content with my expenditure of time each day in the retirement community. Maintenance time (health care, shopping) is obviously essential. However, quality time with family (via email and telephone) comes first, and professional activities that do not involve travel come second. Last, but far from least, social interactions (religious, cultural, and conversational) are very rewarding.

I moved Jean to the nursing facility in mid-2001, and I moved to the assisted living center. When I visited Jean, we sometimes looked at photographs from past wonderful days, and I was grateful that we were able to share so many memorable experiences for 63 years. I do not fear death, but, after living in a retirement community, I do fear various disabilities that cause a severe loss of independence.

I had realized intellectually that Jean's death (February 21, 2005) was inevitable; the event, nevertheless, was a huge emotional shock. I was not prepared for a world without Jean—I had shut this possibility out of my mind. Sometimes denial helps when one's lifetime companion is afflicted with an irreversible condition. I am still adjusting to my new circumstances. Friends from as far removed as Jean's college roommate (Kathy Brady) and up to the Blacksburg community were very understanding and kind. Our children and physicians were especially helpful.

About four months after Jean's death, I began to get seminar invitations because "I was now free to travel again." While deciding on these requests, I had an invitation to give a talk locally. I had to drive less than five miles to present the talk, which was well attended and the questions were stimulating. It was very enjoyable, but tiring. I began wondering how I could travel to a much more distant location and be far away from my apartment where I could rest and elevate my legs periodically. I wear compression stockings (to minimize the probability of blood clots) that must be put on each morning and taken off by bedtime. Nurses perform this duty for me, but the average hotel, motel, or private dwelling lacks this type of skilled help. In addition, I can reach a substantial number of colleagues from my residence and still maintain a lifestyle that protects my health. As of August 2005, I have postponed a final decision on travel. Since I have not fully regained my balance, a walker is essential and I have not fully mastered walking on steps. The effort required for professional travel has increased markedly. Furthermore, I might not have adequate energy for any undertaking when I arrive at my destination.

However, it is worth remembering that in mid-2002 I could hardly walk and had severe pain. Although I am being cautious in the short term, I still have hopes for more travel in the long term.

Whatever happens, I can celebrate the 50+ years of my professional career that I have already experienced.

On August 2, 2005, I decided to devote all my efforts to finishing this autobiography, even though six manuscripts were ready for Darla Donald to edit. Alternating between the book and manuscripts was neither efficient nor satisfying. Even if I could travel more easily, leaving work uncompleted is disquieting. Both Darla and I have benefited from focusing on this book. I am treating the autobiography as a "work in progress" and expect to get help in filling in the gaps from those people who have shared my career.

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CHAPTER 19

RESEARCH, LECTURES, PERFORMANCES, AND OTHER ACADEMIC COMMUNICATIONS

Over my 57-year career, I have given many seminars, presentations at scholarly meetings, plenary session addresses at major meetings, keynote addresses, and banquet addresses. Once, at the University of North Texas, I gave the J. K. G. Silvey Annual Barbeque Lecture. I had known and respected Silvey from the late 1950s or early 1960s and was honored to give the lecture. The atmosphere was more relaxed than on most similar occasions, but was very professional. From the outset of my professional career, I practically never read from a prepared manuscript when making a presentation even if directly afterward I handed copies of the manuscript to be included in the proceedings of the symposium or conference. Almost invariably I used a few cards to prompt me on major issues and to ensure that the sequencing was appropriate. I felt, after observing speakers as an undergraduate, a graduate student, and finally a professional, that audiences did not respond as well to people who were reading from a prepared text as they did to people who were prepared to speak from a few prompting notes. On rare occasions, I have seen a conference speaker read an entire paper in a monotone without once looking at the audience. This situation is substantially different from classroom teaching where the material being taught in the beginning courses is almost always a compendium of research done by others. Even when the teacher is the author of the basic text being used, he has, at some time, spent enormous amounts of time going over the literature, selecting the most appropriate publications, reducing the professional jargon to a level suited to the literacy of the students, and integrating it in such a way that the disparate parts are connected in as smooth a flow as possible.

Although I produced one small high school text decades ago, I much prefer editing books where a knowledgeable person in that field covers each topic. The most effective teachers are those whose joy and zest for the subject are clearly apparent to the audience. A splendid example is Leonard Bernstein explaining classical music to schoolchildren, most of whom probably never heard this type of music or, until then, probably did not want to hear it. I am not denigrating the obviously successful efforts of the great educators by calling them performers. A good performer, above all, is attentive to audience response and, in the scientific world, one can judge this response best while viewing the audience, not the manuscript, when the point is being delivered.

The most exciting professional talks to me have always been when an innovator, who is also a performer, describes personal research. I had the privilege of hearing the late Robert MacArthur give a seminar at the Academy of National Sciences that focused on the development of the MacArthur/Wilson equilibrium model. In developing the model, MacArthur and his collaborator E. O. Wilson of Harvard University amassed persuasive evidence, which subsequently appeared in a monograph and numerous publications. In his seminar, however, the level of detail was kept to that minimum essential to follow MacArthur's train of thought and not so much that it would overwhelm the audience, however intelligent, which MacArthur could easily have done. Furthermore, he started with a brief synopsis of how he had begun asking the questions that led to the development of the model and the progressive steps that led to the final product. Well before the seminar had concluded, I realized that I had all the evidence before me to reach the same conclusions, even though I had not previously viewed the situation that way. This presentation was (for me) the ultimate combination of creativity and performance—namely, the ability to inspire members of the audience to view their own research in a different light. Subsequently, my students, colleagues, and I published a number of papers on the MacArthur/Wilson equilibrium model, and a long period of research on the colonization dynamics of aquatic microbial species on artificial substrates had begun for me. I owe all this professional inquiry to one outstanding seminar by an enthusiastic individual.

However, to be a successful performer (and again this is not a pejorative word), one must invest considerable amounts of time and energy in studying the audience and in connecting the talk to the audience's interests. Benefits of making connections between your research and that of a particular audience are innumerable. Also valuable are face-to-face meetings with colleagues from

other parts of one's own country and other parts of the world. I still regularly receive invitations to submit manuscripts, give keynote and plenary addresses to professional meetings, and the like from people whom I met 10, 20, or even more years ago. I fulfilled some requests by means of electronic transmission of the talks either by tape or disk. These technologies, combined with e-mail exchanges (despite my laborious, two-fingered typing), enable me to continue in the performer role despite the fact that health problems and markedly lower energy levels have virtually eliminated travel for me. Arguably, the most important benefits to professional communication are: (1) reconstructing one's reasoning process for intelligent professionals in some other area of specialization and (2) reacting to colleagues who might not respond to a journal article but will respond via e-mail, letter, phone calls, and so on.

Zest for one's professional activities is dramatically increased when one's colleagues, especially those in other disciplines, commend and literally applaud them! Even strong disagreement is energizing because one has engaged the minds of others, even if they are not yet persuaded to one's personal views. Apathy is another matter entirely! Fortunately, I have encountered it only rarely, always in isolated pockets and isolated incidents surrounded by notable successes.

Unquestionably, in the early part of my career, when biological journals were rather indifferent to manuscripts on toxicology, pollution, and what is now known as ecological restoration, the response of other disciplines, particularly engineers and chemists, to talks on these subjects was extremely reassuring, arguably essential, when colleagues in what I regarded as my own field of environmental biology were extremely doubtful that I was a real biologist. Engineers and chemists also put me in touch with sources of extramural funding, which enabled me to continue my research. Talks given abroad enabled me to establish personal relationships with colleagues who were beneficial in a variety of ways, both professionally and personally. The investment of time and energy, which I believe to be mandatory for anyone with transdisciplinary interests, was amply repaid. On the other hand, I gave huge numbers of both out-of-town and out-of-the-country seminars at times, many of which were not memorable in any way, although I am certain they were personally enjoyable for me at the time. Fortunately, I have always turned in an annual report to the academic institution employing me and have kept a file of these reports for the last 30 years. When I look at a 23+-year-old annual report, I am aghast, from my present viewpoint, at the large number of off-campus seminars, professional meetings, and the like that are listed. I hasten to add that I have colleagues at other institutions whose travel would make mine look trivial, even when I was traveling more. Even in retrospect, I cannot decide what the right balance between being a researcher and a communicator might be.

Immediate, gratifying satisfaction occurs after all successful performances. Each venture into new areas requires a certain amount of emotional support for such performances. However, long-term impacts of this type come from truly magnificent performers, whereas even modest levels of research are likely to be remembered to a certain degree. One can be a performer, even in science, without being a researcher, and a researcher without being a performer. Being a performer requires vast amounts of energy and endurance at particular points in time. Research, on the other hand, requires time for contemplation, reflection, and so on and large blocks of time, unbroken by travel, airport delays, and the like. I realize that the electronic miracles of recent years have extended my career as a performer. Research is more amenable to old age than performance. Being an innovator in one's old age is easier than being a performer, at least for me.

I still publish in professional journals, but many of the manuscripts synthesize, integrate, and interpret concepts and are not based on hard data that I have generated personally. Over half of the recent publications involve ethics, value judgments, and humankind's relationship with natural systems. I also review many more books than I did in the past. The Internet enables me to communicate with colleagues worldwide. I am grateful that these opportunities are still available to me!

CHAPTER 20

LEAVING THE LOOP

At a Warm Hearth Retirement Village holiday party in December 2001, one of my colleagues, the Reverend Al Payne, asked me how I felt about being "out of the loop." My first reaction was one of indignation! I had just sent two manuscripts to scholarly journals, had over a dozen articles in press, and another dozen in review. I had recently agreed to be an editor of a new electronic journal, *Ethics in Science and Environmental Politics*, was on the editorial board of about a dozen other professional journals, was still active in reviewing reports for the National Research Council and the National Academy Press, and was still carrying out reviews for such organizations as the Third World Academy of Sciences, a variety of funding organizations, and the like. How in the world could anyone possibly consider that I was out of the loop? I had more activities of this sort then than many faculty members have in the aggregate over an entire professional career.

But Al Payne was a gentle, compassionate man and would not have asked me the question without reasonable, sympathetic grounds for doing so. One response was to recall mentally my earlier involvement in "the loop," which was quite different from that of other faculty members. Viewed in this context, my particular dissociation from the loop was substantial.

For some years, I had been giving international addresses by means of videotapes or other distance learning systems. Consequently, the part of the loop represented by personal, verbal presentations was over, and, although I had no regrets in making the decision, that part of my career was enjoyable and exciting up to the very end. On reflection, I believe terminating public speaking was a wise decision, despite my tremendous delight and zest from interacting with a live audience. I have fond memories of and enjoyed every minute of public speaking. Videotapes and other distance learning technologies are useful, but do not replace speaking in person.

Another aspect of the loop was keeping in touch with colleagues in other countries, many on the other side of the world, with whom I can communicate almost instantly by e-mail. Some of these colleagues have become good friends, even though it is unlikely we will ever meet. Electronics allows me to remain in this part of the loop longer than I could have when I depended primarily on the postal service and the telephone.

Teaching was not an important part of my professional career from 1948-1961 (although I did teach a 1-year, all-day Saturday class at Temple University). In 1961, teaching became a larger part of my career as a result of summers spent at Rocky Mountain Biological Laboratory in 1961-1963 and the University of Michigan Biological Station after that. In 1966, I joined the faculty at the University of Kansas, and teaching became an integral part of my position there. From 1966 until my retirement in June 1995, I taught at least three courses each year, sometimes more. At some colleges and universities, this number of classes is by no means even an average load; however, in a research university, this number is at least average. I always enjoyed teaching. However, I am definitely out of the teaching part of "the loop" and have fond memories of that part of my professional career.

I essentially gave up consulting in 1993 when I reluctantly decided I should no longer serve on the Environmental Advisory Committee of the Savannah River Site. I had worked on the river survey team of the Academy of Natural Sciences under the direction of Ruth Patrick on this site before the plant was actually constructed in the early 1950s and continued to do so through 1966. Much later, I became a member of the Environmental Advisory Committee. I watched with fascination the ecological recovery of this enormous tract when humans were removed from all but a few areas. Furthermore, I enjoyed serving with the very competent people on both the river survey team and later the committee, which was chaired by Ruth Patrick for almost all of my period of service. Despite my fascination with the ecological recovery and the enjoyment of interacting with competent colleagues, I simply did not have the energy to read through the mounds of material that were essential to informed judgment or advice. After my resignation from the Environmental Advisory Committee, I continued to do a modest level of consulting that could be carried out from

Blacksburg, but nothing more. Again, I enjoyed being in that part of the loop, not because of the money (although it was useful for some professional needs, such as purchasing reprints and the like), but because the consulting brought me into contact with some "real world" problems that I otherwise might not have encountered in such depth. If consulting for money constitutes a portion of "the loop," I have definitely been out of the loop since 1993, except for occasional events. I do not miss being in the part of the loop represented by paid consulting. I no longer need the money it brought, although it was often helpful to my research program and professional career. The information that consulting brought can now be acquired from the Internet, from reprints and other documents sent to me by colleagues, and, most importantly, through professional service (unpaid consulting). No paid consulting came close to providing the information to which I was exposed as a two-term member of the Report Review Committee of the National Research Council and the National Academy Press. Furthermore, neither has any paid consulting come close to such activities as chairing the committee that produced the 1992 National Academy Press book *Restoration of Aquatic Ecosystems: Science, Technology, and Public Policy*—arguably the best publication with which I have been associated in my entire professional career.

I still continue work in the part of the loop that I consider professional service. I serve on the Panel, Court Appointed Scientific Experts, American Association for the Advancement of Science; am a Scientific Adviser to the Academy of Natural Sciences; review proposals and such for the Third World Academy of Sciences; evaluate the credentials of candidates for honorific positions; and review grant proposals and manuscripts submitted to professional journals. Another large amount of my time has been spent on the 15 professional journals for which I have served on either the editorial board or the advisory board. Some obligations take only a few hours per month, others considerably more.

Another important aspect must be considered when evaluating the degree to which any individual is "in the loop." I have been privileged to know a number of "24-hour scientists," among them Ruth Patrick, Paul Ehrlich, Peter Raven, E. O. Wilson, Dan Janzen, and G. Evelyn Hutchinson. I am definitely not one of these completely dedicated individuals, but I have always invested in considerably more time than most of my colleagues at the academic institutions where I have been employed. This expenditure of time began with my first research effort on the Conestoga River Basin where I worked until 1 or 2 am most mornings and through entire weekends. I did not feel impelled to work until those hours, but I wanted to match the performance of my experienced counterpart Dr. Mary Gojdics, the protozoologist on the other field team. Only relatively recently did I realize (as a result of correspondence with Thomas Dolan IV) that other crew members from that era were playing tennis, sightseeing, and so on during weekends. In short, I had personal standards for improvement that required long hours to achieve. However, I must confess to feelings of resentment after this initial stage when I found that many colleagues were spending far fewer hours than I was on their professional positions with relatively little difference in remuneration. We all appeared to be spending comparable amounts of time with our spouses and family, and the difference was in how much time my colleagues had for personal recreational activities that I did not. I also noticed that, regardless of how much time I spent on professional activities, Ruth Patrick almost always spent more. Furthermore, she did not appear to resent the fact that I put in less hours than she did. Finally, I realized that I had the same control over my time that the others did, I just chose to spend it differently. The second thing I learned during this period, mostly by observing scholars that I greatly admired, was that, if I wanted to do something well, I had to take the time to learn how to do it well. I had to pay a price, and I had to accept that paying the price was justified in terms of my personal gratification.

Arguably, being "in the loop" is one of the world's best manifestations of delayed gratification. Unquestionably, becoming a well known author, acquiring an Olympic gold medal, becoming a Nobel Laureate, and the like are at least an order of magnitude more difficult and equally less likely to "pay off." Some people become masters of "the loop" by a single conceptual breakthrough, such as Watson's and Crick's discovery of the double helix. Most of us, however, advance incrementally, some so slowly that the advancement is hardly apparent, even to the most

charitable observers. Obtaining the average PhD requires a long period of schooling with many hoops to jump through and "gatekeepers" to pass. Even after acquiring the PhD, one is generally only at the point where entering the loop is possible, but by no means certain. In fact, speaking of "THE LOOP" is inappropriate except in rare circumstances. In my own field of biology, many sub-disciplines exist; each with its own loop. The typical situation is that each particular loop is not aware of the degree to which others are masters of other loops, especially any outside their professional discipline. The accomplishments of these people, again with rare exceptions, who are masters of a particular loop, are unknown to the checkout people at the local supermarket, the automotive repair shop mechanics, bankers, and the like—they have their own loops.

So, the answer to the question "how do I feel about being out of the loop?" is that most people did not know what loop or loops I associated with, my mastery of the loops, or the degree to which I have left them. Most of the certificates, scrolls, and medals that hang on the walls of my den in Warm Hearth Village (daughter Karen hung them—their main function is to remind me not to stop taking risks; about once a month I also indulge in nostalgia) are viewed mostly by the cleaning woman who visits periodically to vacuum the carpet and dust the bookshelves. The reality is that I have striven hard to achieve some limited and temporary degree of success in a few of the almost infinite number of loops. During one's lifetime, one's awareness of the structure of the loop and the range of loops requires a painful, often almost paralyzing, effort that sometimes is interspersed with periods of feeling overwhelmed by the complexity of the few loops one thinks one can see clearly. However, unless one makes constant, temporarily painful efforts to acquire the necessary new information as the loops evolve, one will be ejected by "the loop," sometimes without even being aware of its existence.

I decided years ago to take the responsibility for the reallocation of personal time to enter the loop, accepted the responsibility and effort needed to stay in the loop, accepted the "price" of entering new loops, and realized that my time in any loop was finite, although, with effort, the time could be extended and the departure could be gradual. Since what I did in the loop is unknown to most of the people I encounter, gratification must be primarily internal, just as is my responsibility for allocating personal time.

I wrote the following section at one of the darkest periods of my life. Jean's Alzheimer's was worsening; we had to leave our beloved woods and house on Bishop Road where we had spent the greatest percentage of our lives; we had too many books to fit into the townhouse so they went to our children and colleagues; and, shortly after our personal move, I learned that I would lose over half of my professional space at the university. As usual, I prepared for a "worst case scenario," which would have been devastating personally and professionally. The following section originally expressed all my fears and how I would confront them. I have tried to retain the despair I felt while recounting what actually did happen.

In mid-January 2000, Joe Cowles, the Head of the Department of Biology at Virginia Polytechnic Institute and State University, indicated to me that departmental space problems might require that I vacate part or all of the space that I was utilizing for publication and professional service activities. Since Joe had been most supportive of my professional activities ever since becoming department head, I was confident that he would not have alerted me to this possibility unless space pressures were extreme. Some universities have not been funded adequately for at least a decade, and the pressures on space use are enormous. I was relieved that I was not responsible for making budget cutting and space allocation decisions because they would have been anguishing.

Helen Keller remarked that when life closes a door, it usually opens another, and it does not pay to stare too long at the closed door. In analyzing my own situation with regards to the loss of space, I found a book by Slaughter and Leslie (1999, p. 243) to be the most helpful: "because of the disparities among faculty, the concept of the university as a community of scholars will disintegrate further, and management will replace governance. Administrators will be most responsible to those elements of the institution which bring in increased revenues—academic capitalists and students."

I planned on vacating the entire professional space occupied by both my editorial assistant Darla Donald and me as early as summer 2001. I was ultimately able to retain part of the space, the most essential area where all the manuscripts were stored and where Darla did her editorial work. Retaining this space meant that my writing could continue with relatively minor disruption. The partial loss of my office space, however, was a fatal blow to many of my professional service activities, including the American Association for the Advancement of Science panel already mentioned and being a science advisor to the Academy of Natural Sciences and the Third World Academy of Sciences. Professional service requires an enormous amount of space for books, grant proposals, and the like that are necessary for a particular, although often temporary, responsibility. My office, which I now visit intermittently, was my repository for the stacks of information that I moved as needed to my den at Warm Hearth Village. I have always enjoyed professional service, and I believe it contributes to the academic stature of any university.

John Tanton had regularly reminded me of the desirability of finishing this autobiography. However, I had too often let other people with more insistent voices divert me from the book itself. Additionally, I had been wondering how to end the book (now it ends with an epilogue). As a number of colleagues and former graduate students have remarked that, on the last day of my life, I will probably be writing my last manuscript while being carried into the emergency room of the local hospital. Perhaps this scenario will cause younger scientists to reflect on how they should terminate their professional careers in an incremental fashion if given the opportunity to do so.

My professional space originally contained my office, an editorial office where Darla did all the manuscript work, and a small office appended to the editorial area that housed all the financial records from nearly three decades for the University Center for Environmental and Hazardous Materials Studies, which I had been requested by the university to retain. As space diminished, contents of many file drawers were shredded and dumped into the recycling bin. Also in my office were the raw materials for already published articles, including references, which I typically kept for a significant period of time in case letters to the journal followed publication, etc. These materials were also helpful for books likely to have a second edition published. I had substantial files on persons capable of completing reviews in different categories; some of these sources of information dated back over half a century. I also had space for filing and working on draft reports from the National Research Council when I was a monitor and a variety of other activities requiring substantial space. Even with my planning for this event, both the timing and the short time for transition caught me by surprise. However, I had already decided on a plan to cope with significant loss of space, which involved giving up many professional activities. I had hoped to have an advance warning of three or four months so as to alert the organizations from which I would be resigning so that they would have time to find a replacement. Eventually, I retained only one area for my professional space, which enabled Darla to move to one room all the publication materials for the numerous articles in various stages of preparation and the two books that were at different stages of completion.

I immediately dictated a single letter of resignation to various professional organizations and described the loss of professional space, the short time (less than a month) that Darla and I had available to make the transition, and the rationale for resigning from much of the public service and professional service because of lack of working space. The situation was exacerbated, particularly for Darla, since we had a number of publication deadlines to meet. Much time for meeting these deadlines was lost to us. Not to be outdone, Murphy's Law was fully operative. During this chaotic time, I was placed on an antibiotic as a consequence of an infection from a tick bite. My spouse Jean began experiencing intermittent, severe chest pains and was admitted through the emergency room to the local hospital.

My time for the move and culling of files was limited even more. I knew I had no time to be selective in discarding my files, except to save a folder or two of early pictures from the Ruth Patrick river survey team era and other portions of my career. Professional files from 53 years were discarded, essentially without examination except for a glance at the headings and tabs, in every wastebasket in sight. The kinds of professional service I had been doing made some of these files

priceless since they contained names, competencies, experience, and the like. However, they were only useful in their entirety and saving a selective few would be the equivalent of saving only a few species in an ecosystem.

My professional books posed an even greater, heartbreaking problem. My office was filled floor to ceiling with books. I had extensive collections of both books and reprints on ecotoxicology and ecological restoration, a significant number of publications on freshwater protozoan and community structure, and a significant number on water chemistry, geology, and the like. My townhouse contained the books and reprints on sustainable use of the planet that I had been accumulating over the previous two decades. The decision was difficult, but abundantly clear! I kept the books on sustainable use of the planet and a few books on ecotoxicology and ecological restoration. I sent the remainder to people who would benefit from them. Without the extensive database on people and the extensive personal reprint and book library, I did not feel comfortable continuing in professional service at my previous level because I had always depended heavily on both the files and the publications for this work—I felt very insecure without them. I realized that giving up publishing would be totally devastating for me, so the space remaining was dedicated entirely to that purpose.

I was pained that half a century of accumulated literature and records of collegial relationships could disappear in a few weeks. The university aspires to become one of the 30 top-ranked universities in the United States, and it needed to use the resources available in ways that would enhance the possibility of achieving this goal. I continue the excitement of writing and publishing in professional journals on topics that interest me most with all the resources available to me (book royalties, small grants, and personal funds) to achieve this important goal. As a consequence, I will not voluntarily leave the part of the loop with which I am still engaged. However, if this part of the loop leaves me as far as the institution is concerned (unlikely in the short term), I will attempt to use other alternatives to continue.

I wonder about the progress of my career had this particular space not have been lost at this particular time. I did, of course, fulfill all obligations already incurred, such as acting as monitor on a National Research Council report. Very likely, I would have continued to respond positively to professional service requests as long as my health and other circumstances permitted. On the other hand, projects such as this autobiography always suffered because I had no particular deadline for completion and the professional service obligations all had deadlines. I realized I would have more time for my own publications, writing reviews of books published by others, and reading for pleasure—an enticing prospect, but one that I probably would not have sought had the loss of space not forced me to do so. I embraced the prospect and refused to let the change and loss depress me! I recently have read a wide variety of books on subjects from anthropology to wild minds, and I have indulged more fully my weakness for detective stories.

Often, institutional events outside of one's control force one into changes that, in the long run, prove more advantageous than continuing on the previous course. The primary question in such situations is "does the future look attractive?" and, in this case, the answer was a resounding affirmative!

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CHAPTER 21

FIFTIETH ANNIVERSARY CELEBRATION OF THE FOUNDING OF THE LIMNOLOGY DEPARTMENT

The Limnology Department of the Academy of Natural Sciences of Philadelphia celebrated its 50th anniversary on October 4, 1997. Scheduling problems caused its postponement from the actual founding month of June. I had wanted to attend and visit with David Hart, Director of the Patrick Center (the new name for the original Limnology Department); Bernard Sweeney, staff member; and, of course, Ruth Patrick. The rigors of flying into and out of Philadelphia in one day for the two-hour celebration made my attendance impossible. Other considerations about travel concerned my being absence from my wife Jean for as little time as possible because of her Alzheimer's and, also, my allergy to second-hand cigarette smoke. However, I did compose the following laudatio, which was read at the meeting.

For the Limnology Department and its Founder Ruth Patrick:
A Laudatio
or
All I Really Needed to Know Professionally
I Learned in the Limnology Department

John Cairns, Jr.

In June 1948, I served as protozoologist on one of the two field teams who participated in the now famous Conestoga/Brandywine River Studies under the direction of Ruth Patrick. I remained with the Academy until 1966. Each of us knew we were doing something quite different, and every participant was changed forever as a result of this experience. At present, the rest of the academic world is still catching up to Ruth's creative approach, as indicated by these examples.

(1) The river survey teams looked at entire ecosystems; in this particular case, they looked at entire drainage basins. The surveys were done at a fine level of detail; for example, identifications were to species in most cases.

I was able to observe directly a system-level study done with such depth and breadth that it would have been impossible for a single investigator. Individual data sets were enriched by the simultaneous generation of other data sets that, in the aggregate, gave a comprehensive view of the entire aquatic community and its chemical/physical environment. Once an investigator has looked at a system, it becomes so addictive that looking only at one group of favorite organisms becomes impossible. Clearly, system-level studies are now "in." This innovation is but one example of Ruth Patrick's incredible ability to anticipate and even precipitate major trends in science.

(2) The Limnology Department's activities had an unmistakable interdisciplinary thrust from the very beginning. I became accustomed to working with engineers, pharmacologists (W. B. Hart introduced me to the mysteries of toxicity testing with fish), chemists, and, later on, with people from a variety of other disciplines. Initially, of course, I worked with sub-disciplines within my own discipline; this innovation in itself was remarkable for that period when "lone wolf" specialization was the dominant role model.

(3) Mentoring in acquisition of extramural funding was spectacular for those who were observant! Incredible as it may seem, a period existed when the National Science Foundation did not exist and graduate research assistantships, and even graduate teaching assistantships, were practically unknown. Ruth has probably forgotten the following exchange, but I never have! I once asked her how she obtained research money. She replied that she found people were more likely to support research designed to solve problems of interest to them rather than those problems of interest to the investigator. Ruth, how right you were!

(4) Solving the world's problems, such as environmental pollution and degradation, once scathingly referred to as "applied ecology," is now increasingly regarded as an important professional responsibility. In the event of a truly severe, arguably critical, environmental crisis, anyone with a conscience would do something about it. In 1948, abundant evidence showed that such severe problems existed, but the evidence

was not sufficiently compelling to be of interest to the average citizen or even the average scientist. Again, Ruth clearly anticipated the worsening of the environmental situation and contributed greatly to the concept of "use without abuse" of natural systems concept (now called sustainable use of the planet).

(5) As a river survey team member, I became quite accustomed to working with women in science. Only later did I realize how few women worked in science at that time. Ruth did not try to achieve any gender or ethnic (or any other sort) balance—she merely employed the best people for the tasks. On February 9, 1996, at the 25th anniversary of the Association for Women in Science meeting, I was delighted to become a fellow in that organization "for having demonstrated exemplary commitment to the achievement of equity for women in science and technology." My early experiences on the river survey crews with two women, Ruth Patrick and Mary Gojdics, were immensely helpful to the development of my career. I aspired to be their colleague, which made concerns regarding equity for women in science inevitable.

(6) Professional achievement comes with a price. I observed this in my advisor at Swarthmore College, Robert K. Enders; my major professor for both graduate degrees at the University of Pennsylvania, David Wenrich; and some of my committee members, such as L. V. Heilbrunn. However, I did not have much opportunity, nor do most students, to observe how their mentors managed time balances between professional and personal obligations. Ruth almost always had the most comprehensive scientific collections, and this attention to detail is very useful in a scientific career. Even now, when I get fatigued, I can hear her voice, "Well, I'll just get one or two more samples." Those one or two more samples are often the difference between success and failure in this era of statistical analysis.

The portrait of Ruth wearing waders in the Ruth Patrick Center in Aiken, South Carolina, captures the ambiance of both the department and the scientist perfectly. May the department and its founder have many more years of stimulating research, and, Ruth, may your waders never leak!

David Hart let me know that my words were "particularly meaningful to Ruth, as well as to the staff of the Patrick Center as a whole." Although I could not attend the anniversary celebration, the date did mark a half century of professional activities for me as well (my career started before I had completed graduate school). I was saddened to think that, of the original crews for the Conestoga/Brandywine River Basin surveys, only a few members were still alive to attend the celebration: Ruth Patrick (founder); Herbert Levy (now at the Harvard Museum); Stewart Bamforth (now a professor at Newcombe College of Tulane University); Thomas Dolan IV (now the only original team member besides Ruth still living in the Philadelphia area); and possibly James Bergsang (a student at that time from Sweden and, just a few years ago at least, still residing there).

CHAPTER 22

COPING WITH BELITTLERS

One lesson one learns early in life is that one cannot please everyone. An affliction common to the academic world, and not uncommon elsewhere, is belittling the accomplishments of others, particularly in a direct confrontation with the person being belittled. As a caveat, the responsibility rests with scientists and other members of academe to test a hypothesis or concept thoroughly and to show both its weaknesses and strengths, preferably through additional evidence. This testing of a hypothesis is an integral part of the scientific and academic process, without which confidence in the hypothesis or concept is not strengthened. Ideally, the attack on a hypothesis should be separated as much as possible from an attack on the originator of the hypothesis. Belittling, on the other hand, focuses on the ego of an individual and gives very little attention to the ideas, concepts, or hypotheses.

I am best able to keep belittlers in perspective by observing their tactics to denigrate world-class scholars. Belittlers attempt to do so well outside of concept testing, which is an integral part of the scientific process. Often, they distort statements by not quoting them in context. Another common tactic is to quote another belittler, especially one who has achieved notable stature for personal accomplishment.

My favorite incident involving a belittler occurred when a world-class limnologist was belittled in a variety of ways during the discussion period following a seminar. The belittler was a young, untenured professor who insisted on pointing out the ABCs of limnology to the distinguished speaker, who was a member of the US National Academy of Sciences with a substantial number of other honors. Although I was not present, several people whom I had known for years repeated it to me in virtually identical detail. The distinguished limnologist finally leaned forward on the podium and said, "Young man, take some advice from an old man—get psychiatric help!" I had the dubious honor of being belittled under similar circumstances by the same person under the same conditions. I limited my responses to noting that I was well aware of these alternative hypotheses and, in fact, had published on almost all of them personally. Obviously, my response to the belittler had little effect, although, of course, I was a much less distinguished target.

I have often felt that the best response to belittlers is to remain calm, smile serenely if possible, and continue with the concepts that they attempt to belittle. One can even turn the belittler's efforts to a personally beneficial purpose. Sometimes, when I feel too fatigued to continue my research without a break, I recall the most irritating belittler (the person described earlier has served this purpose well for over a decade) and find the energy to finish the job.

One of my former students recently wrote to tell me that my method for handling belittlers was working well for him, although sudden retribution is often more satisfying for some people. Since neither approach may have little effect on the behavior of belittlers, the audience can be trusted to judge the validity of the belittler's remarks. The philosopher Heidegger believed that the self is what we truly care about—if we care about belittlers, we demean ourselves. If belittlers care about "sticking pins in would-be philosophers to watch them deflate," do they really deserve serious attention? It is one thing to test hypotheses rigorously and quite another to attack the individuals who espouse particular hypotheses. The latter annoy me but deserve no more attention than a mosquito.

CHAPTER 23

RETIREMENT

During my childhood, thoughts of retirement never crossed my mind. Accidents in the workplace were not uncommon, and the family or the church cared for unlucky ones. People who escaped injury or had safe jobs worked until infirmities forced them to cease working. A few years later, most died. The exceptions were regarded with awe and respect. I never knew one of my grandmothers, but both grandfathers and the remaining grandmother died at what would now be regarded as an early age. All had health problems that prevented activities now regarded as the "right" of senior citizens.

My father's secure job as a freight solicitor for the New Jersey Central Railroad shielded me from the suffering endured by many during the Depression, but the evidence of the hardship it brought to many was observable even to a six-year old. Short-term survival, rather than retirement, was the preoccupation of the Depression years, which continued, especially for those of us in the armed forces, during World War II.

When I joined the curatorial staff of the Academy of Natural Sciences (ANSP) in 1948, most of the senior curators were well past the normal retirement age. The renowned malacologist Henry Pilsbury died at age 95 with papers in press. When Social Security was proposed by the ANSP administration, some senior curators resisted, fearing they would be required to retire. When reassured that no one would be forced out, they ceased to object, but they clearly had no intent to use a retirement plan. What an example to encounter early in my professional career! Age had neither diminished their zest for their chosen field nor had it eliminated their creative abilities. Impaired, perhaps; diminished, possibly; but I would not have stated these assertions since some of them were outperforming people less than half their age.

The microscopy laboratory, in which I worked for a year or so, was on the hall of Dr. Pilsbury's laboratory/office, which had a glass window in the door. When the smoke from his pipe was in an undisturbed layer about five feet above the floor, no one ever disturbed him because we never knew how he would respond to an intrusion. Clearly, his zest for malacology was still a major factor in his life. Although I almost certainly did not appreciate it fully at the time, I had learned a crucial lesson: do what is enjoyable, and retirement will be relegated to an administrative and financial change, but not a total loss of cherished activities.

This view has been continually reinforced over the years. Ruth Patrick remains enthusiastic about and absorbed in her professional activities. She should be remembered, not as a legendary person, but as a person so excited by her "work" that it was always a special occasion! Nearly four decades ago, I had the good fortune to meet Abel Wolman, who died in his mid-90s. A member of one professional organization told me that Abel had been invited to give an "end of career" address at each of the ages 65, 75, 80, 85, and 90. In each address, he challenged members to explore new problems! Wolman always remained fascinated by a wide array of topics. One of my favorite recollections of Abel Wolman is a picture of him in formal attire, holding a glass of champagne with the pyramids of Egypt in the background. Fortunately, a similar version of this memorable photograph (without champagne bucket or glass) is used on the programs of the annual Abel Wolman Distinguished Lecture of the National Academy of Sciences. These two illustrations are just examples of the people who have shaped my view of retirement. My view of a great retirement is for me to die with manuscripts "in press" and to have a trusted person available to read the galley proofs.

I formally retired in 1995. The greatest personal event in this retirement period was the loss of companionship with my spouse Jean to Alzheimer’s and Parkinson’s afflictions and, finally, her death. Now I live in a world without Jean. I have also been forced to give up hiking the Appalachian Trail, fly fishing in small trout streams with slippery rocks, and swimming. I can now walk several miles daily on paved surfaces and hope to return to woodland trails when my balance improves.

The best professional decision in my retirement years was becoming associated with the Eco-ethics International Union and Professor Otto Kinne. Over half my writing is now on the topic of eco-ethics, sustainability ethics, and related topics. I believe that the conflict between economic growth and sustainable use of the planet is basically a difference in values. Economic growth is basically homocentric, while sustainability ethics attempts to integrate homocentric and eco-centric ethics. Failure to achieve this latter goal will, quite likely, damage both human society and the integrity of Earth's biospheric life support system.

Our children, Karen, Stefan, Duncan, and Heather, have been indispensable in keeping me in touch with the past (by recalling shared experiences) and with the future (by discussing my retirement options and by providing basic training for the electronic age). They have been a blessing in helping me cope with both the loss of Jean and my partial loss of personal independence. Former graduate students have also kept me in touch with my professional past.

One of the most important aspects of retirement is socialization. Throughout my early life, I have had interactions with substantial numbers of people. Going to The Pennsylvania State University in 1940 cut me off from frequent association with people who had been so important in my life. My only daily link with that period of my life was my high school companion and Penn State classmate Arnie Perloff. Going to college was my first experience with loneliness. In those days, private phones were rare, cell phones non-existent. Subsequently, Arnie and I joined fraternities. Mine, Alpha Zeta, was both honorary and social, but it was the latter that initially attracted me. Then, I met Jean, who was my companion for 63 years.

In the first five years of formal retirement, Jean and I hiked together daily and attended many social, musical, theatrical, and religious events. In March 2000 when Jean's Alzheimer's worsened and for a variety of other reasons, we moved to a townhouse in Warm Hearth Retirement Village. However, Jean's condition worsened and she needed 24-hour care in the nursing facility. I suffered less mobility from four spinal compression fractures, and I decided to move to the assisted living center at Warm Hearth. I had companionship at meals and other times, and I was close to the nursing home for visits with Jean. I have a three-room apartment, consisting of a living room, two bedrooms (one converted to a den with computer, printer, and many shelves of books), two baths, and a tiny kitchenette. Three meals are served in the dining room; I spend more time writing instead of buying and preparing food. Housekeeping cleans my apartment weekly and does my laundry. All these services save me significant amounts of time. A minor heart attack in early January 2005 validated my decision to transfer to an assisted living center.

My life is rich, despite the loss of many activities I once enjoyed. Even if I could still do everything I could a decade ago, most of the joy would be gone without Jean's companionship.

Years ago, Jean and I frequently hugged each other. We even had a small card that read: "Good for a Free Hug from any Consenting Adult," which we would give the other person if we felt the need for a hug. Alzheimer's blocked such an exchange. However, every time I visited Jean, I would start with some hugs, then seven "unasked for" kisses (Jean always said these were the best kind), then strokes of my hand on both her cheeks, and finally a back rub. Before I began this ritual, I always said, "This is Johnny right beside you." Linking verbal and non-verbal communication seemed to work—Jean would hold out her arms to me when I held out mine to her.

Compared to Jean's losses, mine were trivial! I could, and still do, take one or two walks on the paved path in the village (I use my walker for balance). With Karen's help in transferring the handwritten first drafts of my writings to the computer and Darla's editing assistance, I have been able to continue an intellectual activity that has always given me great joy.

Each year, I send an annual report to the Head, Department of Biological Sciences; Dean, College of Sciences; and University Provost. Since my formal retirement, I have prepared such a report (Appendix 5) each year, although it is not mandatory to do so.

For five months after Jean's death, I was in shock, even though I had already realized that our remaining time together was short. During this time, Kathy Brady, Jean's friend and roommate, and I shared memories of the happy time when I first met Jean. For months, Kathy and I had weekly talks, reconstructing that period. This interaction helped enormously to turn my attention

for loss to the wonderful years that Jean and I shared. I am now adding more images from that period to offset those from the last few years when Jean' continually expressive face became less so.

The National Research Council Committee that I chaired when the volume *Restoration of Aquatic Ecosystems: Science, Technology, and Public Policy* was prepared gave me a tee shirt that was inscribed: "Stochastic Events Happen." This inscription is good to remember in life, but especially in retirement and old age.

APPENDIX 5
Annual Report 2004

Professor: John Cairns, Jr.
Department: Biological Sciences

FACULTY ACTIVITIES REPORT
2004

Reporting Period: Spring Semester 2004
Summer 2004
Fall Semester 2004

I. INSTRUCTIONAL ACTIVITIES

A. Courses Taught: NONE

B. Course Development: NONE

C. Advising: Since I chaired 74 graduate committees and served on over 200 committees chaired by others, I write at least one letter of recommendation per month, often more.

D. Connections to Research and Outreach: (1) Continue to assist faculty member Charles Jervis, Auburn High School, Riner, Virginia, with his special environmental program
(2) Was involved with two different graduate programs at Radford University

E. Professional Development: NONE

F. Honors and Awards: (1) Elected on 8 June 2004 as Honorary Member (its highest honor) of the World Innovation Foundation founded by Nobel Laureate Glenn Seaborg
(2) Invited plenary session speaker (my address will be read by a colleague in the Russian National Academy of Sciences) at the Symposium "The Future of Life on Earth and of Our Civilization," 2-6 May 2005 at Frankfurt University, Germany

II. RESEARCH AND/OR CREATIVE ACTIVITY

Publications Since the 2003 Report

Books

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Articles in Peer-reviewed Journals

1. Cairns, J., Jr. 2003. The unmanaged commons: A major challenge for sustainability ethics. The Social Contract XIV(2):136-145.

2. Cairns, J., Jr. 2004. Self-regulating versus subsidized ecosystems. Int. J. Sustain. Develop. World Ecol. 11:36-47.

3. Cairns, J., Jr. 2004. Sustainability ethics: World population growth and mitigation. Mankind Quarterly XLV:169-194.

4. Cairns, J., Jr. 2004. Sustainability ethics: Zero net immigration. The Social Contract XV(1):58-71.

5. Cairns, J., Jr. 2004. Taboos and denials: Major obstacles to sustainable use of the planet. Sci. Soc. 2(1):15-24.

Articles in Internet, Peer-reviewed Journals

1. Cairns, J., Jr. 2003. Tribute to Garrett Hardin. Garrett Hardin Society. http://www.GarrettHardinSoc.org/tributes/tr_cairns_2003sept.html

2. Cairns, J., Jr. 2003. The unmanaged commons: A major challenge for sustainability ethics. Garrett Hardin Society. http://www.thesocialcontract.com/cgi-bin/showarticle.p1?articleID=1206terms=

3. Cairns, J., Jr. 2004. Allocating finite resources on a finite planet. Ethics in Science and Environmental Politics http://www.esep.de/articles/esep/2004/E47.pdf

4. Cairns, J., Jr. 2004. Choosing model nations to set examples for achieving sustainability. Ethics in Science and Environmental Politics http://www.esep.de/articles/esep/2004/E46.pdf

5. Cairns, J., Jr. 2004. Coping with ecological catastrophe: Crossing major thresholds. Ethics in Science and Environmental Politics http://www.int-res.com/articles/esep/2004/E56.pdf

6. Cairns, J., Jr. 2004. The ethics of global resource allocation. Ethics in Science and Environmental Politics http://www.int-res.com/articles/esep/2004/E58.pdf

7. Cairns, J., Jr. 2004. Future of life on Earth. Ethics in Science and Environmental Politics http://www.esep.de/articles/esep/2004/E41.pdf

8. Cairns, J., Jr. 2004. Is human society in denial regarding the tough questions about sustainability? Ethics in Science and Environmental Politics http://www.int-res.com/articles/esep/2004/E54.pdf

9. Cairns, J., Jr. 2004. Life defining experiences B Rocky Mountain Biological Laboratory. http://www.rmbl.org

10. Cairns, J., Jr. 2004. Remaining on stage in the planetary theater. Ethics in Science and Environmental Politics http://www.int-res.com/articles/esep/2004/E51.pdf

11. Cairns, J., Jr. 2004. Second tribute to Garrett Hardin. The Garrett Hardin Society. http://www.GarrettHardinSociety.org/tributes/tr_cairns_2004mar.html

12. Cairns, J., Jr. 2004. Small islands: Harbingers of Earth=s ecological fate? Ethics in Science and Environmental Politics http://www.int-res.com/articles/esep/2004/E48.pdf

13. Cairns, J., Jr. 2004. Sustainability and the anthropogenic alteration of evolutionary processes. Ethics in Science and Environmental Politics http://www.int-res.com/articles/esep.2004/E55.pdf

14. Cairns, J., Jr. 2004. Sustainability ethics matter. Ethics in Science and Environmental Politics http://www.esep.de/articles/esep/2004/E42.pdf

15. Cairns, J., Jr. 2004. Sustainability ethics: tales of two cultures. Ethics in Science and Environmental Politics http://www.int-res.com/articles/esep/2004/E50.pdf

16. Cairns, J., Jr. 2004. Sustainability and specialization. Ethics in Science and Environmental Politics http://www.int-res.com/articles/esep/2004/E49.pdf

17. Cairns, J., Jr. 2004. Will the real sustainability concept please stand up? Ethics in Science and Environmental Politics http://www.int-res.com/articles/esep/2004/E53.pdf

18. Cairns, J., Jr. 2004. You and Earth's resources. Ethics in Science and Environmental Politics http://www.esep.de/articles/esep/2004/E45.pdf

Reviews/Miscellaneous

1. Cairns, J., Jr. 2004. Review of The Culture of Extinction: Toward a Philosophy of Deep Ecology by Frederic L. Bender. Quarterly Rev Biol. 79(3):329-330.

2. Cairns, J., Jr. 2004. Review of The Essential Agrarian Reader: The Future of Culture, Community, and the Land by N. Wirzba, ed. New River Free Press 22(1):6.

3. Cairns, J., Jr. 2004. Review of Just Sustainabilities: Development in an Unequal World by J. Agyeman, R. Bullard, and B. Evans, ed. New River Free Press 22(2):6.

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3. Cairns, J., Jr. In press - Invited chapter. Ethical issues associated with ecological restoration. In S. Sharma, ed., Perspectives in Environmental Research. Scientific Publishers, Jodhpur, India.

4. Cairns, J., Jr. In press. Forward. In M. V. Reddy, ed., Tropical Eutrophic Lakes: Their Restoration and Management. Oxford and IBH Publishing Co., Pvt. Ltd., New Delhi, India and Science Publishers, Inc., New Hampshire.

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2. Cairns, J., Jr. In press. The age of transition to sustainability: The end of the exponential growth period. Politics and the Life Sciences.

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4. Cairns, J., Jr. In press. Biographical memoir: Garrett Hardin. Proc. Am. Phil. Soc.

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3. Cairns, J., Jr. Submitted. Review of A Poverty of Reason: Sustainable Development and Economic Growth by Wilfred Beckerman. Int. J. Sustain. Develop. World Ecol.

4. Cairns, J., Jr. Submitted. Review of Red Sky at Morning: America and the Crisis of the Global Environment by James Gustave Speth. New River Free Press.

In Preparation Since the 2003 Report

Books

1. Cairns, J., Jr. In prep. Avoiding a Posthuman World.

2. Cairns, J., Jr. In prep. Forum organizer and editor – Consilience of Economics and Ecology: Ethics in Action or Inaction? Theme Section, Ethics in Science and Environmental Politics, Eco-Ethics International Union, Oldendorf/Luhe, Germany.

Articles in Peer-reviewed Journals

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2. Cairns, J., Jr. In prep. The effects of the biotic crisis on evolution and the carrying capacity.

3. Cairns, J., Jr. In prep. Measuring your personal impact on Earth. New River Valley Free Press.

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5. Cairns, J., Jr. Invited paper. The relationship between ecological restoration and sustainable use of the planet. Restoration Ecology.

6. Cairns, J., Jr. In prep. Sustainability and the global commons. Sust. Comm. Rev.

7. Cairns, J., Jr. In prep. Thresholds, application factors, biological monitoring, and the precautionary principle. Environ. Pract.

Articles in Internet, Peer-reviewed Journals

1. Cairns, J., Jr. In prep. Ethics for a finite planet. Ethics in Science and Environmental Politics.

2. Cairns, J., Jr. In prep. Foregoing personal gratification for persons and species unknown. Ethics in Science and Environmental Politics.

3. Cairns, J., Jr. In prep. How many people will nature permit? Ethics in Science and Environmental Politics.

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5. Cairns, J., Jr. In prep. Newspaper articles, website references, the popular press, and ESEP. Ethics in Science and Environmental Politics.

6. Cairns, J., Jr. In prep. Unsustainable living: A major failure in humankind's eco-ethics. Ethics in Science and Environmental Politics.

III. SERVICE

A. Public Service: Assisted a number of communities, citizen groups, and both private and public organizations with development of sustainability policies

B. University Service: I gave a seminar to Professor Marion Ehrich's course, Pharmacology and Toxicology Testing

C. Professional Service:

Section Editor, Aquatic Toxicology, Toxicology and Industrial Health, 1983-present
Section Editor, Aquatic Toxicology, Journal of Environmental Pathology, Toxicology and Oncology, 1983-present
Editorial Board, Aquatic Toxicology, 1989-present
Advisory Board, Environmental Professional (currently Environmental Practice) 1988-present
Editorial Board, Hydrobiologia, 1979-present
Editorial Board, Ecotoxicology, 1991-present
Editorial Board, Issues in Environmental Science and Technology, 1992-present
Consulting Editor, Journal of Aquatic Ecosystem Stress and Recovery, 1997-present
Associate Editor, Ecosystem Health and Medicine, 1993-present
International Editor, Journal of Environmental Sciences China, 1995-present
Board of Editors, Ecosystem Health, 1995-present
Editorial Board, Sustainable Communities Review, 1997-present
Advisory Board, Journal of Frontier Sciences, 1998-present
Editor, Ethics in Science and Environmental Politics, 2000-present
Editorial Board, Common Ground, 1999-present
Advisor, Asian Journal of Experimental Sciences, Elsevier Publishers, 2004-present

IV. STATEMENT OF PLANS AND GOALS FOR ACADEMIC YEAR 2005:

I hope my continuing recovery from four spinal compression fractures will soon permit acceptance of the invitations that have been piling up to give seminars.

The third draft of my professional autobiography is nearing completion and should be ready to distribute to 17 reviewers in a few months.

I have been using peer-reviewed Internet journals frequently because they reach a much broader, sizable readership.

I have a moderate amount of work done on a book *Avoiding a Posthuman World* and will give Part I major attention as soon as my autobiography is in the final stages of publication.

A colleague in the National Academy of Engineering and a publisher have been urging me to get busy on a forum titled "Consilience of Economics and Ecology: Ethics in Action or Inaction?" which I would organize and then serve as editor of the volume.

I have invitations from two journals in which I have not published before to submit a manuscript and open invitations form journals in which I have already published. There are enough manuscripts in various stages of completion to occupy my time for the next year.

Since I have wanted to increase my activities locally, I have been writing book reviews for the *Free Press* because it has a strong interest in environmental matters. Since the editors welcome these reviews, I expect to continue writing them for some time.

I am now nearing 1,500 publications and am deeply indebted to Bob Jones for encouragement and support in continuing to publish. My editorial assistant Darla Donald has been extremely helpful for 30 years in keeping publications flowing—I hope this productive relationship continues for many years.

Signed _____

Date _____

PUBLICATIONS

CAIRNS' PUBLICATIONS SINCE THE 2003 REPORT

	Published	In Press	Submitted	In Preparation
Books	1	0	1	2
Chapters	0	6	5	0
Articles (including electronic)	23	8	2	13
Abstracts	0	0	0	0
Reviews	7	2	4	0
TOTAL	31	16	12	15

Total Publications for 2003 = 59 (excluding in preparation publications)

Career Total Publications = 1489

CHAPTER 24

EPILOGUE

As I approach the end of my professional career, the question inevitably arises: "is the environment better off as a result of my efforts?" On 30 June 1997, National Public Radio Morning News mentioned that my home state of Pennsylvania leads the nation in importing wastes from other states and my adopted state of Virginia is second. Such information is daunting!

Furthermore, the world population has more than doubled in my lifetime; per capita material possessions have increased dramatically at the expense of natural ecosystems; persistent anthropogenic hazardous materials are more numerous; species extinctions are unacceptably high; and exponential growth is more rapid than many systems can handle. Obviously, major ecological, social, and political forces that are operative may be beyond the control of human society, so the impact of one person is miniscule. Arguably, the environmental condition might be significantly worse if a large group of people had not advocated the policy of use without abuse of the environment. In addition, the framework for a major paradigm shift to a new relationship between human society and natural ecosystems has been developed should either reason or consequences make this shift likely.

Although I have published frequently over a half century, most publications are not long remembered. Despite this, shifts in the development of a field do occur, although many practitioners are unaware of the role of the individuals involved. Moreover, one need not have evidence of major changes to feel satisfaction in one's efforts. I have experienced great joy in learning from a distant colleague that a particular publication of mine was a "good read"! Reprint requests and electronic communications bring a satisfaction that someone, somewhere is paying attention to my efforts. The greatest number of people I have reached is undoubtedly through publications. I employed an undergraduate for most of my tenure at Virginia Polytechnic Institute and State University whose job was to fill reprint requests daily. Arguably, the major personal benefit was the reciprocal exchange of reprints. A few of the requests even resulted in decades of correspondence.

I get particular satisfaction from supportive comments from editors and anonymous reviewers; both have helped strengthen manuscripts markedly and aided me in maintaining my zest for writing. Even negative comments have added to the zest, if not the joy. My long professional journey has been possible in a large degree to the steadfast support and skilled efforts of Darla Donald, editorial assistant; B. R. Niederlehner, senior laboratory technician; and a number of other colleagues. An old axiom states that the journey is the most important factor, not the destination—I agree!

Although I was not in a position to chair graduate committees until I went to the University of Kansas in 1966, I have chaired or co-chaired 49 PhD committees and 25 MS committees. I have served as a member of nearly 200 other graduate committees and have been the mentor for over 500 honors and other undergraduate research projects. I like to believe that I have had some influence over the careers of the students in the courses I taught, particularly during the years (about equally divided) from 1961 through 1994 at Rocky Mountain Biological Laboratory and the University of Michigan Biological Station. Generally, my association with the students at the field stations is much more memorable since both the students and I saw each other daily at meals, social activities, and seminars, as well as in class. In addition, classes at field stations tend to be 24 students or less and are almost always in more exotic surroundings than the typical university campus. A number of students have written that working with me during the summer changed their lives because they felt they could do something about environmental pollution. Such letters are always a source of satisfaction and joy. Not surprisingly, I met many of my graduate students for the first time at field stations, where we had a better opportunity to judge how well we would get along with each other better than is possible on a university campus or by correspondence.

Finally, some small (probably immeasurable) effects may have resulted from my off-campus seminars. During one academic year in the 1970s, I gave 52 off-campus seminars in a 36-week

period. Comments from people who remember the seminars and keynote addresses that were given decades ago indicate some lasting effects. Unquestionably, personal benefits were many because I found out about the research of others, met prospective graduate student with similar interests, and was able to test my ideas with persons with a broad array of viewpoints and disciplines.

All these associations have been immensely satisfying, although I experienced some of the usual frustrations associated with the academic world. The strongest evidence of satisfaction with my career is that I have continued working well past the age when retirement was financially possible. I am still working over a decade after having formally retired.

In June 1997, I prepared a "futures paper" for the International Joint Commission (Canada and the United States) and completed a chapter for a book on ecotoxicology. Even so, time management problems were still with me; I had increasingly shared time with my spouse Jean, spent time on meditation (begun in the mid-1960s), and began reading non-scientific literature on history, philosophy, and poetry. Throughout my professional career, Jean and I had enjoyed folk dancing, concerts, plays, and hiking. For much of my life, I had enjoyed fly fishing for trout with barbless hooks. Once a favorite past time, particularly in summers, fly fishing has disappeared for me as aging has prohibited climbing over slippery rocks with any agility. I can recall how everything fell into place for seemingly intractable problems after I stopped thinking about them and went fly fishing, to a concert, hiking, or, until the last seven or so years, a daily one-mile swim weekdays. I miss fly fishing as an aid to consilience, but some of the other activities remain.

The Depression left its mark, and, although I no longer save bits of string and wind them into a ball, the frugal attitude of that era left an indelible mark upon both my spouse Jean and me. Although recycling is now the "in" thing, during the Depression nearly everyone practiced it with fervor. American society has merely reverted to earlier practices, which always had merit. However, many people in all economic brackets refuse to recycle, so we are far from Depression-era efforts. Many of my contemporaries from that period have managed to suppress or abandon the Depression-era mentality, but Jean and I never did because having few possessions always enabled us to lead a more tranquil life. I pay the same rates for trash service, although I have less than 5% of the volume/weight disposed of by other households. I am apprehensive about the future of the planet if every family on the planet reaches this level of material consumption. In this category, I am a curiosity rather than a model.

The standard of living in terms of material goods was far lower during the Depression than that considered necessary these days. Again, this contrarian position seems unlikely to become widespread, absent a major economic downturn, government control, or a revolution—all less attractive than a change in societal behavior. Durant and Durant (1968) note that, every time in history that the disparity in affluence between the rich and the poor becomes too great, the problem is resolved either by revolution or by government partial redistribution of wealth. Now that we are rapidly becoming a global society and have a global economy, I wonder how this redistribution will happen! An attempt to raise the standard of living for the entire world will almost certainly prove fatal to what remains of natural systems, and war and revolution will probably not do natural systems much good either. Despite a sense of foreboding, I continue to work on the possibility of leaving a habitable planet for our descendants because it is a fascinating intellectual problem.

For the past two decades or more, I have been considerably comforted by the fact that, while human society can commit suicide, it cannot extinguish all life on Earth. Recent evidence of microbial species at depths below Earth's surface (formerly thought to contain no life) and the existence of life in thermal vents in the ocean floor, together with many other similar situations, reinforce this belief. This situation, coupled with the evidence of proliferation of a wide diversity of life forms following great extinctions, indicates that, regardless of the outcome of the present ecological crisis, some form of life will exist on Earth and, presumably over geologic time, achieve considerable complexity. Since organisms with quarter-ounce brains or less seem to utilize resources more efficiently and sustainably than humans do, perhaps they are the meek that shall inherit Earth. I am pained to contemplate that great artists, musicians, and other creative talents might disappear entirely. However, this outcome may be the inevitable price of "progress."

Consequently, one must keep doing one's best to espouse sustainability. The persistence may actually have some impact, and, if not, it is an interesting intellectual exercise with the great benefit of keeping in touch with interesting people.

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CHAPTER 25

POSTPONING THE QUEST FOR SUSTAINABILITY: SURVIVAL FIRST – THEN SUSTAINABILITY

I've given up on the whole human species. I think a big, good-sized comet is exactly what this species needs. You know, the poor dinosaurs were walking around eating leaves, and they were completely wiped out. Let the insects have a go. You know, I don't think they'll come up with sneakers with lights in them, or Dust Busters, or Salad Shooters, or snot candy . . . I only wish there were some way I could live out on the moon and watch it all on CNN . . . I just want to describe the mess. But life is dual. If you scratch a cynic, you'll find a disappointed idealist.

George Carlin, The Progressive, July 2001

It ain't over til it's over. Yogi Berra

I rejoice to live in such a splendidly disturbing time. Helen Keller

Humankind's reckless use of fossil energy has created a situation in which Planet Earth is in imminent peril (Hansen et al. 2007). Lovelock has stated that climate change is already insoluble, and life on Earth will never be the same again (The Independent 2007) and that billions will die this century (Bloomberg 2006). I have decided that humankind is creating an alien planet (Cairns 2007a). Heinberg (2005, p. 31) predicts the loss of over 3 billion people by 2200 as a result of passing peak oil. All the above predictions could be wrong, but the preponderance of evidence indicates that rough times are already here and almost certain to become worse – hardly the conditions for achieving sustainable use of the planet. Still, both social and environmental stability may again be achieved and provide another opportunity to engage in a quest for sustainable use of the planet.

For decades I was convinced that sustainable use of the planet was not only highly desirable, but others, if they saw the same evidence I have seen, would feel an equally strong motivation to work toward this end. However, this outcome has not happened, and, now, persuasive (although not conclusive) evidence indicates that some important ecological thresholds and breakpoints have been crossed. Since greenhouse gas emissions are still increasing and peak oil either has passed, or soon will pass, the situation is likely to worsen. This worsening is both because of the long residence time of carbon dioxide in the atmosphere and because of the effects of the loss of cheap, abundant petroleum upon technological societies.

The Bad News

One can easily become depressed about the future of humanity. Some recent incidences point to the continuing lack of attention to mainstream science and its conclusions. In the United States, a young man, with no scientific credentials and not even a BS degree (he has since obtained one), was assigned the responsibility of checking the research publications on global heating that was authored by a world-class scientist (James Hansen). The US Senate attempted to honor Rachel Carson, author of the paradigm shifting book *Silent Spring*, on her 100th birthday, but Senator Tom Coburn (Rep., Oklahoma) objected (Zabarenko 2007). Senator James Inhofe (Rep., Oklahoma) has repeatedly called global heating science the greatest hoax ever perpetuated on the American public. US President George Bush has refused to even discuss limiting greenhouse gas emissions. In my hometown of Blacksburg, Virginia, some people drive alone to "save the environment" meetings in gas guzzling sport utility vehicles. The list goes on and on. The contrast of the United States with what many European nations are doing to reduce greenhouse gas emissions is shocking, especially

since Europeans are leading quality lives while attempting to protect both posterity and natural systems.

The Good News

The New Economics Foundation Report (NEF 2006) provides persuasive evidence that life satisfaction/happiness is not highly correlated with material possessions or profligate use of energy. Hawken (2007, p. 184) states:

> *It has been said that we cannot save our planet unless humankind undergoes a widespread spiritual and religious awakening. In other words, fixes won't fix unless we fix our souls as well. So let's ask ourselves this question: Would we recognize a worldwide spiritual awakening if we saw one?*

Will Reason Prevail?

One of the US founding fathers, Thomas Jefferson, described the American political experiment as follows: "Which we trust will end in establishing the fact, that man may be governed by reason and truth. Our first object should therefore be, to leave open to him (the citizen) all the avenues to truth" (Gore 2007, p. 100). However, Gore noted (2007, p. 13): "Five years after U.S. President Bush first made his case for an invasion of Iraq, it is now clear that virtually all the arguments he made were based on falsehoods." Gore further noted (2007, p. 104): "We were told that the president would give the international system every opportunity to function, but we now know that he allowed the system to function only briefly as a sop to his secretary of state and for cosmetic reasons." The reason for including this information on the falsehoods that put the United States in the Iraq war is that, arguably, the greatest threat to human security is global heating and other types of climate change, not terrorists or the "aggression" of other nations. Terrorists have killed thousands of people, but rapid climate change will probably damage the agricultural system's production of foodstuffs and will kill or harm millions.

Not surprisingly, Monbiot (2007) has found falsehoods in the global heating debate. For example, he cites examples of alterations of climate scientist James Hansen's US Congressional testimony in 1988 by Patrick Michaels, who deleted the "most plausible" and the "least damaging" scenarios presented by Hansen. Events have shown the middle scenario ("most plausible") correct. Unlike Cockburn (2007), who provided no references in his column attacking peer review, Monbiot provides 14 references for his short commentary and asked Cockburn, "Where are the (your) references?" Monbiot's other major example of falsehoods was a denial by a former president of the US National Academy of Sciences, Frederick Seitz, that manmade climate change is happening. The document, known as the "Oregon Petition," maintained that the production of extra carbon dioxide was good for the planet (can be read at http://www.desmogblog.com/files/IREA-memo.pdf). The Council of the National Academy made clear that the report does not reflect the conclusions of expert reports of the Academy (Council of the National Academy of Sciences 1998). Gore (2007, chapter 4) gives a collection of "convenient untruths" that defy reason.

Global Tipping/Breakpoints

A tipping point/breakpoint occurs when a system is so stressed that it goes into disequilibrium. Eventually, a new equilibrium point may be established, but it is unlikely to closely resemble the original one. The driving force pushing toward these multiple tipping points of complex ecological and societal systems is global human population increase. The human population has quadrupled in just one century, as if humankind lived on an infinite rather than a finite planet (http://www.overpopualtion.net). Even the present population will not be sustainable after peak oil. Both climate change and ocean acidification are the result of anthropogenic greenhouse gas emissions, and both are the result of fossil fuel combustion by humans and, therefore, under their control. The goal, of course, is to stabilize the world population at a

sustainable level, taking into account the probability of reduced agricultural production due to both climate change and less petroleum for fertilizer and mechanized agriculture. One fantasy in the United States is that biofuels will replace petroleum, but, as Grant (2007, p. 15) notes: "It might take roughly 350 million hectares – equal to one-quarter of the world's arable land – to replace something like 5 percent of the present world fossil energy production." This situation is reminiscent of the period when about one-quarter of US cropland a century ago was used to raise draft animals (Grant 2007, p. 27).

Living in a Low Energy Society

I was born on May 8, 1923 – a year in which the US population was 111,947,000 – just over one-third of the 300+ million at present. In winter, my family's small, semi-detached home was kept cool (about 65ºF) by today's standards. We had one radio, no telephone, and my father used the family car for business, but we did take short trips (about 20 miles) on some Sundays to visit relatives. I walked to school each morning, back home for lunch, and returned when school closed – one-half mile each way. Well beyond the edge of town was Potts Quarry, my favorite fishing site. A 10-minute bike ride would get me there. The Schuylkill River, polluted but with fish, was one mile away. Summer vacations (two weeks for my father) were spent at Somers Point, New Jersey – about 75 miles away. My boyhood included the US Great Depression, which fostered some common advice:

Wear it out
Use it up
Make it do
Do without.

My companion Jeannie stitched a piece of burlap, at our daughter Karen's request, with these words in different colors.

Neither Jeannie nor I had a desire to have many material possessions and never felt deprived without them. Humankind's low energy era will doubtless be different since the human population is much larger now than it was then. The amount of arable land has not increased, but highly mechanized agriculture and agricultural research have made population increases possible. The end of cheap, abundant energy, combined with rapid climate change, will decrease production of foodstuffs so the era of cheap, abundant food will soon be over, as will the era of cheap, abundant material goods. However, social capital (interactions with other humans) should return to the higher level of the low per capita energy era. In short, an era of low, but efficiently used, per capita energy could be much better than many people think it could.

Ecological Overshoot

Ecological overshoot is using resources more rapidly than they are being regenerated. Inhabitants of the United States and, to a lesser extent, other developed countries tend to view the planet as a vast cornucopia that can spew forth endless foodstuffs, material goods, and cheap gasoline. This point of view was reinforced by perpetual economic growth economists (e.g., Simon 1981) who believed that, when a resource was depleted, human ingenuity and creativity would provide a substitute. Simon (Myers and Simon 1994) was sufficiently optimistic to state: "We now have in our hands the technology to feed, clothe, and supply energy for the next seven billion years." In 2007, this statement sounds overly optimistic. Although Simon's views received strong endorsement from some of the financial news media, contrary views existed (e.g., Boulding 1966; Orr and Ehrenfeld, 1995). Weston (1995) notes that people often attempt, in the race to "do something," to solve a problem without understanding its philosophical and technical elements. This strategy often leads to failure, miscommunication, and delay in real progress. This approach is quite evident in the effort to replace petroleum with biofuels, coal, and nuclear energy. To replace the resources lost by ecological overshoot, the remaining resources must be used more effectively. The tendency to avoid energy conservation by basing hopes on unproven technologies is more than matched by the apathy displayed in coping with the rapidly accelerating global heating crisis. Lean

(2007) notes that carbon dioxide emissions have been rising at three times the rate of the 1990s. Lean's observation was based on a report of the US National Academy of Sciences, which indicated that carbon dioxide emissions have been increasing by about 3% per year in this decade compared with 1.1 % per year in the 1990s. Connor (2007) discusses the effects of increased greenhouse gas emissions in Antarctica. He notes that fears that global sea level rise may be faster and further than expected are supported by a study showing that 300 glaciers in Antarctica have begun to move more quickly into the ocean. Despite the serious nature of these events, Heilprin (2007) reports that the administration of US President George Bush is drastically scaling back efforts to measure global heating from space, just as President Bush tries to convince the world the United States is ready to take the lead in reducing greenhouse gases (Revkin 2007). At the Group of 8 Meeting, US President Bush effectively derailed a climate change initiative that was backed by Chancellor Angela Merkel of Germany (Stolberg 2007). When attempting to address a global problem, such as climate change, common goals and time lines are essential. Otherwise, validation of various approaches favored by different nations or groups of nations will not be possible. Orr (2000) stated:

> *No person, institution, or nation has the right to participate in activities that contribute to large-scale irreversible changes of the earth's biogeochemical cycles or that undermine the integrity, stability, and beauty of its biotic systems; the consequences of such activities would fall on succeeding generations as an irreversible form of remote tyranny.*

Accepting Limits

Living sustainably requires both accepting limits and living within them. Wackernagel et al. (2002) have provided persuasive evidence that, in 2002, the ecological overshoot was 24%. This figure demonstrates clearly a massive failure to accept limits. Such an ecological overshoot is stealing from future generations, which living sustainably seeks to prevent. Sustainable use of the planet aspires to connect the future to the present. As Hardin (1993, p. 111) states: "Binding the future to the present makes sense only if understandable mechanisms connect the two." How did humankind get into a situation where its entire future rests upon its response to global heating and peak oil? Humankind arrived at this situation through numerous possibilities, but a few stand out.

Failure to establish a default status

Hardin (1993, p. 40) uses as an example the process of determining guilt. Evidence that leads to one conclusion is common in the United States or England – in default of absolute knowledge, a conclusion is based on the premise of "Innocent until proven guilty." In France (Napoleonic Law), the rule is "Guilty until proven innocent." Hardin's essential point is that one or the other default positions must be embraced since absolute proof can rarely be demanded, particularly with complex, multivariate issues such as global heating and peak oil. In science, the default position is the preponderance of evidence. Regrettably, the US political system has focused on the uncertainties of science, which, on global problems, will always exist. This situation has seriously, possibly fatally, delayed major remedial action.

Loss of faith in the power of reason

In his book "The Assault on Reason," former US Vice-President Al Gore (2007, p. 1) quotes a comment of US Senator Robert Byrd (West Virginia) on the Senate floor just before the United States launched the invasion of Iraq:

> *This Chamber is, for the most part, silent – ominously, dreadfully silent. There is no debate, no discussion, no attempt to lay out for the nation the pros and cons of this particular war. There is nothing. We stand passively mute in the United States Senate.*

Gore notes that Byrd was really voicing a question that millions of American citizens have been asking: "Why do reason, logic, and truth seem to play a sharply diminished role in the way America now makes important decisions?"

One reason is that persons in positions of power make statements that are not based on reason, evidence, and logic. For example, Holtz-Giménez (2007) calls attention to the assumption that the term *biofuels* evokes the image of a clean and inexhaustible, renewable energy, confidence in technology, and a power of progress compatible with the lasting protection of the environment. Holtz-Giménez (2007) uses easily obtainable information to show that the expectations of biofuels for the future are not likely to be realized: ". . . the attraction of these biofuels resides in the fact that they might prolong the oil-based economy . . ." Biofuels also offer the temptation to continue the present, energy profligate, high energy consumption lifestyle. Energy conservation does not appear to be a major component of this appeal.

Arguably, the worst misleading statements were related to economics. Julian Simon's statement on growth even had a time frame – 7 billion years. Anyone with the slightest familiarity with exponential growth realizes that perpetual growth in population or resource consumption on a finite planet is impossible. However, Simon's publications and views were widely disseminated by the financial news. Hardin (1993, p. 190) provides some illustrative statements from some notable people in the financial world.

> *Prosperity has no fixed limits. It is not a finite substance to be diminished by division. On the contrary, the more of it that other nations enjoy, the more each nation will have for itself.*
>
> Henry Morgenthau, US Secretary of the Treasury, 1933

> *I cannot conceive a successful economy without growth.*
>
> Walter Heller, US President's Council of Economic Advisors, 1962

> *The existing propensities of the population and polices of the government constitute claims upon GNP itself that can only be satisfied by rapid economic growth.*
>
> US President's Council of Economic Advisors, 1971

> *Never has growth been more important. You can never feed the poor or ease the lives of the wage-earning families, ameliorate the problems of race or solve the problems of pollution without real growth.*
>
> John B. Connally, US Secretary of the Treasury, 1972

However, contrary opinions have been voiced by leaders:

> *In a finite world, high growth rates must self-destruct. If the base from which growth is taking place is tiny, this law may not operate for a time. But when the base balloons, the party ends: A high growth rate eventually forges its own anchor.*
>
> Warren E. Buffet, one of America's most successful investors, 1990

A tendency exits to demonize or at least place blame on the erring prophets who "mislead" us, but actually the blame is ours for not requiring evidence, identifying the default position, or, at the very least, determining if the reasoning is suitable for a finite planet with finite resources. If the concept is very attractive, we should be even more cautious.

We're Warned: Out of Gas

In June 2007, the Special Investigations Unit of Cable Network News (CNN) aired a number of times a 1-hour documentary with the above title. The basic message was that the world supply of petroleum is finite and delivery to users could suffer major disruptions from hurricanes or sabotage. Years ago, physicist J. H. Fremlin (1964) asserted that perpetual economic growth required a perpetual increase in humankind's use of energy. Once fossil fuels (ancient sunlight), especially petroleum and natural gas, become increasingly scarce, humans will have to depend increasingly on energy from sunlight (e.g., wind, solar, biofuels) (Cairns 2007b) or nuclear energy, which has severe disadvantages. For example, Elash (2007) reports that France may have to reduce output or shut down 17 of its 58 nuclear power plants. She quotes David Lochbaum, Director of Nuclear Safety at the US-based Union of Concerned Scientists: "People say that nuclear power is going to solve global warming, but I think we're going to have to solve global warming if we're going to have a future for nuclear power." Have we no regard for posterity and the other life forms with which we share the planet? How can we break this deadly lifestyle?

First, Do No Harm

The ancient Greek father of medicine Hippocrates provides the above standard for those entering the practice of medicine, and humankind should have taken that oath with regard to Earth before fossil fuels were discovered. However, the dominant "solutions" to our present problems are "more of the same" – that is, the profligate use of energy will continue. So, we will continue to pour more carbon dioxide and hazardous chemicals into the environment. We apparently do not intend to undertake substantive measures to eliminate ecological overshoot.

Economy vs Environment

Frequent statements are offered that fighting global heating might be acceptable if doing so had no adverse effects upon the economy. However, the environment is humankind's biospheric life support system, which has maintained conditions favorable to our species (and many others) for all of the time humans have been on Earth. If the biospheric life support system is altered so that its functions are drastically changed, *Homo sapiens* will find its society markedly damaged and, in a worst possible case scenario, the species will become extinct. In either case, our precious economy will be badly damaged or even cease to exist. Our economy and environment are inextricably linked, and to pretend they are not is irrational.

Peak Oil

Although coal was important to the Industrial Revolution, the versatile, cheap petroleum provided the most important impetus. However, world oil discoveries peaked at 56 billion barrels per year in 1966, and discoveries are much lower at present. At the same time, the new economic power of China increasingly makes it a major competitor for world oil. The world population is still increasing markedly, so the new oil discoveries will provide even less oil per capita. Actually, much higher gasoline prices will be a blessing because railways, other public transportation, and bicycles, will finally get the attention they deserve. This change is essential because even coal and uranium will become depleted in a few hundred years – much less, of course, if humankind tries to maintain the cheap energy party going at its present rate of consumption. The present response to the energy crisis (i.e., forget conservation – depend upon unproven technologies) is a major reason for concluding that the quest for sustainability should be postponed. The Europeans, now paying over US$7 per gallon for gasoline, must view Americans, who are paying less than US$4 per gallon, with astonishment.

Anthropocentrism vs Ecocentrism

Anthropocentrism is the view that humans are the most important beings on Earth. This view is typical of Judeo/Christian culture, but is now one of the important practices of economic globalization, which is demonstrated by an inclination to evaluate reality exclusively in terms of

human values. Carried to an extreme, anthropocentrism considers that every aspect of the universe is for human benefit. This extreme view is evident in discussions of colonization of other habitable planets to relieve human overpopulation problems.

Ecocentrism recognizes that the ecosphere is the source and support of all life and espouses an ecocentric approach to all human activities. Cairns (2007c) assets that the first step toward ecocentrism is to regard the relationship between humankind and natural systems as coevolutionary. Cairns (2007c) believes that ecocentrism is merely a transitional stage en route to Harden's ecolate perspective, which is humankind's best hope for living sustainably.

Recently (June 2007), US Environmental Protection Agency Administrator Stephen L. Johnson, when commenting on climate change, stated that we do not know if the present climate is optimal for humans. The climate has been suitable for the 160,000 years that *Homo sapiens* has been on the planet and for the approximately 1 million years for the genus *Homo*. Climate changes thus far, extended droughts, floods, increased storm intensity, decreased snow packs on mountain peaks, expanded disease territory, and so on, have not been marked improvements on the previous condition. Since present climate change events have no precedents, predicting the return of comparative climate stability or the state of the new conditions is not possible. Changed and new conditions will not likely be close to the suitable state they were in before the rate of climate change increased. Furthermore, the Director of the US Environmental Protection Agency did not mention other life forms, such as polar bears, that may not survive climate change. This agency should be concerned with protecting the entire environment, using an ecocentric perspective.

However, as Foster (2007) notes, climate change avoidance is often considered good, but only if capitalism can be fully preserved at the same time. Obviously an anthropogenic viewpoint will be counterproductive if we forget that humankind is dependent upon a biospheric life support system and impairs its delivery of ecosystem services.

Thou Shalt Not Transgress the Carrying Capacity

As Hardin (1993, p. 207) notes, the admonition not to exceed carrying capacity is ecology's contribution to ethics since exceeding the carrying capacity in one year diminishes the carrying capacity in subsequent years. The approximately 24% ecological overshoot and the fact that one-half of the world's population is undernourished or starving indicates that Earth's carrying capacity for humans has been vastly exceeded. Heinberg (2005, p. 31, his figure 2) predicts a human population decline from 7 billion in the early 21st century to under 4 billion in 2200, just from the decline in world oil production. Hardin (1993, pp. 212-213) states that an adult human needs about 2,300 calories of energy each day to remain alive and be moderately active. However, in 1993, the energy consumption of the average American was 230,000 calories per day – 100 times as much as the minimum. As cheap energy availability decreases, Earth's carrying capacity for humans will also decrease *if we expect to continue present per capita energy consumption*.

Although exceeding Earth's carrying capacity is a matter of considerable concern, projected population growth in a time of rapid climate change, reduced production of foodstuffs in areas affected by drought, and loss of cheap energy will very likely be disastrous. In 2007, world population is expected to surpass 6.6 billion – a figure not far below Heinberg's 7 billion peak. Moreover, more of the world's population lives in urban areas, far from sources of food, than in rural areas, closer to sources of food. Some predict oil production will peak sometime between 2008 and 2018, after which oil production will begin an irreversible decline, which might well result in a global recession, global food shortages, and resource wars (over both oil and water supplies).

The US Census Bureau predicts world population will reach 7.6 billion people in 2020 and 8.3 billion in 2030. During this period, urban populations will increase substantially. In 2050, the US Census Bureau estimates the world population will reach 9.4 billion people. Contaminated water is expected to be a major issue, as are diseases carried by insects. In 2080, some parts of the planet will become drier, while other parts will experience flood hazards. Coastal populations could increase from 1.2 billion people in 1990 to 5 billion people in 2080. Climate change resulting in sea level rise could produce millions of environmental refugees.

In a period of rapid climate change and loss of cheap, abundant energy, peoples with hunter/gatherer skills should have a marked advantage over urban dwellers. This advantage would be particularly true if global agriculture suffered severe loss of production. If climate change and declining agricultural productivity result in huge numbers of environmental refugees, a hunter/gatherer lifestyle may be the only means of survival for millions of people. However, a predominately urban and suburban population experience in a hunter/gatherer lifestyle has almost ceased to exist. Worse yet, the few remaining hunter/gatherer cultures are often forced out of their traditional territories (e.g., McCrummen 2007). Hardin (1993, p. 201) remarks:

> *Acknowledging the reality of the "greenhouse effect" and modifying human behavior to reduce its consequences will require changes in education and human economy throughout the world. We may fail; if so, we will surely be the first species to have foreseen its own demise.*

Lack of preparation for such catastrophic events may be humankind's biggest mistake.

Transcending Population Taboos

Sustainable use of the planet is simply not possible without keeping Earth's human population within Earth's carrying capacity for humans. In addition, one hopes that the quality of life would be above the subsistence level and that a safety factor would be included in case of disruptions of food and energy supplies. Finally, population increase is a major factor in the 24% ecological overshoot that must be eliminated expeditiously, if only to avoid mass human deaths due to famine. However, powerful taboos keep even minimal discussion of population stabilization from occurring at both national and global levels. The idea of famine from exceeding the carrying capacity for humans in this era of bounty is difficult to accept. However, it has happened; for example, Hagberg (1953, as quoted by Hardin 1993, p. 20) remarks upon biologist Carl Linnaeus' description of a famine in Sweden:

> *I fear that I shall not have any under-gardeners this summer to do daily work, for they say they cannot work without food, and for many days they have not tasted a crust of bread. One or two widows here are said not to have any bread for themselves or their children for 8 days, and are ashamed to beg. Today a wife was sent to the castle (dungeon) for having cut her child's throat, having no food to give it, that it might not pine away in hunger and tears.*

Lest anyone be unaware of present famine deaths, the website http://www.poverty.com reports hunger deaths by the hour – deaths in June 2007 ranged in the high 700's. Some illustrative examples of contributions (for each $100 earned in the respective country) to combat hunger follow: Sweden, 103 cents; United Kingdom, 52 cents; Japan, 25 cents; United States, 17 cents. The World Health Organization (http://library.thinkquest.org/C002291/high/present/stats.htm) estimates that one-third of the world is well-fed, one-third is underfed, and one-third is starving. The Indian subcontinent has nearly half the world's hungry people. Africa and the rest of Asia together have approximately 40% of the world's hungry, and the remaining hungry people are found in Latin America and other parts of the world. Surely, this situation deserves a free and open discussion on population as the world faces rapid climate change and the end of cheap, abundant energy.

Since the Malthus era, technology (especially that based on cheap fuel) has increased the carrying capacity of Earth for *Homo sapiens*. However, the onset of peak oil, combined with rapid climate change, will reverse this trend, while the population is still growing markedly. This situation will result in one of only two basic outcomes: (1) social evolution to keep the population within Earth's carrying capacity or (2) do nothing and let nature take its course and reduce human population size within Earth's carrying capacity. For all other species, nature "solves"

overpopulation with disease and famine (e.g., Klein 1968). However, Earth's carrying capacity has a dimension that makes the optimal number difficult to state. Hardin (1993, p. 213) states it concisely: ". . . the quality of life and quantity of it are inversely related."

However, quality of life rarely enters discussions on overpopulation. In the 1600's, people accepted financial restraint on marriage. For example, the Elizabethan Poor Law of 1601 was summarized in 1651 as: "(T)hey who could not maintain a wife, might not marry, for a License they could not have . . . usually none were permitted marriage until they were thirty five at least, and the woman thirty" (Hardin 1993, p. 221). Historians state that, years ago, at the Pocahontas coal mine in West Virginia, if a miner were killed, then his widow had to marry quickly or be evicted from company owned housing. At present, western societies no longer have decrees that espouse reproductive prudence since the right to reproduce is a strong component of individual rights. This individual right is possible only in societies that can afford a safety net, which theoretically protects the young from reckless reproduction. The African HIV/AIDS pandemic does not have an effective safety net, although some aid is provided by developed countries. However, in the near future, the combination of global heating and the loss of cheap energy may result in a return to the thinking of an earlier era when the family was entirely responsible for the survival of its progeny. This return need not be the case, but, if not, reasoned discussions must occur of society's responsibility in an age of rapid climate change and diminished resource availability (e.g., Howden 2007).

Finally, should this discussion be based on an anthropogenic or ecocentric viewpoint? Exceeding Earth's carrying capacity will damage the biospheric life support system and, thus, threaten human survival. Other developed countries should be better prepared for this than the United States since their administrations have accepted the scientific evidence for global heating. However, the worst case scenario will be horrific unless global remedial measures to reduce greenhouse gas emissions sharply are taken immediately – the prospects for this development are not good.

Conclusions

Arguably, the disinformation campaign attempting to undermine overwhelming scientific evidence on global heating is the greatest hoax ever carried out on humankind. The robust science of the Intergovernmental Panel on Climate Change has been primarily responsible for diminishing, but not eliminating, the disinformation campaign of the anti-global heating lobbyists, and former US Vice-President Gore's "An Inconvenient Truth" documentary has had a major impact on public opinion in the United States.

The reports of Intergovernmental Panel on Climate Change have brought reason and science back into the global heating debate, but humankind will pay dearly for the decades-long inaction on greenhouse gas emissions caused by the disinformation campaign. At present, the debate has shifted from science (although science will remain important) to ethical/moral issues. Can humankind disenthrall itself from the illusion that the cheap energy party will continue and pay attention to the warnings science has been providing for decades on global heating? Does humankind have the ethical/moral convictions to do what needs to be done? We already know what we must do (e.g., reduce greenhouse gas emissions, use energy efficiently, and use less energy per capita) and have the means to do it. Above all, we must resist the temptation to engage in resource wars to solve the present crisis. Preemptive nuclear wars must become unthinkable. A nuclear war, with as few as a dozen Hiroshima-level bombs, would probably result in a nuclear winter, which is definitely not the way to resolve global heating problems. All the nations that possess nuclear weapons need a well informed citizenry that is capable of acting with both hearts (ethics/morals) and minds. Since illustrative ethical/moral questions follow.

(1) Since the large greenhouse gas emitting nations are having serious, adverse effects upon low greenhouse gas emitting nations, should they voluntarily reduce emissions to protect the affected nations?

(2) Since the gap in global resources is vast between the poor and wealthy, and is still increasing, how should this issue be addressed?

(3) Overpopulation is already a major resource problem. In short, humankind has markedly exceeded Earth's carrying capacity. Global heating and increased scarcity of petroleum (important to production of foodstuffs) will almost certainly reduce Earth's carrying capacity. How should this problem be addressed?
(4) Arguably, only few, if any, places exist on Earth that are unaffected by human activities. Humans use a vast disproportion of the planet's space and resources. How much space and resources should be set aside for the 30+ million other life forms with which humans share the planet? Collectively, these other life forms constitute Earth's biospheric life support system, which makes human survival possible.
(5) What is humankind's obligation to and responsibility for the 1 billion environmental refugees expected in the first half of the 21st century (Lyon 2007)? Even with a plan, chaos and anarchy might result, especially if there are no sound plans for housing (including sanitation and water supplies) and feeding them. If medical services are inadequate, a refugee camp might well be the epicenter for a pandemic disease. The refugee problem already exists –the scale and rapidity of change may catch global society unprepared.
(6) A number of proposals have been offered for decreasing atmospheric carbon dioxide by placing iron in the oceans to stimulate algal growth (thereby removing some atmospheric carbon dioxide). In at least one case, the announced purpose of the project is to make a profit selling carbon offsets. Many uncertainties are tied to attempts to use algal blooms to remove atmospheric carbon dioxide. If the bloom is followed by a massive algal die off, will the decomposing biomass release carbon dioxide into the atmosphere and/or increase oceanic acidification? Sergo (2007) give persuasive reasons why the carbon dioxide may not remain in the ocean. Should the profit-making group be required to post a bond to compensate for an "unexpected" ecological disaster? What organization or nation has the authority to issue a permit for the experiment and exact penalties if there is damage to human health and/or the environment? What remedial measures will be undertaken if harm results from the experiment? These illustrative questions should be addressed before any experiment is authorized.

As a scientist, I am enthralled to witness a part of the greatest global experiment in human history. Scientists have continued their investigations, undaunted by the persistent, well funded disinformation campaign. In the United States, persistent efforts have been made to silence prominent scientists, such as government scientist James Hansen; not only have these efforts failed but they have actually brought increased attention to Hansen's research.

As a father and grandfather, I am apprehensive about the world my children and grandchildren will inhabit. However, they are active, intelligent, and creative and will make the best of whatever environment they encounter. Perhaps humankind needed a challenge of this magnitude to counter the fixation on economic growth.

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CHAPTER 26

MY FIRST FULL TIME JOB

Suggestions have been made that I add some vignettes about my personal life to the autobiography. This chapter is the first of this series.

In the early 1940s after I graduated from high school, I tried to earn some money to ease the financial burden on my parents of my college education. Costs were modest then by the standards of 2007. I believe that the tuition at Penn State University was just over $500.00 per academic year. The time I started college was the end of the Great Depression, although no one knew it at that time. I was able to obtain a job roofing houses. Heights have always bothered me, so I lasted just a few days. Then I started a job at the Hamilton Paper Company in Miquon, Pennsylvania, between Conshohocken (my home) and Philadelphia.

I was machine tender on #6, which produced quality paper for stationary. My job was to stand at the end of the machine and throw defective sheets into a bin behind me. The unblemished paper was taken away periodically, but I did not know where it went or what happened to it. When my replacement for the next shift came, I would transport the discarded paper to a huge vat where it was transformed into pulp. I have no idea what happened to the pulp – it was not my business.

The paper mill ran three 8-hour shifts per day (midnight to 8:00 a.m., 8:00 a.m. to 4:00 p.m., and 4:00 p.m. to midnight), seven days each week. The shifts rotated weekly, which was not good for one's biological clock, but I was young and very resilient. Work on weekends earned time and a half, and, every third week, I worked 16 hours at time and a half on one extended shift. Stopping and starting processing machinery was simply too expensive; therefore, if too much processed paper accumulated, the mill simply shut down until the reserves became low. Stopping production never occurred during the summer I worked there. The work schedule was good for the bank account but not good for one's social life, although my social life was minimal, so it was no big deal. In my free time I went fishing or rode my bike. Fortunately, I could use the family car to go to and from work. So, reading and fishing were my major recreational activities.

The paper mill was very hot, very humid, and very noisy. The big machines that produced huge rolls of paper thundered away ceaselessly. Verbal communications consisted of shouting with your mouth close to the other person's ear. Salt tablet dispensers were everywhere, and water fountains were never far away. I am sure a bathroom was accessible, but I do not remember going there, nor do I remember eating lunch. This summer was many years ago, so I probably did both, even though I cannot remember.

I probably had some type of interview when I applied for a job, but I cannot recall it. Once employed, I said "hello" to the people at the time clock when I checked in and out, but no one lingered – on the way in, each of us had someone to relieve, and, on the way out, each of us was anxious to get home. I had shouted exchanges with the shift foreman, but nothing that could be called a conversation or interaction. Neither do I recall the presence of any women, except the ones who did office work. I could not join the group headed for a bar after work because of my age; I would not have spent my hard earned money for liquor anyway. In short, I knew practically nothing about my fellow workers. Some undoubtedly left, as I did, for better employment opportunities, but most probably stayed there until they retired or the mill closed. The wages were attractive for those who could tolerate the working conditions.

Years later, the Hamilton Paper Company had trouble with *Escherichia coli*-type organisms growing in the warm water of its waste treatment vats. Even though the organisms were not fecal *E.*

coli, they tested as if they were. Since the Schuylkill River, Philadelphia, water plant was downstream from Miquon, the Academy of Natural Sciences was asked to study the problem, and I got to work at Hamilton Paper Company again, but as a scientist. Fortunately, the *E. coli* responding organisms in the paper mill treatment system were not pathogenic. What a difference 11 or 12 years made in the time since I was a machine tender in the paper mill to returning as Assistant Curator of Limnology at a research organization, the Academy of Natural Sciences.

My summer employment in the paper mill was a valuable experience since it helped with college expenses. It also increased my sympathy for blue collar workers more than just living in a mill town had already done. It was a hard life, even for a teenager. Details that I expected to remember are forever forgotten, and the people I could have gotten details from are dead.

Thanks to my daughter Heather Chambers, I have *The Paper Mills of Trout Run* to help my memory. Paper was made at the Trout Run Creek Site for 250 years (1746 to 1995). I worked in the W. C. Hamilton and Sons Riverside Mill. The building I worked in was torn down in 1998 (see http://www.paperindustryweb.com/rivermill/miquonvisit.htm) and is now River Park, a large office complex. From 1951 to 1966, Jeannie and I lived across the Schuylkill River in Gladwyne, Pennsylvania, just a short distance upstream from Miquon. For some reason, I never visited Miquon, although I now regret not doing so.

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CHAPTER 27

MY FIRST EXPERIENCE IN A RESEARCH LABORATORY

In the early 1940s, I spent a few months in the U.S. Department of Agriculture (USDA) Research Laboratory (now the Eastern Regional Research Center), 800 East Mermaid Lane, Wyndmoor, Pennsylvania. I don't remember how I knew a position was open – probably my mother found out about it – and the people who might remember are no longer available. I now wish I had kept a diary, but I expected to remember all the details that are now so elusive. Phyllis Davis has very kindly furnished the first name of the person who supervised me, Dr. Charles Willits. She also sent me some photos and literature from the period when I worked at the laboratory. Dr. Willits had a reputation for being an exacting scientist, but I found him to be a warm, wonderful person. I only worked for him for a few months and, since I was on the midnight to 8:00 am shift, I saw him for only a few minutes each morning. However, his appreciation for my work was evident.

As was often the case when I was young, I focused intently on the tasks assigned to me and was not aware of the other research going on in the USDA Eastern Regional Research Laboratory (ERRL), which opened August 17, 1940. In addition, not many people were available for conversation between midnight and 8:00 am. Over the years, research has varied from potatoes to vitamin C. A publication lists Publications and Patents 1940-1990 from the laboratory, now called the Eastern Regional Research Center (ERRC).

The project I worked on involved the extraction of rubber from guayule [*Parthenium argentatum* (Asteracea)] and rabbit-brush (*Chrysothammus viscidiflorus* and *C. nauseosus*). The intent was to find natural rubber that would be useful in medical devices and for other purposes. These plants are now marketed as ornamental shrubs and can be viewed on Google and other search engines. As I recall, the sample was first hydrolyzed and then followed by an acetone extraction and a benzene extraction. The extraction was in a continuous series, and I changed flasks on schedule. In my free time, I washed laboratory glassware. Every morning I reported briefly to one of my supervisors before leaving for home.

A colleague, Professor Duncan Porter, then at Stanford University, remembered that guayule was planted outside of Patterson, California, in the Central Valley during World War II. Porter remembers that the guayule plants (between Patterson and Westley, along California Highway 33) were plowed under when the war ended. Porter's PhD advisor, Professor Reed Rollins, monographed *Parthenium* in the 1940s because of the federal government's interest in North American sources of rubber. Recently, Dr. Daniel P. Schwartz kindly sent me an article by Zack Hall from the *Reno Gazette Journal*, February 12, 2004, that describes a project led by Dr. David Shintani, which might lead to commercial rubber production in Nevada, using the abundant native rabbit-brush. One of the joys of writing an autobiography posted on the internet as a "work in progress" is connecting some of my activities as a youth to the 21st century. A Google search even disclosed that "The Emergency Rubber Project" of World War II involved 1,000 scientists and technicians.

Working on this project, on the continuous extraction, was a valuable experience for me in two important aspects. First, I learned that research involved detailed, painstaking work and meticulous recordkeeping. This learning experience was useful when I began my own research, even though it had quite different goals. Second, I used Dr. Willits as a reference when I applied for admission to Swarthmore College, not knowing that he did his undergraduate work there.

Early in my childhood, my parents had purchased a full set of the *Encyclopedia Britannica* for me – a major expense during the Great Depression. Later, they purchased a beginner's chemistry set and, still later, the advanced set. After reading the first draft of this chapter, my daughter Karen asked why my parents bought me the encyclopedia set and the two chemistry sets. At that age (probably 9 or 10), I just accepted the gifts without wondering about motivation. However, my mother took me to the library frequently, starting at a very early age. As a result, I have always been addicted to reading, and the *Britannica* was probably intended to expand the available resources. I spent a lot of time with the beginner's chemistry set, which undoubtedly resulted in the purchase of the advanced set. In the recent past and current "shop-until-you-drop" era, it is difficult to convey the sacrifice needed to purchase anything beyond the necessities of life.

However, the family did have an Atwater Kent radio. In those days, most quality radios had a short wave band, even though not many short wave stations existed. As a child, I was thrilled to listen to a broadcast from overseas. I could even find the place in a *Britannica* map and learn something about the country from which the broadcast initiated.

I also built model airplanes from kits that contained balsa wood, paper, glue, and instructions. These kits were very cheap, since practically all the work was done after the kit was purchased. These construction activities and the chemistry sets taught me to pay very careful attention to instructions or else the investment of money and time would be wasted. I worked with the chemistry sets for many hours in the basement of our house. The encyclopedia gave me an inkling that many unanswered questions existed in science and that scientists were spending their lives trying to answer them.

With this first laboratory job, I went from two chemistry sets in our basement to an entire building filled with gleaming research laboratories and mysterious equipment . . . and I was going to work there, albeit in the most junior scientific category possible. I simply was overwhelmed by my good fortune. In addition, ever the child of the Great Depression, I was to be paid for this wonderful experience!

I worked entirely in the one laboratory. A training period of a few days gave me detailed instructions on the routine tasks I had been employed to carry out – carefully written precise instructions – and some phone numbers in case I needed help. After this training period, I arrived early (about 11:30 pm) to relieve the person on the preceding shift. He was also new to the job, but he had the luxury of having full-fledged scientists present at the beginning of his shift. After he went home, I was alone in the laboratory with my notes and sheets of instructions. Of course, a guard at the front desk had let me in, but he had his own duties. Although the guard was affable, I suspected he was not a fount of information about guayule and rabbit-brush.

Suddenly, my first shift was over and the time was 8:00 am; my supervisors reported to work, as well as the next shift in my area. My supervisors were smiling, and my relief must have been abundantly clear. They went over the records, looked at the extraction system, and wished me a good morning. After that first shift, things gradually got easier, but I never got overconfident. Even today I "run scared" on all major responsibilities. I always irritated Jeannie when I locked a door and triple checked (at least) to validate that it was locked. She even stitched a sampler for me: "Being Careful Kills the Soul" (William Saroyan). Although the position of research scientist seemed incredibly distant and unattainable, I had survived and even performed adequately on the bottom rung of the ladder.

Although I did not realize it at the time, this experience was one of the defining moments of my professional career. It was my first opportunity to actually see and experience a research environment as a participant, albeit one of very low status. Even as a beginner I felt my contribution was an important part of the project, and I made every effort to do my very best. Dr. Charles (Uncle Willy) Willits was primarily responsible for the "team attitude." (My mentor, Dr. Ruth Patrick, also favored the "team" approach many years later and solidified my faith in it.) Accurate recordkeeping, attention to detail, and precision were very important to Dr. Willits, but he was a warm, caring person as well. Although I was not in the laboratory long enough to learn the outcome of the particular project I worked on (thanks again to Ms. Phyllis Davis, I now know), the experience was extremely satisfying. I was helping generate data that might result in new knowledge and concepts, and this goal was more than enough at that time. .

Stochastic events probably occur in all research projects, but intent focus on a problem blocks all the unpleasant events common to the human condition – at least temporarily. In addition, if one is moderately successful, one gains freedom as an independent scientist and, within limits, the opportunity to move elsewhere if one's present situation is not satisfactory. Of course, none of these possibilities were apparent to me at that time. I knew the joys of a research career that became so central to my life. I realize now that none of these events could have happened in some research laboratories, and I am grateful that fate or luck placed me in the right place at the right time for me!

Acknowledgments. I am indebted to Karen Cairns for transcribing this chapter for my autobiography and to Darla Donald for editorial assistance. Phyllis Davis provided a wide variety of information, without which this chapter would not have been complete.

CHAPTER 28

CRAWLING OVER ICE-COVERED SNOW WITH JEANNIE

Being careful kills the soul.
William Saroyan

I have always thought of myself as a person who takes only carefully calculated risks after considering both pros and cons of the situation – so did my companion Jeannie, who made a sampler (four or five decades ago) stitched on burlap with the above quote. The sampler still hangs in my apartment. For example, I purchased nursing home insurance for both of us before I was 40 years old; I always checked three times if the door was locked before leaving the house; and I always arrived at the airport 30 minutes to 1 hour before scheduled departure times (even before security checks were common). However, reflecting on some incidents from the past has persuaded me that "being careful" did not always apply. Three particular incidents come to mind that probably occurred when Jeannie and I were in our 60's.

Jeannie and I regularly attended concerts, plays, etc. whenever possible. The particular concert I recall was at Virginia Tech on a snowy evening when sleet and freezing rain were forecast. As we prepared to leave for the event, I expressed some reservations about the weather. Jeannie remarked that the icy forecast might be wrong. However, when we left the concert, we discovered the forecast was correct. The campus buildings and grounds work crew had salted the sidewalks and parking lot, so we proceeded to our 4-wheel-drive Jeep cautiously. I removed ice from the windows and we started home. The main streets we traveled had been salted, but the secondary road to our house had not been treated. We slipped and slid on the narrow, winding road, but met no other vehicle. I pulled into our steep driveway and stopped. Four-wheel drive is not much use on ice, and, in addition, the driveway had a steep drop off on one side. I knew I could not navigate the driveway up to the house. Getting out of the Jeep was tricky, but neither of us fell. Our house stood several hundred feet up the very steep hill that was covered by trees of various sizes. However, the trees were too far apart to provide any continuous handholds. We could only navigate the hill by getting down on our hands and knees and crawling. Halfway up the hill, I wondered if my graduate students would retain me as a major professor and advisor if they could witness this undignified return from a concert. I decided they would. Getting across the icy cement porch was not too difficult, and we breathed a sigh of relief. The following morning, I cautiously retraced my path to the Jeep. When I tried to start the engine, it did nothing. The battery had been stolen (the hood could not be locked on this model), and the gas had been siphoned. I believed, perhaps mistakenly, that I had earned a few points as a risk taker the night before.

A second time in the winter, Jeannie and I had to crawl again. Little Stony Creek is in a valley flanked by steep mountains. One of our favorite hikes was a trail along the creek to Cascades Waterfall, which took about 2½ hours to hike in good weather. The ice had melted in the surrounding areas, but not in the creek valley, so the parking lot at the forest reserve was empty. Naturally, we went on the hike we had planned. Actually, rather than the hiking trail, we took the access road on the left side of Stony Creek, so we only crawled in spots. Even though the road was less treacherous than the trail, I realized that one of us could fall and break an arm or leg. Some spots had been flooded and frozen, and no trees or shrubs gave us handholds. After about two hours, I tentatively suggested that we go back so we would not be caught on the access road in the dark. Much to my surprise, Jeannie agreed and we reached the parking lot before dark. Being careful may kill the soul, but it does sometimes protect the body.

A third time we also had a winter crawl. For years, Jeannie led the campus YMCA hikes. This particular one was to Angel's Rest on the Appalachian Trail. The name describes the hike perfectly. The trail goes up steeply from the parking area and then along the ridge to a cliff with a superb view of the New River. As we neared the ridge, Jeannie had trouble with one eye. A doctor on the hike feared that her trouble might be a detached retina. But she was in charge of the hike, so Jeannie insisted on going to the overlook. I decided to stay where I was near the ridge so that I could get Jeannie to the hospital emergency room on our return. We made it to the hospital, but the doctor on call for eye problems could not be reached. The next morning, the problem was identified by our regular doctor as an unusual level of floaters. Sometimes, being careful is good for one's conscience, if not for one's soul.

Looking back over an 84½ -year lifespan, this old geezer feels that he did not lead a life of excessive caution, which is almost certainly due to Jeannie. Her belief in a favorable outcome for any decision was contagious.

Acknowledgments. I am indebted to Darla Donald for typing the handwritten draft of this chapter and for editorial assistance.

Attached to the volume on 16 January 2008

CHAPTER 29

THE CAIRNS FAMILY BEGINS THREE DECADES AT FIELD STATIONS

Although much of my professional life has been spent in field studies, I had never been to a biological field station[1] when my advisor at Swarthmore College, Professor Robert Enders, who was also Director of the Rocky Mountain Biological Laboratory (RMBL), offered me a summer position there in 1961. The entire family was thrilled that it was to have this new experience. We had a Volkswagen microbus – a stripped-down utility vehicle that was spartan, but commodious—that had a 35-horsepower, air-cooled engine. I readied the vehicle for the trip by having the middle of the three seats removed and a wooden platform built that went from the rear seat to the back of the front seat. We packed all my teaching equipment under the platform, plus a tent, camping gear, field clothes, waders, boat oars, and my fishing rods. Ground mats and the sleeping bags were placed on top of the platform. Over the engine in the rear was a large shelf on which we kept food, a camp stove, camping plates, etc.

During the trip, Jeannie and I, and sometimes one of the children, generally would sit on the bench-like front seat. The children usually spent time on the sleeping bags since it was possible to sit upright and even lean against the rear seat. The 12-foot aluminum boat was carried on the roof. The front doors each had a large pocket for maps and camping guides. We must have looked like "Okies" fleeing the Dust Bowl to the Easterners we were leaving, but, as we got farther west, I was complimented on my "rig."

On flat roads, our top speed was 50 miles per hour, but, when we ascended Monarch Pass on Route 50 in Colorado, it was between 8 and 10 miles per hour. Everybody, even huge trucks, passed us. Even with six people and a very heavy load, we got nearly 20 miles per gallon of gas. The gas tank held about 10 gallons but had no fuel gauge. However, a switch had to be engaged to use the last gallon, so we could always make it to the next gas station. In addition, I always stopped for gas every 120 to 140 miles as a precautionary measure. The odometer served as a gas gauge. I always noted how many miles had passed since the last "fill 'er up" and how much gas was pumped.

Jeannie had a high school class reunion the night before we departed our home in Gladwyne, Pennsylvania, for a trip to Gothic, Colorado – about 1,800 miles, including sizable detours to campgrounds. I don't remember what time Jeannie and I got home from her reunion, but it must have been at least midnight. During later trips, Jeannie was able to drive the microbus, but, at first, I was the sole, available driver. The superb vehicle in which we traveled was purchased in the late 1950s for about US $2,000.00; it had no insulation, no radio, and no air conditioner, but it did have a heater and a horn.

With great luck, we arrived at a campground just over the Ohio/Pennsylvania border while the sun was still high in the sky since it was mid- to late June. We pitched our huge family sized tent in a nearly empty campground. We camped every night until, on Route 36 in Kansas, we noticed a big storm approaching. We found a small, old hotel in a tiny town just off the highway where we booked a huge, family-sized, clean room for $16.00. It had one bathroom with a huge tub with clawed legs. Although Route 36 is a primary east/west road, traffic was not heavy in those days. We could usually find a small, clean, grassy, tree-shaded rest stop with picnic benches under a roof, plus a hand-pumped water supply, and two or more outhouses (privies) in the rear of the rest stop. Somehow the trip to RMBL was a relaxed, homey journey in 1961, '62, and '63. We returned to RMBL in 1971 and from 1984 through 1994 when Interstates 64 and 70 were available, but it wasn't the same – faster but not traveler friendly.

The day before we were due to arrive at RMBL that first year, Jeannie found the US Army Corps of Engineers John Martin Reservoir campground in the campground guide. The campground had just been completed, and the reservoir was surrounded, unknown to us, with ragweed and other asthma-inducing plants. Our daughter Karen suffered from severe asthma in her childhood and teens, and the attack she had at the campground was a bad one. We left there immediately and, after dark, found a motel in Pueblo, Colorado, with one unoccupied room that had an air conditioner with a filter. The boys slept in the microbus.

A good night's sleep put Karen in good shape, and we began the final leg of our journey. We had drive through Monarch Pass on Route 50 to reach the western slopes of the Rocky Mountains. The trusty microbus lumbered up to Monarch Pass in first gear at a thrilling 8 to 10 miles per hour. Passengers in passing vehicles (and they all passed us) had expressions of sympathy, amusement, derision, and amazement, but we eventually reached the summit. The rest stop was a blessing – I had been tensing every muscle as if my stress would help the straining engine. Surely the trip

[1] The professional aspects of field stations were mentioned briefly in Chapter 8 of this autobiography. This chapter focuses on the family aspects.

down from the summit would be better. Not so! The microbus was heavily loaded, and I drove in second gear with frequent use of the brakes. Soon we were turning toward Crested Butte and, in that tiny town, found a sign that pointed to Gothic – the location of RMBL. The paved road ended here and became more rugged by the mile. Finally, we reached the part of the road that overlooked the East River meanders. Naturally, no guard rail secured the narrow, rutted, rocky road and a steep drop off existed on the meanders side. On seeing the drop off, Jeannie stated firmly, "No matter what is at the end of this road, I am staying the whole summer." However, within a week we were driving all through the mountains with few qualms.

Back in 1961, the sight of a car driving through Gothic to Emerald Lake and Schofield Pass was uncommon. However, this trend changed, and, when we last spent a summer in the area, hundreds of cars could be seen on a weekend day. Now I am told that the traffic is even more intense. In 1961, Professor Enders saw us arrive and came down to greet us with his granddaughter Abigail.

RMBL was not a typical research and teaching facility in 1961. In 1879, prospectors John and David Jennings discovered silver high above nearby Copper Creek, and the town of Gothic grew to more than 1,000 residents and became a major supply point for mining camps to the north (Connolly 2005). By 1893, the year of the silver crash, the town was nearly deserted (a hardy soul named Judd still lived there). In 1928, the town of Gothic was purchased by Dr. John C. Johnson, a biology professor at Western State College, Gunnison, Colorado. The picturesque cabin (named 'O Be Joyful' after a mountain) we occupied was a relic of the mining era. It had an all-purpose room downstairs with an old full sized bed, a large dining table, and a pot-bellied stove. The children slept in a loft, and Jeannie and I occupied the downstairs bed. A privy was conveniently located on the ridge behind the house. The cabin had electricity but no water inside; water came from a pipe in the middle of the field in front of the cabin. I could walk freely in the area near the bed, but, if I approached the stove unwarily, I cracked my forehead on a rafter. Showers were in a communal facility about ¼-mile away, and wood had to be split to heat the water.

My compensation for the six-week session was meals for two in the dining room (we rotated which four family members ate in the cabin, which had no refrigerator), a rent-free cabin, and $100.00 for expenses on the 3,600-mile trip from Gladwyne, Pennsylvania, and back. I jumped at the chance to work at the field station because it gave me an opportunity to develop an aquatic ecology course. [In 1962-63, I was invited to teach a one-year National Science Foundation course in physiology for high-school teachers, which met all day on Saturdays at Temple University. I had taken Professor L. V. Heilbrunn's physiology courses at the University of Pennsylvania, so I was well prepared for the course. However, the summer course at RMBL gave me more confidence in my teaching skills, but I never taught physiology again.]

About 20 students enrolled in the aquatic ecology course at RMBL the first summer. The number caused quite a transportation problem since RMBL had no vehicles. Fortunately, I could transport six or eight students in the microbus after the platform was removed, and some of the students had cars. Dr. Ruth "Scottie" Willey kindly showed me some good sites for field trips. I had each student do a research project (some worked in groups of two or three), which was ambitious for a six-week, summer course. This requirement startled some of the students, but the design worked out beautifully. Each project was presented to the rest of the class, which gave both undergraduate and graduate students a useful experience in presenting their research. I had to spend a substantial amount of time helping the students formulate a testable hypothesis, select a field site, and develop a simple sampling program. The results pleased the students and helped me get to know them better. From 1961 on, I made research projects part of every course I taught for over three decades at field stations.

The class visited oligotrophic (very low nutrients) alpine lakes, high altitude streams, and the Gunnison River near Gunnison, Colorado. I had no teaching assistant, but two graduate students, Rick Richards and his colleague Jerry, had been at RMBL before and were very helpful. The other students, who carried the boat, sampling gear, and chemistry material, also helped. Each week for six weeks, the class met for one full day and the following morning only. I lectured for one hour after breakfast each day that the class met, took the class on a field trip on the full day, and gave the class free time to work on their research projects after the morning's lecture on the half day. Students spent three full days on classes if they were taking two courses and had four full days for study or research projects and hiking. The front of one new log cabin was for classes, and the smaller rear part for faculty research and for research investigators. That first year, I even managed some research on synergistic interactions among fresh-water protozoans (Cairns 1967).

I tried to leave two days each week for family activities. Our food shopping was in the Gunnison supermarket, and gas was also about 25% less there, an important consideration since I received no monetary compensation for teaching. Nevertheless, the round-trip to Gunnison took nearly four hours. Karen was taking two courses (no tuition for children of faculty), so we usually shopped when she was in class. Our most memorable family trip was over Schofield Pass and down Crystal Creek Canyon. The riskiest part was crossing a ford (which wet the brakes), followed by a very steep, rocky road on the left and a steep drop-off (no guard rail) into the creek on the right. At the bottom was the Devil's Punch Bowl, which had claimed one vehicle before our trip. I asked the family to hike down and braked frequently to dry the brakes. I drove across the small bridge below the Devil's Punch Bowl, and the family got back into the microbus. The trip to the small, picturesque village of Crystal was challenging, but I even got to glance at the

spectacular scenery from time to time. From Crystal, we drove to the hamlet of Marble; from there, we took a paved road to Route 50, which went east to Gunnison, and then returned to Gothic after filling the gas tank.

The RMBL area was a treasure trove of aquatic ecology systems. One of my favorites was the Mexican Cut area, which contained Galena Lake, a high altitude oligotrophic lake and, at a lower altitude (about 100 feet lower), were two strings of "pater noster" ponds. I remember Professor G. Evelyn Hutchinson, the famous limnologist, using the term *pater noster* to describe ponds or lakes that were in a chain or in a series when he gave a week of lectures on the founding of the Limnology Department at the Academy of Natural Sciences. These strings of ponds contained the rare, neotinic salamander *Ambystoma tigrinum*. Neotinic refers to the way these salamanders retain their gills and never become land dwelling. Moreover, the water quality in the two *pater noster* systems differed. A hike through this area yielded spectacular scenery. Of course, the descent from the lake to the two chains was challenging, but most of the students acted like mountain goats. False pride and helping the timid students enabled me to make the journey with the dignity of a 39-year-old, fledgling faculty member.

I had been an unsuccessful devotee of trout fishing since I was 16. The RMBL area was paradise! Trout (brook, rainbow, and German Brown) were abundant in the East River that ran through the laboratory property. Copper Creek, that joined the East River at RMBL, had cutthroat trout. No travel was involved; so a few hours two or three times a week were a lifetime dream come true for me. My trout fishing era began at RMBL in 1961, and it was somehow fitting that it should end there in summer 1994. In 1961, snow melt kept the streams high and turbulent until mid- to late July. Fly fishing improved gradually by mid-July, especially in the headwaters and meanders. In 1971, I returned to RMBL and the fishing was great, just as it was in 1984 when I again returned to the laboratory. However, snow packs were diminished in the 1980s, and fly fishing began in late June. The trout were far less numerous as well. I have heard from Alan Heath, who currently has a cabin near RMBL, that trout fishing has deteriorated in the area. A former graduate student from over three decades ago has a cabin in another part of Colorado and reports the trout fishing remains good there. Still, I was fortunate to be at RMBL when the area was sparsely populated and many of the ecosystems were pristine. Travel over narrow primitive roads was less dangerous because one was unlikely to meet a vehicle coming in the opposite direction.

One's life contains many defining moments, and summer of 1961 at RMBL was one for both my professional career and my family. I survived my first teaching position in unfamiliar surroundings, which included students from a wide variety of academic institutions and ranged from first-year undergraduates to graduate students. Only three other faculty members were there, none of whom I had met before personally, although I knew Professor Jean Langenheim by reputation. The only research investigator I knew was Professor Paul Ehrlich, of Stanford University, from a brief meeting at the Academy of Natural Sciences (ANSP) in Philadelphia.

I am indebted to my mentor, Ruth Patrick, who also chaired the Limnology Department, ANSP, for allowing me to take four weeks leave to add to my two weeks vacation time so that I could teach the RMBL course. Teaching aquatic ecology paid major dividends for such a small investment of time. I had been told by friends in colleges and universities that a person used to the intense focus of a research organization could not cope with the multiple demands of a teaching institution. At RMBL, I had gained confidence that I could cope.

Also, the experience demonstrated that my family could cope with the rugged conditions of a field station. 'O Be Joyful' cabin was very old, very musty, and very dark, but we made out, even with a 45-mile weekly trip to buy food. We were living in an area of spectacular scenery and fascinating wildlife (e.g., elk, mule deer, bear, mountain sheep, golden eagles, marmots, pikas, foxes, and many species of hummingbirds), as well as unforgettable alpine meadows, pure air, and pristine streams and lakes. So what if we didn't have indoor plumbing, a telephone, or television –one look at Gothic Mountain compensated for the lack of all of these amenities.

Suddenly, the time came for us to leave; we couldn't linger any longer as the college and university people could. I was on a 12-month appointment with a two-week vacation, already used, while most of the faculty from academe was on an academic-year appointment and had weeks before they were due back. The morning of the day before leaving was spent cleaning the cabin and my research area, as well as packing teaching equipment. Then we said goodbye to the RMBL family in a variety of ways. I took a number of pictures, since I had no reason to believe we would return – RMBL offered only four courses each summer, and some rotation was essential for variety. For me, the most gratifying event was the number of students who asked if I would be teaching at RMBL again the next summer. I could only tell them to wait until the courses for 1962 were announced. Even if I were asked to teach in 1962, I had no assurance that I would be given permission to be absent for six weeks again from my appointment. Summers were the Limnology Department's most important field season, so requesting a leave during that period was no small matter. However, we were lucky enough to return in 1962 and 1963.

In my experience, students at field stations are more strongly motivated than most of those at the academic institutions from which they come. They have made financial sacrifices because they could not take summer employment, and they are choosing hard field work over the easier physical requirements of typical academic institutions. In addition, handicapped students are not excluded from field stations. My biggest challenge was a legally blind student who wanted a field experience. The other students played an essential role in seeing that she got one. All

these factors contributed to the success of the individual research projects. Although they required a significant additional portion of my time, I was able to share my excitement about research with students and relive my joy when my first successful research project was completed by sharing their joy when their first research project was completed.

All of us were probably drained emotionally by leaving friends we might never see again and an environment we had grown to love. Had anyone told us we would spend summers of 1962, 1963, 1971, and 1984 through 1994 at RMBL, we would have been incredulous. The last ten of these summers were spent in a cabin on the hill below the Ehrlich cabin; it was small but incredibly luxurious – indoor plumbing, a hot water tank, a refrigerator, and a sink. The view of Gothic Mountain was grand. And, to top all this magnificence, our last summer was spent watching four young foxes grow up. They were in a den on the hillside with their mother, who often trotted past our cabin, usually carrying food for her family.

Some of Jeannie's ashes were sprinkled on a cairn not too far from Copper Lake by all the family members, except me. Over half of the remaining ashes are in a milk glass container in my den. Pictures of Jeannie's memorial service and the hike up to and down from Copper Lake were taken by my granddaughter Hannah Cairns and are on my website www.johncairns.net. All these memories are wonderful, and Hannah's photographs help me relive them.

Acknowledgments. I am indebted to Karen Cairns for transcribing and editing the handwritten copy of this chapter and to Darla Donald for editorial assistance and preparation of it for the website. I owe much to Robert Enders for advice while I was a student at Swarthmore and for years after that, almost to the year he died.

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Chapter 30

THE UNIVERSITY OF MICHIGAN BIOLOGICAL STATION (UMBS)

Inevitably, my descriptions of the two field stations, Rocky Mountain Biological Laboratory (RMBL) and University of Michigan Biological Station (UMBS), might be viewed as a comparison of them, since they differed markedly. However, each was critical to the development of my career and provided a unique experience for the entire family. The family experienced many joys from summers in two splendid, but dramatically different, ecological environments.

The UMBS was founded in 1902 and its "Crown Jewel" is Douglas Lake, which is shaped like a fish. A map is available at http://www.lsa.umich.edu/umbs/umbs_widedetail/0,2543,11189%255Farticle%255F18702,00.html. The station laboratories and housing are on South Fishtail Bay. The UMBS currently owns about 10,000 acres and is a Biosphere Reserve of the United Nations Man and the Biosphere Program. It is also a National Science Foundation Experimental Ecological Reserve and has a 3,200-acre tract on Sugar Island on the St. Mary's River near Sault Ste. Marie on Michigan's Upper Peninsula.

In late July or early August 1963, Robert Enders, Director at RMBL, informed me that he had a communication (probably a letter since RMBL had no phone) from Alfred Stockard, Director at UMBS, asking Enders if Stockard could obtain permission to offer me a summer position teaching fresh-water protozoan ecology. The UMBS summer session was then two weeks longer than the one at RMBL, but the distance from Gladwyne, Pennsylvania, to UMBS was less than half the distance to RMBL, diminishing my total time absent from the Academy of Natural Sciences (ANSP). In addition, I was to be paid a salary, which, even after deducting meals for five people for eight weeks and travel costs, was significant. Consequently, I had an opportunity to develop another course and teach at another institution. Unfortunately, I had already accepted an invitation to be a discussion leader in New Hampshire at a *Gordon Research Conference on Environmental Sciences: Water*, which was scheduled for the week before the summer session at UMBS started. It was a stretch to travel from New Hampshire to Michigan for the beginning of the session. I was scheduled to meet with UMBS students on the Saturday before classes began, but Director Stockard graciously gave me permission to arrive on Sunday – the day before classes actually began. I had been a discussion leader at a *Gordon Research Conference: Stream Sanitation* in 1956, so I had already experienced the system used in that setting.

As soon as the Gordon Conference ended, the family began the trip to UMBS (probably Friday afternoon). We took Route 89 from Tifton, New Hampshire, to the Trans Canada Highway, and from there to Sault Ste. Marie, Canada, where we stayed in a motel for the second night. From there, we crossed the border and drove south to Pellston, Michigan, then east to UMBS. We arrived early Sunday morning, unpacked, and I went to inspect my classroom/laboratory, talk to Director Stockard, and meet some of the other faculty. The family was assigned a spacious log cabin with room for everyone. The cabin had a bathroom, hot water, and a refrigerator, and it was only about 200 feet from Douglas Lake, which had a sand beach. The water was still chilly in June, but soon warmed up. Three meals each day were served to the entire family for the entire summer, but the costs took a substantial portion of my salary.

The UMBS had vehicles for class trips, microscopes, facilities for chemical analysis, and a superb library for a field station. My formal teaching covered two full days each week, plus much time between formal class periods spent with students on identifications. A large variety of aquatic habitats (bogs, fens, swamps, streams, lakes) was nearby, so the class had a superb opportunity to view a wide range of habitats. My 1964 class was small – six students – with only one US citizen, Bill Yongue, who subsequently worked with me for his PhD at Virginia Tech and was a valued colleague for many years after he joined the faculty there.

From 1961 to 1995, when massive blood clots in my right leg ended my field station days, the family spent about equal time at RMBL and UMBS, which is arguably the best evidence that we valued each field station. At RMBL in 1961, most people washed their laundry in tubs or at the Laundromat in Gunnison. Later, another faculty member, Keith Justice, found two old but serviceable Maytag washers for $15.00 each. I paid for one and Keith paid for the other. The washers even had power-driven wringers. Jeannie preferred using the old Maytags to driving to Gunnison. Also, she could step outside the shower/laundry facility and look at wildflowers in the meadow as she hung up the clothes to dry. Clothes dried rapidly in the dry mountain air on the clotheslines outside the washhouse.

At UMBS, a number of modern washers and dryers were available. Before becoming a biological station, UMBS was used for training engineers in surveying. Since the physical area had been extensively logged and was relatively flat, lines of sight were good. However, trees grew, especially aspen, and blocked lines of sight, so the facility

was turned over to biologists at the University of Michigan to be used as a field station. The housing in 1964 consisted of what was left from the engineering times – metal roofed buildings with wooden frames, floors, and doors. The student cabins were in clusters: "Manville," "Ladyville," and "Blissville" (married students). Most faculty lived in larger metal-roofed cabins along the lakefront. Each had a sizable living area with a pot bellied stove, two bedrooms, and a bathroom. A large communal dining room and a sizable kitchen were both in the same building, which was not far from our cabin. Students could earn money working as waiters and waitresses. Mrs. Hilda Kargo, in charge of the dining facility, had served as cook in a lumber camp before joining UMBS, which was a blessing because students at field stations are hearty eaters. Sidewalks were concrete, courtesy of the engineers, but the streets were dirt.

After the summer of 1962, the family began to miss one person, Karen, at a field station or at home since she began college that fall. Paul Ehrlich had not yet published *The Population Bomb* (1968), and Jeannie and I had four, widely spaced children: Karen, 3 November 1945; Stefan, 9 July 1949; Duncan, 2 March 1954; and Heather, 1 May 1959. In our youthful innocence, Jeannie and I thought that the spacing of the children's births meant we would never have two children in college at the same time. It almost worked!

Jeannie loved to swim, and a lake was practically on our doorstep at UMBS. Starting in 1965, our cabin was lakeside in faculty row and we remained there, but not in the same cabin, through 1983. On Saturday nights, Jeannie taught folk dancing on the volleyball court. During the warm parts of the summer session, the hot, tired dancers would, with a few exceptions, plunge into the lake at the end of the dancing. In later years, Jeannie kept score at the ball games played on a level field up the hill from the dining hall. The best insect repellent was Avon's *Skin-So-Soft*, so not only the ball teams, but also the spectators, smelled heavenly.

Arguably, the major social event of the session was the trip to Mackinac Island from Mackinaw City via ferry boats of the Arnold Transit Line. The island is a major tourist attraction, notable for the Grand Hotel and the fact that cars and other motorized vehicles are banned (except for fire engines). Horse-drawn carriages are a major form of transportation, and streets, especially in the hotel area, are swept regularly. Bicycles are also very common, and our family rented a batch and rode the approximately six-mile, paved road around the island. We usually ate lunch on the lawn of the large park overlooking the yacht basin. Our annual trip coincided with the Chicago to Mackinac Island yacht race on Lake Michigan. The harbor and yachts can be viewed at http://www.mackinac.com/content/general/about.html.

One of the family's favorite Sunday trips was a group picnic at Sturgeon Bay on Lake Michigan, about twenty miles west of Pellston. Usually three or four families participated: the Paterson family (Bob and Marian and their children Drew, Virginia [or Ginny], and John), the Cairns family, the Williams family (Gary and Gussie and their children Kevin, Brian, and Eric), the Shaffer family (Bob and Jocelyn and their daughter Martha), and Bill Fennel (a bachelor). The kitchen would supply picnic materials, such as hot dogs and hamburgers, and we would have an all-ages softball game, hike along the beach, and just sit on the beach chatting. We only had this group picnic two or three times each summer, but we all have fond memories of those beach parties.

Soon after my arrival at UMBS, I had a memorable short conversation with Alex Smith (mycologist), an avid trout fisherman. "Do you enjoy fishing?" *"Very much."* "Do you use artificial flies?" *"Of course."* "Do you tie your own?" *"When I have time."* "Do you use worms?" *"Not since I was 12 years old."* "Would you like to go fishing on Maple River tonight?" *"Delighted."* The third person that night was George Saunders (limnologist). I was dropped off at a deep pool on the Maple River just before sunset with the largest dry flies I had ever seen. I was told to practice casting, so I would know where the obstacles were in the dark. The fly was so large and had such wind resistance that "lobbing" would describe the process better than "casting." Alex and George had been gone for some time when the whippoorwills began to call. I assumed my situation was similar to the snipe hunt all novices are exposed to, and I checked the bushes to see where Alex and George were hiding. As darkness deepened, the hatch of the largest aquatic insects I had ever seen began. I could hear the "plops" of large, feeding fish. I was not on a snipe hunt! This scenario was fishing for large brown trout at night. I didn't catch anything that night, but, on two or three occasions, I managed to hook a trout briefly and felt a surge of power unique from all other experiences. Over the years, I managed to go fishing with Alex two or three times each summer, often in daylight but sometimes at night. When we got to the stream, one of us would head upstream and the other downstream – on small streams, it is best to be solitary. I never mastered night fishing for brown trout, perhaps because, for me, trout fishing is making a skillful cast that places the fly and line lightly on the water so that it floats down past the spot where a trout might be stationed with no drag (i.e., as if the fly were not attached to a line). One of my graduate students once remarked that fly fishing was my form of transcendental meditation. True, since fly fishing requires a focus that does not permit other thoughts to intrude.

Teaching at RMBL and the academic year National Science Course at Temple University provided me with confidence that I could teach and, since I produced four journal articles in 1962 and 1963, that I could get manuscripts published while teaching. I didn't know if two publications each year would be enough to show professional development. In 1964, my first year at UMBS, I had four manuscripts published, and all but one were in *Notulae Naturae* of the Academy of Natural Sciences Philadelphia, where I worked. The other publication was in *Industrial Water and Wastes*, a journal primarily devoted to industrial waste discharges. I

taught, carried out research on protozoan community dynamics, and worked on manuscripts, including books, at UMBS. These activities would not have been possible without the collaboration with William (Bill) Yongue, Jr. He was often, deservedly, first author (Yongue and Cairns 1971a,b,c) on the publications. Over the years, UMBS provided superb opportunities for collaboration (see http://www.umich.edu/~umbs/edu/research.htm under the UMBS bibliography).

In addition to the two field stations, another major factor influenced my professional career. In 1965, I published a small booklet, *Population Dynamics*, in the American Institute of Biological Sciences, Patterns of Life Series, published by Rand McNally, Chicago, Illinois; one book chapter; six journal articles; and one editorial. Not a bad total, but I did not have any publications on rapid biological information systems and fresh-water protozoan colonization dynamics – two areas I was eager to explore. UMBS gave me the opportunity to begin research on the latter. I was able to begin research on the former at the University of Kansas (KU).

Pellston Book Series

In 1976 I was asked to chair a committee for a workshop, "Estimating the Hazard of Chemical Substances to Aquatic Life," to be published, if the workshop turned out well, by the American Society for Testing and Materials as a Special Technical Publication. Two colleagues, Dr. Kenneth L. Dickson and Dr. Al Maki, would be co-editors. Participants were to include representatives from industry, regulatory agencies, and academe – a potentially explosive mixture! After consulting with Dickson, who had spent a summer at UMBS, and Maki, I approached David Gates, the UMBS Director at that time, to ask if the workshop could be held at the Biological Station before the 1976 summer session began. He agreed. I felt that the ambiance of a field station, surrounded by natural beauty, would compensate for the comparatively Spartan accommodations. They did.

In order that workshop discussion sessions be recorded accurately and be available immediately, I employed court recorders from Northern Court Reporters, Alpena and Petoskey, Michigan, to record the entire proceedings. I sat beside the court recorders throughout the week-long conference to be certain that technical terms were recorded accurately. The conference exceeded all our expectations, probably influenced by the following factors.
(1) All participants were convinced that all three groups could offer something valuable.
(2) Jim Daunter, the station chef, taught courses on the culinary arts during the academic year and could be depended on for superb meals.
(3) Daunter not only selected the wines and cheeses for the social hour, but he also gave such interesting talks about the qualities of each that the attendees requested unanimously that he do so every evening.

The workshop worked out well that first year (Cairns et al. 1978); consequently, Dickson, Maki, and I kept the series going (Dickson et al. 1979, 1982, Maki et al. 1980), but I dropped out after the 1982 book. The series is still going on (search "Pellston Series Books" on the Internet).

The workshop was named after Pellston by the 1976 participants because they were intrigued by tiny Pellston's "international" airport. The name stuck despite the various geographic locations where the meetings have been held and the different publishers. The second book was published by the American Fisheries Society, the third by The American Society for Microbiology, and the fourth by Ann Arbor Science Publishers, Inc., because we wished to call attention to the series to a diverse group of professionals. In recent years, the Society for Environmental Toxicology and Chemistry has sponsored the series.

UMBS and My Research Career

My son Duncan read the first draft of this chapter and observed that the sequencing is confusing. True, but this period was chaotic for both my professional career and our family life. Only with 20/20 hindsight do some of the events fit well. For example, the invitation to chair the Pellston Book Series indicated that my research on toxics and biological information systems had some merit. The research space assigned to me at KU was barely adequate for developing the apparatus and some preliminary testing. The steam pipes in the ceiling made temperature control impossible and would have diminished confidence in any data generated. Similarly, the research begun at UMBS on protozoan colonization of artificial substrates had merit as evidenced by my election as President of the American Microscopical Society from 1980-1981. One only knows these things years later, so chronological sequencing is not always the best way to consider this time period. Forty years later, I still view these events in terms of their impact on the family and my professional career. Life is often influenced by stochastic events.

When I went to KU in fall of 1966 and saw the tiny, inadequate research space assigned to me, I was in deep shock. I had expected to be at this institution for the remainder of my professional career. Worse yet, I had taken my family from lifelong friends and surroundings. I had left a secure position to develop new research areas that could not be easily developed at ANSP. Finally, I had always wanted to be with students but not sacrifice my research career. Writing my autobiography has made me realize how stressful that period was. For example, I cannot remember what the inside of our house in Lawrence, Kansas, looked like, although we lived in it for two years. This time must have been ghastly for Jeannie, but she never complained. Our personal lives were saved by three groups: two square dance

clubs, Jeannie's folk dance group, and the local Unitarian Fellowship. Jeannie had many friends in the Head Start Program, and we went to every concert and play at the university.

Professionally, three factors helped maintain my research momentum.

(1) I had two file cabinet drawers with sufficient data for quite a few manuscripts, which I began working on with considerable energy. Roger Kaesler (Geology, University of Kansas) was a blessing; he knew pattern analysis, and we completed many manuscripts together. In fact, our collaboration was so productive that we continued to co-author manuscripts after I left KU. Later on, Roger spent a sabbatical year with me at Virginia Tech.

(2) By the time I moved to KU, I had established a significant research program at UMBS, where I had superb research facilities. This situation alone might have maintained research momentum, but the appointments for UMBS were made one year at a time, and I was feeling insecure at that time due to the situation at KU.

(3) I had the great, good fortune at KU to meet Hampton Shirer, who knew more than enough to understand the rapid biological information systems I wished to develop but also enough about electrical engineering to build the monitoring systems (Shirer et al. 1968). Shirer (a faculty member) and two graduate students, Tom Waller and Rip Sparks, did much of the testing of the apparatus. Also, Frank Cross, an ichthyologist with the UK Museum, Rip Sparks, and I collaborated on toxicity testing research (Sparks et al. 1969). The KU Water Resources Research Center gave permission for me to take the monitoring apparatus, developed on a Water Resources grant, to Virginia Tech; I returned it as soon as the second version was completed. Three graduate students in my limnology course (D. W. Albaugh, F. Busey, and M. D. Chanay) worked with me on a biodiversity method useful for persons without formal taxonomic education (Cairns et al. 1968).

While Phil Humphrey was still at the Smithsonian, we co-authored a report for the US Army Corps of Engineers (Cairns and Humphrey 1969). We worked well together and would almost certainly have continued to do so, but I needed adequate space (unavailable to me at KU) for long-term research on rapid biological information systems. When Humphrey took over the administration of the KU Museum and the Department of Zoology, I was appointed Associate Chair Zoology for 1967-1968, which enabled me to stay in Lawrence until Stefan graduated from high school.

In fall 1967 and early 1968, I had attractive offers from four universities, three higher ranked nationally than Virginia Tech, but Bob Paterson, a colleague of many years at UMBS, was the new head of the Department of Biology at Virginia Tech. He offered me, in writing, a research position with abundant research space. Despite my recent bad experience at KU, loss of trust did not seem appropriate.

During this stressful period, UMBS was Jeannie's and my refuge. The natural surroundings were therapeutic – so was the UMBS "family." In fall 1967, I asked Waller and Sparks if they wanted to come along to my new position – they had already guessed that I was leaving KU. I was blessed that they were just waiting for me to tell them where and when. Four students (Jean Ruthven, Dickson, Waller, Sparks) were confident that we could establish a research program, and we did.

I will never know what Jeannie endured – she never discussed these things with me or the children. Her attitude was to put unpleasant events behind and "get on with life!" However, she had many friends in Lawrence, judging from the attendance at her "house closing" party. From Lawrence, Kansas, we drove to Blacksburg, Virginia, in spring of 1968 to a house we had never seen. Housing was tight, so we authorized Marian Paterson, who heard of a house that was available while at a bridge party, to pledge us to buy the house before the availability of it was announced to others. We arranged, again by phone, for a lawyer to take care of the legal details. The house was new and never inhabited; the owners were leaving town. The wife had visited it daily during construction, so we found only one defect.

The van with our furniture was a week late arriving, so we left from Blacksburg for UMBS as soon as the van had been unloaded. I didn't even check three times to verify that the doors were locked. We arrived in Michigan barely before UMBS classes began. However, the high stress period was over, and we were back in the UMBS family. I began teaching Stressed Ecosystems in 1973 and continued teaching that course through 1983. In 1995, I returned to UMBS to teach a one-week, pre-session course on stressed ecosystems" and that summer was the last time Jeannie and I were at UMBS. Our daughter Heather and her family live in Ann Arbor, Michigan, and have visited UMBS many times. Our granddaughter Laura Cairns Chambers, Heather and Carl's youngest daughter, was a student at UMBS in summer 2006.

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CHAPTER 31

THE MOST IMPORTANT DAY OF MY LIFE

At 85, I can recall many important days in my wonderful life: (a) the day Jeannie and I were married and the day each of our children was born, (b) the day I survived an appendectomy in Iquitos, Peru, (c) the days on which I received various educational degrees, without which my professional career would not have been possible, (d) the days on which I received a Presidential Commendation or became a member of the American Academy of Arts and Sciences, the National Academy of Sciences, and the American Philosophical Society, and (e) chronologically last, but not least, the day (February 21, 2005) that my companion of 64 years died.

None of the days mentioned above would have ever happened if I had not met Jeannie in fall 1941 at Penn State University. The moment is as vivid as the day it happened. One of my three roommates, Bobbie Beacher, at Alpha Zeta (AZ) fraternity had a date with Betty Ann Spear, and I was to date Betty Ann's friend, Jeannie Ogden. Bobbie and I walked into the lobby of Grange Women's Dormitory, a hall door opened, and Jeannie came walking toward me, smiling and with her hair bobbing slightly. My memory is blank on everything but her face and smile. I currently have on my desk a picture of Jeannie taken with two of her friends (Kathy Osgood and Evelyn Godfrey); it reminds me of that moment – same smile, same "hairdo." After that meeting, she and I spent lots of time together hiking and spending Saturday nights at the AZ house.

Jeannie always had high grades and was often on the Dean's List. When she missed the List, it was by only a narrow margin. My grades were nothing to brag about. When we were married on August 5, 1944, by Rear Admiral Duncan McNair (a friend of Jeannie's family), Jeannie had a BS in biochemistry and I had no college degree – but, Jeannie had confidence in my future. I finally got an AB from Swarthmore College in 1947, and Jeannie strongly supported my efforts to get this first degree, then the MS in 1949 and the PhD in 1953 from the University of Pennsylvania.

After one year of coursework at the University of Pennsylvania in my MS program, my major professor, David Wenrich, recommended me for a summer position as a protozoologist on one of Ruth Patrick's two teams studying the effects of pollution on the aquatic communities in the Conestoga River and its tributaries. In the Zoology Department at the University of Pennsylvania in the late 1940s, the primary focus was on the genetics and physiology of cells. Suddenly, I was in a position that involved studies of single celled organisms in the context of an entire complex system. This era was one of "lone wolf" research. Moreover, this science affected the general public (e.g., jobs), industry (e.g., waste treatment costs), and regulating agencies (e.g., adherence to law). Most scientists of that era were not accustomed to such interactions. Jeannie and Karen (not yet 3 years old) came along. We shared breakfast and dinner, but I was rarely finished before midnight each day. Mary Gojdics, a senior protozoologist with publications, was on the other team. She sat across a lab table from me and kindly helped me along. The teams had weekends off, but I worked the usual hours of the weekdays, as did Ruth Patrick and Mary Gojdics. If my research as part of the team's studies met the University of Pennsylvania's scientific standards, it could serve as an MS thesis – it did. Jeannie never complained and treated this time as an interesting experience. We both got to experience the life of a research scientist. At the end of summer, a single, permanent river survey team was formed, and I was invited to join.

The Limnology Department at the Academy of Natural Sciences was founded, and I worked my way from Assistant Curator of Limnology, to Associate Curator, to full Curator (the last about 10 years after I received my PhD). As a member of the river survey team, I had to be in the field a significant amount of time, which placed the full parenting burden on Jeannie. Between surveys, I was learning aquatic toxicology under the tutelage of Dr. W. B. Hart, one of the pioneers of the field, and carrying out research for my PhD dissertation. I was also learning how to acquire extramural funding under the tutelage of Ruth Patrick. This schedule sounds worse than it seemed because research is so exciting. Even so, I could not have adhered to the time consuming schedule without Jeannie's support and encouragement.

Jeannie, Karen, and I lived with her supportive mother, Eleanor Ogden, until we were able to get our first house in 1949. Times were difficult, both financially and in terms of time I could spend with my family, but Jeannie's support never waivered. I never planned for or hoped for an academic life with much travel. Inside, I have always been a boy from a small mill town in Pennsylvania who experienced the Great Depression and World War II. However, I never lacked opportunities and challenges, most of them daunting. Jeannie and I never had long discussions about our life's path, but her attitude was unmistakable –"sounds interesting; go do it." So I did.

The 20th century was an age of specialization in science and, although I viewed my research as the study of the effects of stress (e.g., pollution) on aquatic ecosystems, to most people it appeared to be a hodgepodge of unrelated components. The view of most of my fellow graduate students was that one's research should be in a single specialty and one should not transgress disciplinary or subdisciplinary boundaries. An old saying expresses this view well: "Shoemaker, stick to your last." This "lack of focus" did not bother Jeannie – when asked what I did, she replied simply: "He's trying to save the world from pollution." In addition, some people thought that scientists of that time should never become involved in

public policy issues, especially controversial ones. This attitude never bothered Jeannie – she felt everyone should be involved in public policy issues. She was always involved in a number of such issues, including fair housing, discrimination, head-start, etc.

Although research scientists are always doing something exciting to them, which usually requires long hours, they are also perpetual students who are attempting to keep up with an ever increasing volume of new literature. Scientists are also, as they should be, subject to continuous scrutiny and peer judgment when they submit manuscripts for publication, apply for extramural funding, or present papers at professional meetings. Ironically, in addition to Jeannie's unwavering support, I received much of the ego support everyone needs from non-scientists. My research on pollution effects on aquatic organisms was useful to waste treatment engineers. I had articles published in *Industrial Wastes* and the *Purdue University Engineering Bulletin*. I even gave a keynote address on the effects of thermal waste discharges on aquatic organisms at one of the annual waste conferences. However, I would not have tried these then contrarian activities had Jeannie's support not preceded them. Becoming a Fellow in 1969 of the American Association for the Advancement of Science was the first major indication that my career path had merit. The Presidential Commendation in 1971 indicated that involvement in the public policy aspects of science had value.

One of Jeannie's favorite books was *What Do You Care What People Think?* by Richard P. Feynman. Her attitude was: if what you are doing is interesting or useful or both, just go with it despite negative comments or ridicule. Not surprisingly, this attitude applied equally well after my receiving major awards and acquiring modest success. Jeannie did not believe that resting on one's laurels led to any significant degree of life satisfaction. My awards hang in my den in the assisted living center where I live, not as memories of past success but to remind me that every worthwhile undertaking involves some risk. Jeannie's attitude of "Get with it" still guides me today.

When Jeannie died in February 2005, part of me died with her. Sixty-four years of companionship makes a lasting impression, as it should. I tend to be realistic (or pessimistic) and see all the obstacles in my path. People often ask how I can carry out research on environmental destruction and not get severely depressed. I do not get depressed because I can still see the world, although less clearly, through Jeannie's eyes. It is, as always, a fascinating place to be, and I am blessed to have had the opportunity to live on this wonderful Earth – and it all began on that most important day of my life in 1941 when Jeannie walked toward me with her enchanting smile.

CHAPTER 32

CO-EVOLVING WITH JEANNIE

Since Jeannie is no longer with me, I have no choice but to describe the challenges we met together as I remember them. These incidences could have destroyed our relationship, but they strengthened it instead. Our children have been extremely helpful in recalling memories of the early days, and Kathy Brady (nee Osgood) has been an invaluable source of information about the early days at Penn State, where Jeannie and I first met. Reconstructing the past is extraordinarily difficult. Irving notes: "Your memory is a monster; you forget – it doesn't. It simply files things away. It keeps things from you, or hides things from you – and summons them to your recall with a will of its own. You think you have a memory; but it has you!" (as quoted in Marcus 2008, p. 18). Also, Pinker remarks: "To a very great extent, our memories are ourselves" (as quoted in Marcus 2008, p. 36). "Yet memory is arguably the mind's original sin. So much is built on it, and yet it is, especially in comparison to computer memory, wildly unreliable" (Marcus 2008, p. 36). These issues were particularly troubling while I was trying to recall some dates that I thought I could never forget. However, the co-evolution of my relationship with Jeannie remains vivid, and the dates are probably still available if I could find old records. Carey (2008) remarks: "Scientists have for the first time recorded individual brain cells in the act of summoning a spontaneous memory, revealing not only where a remembered experience is registered but also, in part, how the brain is able to recreate it." Writing this autobiography is a mixture of joy and pain.

Jeannie faced many more challenges than I as she adjusted to my evolving career, but two widely spaced incidents illustrate unexpected events to which I had to adjust.

(1) We were at Rocky Mountain Biological Laboratory (RMBL) for a summer in the early 1960s. At the end of the session, I had flown to Purdue University to present a paper at the Annual Waste Conference. When I returned to our cabin, late at night, people were sleeping all over the floor. I tiptoed over to our bed (the children were sleeping in the loft) and quietly crawled in. When I awoke in the morning, Jeannie said to me, "The students needed some place to stay." Apparently, the director of RMBL had asked the students to vacate their cabins the day before so that the cabins could be winterized. Naturally, Jeannie offered a refuge (our cabin) for those students who wished to stay one more night.

(2) In 1984, I had planned to return to RMBL as a research investigator. In the afternoon two days before we were to leave, Jeannie showed up at home with a Weimaraner puppy and said with delight, "He has blue eyes!" Jeannie found a box and bedding for newly named "Argus," got appropriate shots for him at the veterinarian's, and a dog license at the county seat. The next day, the three of us left in the Volkswagen diesel bug and camped that night in Ohio. Argus slept in his box outside our tiny pop-up tent. He was intelligent and well behaved. When we visited a rest stop, he jumped out and in the vehicle when I gave the signal. The following night, Argus adapted to a motel. The next night, we camped beside a lake at a Kansas state park, and we all went swimming. The last night we spent with Karen's family in Boulder, CO, and Argus adapted to that new environment. The day after we arrived at RMBL, the three of us went hiking in the mountains and saw a bear across an alpine meadow. All of us froze and, after what seemed an eternity, the bear turned and ambled into the aspens on the other side of the meadow. Argus had never seen a bear before, but he passed another test with flying colors. I did not do too badly either – I never asked, "What is this dog doing in our lives?"

Elsewhere in this autobiography, I have used the name "Jean" for my wife, but our children, grandchildren, and close friends always called us "Jeannie" and "Johnny." Calling her "Jean" seemed appropriate in the professional career parts of our lives and "Jeannie" for the more personal parts. For the record, her closest college friend, Kathy Osgood, and I both called her "Og" (a shortening of her maiden name) and still do.

World War II began for the United States shortly after Jeannie and I first met in fall 1941, but even then we felt a strong companionship attraction that grew into a lifelong commitment. For 64 years, we always wanted to be together and do things together. As Pinker (1997, p. 417) remarks, "Offering to spend your life and raise children with someone is the most important promise you'll ever make, and a promise is most credible when the promiser can't back out." Those days were "idyllic," when we studied and met on Saturday evenings, Sunday afternoons, and briefly on evenings before Jeannie had to sell "Sally's Sandwiches" and take orders for the following day. We knew we liked each other, the outdoors, reading and books, concerts, and so on. Neither of us had a passion for material possessions and, consequently, spent as little time shopping as possible. Since we were children of the Great Depression, this abstention was easy because neither of us had lots of money anyway. When we had arrived at college, we each had brought all our clothing in one suitcase and carried a winter coat over one arm. Few other students were much better off. However, this lifestyle went much deeper for us – Jeannie's mentor at Penn State, Professor Stevens, was a member of the Society of Friends (Quakers), which espoused a simple life in terms of material possessions. Jeannie and I adhered to these tenets throughout our lives.

Companionate Love

Jeannie and I were married on August 5, 1944, just after Jeannie acquired a BS in biochemistry and while I was in the US Navy. For the part of our marriage after World War II, I had to spend about 1½ years completing my AB at Swarthmore College (degree in 1947) and then 1 year of graduate school at the University of Pennsylvania. I accepted a temporary position at the Academy of Natural Sciences (ANSP) in June 1948, which became a full-time position in September 1948. I was able to use the research I did at ANSP for an MS (1949) at the University of Pennsylvania. From June 1948 to 1953 (PhD, University of Pennsylvania), I worked full time at ANSP (except for a few months in summer 1952 when I completed my dissertation) and spent long hours completing the requirements for the PhD. From 1946 to 1953, I had to focus intently on professional activities and Jeannie developed many interests of her own.

Until Jeannie became pregnant with Stefan (born July 9, 1949), we lived with her tolerant mother, Eleanor Ogden, in Havertown, Pennsylvania, so Jeannie had the help of her mother with Karen (born November 3, 1945). In 1949, we purchased a tiny, new, ranch house – three bedrooms, one bathroom, a tiny kitchen, a tiny dining space, a tiny living room, and a detached garage on ¼ acre – in a development in Plymouth Valley, near Plymouth Meeting, Pennsylvania. I used one of the tiny bedrooms for my dissertation research; Karen was in one bedroom; and Stef was with us in the "large" bedroom. I was still going on five or six river surveys of about two weeks each year. Jeannie and I continued to look forward to the other's company whenever time permitted. Jeannie and I were so enthralled with doing things together that I never thought to analyze why this relationship was so fulfilling, even when the marriages of some of our friends broke up. Pinker (1997, p. 507) notes: "Companionate love, the emotion behind close friendship and the enduring bond of marriage (the love that is neither romantic nor sexual), has a psychology of its own. Friends or spouses feel as if they are in each other's debt, but the debts are not measured and the obligation to repay is not onerous but deeply satisfying." This description of companionate love initially looked promising to describe mine and Jeannie's relationship, but on close examination, it does not seem appropriate. We never felt an obligation to repay – our life together was a partnership.

The move to our own house in Plymouth Valley crystallized our division of labor. Jeannie took care of Karen and Stefan, washed the clothes, and did the housekeeping essential to our tiny home. I usually made the large omelet and oatmeal for breakfast, sometimes prepared dinner, and mowed the lawn. From the end of the US Great Depression, during World War II, and the period until I acquired the PhD, our relationship was the core of our lives. We had time for each other and the children, but not much else. In 1953, when I acquired the PhD, twelve years after Jeannie and I first met, we had a "normal" American income, even though a regular income was never an issue in our relationship. While I was getting two graduate degrees and was employed full time at ANSP, I probably spent 60-80 hours each week on professional activities. I never kept track of time – just what goals had to be met. The field survey team worked seven days a week, often working to or past midnight (collections had to studied and processed). This schedule never seemed burdensome to me because the "work" was exciting, and my colleagues and I did not regard ourselves as "workaholics." I gradually learned that other research investigators spent comparable amounts of time without regret or feelings of sacrifice. In most fields of research, one must reach a critical mass of accomplishments for even modest success, although success is never guaranteed.

Major Co-evolution Begins

When I acquired the PhD in 1953, the amount of free time I had increased dramatically. Although I took a 1-year postdoctoral course in isotope methodology at Hahnemann Medical College in 1954-1955, my years of formal education were essentially over. While at our house in Plymouth Valley, Jeannie had discovered the Ethical Culture Society (ECS) in Philadelphia, PA. The entire family went to the meetings on weekends. We had both been interested in ethics – Jeannie especially due to her Penn State mentor Dr. Stevens. The building owned by ECS was on Rittenhouse Square in Philadelphia, not far from ANSP. Now we could socialize at the meeting house and visit homes of other members. At an ECS social event, we became interested in folk dancing.

About 1950, my father purchased a small cottage in Surf City, Long Beach Island, New Jersey, between Barnegat Bay and the Atlantic Ocean. At that time, only one main thoroughfare existed, with small dead-end streets that led either to the bay or ocean. Jeannie, Karen, and Stefan spent the entire summer there.

I kept a small, wooden boat at the end of the large marshland on the bay side. We usually drove to the boat because I kept the "powerful" 5-horsepower (yes, it was really 5) outboard motor in the trunk of the pre-World War II Chevrolet sedan my father had given Jeannie and me. Summer was the busy time for me because of field trips at ANSP, so I could only get to Surf City on weekends when I was not on a field trip. Jeannie's Aunt Francis and my father were always there on the weekends, and Aunt Fran loved to cook.

Summers there were peaceful and tranquil times for the entire family, but especially for Jeannie and me. At the northern end of Long Beach Island, an inlet provided access for boats to and from Barnegat Bay and the Atlantic Ocean. A beautiful lighthouse (aren't they all?) with a parking area also sat there. A long spit, perhaps over one-half mile long, was at a right angle to Long Beach Island, which led into the bay. On one abandoned telephone or power line pole sat an osprey nest. Even on hot summer days, a good breeze stirred. We sometimes hiked to the end of the spit (I carried Stef in a baby back harness and Karen walked) for a picnic. At that time, most of the small islands in the bay had large numbers of nesting gulls, including skimmers, which were fun to watch. Sometimes Jeannie and I would take the boat across the bay and hike up a tree- and brush-lined, tea-colored, freshwater tributary stream. In August, we would lie on the beach at night and watch shooting stars (the Perseids) until the mosquitoes drove us indoors. Some summers, Will Snyder (the only house on the bay

side in the early days) would take Jeannie and me sailing in his Barnegat Bay skiff. Of course, we always swam at least twice daily when I was there for a full day. During weekends, we went to the summer theater at Beach Haven on the island. Summer weekdays when I worked at ANSP (between field trips), I would leave just before 5 pm to beat the Delaware River Bridge peak traffic and drive the 70 miles (we didn't worry about carbon footprints back then) to take a swim in the ocean, have a quick dinner, read a story to the children and tuck them into bed, take a brief walk on the beach with Jeannie, and go to bed. Some days, I rose early, had breakfast, and started the 1½ –2 hour drive to ANSP. Naturally, I could not manage this schedule every day. This time was, in a sense, a long delayed honeymoon with two children along. However, the most important factor was that, when I finally had a significant amount of free time, Jeannie and I wanted to spend it together in ways that mostly involved the children.

In 1953, we purchased the 110-year old gatehouse of a large estate near the Philadelphia Country Club in Gladwynne, PA. It stood on 2½ acres of wooded land with a small pond. As soon as we saw it, we knew it was our dream home. A tract home was acceptable for a few years, but this land had a heron, pheasants, and a fox. The realtor had given us the wrong key the day went to see the house, so we saw the inside only after we had signed a purchase agreement.

The kitchen was large but archaic – the single bathroom only marginally better. The second floor had a large bedroom overlooking Woodmont Road and was protected by a huge yew. The other bedrooms were tiny, even by the Spartan standards of the 1800s. The staircase was steep and narrow, with a sturdy railing on one side. On the first floor was a tiny living room, which was protected by the same huge yew that shaded the master bedroom. The dining room was large and had a huge multi-paned bay window that almost covered the side overlooking the driveway and hillside field above it. I had a large desk on the side opposite the bay window. The side backing the second floor stairs had a large cupboard with bookshelves behind glass doors. A door to the basement was nestled between the cupboard and the bay window. Marbles in the dining room rolled toward the desk. The basement had an oil furnace that sent steam to other parts of the house. The ancient electrical wiring (exposed in the basement) consisted of two substantial, parallel wires that were protected at intervals by insulators. The 250-gallon oil tank was in the basement below a small window near the driveway. The roof consisted of cedar shingles. I could see light though the cracks, but the shingles swelled and closed the cracks as soon as they got wet. The living room and dining room floors had at least six layers of paint, which I removed with a large sander and sandpaper and varnished once the paint was removed. I put four jackposts in the basement to shore up the large dining room floor. I also sanded and painted the "dewlap" shingles on the second floor outside walls. The metal roof of the kitchen and the shed behind it required tar, and I remember putting new tar on it when Prophet Jones, in his white ermine robe, was giving an address to a large crowd from the balcony of the estate house. At the end of the house was a tiny garage in which the wheelbarrow, bikes, and sleds were kept.

Most women would have had hysterics after seeing the interior of the gatehouse, especially since we first saw it after purchasing the house – but not Jeannie. The fine, old trees on the 2½ acres were superb and the old house was picturesque. The entrance to our driveway was flanked by two enormous stone columns that held up two huge wrought iron gates. A quaint, hand carved, wooden door under a stone arch went from one column to the house. Our gate house was once for deliveries, work persons, etc., so, because we had two small children who would play on the driveway, closing the gate would stop any through traffic. The new owners of the estate, Palace Missions Incorporated (headed by Father Devine), graciously agreed and erected a substantial but aesthetic barrier on the other end of the road where the two properties joined.

Jeannie was entranced when she first saw "our" gatehouse and so were the children. They had a "thinking tree," a very old Japanese maple that they could climb and sit in and dream. The financial situation was challenging for a few months, but Jeannie and the children stayed at Plymouth Valley while I sanded and varnished floors at the gatehouse. The large bedroom had many layers of wallpaper – some layers covered with paint – that Jeannie and I steamed and removed. Underneath the wallpaper were numerous cracks, but Jeannie found a paint that was almost a paste. We spread it over the walls and cracks and stippled it with a special tool that came with the paint. Between the kitchen and the garage was an enclosed, unheated space with a door on the driveway side and a window on the wooded side. The metal tarred roof that covered the kitchen also covered this space. One day Jeannie removed both ends and made a breezeway.

Many of the regional mills had closed after we purchased the Plymouth Valley house, and sale signs popped up all over the development. Ultimately, we lost nearly 20% of the value of our first house and, for a few months, owned two houses. However, we were in a house where we knew we belonged. Initially, we felt as if we were living in some remote area, although later it was "discovered" and many houses were built on Woodmont Road. The schools were first-rate, and just down the hill was the Schuylkill Expressway that got me to the ANSP parking lot in about 20 minutes. My colleague, entomologist Selwyn (Sam) Roback, lived further out in King of Prussia, so we could carpool to ANSP. Thus, Jeannie had our Volkswagen "bug" every other day.

Finally the Plymouth Valley house sold, and I was able to return the thousands of dollars my father had lent us. Despite the risks we had taken and the hard work, new bonds were forged between Jeannie and me. The children had been involved in the selection of our new house and were pleased with it and the far better school system.

Post-degree Research

In 1953, my dissertation was published (Cairns 1953) – a requirement of the University of Pennsylvania – and I had three other publications, which was a slim array for a research career. Isaac Asimov once defined academic freedom as

extramural funding. My mentor, Ruth Patrick, was a past master at acquiring grants and contracts, and, from 1948 to 1953, I was entirely dependent upon her for extramural funding. In 1953, I began to acquire grants and contracts on my own, but could not have survived as a research scientist without her help. Fortunately, I had taken Professor L. V. Heilbruns physiology course while a graduate student, so when Ruth Patrick acquired a grant to study the effects of increased water temperatures from power plants and reactors, I felt somewhat prepared. The research was on freshwater fish, invertebrates, and diatoms. Consequently, Ruth Patrick introduced me to studies on three levels of the aquatic food chain. The results were interesting, and I even was invited to present a paper at the 10th Annual Purdue Industrial Waste Conference. Professor Don Bloodgood, who was in charge of the conference, published my paper and also saw that it was published in two parts in *Industrial Wastes* (Cairns 1956a,b). This valuable experience not only acquainted me with a new research area but also with the discipline of waste treatment engineering, which had money to support the research. Still, this new research area was another risky undertaking, which Jeannie fully supported. The risk turned out to be a justifiable one since a wide variety of engineers had become interested in toxicity testing. This research on toxicity of chemical substances to aquatic life continued through the 1950s to the early 1990s. In addition to engineering journals, I used *Notulae Naturae* of ANSP for publishing many papers. Classical biological journals did not welcome toxicity testing manuscripts, although they involved living material. The few journals that were willing to consider the manuscripts lacked experienced reviewers. In 1966, I managed to have an article in *Progressive Fish-Culturist* (Cairns 1966) and, in 1967, an article in *Scientist and Citizen* (Cairns 1967). Fortunately, engineering journals kept alive my hope that environmental toxicology would be accepted outside of engineering.

In 1961, I began teaching, first at RMBL (1961-1963) and then at University of Michigan Biological Station (1964-1970), and I continued at these two field stations through 1994. In the academic year 1962-1963, I taught an all-day Saturday course in physiology for the National Science Foundation Institute to high school teachers. I enjoyed teaching, and now I had three courses I was confident in teaching in case my research career failed. Jeannie's support never wavered despite the fact that, in 1963, I turned 40 years old and was working 60+ hours per week and still had not accomplished anything notable.

Jeannie and I had always liked hiking in natural systems, but hiking developed into a necessity in 1961, our first year at RMBL. After that, we felt that a 1- or 2-mile hike daily, except in inclement weather, was a necessity. We felt serene hiking the Appalachian Trial or even the second growth forest in which our Virginia house was located. Some Indian tribes call trees "the standing people." In Greek myths, spirits known as hamadryads dwelt in the trees of sacred groves (Little 2008, p. xiii), but were not immortal – if a tree died, its spirit died with it. All old growth forests are sacred, but even second growth forests have a spiritual quality – both Jeannie and I felt this very strongly. Of course, we had hiked on the beach on Long Beach Island, New Jersey, where my father had a summer cottage, but not until we got to the Colorado mountains did daily hikes become a necessity. When we arrived at Virginia Tech in 1968, swimming 1 mile in indoor pools became a daily, weekday event. Of course, we had both swum as children, summers at Long Beach Island in the 1950s and summers in Douglas Lake at the University of Michigan Biological Station from 1964 through 1984, but the activity was then seasonal.

In the late 1950s and early 1960s, I began to spend increasing amounts of time on administration. In 1960 and 1961, I had no journal articles, no books, no chapters in books edited by others, and not even a single abstract. This period was scary for a person who hoped for a research career with significant teaching responsibilities. Jeannie's support never wavered, but I had significant doubts. I was in a new field, environmental toxicology, which was not exactly a "hot, new" field. However, I was confident it would become a major factor in environmental decisions, which it did.

The Defining Decision

In the mid-1960s, I decided I needed more control of my time to develop a computer interfaced toxicity monitoring system. I had been at ANSP (from 1948) for so long that colleagues assumed I was there for life. In 1965, I had only one tempting offer from a small, quality university, but it required major administrative time and starting a biology department that would award the BS, MS, and PhD degrees.

Some of these events have been covered elsewhere in this autobiography, but I am emphasizing here the events that could have strained or destroyed the relationship between Jeannie and me. We both grew up in the Philadelphia area and had lived there for 43 years (1923-1966). We would be leaving a secure position at ANSP, family, friends, and the superb Lower Merion Township school system – all for the hope that I could manage to develop a significant research career.

The best employment offer I had in spring 1966 was from the University of Kansas (KU). I gave the customary seminar and described my research plans to the faculty and various administrators. I was shown an old frame house and was asked if it would be suitable for my research; it would have been sufficient for the first phase. I assumed that, if my research was satisfactory, then more suitable space would be found. I never saw the house again, nor was it ever mentioned.

When Jeannie and I went to KU, we assumed it would be for the rest of our lives. We were both 43 years old, and the typical faculty retirement age was 65. Shortly after I arrived, I was shown my "research space" on a take-it or leave-it basis. It was totally inadequate. My heart sank! What had I done to Jeannie and the children? I couldn't bring myself to discuss it. I would simply work hard and hope for the best.

Jeannie's amazing adaptability soon resulted in: (1) starting a folk dancing group at KU, (2) getting deeply involved in Head Start, (3) becoming, as a couple, members of three square dance groups, and (4) joining the local Unitarian

Fellowship as a family. Jeannie was obviously aware of the problem with my professional position, but we never really discussed it.

In retrospect, my despair was comparatively short lived, although, at the time, it seemed endless. I began getting increased numbers of seminar invitations from academic institutions. I had acquired enough extramural funding for my research needs and worked on manuscripts, which I had not had time for in years past. Jeannie had no intention of becoming depressed, and her attitude kept my spirits much higher than they would otherwise have been. Jeannie felt that, since we were in Kansas, we might as well enjoy square dancing and the local people.

Lift Off at Virginia Tech

In spring 1968, I had four interviews at quality universities. I required detailed specifications of my research space – I had learned an important lesson at KU. Although Virginia Tech was lowest in the national rankings, I accepted a research professorship and temporary research space until a new building was completed. Shortly after I had arrived at Virginia Tech, I was asked to visit the university's president. The KU Chancellor and Provost had called him to say that, if they had known of my circumstances, I would still be at KU. Their acts were among the kindest I have experienced in my lifetime, and the immense gratitude I felt has not diminished.

Bob Paterson (Head, Department of Biology), his wife Marion, and his family, who were old friends from summers at the University of Michigan Biological Station, made the transition to our new home less difficult. Marion even found a house for us in a very tight housing market. Two graduate students, Tom Waller and Rip Sparks, came with me from KU, and two other graduate students, Ken Dickson and Jean Ruthven, joined me at the University of Michigan Biological Station for the summer session, and then we all met at Virginia Tech. Without these fine young people, a "lift off" at Virginia Tech would not have been possible. With the combination of adequate quality research space and highly motivated graduate students, the situation improved dramatically. In just over one semester, we had funding for the computer interfaced monitoring systems, for protozoan colonization dynamics research, and, somewhat later, aquatic ecosystem research. Although I would never have believed the possibility at that time, funding would be unbroken beyond my retirement in 1995. Publications also took off. The research space was critical!

Not until 1971 was there persuasive evidence that my research program was moderately well established. The Aquatic Ecology Program had developed nicely: (1) five faculty had joined the program, (2) graduate student enrollment was heartening, and (3) extramural funding had increased substantially. In 1970, I was asked to become Director of the University Center for Environmental and Hazardous Materials Studies. The Aquatic Ecology Program in the Department of Biology and my research was increasingly transdisciplinary, so the extreme administrative effort and time were justified.

Jeannie was a remarkable person! She selected the most interesting opportunities available to her at any particular time. She always focused on now – not what was or what might have been. Jeannie found the campus YMCA and was immediately at home. When awards for the research program I had started began shortly after our arrival at Virginia Tech, Jeannie treated them with the same detachment with which she responded to setbacks – one's "inner light" should not be markedly changed by either.

Our Family

I was an undergraduate student until Karen was 2 years old and a full-time graduate student until she was 3. After that, I was holding down a full-time job and working on my MS research. Karen and Jeannie actually accompanied me on the first summer of the Conestoga River survey. Stefan was born in 1949 – the year I acquired the MS. Both Duncan (1954) and Heather (1959) were born after I acquired the PhD. Stefan graduated from high school in Lawrence, Kansas, and went to Montana State University. By the time we got to Virginia Tech, only Duncan and Heather were with us.

Jeannie was not a good cook or housekeeper. Above all, she hated being a hostess. Once she said to Karen, "Perhaps I should have taken home economics instead of biochemistry." Neither Karen nor I could visualize Jeannie in home economics. She did like things to be clean and solved that problem by favoring small houses. I enjoyed cooking, especially breakfast, so that problem was solved. She was not particularly interested in clothes, makeup, or household furnishings. Karen remembers that we dropped her off at Colorado College at age 17 directly from RMBL, also in Colorado. She had a small suitcase and a backpack. Jeannie observed the other well dressed freshwomen and whimsically wished Karen "good luck."

While Jeannie was still alive, we lived in the present; but, now that she is gone, I sometimes reflect on the past. As a start, I have never been able to visualize being married to anyone but Jeannie. We both loved our children dearly, but neither of us was a conventional parent. I also cannot visualize being anything other than a research scientist, but had that goal not materialized, I am confident I could have earned money in some other activity. My life would have had less joy, but many other people have endured such lives. Jeannie could definitely have obtained a good position as a biochemist, but that career would not have permitted the freedom of choice in which she flourished. She could handle all the risks, and we hoped each time that changes would not adversely affect our children. Even the two unfortunate years at KU, which did adversely affect our children, did not alter our view of the risk. Even with 20/20 hindsight, the decisions seemed appropriate.

Kahlil Gibran (1923, pp. 21-22) states:

Your children are not your children.
They are the sons and daughters of Life's longing for itself.
The come through you but not from you,
And though they are with you yet they belong not to you.

You may give them your love but not your thoughts,
For they have their own thoughts.
You may house their bodies but not their souls,
For their souls dwell in the house of tomorrow,
which you cannot visit, not even in your dreams.
You may strive to be like them, but seek not to make them like you.
For life goes not backward nor tarries with yesterday.
You are the bows from which your children as living arrows are sent forth.
The Archer sees the mark upon the path of the infinite,
and He bends you with His might that His arrows may go swift and far.
Let your bending in the Archer's hand be for gladness;
For even as He loves the arrow that flies,
so He loves also the bow that is stable.

The Empty Nest

Duncan got his BS from Virginia Tech and left for a MS at Florida State; shortly after that, Heather left for Swarthmore College. All four of our children acquired graduate degrees. We missed the children, but Jeannie and I enjoyed each other as much as we did when we first met. We loved summers at field stations until I officially retired in June 1995.

In 1999, I realized we should no longer live in our "treehouse" on a steep hillside on Bishop Road. Ice and snow made our long driveway impossible, and a power outage was a disaster. In early 2000, we moved into a townhouse at Warm Hearth Village, a local retirement development. We enjoyed concerts, writers group, and hiking on the many Warm Hearth trails in the woods.

The Long Goodbye

In May 2001, Jeannie complained about severe pains in the left side of her chest. Our local hospital did extensive tests, but could find nothing. Since Jeannie very rarely complained about pain, this incident caused me deep concern. She was definitely not acting normally. In June 2001, the pains returned and were worse. This trip to the hospital revealed substantial blood clots in her left lung. Her rapid decline emphasized that I would no longer be able to care for Jeannie. After discharge from the hospital, she went directly to the nursing care unit at the Kroontje Health Center in our retirement community.

Suddenly, after many years together, our lives were dramatically changed. I could walk from our townhouse to see Jeannie in about 10-15 minutes. She could not understand her new situation, and I could not explain it to her. Sometimes when she first saw me on a visit, her face would crumple up as if she feared I had been gone for good.

I tried to visit three times each day for meals. We were invited to join two other couples for lunch and dinner – three men whose wives were in the nursing unit. All us men ate before or after the wives' meals so that we could help them. I was there for breakfast – the other men were not. Lunch and dinner were the big social events of the day. Jeannie recognized me and the children, but I doubt she recognized the grandchildren, who could not make the long trip from their homes easily. At that time, Stefan in Warrensburg, MO (863 miles), Karen lived in Louisville, KY (425 miles), Heather near Ann Arbor, MI (550 miles), and Duncan in Tallahassee, FL (about 715 miles).

Jeannie started in a shared room at the nursing unit, then a private room with a shared bathroom, and finally a private room. Then, she fell and broke the same hip twice about ten days apart. Before that, I had a companion with her eight hours daily. After her falls, companions sat with her around the clock until she became incapable of falling out of bed. At that time, companions were present for only sixteen hours daily. The companions were always there when Jeannie was awake. The service placed four, fine women with her, and Jeannie obviously recognized them and felt reassured when they were there. Occasionally, all of us saw brief flashes of the earlier Jeannie, and we rejoiced each time.

I may have been delusional, but I always felt that Jeannie knew when I was there. When I read folk tales to her, her facial expressions indicated she was paying attention. However, in February 2005, she had aspiration problems and was sent to the local hospital. Our family was told she would not recover. I sat beside Jeannie's bed during the morning of February 21, 2005. When I went back to the assisted living facility for lunch, Karen and Heather sang the songs Jeannie sang to them when they were children and we could not afford a car radio. Jeannie quietly died while they were singing. One minute she was there and the next minute she was gone. Few women would have tolerated our low income for as many years as Jeannie did. Few spouses would have maintained a supportive attitude while my research career required so many sacrifices and simultaneously remained such a wonderful companion.

One of Jeannie's favorite books was Kahlil Gibran's *The Prophet*, published in 1923 by Alfred A. Knopf in New York. The section in that book on marriage captures my and Jeannie's evolving relationship more closely than anything I have read.

> *Then Almitra spoke again and said, and what of Marriage, master?*
> *And he answered saying:*
> *You were born together, and together you shall be forevermore.*
> *You shall be together when the white wings of death scatter your days.*
> *Aye, you shall be together even in the silent memory of God.*
> *But let there be spaces in your togetherness,*
> *And let the winds of the heavens dance between you.*
>
> *Love one another, but make not a bond of love:*
> *Let it rather be a moving sea between the shores of your souls.*
> *Fill each other's cup but drink not from one cup.*
> *Give one another of your bread but eat not from the same loaf.*
> *Sing and dance together and be joyous,*
> *but let each one of you be alone.*
> *Even as the strings of a lute are alone*
> *though they quiver with the same music.*
>
> *Give your hearts, but not into each other's keeping.*
> *For only the hand of Life can contain your hearts.*
> *And stand together yet not too near together:*
> *For the pillars of the temple stand apart,*
> *And the oak tree and the cypress grow not in each other's shadow.*
>
> *The Prophet*, Kahlil Gibran, 1923, pp. 19-20

Shakespeare's Sonnet 116 *Let Me Not to Marriage of True Minds Admit Impediments* complements Kahlil Gibran's statement on marriage.

> *Let me not to the marriage of true minds*
> *Admit impediments. Love is not love*
> *Which alters when it alteration finds,*
> *Or bends with the remover to remove:*
> *O no! it is an ever-fixed mark*
> *That looks on tempests and is never shaken;*
> *It is the star to every wandering bark,*
> *Whose worth's unknown, although his height be taken.*
> *Love's not Time's fool, though rosy lips and cheeks*
> *Within his bending sickle's compass come:*
> *Love alters not with his brief hours and weeks,*
> *But bears it out even to the edge of doom.*
> *If this be error and upon me proved,*
> *I never writ, nor no man ever loved.*
>
> William Shakespeare (1564 - 1616)

Retrospective

In retrospect, mine and Jeannie's coevolution was made possible by a few shared values.

(1) We wanted to spend our lives together. Possibly neither of us could have explained why – I certainly could not, although I could make a list based on 20/20 hindsight.
(2) Neither of us placed much value on material possessions (i.e., stuff).
(3) We both loved natural systems and felt serene when we were in them. To the extent possible, we tried to live in houses that were in natural systems.
(4) Neither of us attempted to change the other to fit some socially acceptable stereotype (e.g., model hostess, man of the house).
(5) Clothes were utilitarian, not decorative.
(6) Daily shared activities were important (e.g., folk dancing, hiking).
(7) Each of us should have personal activities for which the time was "sacred."

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CHAPTER 33

BECOMING A RISK TAKER

Being careful kills the soul. William Saroyan

Oh, Magoo, you've done it again! Mr. Magoo (Jim Bakkus)

Like Mr. Magoo, humankind is taking risks of which it is totally unaware. Since the global crisis is not a cartoon, the outcomes will be more tragic than in the Mr. Magoo cartoons.

One doesn't discover new lands without consenting to lose sight of the shore for a very long time.
Andre Gide

The important thing is this: To be able at any moment to sacrifice what we are for what we could become.
Charles Dubois

It is better to have enough ideas for some of them to be wrong, than to be always right by having no ideas at all.
Edward de Bono

Creativity requires the courage to let go of certainties. Erich Fromm

I have always regarded myself as an unadventurous, almost plodding individual. I do have justification for this view of myself. In elementary school, I was never combative. In high school, although I was much taller and heavier than my classmates, I played a clarinet in the band instead of being on the football team. In my adult life, I never missed a plane flight, although I took about 35 flights in some years (Carbon footprints were not a concern back then, and, by the time they became a concern [late 1980s], I had markedly reduced my travel.). However, reflecting on my life from the vantage point of almost 86 years, the evidence does not support the view that I did not take risks in my life.

The summer after high school graduation, I began a construction job that only lasted a few days when I realized I could not stroll casually around on roofs of two-story houses – too much of a risk for me! I then acquired a job in a paper mill (Chapter 26 in this volume) in Miquon. This job was not adventurous, but the wages were good.

I never had a date in high school (too much a risk!) and only dated my freshman year at Penn State because the rest of Alpha Zeta, my fraternity, did. For the October formal pledge dance, I invited Christine French from my home town of Conshohocken, Pennsylvania, who played a clarinet next to me in the band. Fortunately, she accepted, so I was not a dateless object of scorn. My fraternity brothers talked me through the formal dance process with considerable amusement – renting a tuxedo, ordering an orchid. Their dates tutored me in the rudiments of dancing and finding overnight accommodations. Since Christine was my age, 17, her brother and his fiancée came along as chaperones and drove Christine to Penn State – transportation was something I had not even considered. Later in the semester, some of the co-eds who dated my fraternity

brothers found local dates for me out of pity or kindness, and, in the second semester of my freshman year, I even managed to acquire dates for two formal dances and Saturday evenings on my own. Fortunately, in fall 1941, I met Jeannie and the risk involved in asking girls for dates was solved.

I enlisted in the US Navy after the Japanese attacked the naval base at Pearl Harbor in the Hawaiian Islands in 1941. I would probably have been drafted anyway, but I wanted to choose my branch of the military – the Navy. In any case, even risk adverse people must take chances during unusual circumstances. Getting married during World War II was an optional risk. Jeannie and I were warned that wartime marriages would probably not last, but we had almost 64 years together before Jeannie died in February 2005.

Continuing my college education after World War II to complete my undergraduate degree (from Swarthmore College in 1947) seemed to be a necessary risk even though the timing was not the best because of finances. Undertaking this endeavor meant not having a home of our own for Jeannie and our new daughter Karen. We lived very frugally with Jeannie's mother. After this AB was achieved, we decided I needed to get a graduate degree at the University of Pennsylvania. Jeannie and I were both acutely aware that a routine job without new, different intellectual challenges on a regular basis would not suit me. Jeannie also knew that she was not suited to be the wife of a person who was leading a conventional life. We later decided that I needed to get the necessary academic credentials (an MS and ideally a PhD) and hope that an attractive opportunity would develop. Money was necessary, and a challenging position for me with enough money for frugal living was the goal. The opportunity came after one year of graduate school (details in Chapter 21 in this volume) in the form of a summer position in the newly founded Limnology Department of the Academy of Natural Sciences in nearby Philadelphia. The work that summer was to be on a team surveying the Conestoga River Basin and its tributaries. After the summer work, I was offered a position on the newly formed river survey team. Utopia – right? One significant flaw emerged – salaries were entirely dependent upon outside grants and contracts. On the plus side, I could be a candidate for the PhD in the Zoology Department at the University of Pennsylvania while being employed on the survey team. Obtaining the PhD in this manner meant long hours (60-80 hours each week) and many years (actually four).

Due to my mentor Ruth Patrick's skill in obtaining extramural funding, the cash flow was steady, although some anxious moments did occur. The river surveys were, initially, the major source of funding, but the environmental toxicology program quickly began to get contracts, and grants came from diversified sources. The Limnology Department began to grow slowly, even though it was still a small department and the type of research being done was not then common. Our situation seemed more secure, so Jeannie and I bought a tiny house and decided to have another child (all of our children were planned). My mother, who died while I was in the Navy, left me a few thousand dollars. I suspect this inheritance was partly the money I had turned over to her from my two summer jobs when I was a teenager. I used this money to reduce the mortgage payments to a level we could afford. In retrospect, buying a house was still a risky situation: (1) could I work the long hours needed for both a full-time job and candidacy for the PhD at an academic institution with high standards? (2) could Jeannie care for two children while I was in the field for four or five major river surveys each year? (3) would the cash flow from grants and contracts continue? (4) what effect would this complex/stressful situation have on the two children?

Isaac Asimov was once asked to define academic freedom. He reportedly responded "outside money." Cash flow was one reason I avoided taking a sabbatical at any time in my career. I owe much to Ruth Patrick, who served as my model for obtaining extramural funding. One can do research of one's choosing if one can find money for it. On average, about 142 hours, including staff time, was needed to prepare a grant proposal – with no assurance that the proposal would be funded. Small proposals took less time, but did not provide the stability of cash flow that larger grants did. My view was that taking a year off would endanger the cash flow that was essential for keeping even a small group of graduate students, technicians, and faculty continually funded and meeting the requirement of the grant proposal. I felt reasonably confident in taking a summer off every year, most faculty did, but an entire year seemed far too risky. In any case, this approach worked – my research was supported by continuous grants and contracts from 1948 until 1997 (two years after I retired). Colleagues have given me enthusiastic accounts of an entire year spent traveling in Europe, Australia, and so on. I enjoyed the pictures and tales, but felt no envy – that was not for me! Last, but far from least, extramural funding meant publications in professional journals, since not publishing research results would lead to the funding drying up. Publications plus extramural funding plus a good teaching record improves one's candidacy for academic positions if changing institutions becomes necessary. I have always considered the risk of being in an institution whose goals and objectives are no longer congruent with my own to be unacceptable, which could happen at any time in one's career.

Fortunately, my father purchased a small cottage in Surf City, Long Beach Island, New Jersey, and Jeannie, Karen, and Stefan (our second child) could spend summers there with my father and Jeannie's Aunt Francis. Summers were the most intense working period for the river survey team, but I went to Surf City as often as my survey schedule permitted. Even so, I never had a traditional, American style vacation (i.e., two or

more weeks off) or a sabbatical year until I formally retired in 1995 at 72 years of age. This lack of relaxing time might seem a hardship if it were not for the joy I feel for my research. Besides, for 33 years, the family spent summers at field stations in the Colorado Rocky Mountains or at a beautiful lake at the tip of the lower part of Michigan. Except for the first three years in Colorado, the entire family ate three meals each day in the field station dining room. As Jeannie said, any meal she didn't have to cook was a banquet. So, the family spent summers at the beach or at field stations that were small communities of people who thrived in natural surroundings. Even though being away from the children and Jeannie for extended periods while I worked or did research was a risk to my family life and marriage, this risk was mediated by family and associations with people who felt like family.

Summers at field stations provided a superb opportunity to both hear about and observe research carried out by colleagues at other institutions in North America and abroad. Opportunities were available to test the validity of my current research by giving a pre-publication seminar. I met nearly half of my graduate students at field stations, which presented an unmatched opportunity for them to observe my research and for me to observe them. Students at field stations tend to be very highly motivated and to enjoy field work – an essential component of environmental studies. The field stations where I worked for 33 years also had good trout fishing. At Rocky Mountain Biological Laboratory in Colorado, one good trout fishing site was on the laboratory property. At the University of Michigan Biological Station, Douglas Lake served as a fishing spot right on the station property, but I usually drove about five miles to the west branch of the Maple River for trout fishing. I could even fish at night for brown trout. Trout fishing requires constant concentration – selecting a spot to float the fly past, making a cast with no drag on the line, setting the hook (I used barbless hooks) at just the right time. For me, trout fishing was a form of transcendental meditation – mind totally clear of all thought except fishing. I must admit that trout fishing provided me with the respite I needed from the risks I was taking, even if I didn't realize the magnitude of the risks at the time.

A danger is always present that the life path of one's spouse will diverge from one's own. Fortunately, this problem never occurred for Jeannie and me. Jeannie and I co-evolved rather well, although we each always had our own "space." However, divergent careers could obviously be troublesome, especially if young children must be considered. Once or twice I have speculated what would have happened had I not met Jeannie in 1941 because she supported my quest for the PhD, which was essential to a research career, and she understood the many trials at the outset of a research career. I image that I would have immediately returned to school after the war ended and remained an academic hermit until I acquired the PhD in 1953. One of the risks of engaging in scientific research is a reduced time for socialization in the early stages of the career. For this scenario, my imagination is inadequate.

Of course, by the time I was established as a research scientist in the early 1970s, the two oldest of our four children were on their own, and our third child Duncan was an undergraduate at Virginia Tech and the fourth one, Heather, was in her teens. I had more time for family activities. From 1966 on, I was not away from home for weeks at a time on the river survey team.

The uncertainties involved in acquiring grants and contracts remain constant since the competition for research funding has always been high – as it should be. The usual problems of carrying out research were always present – equipment failures; a colleague who promised to take responsibility for a component of an interdisciplinary grant and did not, so a replacement had to be found; critical space taken away at an awkward time (this rarely happened); bad weather (e.g., heavy rains with floods). Such problems markedly alter the schedule of a field project. In the case of the Savannah River site, long delays were experienced with perishable samples from riding slowly behind the heavy equipment used in the early days of construction of the plant. However, none of these were major risks that could have profoundly affected my career.

In the early 1960s, a major threat to my research career developed so gradually that I was not fully aware of it until several years had passed. More and more of my time was being devoted to administration at the Academy of Natural Sciences and less and less to research. I had been teaching and carrying out research and also taught an all-day class on Saturdays for the full academic year. I was trying to "have it all" and dissipated my energy in the process. I could have given up teaching and used that time for research. Even though I found some of the administrative problem solving interesting, research and teaching were fascinating! I appeared to be losing time for the activities that gave me the greatest joy in my professional life.

In 1965, when I had already been a full curator for four years, I was offered a full professorship at a quality university with a substantial increase in salary and tremendous fringe benefits (e.g., free tuition at nearly 30 colleges and universities for all my children). The university had a good reputation in chemistry and engineering, so I would have fit in well. The drawback was that I would be charged with founding a department of biology that would ultimately grant the BS, MS, and PhD degrees. The administrative load was just too heavy, so I reluctantly did not accept the offer, despite the very attractive financial rewards. The next year, a state university offered me an assistant professorship without tenure after hearing some of my seminars. Since the Academy had historically strong ties with the University of Pennsylvania and used comparable standards for

tenure and promotion, I was insulted. I had a far better publication record than the full professors who made the offer. Naturally, I refused the offer. Shortly afterwards, two other full curators in limnology were offered assistant professorships without tenure by the same institution. They also turned down the "offers."

I had other offers, but, in 1966, the University of Kansas made an offer that seemed best suited to my goals. The offer was for a full professorship with immediate tenure. I would take a loss of salary (a 12-month position to a 9-month position), but the pay was comparable to other University of Kansas full professors at my stage in career development. I accepted. The decision was a disastrous one that almost ruined my research career. I didn't even realize I was taking a risk (full account is given in Chapter 30 in this volume).

Even after I realized the risk I had taken by moving to the University of Kansas was leading to disaster, I didn't want to move until our oldest son Stefan had graduated from high school. When word got around that I had moved from the Academy, a very satisfactory number of invitations to present seminars at other institutions arrived. In early 1968, I had professional offers from four universities, three much higher ranked nationally than the University of Kansas. Each committed to ample research space and all offered a much higher salary than the University of Kansas, although research space was by far the most important factor for me by then. My rapid biological information systems program was off to a superb start, but needed ample research space for the next phase. Two universities were in sizable cities, which did not appeal to either Jeannie or me, even though they would have been great places for my research program. Still, we didn't want to lose daily contact with natural systems. The most prestigious institution of the four was in a superb location with easy access to natural systems. The drawback was a significant administrative requirement. The least prestigious offered the best research space and had easy access to natural systems. Most important was the assurance of the president, dean, and department head that adequate research space would be available for as long as my research productivity continued. My part of the bargain with Virginia Tech was that I would buy all the equipment I needed with grants and contracts and also pay graduate student stipends plus technician salaries. This tall order and major risk would indicate that I was willing to work hard to ensure the success of the program. I had only four graduate students at the time, and Virginia Tech generously offered to pay their stipends for four months. Ken Dickson, Jean Ruthven, Richard Sparks, and Tom Waller, my four graduate students, were the key to getting the program started. We wrote grant proposals and industrial contracts and made cost estimates for field studies. My debt to those graduate students is beyond calculation. Each kept a personal research program going, and we all worked long hours to get the aquatic ecology program started. We never doubted that the program would be successful. Nevertheless, the greatest risk to my research career was this period of starting anew, and these four superb graduate students were there to help me through it. Perhaps the initial stages, with all our work in one large room, enhanced our sense that we had nowhere to go but up.

Goals, Fall 1968

The space research promised to me at Virginia Tech was in Derring Hall and would not be available until summer 1969. A room in very old Price Hall was our sole domain. However, this setup gave us time to prepare grants and contracts and redefine goals. We were realistic and listed objectives for each goal accordingly:

(1) to develop rapid biological information systems so that deleterious effects of chemicals on fish could be immediately detected, displayed, and stored – preliminary research at the University of Kansas demonstrated that these aims were feasible, but more evidence was needed to confirm that the systems would work well.

(2) to study both natural and assisted recovery in damaged aquatic ecosystems (i.e., ecological restoration) to validate the predictive models that were based on ecotoxicological tests.

(3) to research both theoretical and practical components of microbial colonization dynamics – the processes of decolonization and colonization of microbial communities had theoretical interest, but might also be used to monitor the integrity of natural systems.

(4) to develop methodology based on aquatic community complexity and structure that would be applicable in a wide variety of aquatic ecosystems – the method should be both quantitative and qualitative.

(5) to develop and study microcosms and mesocosms that simulated one or more important attributes of stress effects of chemical substances, using end points not possible in single species toxicity tests.

Resources, Fall 1968

The goals of the research program were ambitious, but they were essential for the related fields of ecotoxicology and restoration ecology. We were not daunted because we believed our goals were worthwhile and not beyond our capabilities. The communal office/research space had a large table in a long, narrow room, seven chairs, a small table against one wall, a large coffee pot, five coffee mugs, and a spoon. We had the large monitoring unit on loan from the Water Resources Center at the University of Kansas, a borrowed microscope, and equipment to measure respiratory rate and heart rate of fish that was lent to us by Alan Heath, a biologist on campus. We also had a variety of books, which I had accumulated over 20 years. Tom Waller

remembers that the room was really for group activities and he studied elsewhere. Since the colonization dynamics research needed no immediate funding, Jean Ruthven did microscopy in a quiet area. As a consequence, practically all grant and contract writing was done in the long, narrow room. The graduate students had classes, and I was teaching, so we were not all in the communal room at the same time for much of the day. Of course, the risk taken here was to start an ambitious program with such inadequate resources.

In the early part of 1969, grants and contracts began to be funded and our lifestyle changed accordingly. The waiting period was over and we were busy carrying out as much research as possible before we were able to move to our new space in Derring Hall. Getting extramural funding and the waiting period before one hears whether the proposal has been funded or rejected is always stressful – one would have to be either a fool or a supreme egotist to lack concern. However, what turned out to be one of the critical periods of my professional career was over, but the risks always continued. My professional life and fate were always strongly influenced by my graduate students, colleagues, and staff. Since the original group was tiny and the period was such a decisive one, the bonding was stronger than normal. The risks were great, but so were the rewards.

Graduate Students

Graduate students who entered the aquatic ecology program carried out research suitable for a thesis or dissertation. Most grants and contracts were not proprietary – that is, the sponsor could not determine whether or not the results would be published. The very few exceptions were transition contracts (i.e., between grants for cash flow) and specific funds to buy equipment a graduate student needed. This insistence on no restriction on publication lost the center lots of grants and contracts, but it was the only choice possible.

The Aquatic Ecology Group

Part of my responsibility when I accepted the Virginia Tech biology faculty position was to form an aquatic ecology team. After a few years, six, young, untenured, assistant professors, plus two technicians, were aboard. At the peak of the team's work, 20 graduate students were also studying. The university paid the faculty salaries. One young faculty member disliked team research but did teach courses useful to students. In the 20th century, "lone wolf" research was the norm. Team work was the exception, and tenure and promotion committees did not look on it with favor. The operation was a high risk operation. When the aquatic ecology group members acquired tenure, the group gradually dissolved, although two continued collaboration. Although the head of the Department of Biology was disappointed, he raised no objection. Neither did I – the risk would have been greater in trying to retain the group than to letting it disband. In any research project, enthusiasm, motivation, and confidence are essential, but are not substitutes for a good grounding in science. In this case, the big risk would have been trying to keep the group going.

The UCE&HMS research activities were increasing. Teams were not permanent, so a risk existed for each project that the interpersonal "chemistry" would be inadequate, but the rewards compensated for the risks. This transitional stage was based on a small group of multidimensional people capable of and interested in working together on complex, multidimensional projects that required quite a few years of collaboration to resolve. This period was exciting, and every project brought a feeling of joy and excitement. One might think that risks associated with research would diminish toward the end of one's professional career. I learned to live with them because they were always there.

The University Center for Environmental and Hazardous Materials Studies (UCE&HMS)

In 1970, I was asked to become the director of a new university center, initially named The Center for Environmental Studies. Later, "Hazardous Materials" was added to the title. The charge was to carry out research that transcended the capabilities of a single academic discipline. Since I was already integrating other disciplines into my work, the establishment of the center was merely a formal acknowledgment of the work in progress. I was allocated one secretary/accountant, but no additional space.

A different "team" or array of disciplines would be organized for each project – problems/issues for each situation would not be identical, even though they might be similar. The research was to be entirely funded by extramural money. A portion of my salary was to be paid by the university, and my academic appointment was Research Professor of Zoology. I requested an academic year appointment, although a calendar year appointment was the norm for administrators.

The risks were not entirely new – I had worked on interdisciplinary teams for my entire career. These experiences were with team members completely dependent upon extramural funding for their salaries, and the primary loyalty of the participants was to the interdisciplinary department that employed them. In a university, salaries are dependent on the department, so the primary loyalty in the academic institution is to the disciplinary department.

In addition, participants in a university would serve on the team for a year or two (part-time) and then return to their disciplinary department, possibly never to work on an interdisciplinary team again. Typically some of their colleagues would regard them as disloyal to their departments and question their carrying out applied (as opposed to theoretical) research.

On the constructive side, the best research investigators in all disciplines were intrigued, even excited, at the prospect of collaborating with a team of other like-minded people. Of course, money was available to employ their graduate students and buy equipment, and the data produced were suitable for one or two papers in professional journals (the research was both theoretical and applied). In addition, a huge amount of background data were available for shared use.

Finding the right people was not an easy task, but the volume of research had increased dramatically for the group, and the university provided funds to employ Ken Dickson, who had acquired the PhD, as Assistant Director of the Center. He was astute at assembling interdisciplinary teams and conveyed his excitement for working on such teams.

Nevertheless, such interdisciplinary undertakings are not easy because members of one discipline often have difficulty working with those in other disciplines, especially in temporary situations. Gathering data within a disciplinary framework is well established. Integrating an array of dissimilar components into a document intelligible to decision makers is a daunting task and is not a skill that can be developed in a short time. The complexity of overseeing and financing several interdisciplinary teams was a risk, but one that was productive for over 25 years.

Beyond Interdisciplinary

After some years, I realized that interdisciplinary teams were merely a transitional stage to something more integrated – consilience (literally, "leaping together") of the components (originally the classic disciplines) was occurring, but not at the rate needed to cope with the rapidly developing global problems. For true consilience, a transdisciplinary approach was needed (Cairns 2001). A superb model for this approach was WorldWatch, developed by Lester Brown (e.g., Cairns 1992). This group demonstrates a transdisciplinary perspective in which detecting boundary lines between the classic disciplines is difficult. Classic disciplines are essential to a truly professional study, but are subordinate to the contextual perspective. The challenge of global problems is still stupendous since "A record number of Americans – 41 percent – believe that the seriousness of global warming is 'exaggerated' in the media" (Marshall 2009). The risk to both individuals and humankind of failure to communicate the dangers of global climate change to the general public is appalling.

Ecosystem Recovery and Restoration

My first association with ecosystem recovery and restoration occurred when I was part of a team studying the colonization of a newly created channel of a small, unpolluted stream in Pennsylvania on a project designed and supervised by Ruth Patrick. In the early part of my professional career, studies considered both the causes of ecosystem stress (e.g., toxics) and (1) the natural recovery when the stress was removed or reduced and the (2) assisted recovery when the colonization process was assisted (i.e., ecosystem restoration). When viewed from a disciplinary perspective, the process included both toxicologists and ecologists, who, even at present, have only minimal interactions (with some notable exceptions). Research on stressed ecosystems should include both components, but each basic group has markedly different contextual viewpoints. Ecotoxicologists (using the term now in vogue) used endpoints (e.g., lethality) most commonly associated with species, and ecologists used attributes most commonly associated with systems (e.g., energy flow). This situation has changed appreciably in the last two decades, but "eco" is still not adequately represented in ecotoxicology. Ecology must focus more on anthropogenic stress on ecosystems (e.g., global climate change).

Soon after my arrival at Virginia Tech, a splendid opportunity arose to reenter the area of natural recolonization of damaged streams. The fly ash retention pond dam of an Appalachian Power Plant collapsed, and a surge of fly ash slurry entered the Clinch River near Carbo, Virginia. I was asked to ascertain the biological damage to the Clinch River (it was substantial) and to determine how much natural recovery had occurred. Naturally, this research was a multi-year project – almost a textbook case. Practically all of the fly ash had flushed out quickly (it ended up in a TVA impoundment), and 17 unaffected tributaries and an unaffected headwater supplied recolonizing organisms (Cairns et al. 1971, 1972).

The risk here was taking on too much work, especially in what were commonly viewed as separate research areas: (1) colonization dynamics, (2) rapid biological information systems, (3) ecotoxicology, (4) recovery and restoration of damaged ecosystems. However, at the time, I had ample research space; more students and staff; adequate equipment; and, thanks to grants and contracts, an adequate, reliable cash flow. The risk of multiple activities seemed justified. A better opportunity might never appear. I took the risk.

Restoring Aquatic Ecosystems

In 1988 or thereabouts, I was asked to chair a National Research Council (NRC) committee charged with producing a major report title *Restoration of Aquatic Ecosystems: Science, Technology, and Public Policy*. Sheila David was the staff officer, and, since I had worked with her before, I knew her to be capable and systems oriented. John Berger was available as a consultant. The committee members were: G. R. Best, P. L. Brezonik, S. R. Carpenter, G. D. Cooke, D. L. Hey, J. A. Kusler, C. L. Schelske, L. Shabman, R. R. Sharitz, S. Sorooshian, R. E. Sparks, J. T. B. Tripp, D. E. Willard, and J. B. Zedler. I was then 63 years old and was chairing or co-chairing 12 graduate student committees, was responsible for the UCE&HMS, and was teaching 3 courses each year. The usual extramural funding and publications were also demanding. This endeavor was both challenging and exciting because of the scope and the inclusion of the area of public policy. I quickly decided to accept – if not now when would there be another such opportunity?

The committee was superb – knowledgeable, highly motivated, eager to collaborate with others, and enthusiastic. After the 552-page volume was published by the National Academy Press in 1992, Eugene Odum wrote a book review stating that the book flowed from one subject to another as if it had been written by a single person. This comment described the committee's performance perfectly. Serving with this committee was one of the major high spots of my professional career. Seventeen years after publication, I still hear favorable comments about it and have many fond memories of working with the committee. The lesson from this story is: don't hold back from a very attractive opportunity because of a reasonable risk of an overload of work.

The Last Graduate Students

When I formally retired in June 1995, I was 72 years old, but I was chairing three graduate committees – two PhD and one MS. I needed to fulfill my responsibilities to those students. I had grants to support them and was able to keep my senior technician and editorial assistant. A new director would be named for UCE&HMS, and I had to clear up some activities before then. I was also reading page proofs as an editor or co-editor for five books. After experiencing a blood clot in my right leg in January 1995, I had to avoid the long drive to Rocky Mountain Biological Laboratory, which gave me three extra months for working with the graduate students and wrapping up administrative duties.

Sustainable Use of the Planet

In 1995, I began publishing on sustainable use of the planet and finally published a framework for achieving this goal – "Commentary: Defining Goals and Conditions for a Sustainable World" (*Environmental Health Perspectives* 105(11):1164-1170). The article title also became the title of my first e-book on sustainability – *Goals and Conditions for a Sustainable World* (Eco-Ethics International Union, Oldendorf/Luhe, Germany; also available online at http://www.esep.de/journals/esep.esepbook/CairnsESEPBook.pdf or http://www.johncairns.net under "Archives"). In 2001, I began to publish in the e-journal *Ethics in Science and Environmental Politics* and continued until 2005 when my daughter Heather set up a website for me. In 2004, I began publishing in the *Asian Journal of Experimental Sciences* and still continue to do so. The Editor-in-chief, Professor A. L. Bhatia, very kindly sends pdf's of my articles for me to post on my website, as do other journals. Therefore, my website serves as easy access to my publications. A staff member at the assisted living center where I reside noted that the Internet arrived just in time for the last part of my professional career. The risk of not keeping in touch with geographically distant colleagues, of not keeping track of developments in the scientific world, and of not having instant access to a mind boggling array of information are all reduced by the Internet.

Stochastic Events Occur

"Stochastic Events Occur" was the inscription on a tee-shirt given to me at the last meeting of the National Research Committee that produced the volume *Restoration of Aquatic Ecosystems: Science, Technology, and Public Policy*. In 2009, humankind is well aware of stochastic events, although not expressed in these words. Frantic searches for security still occur, and belief abounds that stochastic events can be prevented. In short, humans refuse to believe that so many events in their lives are random. More important, the quest for the illusion of security may prevent humankind from taking calculated risks in circumstances where it has a significant chance of influencing the outcome.

In retrospect, all the risks I took in my personal and professional lives pale in comparison to the global risks humankind is now unresistingly taking. For example, in the United States, attention is concentrated on the type of terrorism that occurred on September 11, 2001, while humans were unthinkingly pouring ever more greenhouse gases into the atmosphere. Overpopulation is worsening daily, but attention is focused on when life begins rather than the quality of life after birth. Acidification of the oceans continues with no robust attempts at remediation. These and other global problems are rapidly getting to "the point of no return," at which time any attempts at remedial action will be ineffective. Billions of people could die due to dramatic disruption of

agricultural productivity, pandemic disease, or both. "Pro-life" in the United States is focused on anti-abortion. However, what of the billions who will die if overpopulation continues and the food supply decreases due to global climate change? The risks I took voluntarily were nothing compared to the risks I am now exposed to involuntarily.

Present Risks

I cannot avoid the risks of old age – decreased health and vigor, inability to travel even modest distances, and loss of my companion of 64 years. However, I find joy in still making contributions to science and to having assisted living care that enables me to spend more time on enjoyable activities!

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CHAPTER 34

THE ECOSYSTEMS IN MY LIFE

As soon as I could ride a bicycle, my close association with ecosystems began. An early favorite one was Pott's Quarry, which had filled with water when the operators hit a spring at the bottom. The area was not very productive because the sides were mostly steep rock. However, at the foot of the only cliff was a sizable rock ledge with shallow water and a good growth of vegetation. From the top of the cliff I could see about a dozen very large goldfish, which I never saw anywhere else in the quarry. A narrow, paved road was at the end of the area that was away from the town of Conshohocken where I lived. My favorite fishing spot was a rock promontory not far from the road. I could catch bluegill sunfish and, rarely, small bass. I always carefully unhooked each fish and returned it to the water. Optimistically, I expected to catch that returned fish again. Since Pott's Quarry water surface was only a few acres, I did everything a young boy with no fishing mentor would do. I circled the quarry pond and fished everywhere except the underwater ledge below the cliff, which was difficult to access. Except for one "bathing beach," which was occasionally occupied, I usually had the area to myself. My mother and father sometimes took me to a tiny beach down a steep hill not far from my favorite promontory fishing spot. I soon learned that some spots were great at some times and not others.

My other boyhood ecosystem was the Schuylkill River (pronounced SKOO-KILL, Dutch for "Hidden River"), which was badly polluted in the 1920s and 1930s. Only carp and small, brown catfish were in the section my uncle Alex Brown and I fished. The carp were large but not abundant. We caught them with balls of rubbery corn meal mixed with cotton. Chicken entrails were the bait *de jour* for the small catfish. I even built a small wooden boat to fish the river and was deeply offended when a local contractor offered to buy it so he could mix cement in it. "Still" fishing taught me patience – first, the long wait between nibbles and second, not to strike until the carp began its run.

When I was about 13, my family visited the Latshaws, relatives in Salt Lake City, Utah. My cousin Walter and I were taken to Yellowstone National Park with a scout troop. Since we were a few years older than the other scouts, we were allowed to trout fish on our own. We fished a large beaver pond with fly rods and dry flies. On my fist cast, I hooked a 10-inch, male brook trout, and I was "hooked" for life. The next day, both Walter and I came down with dysentery, and our families drove to Yellowstone to take us back to Salt Lake City.

As soon as I got a driver's license, I fished the upper reaches of Wissahickon Creek, a trout stream partly within the Philadelphia city limits. I also fished Valley Creek, a trout stream in Valley Forge National Park. I never hooked a trout at either place. I entered Penn State University at age 17, but had no transportation to access not-too-distant trout streams. In addition, World War II, three academic degrees, and my professional employment prevented me from fly fishing for trout for 21 years. I am thankful that summer employment at Rocky Mountain Biological Laboratory and the University of Michigan Biological Station returned me to trout streams for another 34 years, when I had to stop for health reasons. The trout streams of Colorado and Michigan are quite different, but I studied them and felt at home in each.

The Conestoga and Savannah Rivers

In 1948, I became a member of one of the two river survey teams studying the Conestoga River and some of its tributaries in Pennsylvania. The overall goal of the study was to develop a biological assessment of water pollution. At that time, the Conestoga was ideal for this purpose because much agriculture and some industry were situated in its drainage basin, along with some comparatively pristine areas. My assignment was to study the effects of pollution on protozoan communities (e.g., Cairns 1949). Professor Mary Gojdics, an experienced protozoologist, sat across from me at a long lab table and was very helpful to the novice who had to "run flat out" to maintain the desired pace. As a team, we had to finish each sampling area or station so everyone could begin on the next area/station at the same time. The bacterial, protozoan, and water samples were perishable – the other samples could be preserved for later study. In addition, the main books for identification of protozoans were in German (e.g., Kahl's *Tierwelt Deutschland* for ciliates and Pascher's *Susswasser Flora* for flagellates), although many reference books were in English. Of course, I had some language courses in college, but major, daily use of German was a new experience. I often had to work past midnight, although the teams did not do field work on weekends. I drew sketches of specimens I could not immediately identify and attempted to label them with a Latin name on the weekends. Sometimes I had to settle for the genus name with an "sp." after it. The primary benefit for me of summer 1948 was the development of a systems perspective – I was able to "see" the aquatic communities through the eyes of a bacteriologist, an

algologist, an invertebrate zoologist, an entomologist, an ichthyologist, and a chemist. I also learned to pay attention to organisms that were not my primary responsibility.

At the end of summer, I was offered the position of protozoologist on the single, permanent Academy of Natural Sciences Limnology Department river survey team, which lacked an ichthyologist. The entire team helped gather fish with seines, hoop nets, and by other methods. Ruth Patrick selected the study areas, but she was also running the Limnology Department and writing a massive volume on diatom identification. Funding switched from a single grant (for the Conestoga survey) to single grants for river surveys in various part of North America. These additional studies provided a broad, comparative perspective, but not the same "sense of place" that develops with substantive association with ecosystems.

The Savannah River was the next ecosystem in my life. I think my association with the river began when E. I. DuPont de Nemours and Company assumed responsibility for building and operating the Savannah River Plant. I believe the survey team began work there in 1950 (this is a guess) before major construction began. The first year, the team was on the river four times for two weeks each time. At that time, the Savannah River was not heavily used. For example, the mere sound of an outboard motor would cause the water moccasins to drop off the willow branches overhanging the river. Later on, as river traffic increased, the snakes just stayed on the branches. I worked on the river itself, as well as a tributary called the Upper Three Runs, until I left the Academy in June 1966 and was no longer associated with the survey crew. I can remember that the metal boat seats became too hot to sit on in summer unless we splashed water on them. The crew worked in winters sometimes so cold that small logs froze to our waders when we put them upon our knees to pick off insect larvae. We worked when sudden severe thunderstorms came up and the boom of the dredge boat suddenly looked like a lightning rod.

Although some of the Savannah River Site construction crew stayed in Augusta, the river survey crew stayed at the Plantation Motel in Allendale, South Carolina. The motel was on US Route 301, then a major north/south highway and, in this 1950 era, was the only modern motel in Allendale. The small dining room there served a typical breakfast and a dinner with only two entrees (fried chicken and ham). The only other food service in town was a Dairy Queen with hot dogs and hamburgers. This somewhat limited selection was tiresome for a two-week stay four times a year. Later, a Howard Johnson's came to Allendale. For lunch, the crew dined on "beanie weenies" from a can.

None of the crew took showers until every team member had returned since they might need some help. The crew dealing with perishable samples always returned to the motel early while the samples were still fresh. However, no team member ever stayed on the river alone. A 298-page publication detailed the data gathered through 1966 (Patrick et al. 1967). The experience was a superb one that I treasure.

Some years after joining the staff at Virginia Tech, I was invited to become a member of the Environmental Advisory Board of the Savannah River Site and served on it until 1993, during which time Ruth Patrick chaired the committee. In that service, my relationship with the Savannah ecosystem (including adjacent land) continued.

The Amazon River

After a few years of study of the Savannah River, the team began a study of the Amazon River (probably in 1954) to determine how well methods developed for North American rivers worked in South America. Before leaving on the survey, I collected a sample from the pool of the fountain at Logan Circle, which I could see from my office window. I placed a few drops on a microscope slide, and the first species I recognized was *Urocentrum turbo* – a very distinctive protozoan. When we arrived at Tingo Maria, Peru, to study what was then thought to be the headwaters of the Amazon, I repeated this exercise (without a fountain) and found *Urocentrum turbo*. I was not surprised. After all, I had used European protozoan keys for Conestoga Creek, the Savannah River, and other North American rivers, and Charles Darwin had already noted that protozoans had a cosmopolitan distribution.

We went from Tingo Maria to Iquitos, Peru, on the Brazilian border. The team was only on the Amazon River for over eight weeks (e.g., Patrick et al. 1966), but the experience was very valuable. The intensive study of a few ecosystems, combined with detailed but less intensive studies of a variety of other ecosystems, worked well for me, especially in a team context. I remember each riverine ecosystem well, although longer time spans enable a researcher to better evaluate episodic events such as droughts and floods.

Rocky Mountain Biological Laboratory (RMBL)

In 1961, Director Robert Enders invited me to teach a six-week course on the aquatic ecosystems unique to the western slopes of the Rocky Mountains. The class met for 1½ days each week, and the students usually took one other course (see Chapters 8, 29). Dr. Scottie Willey gave me a crash course in the local aquatic ecosystems. In the 13 years between 1948 and 1961, I had seen quite a variety of aquatic ecosystems, but not like the ones in the Rocky Mountains. I taught the aquatic course at RMBL in summers of 1962 and

1963, a stressed ecosystems course 1971, and a restoration ecology course from 1984 through 1994. The stressed ecosystems and restoration ecology courses involved terrestrial ecosystems about 60% of the time. At RMBL, my research did not lead to many publications. However, teaching placed me in a wide variety of ecosystems, as did hiking or fishing on non-teaching days.

These years at RMBL were an ecological/spiritual experience. When Jeannie and/or the children and I hiked, we rarely talked since it would have been a sacrilege most of the time. On the steep trails at 10,000+ feet, we did not stop breathing deeply to talk. When I was a boy on trips to Potts Quarry, I was usually alone and enjoyed the solitude and beauty. When I was engaged in gathering data in spots of beauty, enjoying it took second place. At RMBL, I had the perfect companion, Jeannie, for hiking and had no perishable samples to worry about. In addition, I was selecting the ecosystems that I visited rather than going to them because a grant or contract was involved. However, the problems encountered through grants and contracts were often at least as interesting as the research I could design, and sometimes even more interesting.

Although an in-depth association with an ecosystem is usually very rewarding, falling in love with an ecosystem can result in major pain and sadness as exponential population growth impairs many wilderness attributes and qualities. When our family visited RMBL in 1961, only a few non-RMBL cars per day went through on the narrow, dirt road. In our last year, 1994, the car and truck traffic on weekends produced an almost constant plume of dust over the road. Some special trails (e.g., the one to Copper Lake) prohibited motorized vehicles, as did one of our favorite all-day hike areas (Rustler's Gulch), so a satisfying wilderness experience was still possible. Many trails were too narrow for motorized vehicles, but not for mountain bikes. Most mountain bikers were considerate and courteous. If a group came by, the lead bikes would often tell us how many bikers were in the group. By the end of the time we were going to RMBL, trout fishing declined considerably in the stream section near a road, but generally improved markedly just a mile or two away from the road.

Even though Jeannie is gone and I cannot hike any of the trails we treasured, I can remember vividly many of the details of our favorite hikes and relive them. We hiked at least once each summer in Crystal Canyon to the tiny hamlet of Crystal. In 1961, when Heather was about 3, we drove the entire way in our trusty Volkswagen microbus. The ford at the top of the canyon wet the brakes of the microbus, so I asked the family to walk down to the Devil's Punch Bowl and reboard for the trip to Crystal hamlet. The microbus could not go back up the steep canyon slope, so we kept going beside the Crystal River to the town of Marble. From there, we took a paved road south to Route 50 to Gunnison, then Crested Butte, and then RMBL. That summer was the last time the entire family was at RMBL because Karen went to Colorado College at age 16 (we had been advised to get Karen away from the East Coast because of her asthma).

University of Michigan Biological Station (UMBS)

In 1964, I was invited to teach a course on free-living protozoans at the University of Michigan Biological Station (UMBS), which is located on Douglas Lake at the northern tip of the lower peninsula. The Station has 10,000 acres, including most of Douglas Lake, on its main campus. The area around UMBS has a rich variety of aquatic habitats: lakes, bogs, fens, swamps, streams, and wetlands. The Alfred Stockard Laboratory had good facilities for microscopy, plus a good chemistry lab for a field station. Most of my research was on microbial colonization of artificial substrates since I could get a sample from the lake to the microscope in 5-10 minutes. During the most productive part of the colonization research, I had the good fortune to have my colleague Bill Yongue, (e.g., Yongue and Cairns 1971) working with me. Later, Paul McCormick (e.g., McCormick and Cairns 1991) assisted me. Another piece of good fortune was having algologist Professor Rex Lowe, Bowling Green University, and his students in the laboratory next door. During the early portion of this research, we were able to confirm the MacArthur/Wilson equilibrium model (Cairns et al. 1969). Working with perishable samples requires time free from interruption (e.g., telephone calls, committee meetings), as well as good water chemistry facilities, so UMBS was ideal. Of course, during the two full teaching days each week, I had no hope of working with perishable samples.

The RMBL and UMBS studies have some strong contrasts. At RMBL, I tried to look at entire systems (Cairns and Pratt 1995), and, at UMBS, I worked toward an in-depth view of an aquatic ecosystem using the only group of organisms I could identify to species – protozoans. Since Protozoa have a cosmopolitan distribution, they are advantageous to use. Their chief disadvantage is that preserving entire communities is impossible. Their perishable nature leads to a concomitant disadvantage – huge amounts of time must be spent indoors while at the field station. However, each approach provides an interesting but incomplete view of an ecosystem.

The New River and Other Rivers of Western Virginia and West Virginia

Traveling down a white-water river provides a hydrological perspective not obtainable in any other way, especially if trips are made at high flow, low flow, and medium flow. Jeannie and I began this experience about

a year after we arrived at Virginia Tech (we were only 46). Our friends Alan and Gloria Heath told us where to buy a folbot (www.folbot.com) and suggested we get a copy of *Canoeing White Water: A Guide Book to the Rivers of Virginia and Eastern West Virginia* (Randy Carter, 1976, Appalachian Books). One of my first graduate students, Rip Sparks, and his wife Ruth actually assembled their folbot and joined us on white-water trips until Rip acquired the PhD in 1971. Jeannie and I continued white-water trips until about 1984 (age 61), but the number of trips per year had declined well before this time.

Terrestrial Ecosystems

Although much of my research involved aquatic ecosystems, in terms of total time spent in them, terrestrial ecosystems were dominant in my life. Shortly after arriving at Virginia Tech, Jeannie and I went on campus YMCA hikes every Sunday afternoon. In fact, Jeannie had charge of them for 12-15 years. The YMCA hikes were mostly on different parts of the Appalachian Trail, sometimes for an entire day. After we gave up white-water folboting on Saturdays, we hiked parts of the Appalachian Trail to decide where the YMCA hikes should go.

When we moved to another house on Bishop Road in 1971, we could ascend the hill behind our house and walk for miles on some regular trails and deer trials that we could follow. In 1999, the hill behind our house become too steep for Jeannie, but the Appalachian Trail was nearby, and Pandapas Pond, a nature preserve, had a variety of trails. Association with natural systems (I include second growth forests) was a spiritual experience, especially when I shared it with the companion I loved. Hiking the same trail several times weekly even heightened the benefits because of the awareness of the small changes that occurred continually in nature. Even after moving to a townhouse in the Warm Hearth Retirement Village in early 2000, we could hike daily on parts of the seven miles of trails then available or on the Huckleberry Trail that passed the edge of Warm Hearth's property. We kept hiking together until Jeannie had severe blood clots in her left lung in May/June 2001, which worsened her Alzheimer's and made transfer to the nursing home in the community essential. From then on, my hiking consisted of three trips daily from the townhouse to the nursing facility, for a daily total of nearly six miles. Jeannie could not understand why I was not with her all of the time. Except for time out for World War II, we had been together for 60 years. Under these circumstances, the loss of my companion was a defining moment in my life. I can do little without being reminded that Jeannie is no longer with me.

My retirement apartment allows me to see the edge of the Warm Hearth forest about 100 feet from my large living room window. I write at a small writing table next to a comfortable chair. I can use my large-wheeled walker to go about a mile on a paved path to the edge of Warm Hearth's property; from there, I can see the mountains Jeannie and I once hiked. Neither the view from my writing desk nor the view of the distant mountains provides the sounds and smells of walking through them, but they awaken memories of good times from the past. For nearly 40 years, I enjoyed hiking the Appalachian Mountains with Jeannie – I am grateful I had that experience!

"Other" Ecosystems

I have visited many ecosystems not mentioned here that made an enormous impression on me, but I did not experience them sufficiently to make them part of my life. After 1961, Jeannie accompanied me on practically all my travels. Five or six of those were working trips to Bermuda, where the base of operations was the Bermuda Biological Station. Among the areas visited was Beebe's Nonesuch Island. Dr. David Wingate was carrying out research on ecological restoration on Nonesuch to maintain the biota of Bermuda before it became extensively settled. My strong attachment to Bermuda was based on the quality of life, despite the dense population. However, local resources were far from adequate to maintain the then present population density. Tourism and military bases were a major source of income, but they required large amounts of fuel, food, and water. In the present economic downturn (e.g., 2008-2009), I cannot help but wonder how dependable these resources may be in the future. The financial system has been a significant source of income but is less resource dependent.

Sense of Place

Mary Price (personal communication) has told me a bit about a book she and Ian Billick (both of RMBL) are writing about sense of place. She also mentioned how much RMBL had changed since Jeannie and I had last been there (1994). I realize that sense of place depends on a continual, intimate relationship with the area. Components of the RMBL that Jeannie and I knew are still there – for example, our first cabin (O Be Joyful) and the other cabins we inhabited (except for our second, which was taken out by an avalanche). Copper Creek and Rustler's Gulch Trail remain, but probably have changed due to increased traffic and climate change. If I returned now, Jeannie would not be with me and I could not see the world through her eyes – and her view of the world transformed my view. I often discuss RMBL with the children, but we recall the historic RMBL that is a

joy to "revisit" as it exists in our minds – the students and the faculty have changed. One of the many online definitions of *sense of place* is "an intensely personal response to the environment, social and natural, which the individual experiences in daily life." Some definitions state: "those characteristics that foster a sense of authentic human attachment and belonging." My own definition focuses on a place that has a harmonious relationship between humans and the natural world. The Savannah River Site may seem an odd choice, but, at the time I worked there, little or no observable effects were evident in the Savannah River, and the terrestrial tract was mostly forested. Where we worked, except for part of the Upper Three Runs, we were more likely to see wildlife than humans (other than crew members, of course). When our children were young, they were strongly attracted to RMBL and UMBS. After the children left home, they were likely to visit Jeannie and me when we were at a field station, even though we were only there for eight weeks each year.

I disliked returning to the early ecosystems in my life years after I left them (e.g., White 2009). After World War II, we moved to Plymouth Valley (1949), and Pott's Quarry was only a few miles away. I had been told about the changes that had occurred and decided not to see what had happened to the favorite fishing spot of my youth. In addition, I went through Conshohocken to paint the outside and sand the floors of the inside of our gatehouse on Woodmont Road, but I never went down 6th Avenue to see the house I was born in. I was establishing a new sense of place and reconnecting with the old would have been trying.

As a family, our first sense of place occurred when we initially saw the gatehouse on Woodmont Road in Gladwynne, Pennsylvania. Jeannie knew, without even talking to me, that this house was our place. In retrospect, I believe that feeling was one of the most critical moments of my life – Jeannie and I had the same sense of place, which was based on the setting and people.

I also felt a sense of place at the Academy of Natural Sciences (ANSP) of Philadelphia, Pennsylvania. The building was very old, even when I worked there, but it had a strong connection to ecosystems and species that no longer existed. Coming in the back door from the parking lot, I was at one end of Dinosaur Hall, which had a huge skeleton of *Hadrosaurus foulki* (found in New Jersey), and I was reminded that it came from a different world than the one I lived in. The school children from South Philadelphia called ANSP the "dead zoo" – an apt designation since the exhibits were mostly, at that time, not alive.

When we moved to Lawrence, Kansas, we were leaving the area where we had lived most of our lives and where our families still lived. I was leaving the mentor (Ruth Patrick) who had introduced me to the research that was now my professional career. Jeannie was leaving the places of her childhood, and we were both leaving the gatehouse with surrounding woods – "our place." The move was less painful since, when we left, we were going to UMBS for the summer and where the family had already acquired a sense of place. The transition via UMBS was even more important than we realized since, when the research space at the University of Kansas was inadequate, we still had a sense of place – UMBS. We returned to UMBS the next summer and were "anchored" by our sense of place. When we left the University of Kansas in spring 1968, we drove to Blacksburg, Virginia, to a house we had never seen. After the moving van unloaded our furniture, we left immediately for UMBS – again connecting with our sense of place. A few years passed before we developed a sense of place in Southwest Virginia.

Developing a Sense of Place in a Humanized Ecosystem

I have followed Rene Dubos' description of a "humanized ecosystem" as one that is attractive to humans as well as wildlife. The hedgerows of France and the United Kingdom come immediately to mind. When we had to leave our house on Bishop Road in Blacksburg, Virginia, we left ten acres of sizable trees that almost touched the house and a 600-foot, gravel driveway. We saw deer almost daily, wild turkey frequently, raccoons too frequently, and foxes rarely. Piliated woodpeckers and many other bird species were common. We even once saw a wood duck perched in a tree beside our house. At the bottom of our hill was a wetland with spring peepers (small frogs). We had no lawn – a sensible feature since we spent summers at field stations. The children were correctly concerned that living on a steep hill, which sometimes had ice on the driveway, and experiencing multi-day power outages were not good when Jeannie's Alzheimer's began to worsen. We were early contributors to the Warm Hearth Foundation, so we were high on the priority list for acquiring a townhouse – in a setting of old trees at the end of a row of four attached townhouses, which was somewhat more private. By then, Jeannie's Alzheimer's had progressed so far that I could not tell whether the setting had any beneficial effect – I like to think it did.

I would rather be in any one of the ecosystems that were part of my life years ago, but I am blessed that I can see the edge of a forest and the large meadows with the Appalachian Mountains in the distance. This setting is enough to give me an inner peace.

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Chapter 35

Transcendental Fly Fishing for Trout

Allah does not deduct from the allotted time of man those hours spent in fishing. Babylonian Proverb

Years ago, Jim Plafkin, then one of my graduate students, commented, after watching me fly fish for trout, that I was just practicing another form of transcendental meditation. True – my mind was so concentrated on fishing that no other thoughts intruded. I had to decide where in the stream the fish were located, where to cast the fly upstream so that it would float past the trout convincingly, when to pick up the fly and recast, and if the cast should be a repeat or if I should try a new location. After each fishing trip, I felt refreshed and at peace. Trout fishing small brush streams is a solitary adventure, but I achieved nearly the same peace of mind while folk dancing or hiking when I had Jeannie as a companion and partner and numerous friends were always around.

Early Fishing Experiences

During my early childhood, my family rented a cottage at Somers Point, New Jersey. From there we drove to nearby Ocean City for ocean bathing. In the 1920s and 1930s, summer vacations were typically two weeks. The owners of the modest, wooden frame cottage that we rented each summer lived in an identical cottage just a few feet away. The access road to the cottages was unpaved, and a few, somewhat similar, modest cottages were a short distance away. A number of hotels stand in Somers Point at present and much private housing. This growth is not surprising since the population has increased in both numbers and affluence over the past 80 years. I have mentioned before in this autobiography that, during the years while I was in good health and could still easily travel, I have never returned to the haunts of my youth. Places for enjoying solitude were much easier to find during those days; such places became increasingly more difficult to find.

Behind the cottage was a path leading through what then seemed miles of cattails and marsh vegetation to an old rickety wooden pier on a salt water inlet from the bay. My mother always accompanied me in this adventure because the pier was not a safe place for a very young boy. Despite lack of experience with fishing, I actually managed to catch a few fish – they usually came around a dock or pier.

For me, the high point of each summer was a fishing trip on the bay, which I am nearly certain was the Great Egg Harbor Bay. Outboard motors were rare in those days, but the rowboat rental place had one. Would-be fishers in the wooden rowboat they had rented were towed by the single outboard motorboat to various points in the bay at well spaced locations. My mother, father, and I fished with hand lines with a sinker, hooks, and bait (clams and squid), which were all included in the boat rental fee. Each boat was provided with a burlap bag to hold any caught fish. Naturally, we took our own food and beverages – no drive-through in those days. Also, we had no styrofoam cooler for food and beverages. We carried peanut butter and jelly sandwiches, hard boiled eggs, and fried chicken, all of which kept fairly well. Water (not chilled) was the beverage de jour, with coffee in thermos bottles for the adults. Today's teenagers have trouble visualizing such a comfortless life, but it seemed great to me. I probably enjoyed only five or six trips to Great Egg Harbor Bay since I went to work after graduation from high school in 1940 when I turned 17.

I have reflected on why the fishing trips on Great Egg Harbor Bay made such a big impression on me. In retrospect, I believe my perception was that our rowboat was a tiny island in a big bay. Other boats were visible but not intrusive. We could hear the water lapping against the side of the wooden boat and the cries of gulls. Portable radios were not common in those days and would have ruined the ambiance. The ever-alert gulls flocked around a rowboat when an oar was raised to request a tow back to the dock. They knew that leftover bait might be thrown into the water for them. Outboard motors were rare, and water skiing was not yet common. Sometimes, a few large sailboats could be seen. In short, the sounds of nature were not drowned out by technology.

In summer 1941, I had a summer job that took all my time and energy. Then came World War II, three college/university degrees, marriage, and finally a full-time professional job on a field survey team that was very active during summers. I did not even have time to take the children fishing. I am sure this span of time was

very difficult for Jeannie and the children. Most individuals have probably had similar experiences at the beginning of a research career.

This retrospective makes me remember my first date with Jeannie. We talked about the books we had read as children (e.g., *Till Eulenspiegel*), all of which required a vivid imagination. I sometimes wonder if this sharing is part of the compatibility tests that persons seeking a companion take. We both enjoyed the outdoors and the organisms we found there. I had never had a similar conversation with any date during my freshman year (although Jeannie and I were both born in 1923, I started college a year earlier). By the end of our third date, we each had a synopsis of the other's life. Jeannie's mother was somewhat concerned that I came from a mill town, but it did not bother Jeannie. Neither of us pretended to be something or someone that we were not. I was a mediocre student and Jeannie was a regular on the dean's list. In retrospect, the difference in our academic performances was the most surprising aspect of our relationship. Fishing never entered our conversations since I didn't know when, if ever, I would get an opportunity for serious fishing again. In a sense, I was fishing on the field crew at the outset of my research career when I used hoop nets, seines, etc. However, these activities were a far cry from fly fishing for trout. I did a bit of surf casting for blue fish at Surf City and actually caught a few fish, but I was never very adept at it.

Later, at age 38, I was at Rocky Mountain Biological Laboratory (RMBL) – with a stream "running through it." I dusted off my old fly fishing gear and bought a one-piece, 6-foot fly rod that was awkward to transport but perfect for short, precise casting. The East River was teeming with rainbow and brook trout and even a few browns and cutthroats. The dreams of my youth came true. Moreover, I could be fly fishing in a short amount of time – I just put on my waders, walked a short distance, and I was ready. The children got tired of trout for breakfast.

University of Michigan Biological Station

I did research and taught at the University of Michigan Biological Station (UMBS) during summers from 1964 until 1983; then I returned for summers at RMBL. The trout streams of the upper part of the lower peninsula of Michigan were vastly different from the trout streams of the Rocky Mountains in Colorado. The stream bed was sand, not rocks, and the streams were lined with trees instead of the arid vegetation of Colorado. I did not fish in the middle of the day but in the evening when my research did not occupy all my time. After all, I was a faculty member and a research investigator at UMBS whose primary duties were to teach and do research, not fish or recreate in other ways. I could not relax and fish if my research program had to be put on hold. I spent two full days each week teaching, plus meeting with students outside of the class day (I had 24 students in my class most years). By careful allocation of my time and the ordering of my priorities, I managed 16 hours/week of lectures in direct teaching, continuous field trips, and publication of results over a mutli-year period. I still spent time with my family and on personal recreation about 2-3 times each week.

Carrying out research on perishable materials, such as freshwater protozoan communities, is a fairly demanding process. Once the sample is collected, it must be completely analyzed before it deteriorates. A complex sample cannot be preserved in any acceptable way with so many low density species. On the plus side, protozoans have a cosmopolitan distribution – once an investigator has mastered identification, (s)he can work with them anywhere in the world in freshwater (my experience) or in saltwater (I lacked training in identification of marine species). Protozoans readily colonize polyurethane substrates and are easily removed from them – a blessing when studying colonization dynamics (e.g., Cairns et al. 1969).

Little Stony Creek

When I arrived at Virginia Tech in 1968, I was prepared to confine trout fishing to summers at field stations, even though I had hiked the beautiful nature trail along Little Stony Creek a number of times. However, Jay Stauffer, then one of my graduate students, would drop by my office on Saturday mornings and ask, "Why are you here when you could be trout fishing?" I had no good answer to this question, so we went trout fishing. Our favorite streams were Little Stony Creek, John's Creek, and Big Stony Creek. Little Stony Creek became my favorite because it became restricted to artificial lures and barbless hooks only a few years after I arrived at Virginia Tech. John's Creek was a backup for Little Stony Creek – small, brushy, very lightly fished, and, naturally, a brook trout stream. The upper reaches of Big Stony Creek had both rainbow trout and brook trout, as did Little Stony Creek. I probably went trout fishing only 9-10 times during the academic year, which was enough for me to learn the creeks fairly well. I always returned all the fish I caught after wetting my hand to avoid damaging the mucus coat of the trout. Barbless hooks made releasing the fish uncomplicated. I could easily have gone fishing more often, but I enjoyed the activities Jeannie and I shared (hiking, folboting, swimming, concerts, folk dancing, etc.), which I gave the highest priority.

Tieing My Own Trout Flies

I felt that I would never experience the full ambiance of trout fishing unless I tied my own flies. So, I purchased a wooden fly-tieing box with all the needed compartments and stocked it with all the hackle, vises, hooks, and so on. I also purchased a pair of fly-tieing magnifying glasses and three instruction books. One piece of good luck was the opportunity to watch a skilled fly-tier at work.

I would never have been able to support myself by tieing flies, but I caught trout with mine, and the flies held together almost half as long as the ones I purchased from a professional in North Carolina, which is good for an amateur. A major benefit was that I never thought the flies I purchased from the professional were overpriced. However, the primary satisfaction was my knowing that the ones I tied fooled the trout.

LITERATURE CITED

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Added to volume on: 12 August 2009

Chapter 36

Six-Word Stories

In August 2009, I was alerted to the concept of the six-word story, which may have originated with Ernest Hemingway's classic creation: "For Sale Baby Booties Never Used." Following are my first attempts at this genre.

(1) **Mill Town Married Jeannie Research Career**

Most research investigators have a support person without whom the career would not have been possible. The support person is not the indispensable mentor(s), but rather one who smoothes the emotional path.

(2) **Writer Eighty-six Still in First Draft**

Most research careers change dramatically every decade. The story is still in the first draft until the last change has occurred.

(3) **Writer's Block Love Writing Can't Stop**

I took "forever" to complete my first manuscripts. Some people may be born with a flair for writing – for me, it has been an acquired skill.

(4) **Unimpressive Start National Academy American Philosophical**

My freshman and sophomore years in undergraduate school could most charitably be described as mediocre, but I was eventually elected during my professional career to membership in both the National Academy of Sciences and the American Philosophical Society. Yogi Berra has stated: "It ain't over till it's over."

(5) **Manuscript Unsuitable Manuscript Suitable Manuscript Invited**

My memory is not always accurate, but I think a world-class scholar asked me at one time how many of my manuscripts had been rejected last year. "Four," I replied. "You're not aiming high enough," he said.

(6) **Tried Research Enjoyed Research Research Addiction**

Some people believe the driving factor for research is the recognition that successful research brings, which is almost certainly true for some people. However, one of my favorite research areas was protozoan colonization dynamics, which I carried out for about four decades with great pleasure.

(7) **Science/Politics Two Cultures No Agreement**

(8) **First Date Talked Books Roommates Horrified**

(9) **Wonderful Life Alzheimer's Struck Love Remained**

(10) **Lone Wolf Team Member Lone Wolf**

Before joining the field team at the Academy of Natural Sciences, I did individual research for a brief period. When I joined the river survey team, I became an ardent advocate of well planned, group research. I continued team work until Jeannie was afflicted with Alzheimer's. When she died, I no longer had the stamina/energy to return to team work. Around 2000, I became a permanent lone wolf research investigator again.

(11) **Reluctant Speaker Shy Speaker Keynote Speaker**

In high school, I was an extremely shy student. College was no better. When I began a research career, I had no choice but to become a public speaker, however painful it might be. I gave talks everywhere and to any group that invited me. Later, I began getting a satisfactory number of invitations, not because I was a good speaker but because my research was cutting edge. Gradually my speaking ability caught up to my research. Finally, I was invited to give keynote addresses.

Writing these six-word stories is fun, and I might add a few more to this chapter later on. Everyone should try it!

Added to autobiography on 11 December 2009

CHAPTER 37

OUR WORLD WAR II LOW-BUDGET MARRIAGE AND HONEYMOON

When I see today's lavish weddings while surfing the television, I have to wonder if the complex preparations, spectacular ceremonies, and huge receptions increase the prospects for long-term happiness, an improved relationship, and a stronger bond between the couple. Jeannie's and my simple wedding was probably the opposite extreme of today's extravaganzas.

Jeannie and I met halfway through fall semester at Penn State in 1941. She was a freshman and I was a sophomore (I had skipped a grade in school). However, World War II began for the United States in December 1941, and I enlisted in the US Navy. Jeannie took the accelerated program and graduated from college in June 1944. We were married on August 5, 1944, when Jeannie was 9 days short of her 21st birthday. I had turned 21 in May 1944.

At that time, I had a two-day leave from the Navy for the wedding. I left Bainbridge Naval Station, Maryland, in a bus for the train station to board a train to Philadelphia, PA, where I caught a subway train to 69th Street in Philadelphia, and then the Philadelphia Electra train to Havertown where Jeannie lived with her mother. From the Havertown station, I walked about a mile to their house where the wedding was to take place. I was on time – a minor miracle considering the many connections in my route.

Jeannie and I were married by Chaplain Rear Admiral Duncan McNair, US Navy Retired. He was a friend of Jeannie's family. I had never seen a Rear Admiral before and was properly awed. The wedding party consisted of Jeannie's mother, my father (we had each lost one parent by this time), and Jeannie's Aunt Frances, Aunt Chris, and Uncle Henry. Aunt Fran served as wedding photographer with a simple Brownie box camera. We had the traditional wedding cake and coffee. Probably the biggest expense was my travel fare. No rice was thrown (too valuable). Jeannie wore a simple white dress, and I wore my uniform. We were married in the living room with no flowers or music. Certainly, the wedding has to be considered at the most basic level. We repeated the traditional vows, and I put a ring on Jeannie's finger. The occasion was solemn. Since Jeannie was not yet 21, she and her mother both had to sign a simple form affirming that her mother approved of the marriage and that Jeannie wanted to be married (this Quaker area did not swear any oaths, but one "affirmed").

My father had saved a full tank of gas (a feat during the war), so after the wedding Jeannie and I set off for the Pocono Mountains in Pennsylvania. This decision turned out not to be so wise, but we wanted our brief honeymoon to be in a natural setting. We arrived just as the sun was setting and found a typical summer cottage on a lake. I think we were the only guests – people did not travel much during the war when gas was rationed. The cabin rental was probably less than $10. The next morning we rowed a boat in our part of the lake for a few hours and then reversed our trip. I was back at the naval base before taps that day. The wedding was what Jeannie and I would also have chosen in peacetime. Neither of us liked large public displays that detract from the primary purpose of the event – a pledge that two people have bonded for life.

In any case, our marriage lasted 61 years and was ended by Alzheimer's. For that entire period (except the last four years), we looked forward to doing things together. Like our wedding, our life was not based on material possessions or lavish displays. Even during the worst, last years of Alzheimer's, we were content just being together – we just couldn't hike, folk dance, travel on white water rivers, and so on. Summers at field stations enabled us to spend more time almost daily in natural systems until health problems in the last four years of our marriage put an end to being in natural systems as a couple.

Our married life, after I left the US Navy, began with one child and no debt as we were living in Jeannie's mother's house. We were living on a welcome allotment from the GI Bill of Rights. We were blessed not to be in debt. A few people cautioned that wartime marriages did not last, but ours did. One thing our experience did prove was that a lavish wedding and honeymoon were not essential for a long and happy marriage. Another major factor was that neither of us were interested in material possessions or large houses. Our children enjoyed being in the small, simple cabins of field stations with no phone, television, washer and dryer, etc. After all, we were surrounded by a spectacular natural world. As a consequence, we always had money for the things that mattered, such as educations for our children. Jeannie and I never had an argument about money – our needs were simple and we never had to go in debt except for our first and second houses.

We had our share of crises, but we faced them as a couple; the biggest crisis was the Alzheimer's that ended Jeannie's life. We handled it fairly well, considering how challenging Alzheimer's is. Before this illness, we had 57 wonderful years. Jeannie died quietly with no fuss (as she did everything else) in February 2005. When Jeannie died, something in me died with her. She saw beauty everywhere, but especially in natural systems. I still remember those two days of our honeymoon vividly – two days out of 61 years together with many other treasured memories.

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Added to autobiography on 17 December 2009

Chapter 38

MY BOYHOOD HOME

I was born in Conshohocken, Pennsylvania, on May 8, 1923. The small town – which probably occupied a square mile – had two or more steel mills (e.g., Alan Wood, specialty steel), Lee automobile tire factory, a textile mill right in town (next to my grandparent's house), a small chemical factory, and a paper mill in nearby Miquon. My father, mother, and I lived in a small, semi-detached house at 204 6th Avenue. On one side, our house shared a wall with another house and on the other side was a narrow, paved passage between our house and the next one. We had a tiny front yard and a long narrow backyard with a garage on the alley. Few families had cars in those days, but my father needed one for his work as a freight solicitor for the New Jersey Central Railroad. Our house had three bedrooms and a bathroom on the second floor. The first floor had an enclosed front porch, a living room, a dining room, and a kitchen. The backyard had a privy, which was never used, and I never asked why it was there. The basement had a large coal furnace for heating the house and a small "bucket a day" coal stove for heating water. The anthracite coal was delivered by a chute through a small window in the front of the house into a room that held several tons of coal. Coal was put into the furnace once or twice daily and into the stove with a shovel, which was also used to remove ashes from the stove. For air conditioning we opened a few windows. The "ice man" delivered ice from an ice house twice weekly. The ice was carried through the narrow passage way to the ice box in the kitchen. We had no telephone – most houses didn't – but the house on the other side of the passageway was owned by a member of the town police force and had a telephone that could be used in emergencies. We had a large Atwater Kent radio (my uncle worked at that factory) that stood on four sturdy legs. I listened to Jack Armstrong, the Shadow, and the Lone Ranger. All programs had offers from the program sponsor. I obtained my first crystal radio for 10 Wheaties box tops and 25 cents.

At the other end of the block was a small grocery store that was about the same size as the downstairs area of our house (the two brothers who owned it lived upstairs). Choices were limited, and what was available was in minimum quantities. However, expectations were not high since the Great Depression era was in full force and "make do" was the name of the game then. Of course, another grocery store was only a short walk away – walking to shop was the norm. On Fayette Avenue (the main street), the Nardi family had a store that was devoted entirely to fruit. I always walked home, which was never more than three or four blocks, for lunch on school days. In those days, only one working parent was the norm. Physicians also made house calls – walking was good for them, too, and it gave them an opportunity to view the patient's home environment.

I could go fishing in about 10 minutes by using my one speed, fat tire bike. The choices were Schuylkill River or Potts Quarry – or I could cross the river, take a river road to the edge of Philadelphia, and come back to Conshohocken on the other side of the river. I played a clarinet in the high school band, but there was little social life, especially for the group of eggheads I associated with. Our Saturday evening recreation was to walk to a small general store just outside town and have a soft drink and a snack (usually a doughnut or a "Tasty Cake") for a total of 15¢ – which was a sizable portion of my weekly allowance of 25¢. The other major expenditure was the 10¢ Saturday morning movie. I was lucky because many teenagers during the Great Depression did not get an allowance.

Winter recreation in Conshohocken was limited but adequate for that era. Our part of 6th Avenue was on a steep hill and cars rarely used it after a snow or an ice storm. Automobile traffic was very light at all times and essentially nonexistent after a snow storm. All the children got out their Flexible Flyer sleds and enjoyed a fast one block ride and a lower additional block for those who liked a long ride. That meant going through an intersection, but I do not remember ever encountering an automobile. Our local "lake," Potts Quarry, was available for ice skating most of the winter. Most years an adventurous person drove a Model A or T Ford onto the ice, and I was confident everyone would have known if the ice had broken and plunged the automobile to the bottom of Potts Quarry.

One of my favorite places was the town library, which was housed in an old wooden house on Fayette Avenue. The wooden floors were badly worn by years of use, but the selection of books was more than adequate for a small town during the Great Depression. My parents had also purchased a 20-volume (I think) set of *Encyclopedia Britannica* for me. Some parts were beyond my comprehension, but it gave me a superb exposure to a vast array of knowledge and reminded me how little I knew and understood. It was a decisive period of my life.

Most people lived in a very small space on Earth during the Great Depression. All but one family of my relatives, the Latshaws, lived within 20 miles of Conshohocken. At funerals, weddings, and christenings, people brought food to contribute to a meal because travel was difficult when most people did not own a car and used public transportation. Since my father worked for the New Jersey Central Railroad, we got free passes for coach seats on other railroads, but Pullman berths were extra as were meals in the dining room. However, since my father only got two weeks vacation each year, time for travel was limited.

My first date occurred during my first year at Penn State. Young people in the 21st century are incredulous at such an abnormal life, but it was the norm for the males I knew. I had turned 17 in May 1940 and entered Penn State that fall. Most of the students in the freshman class were at least a year older than I was, which was a major difference for teenagers. The college ration was about five males for every female back then – a quite different situation for US colleges and universities today where women commonly outnumber men. Despite this challenging ratio, I actually had some dates during my freshman year. It was, to say the least, quite different from Conshohocken – or perhaps it was me.

I have tried to describe life in my small town in detail because it is essential to understanding the cultural shock of going to a university at age 17. Penn State had only about 5,000 students in 1940 – small compared to many universities in 2009 – but, to a 17-year old from a small mill town, it appeared huge. When I joined the Navy in World War II, I traveled the vast Pacific Ocean with comparative strangers. After World War II, things changed dramatically. Small towns near big cities became bedroom communities, foreign travel became the norm, and family ties became difficult to maintain. As a field crew member at the Academy of Natural Sciences, I traveled to many places I had never heard of and was there for only a few weeks. In 1966, when Jeannie and I were 43, I left the Academy after 18 years to become a university faculty member. Our daughter Karen was already an undergraduate in Colorado College, but all the children were with us when the “field station era” began in 1961. We are now all in touch via the Internet. Life is a big paradigm shift with which we cope as best we can.

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Added to autobiography on 4 February 2010

Chapter 39

GREETING RITUAL FOR ADVANCED ALZHEIMERS

In June 2001, Jeannie, my spouse and companion of 60 years, was discharged from the Montgomery Regional Hospital following a bout with severe blood clots in her left lung. She was sent directly to the Cove Nursing Facility in Warm Hearth Retirement Village (WHRV) where we lived. Until the blood clots occurred, I could care for her at home. We could even go for short hikes on the Appalachian Trail and on the forested trails at WHRV.

After the hospital stay, her Alzheimers worsened and she required nursing care. I visited Jeannie three times daily, usually during her meals, and she could still recognize me. However, she could not understand why I had not been with her continually. Even from the beginning of her stay at the Cove, she was not always sure who our children were.

I began to feel that three visits a day were not enough to keep her familiar with me. All biologists have seen videos of the greeting ritual that birds and mammals use when they have been absent from their mates for even a brief time. I decided to try a greeting ritual with Jeannie. She disliked sudden, "loud," unexpected noises. She especially disliked hearty, high volume "how are you dear?" greetings that are often used with elderly people, even those with superb hearing. So, I always approached her slowly and said in a normal tone of voice: "This is Johnny." Sometimes I had to repeat this greeting several times, but I always did so without raising my voice. Then I would extend my arms and say: "A hug would be wonderful." At our home, we had some small cards that said: "Good for one free hug from any consenting adult." Jeannie had found the cards years earlier at the health food store we frequented. We often passed these back and forth, so a hug was not an unusual event for us. For a long time after she first resided at the Cove, Jeannie would hold out her arms to me, but in the last six or eight months of the Alzheimers, she either raised her arms only slightly or did nothing. I may be delusional, but I think she appreciated, even treasured, that part of the greeting ritual.

Jeannie always, and frequently, during her life would say: "Unasked for kisses are the best kind." So, next I would say: "Here are seven unasked for kisses for you." Jeannie, a biochemist, had a predisposition for numbers – for example, she could always remember the number of steps in each part of any complicated folk dance. When I was giving a lecture at our daughter Karen's university, Jeannie sat in the back row with Karen and counted the number of times I took my glasses off and put them back on (17). Jeannie never mentioned this to me, but Karen did. So, I would tell her she was getting seven unasked for kisses, and then I counted them as I kissed Jeannie's cheek. After this part of the greeting, I caressed Jeannie's cheeks with my hand. Each time I was gone for more than a half-hour (such as consulting with the nursing facility staff), I went through the entire ritual with no corners cut.

Did this greeting do any good? Perhaps not – but I was convinced it did. Jeannie dealt with Alzheimers as she handled everything for her entire life – with no fuss or bother. Her wit, humor, and keen observations were markedly reduced, but they did appear briefly at rare intervals.

During the last months of Jeannie's life, I read folk tales and children's stories to her. One of her childhood favorites was *Till Eulenspiegel* (also *Thijl Ulenspiegel*), a prankster and German folk hero. I read in a calm, normal voice, which I never raised. Before the Alzheimers, when I returned to her from a lecture to a large group, Jeannie would give me a hand signal to turn the volume down when I began speaking to her. Again, I have no idea what these readings meant to Jeannie – perhaps just the sound of my voice reassured her.

Those of us with a loved one afflicted with Alzheimers constantly ask ourselves if we could have done more to make our loved one's life better. In the early stages of Alzheimers, Jeannie would say to me, just a few times: "Johnny, something's wrong." My replies to her never seemed satisfactory. She always dreaded Alzheimers (not death) because of the loss of creativity and independence. To this day, nearly five years after Jeannie's death, I have yet to construct a satisfactory reply to Jeannie's statement.

I am convinced that, while Jeannie did not appear to be aware of events, some part of her brain was receiving information. To avoid falls, I had full-time companions with her during the day. They kept notes on Jeannie's condition. One of them, Jenny, told us that she always told Jeannie what she was doing when Jeannie was awake. Once Jenny said: "I'm just tidying up the room." Jeannie instantly replied: "Johnny will help." I wasn't visiting at the time, so Jeannie's remark indicated that she remembered me and my name, even though I wasn't present. Another time, Jenny had a wildlife program on the television, and the commentator said: "If I was . . ." and Jeannie instantly said: "If I were !" I wish I had been present – the person I had with me for many years had returned at that brief moment. The other companions reported similar brief times.

As a result of these experiences with Jeannie, my view of Alzheimers has been altered. The absence of normal response is deceptive. Parts of the brain are more active than appearances indicate. This observation is probably not true in all cases, but it may be in some. In retrospect, the three daily visits were worthwhile – if I could relive those years, I would not change the time I spent connecting with Jeannie. Those brief moments when the “old” Jeanne returned were worth any amount of time! Each Alzheimers case is unique, but perhaps these insights may help others facing a vastly altered relationship with a loved one. The situation will not be easy, but some brief moments of pure joy may still arise.

Added to the autobiography on 22 April 2010

Chapter 40

CHANGES IN PUBLIC ATTITUDE TOWARD SCIENCE DURING MY LIFETIME

We must remember that in nature there are neither rewards nor punishments – there are consequences.
Robert Green Ingersoll

There can never be a conflict between true science and true religion, because they both describe reality.
Anonymous

We will not be driven into an age of unreason if we dig deep into our history and remember we are not descended from fearful men. Edward R. Murrow

I must not fear. Fear is the mind-killer. Fear is the little-death that brings total obliteration.
Fran Herbert in his novel *Dune*, 1965

I was born in 1923 when science evoked more than mere respect – many school children wanted to become a scientist. My high school class trip was to the 1939-1940 World's Fair in New York City, whose unifying theme was "The World of Tomorrow." In those days, most students did not distinguish between science and technology, so all of us were properly awed. The world we saw on display was so different from the depression era world we were living in that it seemed more like a dream. However, I had just turned 17 and was not a doubter. The subliminal message was that "MORE" was possible and it would be "BETTER."

In my teens, I had a beginner's chemistry set that I used in my basement, and later on I had the "master" chemistry set – the best and most expensive then available. I was also given a 20-volume set of the *Encyclopedia Britannica*. Some parts of the volumes were far beyond my understanding, but I understood enough to know that I wanted to learn more. I realized that someone had to gather all the information in the *Encyclopedia Britannica*, but I never fully realized the energy needed for the systematic and orderly gathering of data to test a particular hypothesis. Learning about what other people did was interesting, but, in retrospect, I really had very little exposure to research. My big opportunity to observe research came when I visited my Aunt Margaret, one of my mother's sisters, and her husband Walter in Salt Lake City, Utah. Uncle Walter had a PhD in biochemistry and had been a professor at Kansas State University. He took a professional position measuring air quality around a huge, open, copper mine when the biochemistry building burned down at Kansas State. He was one of the early ecotoxicologists, and the array of sampling units spread over quite a large area was one of the early systems for monitoring the distribution of potentially hazardous materials. I was either too young or too naive to understand the implications of the monitoring system that Uncle Walter had shown me.

Uncle Walter had a company car and had graduated from Penn State University. Naturally, I wanted to attend Penn State and major in biochemistry – probably a major factor in this decision was the fact that Uncle Walter's company provided him with a full-time car for his use. Careers have begun with less inspiration than the possibility of a car! Besides, at 17 and just out of high school, I was not ready to select a career. Some of my best friends and colleagues, such as Sam Roback, knew what they wanted to do for the rest of their life at a far earlier age, and their joy in their profession continued until they died.

In a summer position (I think in 1941) at the Eastern Regional Research Laboratory at Wyndmoor, Pennsylvania, I worked the midnight to 8 am shift, so my contact with the research staff was minimal. Still, I learned about the team work needed for research despite being a sub-professional 6 – the lowest technician rank. I also learned the importance of careful data gathering and recording. Then came World War II, and any thoughts of research were postponed.

After World War II, I finished my undergraduate degree at Swarthmore College and began my graduate studies at the University of Pennsylvania in the zoology department in fall 1947. As a returning veteran with a wife and child, I needed to finish undergraduate school and begin graduate school as soon as possible.

Swarthmore students could have small research projects, and the faculty did research to the extent possible. Graduate students and faculty at Penn all conducted research projects and were delighted to discuss them.

During my first academic year at Penn, I took "The Fundamentals of Systematics," a two-semester course at the Academy of Natural Sciences Philadelphia (ANSP) taught by Drs. Ruth Patrick and Radclyffe Roberts. Neither Ruth Patrick nor I could know that she would offer me a position in spring 1948 on one of the field survey teams at ANSP and that she would eventually become my mentor.

Until spring 1948, the general attitude toward science was respectful, but not well informed. A few typical movies showed mad scientists or the absent-minded scientist, but they were in the poking-fun spirit. In fall 1948, I was offered a full-time curatorial position and was taught how to carry out toxicity tests with fish by Dr. W. B. Hart, a pharmacologist, who, with ichthyologist Dr. Peter Doudoroff and J. Greenbank, had developed a toxicity test using bluegill sunfish (*Lepomis macrochirus*). The river survey team had studied the effects of industrial waste discharges on the Conestoga Creek in Pennsylvania and continued to carry out such studies all over the United States for various companies. My new responsibility, in addition to my work on the river survey team as protozoologist, was to carry out toxicity tests on actual or simulated wastes (for plants under construction). The Limnology Department at ANSP carried out surveys and toxicity tests for E. I. DuPont de Nemours & Company, Procter & Gamble, Potomac Electric Power, and so on from 1948 through 1966.

During this entire period, I was given permission to publish the findings from all projects suitable for scientific journals. The corporations for whom I did studies felt that publication in a peer-reviewed scientific journals increased credibility. Discussions of the results were always civil, and none of the studies with which I was involved were politicized. Some academicians were scathing about any studies carried out for industry. "Dirty money" was a term often used for such grants and contracts. I always felt that how the money was used was the key issue, and any study that could be published in a peer-reviewed scientific journal was professionally ethical. In short, none of the witch hunts* in the news in 2010 occurred during this period.

The best indication that polarization was beginning to occur was the uproar associated with Rachel Carson's book *Silent Spring*, published in 1962. She was a superb writer about the interconnections in natural systems (e.g., *Under the Sea Wind, The Sea Around Us* [which remained on the *New York Times* best seller list for 86 weeks], *The Edge of the Sea*). However, a headline in the *New York Times* in July 1962 captured the response to *Silent Spring* well: "*Silent Spring* in now noisy summer." The book alarmed many people and resulted in a national debate about the responsibilities of scientists, producers of chemical pesticides, and unrestrained technological progress. The situation was not helped by the Cold War and the Senator McCarthy witch hunts for communists, with their generation of intolerance and suspicion, which were landmarks of that era. Some of the lack of civility spilled over into the *Silent Spring* debates. The chemical industry, which was the economic basis for American prosperity, felt threatened. Suddenly, the relationship between humankind and nature shifted. Science and scientists, in their dazzling white lab coats, were oracles of the future. Chemistry and physics, the hard sciences, were held in high esteem. Biology, a soft science, was held in low esteem. However, Rachel Carson had another major asset in that era of scientific specialization – she wrote for the general public. The "outsider" role had a distinct advantage when her opponents were large corporations. With only a MA in zoology and being a woman, her employment opportunities were limited, so she combined science and writing. During her teenage years, the Pittsburgh, Pennsylvania, area was known for sooty air and polluted water. These conditions probably made her well aware of the drawbacks to industrialization, and she became one of the leading writers on natural systems. In 1945, she tried to interest *Readers Digest* in writings on chemical stress in the environment. In 1957, she thought that persistent, hazardous chemicals were a long-term threat to natural systems. She began to assert that scientific and technological use for profit had no accountability for environmental damage. She also believed that science should not be used to dominate nature, especially when long-term effects of bioaccumulation in the biota were not well understood. Finally, Carson discussed the link between environmental and human health, but the scientific community was slow to acknowledge this concept. She was labeled an "out-of-control" woman, and the chemical industry spent large sums of money to discredit her ideas. Finally, scientists were forced to admit they had little knowledge about the field now called eco-toxicology. An environmental movement began to demand more accountability from the producers of hazardous chemicals – for example, chlorofluorocarbons (CFCs) were created in 1928 and introduced as refrigerants by DuPont in the 1930s. In 1974, M. J. Molina and F. S. Rowland published research that demonstrated the ability of CFCs to break down ozone catalytically in the presence of high frequency UV light. However, not until 1985 did Farman, Gardner, and Shanklin describe research carried out by the British Antarctic Survey that showed ozone levels had dropped to 10% below normal January levels. The satellite data had not shown a drop during the Antarctic spring. The National Aeronautics and Space Administration found that the drop had been missed in spring because the computer program had been designed to discard sudden, large drops in ozone as errors.

**witch hunt – an investigation usually conducted with much publicity, supposedly to uncover subversive political activity, disloyalty, etc., but really to harass and weaken the entire political opposition*

Carson's *Silent Spring* is a wake-up call that the free market system is not effective in preventing pollution. In the United States, some essential legislation has been passed (e.g., Clean Water Act, Clean Air Act), but environmental protection was weakened during the two terms of US President George W. Bush. Six years after Carson's death, the first Earth Day was celebrated – a testament to the validity of her belief in the interconnectedness of human activities and the environment. She was one of the first to accept that scientific evidence would inevitably have uncertainties, just as all aspects of life do.

Climate change is another issue that has brought attacks upon the scientific community. Although the scientific data base and information grew throughout the 20th century, the number of publications on global climate change from 1990 to 2010 increased at a phenomenal rate. However, the information was often not what society and corporations wanted to hear, and the response was a denial of climate change and attacks on science and scientists. Most of the attacks were from individuals without training and credentials in science and were not in peer-reviewed, scientific journals but in the popular news media. Hoggan (2009) discusses tobacco apologists and then partisan, political spin doctors who lead attacks on global climate change science. Others (Gelbspan 1997, 2004) have uncovered first-hand evidence of an organized campaign, largely financed by coal and oil industries, to make the public think that climate science is somehow still controversial and unproven.

In theory, scientists should be able to develop any hypothesis and gather evidence to either confirm or falsify it. In fact, most research requires both money and time – as it should – for every hypothesis does not deserve to be tested. However, if science is to flourish, societies should neither tolerate nor encourage egregious (i.e., conspicuously bad or offensive) attacks on scientific freedom. In the United States at present, anti-science persons use theory or hypothesis as if they were synonymous with a guess. However, a theory/hypothesis is a carefully structured statement in order to test its logical or empirical consequences. To be widely accepted, the results must be congruent with other validated theories.

The decade beginning with 2010 is witnessing a marked increase in the threat to scientific freedom – for example, the attempt by Senator James Inhofe "to criminalize and prosecute 17 leading climate scientists" (http://climateprogress.org/2010/02/25/sen-inhofe-inquisition-seeking-ways-to-criminalize-andprosecute-. . .). This attempt is definitely an escalation of the political assaults on scientists and science in general. The attacks on science prompted an editorial ("Climate of Fear") in the prestigious scientific journal *Nature* (http://climateprogress.org/2010/03/10/nature-editorial-scientis-msut-now-emphasize-the-science-while-. . .), which notes: "Climate scientists are on the defensive, knocked off balance by a re-energized community of global-warming deniers who, by dominating the media agenda, are sowing doubts about the fundamental science." The situation has worsened since "people who say human-induced climate change is a fact that demands urgent action are described as 'believers' or 'climate evangelists,' while those who reject the concept are 'deniers,' 'skeptics' or 'atheists.' Those in the middle who say they are unconvinced either way are 'agnostics'" (Lovell 2010).

The sixth extinction crisis, "the first such wave to occur during the existence of *Homo sapiens*," is the worst time to distract scientists by attacking both them and their science (Ceballos et al. 2010). If scientists must take a significant amount of time to refute deniers, they must reduce the amount of time spent on gathering and analyzing information. If the denigration of scientists worsens or even remains at the present level, the situation could cause the downfall of civilization and the extinction of the human species. After a lifetime of scientific research, I view the attacks on the scientific community as a horrible situation!

Even though I have expressed my reasons for concern, I can also state reasons for guarded optimism about the relationship between scientists and the general public. A recent Yale/George Mason University survey finds that 74% of Americans trust scientists as a source of information about climate change (one of the current environmental crises). In addition, Mother Nature is providing a continual stream of evidence that climate change is happening, which should convince even more of the general public of the crisis. Almost all deep love involves pain – when the loved person dies, when physical limitations prevent visits to treasured ecosystems. Scientific research under the attack of deniers is also painful – joy of research is being supplanted by the pain of anti-science zealots. Few would trade the love of a person or a career to ease or eliminate the pain. Jeannie and I were together for 64 years and married for 61. Alzheimer's and Parkinson's resulted in much pain for 5-6 years before she died, which has continued for over five years after her death (at present time). I would not trade any of our days together to reduce the pain. Science is not similar, but it does provide joy, and I would not diminish the joy that being a scientist and doing research has brought to me to reduce the pain, even if it were possible.

In short, responding in a major way to the skeptics further worsens a painful situation with no assurance it would help science. This situation cannot even be resolved by more robust evidence. Why not stick with reason! A good way for scientists to begin the dialogue with the general public would be to describe how the scientific process works – not in theory but in one's professional life.

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Chapter 41

GROWING UP IN A SMALL FACTORY TOWN DURING THE GREAT DEPRESSION

If you don't know where you come from, you don't know where you are going. Jonathan Erlen

In the 1920s and 1930s, extended families normally spent their entire lives living within a short distance of each other. My paternal grandparents lived 1½ blocks from my family's home in Conshohocken, PA, and my maternal grandparents lived in Philadelphia – a short train ride away. The Browns, my father's cousins, lived in a house less than 1 mile away. A few relatives lived a bit farther away, but still less than an hour by automobile. Most of my relatives lived near public transportation, but some on farms did not.

Since few homes had telephones in those days, relatives usually did not know when visitors might come; however, they were always warmly welcomed, although farm work was never interrupted. My Aunt Reba Fischer, my mother's sister, lived with her husband Uncle Bill in a row house in Philadelphia, less than an hour's drive. However, going there by public transportation involved a mile walk to the train station, then a trolley car ride to an area near their house, and then another walk of half-mile or so. A wait for the trolley or train usually added another 10-20 minutes to the trip. Use of public transportation may seem archaic to some younger readers, but increased use of public transportation may be the future norm in the United States. Anyone who has used public transportation in most of Europe or Japan will not dread this change.

Howard Kent was my only cousin on my father's side of the family. He was a physician and was killed in a head-on collision when two younger people reportedly were driving on the wrong side of the road. I still keep in touch with his wife Peggy, who survived the crash. Howard had a successful practice in Hammonton, NJ, and even ran a 300-bed hospital there. He later joined the faculty at Jefferson Medical College in Philadelphia, PA. By then, I was working at Virginia Tech, so we rarely saw each other – a vastly different situation from my childhood. The many cousins on my mother's side of the family were always easily accessible, so I know something about them even though I can no longer travel. In the "old days," we would have been in close touch.

When I was growing up, most people in Conshohocken walked to work. When my father went to work at the office of the New Jersey Central Railroad in Philadelphia, PA, he walked about a mile to the train station to travel to work. Most days, he drove his car around eastern Pennsylvania to solicit freight shipments for the railroad to various parts of the United States. He worked for the railroad his entire life and retired with an adequate pension, which is far from common in the 21st century. My Uncle Bill Fischer worked his entire life on the huge Sears and Roebuck annual catalog (which was the foundation of many outhouse jokes). He and Aunt Reba moved to Palo Alto, CA, when he retired. They had no children, but, even so, retiring so far from family and friends was unusual in those days. I was able to phone them many years later while giving a seminar at University of California, Berkeley, but I never saw them after they left Philadelphia.

I don't recall going to a clothing or shoe store during my childhood. Everything came from the huge Sears and Roebuck catalog – orders were placed by mail and accompanied by a check or money order. Credit cards didn't exist in our town back then. The parents of my good friend Arnie Perloff owned a store that sold clothes and shoes to factory workers – strong, sturdy material that could stand many hours of hard work. Few people had many clothes during the Great Depression, and the closets in our small home were tiny by today's standards. Monday was clothes washing day, and homemakers engaged in a good natured race to see which one had the clothes hung on clothesline in the backyard first. No one had electric clothes dryers. Maytag washing machines had attached motor driven wringers to remove excess water – no spinning dry. At Penn State University, we all had laundry boxes. Richard Rusk, who lives in the room next to mine at the retirement center, remembers an address holder that held a card with the destination (usually one's mother) on one side and the return address on the other. No laundromats existed in those days.

I know of no parents who both had earned income, although, even in a small town, a few probably did. My mother worked hard – no labor-saving devices or shopping for a whole week of meals at a huge supermarket. Shopping daily or every other day was the norm. The tiny grocery store at the other end of our

block had very limited supplies of everything, and the range of choices was also very limited. Every home had an icebox with ice delivered daily – at least in summer. Naturally, food did not keep as well as it does in modern temperature-controlled refrigerators. Conshohocken had no school cafeterias, and all students walked home for lunch on a 1-hour lunch break. My walk was about 20 minutes round trip, so I had about 40 minutes available for lunch.

I never realized how lonely my mother must have been. Anyone coming to a small town undergoes trying to join a close knit tribe. The Conshohocken women were not hostile – my mother just wasn't one of them. I am confident that my realization is close to the truth because my mother did not have a single close, or even somewhat close, woman friend during my entire childhood.

Grandfather Fesmire left our house in Conshohocken to become a resident in the Hayes Mechanics Retirement Home. The man assigned to care for him was very fond of male canaries and their beautiful songs. In those days, many homes had a male canary. Grandmother Cairns raised canaries in a huge room on the third floor of her house. The canaries had been in captivity in Germany for many years and easily raised a brood indoors. Usually young, male canaries were taught to sing by putting them in a room with a "master singer," male canary. From that group, the best of the young, male canaries was selected for important gifts. My mother gave such a gift to the man responsible for Grandfather Fesmire at the Hayes Mechanics Home who had expressed a deep interest in singing, male canaries. This gift was typical for that era – the giver had invested a substantial amount of time in producing the gift.

Even the houseplants were different when I was growing up. They were the kind that tolerated, even thrived, at 50° to 60°F – the ambient temperature of most houses. If the man of the house were at work, the woman of the house had to shovel more coal and remove coal ash from the bottom of the furnace. Putting on a sweater was much simpler.

Once, rarely twice, each summer, our family made root beer. Sugar, yeast, and flavoring were added to warm water and placed in a clean bottle that was capped with a special device. When I became older, capping the bottle was my job. The bottles were wiped with a clean cloth and placed on a blanket in the sun for about 1 hour. Probably about 30 bottles fit comfortably on the blanket. The bottles were then taken to the cellar. At the proper time, some of the bottles were placed in the ice box. This precious fluid was not gulped down, it required too much work! Every sip had to be savored. During World War II, I found that I could get Coca Cola from a machine for a very low price. Drinking it was a normal event, but lacked the memories of the homemade root beer.

Once each summer, my mother and Aunt Reba took a trip to Riverview Beach on an excursion boat that ran during summer. The beach was downstream of Philadelphia on the New Jersey side of the Delaware River. Swimming there is probably not encouraged now. Riverview Beach was an amusement park with all sorts of "rides." Jimmy Koch, who lived down the block, was Aunt Reba's guest. He and I were each given the same amount of money, and we scouted the rides to determine how to get the most for our money. Homemade sandwiches were for lunch, and Jimmy and I were treated to milkshakes. We rode the ferry back to Philadelphia, and my mother, Jimmy, and I rode a streetcar to the train station. Aunt Reba took a different streetcar to her home in Olny (a part of Philadelphia).

I belonged to a motley group, made up of Arnie Perloff, Bill Bates, Herb Ridly, and Bill Fozard, of male social misfits that met for about 2 hours every Saturday night. We had no leader, and our only activity was to meet Saturday evenings across the street from the high school. We walked about 1 mile to a general store just past the town boundary and had a soft drink and a Tasty Kake for 15¢. We only walked in warm weather, probably 10-15 times each year. This activity and the Saturday morning movies shot my 25¢/week allowance. None of us played football or basketball, so we were not associated with groups that did. I played in the high school marching band that performed at football games and parades, but the band had no social activities.

Except for high school football and basketball, no other organized sports were available for growing children. Organized sports for children like us came after World War II. Eight town-owned tennis courts were open to anyone with a tennis racquet and balls – I had both, but most children my age did not. However, groups formed, especially during summer, to play "kick the can," softball, etc. We made our own rules and participants came and went whenever they felt like it. The few disputes were settled by a group vote. If that failed, everyone went home. Our groups were not competitive, and I treasure those days.

During my teens, I developed a special interest in tropical fish, and my parents bought a copy of the illustrated William Innes book *Tropical Fish* for, I believe, $5. It had color pictures of both male and female fish, and the text described their origins, the habitat they favored, and their breeding requirements. (Later in my life, I would see many of these species as a member of a survey crew studying the Amazon River in Peru.) I was entranced and even managed to breed some zebra danios that laid non-adhesive eggs. The eggs fell to the bottom of an aquarium covered with marbles, so they were not eaten.

Our family physician was Dr. Perkins, whose office was on Lafayette Street (the main street), roughly in the center of town. He had a rather varied practice because Conshohocken was a factory town. He made

house calls, which provided an opportunity for him to see the home environment of his patients. Dr. Perkins actually lanced my left ear, which was prone to infection, 28 times during my childhood. I must have been a troublesome child to care for, but I rarely missed a day of school and had perfect attendance for each of my 12 years in Sunday School.

I was able to attend only one of my high school class reunions when I lived near Conshohocken. Later in my life, the warmer months of the year had me on field trips at the wrong times for reunions. Even later, I started many years of teaching and research at field stations, which also were during the times of class reunions. Jeannie missed all of her college reunions, which meant a lot to her.

The Great Depression seems to have affected people in one of two ways. Either "bad times are coming; best to be prepared for them" (Jeannie and I were both in this category, which prepared us for the expenses of years of Alzheimer's even though neither of us expected that kind of trouble) or "spend money as fast as you wish while it still has value." Given the financial, worldwide events since 2008, the second choice is suicidal. The first choice has lost some of its allure, but I could not have cared for Jeannie as well as I did from 2001-2005 had I not held to the "bad times are coming" perspective.

Growing up in a factory town during the Great Depression was a great lesson in the uncertainties in life that are always with us, but the uncertainties of life are often pushed to the back of our awareness. Living in a "blue collar" town and actually working in a "blue collar" job in a paper mill gave me an appreciation for the endurance it takes to survive in that culture. It was my personal "rite of passage."

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Chapter 42

CLOSING A LIFETIME OF DOORS

"Closing doors" is a phrase used by Kathleen Brady (nee Osgood), who was both my wife Jeannie's friend and roommate at Penn State and our lifetime friend. Kathy is one day older than I am, and we still have weekly telephone chats. She commented recently that she is closing some doors – at the moment, the door she is closing is travel to Chautauqua's and other similar events dear to her heart.

When people are young, many doors are opened to them; however, many are lost throughout the lifetime. Some opportunities are grasped with both hands and never relinquished, but reflections on the ones never grasped sometimes cause great regret. Reading was a door opened to me before I was in elementary school, and the passion continues to this day. I am happy that this particular door has not been closed to me.

On about my 12th birthday, I was given a heavy, fat tire, one-speed bicycle, the common bike in those days. This gift opened the door to my association with natural systems initially because the bike drastically reduced the time needed to travel to both Pott's Quarry and the Schuylkill River – I could fish by moving from one location to another within an area. I went on all these trips alone, partly because I didn't know anyone else with a bike who wanted to go fishing and explore natural areas as much as I did. Also, during the Great Depression, even bikes were not common. In any case, this adventure was my first experience of outings entirely on my own. I missed the companionship of others, but this time alone allowed me to experience the exhilaration of trying out new ideas at times of my own choosing. I had great adventures by opening the easy-mobility door. In those days, fishing was primarily a summer – out of school – activity. A major door had opened for me, which I closed only when Jeannie's Alzheimer's worsened and I had to be with her constantly. I closed the door firmly with thanks for being able to enjoy fishing for most of my life.

Two doors I opened myself were the jobs at Hamilton Paper Co., Miquon, PA, and the Eastern Regional Research Laboratory at Mermaid Lane, Wyndmoor, PA. The first opportunity gave me confidence that I could earn money, even in a depression, and the second exposed me to a major research program. Both jobs are described elsewhere in this volume, as are the educational doors I opened.

Individuals have doors they open and quietly shut after a few years. Some doors are opened, enjoyed, and then closed because other, attractive doors present themselves. One such door for me was an interest in house plants in the late 1970s and 1980s. A house plant society offered a seed "bank," from which I could obtain seeds for a modest shipping fee, for plants that could not be purchased in most areas. I usually obtained one species at a time. Jeannie and I had always had house plants, but, during this time, one of the graduate students who took care of our Blacksburg house and plants while we were at a field station during summer told me I had 64 plants. Our house had huge windows on the second floor (the house was on a very steep hillside) that faced west. At first, I used an assortment of tables to set the plants on, but Jeannie, as a surprise, took the course "Carpentry for Incompetents" and built two long tables with upper and lower shelves for the plants. Eventually, other interests (research) lured me away from my infatuation with house plants.

Another door that quietly opened and then closed for Jeannie and me was folboting. The faculty member who helped me most when I arrived at Virginia Tech in 1968 was Alan Heath, who let my graduate student Rip Sparks use his laboratory and equipment to measure the respiratory rate of fish until my laboratory in uncompleted Derring Hall was available the following year. Alan and his wife Gloria were avid white water folboters. Jeannie and I purchased a folbot and were tutored by the Heaths, starting with #1 rapids and moving up to #4s. I sat on the seat back when going through rapids because the view was better. Once, on a steep rapid, the boat went out from under me and I landed in the New River. The boat went on through the tail of the rapids with Jeannie still giving alerts to obstacles. We gave up the white water trips after about 12 years because we were getting older and could not manage folboting during the day on Saturday and folk dancing in the evenings.

Jeannie and I opened many doors together (e.g., folk dancing, field stations) or had opened identical doors before we met (e.g., swimming, hiking). When Jeannie's Alzheimer's worsened, many doors closed (e.g., folk dancing, high altitude hiking) because we could not do them together. I wanted to be with Jeannie, even though we could not continue to share previous life long experiences together. I have no regrets that these doors closed – Jeannie and I had been experiencing and enjoying them since 1941 and being with her was more important than my attempting to keep the doors open.

In April 2010, the lymphedema in my right leg worsened significantly. The initial treatment has been to elevate my right leg on pillows for 40 minutes, after which I can sit or stand for 30 minutes. Walking can be

done anytime since the muscle contractions moves the lymph. This regime is curtailing my scientific writing because I have only a few minutes of sitting time to write. A door has partly closed – no longer can I spend 2-4 hours daily reading professional books and journals plus 2-3 hours writing. The reading and writing, which have been a life long source of joy, have been markedly reduced. However, I was only initially depressed – I had been able to publish up to and including age 87 from age 25. Still, I feel a pang of regret that this important joy of my professional life has been diminished.

For most of my professional career, I dictated manuscripts, which were transcribed and then given to me for editing. I acquired the dictating habit at the Academy of Natural Sciences. The Limnology Department was supported by grants and contracts that typically involved both proposals and periodic reports that developed my writing skills. At that time, I was invited to produce a book for the American Institute of Biological Sciences Patterns in Life Series. I could not produce the book, *Population Dynamics*, during working hours, so Jeannie transcribed the dictation for me at my desk in our huge dining room. I dictated in our tiny living room (we lived in the gatehouse of a large estate that had been divided). Usually I looked after the children while Jeannie transcribed. Our marriage might not have survived a second book since I was so focused on dictating and that I often failed to hear the children "fighting" at my feet. Jeannie could hear the children over the tape when she transcribed it. Needless to say, when the book was finished, I employed another person to transcribe my dictation.

Quite a number of people transcribed for me until I formally retired in 1995 and returned to writing manuscripts, but three stand out. (1) Teresa Moody was the fastest typist I have ever encountered. She would not leave the office each day until all the tapes had been transcribed. Consequently, I always hid those tapes dictated late in the day so she could lead a normal life. (2) Eva Call was a linguist who quickly absorbed scientific terminology and who took a great interest in the transcription of my autobiography. In fact, I became convinced that writing the volume was worth the effort when Eva commented that she and her husband (a graduate student in another department) hoped I would finish the chapter (Chapter 6) on working for a woman scientist (women scientists were rare at that time) while they were still at Virginia Tech so they would know how the chapter ended. (3) Last, but far from least, my editorial assistant Darla Donald has been with me for over 35 years and even takes dictation over the telephone. I still have three or four dictaphones and Darla has the transcribing equipment, but the problem with dictating is that I can no longer keep all the information in my head that is needed for steady and consistent dictation. So, my production of manuscripts is diminishing, but will not cease – at least not until some new affliction of old age appears.

Except in cases of sudden death, most people will need a caregiver at some point in their lives. Institutions, such as assisted living and nursing homes, are essential, but caregivers are still necessary. One problem with caregivers is that they forget to give themselves adequate care. As Jeannie's caregiver for years, I managed to end up in the same hospital she was in at a point when nothing I could have done would have extended or improved the quality of her life. My daughter Karen is currently my primary caregiver (she lives in Blacksburg where I live), and she returned from 2½ months in India just as I needed to be taken twice weekly to the wound care center in the town about 10 miles away. This routine has been followed immediately by two trips weekly to a nearby lymphedema care facility near my assisted living center.

I have never had a group discussion on my aging with all our children. However, during Jeannie's long bout with Alzheimer's and Parkinson's, I touched on related subjects with them, such as my feelings on nursing homes, death, and so on. I do not fear death, but I do fear near total loss of independence if my mind is still working. Recent changes in my health have prepared me for the need for increased services in my assisted living center or in a nursing home. On trips to the wound center and for lymphedema care, Karen and I have discussed such things as my reaching 90 years of age and the adjustments that may require – I may need a companion during the day if I experience a serious decline in self-care abilities. Living in a retirement village reminds me frequently of the precarious aspects (e.g., fall, heart attacks) and the rewarding activities (e.g., writer's group) that I may have time for. However, I remember that everyone lives in precarious times and the major risks (e.g., climate change, food and water shortages) affect all ages. If the positive carbon feedback loops are activated, runaway climate change is almost certain to occur, and such a catastrophe could easily override my personal plans. A number of irreversible changes will occur, and humankind must attempt to adapt. I will not get to select which risks I will be exposed to, and I cannot expect a risk to go away just because I ignore it.

The only closed door that is still on my mind daily is the loss of Jeannie in February 2005. Alzheimer's and Parkinson's typically produce a long, drawn-out door closing. I have pondered the difference between a long "goodbye" and a sudden death. Probably death will be a huge shock however it occurs. Jeannie and I had been companions for about 64 years and married for approximately 61 years. That was the longest continual association in my life, and loss was certain to bring pain. However, the joy of those years exceeded the pain by many orders of magnitude.

Our children have said they cannot imagine Jeannie's and my being married to anyone else – neither can I. I was blessed that the door to Jeannie opened in 1941 and did not close until 2005. The life we shared was full of joy.

Acknowledgments. I am indebted to Darla Donald for transcribing the handwritten draft and for editorial assistance in preparation for publication.

Added to autobiography on 9 November 2010

Chapter 43

LIVING WITH LYMPHEDEMA

My lymphedema primarily affects my right leg, but coping with it has altered my entire life. In April 2010, swelling in my right leg caused me to seek medical attention for a hematoma that became infected. After this episode, I began treatment for lymphedema. When the swelling was reduced, fluids were still coming through the skin and my right leg was a riot of color. To keep the right leg in nearly normal condition, I must follow a fairly rigid schedule — 40 minutes with my right leg elevated higher than my heart and 30 minutes sitting or standing. Walking for as long as I wish has been encouraged between the two times because muscle contractions keep the lymph moving. Gravity is both a friend and an enemy. It drains the lymph from the right leg when the leg is elevated but lets fluid accumulate in the leg when sitting or standing.

The 30-minute span of sitting or standing also includes my three daily meals. I used to read scientific literature for at least three hours daily and write for at least two hours. Clearly, these activities are no longer possible. Since I must lie flat, reading is difficult for someone who underlines important sentences and paragraphs. I do a small amount of reading at meals, especially when my tablemate is off eating at a local restaurant, which he does five to six times most weeks.

I have begun taking a 40-minute nap after lunch and counting it as part of my "down" time. I also walk about a mile two or three times daily with the assistance of a 4-wheeled walker. On my walks, I can see the mountains in the distance where Jeannie and I once hiked and experience the joy of a setting of trees and mountains.

The simultaneous lymphedema and infected hematoma have made me realize that the presence of a family member or a close friend of many years is welcome when an elderly person is afflicted with some loss of independence. I have been blessed that my daughter Karen moved to Blacksburg a few years ago. She has transported me to a superb wound center at nearby Radford Hospital where the staph infection was eliminated and to the lymphedema facility at nearby Heritage Hall. I am fairly sure I could not have coped with nearly seven months of treatment had Karen not been present. She is a registered nurse and has understood all the details of my treatment. More than this assistance, she has provided emotional support when I became depressed from what seemed to me a slow recovery. Our conversations during trips in the car and in my apartment kept my spirits up. I probably could not have endured this stressful period without her! I could not have survived either if I were not in an assisted living facility. The nursing, housekeeping, and food services staffs have enabled me to adhere to a demanding schedule. My family physician, Dr. William Hendricks, helped me cope with the blood clots in my right leg in 1995 (which started the leg problems) and then Jeannie's Alzheimers that began a few years later. Old age is clearly a time of challenges, as are the early stages of life.

My new "normal" life is markedly different from the one I lived seven months ago. The primary difference is the five 40-minute periods I must elevate my leg each day. This protocol has decreased the amount I can spend both reading and writing. I have been fortunate to have had the opportunity to publish for 62 years. I may even be able to continue writing for a year or two longer at a substantially reduced rate.

Both the beginning and the end of life are periods of rapid adjustments. Life is a series of new "normals" and, as soon as one appears to be adjusted to the present situation, another appears. No matter how aggravating the "normals," aren't they better than the alternative?

As a citizen of the United States, I have had opportunity to observe the frantic effort to retain a youthful appearance with cosmetic reshaping of the face and other parts of the body. This delusion makes aging difficult for everyone. Some cultures revered old age because of the accumulated experiences and sometimes even wisdom. At present, climate change and technological change have diminished, but not eliminated, these values.

Writing about global crises has far less incentive than I had at the outset of the 21st century. For example, the planet's nations appear to lack even the minimal commitment to address the problem of global warming that they did at the Kyoto Conference, despite the massive increase in scientific information in peer-reviewed publications. Conferences do not solve anything unless they are followed by well informed action rather than the perpetual "blame game." Crises, such as exponential human population growth and ecological overshoot, are not even discussed in a substantive way. Last, but far from least, global warming and other environmental crises are never high on any of the public or political opinion polls. I continue to publish hoping, with ever decreasing optimism, that scientific evidence will prevail. The war on science in the United States is discouraging to an 87-year-old scientist, but I will not cease doing whatever scientific work I can.

Acknowledgments. I am indebted to Darla Donald for transcribing the handwritten draft and for editorial assistance in preparation for publication. I am indebted to the many people, starting with my parents, who made adjusting to the new "normals" possible.

Added to the autobiography on 13 December 2010

CHAPTER 44

The Catherwood Amazon River Survey: Non-scientific Memories from a Distant Past

In 2010, my granddaughter Laura Cairns Chambers joined the Peace Corps and is working in an 11,000-foot high Peruvian village not far from the village of Tingo Maria, which was where the river survey team from the Academy of Natural Sciences (ANS) began studies about 1953 of the Amazon River. The team consisted of Dr. Ruth Patrick, leader; Ms. Josephine Henry, photographer; Mr. Charles Chaplin, representative of the Catherwood Foundation, which funded the study; Ms. Yvonne Swabey, chemist/bacteriologist; Dr. Matthew Hohn, algologist; Dr. Frederick Aldrich, invertebrate zoologist; Dr. Selwyn Roback, entomologist; and me, protozoologist. Because we had a substantial amount of scientific equipment, the firm of Thomas Cook was employed to make travel arrangements and cope with customs. Drs. Patrick and Hohn carried out a preliminary survey to select study sites and make estimates of time required. Although I had seen a substantial portion of the Pacific Ocean during World War II, I had never visited South America, nor had other survey team members.

We departed Philadelphia, PA, in a Constellation, a four-engine propeller plane. (In the 1980s, I flew into the Philadelphia airport and saw the tiny, original airport – what a shock!) We changed to another Constellation in Miami, FL, and flew directly to Lima, Peru, where we spent a day or two at a hotel run by a Swiss family. An old, unpressurized DC3 took us through the mountains into a tiny airport near Tingo Maria. My left ear has always been sensitive to changes in air pressure. The plane steward gave Fred Aldrich and me instructions on how to "pop" our ears.

In the early 1950s, a road — a narrow, unpaved path through the Andes — from Lima to Tingo Maria was open. Matt Hohn rode from Lima in a truck with most of the field equipment. Hohn described a scary trip — steep drop offs with no guard rails and no places to stop the vehicle for a rest. The traffic was designated as going from Lima to Tingo Maria one day and from Tingo Maria to Lima the next day.

Tingo Maria was ideal for a base camp — it had an agricultural experiment station, modest housing, and a dining facility for visitors. However, the local people did not speak Spanish – they spoke Quechua. .

While in Tingo Maria, we heard of a man who was paying superb prices for bananas. He turned out to be Theodosius Grygorovych Dobzhansky, the famous geneticist, who was collecting wild *Drosophila* for his research. He was accompanied by a young geneticist from the University of Sao Paulo in Brazil. I had met Dr. Dobzhansky when he gave a seminar at ANS in 1947. Our paths crossed again in 1961 at Rocky Mountain Biological Laboratory in Gothic, CO, where I was teaching and Dr. Dobzhansky was collecting more wild *Drosophila*. He gave our daughter Karen, a student at the laboratory at the time, a ride behind him on his horse. She still has the picture, which she treasures.

We were fortunate that Dr. Dobzhansky had arrived at Tingo Maria shortly before our departure for Iquitos, Peru, and was able to join the survey team for a farewell dinner, consisting of roast monkey (so I was told), heart of palm, and a number of other heavily spiced dishes. I could not eat the monkey, but the heart of palm was delicious. Dr. Dobzhansky was fluent in the local language, at least by my standards, since he conversed freely with the local people.

During the stay at Tingo Maria, the village had a party for the survey team. Even though we had no common language, we all enjoyed it very much and got along well. The mayor gave a welcoming speech and Dr. Patrick expressed thanks for the celebration, each of which was translated. The high spot of the evening for me was a quartet of musicians, consisting of an Andean harp, which served as a drum (the wooden part) and two Andean flutes. Dr. Patrick bought the harp (which had a personal name) and took it back to Philadelphia. A harpist there told her it had a different harmonic scale than the local harps.

I remember very little of the area because we were all focused on the scientific study that most other things got little attention. I faintly remember the room I shared with Sam Roback in the building we slept in and the building we used as a laboratory.

Many members of the team found exciting new species in the Amazon headwaters near Tingo Maria. Frequent cries of exaltation could be heard. The many new species explain the long lag time (13 years) between collection of specimens and publication of the monograph (Patrick et al. 1966). The freshwater protozoans I studied have a cosmopolitan distribution — Charles Darwin had called attention to this characteristic many years before. The basic taxonomic keys I used were in German and French and were based on specimens collected in Europe. Since protozoans are perishable, the analyses had to be done then and there. I missed the excitement of the other team members of finding new species, but I was comforted that I could work anywhere

in the world on freshwater protozoans. Before leaving the area, I made two extra copies of all my identifications and sent them to two sites in the Untied States. My identifications had to be congruent in time and place with those of other team members.

When our work was finished in the Tingo Maria area, Fred Aldrich and I volunteered to fly to Iquitos with the equipment. We had to stay overnight in the village of Tarapota, while the others flew to Lima to take a direct flight to Iquitos early the next day. Fred and I stayed in a small cabin near the "airport." That night I had an appendicitis attack and Fred came down with malaria. My recollections of events were and are dim or nonexistent. Fred found the radio operator, who raised Quito, Ecuador, and managed to converse, in German, with a former Luftwaffe pilot who promised to pick us up as soon as possible and take us to Iquitos, which had a hospital. When we arrived, Fred immediately got in touch with Dr. Patrick, who tracked down the best surgeon. I had my appendix removed almost at once.

I remained in Hopital Santa Rosa for about five days in a small room that was in a row with other rooms and opened to the outdoors. Usually, the family took care of the patient, including meals. Since I had no family there, a young woman was employed to care for me. She had probably been recommended by the surgeon because she was competent. We communicated mostly by "sign language." Appendectomies in those days, especially in that part of Peru, were no small matter — for example, the incision was not small. However, I was young and healthy and able to go to the hotel after five days.

I never saw any of the sampling areas when the team was working in the Iquitos area, but a number of samples from each sampling area were brought to my hotel room. The room had a stable table for my microscope and chairs to hold my taxonomic books. I actually only missed doing identifications from one sampling area, so not much evidence was lost because of my appendectomy. I was still fairly weak, so sitting in the hotel room and reading taxonomic keys in German, French, and English was about all I could manage.

When time to leave for home came, I was pronounced fit for travel by the surgeon, but was advised to fly to the coast of Brazil (Beleng) and from there to the United States. Dr. Patrick kindly offered to accompany me in case I suffered a relapse. We flew from Iquitos, Peru, to Manaus, Brazil, in a Catalina – a welcome sight because it could land on either water or land, and, in those days, deforestation of the Amazon forest was minimal. At the old Brazilian "rubber capital" of Manaus, we were met by a German limnologist who knew Dr. Patrick and who escorted us to our hotel. He gave us a tour of his laboratory (impressive, especially after where we had been) and we viewed the famous Opera House. The flight to Beleng (near the mouth of the Amazon River) was uneventful.

I cannot recall any hotel in Beleng, so we must have simply transferred to a flight to the United States. By then, I was beginning to suffer from *Salmonella newporti* – probably from contaminated food or drink. I recall not wanting to leave the restroom when the plane landed. The next day, I saw my family physician for diagnosis, and the entire family was placed in quarantine for eight weeks. Daily stool samples had to be collected from each family member during the entire period. This time, at the very least, was an ordeal for Jeannie.

During my absence from home on the Amazon survey, Jeannie had to take our oldest daughter Karen to Bryn Mawr Hospital. When she returned home, she had mail from me. The first letter she opened started with "I can now sit up in the hospital bed." An earlier letter had not yet arrived, and Jeannie did not know the reason for my being in the hospital in South America. Fortunately, my delayed letter and one from Dr. Patrick arrived the next day to explain matters more fully.

Was this trip worthwhile from either a professional or a personal viewpoint? From a professional viewpoint, it did provide evidence that the structure of freshwater aquatic communities in South America was similar to communities in North America (e.g., the Savannah River), although most of the species (e.g., except protozoans) were dramatically different. Research of any kind is a high risk endeavor, and one never knows how it will turn out until a significant amount of time, and usually money, has been expended. My view of scientific research is that the journey is fascinating, although the final destination is obscure. As has often been said, most research starts with the comment "Now that's funny!"

From a personal standpoint, the first thing I did after returning to Pennsylvania was substantially increase the amount of term life insurance on me. If anything happened on a field trip, I wanted to be confident that Jeannie had the money she would need for herself and the children.

Finally, I gave serious consideration to the type of research I would be doing when I was 50 or 60 years old. Above all, I wanted to explore new areas, such as protozoan colonization dynamics, ecotoxicology, rapid biological information systems, and ecological restoration. The unifying theme would focus on: what places ecosystems in disequilibrium and how can equilibrium be restored? This approach would require many more disciplines than were available at the Academy of Natural Sciences. However, I did not know if I could find a university that would be interested in a faculty member who carried out research in one or more areas. First, I needed to be better established professionally. This objective was no hardship since both Jeannie's and my families lived in the Philadelphia area, and we loved our 100+ year-old gatehouse and the huge trees around it.

The Lower Merion Township schools were superb, and I could drive from Gladwynne to ANS on the newly constructed Schuylkill Expressway in 20 minutes. The family thrived where we were, and I managed to initiate some of the new research and acquire more teaching experience in preparation for the research I envisioned. In retrospect, the Amazon survey had effects on me both personally and professionally – far beyond the short time spent on it.

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Added to the autobiography on 24 August 2011

CHAPTER 45

The Long Goodbye

A Poem for You on Father's Day

by Karen Cairns, June 17, 2001

She swims in sleep
skin caressed by water
soft and warm as air.
Without thought
without words…
Timeless.

And awakens to the terror of a dry land
where all is named.

Lost and alone
abandoned by the past
and in an unfamiliar present.

My father hugs her,
holds her close to his heart
as her eyes fill with tears.

"Why…
Why…
Who would do that?"

She begs.

My father hugs her,
holds her close in his heart.

Comforted, she returns to sleep
traveling lightly in a world
without words…

Resting in deep time.

When our daughter Karen wrote this poem, Jeannie and I were separated by her health issues after 55 years together. We were married in August 1944 during World War II, but the 55 years together began in February 1946 when I was discharged from the US Navy.

The older I get the more clearly I remember things that never happened. Mark Twain

Jeannie and I were products of our culture, which was shaped by the Great Depression and World War II. Also, we each spent our childhoods in different environments — mine in a blue-collar mill town and Jeannie's in middle-class suburbia. Pipher (1999) comments on the relationships of each generation with its parents and grandparents:

> *If age is Another Country, then we must all learn to speak its language. As our parents and grandparents grow older, it's hard to find the words to talk about medicine, loneliness, love, forgetfulness, or selling the house. It's difficult to decipher our feelings and their feelings, and the things are left unsaid. We need a language that bridges the gap between generations and takes into account that our elders can be just as reluctant to ask for or even accept our time and our efforts as we are eager to understand the wants and needs of people who lived before television, Freudian psychology, e-mail, and the first trip to the moon.*

I am writing this book to help those of my children's generation better understand those of my generation and our needs as we age. My daughter Karen and I have been having many long conversations as we try to find the words and language to bridge the generation gap. As Karen says, this process is sometimes frustrating, sometimes humorous, but ultimately worthwhile and rewarding for both of us.

As a scientist who studies anthropogenic effects upon Earth's life support system, the Biosphere, I am appalled at the rapid decline of the Biosphere that is threatened by eight interactive global crises (Cairns 2010). These crises will be worsened by exponential population growth, overconsumption, and inefficient resource use. If they are not eliminated, humankind will need 27 planet Earths by 2050 (Leahy 2011). As a result, except for a few people deeply concerned about damage to the Biosphere, I feel I am living on an alien planet where the main focus is on THE ECONOMY, not Earth's life support system. I confess to retreating to a "too good to be true" past that becomes more attractive every day.

When I share memories of Jeannie's and my life together, a common comment is "It seems too good to be true" — my thought precisely when I reflect on those wonderful days. We certainly had arguments and disagreements, but I have no lasting memory of these except about three that related to stress caused by professional, administrative responsibilities that I failed to keep in perspective. I truly cannot remember other arguments, either because "I am rewriting history to fit some abstract model" or, more likely, because, once a disagreement was over, it was forgotten and never mentioned again. Neither of us bore a grudge against the other. The most common argument for married couples centers on finances. Jeannie and I never had a single argument about money. She had no interest in financial matters beyond the basics of food, shelter, and clothing, and she hated shopping. However, sometimes one of us would make a remark that horrified others — for example, once Jeannie counted the number of times I took my glasses off and put them back on (17) during a lecture. But that trait was just Jeannie, who counted the number of bridges on the Appalachian Trail en route to the Audie Murphy Memorial. Another example is that I always checked at least three times to verify that the door we left through when leaving the house for a trip was locked. Jeannie never failed to make a sarcastic remark about this practice.

A year or two after Jeannie's death, I wrote a chapter for this autobiography, but did not use it in the volume because I did not feel that it was satisfactory. I wrote it too soon after losing her. At present, I feel I have a better perspective on the last years we had together. Her presence with me is woven throughout this autobiography, especially in chapters 31, 32, 37, and 39. The last days of our life together benefited from a bond that was not broken, even when tested by Alzheimer's.

Our life together really began when I walked into the living room at Kenmore Road, Havertown, PA, in early February 1946 with my honorable discharge from the US Navy and was greeted by Jeannie, Mother Ogden (Jeannie's mother), Kathy Osgood (now Brady), and later by our nearly 3-month old daughter Karen. Our children and some of our friends have remarked that they cannot visualize either of us being married to anyone else. However, at that time, neither of us did many of the things we did later in our lives that were very important bonding factors — folk dancing, long hikes, summers at field stations, concerts, plays, simple living, daily visits to the outdoors, folboting (open cockpit kayaks), vegetarian diets, ethical culture, and swimming up to a mile each day. We each also did activities on our own — trout fishing and research for me and fair housing, head start program, interracial activities, book discussion groups, recorder group, score keeping at softball games, and YMCA craft fairs and other Y activities for Jeannie.

In the early stages of my research career, Jeannie and I did not have large amounts of time together. However, the family was together for meals (except lunch), and summers were spent in a small cottage, which my father owned, one-half block from the ocean in Surf City, NJ. Starting in 1961, summers were spent in a small cabin at

Rocky Mountain Biological Laboratory near Crested Butte, CO, and later at the University of Michigan Biological Station near the tip of the Lower Peninsula of Michigan. The entire family was together for the entire summer.

Jeannie and I forged an exceptionally strong bond, even under less than ideal conditions. When our youngest child Heather left for Swarthmore College in 1976, Jeannie began to accompany me on trips within the country and abroad. She had previously been with me on a few trips when the two youngest children could fend for themselves. Jeannie traveled with me on a speaking tour of Great Britain, including Durham, Liverpool, Newcastle, London, Scotland, and Wales. She also accompanied me on a speaking tour of Eastern Australia, including the University of Tasmania and other spots in or near Sydney and Melbourne.

Jeannie and I both believed that actions communicate values better than words. Values should influence behavior and actions should be congruent with professed values. Spending time together is the best way of showing that actions match words. Jeannie believed that how one lives one's life is what is most meaningful. Her mentor at Penn State during her undergraduate years was a Dr. Stevens — she was both a medical doctor and a member of the Society of Friends (Quakers). She held Sunday evening discussions for students, and Jeannie attended these meetings regularly. Dr. Stevens also took students on weekend retreats to such places as Black Moshannon State Park near Philipsburg in Centre County, PA.

When Jeannie and I met at age 18, we had a rough idea of the values we cherished, since we both had the background of the Great Depression.

(1) We valued social capital more than financial capital. Neither money nor materials goods, beyond a certain level, result in a quality life.
(2) We both had a strong attachment to natural systems and needed to enjoy being in them as much as possible.
(3) We both read quite a bit, even though the selection of books was mostly different.
(4) We both enjoyed seminars of all kinds.
(5) In 1949, Jeannie discovered the Philadelphia Ethical Culture Society, which the entire family attended regularly.

All these values are part of the bonding that helped Jeannie and me cope better in the four years that Jeannie was in a nursing home and the previous three years of her "moderately" severe Alzheimer's. Sudden separation after over half a century of living together is a huge shock — especially when one of you cannot understand what is happening or why. In Jeannie's early days in the Warm Hearth nursing home, her face would crumple in distress when I appeared. Her reaction was as if she had never expected to see me again after my last visit. I visited her three times daily, most commonly at mealtime. The husbands of two other women were generally there at these times also. At this time, I also began a greeting ritual (see Chapter 39 in this volume) when I first saw her each time. The sameness of this ritual either worked or Jeannie's Alzheimer's shut out the memory of our former life together. I am convinced that the ritual worked because Jeannie was much more serene than other patients I saw with Alzheimer's. More important, the "old" Jeannie returned occasionally for brief intervals.

When Jeannie was in the intermediate stage of Alzheimer's, someone might tell her about an award that had been given to me. She listened and then, her eyes twinkling, she'd say "BIG DEAL." The Jeannie whose comments I always loved had returned briefly. She believed that honors and awards should never distract from the work that gave me satisfaction, and she was reminding me of that message. I was deeply touched that she transcended Alzheimer's to remind me!

The day Jeannie died, February 21, 2005, our daughters Karen and Heather were beside her bed, singing songs they had learned from Jeannie as children. Suddenly, she was gone — peacefully and quietly. When Heather appeared in the doorway of the dining room of the assisted living facility, no words were needed —– we just hugged each other. Karen remained at the hospital to make the final arrangements and accompanied Jeannie's body to the funeral home.

At the memorial service for Jeannie, preceded by the playing of the Zillertaler Lendler, our favorite folk dance, our minister Christine Brownlie, who had never known the pre-Alzheimer's Jeannie, gave a splendid eulogy of Jeannie's life based on a series of comments by Karen, Heather, and me. Then Karen, Heather, and Duncan gave brief descriptions of Jeannie's life (Stefan could not leave work but was part of the Colorado distribution of her ashes). I wisely did not attempt to say anything — it was too soon. Karen thanked Jeannie's companions who were with her constantly during the days she resided in the nursing home. She always had someone sitting with her; I believe this companionship contributed to the quality of her life.

When people have been strongly attached for many decades, one can accept death in the sense of finality of physical presence, but I have wanted to retain her values and thoughts — for example, on the drive from Blacksburg to the University of Michigan Biological Station is a rest stop that includes a small hill. When the VW microbus stopped, Jeannie would say "everybody run up the hill – you, too, Cairns." I was Johnny most of the time, but the use of "Cairns" meant an important message was being given. In this instance, a brief run up a small hill eliminated the kinks and raised the spirits after hours and hours of driving. The entire family enjoyed it, and it benefited us all. In another instance, we would return from a day of whitewater folboating on the New River, weary and wet, and Jeannie would

say “OK, Cairns, let’s get a move on or we’ll be late for folk dancing.” Also, when something discouraging would happen professionally, she would say “OK, Cairns, you can rise above that.”

Jeannie had a wide range of interests in books. They were mostly about ethical values and interesting people, including such factors as autism. She would tell me about the books with an attitude ranging from glee to awe. Her interests in seminars and courses ranged from Paul Buck’s alpine plants course at Rocky Mountain Biological Laboratory in Colorado to Rex Low’s algae course at the University of Michigan Biological Station. I was determined that anything important to Jeannie would become important to me. Consequently, in her intermediate stage of Alzheimer’s, I was able to point out and name some of the species that she had years before pointed out to me. Jeannie had made a tapestry, with Heather’s help, that displayed some of the protozoan species involved in my research. It was always displayed in our houses and now hangs in the living room of my apartment in the assisted living facility — I can see it each time I look up from my writing table.

Jeannie and I never talked much about death and old age, although she lost her father when she was 13, and I lost my mother when I was 19. We both believed in living life fully. My brush with death on the Amazon River survey reminded us that life is risky. My response was to increase life insurance and take out nursing home insurance for both of us while still in my early 40s. Then I went back to enjoying life. We both tried to keep physically fit, but only by activities we enjoyed, such as hiking, swimming, folk dancing, and whitewater folboating. We also ate a good diet. Even when we were middle aged, all these activities continued, although whitewater folboating become too strenuous earlier than the other activities.

Jeannie and I agreed that neither of us wished to be in a nursing home. However, since I was with Jeannie about three times daily while she was in the nursing home, I have changed my mind. For someone with Alzheimer’s, who appears tranquil, a nursing home may be the best solution, if the family can afford it. However, most people in the United States do not care for visiting nursing homes because old people are supposed to be in the “golden years” and many clearly are not.

Based on my personal experience and assumption, last stage Alzheimer’s patients are more aware of human interactions, at least with people whom they encounter for at least several hours daily, than most people think they are. Even if this assumption is not valid, I would have continued my interactions with Jeannie in the same way. My life with her made any other choice unthinkable. Photo albums of family pictures and our life together got Jeannie’s attention, especially when each picture was accompanied by a commentary. The bird feeders outside her window kept her attention, as did rides in a wheelchair in her last year to see the forest that surrounded the nursing home. One of her companions brought Jeannie in her wheelchair down the hill to the assisted living building to see me. I met them in the lobby, and I went through our personal Alzheimer’s greeting ritual (Chapter 39 in this volume). I am convinced it helped assure Jeannie that all was well in a strange lobby. I also feel that being around the protozoan wall hanging she had made, her father’s handmade wood carving, and the usual piles of books and reprints that had always been standard in our home reassured her that the strange apartment I lived in was a comfort.

When I reflect on our long life together and why we retained our strong attachment to each other, I believe our focus on values rather than issues of daily life made this bond possible. When we finally moved into our first house, I made breakfast often for our two young children when I was at home. The value was that each of us should do a fair share of the mundane but essential chores. After the children were gone from the house, I continued to make breakfast frequently. In the early stages of Jeannie’s Alzheimer’s, I did all the cooking. Another value was that each of us should have time for creative activities of our choice. Jeannie’s choices varied from fair housing petitions to craft fairs. My interests were mostly centered on trout fishing, which I could only enjoy in the years after my research programs were established. Many values were shared — ethics, the outdoors, service (campus YMCA activities), and books written by people we admired. We agreed that all our children should have a good education and opportunities for developing creative activities. Our houses had to be sited in natural systems to the greatest extent possible. Only two of our houses did not fit this requirement, and we only lived in them for about five years total. Well before the field station era ended in 1995, the entire family, including the four children, their spouses, and grandchildren, met from Christmas to New Year’s Day at St. George’s Island on the Gulf Coast of Florida, not far from Tallahassee. During these memorable periods, the entire family got reacquainted. Our last such meeting was at nearby Bluestone State Park in West Virginia because I feared taking Jeannie on a long trip. We rented three small cabins adjacent to each other (the park was nearly empty at that time) and had a wonderful time hiking and tobogganing on the snow despite the frigid temperatures. We were all together in a natural system.

In February 2000, Jeannie and I moved to a townhouse in Warm Hearth Village, a retirement village at the edge of Blacksburg, where we had lived for nearly 32 years. The townhouse was surrounded by huge trees, and a 6-mile hiking trail ran through the forest. On the edge of Warm Hearth is Huckleberry Trail, a converted railroad track bed between Blacksburg and the neighboring town of Christiansburg. Jeannie and I were still able to walk together until June 2001, when Jeannie was hospitalized in nearby Montgomery Regional Hospital with blood clots in her left lung. She went directly from the hospital to the nursing facility in Warm Hearth — she never lived in the townhouse again.

When we moved to the townhouse, Jeannie kept commenting that something was wrong. Before this time, she was worried about Alzheimer's because she saw a difference in herself when she spent time on some of her interests, each of which required mental alertness. I had difficulty reassuring her that she was fine; then the classical symptoms of dementia appeared. At this time, I could only hug her – words were not adequate.

We had always slept peacefully and soundly in a queen-sized bed. Definitely reassuring is the presence of a person who has been there for most of your life and with whom you have done so much in so many places. Even after the 10 years that I have been alone, it does not yet seem natural. When one loves a person deeply, loss brings pain. I feel that many years of love more than compensates for a few years of pain.

For a couple to die simultaneously, except in a car accident or some similar event, is fairly rare. In the absence of an accident, the survivor is usually the woman, not the man. The bereaved person usually turns to activities he or she is familiar with. My two activities are writing and walking. In the 1980s, John Tanton of Petoskey, MI, urged me to write my autobiography. Before he made the suggestion, I had never thought about such a venture. At present, writing for the volume is my favorite way to awaken memories of loved ones and the "good old days." In the 1990s, I dictated a few chapters that were transcribed by Teresa Moody and Eva Call, who were secretaries for me at the time. I never did much with the chapters until my daughters Karen Cairns and Heather Chambers urged me to establish a website. Heather set it up for me (www.johncairns.net). Both the autobiography and the website are now major factors in my life. They reached that status when I could no longer be with Jeannie continuously but 2-3 times a day. Kathy Brady (nee Osgood), Centerville, PA, has been extremely helpful in helping me remember Jeannie's life as a college student at Penn State. Tom Dolan IV has recalled events for me in the formation of the river survey team at the Academy of Natural Sciences (ANSP), Philadelphia, PA, and has kept me apprised of my mentor's (Ruth Patrick) health and activities. My granddaughter, Laura Cairns Chambers, is now in the Peace Corps in a small village in the Peruvian Andes about 35 miles from the village of Tingo Maria where the ANSP river survey team began studies of the upper Amazon (Chapter 44 in this volume). Even now, Christopher Densmore, Curator, Friends Historical Library, Swarthmore College, Swarthmore, PA, is kindly attempting to trace Dr. Stevens (medical doctor) who held Sunday evening discussions for students at Penn State on Society of Friends (Quaker) and other ethical values. Those evenings were very important to Jeannie, and I hope to thank Dr. Stevens and acquire more information about that part of Jeannie's life.

Writing an autobiography surely exposes gaps in my own knowledge of the past: How did my father meet my mother? How did my mother feel about moving from a city, Philadelphia, to a small mill town, Conshohocken, which were both in southeastern Pennsylvania? Even though I may never be able to answer these questions, I have been helped by writing this autobiography.

We do not get to choose how our life, or that of our spouse, will end, but the value system we develop during the good years will prepare us for the tough years, whatever they may bring. The Great Depression demonstrated to both Jeannie and me that, while some money is essential to provide food, clothing, and shelter, anything beyond that (more money or more material goods) is not necessary for a quality life. When our income finally rose beyond the amount necessary to provide the basics, we saved the rest for emergencies.

Alzheimer's seems to be increasing and occurring at earlier ages. Many long goodbyes have already been experienced by large numbers of people, and the number may well increase. Each situation has unique aspects, so empathy will be expressed in many different ways. Although I made many errors (in retrospect), I do not regret the basic path I chose in dealing with Jeannie's Alzheimer's. I recommend spending a significant amount of time daily with loved ones who are afflicted because I now believe they are more aware of their limited worlds than they appear to be.

Acknowledgments. I am indebted to Darla Donald for transcribing the handwritten draft and for editorial assistance in preparation for publication. I am also deeply indebted to our children for emotional support during the long goodbye. Karen lent me her copy of *Another Country* and provided very useful comments on several drafts. Others who have helped are acknowledged at appropriate places in this chapter.

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CHAPTER 46

Magical Moments and Places: Part I

Probably everyone has magical moments from the past, sometimes the far distant past, that pop in as thoughts uninvited and unexpected. With a strict regime of elevating my legs six or seven times a day for 40 minutes each time, I have had increased time for recalling magical moments. The vignettes are vivid and in great detail, including sounds and smells. However, the moments are brief and are only loosely connected with what happened before and after them.

Brief Meeting

One particularly vivid memory is almost entirely detached from events before and after it occurred. During World War II, I had taken a bus to State College, Pennsylvania, the location of Penn State University where Jeannie and I had met and where she was still a student. I only had part of the day before I had to catch a return bus to the US Navy facility where I was stationed. In my remembered moment, we were sitting on a tiny grassy bank surrounded by trees and just above a railroad track on a warm, sunny day. While we were sitting there, the coal train passed on the single track. We waved to the two people in the locomotive and they waved back. I have tried to remember how we happened to be at that magical spot and what we did when we left it — but recalling those facts are a total failure. Jeannie and I met in October 1941 at Penn State just before World War II began, a period of turbulent times. However, that brief period of time, probably less than one hour, was an idyllic moment that remains in my memory, and my subconscious sometimes sends me back. Even though I cannot recall what happened before or after the moment, that brief time was perfect!

Climb Any Mountain

Another magical moment involves one of the pictures on the home page of my website. Jeannie and I had hiked up one of the mountains around Rustler's Gulch near Gothic, Colorado, with two friends from Blacksburg, Virginia. The summer day was splendid and the view was spectacular! In the picture, Jeannie is pointing out and naming some of the mountains, all of which have great names. Every time I look at that picture, I remember that day and looking down at the splendid valley below and around at the tops of the mountains. I am reminded of the years up to and including 1994 when both Jeannie and I could still hike in those mountains. We saw all the wonders of nature on those hikes — swarms of hummingbirds visiting the flowers in an alpine meadow, the alarm signal of the tiny pikas at the foot of a rock slide, the eagles gliding far overhead, the elk on a distant mountain slope, the marmots on a meadow near Copper Creek trail to Copper Lake, the pack rats in a small cliff, a water ouzel diving for insect larvae in the East River, the tiny flowers in high alpine meadows, the barn owls resting high in a pine tree, and beavers frantically repairing their dam after a flash flood (Jeannie threw small branches to them, which they quickly accepted). In the assisted living facility where I have used a walker since 2002, I am blessed with these magical moments!

Family Gatherings

When I was a boy, most family members lived a short distance from each other. My father's parents lived one block away, and my mother's parents lived in Philadelphia, a train ride of about one-half hour. My father's sister, Aunt Margaret, and her husband and son lived with her parents (it was the depression era). My mother's family all lived in Philadelphia or in nearby suburbs, with the exception of Aunt Margaret and Uncle Walter, who lived first in Manhattan, Kansas, and then in Salt Lake City, Utah. Since my father could obtain free coach passes on all US railroads, we visited this aunt and uncle once in each place.

However, after World War II, many families, especially academic families, became widely separated. Our four children were educated in distant colleges and universities, except for Duncan, who received his BS at Virginia Tech (although he has two graduate degrees in Florida). After graduations, the children lived at times in Michigan, Washington State, California, Texas, Maine, and Florida.

Consequently, Jeannie and I had a magical week each year for over a decade when the entire family, including grandchildren, met from Christmas through New Year's Eve at St. George Island on the Gulf Coast about 70 miles from Tallahassee, Florida. Except for the first time, when I rented only one beach cottage, we had two adjacent

beach cottages. Jeannie and I watched our grandchildren grow up and play with each other, and the entire family got reacquainted. We mostly swam, except on very cold days, walked on the beach, and just enjoyed being together. One cool day, we walked on the beach to the inlet on the opposite end of the state park. The walk was about six miles each way. On the long table in my current living room, opposite my writing table, are two pictures from one cool day walk. Jeannie and I are both wearing wind breakers and we are raising our arms in exultation after reaching the inlet — Jeannie's high and mine less high; perhaps I was contemplating the return trip.

The Last Family Week

The last entire family get together was from Christmas through New Year's Day 1999. Jeannie's Alzheimer's was becoming apparent, and we all worried about taking her to Florida. That year, we had three adjacent cabins in Bluestone State Park in West Virginia, which is just across the state line from Blacksburg. I drove the hour trip into Roanoke to pick up Duncan and his wife Debbie at the airport in the 4-wheel drive Isuzu Trooper — it was that kind of day. We picked up Jeannie in Blacksburg and apprehensively left for the Bluestone State Park because driving was a challenge, especially on the secondary road to the park. Each cabin did have an inviting fireplace, and the grandchildren enjoyed using toboggans. The highlight of this gathering was New Year's Eve when the teenagers demonstrated their latest dances. I have memories of Jeannie, her face alight with joy, watching the dancing.

Our Hidden Stream

My father owned a small cottage in Surf City, New Jersey, between the Atlantic Ocean and Barnegat Bay. I had a small wooden boat with a 2.5-horsepower outboard motor. Barnegat Bay had some tiny islands that were used as nesting sites for one of our favorite birds, the skimmer — gulls that fly over the water with their lower beak in the water. When they locate a fish, they snap the upper beak down on it and fly back to the nesting site to feed their hungry chicks. With hungry youngsters to feed, the activity on and around the island was busy but purposeful — each bird flew directly to a particular nesting site, put food in the gaping mouths, and went out again for more fish.

One day while exploring the mainland side of Barnegat Bay, we found a small stream deep enough to run the boat up for about one-fourth mile. From there we walked on the sand bottom for about another one-fourth mile. The banks were heavily vegetated and the water was cool and refreshing. No traffic noise intruded and we could hear only the sounds of the birds. Because of the long ride across the bay, with just a 2.5-horsepower outboard motor, Jeannie and I only visited this peaceful oasis once or twice each summer, but it was always a memorable experience. At present, Barnegat Bay is in ecological decline. Degradation has been the story of our lives with ecosystems; we had to seek relatively or almost entirely natural systems when the ones we were fond of became less attractive. When we moved to Lawrence, Kansas, and then to Blacksburg, Virginia, we only returned to Surf City once. The area was crowded, and we were told that access to the beach was restricted.

Our Old Gatehouse

Our first house was a tiny tract house in Plymouth Valley, Pennsylvania, a suburb of Philadelphia. It had three tiny bedrooms and a detached garage on a very large lot, and a large empty tract of land adjoined our lot because a large electric power line was on it. When I had completed my PhD, we learned that some very old houses were available in Gladwyne with wonderful locations. The school system was superb. We contacted a real estate broker and were sent to a gatehouse on an estate where the main house had not been occupied for many decades, but the gatehouse had always been rented. It sat on 2.5 acres of a wooded area just down the hill from the Philadelphia Country Club. The first floor was stone and the second was cedar wood shingles. When we went to see it, we observed a deer, a fox, a pheasant, and several rabbits. The realtor had given us the wrong key, and even though we were unable to get inside, we told the realtor we wanted to buy the house. The lot was mostly old trees, and huge iron, ornamental gates were attached to two stone pillars with a hand carved door between one of the stone pillars and the gatehouse. The house was in one corner of the property, about 30 feet from the boundary of a huge empty field. The front of the house was only about 15 feet from the road and only two other houses sat on Woodmont Road in 1953. Across the road was another huge field with some trees, but no houses. A large, old yew tree was between the large living room window and the road. I had easy access to the newly completed Schuylkill Expressway. In those days, I could get to the Academy of Natural Sciences parking lot in 25-30 minutes since no traffic lights hindered the entire trip.

Our gatehouse was the tradesman's entrance to "Woodmont," the estate built by Alan Wood, Jr., which had been unoccupied for many years before being purchased by Palace Mission, Inc. to serve as the primary residence of Father Devine, a religious leader. The huge mansion is now a National Historic Landmark (http://www.upspring.com/palace-missiion-inc-gladwyne).

Before purchasing the house, we asked that the easement on the private road leading to the estate be removed because we had young children and the main entrance to the estate was on Country Club Road. Palace Mission, Inc., graciously honored our request, and all our interactions with their staff were most cordial. When our

daughter Heather took her children to see the house where she had lived from birth, she was graciously invited to see her former bedroom.

Picture 1 (http://www.libertynet.org/fdipmm/word3/51012505.html) shows the gatehouse —with a new garage and the gatehouse painted white. When we lived there, the stones on the first floor were not painted white, neither were the second floor shingles. The stone pillars and the hand carved door are still there, but the picture does not show the lovely wrought iron gates. The original chimneys remain. Most important, the wonderful trees are still there. Picture 2 (http://www.maps.google.com/maps?hl=en&cp=33&gs_id=11&xhr=t&qscrl=1&nord=1&rlz=1) shows the trees in the area — the driveway to the gatehouse is on the right between the two Woodmont Road labels.

In Picture 3 (http://www.google.com/maps?hl=en&cp=33&gs_id=11&xhr=t&gscrl=1&nord=1&rlz=1), the balloon "A" marks the location of the house. The Schuylkill River is in the upper left-hand corner, and next to it is the Philadelphia Expressway – Route 76. I can visualize the area with most the houses removed, and it becomes the magical place where we lived for well over a decade. It was also special because it is the only house that Jeannie and I, plus Karen, Stefan, Duncan, and Heather, occupied together as a permanent residence.

Picture 4 (http://en.wikipedia.org/wiki/File:Woodmont_Moses_King_1902.jpg) shows "Woodmont," the Alan Wood, Jr. mansion (built in 1891-1894, William Price, architect). I still remember our first sight of the gatehouse — we knew it was right for us immediately. We did add a bedroom and bath on the side not shown in Picture 1. The addition had a large picture window overlooking the woods.

The gatehouse was where our family forged strong bonds, and the place from which we confidently set out on great adventures such as summers at Rocky Mountain Biological Laboratory (RMBL) and University of Michigan Biological Station (UMBS) and, finally in 1966, to a life in universities where I could carry out transdisciplinary research. During talks the week of Thanksgiving 2011 when Stefan and Heather were visiting, we decided that the gatehouse was our most magical home. Karen and Duncan would surely agree. For all of us, it will remain a magical place.

Our "Tree House"

Jeannie and I lived in our Bishop Road, Blacksburg, Virginia, home for 29 years. Before that, we lived in a large, new, comfortable tract house that we had bought without seeing. It was the only available house that Marion Paterson, the wife of the head of the Department of Biology, heard about during her bridge game in Blacksburg while we were still in Lawrence, Kansas. We knew any house that Marion picked would resell quickly, and it did in1971 at about the price we paid for it.

In 1971, Jeannie found 12.5 acres of entirely wooded land on a steep hillside on Bishop Road, just outside the town limits of Blacksburg. In a few years, the town limits engulfed us. Jeannie selected the location for the house — near a big outcropping of rocks —and found a geologist to check the site and decide that blasting would not be needed. She also found a small, prefabricated house that she thought had a good design, and we contracted with Sonny Dillon, a reliable contractor, to assemble it. Then the firm that sold prefabs had a strike, and we were told that no delivery date could be guaranteed. Fortunately, Sonny Dillon felt he could build a similar house for the same price. In the meantime, we had put our other house on the market and the realtor found a buyer who was anxious to move in as soon as possible. I had agreed to return to RMBL to teach the course "Stressed Ecosystems" during summer of that year. We would be gone eight weeks, including travel time. The contractor knew that he could have the new house finished by that time. Our furniture was taken out of storage and moved up our long, steep, curved, gravel driveway — no mere feat — and we were not greatly concerned because we only had the final payment to make. The couple who bought the house from us in 2000 said "This is it!" before even seeing the inside of the house — our reaction precisely when we first saw the house in 1971 after returning from the field station.

Because the house was built on a steep hillside, the upper floor was the main living area and consisted of one large room with a fireplace, dining area, and kitchen. At one end was a bathroom and bedroom (behind the fireplace). The side looking down the hill was taken up with large windows and two floor-to-ceiling sliding doors. All of these windows, plus our bedroom windows, looked out on the leaves of large trees that we had deliberately left close to the house — this configuration is why our children referred to it as our "tree house." Downstairs was a bedroom and bathroom directly under the ones upstairs. The rest of that floor was another large room with a fireplace with an enclosure under the stairs for a washer, dryer, and water heater behind large folding doors. Jeannie's large loom, my old fashioned wooden desk, and a sofa foldout bed furnished this area. At the foot of the stairs was a large closet, and at the opposite end from the fireplace was floor-to-ceiling and wall-to-wall cupboards with swinging doors. The second floor had a large wooden deck with a cemented area under it. A long, curved, gravel driveway ended in a loop, with several large trees in it, just beside the house. We did not need much space indoors because we had so much space outdoors. The simple, compact house enabled us to spend a lot of time outdoors — we were "house poor" and "nature rich!"

Pandapas Pond

Just a few miles from our house was Pandapas Pond, a recreational area established by a Blacksburg business man. The hiking trail downstream of the pond provided a view of a beaver lodge and several hiking trails that led up to the mountain ridges. In the late 1990s, we went there several times weekly. One winter, we even "adopted" (daily feeding) a duck that did not go south with the other ducks. We left the duck when we went to Hawaii for a Pacific Rim Meeting of the American Chemical Society where I received the Morrison Medal. When we returned, the duck was gone. We have always hoped that it decided to fly south rather than thinking about other less appealing possibilities.

The Appalachian Trail

For over two decades after we arrived in Blacksburg, we went on the Sunday afternoon campus YMCA hikes (Jeannie actually led them for about a decade), which were often on parts of the Appalachian Trail. Those of us with cars provided rides for students (often from other countries) who had no car. I still hear from some of the students who went on these hikes, even though over two decades have passed. The Appalachian Trail is a magical place. Just setting foot on the trail brings a sense of tranquility and peace. Jeannie and I also felt joy to share the trail experience with students who were seeing the trail for the first time. Places on the trail offered the observation of hawk migration, and side trails led to shelters where we often stopped for lunch on an all-day hike. Jeannie was fascinated by the log book in each shelter, which contained "trail names" of individuals hiking the entire length of the trail and their experiences, plus advice on where to eat and stay in towns close to the trail.

One of our favorite hikes was from a valley floor that crossed to a mountain ridge that had the Audie Murphy (a World War II hero) Memorial (he had crashed there in a light plane many years ago). Notes and flowers indicated that the site was visited regularly. An access road from the valley on the other side of the mountain allowed for driving to a parking area for a nearly level trail to the Memorial or for a long hike along the ridge. The trail is maintained by dedicated individuals who live in towns fairly close to the trail, e.g., repainting the trail markings when they fade. These markings are reassuring, especially when "first-timers' are involved. The people who maintain the trail also feel it is a magical place. After an ice storm or high winds, they often work long hours clearing the trees that have fallen. I had one graduate student, David Jones, who took time off to hike the entire length of the Appalachian Trail. He told me that, when he reached Mt. Katahdin in Maine, he felt a deep sense of loss at the end of trail. He also felt it was a magical place.

University of Michigan Biological Station (UMBS)

In 1963, Dr. Robert Enders, Director of RMBL received a call from Dr. Alfred Stockard, Director of the UMBS asking if Enders would mind if Stockard offered me a summer position. Enders was also my advisor at Swarthmore College and thought it would benefit my career to work at another field station. As a result, I was given a contract for summer 1964. I had already agreed to be a discussion leader at a Gordon Research Conference the week before, so I would miss registration day on Saturday, which Dr. Stockard approved. We left New England on Friday afternoon, drove across Canada, and arrived so early on a Sunday morning that only one person was up and walking around at the Station. We did not know which cabin was ours, the location of my classroom, or where the dining room was. We soon were told everything.

The whole family was together again for summer — Karen had joined us from Colorado College. Because of six in the family, we had a large log cabin with bedrooms and a bathroom with a hot water shower and even a laundry room with a washer and dryer. At RMBL, two old wringer Maytags that Keith Justice and I had purchased for $15 each were used, and the clothes were hung out on clothes lines to quickly dry in arid Colorado air. We were initially intimidated by all this luxury, as field stations were supposed to be primitive, but we quickly became accustomed to the conveniences.

UMBS is located on Douglas Lake, which is about 5 miles long and shaped like a fish. The Station itself is on south Fish Tail Bay. One can actually take water samples into Lakeside Lab by boat, although this process was rarely necessary. The tip of the Lower Michigan Peninsula is blessed with bogs, fens, swamps, wetlands, lakes, and streams — an ideal place for the study of freshwater protozoan colonization processes.

The total area of the Station is 4,048 hectares, and it is also a UNESCO-MAB Biosphere Reserve. The area was heavily logged before the Station was established in 1909, but, by the time the Cairns family arrived in 1964, it was heavily forested. The simple but adequate cabins clustered in a small part of the total acreage enabled one to feel a part of nature rather than apart from it. All residents shared a love of nature.

Most field stations are like the mythical Scottish village of Brigadoon that reappeared for one day every 100 years, but the villagers were not aware of the interval and acted as if no time had passed. Similarly, each field station has a number of faculty and staff families who are regulars and a few graduate students with multi-year research programs. These individuals resume relationships with a brief acknowledgment that an academic year intervened. Even a few undergraduates are multi-year returnees. This situation provides a sense of continuity and stability for

families, such as ours, who changed academic year residences (three times for us) while teaching at UMBS. Our children also keep in touch with individuals who grew up with them at UMBS.

Field stations are also big on sharing. Each time the Northern Lights displayed, the first to see them would run through the Station banging on a large galvanized tub or similar noisemaker. Jeannie and I would paddle out to the middle of Douglas Lake in our folboat and watch, relatively free of mosquitoes.

Nonsuch Island, Bermuda

Nonsuch Island is a magical place for two reasons. First, it provides an indication of what Bermuda was like before humans colonized it. Jeannie and I had the honor of being escorted to Nonsuch by David Wingate, whose holistic, ecological approach restored not only the native forest but also Bermuda's endangered petrel, the Cahow. Extensive culling of invasive exotic plants was originally mandatory, but the need for this activity diminished, but not vanished, as the forest matured. Every eco-region of the planet should have a Nonsuch Island. Second, William Beebe had trod on Nonsuch, and a special feeling is associated with any place where a "world-class" person has been for a significant amount of time.

Cascade Falls Recreation Area, Jefferson National Forest, Pembroke, Virginia

Before leaving Blacksburg the week of Thanksgiving 2011, our oldest son Stefan and our youngest daughter Heather and her husband Carl hiked to Cascade Falls (http://gilescounty.org/cascades.html). It was the first hike Jeannie and I took after arriving in Blacksburg. The hike up Little Stony Creek, a superb trout stream, has always been the most magical part, but Cascade Falls are also memorable. They are not huge like Niagara Falls, but they are magical because they are small and bring peace of mind. Jeannie and I hiked to Cascade Falls at least once a month from 1968-1998, when Jeannie's Alzheimer's made the hike too trying.

The secret of truly magical places is that they never lose their magic, unless they are developed by humans. I am thankful that I had the good fortune to meet Jeannie, who shared my love of magical places and who generated magical moments, and I am thankful to our children who shared our love of magical places and produced magical moments that make life a joy!

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Added to the autobiography on 14 March 2012

CHAPTER 47

Magical Moments and Places: Part 2

Tasmania

Islands have always fascinated me, both biologically and esthetically. When I received an invitation in 1978 to present a one-week series of lectures for the Australian Water and Wastewater Summer School held in Hobart, Tasmania, I quickly accepted. All our children had "left the nest," so Jeannie accompanied me. We had planned to spend a day in Sidney to recover from the long flight from Blacksburg, VA, and had reservations at the Florida Hotel, which was just across the harbor from the famous Sydney Opera House. However, the travel agent had neglected to take into account the International Date Line, and our flight to Hobart was scheduled to leave Sydney's domestic airport within half an hour after we landed at the international airport. The customs agent said to us, "Not to worry, mate" and phoned the nearby domestic airport and a taxi to take us there. When Jeannie and I boarded the plane, we were met by broad grins and comments about the International Date Line. Many passengers on the domestic flight had enrolled in the Summer School for which I was one of three invited lecturers. We were in Oz!

We were met at the airport in Hobart by Dr. Roy English and Mr. Henry McFee McFee, both residents of Hobart and affiliated with the Summer School. Jeannie and I were escorted to lunch in the revolving restaurant atop the Wrest Point Hotel, which has a splendid view of Derwent River and Sandy Bay. Clearly, Hobart was not the bucolic village I had envisioned, although I was later to find it had both rural and wild natural places as well — Tasmania is a land of contrasts.

The University of Tasmania, just up a hill from the Wrest Point Hotel, where I was to lecture, was not just a state university in a remote location, but rather an international institution with students from a wide variety of locations, especially Asia. The close proximity of an intellectual place and a wild place is always magical because I feel both are essential components of a quality life.

Jeannie and I never were able to return to Tasmania. However, our only visit was memorable, not only because of the places but also due to the friendly Australians who attended the lectures.

Rocky Mountain Biological Laboratory (RMBL)

Each magical moment and place comes with a highly visible price tag, and RMBL illustrates this fact quite well. In 1961, Robert Enders, my Swarthmore College mentor, was also serving as Director of RMBL in Gothic, Colorado. I was invited to teach aquatic ecology that summer. I received no salary, but $100 was included for travel expenses, housing for six, and three meals per day for two. The session was for six weeks, but, with five days of travel each way, the total was eight weeks of time. Even though I had two weeks of vacation time, I would lose 1½ months of salary since I was on a 12-month appointment. Since 100% of my salary at the Academy of Natural Sciences came from grants and contracts, a danger existed that the cash flow would diminish if research lessened because of fewer personnel.

After minimal discussion, Jeannie and I decided that life is never free of risks, so "let's go!" The decision turned out to be a sound one. RMBL was then in a remote area, and the scenery was spectacular. A superb trout stream was a short distance from our old log cabin (that was named "Oh be joyful"). None of the family had ever been in the Rocky Mountains.

Our blue, utility VW microbus, with a 35 horsepower engine, crawled in first gear up to Monarch Pass on Route 50 — we knew we were in the mountains! Behind the front seat of the bus was a large platform that extended to the rear seat (the middle seat was removed). On top of this platform were sleeping bags and four children. Underneath the platform were our large tent, duffle bags with clothing, sheets, towels, and my teaching equipment. On the roof was a small aluminum boat that I would use for sampling lakes. On a shelf behind the rear seat were insulated carriers for food and so on. We must have looked like refugees headed for the promised land, but, as we got farther west, I received many compliments at gas stations on "my rig."

A magic moment occurred when we embarked in front of the cabin at RMBL. I can still picture the entire Cairns family, plus Professor Enders and his granddaughter Abigail, standing in front of the cabin. Abigail, who was then about our son Duncan's age (about 6), demonstrated her practical side by showing us the location of our privy on a ridge behind the cabin and the community water pipe in the middle of a field. Majestic Gothic Mountain towered high above us across the East River, which became one of my favorite trout streams.

The family was about to begin its "Field Station Era" (see Chapter 8 in this volume), which lasted from 1961, when Jeannie and I were 38, to 1994, when we were 71 and the staff, students, and faculty celebrated our 50th wedding anniversary with us.

Karen and Heather received their undergraduate degrees in Colorado — Karen at Colorado College and Heather at Colorado State. Stefan got his first horse (rented) at RMBL, and, many years later, Heather and her husband Carl Chambers honeymooned at Mexican Cut in the mountains near RMBL. In summer of 2005, the entire family, except me, gathered to each sprinkle Jeannie's ashes over a cairn (mound of stones) at Copper Lake. Her ashes rest in this magical place. A series of pictures (Photos, www.johncairns.net) of the memorial service for Jeannie include the trail to Copper Lake. RMBL is more modern now than it was a half century ago, but the views of Gothic Mountain are still spectacular.

Going Beyond Your Comfort Zone

Scientific research means going outside one's comfort zone as a norm. By staying within one's "home discipline," one may feel more secure but will miss many superb opportunities. However, at present, scientific disciplines are so subdivided that doing research without going outside one's comfort zone is extremely difficult. Yet many individuals stretch themselves in both science and "everyday" life — some flourish, some don't.

My first exposure to leaving my comfort zone in science came in 1948. Ruth Patrick invited me to be the protozoologist on one of her two river survey teams just being assembled. I had just one semester of protozoan identification, which included both free-living and parasitic protozoans. The two primary keys for identification of freshwater species were Kahl's *Die Tierwelt Deutschlands* and Pascher's *Süsswasserflora* (both in German). My samples were highly perishable, and I had to keep up with the team, making very long hours of work on both week days and weekends. Professor Mary Gojdics, the protozoologist on the other team, also worked very hard, but her long experience made her much more efficient. We rented a furnished house for one month so that Jeannie and Karen, then 2½ years old, could accompany me. After this time, we lived in Franklin and Marshall College's dormitories for over a month. One other team member had a wife and two children with him, and another had a wife so that Jeannie had people to visit while the teams were in the field.

I was definitely outside my comfort zone, and so were Jeannie and Karen, but we endured. The hardest part was probably eating every meal in a restaurant, which used a substantial part of my monthly salary. On the other hand, the dormitory room was not nearly as expensive as the rented house. Jeannie and I learned a valuable lesson on the demands of scientific research. Chapter 5 (in this volume) describes these experiences more fully. Working on a team does not lead to a dramatic loss of individuality as the stereotype suggests.

All too soon, our summer at Franklin and Marshall College was over — our dormitory rooms were needed for students and the data we had gathered on the effects of pollution on aquatic life in the Conestoga Creek Basin needed to be analyzed. I was offered, and accepted, a permanent position at the Academy of Natural Sciences. I was convinced that research on environmental pollution was attractive and that Jeannie and I could maintain our good relationship during temporarily stressful conditions.

I was to leave my comfort zones throughout my career — scientific research cannot be carried out without doing so. One should always be cautious when leaving one's comfort zone, but not fearful. One may be traveling on a dead end road — which happens in scientific research and explorations in general. However, often, with luck, hard work, and persistence, something very, very special results that is forever memorable and can be shared with others. No matter how old one gets, the pure joy of discovery that one felt the first time can occur over and over again.

Crystal River

Each year we were at RMBL, we traveled to Schofield Pass and down into the Crystal River drainage basin. Crystal River is a transparent, splendid trout stream, and it cascades over rocks in a steep decent. All the waterfalls are superb. In 1961, we drove past Crystal hamlet and down to Marble, named after the marble quarry where chunks and slabs of marble remain. This first trip was Colorado at its best — we saw no people, except at Crystal and Marble, until we hit the paved road that led to Route 50, our round-about to RMBL.

Schofield Pass (elevation 10,707 feet: Gothic, 8 miles, Marble, 10 miles) is followed by a long, alpine meadow with "magic" (as described in http:www.coloradofishing.net/ft_crystal.htm). Crystal River runs through the an alpine meadow. At the end of the meadow is a steep gorge with a usually passable ford at the top. Since driving through this area could wet the brakes, I asked the entire family to walk while I drove cautiously down in low gear, braking frequently. I missed fully appreciating the scenery, but took a good look when I arrived at the bridge across the Crystal River. Heather, then about 3, remembers getting a splinter in her toe from the wooden bridge (she was barefoot on the bridge). Jeannie removed the splinter with a fishhook from my hat. A hail storm and a rock slide across the road during the next two years kept us from going very far. One year, Schofield pass was blocked by snow.

Only once more in the 15 years we spent summers at RMBL did we make the trip to both Crystal and Marble. In 1970 and 1984 through 1994, I parked the 4-wheel drive about a mile from Schofield Pass and we hiked from there

to Crystal and back. The trips were always memorable, even magical, and I have fond memories of this annual trip through a magical pass.

Our Post World War II Second Honeymoon

When I received my honorable discharge from the US Navy early in 1946, I barely had time to be admitted to Swarthmore College before the first semester of 1946 began. I was able to complete all the registration only because of the helpful faculty at Swarthmore. This era was not the blissful time I had dreams of while I was in the Pacific. However, I found joy in returning to the academic life, and Jeannie's support was inspiring.

When the first semester ended and I had successfully readjusted to college life, the summer of two free months was available. I had been so focused on course work that I had no plans for summer. Actually, my memory of that summer consists primarily of one "magical moment."

My father decided that Jeannie and I should be taken to Niagara Falls, and grandmother Cairns, who longed to see the falls, would accompany us. We were an unusual wedding party — a grandmother, a father, a young couple, and a baby. Naturally, the trip was by automobile since gas was no longer being rationed.

I have some pictures of the trip that help refresh my memory. In one picture, I am holding tiny Karen with Jeannie on my left at Niagara Falls. However, the really magical place was Watkin's Glen State Park in Schuyler County in New York state, about 156 miles from Niagara Falls.. The glen had a tiny stream and at least two spectacular waterfalls and many small ones. Steep stone steps are built in places and a well built stone wall lies between the path and the stream. The water was clear and the rock formations were impressive. Jeannie and I always loved this kind of ecosystem — intimate and ever changing as we walked up the path. Vegetation was abundant.

In one photograph I still have, my grandmother, my father, and Jeannie (holding Karen) are at the entrance to the park. The other picture is of me, my father, and Jeannie sitting on the stone wall with Karen on Jeannie's lap. We never returned to Watkin's Glen. I was 23 when we made the trip and Jeannie was only a month from 23. We both looked so young!

Added to the autobiography on 21 August 2012

CHAPTER 48

Magical Moments and Places: Part 3

Viewing the Universe Without Light Pollution

The inspiration for this magical moment remembrance comes from page 7 of the second edition of Brian Green's book *The Elegant Universe*. He discusses the ancient Greeks and wonders why they were so much more interested in the universe than people are today.

This reading prompted me to remember our family's first summer at Rocky Mountain Biological Laboratory (RMBL) at 9,600 feet, where the air was pure and residents enjoyed zero light pollution. I could merely walk outside the cabin at dark and the heavens blazed —a universe I had never seen before! The beauty was stunning, and thoughts about the universe that had never entered my mind flooded in. I was suddenly in the world experienced by the ancient Greeks. My thoughts were probably similar to those they took pleasure in every night.

The summers at RMBL extended intermittently from 1961 through 1994. During this period, the family spent about half the summers at RMBL and the other half at the University of Michigan Biological Station (UMBS). Some light pollution was present at UMBS, and the air was not as clear as at RMBL, but the nightly display of stars was still stunning. One added benefit at UMBS was the Aura Borealis display, which usually occurred once or twice each summer.

Light pollution has become an anthropogenic barrier for most of humanity to the daily view of its part of the universe. The mystery has been obscured by light and air pollution. I am reminded of Aldo Leopold's essay "The River of the Mother of God." The river originates on the eastern slopes of the Andes Mountains in South America and disappears into the vast Amazon forest. Where it went and where it entered the huge Amazon River were unknowns. However, scientific exploration disclosed both of these unknowns, and Leopold deplored the loss of the mystery, even though he was a renowned scientist.

I share his regret. I remember when Africa was a mysterious continent. When I was a boy, films such as "Tarzan of the Apes" and "Elephants Graveyard" conveyed some of the mysteries, as did films on hidden treasures, such as "King Solomon's Mines."

I also recall travelogues featuring two zebra striped planes flying over vast herds of game. I cannot recall the names of the husband and wife explorers, but the pictures of the animals and forests remain in my memory. I still enjoy books about the present day Kalahari bushman and their hunter/gatherer counterparts in other parts of the world. However, these memories are of long ago, and most of the present generation will never experience such wonders. Have life's mysteries disappeared?

Even though present and future generations cannot marvel at areas of the world unexplored, fortunately an abundance of new mysteries are discussed in *The Elegant Universe*, and other publications include how the human mind works, speculations on when the present Biosphere will collapse, and what the replacement Biosphere will be like. However, scientific literacy is essential to even being aware that these mysteries exist. The assault on science may be discouraging the present generation from considering a career in science and older generations from becoming more scientifically literate. I hope that this situation will improve.

If inquiring minds and critical, evidence-based thinkers continue to exist, mysteries will be solved and new mysteries identified. However, humanity must be willing to pay a price as new evidence will often be perceived as a threat by special interest groups from horse-whip manufacturers to fossil fuel corporations. New understandings of the universal laws of physics, chemistry, and biology (e.g., anthropogenic greenhouse gases) will alter the way humans live and will be labeled "bad news." Science provides many benefits to humanity, but science is not a risk-free profession, and to nurture the scientific process will require greater public, scientific literacy than exists at present.

If you are concerned that science will replace all the mystery of the universe with scientific evidence, refresh your memory of chaos theory!

Added to the autobiography on 31 October 2012

CHAPTER 49

Magical Moments and Places, Part 4

There never was a democracy yet that did not commit suicide. John Adams

Teach the children so it will not be necessary to teach the adults. Abraham Lincoln

Reality is a question of perspective; the further you get from the past, the more concrete and plausible it seems — but as you approach the present, it inevitably seems incredible. Salman Rushdie

Reality is that which when you stop believing in it, it doesn't go away. Philip K. Dick

Reality is divinely indifferent. Richard Bach, Illusions

I believe in looking reality straight in the eye and denying it. Garrison Keillor

It's not denial. I'm just selective about the reality I accept. Bill Watterson

Know what's weird? Day by day, nothing seems to change. But pretty soon, everything is different. Bill Watterson

This requiem to a magical place (Rocky Mountain Biological Laboratory [RMBL]) is the result of reading Dr. Mary Ellen Harte's (2012) poignant description of what has happened at the laboratory that her family, ours, and the numerous other members of the extended RMBL family enjoyed for many years. In each late May and early June, as if drawn by a powerful magnet, the new and old residents of the town of Gothic, Colorado, assembled in this awesome setting of snow draped majestic mountains and alpine meadows carpeted by newly emerged wildflowers with a multitude of colorful, vibrant hummingbirds. The long time residents of Gothic resembled the mythical Scottish village of Brigadoon and resumed their relationships with old friends returning for the summer almost as if they had not been absent for most of the year.

The laboratory is discussed elsewhere in this volume, particularly in Chapter 8 – "The Field Station Era," Chapter 29 – "The Cairns Family Begins Three Decades at Field Stations," Chapter 34 – "Ecosystems in My Life," Chapter 46 – "Climb Any Mountain," and Chapter 47 – "Rocky Mountain Biological Laboratory." In addition, Harte (2012) covers the biotic and hydrologic changes in the high altitude RMBL area that are a warning of the climate changes that are already occurring or will soon occur in the rest of the Biosphere. Even though changes differ in each component of the Biosphere, some share important similarities.

(1) Most, possibly all, of the changes are irreversible, so return to an earlier ecological condition is unlikely.
(2) Global climate changes will markedly influence evolutionary processes and trajectories.
(3) If business as usual (e.g., exponential economic and population growth) continues, a series of new "normals" will emerge until a dynamic equilibrium is reached — probably a new, quite different, Biosphere.
(4) No sovereign nation or individual human can negotiate with nature (i.e., universal natural laws)!

(5) Food and water shortages already exist at the global level. Even before recent evidence documenting a decline in renewable resources, riots and civil unrest occurred over food and resource wars over water.
(6) "Even before recent predictions that Arctic sea ice would melt before the summer of 2016 in a 'final collapse,' setting off a 'global disaster,' the Pentagon and the Center for Naval Analyses's Military Advisory Board [both US] had already gone on record warning about the impacts of climate change as a threat to national security" (Parsons 2012).

Even as the magical places of our lifetimes are destroyed by climate change, climate change skepticism is highest in three countries (Japan, Britain, and the United States [Gates 2012a]) that should be persuaded that climate change is real and caused by anthropogenic greenhouse gas emissions.

Only a relatively small number of humans have the opportunity to see alpine meadows similar to the ones near RMBL, but most have an opportunity to see forests — "*Forests cover some 30 percent of Earth's surface, and it's hard to overestimate how crucial they are to the functioning of the planet. Forests provide shelter for uncountable numbers of species, hold soil in place that would otherwise wash away, pull excess carbon out of the atmosphere, absorb and re-emit water at such a rate that they literally control the weather, and serve as an economically vital natural resource*" (Lemonick 2012). And yet humanity, which can hardly miss seeing forests, is causing climate change stress that is killing forests (Lemonick 2012) and allowing organized crime to deforest up to 90 percent of tropical forests (Gates 2012b).

Shame on humanity for having a lifestyle that both despoils nature's beauty and life-support capabilities! This situation is not only affecting generations now alive but is a disgrace to intergenerational ethics.

I remain optimistic enough to believe that we can still prevent more irreversible damage to the present Biosphere, but pessimistic about what, if anything, will be done.

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