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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