Alas, the book never found a publication.

E. Gorham (submitted). In M. Aleksiuk and T.M. Nelson. Nature, Environment and Me: Personal Explorations in a Deteriorating World.

The Accidental Environmentalist

"Chance favours the prepared mind."
(Louis Pasteur)

by Eville Gorham

I cannot remember not being interested in nature. One of my very first memories is of being taken by my mother Shirley (whom I always called by her first name -- unheard of in those days) in search of mayflowers in the woods on my grandparents' apple farm in the Annapolis Valley of Nova Scotia. In 1931, when I was still a very young child, she gave me a set of bird books that led to my early interest in bird-watching. Later I was lucky enough to spend summers at a cabin, "The Owl's Nest," on an island in Lake Micmac near Dartmouth, Nova Scotia, owned by Shirley and her siblings. There were no other occupied cabins at our end of the lake, and I was without brothers and sisters, so my time was spent roaming the island alone, or rowing around it, and coming home with face and hands stained by the juice of Indian pears or blueberries. I came to know the local birds, such as the common yellow throats who nested on the way to the outhouse, and the cedar waxwings whose purple droppings on the rocks betrayed that they too enjoyed blueberries.

As an only child growing up in the Nova Scotian capital of Halifax, I was shy and introspective, poor at sports, and a compulsive reader of anything I could lay my hands on. It was natural, therefore, that I took to the schoolroom, the only place I could excel. Fortunately I had

good teachers, or so they mostly seemed to me. I was especially lucky to have Don Crowdis as my science teacher in grade 8 and Charlie Allen as my biology teacher in Grade 10. Both had been graduate students in biology and made the subject seem wonderfully exciting, especially on the field trips that Charlie led across the Northwest Arm. (Forty years later I wrote to thank them, and received the most touching letters in reply.)

Dalhousie Days

At Dalhousie University in Halifax, where I spent the period 1942 to 1947, I was again fortunate to have great teachers. Hugh Bell in Botany infected me early on with his contagious enthusiasm, while Bill Dore introduced me to the joys of field botany. In Zoology, Dixie Pelluet led me to appreciate the history of science (which became an abiding interest) and Ronald Hayes taught me the critical rigour so necessary to a life in science.

It was in 1945 at Dalhousie that chance and serendipity began to play a major part in my life. My first love was botany, and I decided to go on for a Master's degree. However, in the course of a summer of field work on mosses in the pastures of the Maritime Provinces I had a major clash with my advisor, Bill Dore. He was a champion workaholic, and could not understand why I might want to spend a single weekend out of the whole summer visiting a girlfriend in the Annapolis Valley. In fact he pointed out that when he was taking a Master's degree at Macdonald College, he and his colleagues really knew the meaning of hard work, for he and two others developed ulcers! I did not see that as a good thing, and the accident of that quarrel caused me to shift to a degree in zoology with Pelluet and Hayes, studying the effects of temperature on the development of gut cells in salmon embryos. My original thesis project failed, but in the course of it I was able to show that altering the temperature at which embryos

developed could dislocate the processes of development even within the normal temperature range, and cause various malformations and death beyond that range. In this connection, embryos are often unusually susceptible to thermal pollution, although at the time I did not see or even consider any practical significance in my research.

By this time I had my heart set on an academic career in biology, though I had not given any thought to how I could attain that goal beyond realizing that a Ph.D. degree would be a prerequisite. I had also realized that I myself did not want to continue killing animals, as my research had forced me to do, and that I preferred field to laboratory studies. This feeling was reinforced by another stroke of luck: Ronald Hayes was in the process of shifting his research focus from salmon embryology in the lab to field studies of Nova Scotian lakes, and I took part in his first two field seasons of lake survey.

Overseas to London and the Lake District

My quest for the Ph.D. was guided by the recommendations of both research advisors that I go abroad, preferably to Britain where I would be exposed to a very different kind of academic life. This sounded great to me, but I had no money. Fortunately my father Jimmie agreed to stake me for a year to see how things went. I decided, therefore, that I would have to go to a provincial university, which was bound to be cheaper than Cambridge, Oxford or London. I had already planned to study plant ecology, which would get me out into the field and away from killing animals. Knowing nothing about British plant ecologists, I consulted the Journal of Ecology and found that the secretary of the society, A. R. Clapham, was professor of botany at the University of Sheffield. Thinking that he must therefore be a good ecologist, I wrote asking if he would accept me as a Ph.D. student -- but got no reply. A few weeks later, in the spring of

1947, I was awarded an Overseas Science Research Scholarship by the Commission for the Royal Exhibition of 1851 in London. (The exhibition had been a pet project of Prince Albert, consort to Queen Victoria, and it was the proceeds of the exhibition that funded my scholarship. Occasionally, therefore, when passing the Albert Memorial in London I would genuflect in thanks). The letter of award asked where I planned to study. I replied (without mentioning Professor Clapham) that I had planned on the Botany Department of Sheffield, but had received no answer to my application. Thereupon I received a letter from the award administrator advising me (why, I never knew) that Professor Pearsall (of whom I had never heard) had left Sheffield two years before and was now professor of botany at University College, London, so I should write to him there. With the scholarship in hand I did so, and he accepted me at once (I never did hear from Sheffield). Pearsall and London proved a magnificent stroke of fortune, for the former was one of the two best plant ecologists in Britain as well as a truly wonderful person, and London provided a marvelous extracurricular education in theater, opera, ballet and music. That became especially exciting when I married, in 1948, Ada MacLeod of Summerside, Prince Edward Island, whom I had met when she was doing research on child nutrition at Dalhousie. (During my first year in London she had stayed behind in P.E.I. to help look after her ailing father).

My Ph.D. research began with a study of the mineral content of plants in natural communities in the Lake District and, like my initial Master's research, did not get very far. In the course of it, however, as I gathered data on the soils inhabited by the plants, I became interested in the acidification of woodland and wetland ecosystems. That became the chief subject of my thesis research (and a continuing interest), in the course of which Pearsall gave me some articles

on the development of the organic humus layer in forest soils (written by Professor Lars-Gunnar Romell of the State Forest Research Institute in Stockholm, Sweden). These I found fascinating, and mentioned to Pearsall that I would like to get my own copies of Romell's papers. In those days there were no photocopy machines, so Pearsall suggested I write to Romell, and not on a printed card such as Americans used, because that would end up in Romell's wastebasket. I should send a personal, handwritten letter. This I did, and casually remarked in a postscript that it must be fascinating to work on such matters in Sweden, where the forests had not been so heavily altered by human activities as in Britain. Romell sent back a personal, handwritten reply to say that several reprints were being sent separately, and yes, Sweden was a great place to work on such matters; why didn't I come over to his lab for a year after my Ph.D? He added that he might be able to find some financial support, but I should see if I could find funds myself. This I did, and succeeded in obtaining a research fellowship from the Royal Society of Canada. But then I was faced with a terrible choice. Pearsall had become Dean of Science and needed to appoint a new faculty member to do his teaching. Very kindly, he offered me the job, just as I received news of the fellowship. Given such a choice I opted to stay in London, and told Pearsall that I would write Romell and the Royal Society of Canada to decline their generous offers. Whereupon Pearsall suggested that I should not be so hasty; it might be wise to wait a few days before making such an important decision. Two days later he informed me that I was to take up my position at London with a 9-month leave to work in Sweden. What a marvelous offer! I was overwhelmed, but not too overwhelmed to accept. This meant, of course, that I did not even have to look for my first real job.

In Sweden

That postscript in my letter to Romell led, by a very circuitous route, to my first study of an environmental problem, namely acid rain. Early in my visit to Sweden, in late 1950, I went to Uppsala to meet the doyen of plant ecology in Sweden, Professor Einar DuRietz, and his student (and future wife) Margareta Witting. She was studying the chemistry of bog waters, and told me that such waters derived their supply of calcium, magnesium and other salts wholly from rain and snow. This was so because the bogs -- formed from peat deposited by plants -- were domed convexly above the surrounding ground, so that water from the uplands drained around rather than over the bog surface. That seemed entirely reasonable, but I thought to myself: "She ought to check the chemistry of the rain just to be sure," and then thought no more about it. Because she and DuRietz had gotten me interested in such matters, however, I did a water-chemistry project of my own on Swedish peatlands, looking at the differences between domed bogs and adjacent fens that received water from surrounding mineral soils. If I had thought more about Margareta's hypothesis I could have made good use of the studies of Romell's associate, Carl Olof Tamm. He was at that time analyzing the contribution of rain and snow to certain chemical elements required by the forest moss that was the subject of his doctoral thesis. I lunched regularly with Carl Olof, who remains a good friend to this day, and learned a great deal from him about plant ecology. It now seems strange to me that I did not think to relate his results to those of Margareta Witting. He did, however, show me that the chemical analysis of rain could be ecologically important, so that I was motivated to test Margareta's hypothesis several years later.

Back in London

After a few more years in London, working mostly on the water chemistry of peatlands following my exposure to the subject in Sweden, another chance incident led me back to the Lake

District and to the laboratory of the Freshwater Biological Association (FBA) where I had done my Ph.D. research. The Association had advertised in 1954 for a physical chemist to work on chemical exchanges between bottom waters and sediments in lakes, which were ecosystems of great interest to me from my lake-survey days. I longed to get back to the Lake District, where I had learned so much from the resident scientists. Moreover, Ada and I were growing tired of life in the big -- and very polluted -- city. We had, incidentally, lived through the Great London Smog of 1952, which killed four thousand people in a week. That experience, together with my work on acid rain, led me a few years later to publish three papers in *The Lancet* and *The Medical Officer* on the relationships of three different respiratory diseases to three different air pollutants -- quite a departure from my usual topics of study!

Although the FBA job interested me greatly, I had never taken any courses in physical chemistry, so I gave no thought to applying until Clifford Mortimer, a member of the FBA scientific staff, came down to London and dropped into the department for a meeting with Pearsall. As it happened, Pearsall had not finished with another meeting, and, because Clifford knew me from my Ph.D. days, he came by my lab for a chat. Among other things we talked about the position they were advertising, because the new appointee was expected to follow up earlier work on the topic by Clifford, who had shifted his attention to the physics of water-mass movements in lakes. After some time he asked if I'd applied for the job, and I pointed out that my knowledge of physical chemistry was nil. Thereupon Clifford remarked that I seemed nevertheless to have some useful ideas on the subject, so why not apply? That encouraged me to do so, and I remain convinced to this day that no physical chemist applied, because I got the job.

Back to the Lake District

Once back in the Lake District and embarked on my new research, it occurred to me that I might at the same time check up on the ideas of Witting and DuRietz. With from 6 to 12 feet of rain a year, in a rural and seemingly unpolluted landscape, it should be a great place to discover whether the chemistry of rain and bog waters was truly similar. So I proceeded to collect and analyze samples of rain and bog water, and was able to that show that Margareta had been perfectly right. But imagine my surprise when I found, serendipitously through my studies of rain chemistry, that in such a rural area we were being drenched not only by sea salt when the winds blew from the Irish Sea, which was not at all unexpected, but also with sulfuric acid when the winds blew from urban/industrial areas in Lancashire, Durham and Northumberland. Thus began, in 1955, my long series of papers on acid rain, which I viewed not as an environmental problem but simply as a fascinating subject for research. That series of papers also lent strong support to the idea that chemical inputs from the atmosphere played an important role in the biogeochemistry of ecosystems. Those papers would never have been written, however, without the concatenation of chance events that sent me from Halifax to London to Sweden and eventually to the Lake District.

While in the Lake District another chance event led me to the study of radioactive fallout. But it all began in Halifax, when as a young teenager in the late 1930's I often went to Buckley's drugstore on Quinpool Road because of the excellent milkshakes served at its lunch counter. Had I not done so, I would probably never have concerned myself with fallout. As it happened, the drugstore manager also owned a bloodhound I admired, and I vowed that I too would own one

someday. When, in 1954, Ada and I were living in a 17th century stone cottage in the Lake District, where exercising a big dog would be easy, that bloodhound came to mind. I began looking around for someone who might sell us a puppy. The only person in the Lake District who owned a bloodhound was Frank Madge, Medical Officer of Health for the County of Westmorland. The man not only pointed me toward a breeder - he and his wife Jean became great friends of ours.

One of the topics we discussed was Frank's attempt to set up a liaison with the British Atomic Energy Authority. He had become concerned that radioactive fallout from an accident at their Windscale facility for making plutonium, on the western edge of the Lake District, would contaminate the mountain lakes that were his reservoirs, supplying water to Westmorland villages. Frank was told very firmly that there was no need for liaison: because every possible safety measure had been engineered into the plant, an accident was impossible. But in 1957 it happened; a fire at Windscale caused large amounts of radioactivity to fall on the Lake District! At first the newspapers assured us that there was no problem. Four days later, however, milk deliveries were shut down over an area of 200 square miles, just as Ada was transferring our new baby Kerstin (named for the daughter of a Swedish ecologist, Hugo Sjörs, who became a long-time friend) from the breast to cow's milk. What a shock! (It took ten years before the public was informed that the Windscale Fire had emitted thousands of curies of radioiodine and hundreds of curies of radiostrontium and radiocesium).

Despite assurances from the Authority that his reservoirs were not contaminated, Frank was skeptical and asked if I could test some of them for him. Never having used a Geiger counter, I at

first demurred, but in the end was persuaded and got my colleague Don Swift, who was working on the uptake of radioiodine by fish, to provide me with instructions. For several days thereafter I evaporated to dryness water samples from Frank's reservoirs, burned the residues in a furnace, and checked the ash for radioactivity. Fortunately, the count-rates turned out to be scarcely above background levels for the system. Nevertheless, because rumour had it that there were local radioactive "hot spots," Frank kept bringing me new samples. Evaporating water was not a very exciting pastime, however, and I got fed up with the negative results. One night, as I was lying in bed considering how to tell Frank it was time to stop testing, a thought came unbidden into my head: why not concentrate the fallout much more effectively by passing much larger samples of water through an ion-exchange resin, of the type that I had been using, serendipitously, in chemical analyses of other waters for chloride and sulphate? The organic resin would absorb such radioisotopes as strontium-90 and cesium-137, and I could burn it and count the ash. Then, as if a light bulb flashed on in my head, I suddenly realized that the local hillsides contained their own ion-exchangers in the form of *Sphagnum* moss, a plant whose cell walls have an unusually strong absorbent capacity. Next morning, on my usual walk before breakfast with our bloodhound Moses (named for a biochemist friend whom he resembled, but who did not appreciate the compliment), I gathered some moss, brought it back to the lab, burned it, and placed the ash in the Geiger counter. When I switched on the counter it began chattering (one could hear the machine counting) so rapidly that I rushed down to Don Swift's office shouting "Don, come quick, something's gone wrong with the counter." He came, took a look and a listen, and accused me of contaminating the machine with radioisotopes from the fume-hood where he stored them. I swore I'd not been near his fume-hood, and he replied that these count-

rates were impossible; I must go and get another sample of moss. I did, and the result was the same. So I began testing the ashes of all sorts of plants: mosses, lichens, ferns, herbs, grasses, tree leaves and garden plants, and found that mosses and lichens were far higher than the others in their fallout content. This was because mosses and lichens -- lacking roots in the soil -- derive their mineral supplies mostly from rain and snow.

Another chance event then led to a new idea. While idly browsing the "in" shelf of our FBA library, I came across a brief mimeographed report from a Norwegian government agency. (We only received three or four of those reports, and why they came to us was unclear). In the report there was a very brief paragraph remarking that the bones of reindeer in Norway were much richer in radiostrontium than the bones of sheep. It was immediately obvious to me that this was because reindeer make lichens a large part of their diet, whereas sheep do not. That realization led me to predict that if someone were to look at the bones of reindeer herders they would get a nasty surprise, and indeed subsequent work by others in Alaska and Finland verified that northern native peoples living on food chains from lichens to caribou and reindeer accumulated fallout radioisotopes to an extraordinary degree. But remember: I never would have done that study had I not drunk milkshakes at Buckley's drugstore on Quinpool Road!

Despite the fact that major parts of my research program involved acid rain, radioactive fallout, and the relationship of respiratory disease to urban air pollution (all accidentally!), I did not regard myself as an applied scientist. Indeed, I would have been offended had someone so described me. At that stage of my life I believed that applied science is second-rate science, a view common at the time and still encountered today. Moreover, I felt no responsibility whatever to communicate my studies beyond my usual science journals, none of whose titles included the

word "applied." Like many young scientists, I was arrogant and had a lot to learn. That said, I believe now – as I did then – that the best science usually comes from one's fascination with the natural world, often without regard to any practical concern, although solving the problems that the natural world presents will often lead eventually, and sometimes surprisingly, to results of practical importance. It seems to me, therefore, important that society should continue to fund a large number of scientists – probably young – to pursue whatever problems they find interesting, without regard to practical outcomes.

At the time I was working on fallout and acid rain, another chance event led me back to Canada; on November 4, 1957, my father Jimmie died suddenly at the age of fifty-five. Because I was an only child and neither Shirley nor I could afford to cross the Atlantic Ocean to visit one another, Ada and I decided that we had to leave England. We had always intended to do so eventually, but life in the Lake District was pleasant, and jobs in my specialized field were not easy to find.

Return to Canada

Academic positions in North America were especially hard to find for an expatriate Canadian living across the Atlantic. Tn those days research universities did not advertise positions, but relied on the "grapevine," or "old boys' network," to fill them. Lacking connections, I sent out sixty resumés to likely places, and eventually received an offer of a lectureship in botany at the University of Toronto. I would be required to teach a large introductory class of two hundred students who were not qualified to enter the Honours program. I had no choice but to accept. At the time I had seven years of experience beyond the Ph.D. -- three as a lecturer at the University of London and four as a research scientist at the FBA -- and I had published more than thirty

scientific papers, mostly as sole author and in excellent journals. The prospect of starting again, at the very bottom of the academic ladder, was not appealing. Moreover, I had to cash in my six years of English pension rights to pay for ship's passage back to Canada for the four of us (Ada, daughter Kerstin, dog Moses and me). Alas, Moses -- who was devoted to Kerstin -- died shortly thereafter of kidney disease, although he was only three years old. We were devastated; it was almost as if we had lost a child.

Although I was soon promoted to Assistant Professor, I was not happy at the University of Toronto. It struck me as being a very authoritarian organization, and during my last two years there, from 1960 to 1962, I worked under a new department head who had little use for ecologists. Another stroke of luck, however, made my stay there interesting scientifically. Alan Gordon, a fellow Canadian who was a student of Pearsall's after I left London and knew of my work on acid rain, called me soon after we had returned to Canada. He was employed by the Ontario Department of Lands and Forests, and asked if I would be interested in working with him on the effects of smelter pollution on the forests around Sudbury. I jumped at the opportunity, and spent some very happy times with him investigating the effects of gaseous sulfur dioxide emitted from the smelters upon forest communities at varying distances from the smoke stacks. We also used small ponds and lakes as natural collectors for the severe acid rain that was caused by the further oxidation of sulfur dioxide to sulfuric acid. In that way, we had a measure of pollution intensity against which to measure damage to both terrestrial and aquatic vegetation.

Another person who made life interesting for me at Toronto was an excellent American plant ecologist, Roger Bray, who had moved to the University of Toronto from the University of Minnesota to avoid supporting the U.S. war machine through the paying of taxes. (When Canada

placed American missiles in North Bay, Ontario, he left for New Zealand, where he remains.) Unbeknownst to me, Roger suggested my name to former colleagues at the University of Minnesota who had an opening for an Associate Professor of Botany specializing in ecology. While I was spending the summer of 1961 working in the laboratory of Professor Hans Jenny, a soil scientist at the University of California in Berkeley, I was visited by a plant-taxonomist friend of Roger's from Minnesota. I was surprised by how interested he seemed to be in my ecological studies -- quite strange for a taxonomist. He wound up our conversation with a request that I send him reprints of a bunch of my papers, which seemed even more strange! Nevertheless, I sent them. A few months later I received an invitation to interview for their open position, and shortly after that I was offered the job.

Although Ada and I did not really want to leave Canada, we were strongly tempted. The offer meant promotion, academic tenure, and a very substantial increase in salary. So I took the Minnesota offer to my department head and asked him what my prospects for advancement were at Toronto. By this time my number of publications had grown to fifty-seven, so I hoped those prospects would be good. Alas, my department head -- who had never been friendly -- regarded my bringing an outside offer to him as blackmail, and told me to go to Minnesota. I did, in 1962, and we have been there ever since.

We and our four children have been very happy in Minnesota, although we still have many ties to Canada. These have been strengthened in recent years because in 1989 our second daughter Vivien married Adrian MacDonald, oddly enough an aquatic biologist like me who had also graduated from Dalhousie. She met him while visiting a college friend at Fort Simpson in the Northwest Territories, and they now live in Dartmouth, Nova Scotia, where he works for

Environment Canada.

Minnesota and the Making of an Environmentalist

I was welcomed with open arms at the University of Minnesota, where the other ecologist, Don Lawrence, made room for me in a crowded building by splitting his lab in two and giving me the larger portion -- an extraordinarily generous gesture that I learned was typical of him. He also, in 1963, organized a seminar for faculty and students on Rachel Carson's new book "Silent Spring." Don had been a graduate student with Rachel at Johns Hopkins, and admired her greatly. Some time after that a colleague, who was scheduled to speak to the Minneapolis Park Board against the use of DDT to treat Dutch elm disease, fell ill. Knowing of my participation in the "Silent Spring" seminar, he asked me to substitute for him, which I did. That led to my being asked to testify on the same matter to the Agriculture Committee of the Minnesota House of Representatives. That in turn caused some confusion among the committee members, because testifying on the other side of the issue was a faculty member from our College of Agriculture, which they regarded as the fount of all wisdom on such matters. Was the university speaking with a forked tongue? (This and similar incidents led ultimately to a university policy that prohibited faculty members from claiming the authority of their university position when speaking on issues before the legislature). Shortly thereafter, and because of my prior experience with radioactive fallout, I joined a newly-formed organization, the Minnesota Committee for Environmental Information, to fight for stricter environmental monitoring of the first nuclear power plant to be built in that state. I had at last become, without consciously willing it, an environmentalist.

These events, and others that followed, brought home to me personally the lesson I had been learning from Rachel Carson and other environmentalists -- that scientists who join their ranks

face strong and well-financed opposition, often from scientists whose research support depends not upon impartial government agencies such as the National Science Foundation in the U.S.A., and the Natural Sciences and Engineering Research Council in Canada, but upon polluters with a vested interest in opposition. There are always "hired guns" to take their money, though this is not to say that all scientists who depend upon polluting industries for their research support are venal. Many are not, and believe completely in their science. One well-known tactic of the "hired guns" is to hammer away at the weakest evidence for the environmentalists' case, while totally ignoring the strongest and most compelling evidence.

My environmental activities led me to consider whether there were other things I should do to advance the environmental agenda, and of course there were. For one thing, I began accepting invitations to speak about environmental topics to groups of citizens in schools, churches, garden clubs, and so on, and to a wide variety of professional organizations. At about the time of the first Earth Day in 1970 I also began, with my colleague Douglas Pratt, to teach during both day and evening school a new course for non-scientists entitled "Biology and the Future of Man." (It is now taught by a friend and colleague, Bill Cunningham, under the gender-free but less engaging title "Environmental Studies.") The course looked at environmental pollution, population growth and increased use of resources as an interrelated, complex set of problems deserving serious attention from both students and the general public. When I proposed it to the Dean and the administrative committee of our College of Biological Sciences, it was not looked on with favour: to them it smacked of "soft science." This, they said, was not the business of the college, which was to train prospective professional biologists and doctors. In the end, and after considerable argument (as department head of Botany I was a member of the administrative committee), we

were permitted to teach the course, but only on an overload basis! (That changed a few years later as a college curriculum committee was formed and took a different view of the matter.) I later developed a course for seniors and graduate students entitled "Ecological Assessment of Environmental Pollution," and more recently taught with astronomers and geologists a unified-science course for freshmen entitled "Our Changing Planet." It started with the "big bang" at the very beginning of the universe and wound up with the global crises of population growth, increased resource use, chemical toxification of ecosystems, global warming, ozone depletion, and the loss of biodiversity. All in 29 lectures, which entailed a lot of argument about what had to be included and what had to be left out.

Since my conversion to environmentalism, I have become active nationally and internationally in committees and workshops dealing with environmental problems (including several sponsored by the Royal Society of Canada and the U.S. National Academy of Sciences). My research on such problems is now focussed largely on chemical aspects of the interactions between plants and their environment, in an emerging discipline called biogeochemistry. My chief concern is with the possible effects of global warming on the vast peatlands of the boreal zone. Those peatlands store about 400 billion tons of carbon, and play a major role in the global carbon cycle. They might, therefore, provide a massive positive feedback to global warming if the frequency of droughts and fires increases sufficiently in the next century to cause subterranean peat fires to smolder for years in remote areas of Canada and Russia, emitting vast amounts of carbon dioxide and other trace gases to the atmosphere.

Looking to the Future

In my view, our global environmental problems stem ultimately from the fact that physics and

chemistry are the easy sciences (a claim that some students may be disposed to dispute). Physics, which examines the fundamental laws of matter and energy, is concerned with the range of phenomena involved in subjects such as mechanics and electricity, which can be described by more or less straightforward mathematical relationships. Chemistry likewise examines the composition of the various kinds of matter and the energetics and dynamics of their reactions, again expressed mathematically. The life sciences are much more complex, involving as they do both physics and chemistry in an attempt to explain -- often in ways not yet reducible to mathematics -- how microbes, plants and animals function, and how they interact with one another and with their environments. The social sciences are far more complex yet, because they involve not only all the phenomena of the life sciences but also those interactions within and among human beings and their societies that we include under the topics of political science, sociology, economics, philosophy and ethics.

New technologies, based on physics and chemistry, continually produce new threats to living systems, and therefore to human well-being. Because the scale of human activities is increasing exponentially, the threats increase similarly, despite the fact that some technological advances do indeed slow the rate of environmental degradation. Advances in the life sciences, and especially in the enormously complex social sciences, come much more slowly than advances in technology, so that we remain unable to assess adequately the multifarious and interacting technological threats to the planetary ecosystem, or to design effectively the scientific and socio-political measures necessary to cope with them. As I have said elsewhere (in a book, "Placing Nature: Culture and Landscape Ecology," edited by Joan Nassauer): "This imbalance in the pace of technological and social change, which we should seek far more actively to address, may well become, if it is not

already, the chief limit to progress in environmental protection and perhaps ultimately the cause of ecological catastrophe." To address such imbalance will require students trained far more broadly than are all but a few today, students who are able to devise social and geopolitical policies far better grounded in science than those now in force. Let us hope that our universities will be up to the task of producing them; the raw material is certainly there in terms of both intelligence and idealism.

Advice to Students

Graduate students usually have a pretty good idea of what they want to do, and so I can usually help them with the mechanics of how to go about it: what courses to take, which faculty members to consult about specific scientific problems, and what the employment prospects are for their particular disciplines. They are seldom aware, however, of the important roles chance and serendipity are likely to play in their scientific lives. Because of the part chance has played in my own education, research career and employment, I find it difficult to provide undergraduate students with any detailed advice concerning their as yet unfocussed plans for a career. Indeed I am reduced to a few simple admonitions. Try to work toward what is most meaningful to you because that is what you will enjoy most and do best. What your parents want, or what is most profitable, may not be right for you. In this increasingly competitive world, get as much education as you can. That education will necessarily be specialized within the field of your choice, but you should also pursue broadly interdisciplinary studies because the world needs generalists who can see and evaluate the big picture of where human societies, and the planetary ecosystem, are heading in what seems an increasingly precarious future.

For students who are planning careers as environmentalists, I have two more pieces of

advice. First, if you pursue your studies because you want to save the environment, you are to be commended. If, however, you do not believe that to be also the most interesting thing you can do with your life - something that engages your intellect as powerfully as your idealism - you are unlikely to be as successful as you might wish. Second, you should if possible become a recognized expert in a subfield of environmental studies. Your credibility in discussing the big picture will be greatly enhanced if you can show that you are an expert in your own subfield, even though you cannot claim expertise beyond it.

Finally, all students should bear in mind two famous aphorisms. One, attributed to Linus Pauling, is this: "In order to have good ideas it is first necessary to have lots of ideas." The other is the famous dictum of Louis Pasteur: "Chance favours the prepared mind." Chance has often been my friend, as these pages attest. Prepare to make it yours as well!