THE WAYS IN WHICH

I HAVE

PURSUED SCIENTIFIC

INVESTIGATIONS

OVER 60 YEARS

By

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There are diverse ways of approaching a scientific problem. Often one is only following up one or more aspects of one's earlier work, but truly original research can come about in many ways. In my case I have often followed a path described in 1754 by the writer Horace Walpole, who coined the term "serendipity" from the characters in a Persian fairy tale: "The Three Princes of Serendip", who were continually discovering interesting things for which they were not searching.

Both my graduate theses followed this path. My M.Sc. thesis, suggested by my supervisor at Dalhousie University in Halifax, Nova Scotia, was to have been a study of the effects of temperature on a secretory organelle, the Golgi body, in the cells of salmon embryos at different stages of development over time. The range of temperature was selected to go well beyond the range at which the salmon could survive to adulthood. I was never sure of the significance to be attached to such a study, but I set about growing salmon eggs in a series of baths of running water set at different temperatures. The project made me uneasy because the embryos were transparent, so that one could see their little hearts beating and the red cells circulating through their veins. I had never liked killing animals, and it was particularly unpleasant to be destroying such beautiful little creatures by the hundreds. I persevered, however, but the experiment's original purpose failed utterly because the technique that I had been using to demonstrate the Golgi bodies in other organisms did not work with the embryos. What on earth was I to do?

Fortunately one of the purposes of the experiment had been to see what the Golgi bodies looked like in the cells of embryos at different stages of development, which had been described much earlier by my supervisor. I happened to notice that shifting the temperature, even within the normal range, could dislocate the appearance of these stages. Outside the normal range the dislocations became severe enough to produce "monsters", such as fish with curved backs or two heads! More extreme temperatures resulted in early death. I had found, most fortunately, something for which I was not looking, and so was able to produce a successful thesis after all.

Such temperature-involved dislocations of development are undoubtedly involved in the susceptibility of fish to thermal pollution, and are certainly of far greater importance than anything I might have found out about the Golgi bodies of salmon embryos!

My Ph.D. project at University College, London, (in botany – no slaughter of animals involved this time!) ended up in much the same way. My supervisor, a superb mentor and the kindest of men, suggested that I investigate the mineral uptake of diverse plant species in a variety of plant communities in the English Lake District. This study did not get very far, although I incorporated some of its results in my thesis. As I gathered data to characterize the soils in which my plants were growing, I observed that as their organic matter increased so also did their acidity, while saturation of the ion-exchange capacity of the soil with bases (chiefly calcium) declined. The acidification of both woodland and wetland soils as they accumulated increasing amounts of organic matter became the focus of my thesis, again, something I had not had in mind when I started.

Two other examples of serendipitous research, in which chance also played a vital role, are detailed in two published popular articles: (1) on lake acidification by acid rain caused by long-distance transport of air pollution, and (2) the extraordinary capacity of lichens to accumulate radioactive fallout. These are included here in their published form. The acid-rain paper also describes how I was led to discover mortalities from three distinct lung diseases related to three different air pollutants.

Another research technique is simply to gather some data about what is often a vague idea one has, and see whether there is perhaps something they can tell us. This is how I and several colleagues were able in 2007 to publish in an excellent journal: "Quaternary Science Reviews", a paper entitled "Temporal and spatial aspects of peatland initiation following deglaciation in North America". This is, I think, the best research I have published in the last three decades, owing chiefly to the modeling skills of my colleague Clarence Lehman. It all started, however, from a casual conversation about 25-30 years ago with my research associate Jan Janssens, in which I remarked that in the course of our peatland studies we were coming across lots of data on the depths and ages of peat deposits. "Suppose", I suggested, "that we were to collect a large bunch of these data; I bet they could tell us something". So we set about doing so, and when we had accumulated almost 400 data sets I wrote a short paper about what I thought they could tell us. I knew, however, that if we could relate peatland initiation to the retreat of the ice sheets the paper would be much more meaningful. Somehow I learned that the expert on the retreat of the Laurentide ice sheet was Art Dyke of the Canadian Geological Survey. I sent him our brief paper, wondering whether he could help us . He replied that there must certainly be a connection, and by the way, he had another thousand or so more peat depths and dates, why didn't we throw them all together and see what came out of it – an extraordinarily generous gesture. At about the same time my colleague Margaret Davis said that I should get in touch with Clarence Lehman, an unusually talented mathematical modeler, who could probably devise a model to fit our mass of data. This he did, and our recent paper is the result. Because we know the average amount of peat in a cubic meter of peatland and its average carbon content, we are now following up with a paper on the accumulation of carbon in North American peatlands, an important reservoir in the global carbon cycle, over the past 20,000 years. That casual conversation is at last paying off!

One of the commonest ways of doing science, though rarely practiced by me, is to have an idea, set up a hypothesis to test that idea, and proceed to whatever new ideas are raised if the hypothesis is correct. If it is not, try another hypothesis. I followed this practice after I failed to persuade colleagues working on acid rain that colored organic acids from peat bogs were an important but neglected source of acidity to lakes in the boreal zone. To test this hypothesis, I and some colleagues collected lake waters at varying distances from the Tufts Cove Power Plant, in the middle of Halifax, Nova Scotia, its capital city. Some of the waters were extremely clear, whereas others came from tea-colored bog lakes. Analyzing the data in 1986 in the Journal "Nature", we made it clear that both acid rain and bog waters were acidifying lakes around Halifax, thoroughly vindicating my hypothesis. It was this article that I had in mind when considering our recent paper the best thing I had done in three decades!

One of the consequences of the ways in which I have pursued research, relying so heavily on chance and serendipity, is that the research itself has been unusually diverse. Along with projects related to those mentioned here have been topics such as fossil pigments in lake sediments, chemical classification of lakes, litter-fall in forests, observations on the formation and breakdown of the oxidized microzone at the mud surface in lakes, shoot height, weight and standing crop in relation to density of monospecific stands of plants, and the history of ecology and biogeochemistry.

Lake Acidification by Long-Distance Transport of Air Pollution: A Serendipitous Discovery at the FBA in the Mid-1950s*

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Most limnologists and freshwater biologists are aware of the importance of acid rain, caused by urban-industrial air pollution, in the acidification of lakes on base-poor soils, with consequent effects on their biota. Probably few, however, even among members, recall that research on the subject originated – largely by chance and serendipity – at Ferry House in the mid-1950s.

After doing research on ecosystem acidification at Wray Castle for my Ph.D. in 1951 at University College London, where I stayed on as Lecturer in Botany, a chance encounter with Clifford Mortimer led to a staff appointment in 1954 at Ferry House (Looking Back – *FBA News* No. 14, 2001). Because I was now in a rural – and as I thought unpolluted – area, it occurred to me to check out a claim by Margareta Witting, a graduate student I met while on a fellowship in Sweden. She was studying the chemistry of peat pools there, and her results suggested that raised bogs received all of their ion supply from atmospheric deposition. It seemed entirely reasonable, given their domed shape, but I thought to myself: "She's never analyzed the rain".

In 1954, therefore, I started to collect local bog waters and rain samples for analysis, and found, not surprisingly, that Margareta was right. I was, however, greatly surprised to find that when the wind blew from the heavily polluted urban-industrial regions of Lancashire and Northumberland/Durham we were drenched by dilute sulphuric acid. As it happened, John Mackereth and Jack Heron were at this time surveying the ionic composition of lakes and tarns all over the Lake District and finding that the waters of tarns on the hard rocks of the central mountains were not only very dilute but also unusually acid. Fortunately the explanation was at hand – acid rain!

Although Mac was not ready to publish their data, with characteristic generosity he allowed me to point out (Gorham 1955) that: "While upland tarns owe most of their acidity to rain, which enters them chiefly as superficial runoff, they may also receive added Ca and K from atmospheric pollution. Lowland waters may also obtain more nutrients due to neutralization of rain acids by bases from soil and rock minerals". After publication of their data (Mackereth 1957) I was able to use them (Gorham 1958a) to demonstrate quantitatively the importance of atmospheric contributions of several major ions to natural waters. I continued to study acid rain for the next forty years.

Realizing that it must have biological effects on the biota of the tarns, I could not think of any available data to examine the matter. It occurred to me, however, that the air pollutants associated with acid rain might have effects upon human health, and fortunately the Department of Scientific and Industrial Research had been analyzing precipitation in a large number of urban boroughs for which the Registrar-General had recorded data on mortality from respiratory diseases. With Charlotte Kipling instructing me in the mysteries of partial correlation and regression, I was able to show that bronchitis mortality correlated with the acidity of precipitation (Gorham 1958b), in these urban areas largely owing to hydrochloric acid (Gorham 1958c), whereas mortality from lung cancer correlated with tar deposition (Gorham 1959a) and pneumonia mortality with sulphate deposition (Gorham 1959b). It was all a long way from freshwater biology, but Hugh Gilson never even hinted that I was straying too far from freshwater biology. For that I remain truly grateful, and I believe that one reason that the FBA was so successful in those days was his tolerance in allowing its scientists to follow their inclinations unconstrained by any particular "mission".

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**With a foray into the field of public health.*

THE TRENT: A RIVER REGAINED

The fourth newsletter of the River Trent and Trent valley rehabilitation project *"ON TRENT"* is recently published. It makes stimulating reading. It can be obtained via www.ontrent.org.uk

Windscale, Ferry House, and Alaska: An Unlikely Connection

Eville Gorham tells us why

Non-scientists, and even science students, seldom realize the importance of chance and serendipity in research. Both are very evident in the research I describe here.

The story began in Halifax, Nova Scotia, where as a teen I often went to Buckley's Drug Store because of the excellent milkshakes served at its lunch counter. The manager owned a bloodhound I admired, and I vowed that one day I would own one. When, in 1954, I came to work at Ferry House that bloodhound came to mind. The only person in the Lake District who owned one was Frank Madge, Medical Officer of Health for Westmorland. He suggested a breeder, and he and his wife became our great friends.

One topic we discussed was his attempt at liaison with the Atomic Energy Authority. He worried that fallout from an accident at the Windscale plutonium factory, on the western edge of the Lake District, would contaminate mountain tarns that supplied water to Westmorland villages. Frank was told that there was no need for liaison; every possible safeguard had been engineered into the plant. Nevertheless, an accident happened on 9 October 1957, when a fire at Windscale caused large amounts of radioactive material to fall across the Lake District! At first we were assured there was no problem. Four days later, however, milk deliveries shut down over 200 square miles (Madge 1957), as Ada was weaning our baby Kerstin from the breast to cow's milk. What a shock! (A decade later we learned that the fire emitted thousands of curies of iodine^{131} and hundreds of curies of strontium^{90} and cesium^{137}. We never did learn whether plutonium isotopes were emitted).

Despite assurances that his reservoirs were not contaminated, Frank was skeptical and asked me to test some of them. Never having used a Geiger counter, I demurred, but finally was persuaded and got Don Swift to instruct me. For several days I evaporated water samples from Frank's reservoirs, burned the residues in a furnace, and checked the ash for radioactivity. Fortunately, count-rates were scarcely above background.

A youthful Eville Gorham taken by John Mackereth.

Nevertheless, rumours of local "hot spots" kept Frank bringing samples. One night, lying in bed considering how to persuade him it was time to stop, a thought came unbidden into my head: why not concentrate the fallout by passing much larger samples through an ion-exchange resin of the type I had been using, serendipitously, to analyze waters for anions? The organic resin would adsorb radio-isotopes such as strontium^{90} and cesium^{137}, and I could burn it and count the ash. Then, as if a light flashed in my brain, I realized that local hillsides were covered with *Sphagnum* moss, a plant whose cell walls have unusually strong adsorbent properties. Could mosses tell more about Windscale than water samples?

Next morning, walking with our bloodhound Moe, I gathered some *Sphagnum*, brought it to the lab, burned it, and placed the ash in the Geiger counter. When I switched on it began chattering (one could hear the machine counting) so rapidly that I rushed to Don Swift's office shouting: "Don, something's wrong with the counter". He came, took a look and a listen, and accused me of contaminating the machine with iodine^{131} stored in his fume hood. I swore I'd not been near it, and he replied that these count-rates were impossible: I must get another moss sample. I did, with the same result. So I began testing all sorts of plants: mosses, ferns, herbs, grasses, tree leaves and garden plants. Mosses were far higher than others in radioactivity because – lacking roots – they derive minerals chiefly from atmospheric deposition (Gorham 1958).

Was Windscale responsible for the radioactivity I measured? Analysis of mosses close by, and as far away as Wales and northwest Scotland, yielded similar count-rates, so it was not. If not, was the radioactivity natural, or owing to fallout from nuclear-weapons tests? Two circumstances indicated fallout. Comparison of my data with analyses of herbarium specimens collected before nuclear testing (whose radio-activity was due to long-lived isotopes) showed the latter to be very much lower in activity. My samples, more-over, exhibited substantial decay over five months, a certain indicator of fallout.

I continued testing, and this time included lichens. They, like mosses, were highly radioactive, for the same reason. Another chance event led to a new idea. Browsing in our library I came across a brief report from the Norwegian Defence Research Establishment (Hvinden, no date). We received only a few of these, and why they came was unclear. The report had a paragraph noting that reindeer bones were much richer in strontium-90 than sheep bones. Obviously this was because reindeer feed extensively on lichens, whereas sheep do not, as I wrote in a second paper (Gorham 1959). I predicted to colleagues that if someone were to analyze bones of reindeer herders they would be surprised.

Following these studies I returned to limnological research. In 1961, however, I learned that my radio-activity studies were playing a part in

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the battle over the U.S. Atomic Energy Commission's proposal to use nuclear explosives to blast a harbour on the northwest coast of Alaska, an area inhabited by Eskimos eating substantial amounts of caribou meat. The project was code-named *Chariot*, part of *Project Plowshare* to develop peaceful uses of atomic energy.

The Committee for Nuclear Information (CNI) of St. Louis, Missouri, undertook to examine environmental and biological information gathered as background for *Chariot*, and to assess risks and benefits. They published the assessment (CNI 1961) in their magazine *Nuclear Information*, and concluded that local food chains would be contaminated by fallout: caribou would eat lichens and Eskimos would eat caribou, concentrating fallout at each step. Their concern about lichens was based on my research: "A direct comparison of ... various types of plants reported from studies in the English Lake District shows that lichens take up several times more ... fallout than other plants in the same location" (Commoner 1961). They also understood the link I established between lichens and reindeer. A few months later, analyses of strontium[90] in Eskimos (Schulert 1962) showed body burdens much higher than in other Alaskans and inhabitants of the lower forty-eight states. Investigations from 1964 on also showed extraordinary levels of caesium[137] in Arctic caribou and reindeer herders (table, p. 15, Brodine 1975).

Project Chariot died, in part owing to Eskimo protests, and the battle to kill it lent strong support to growing concerns over testing nuclear weapons in the atmosphere (Brodine 1975), which led eventually to the treaty that banned them. I was pleased that my fallout studies – heavily influenced by chance and serendipity – had a small part to play in such momentous events. It was a long and tortuous journey from drinking milkshakes in Halifax! But as Louis Pasteur famously observed: "Chance favours the prepared mind".

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