CONTRIBUTIONS

Two Contrasting Approaches to Ecological Research

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Recently three graduate students (Prather et al. 2009) decided to put the "Ph" back into Ph.D., framing graduate research in a theoretical context. They prepared four critical questions as follows:

1) What explicit theories have inspired my hypotheses?
2) How do components of these theories fit together in framing my hypotheses?
3) How can the results from my research be generalized?
4) What components of existing theories are changed by the generalization of my results?

The faculty responder to the students (Crowl 2009) heartily approved this approach: "All scientific advances come from an understanding of the conceptual underpinnings, the literature, and knowledge currently amassed, followed by asking the critical questions necessary to fill the knowledge gaps." Indeed, he went further: "Perhaps the most important insight…is the nuance that guiding questions come before research design and implementation. I've seen many, many instances of researchers searching for theoretical linkages to interesting and important data sets after the data have been collected. Rarely, if ever, would such an approach result in major theoretical breakthroughs."

Useful as the students' theoretical, rule-based approach may be, and I have followed it myself, particularly questions (3) and (4), there is another, distinctly different path to success offering exciting possibilities. I call it: "the opportunistic approach to doing research." It uses chance and serendipity as guides, following Pasteur's celebrated dictum: "Chance favors the prepared mind." Such research sometimes involves only the thought: "What can this mean, I know of no studies? So let's collect some data and see what they suggest."

Chance and serendipity are everywhere; researchers should recognize and exploit them. Alexander Fleming, for example, would have made no progress with penicillin had he started with what is known about curing disease. What hypotheses could he test to discover it? What explicit theories could inspire these hypotheses? Rule-based research can follow, but only *after* the essential discovery, in this case examining a plate of bacteria contaminated by a fungus, rather than disposing of it.

Because the "back stories" of opportunistic research are rarely available, Fleming's being the only exception known to me, I shall continue with explicit examples of my own. The first involved making lemonade out of lemons. My M.Sc. in zoology at Dalhousie University was to be a study of temperature effects on a cell organelle, the Golgi apparatus, in salmon embryos, at different stages of development already defined by my supervisor. Salmon eggs were grown in baths of aerated water at different temperatures, the range going well beyond that at which embryos developed normally. However, the experiment failed because I over-stained the organelles. Might this end my academic career? I examined my notebooks in desperation to see if anything could be salvaged. Fortunately I noticed that shifting temperature, even within the normal range, dislocated the appearance of some developmental stages. Outside that range dislocations were sometimes severe, producing "monsters" such as fish with two heads or crooked backs. Extreme temperatures resulted in early death. Serendipitously, I found something for which I was not looking and produced a successful thesis (Hayes et al. 1953). Such temperature-induced dislocations are undoubtedly involved in susceptibility to thermal pollution, a topic not then of much concern.

Likewise, my Ph.D. thesis in plant ecology at University College, London, changed radically over time. I was to investigate mineral uptake by diverse species in woodland and wetland plant communities of the English Lake District. The original study did not get far, although I incorporated some results in my thesis. As I gathered data characterizing the soils in which my plants grew, I noticed that as organic matter increased, so did acidity. Acidification of woodland and wetland soils accumulating increasing amounts of organic matter became the focus of my thesis, nothing like the original one (Gorham 1953*a*, *b*).

Capitalizing on serendipity played a major role after joining the Freshwater Biological Association's laboratory in the English Lake District, a rural and—I assumed—unpolluted area. On a field trip in Sweden, Margareta Witting, studying the chemistry of raised-bog pools, told me they received mineral inputs solely from the atmosphere. Quite reasonable, given their domed shape, but I thought: "She's never analyzed rain." Back in the Lake District I collected bog waters and rainfall samples. Not surprisingly, Margareta was right. I was surprised greatly, however, by some of my rain-chemistry data. When wind blew from the west we were drenched, not unexpectedly, by sodium chloride from the Irish Sea, but when it blew from the south and east we were drenched by dilute sulfuric acid! It was industrial pollution, now described as "acid rain."

Serendipitously my colleague John Mackereth was surveying water chemistry throughout the Lake District and finding that lakes on hard rocks of the central mountains were unusually acid. Fortunately I could tell him the cause was acid rain. Although Mac was not ready to publish (Mackereth 1957), he generously allowed me to state that small lakes "owe most of their acidity to rain, which enters them chiefly as superficial runoff" (Gorham 1955). After he published his data I used them to demonstrate the importance of atmospheric deposition of several major ions to dilute natural waters (Gorham 1958*a*).

Realizing that acid rain must have biological effects on aquatic biota, I could think of no available data to exploit. I realized, however, that air pollution associated with acid rain might affect human health. The Department of Scientific and Industrial Research analyzed precipitation in urban boroughs for which the Registrar General recorded mortality from respiratory diseases, so relationships could be sought (*at last, a hypothesis!)*. After partial correlation and regression, bronchitis mortality related positively to acidity (Gorham 1958*b*), whereas mortality from lung cancer related to tar deposition (Gorham 1959*a*), and pneumonia mortality to sulfate deposition (Gorham 1959*b*).

Another serendipitous project developed when, on 9 October 1957, the Windscale plutonium factory on the western edge of the Lake District caught fire and released thousands of curies of ^{131}I and hundreds of curies of ^{90}Sr and ^{137}Cs. My friend Frank Madge, Westmorland's Medical Officer of Health, urged me to test some reservoirs for his villages. For days I evaporated water samples, burned the residues in a furnace, and checked ß-radioactivity with my colleague Don Swift's Geiger-counter. Count rates were scarcely above background, but rumors of local "hot spots" kept Frank bringing samples. Wondering how to persuade him to stop, a thought came unbidden into my head: "Why not concentrate fallout by pouring much larger quantities of water through an organic cation-exchange resin I use for analytical procedures? It will adsorb ^{90}Sr and ^{137}Cs, and I can ash it in my furnace." Suddenly that night, while lying in bed, I had an epiphany, remembering that *Sphagnum* moss was a powerful ion-adsorber ubiquitous in the Lake District!

Next morning I gathered a moss sample, burned it, and placed the ash in the counter. When the counter was turned on, it began chattering so rapidly that I ran to Don's office shouting: "Come quick, something's wrong with your counter." He came and listened, accusing me of contaminating his counter with ^{131}I from his fume hood. I swore I'd never been near it, so he said: "Those count rates are impossible; get another sample." I did, with the same result, and began testing other plants: ferns, herbs, grasses, tree leaves and garden plants. Mosses were far more radioactive than all others, because, lacking roots, they derive minerals chiefly from the atmosphere (Gorham 1958*c*). Was Windscale responsible (*a hypothesis again*)? Analysis of mosses close by and far away in Wales and Scotland yielded similar count-rates, so it was not. If not, was it natural, or global fallout from nuclear-weapon testing? Two circumstances indicated fallout. Herbarium plants collected before nuclear testing (whose radioactivity involved long-lived isotopes) were distinctly lower in activity. My samples, moreover, exhibited substantial radioactive decay over five months, a certain indicator of fallout.

I continued testing, this time including lichens. Like mosses, they were highly radioactive, for the same reason. Another chance event led to a new idea. In our library I encountered a brief report from the Norwegian Defense Research Establishment. One paragraph noted that reindeer bones were much richer in ^{90}Sr than sheep bones. This, I was sure, must be because reindeer feed on lichens, not grass (Gorham 1959*b*). I predicted to colleagues that if someone analyzed Eskimos or Lapp reindeer herders they would get a nasty shock! Sure enough they did; both accumulated extraordinary levels of ^{90}Sr and ^{137}Cs. Such food-chain accumulation, in which my research played a small but significant role, bolstered the struggle to ban nuclear testing in the atmosphere (Brodine 1975).

Another opportunistic technique is simply to gather data about an interesting topic to see whether they provide a story. This is how colleagues and I were able to study peatland initiation across North America following deglaciation over the past 20 000 years (Gorham et al. 2007). The research began from a casual conversation in the mid-1980s with my research associate Jan Janssens. I remarked that in our peatland studies we saw lots of data on ages and depths of peat cores. "Suppose," I suggested, "we collect lots of these data; I bet they'll tell us something." We gathered almost 400 data sets, whereupon I wrote a brief manuscript about them. Our data would be more meaningful if we could relate initiation to glacial retreat, and I learned that the expert was Art Dyke of the Canadian Geological Survey. I sent him our manuscript, wondering if he might help. He replied that there must be a connection, and he had another thousand or so dates. Generously he suggested we put them together and see what happened. Then my colleague Margaret Davis suggested I contact Clarence Lehman, a mathematical modeler in our department, who could probably devise a model. He was excited to do so, and our 2007 paper was the collaborative result. Because we know the average amount of carbon in a cubic meter of peat, our depth data are being used for a paper (Gorham et al., *in review*) on carbon accumulation in North American peatlands, an important reservoir in the carbon cycle, over the past 20 000 years. That casual conversation finally paid off, with strong relevance for the most important environmental problem of our time, global warming!

One consequence of "opportunistic research" is its diversity. In my case, along with topics related to those mentioned, have been others such as the significance of fossil pigments in lake sediments, formation and breakdown of the oxidized microzone at the sediment surface in lakes, defining floristic boundaries, shoot height, mass, and biomass in relation to density of mono-specific plant stands, litterfall in forests, habitats of photosynthetic bacteria, and the history of plant ecology and biogeochemistry. Finding colleagues able and willing to help has been of great importance in carrying out many of these opportunistic projects, which then turn into team projects.

I suspect other ecologists operate equally haphazardly. Chance and serendipity have favored me, and led to my most exciting research. I am sure they will assist researchers in the future as they did Fleming and many others in the past. It is also important to remember another dictum, from Linus Pauling: "The best way to have a good idea is to have a lot of ideas and throw away the bad ones."

Acknowledgments

I appreciate advice from Clarence Lehman, Celia Hemmerich, and Shelby Williams.

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