of science—the role of women in science and the history of ecology.

Mrs. Richards is usually honored as the first woman to attend the Massachusetts Institute of Technology and as the guiding spirit of the home economics movement from about 1890 to 1910. Robert Clarke, cofounder of the International Institute of Euthenics, argues, contrary to other assessments, that rather than coming to home economics at the climax of a long career in food and sanitary studies Mrs. Richards arrived at it only after her vision of a more comprehensive "home ecology" or "euthenics" had been "slapped down by the hierarchy of science" (p. 211) and she was forced to retreat to various women's groups and reformulate her ideas (which Clarke admits elsewhere were a "mixed bag") as the largely female science of "home economics." If true, this episode would present an interesting case of sex discrimination in the 1890's. Unfortunately Clarke does not make a convincing case and cites no direct evidence for this interpretation. Although he used most of Mrs. Richards's publications and many letters still in private hands, he relies heavily on the 1912 biography by Caroline Hunt and gives little additional information on Mrs. Richards's relationship to the rest of the scientific community. Mrs. Richards encountered difficulties that are probably attributable to her sex (she was frequently a hardworking assistant on a project for which the male director earned all the credit), but she was also a chemist interested in water studies at a time when bacteriology was sweeping the field, she lacked a Ph.D. at a time when the degree was becoming increasingly important (although still well-nigh unobtainable for a woman in the 1880's), she was a firm "environmentalist" at a time when many other Bostonians were becoming "hereditarians," she preached conservation at a time when the industrialists were discovering "planned obsolescence," she was interested in applied science rather than in pure science, and, perhaps worst of all, she was concerned more with diffusing applied science to laymen (especially housewives, children, and immigrants) than with advancing it through her own researches, as did W. O. Atwater, for example. Thus her work was more akin to agricultural extension and public health than to pure science, and she would have occupied a marginal position in any scientific community regardless of her sex. Moreover, in her reform interests Mrs. Richards, for all her differences, was not unique, and from Clarke's account she seems curiously out of touch with other reformers of the time; for instance she seems, from Clarke's account, to have had no association with Harvey Wiley, who crusaded for the pure food and drug law of 1906. Clarke would have given us a better picture of Mrs. Richards had he tried to place her more within her own context (or "environment" in a certain sense) and not tried so hard to make her speak like an environmentalist of the 1970's.

Clarke raises, however, the interesting additional question of the relationship of these various environmental reformers between 1890 and 1920 to the emergence of the science of ecology. He sees early ecology, which was limited largely to studies of plant and animal communities, as reflecting a reluctance or unwillingness on the part of zoologists and botanists to take on the larger, more political concerns of protecting the environment. But suggestive as this interpretation is, we have as yet too few studies of the history of ecology to judge its validity with certainty.

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Unbalanced Ecosystems

Peatlands. P. D. MOORE and D. J. BELLAMY. Springer-Verlag, New York, 1974. viii, 222 pp., illus. $12.

This information-packed little book, the first of its kind in English, is a compact summary of an important and little-known subject. Peatlands—unbalanced ecosystems that store much of their organic product—cover 230 million hectares, or about 1.5 percent of the earth's land surface. Nearly all of this is in Canada and the U.S.S.R., but about a third of Finland and 14.5 percent of Sweden are occupied by peatlands. A single subtropical country, Pakistan, claims 200 million tons of air-dry peat. The total carbon reserve of peatlands, long given as 223 billion tons, is raised in this book (without documentation) to 330 billion tons. At the lower estimate, the reserve amounts to about 20 milligrams of carbon per square centimeter of earth's surface (0.02 g/cm_e^2), some 16 percent of the atmospheric reservoir of CO_2, or 7 percent of the terrestrial biosphere. As a combustible source of power, peat is unimportant compared to coal, but the known reserve, if burned, would more than duplicate the "industrial effect"—the 15 mg/cm_e^2 of fossil carbon that had been added to the atmosphere by 1950. Whether it is burned, exploited for horticulture, or left in place to perform its manifold ecological functions, peat is not a neglible resource.

The authors do not neglect the economic geology and conservation of peat, but their main interest is in peatlands as ecosystems specialized for storage. The great strength of the book, in fact, is its "IBP (International Biological Program) viewpoint"—the espousal of biogeochemical ideas about ecosystems as productive systems with definable inputs and outputs. This approach appeared on the IBP scene in 1967, in a peatlands setting, as the "Moor House model" of production, and has given a new look to much terrestrial ecology. Biogeochemistry is here applied not only to quantitative considerations of production, decomposition, and mineral cycling but to the morphology of bogs and the geography of boggy landscapes. One effect is to dissect out an element of "climate" that climatologists usually ignore and to put the old topic "climate and peat growth" on a new footing.

Nutrition of bogs, during upward and outward growth, has long been known to be increasingly independent of surface- and groundwater supply and to become totally dependent on solutes supplied by rainfall. To an older generation of bog geologists (who were botanists by training), differences in bog structure were sufficiently accounted for by "pure water" (for high moor or raised bogs) and "water plus minerals" (for low moors). Now that we know that rainwater is far from pure, ecologists can focus on inputs—concentrations and fluxes of various solutes—and ask whether all solutes are equally important, what proportion of them is airborne, and which bog-plant associations select and concentrate which nutrients and export others.

Making good use of recent studies, some of which were their own, the authors consider these questions and give some tentative answers. Analytical data on rainfall are still scanty outside the western British Isles, a fact that perhaps accounts for a red-brick-university or Celtic bias that is occasionally evident in the book. Within Britain, one outstanding discovery is that three major plant nutrients, nitrogen,

phosphorus, and potassium, which have very different geochemistries, have airborne inputs to Welsh and Pennine bogs that are of the same order of magnitude, ~1 kilogram per hectare per year. Inputs of major cations (sodium, calcium, magnesium) are an order of magnitude higher; they are also higher, by an order of magnitude, than those from continental air over Hubbard Brook, New Hampshire.

Since the peatlands of maritime Britain are nourished by a specially nutritious rain, the argument can be defended that such a characteristic bog plant as *Eriophorum* is limited by airborne supplies of potassium. The argument, which seems at first sight implausible, is justified not alone by the small inputs of potassium but by the unexpectedly high inputs of phosphorus. Unfortunately, for its generality, we do not know how far eastward the Lancashire air continues to shed kilogram quantities of nutrients. The phosphorus may all be derived from the Irish Sea, in which case the potash limitation may be strictly maritime. If more or less equally nutritious rain falls elsewhere in northern Europe (rain now dangerously enriched in sulfate falls in Sweden), we can begin to see why ombrotrophic systems are prevalent there and why they are scarce in North America west of the Maritime Provinces. (The extensive peatlands of subarctic Canada appear to be mainly rheotrophic, "nourished by the flow," and not ombrotrophic, "nourished by the sky.")

The special ecology of ombrotrophic bogs has always been appreciated by biologists, but the enormous differences in productivity, compared to rheotrophic systems on calcareous substrata, will probably surprise many ecologists. Figures for primary production, in grams of dry matter per square meter per year, range from about 100 in arctic tundra and 635 in Pennine blanket bog to 4600 in a German reed swamp, a rheotrophic "rich fen."

The classification of peatlands as rheotrophic or ombrotrophic is not new, though the terms are; it is the familiar "morphologic-climatic" dichotomy of low moor and high moor, with gratifying attention to that element of climate that is critical for morphology. Since it pays attention to growth and internal structure and their causation, the classification also seems to be genetic, as a sound geomorphic classification should be. Whether it is *adequately* genetic, embracing the entire Holocene history of all peatlands, or takes into account only their most recent stages, is a different question. It seems to me that the biogeochemical approach encounters limitations when confronted by significant climatic change.

Many bogs, under similar topographic and climatic conditions, have had rather different histories. Moreover, these histories are not all of the same length. Accumulation rates of peat, as measured in dated bog sections, are of the order of a meter per millennium, rising to half a meter per century in fresh *Sphagnum* peat. Yet, apart from those on filled-in lakes, extremely few mires or blanket bogs have total depths greater than 2 to 4 meters. Within a Holocene span of ten millennia, then, either the growth of most bogs is a post-Little-Ice-Age phenomenon or episodes of bog growth have been interrupted by periods of stability, intensified decomposition, and erosion. Such nongrowth periods are well marked in many bogs, especially north of latitude 50°N in Europe and 45°N in North America, by recurrence horizons. The climatic changes they imply have correlates in the pulsed growth of glaciers, sea ice, and permafrost. The chapters on history and stratigraphy mention these points, but in perfunctory fashion. The authors virtually dismiss recurrence horizons, on evidence that they are not synchronous across one German bog, and in numerous small ways these chapters do less than justice to an enormous paleoecological literature. Testaceous rhizopods, for example, are not mentioned, and reference to pre-Iron-Age archeology is inadequate.

I have referred to a Celtic bias, but the book has a subtler bias that is philosophically more interesting. The authors are young, and it is well known that interest in history intensifies as a person lives through more of it. In a word, the bias of this book is ahistorical. Such an approach, though rarely explicit, is characteristic of the IBP effort in ecology. It is a natural one for young, chemically oriented biologists who are prepared to set aside ancient history, not as irrelevant, necessarily, but on methodologic grounds: to see how far they can get on steady-state assumptions.

One must agree that the input-output approach to modern ecosystems has taken us a long way in a short time. Steady-state assumptions, however ahistorical, have proved methodologically powerful in other fields as well. In anthropology the same approach is called functionalist, and geologists call it geophysics. Though an elderly historian finds it a bit surprising when applied to so richly historical a field as bog stratigraphy, he does not find an ahistorical view a serious flaw. Rather, it is a necessary but temporary stage in the development of a historical science, and has the defects of its virtues. In the next phase, after the steady-state assumptions have been explored and then relaxed to allow for known or likely perturbations of the steady state, the new methodologies bring radically new insights into the nature of historical processes. Biogeochemical climatology has not yet transformed Holocene paleoecology, but Moore and Bellamy have brought the next phase a long step closer.

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