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 steadystate 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 method-

ologically 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.

EDWARD S. DEEVEY, JR. Florida State Museum, University of Florida, Gainesville

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RESEARCH NEWS

(Continued from page 1038)

such as the speed of light, the electronic charge, Avogadro's number, and others, metrologists do not rely only on direct measurements. Rather the "best" value of each of the constants is tied to measurements of other fundamental constants via a statistical procedure called a least squares adjustment. To see the effect that a more accurate value of Avogadro's number might have on the values assigned to the other constants was a major motivation for undertaking the project at NBS. However, each of the phases of the project has also yielded important results apart from the improvement in the knowledge of fundamental constants.

For example, the SRM 990 will be the publically available (for about \$50 a wafer, according to Barnes) silicon standard with a known atomic weight. In addition, techniques akin to those used in the silicon studies are being used both for the determination of the atomic weights of other elements with several naturally occurring isotopes, and in isotopic dilution methods for the quantitative determination of trace impurities in various substances. Similarly, an immediate outcome of the density studies is that the intermediate silicon blocks are now serving as "working" standards for density measurements throughout the United States. And, in a separate experiment, Deslattes and Henins were able to accurately determine the correction factor for converting kxu units to angstroms, by (in effect) reversing the usual x-ray diffraction procedure. Thus, by using silicon crystals of known lattice spacing as the diffraction medium, they measured x-ray wavelengths.

As for the possibility of redefining the kilogram, Deslattes pointed to several possible improvements (such as the use of isotopically pure silicon crystals) in the procedures necessary for finding Avogadro's number, which together hold the possibility of reducing the overall uncertainty by another factor of 100. However, because of the magnitude of the effort that would be required, such an "esthetic problem" likely would have a lower priority than other more pressing and practical problems.—ARTHUR L. ROBINSON

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