lish a lead (or a single) agency controlling all the funding from one pot and to prepare for coordination. Moreover, policy is made in many places, and asking for crashing initiatives in Congress is not always the best strategy. Even when medical prospects are straightforward, stepwise decisions may be easier to secure. Thus NIH got a program started within the bounds of existing missions. Yet, the increment was not envisioned. Expansion resulted in redefined goals like the short-term priority accorded small organisms.

Finally, the book shows that the making of scientific consensus was not isolated from the search for political support. Though letter-writing campaigns focusing on "big science" have attracted attention, Cook-Deegan recollects more substantial issues. For instance, the argument that, in 1987, opposed Watson and Ruth Kirschstein, then director of the National Institute of General Medical Sciences, was a matter of administrative structure (investigator-initiated grants versus concerted program) as well as an echo of practical and ideological rifts within the biomedical communities (individual gene hunting versus centralized data collection). The end of the conflict as well as the medical reshaping originated in the formation of a core set of biologists who assessed the program in various places, among them the National Research Council, the NIH Director's Advisory Committee, and the Delegation for Basic Biomedical Research. Accordingly, an implicit message of *Gene Wars* is the subtext underlying the comparisons of the HGP with the moon-shoot: what was at stake was the fate of an NIH–university nexus jeopardized by governmental emphasis on biotech companies and budget deficit.

The observations of an "insider" are enlightening but not always satisfying. Cook-Deegan's account of the role of OTA suggests that the office was instrumental in helping molecular biologists secure a large-scale initiative. Rather than assessment of the HGP, OTA was rapidly involved in its enhancement. This may explain hesitations in addressing the broader economic, and social issues that the HGP forces society to face. Cook-Deegan presents the program on ethical, legal, and social implications as a welcome addition to the HGP, but he clearly favors studies preparing for implementation and regulation of the applications of genetic knowledge. Unfortunately, the book suffers from a narrow definition of these uses. Though interesting, the patent issue is a minor one compared to changes in medical practice, if only because of the complex linkages among the politics of biological identity, attempts to reduce the cost of health care, and the increasing prospects for the diagnosis of predisposing factors. Pointing to the impossibility of a return of Nazi-like eugenicism in a democratic society or to the fallaciousness of the choice set up between genetic determinism and environmental determinism, although not mistaken, is of little help here. One may wonder if a scant interest in the problems regarding medical practice is not a price the author paid for the distinction between the HGP as political phenomenon and the HGP as science. The fate of PCR-based diagnostic techniques shows that, contrary to what Bernard Davis claimed before the U.S. Senate Committee on Energy and National Resources in 1990, biomedical research is not exactly "what we would be doing today if there were no human genome project." Five years later, these technological developments still are in need of good social studies.

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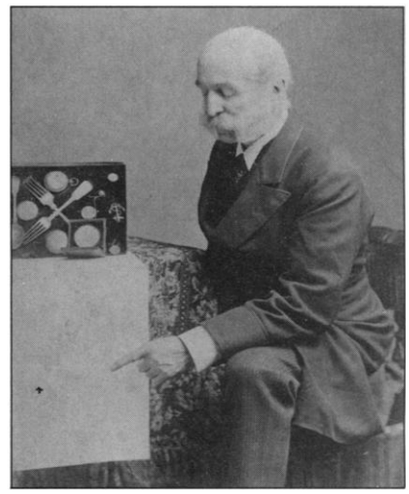
Fieldwork in the North

Science in the Subarctic. Trappers, Traders, and the Smithsonian Institution. DEBRA LINDSAY. Smithsonian Institution Press, Washington, DC, 1993. xviii, 176 pp. + plates. $34 or £26.50.

Science and the Canadian Arctic. A Century of Exploration, 1818–1918. TREVOR H. LEVERE. Cambridge University Press, New York, 1993. xiv, 438 pp., illus. $64.95 or £40.

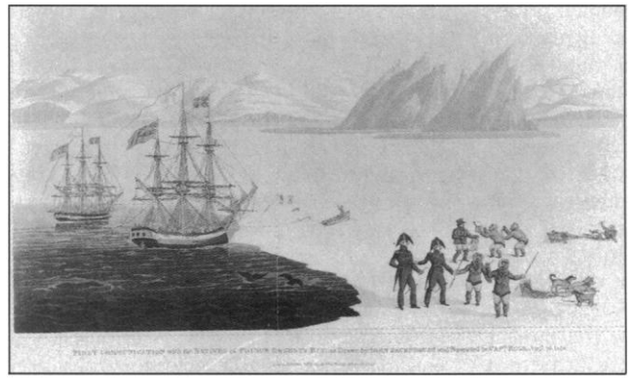
These are decidedly not trivial books, of provincial interest only to Canadians. They involve the transformation of field sciences such as botany and geophysics during the 19th and early 20th centuries into systematic sciences. They concern the importance of organization, abstraction, and politics in sciences often wrongly categorized as merely descriptive. They are about the genesis of the support mechanisms needed for science conducted in the polar regions. They should interest historians of science, but also scientists interested in polar work.



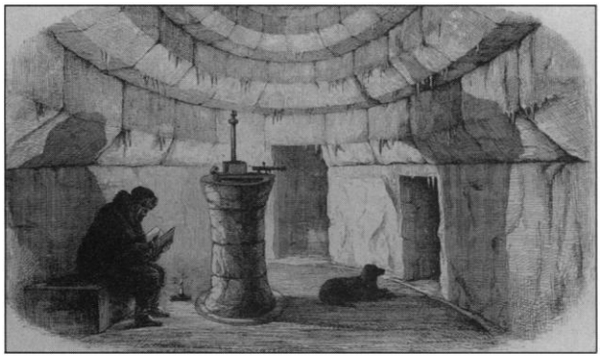
John Rae showing map and relics of the Franklin expedition after bringing them home from the Arctic. [From *Science and the Canadian Arctic*; Byrne & Co. Photographers, National Archives of Canada]

Debra Lindsay's book examines three interlocking case studies. First she focuses on the early Smithsonian Institution and Spencer Fullerton Baird's responsibility—first as assistant secretary and later as secretary of the institution—for building a network for systematic natural history collections in subarctic North America. Baird believed that the field data available in the 1850s were inadequate for developing or testing theory. He promoted a reformation of collecting to de-emphasize rare specimens in favor of larger samples. To settle questions about geographical distribution, he stressed representative sampling techniques. He developed an early biometrics.

Lindsay's other two case studies follow the working out of Baird's collection network through the actions of Robert Kennicott, now a rather obscure figure. Kennicott recruited native residents of the Mackenzie River region and factors of the Hudson's Bay Company to collect, ultimately, over 12,000 specimens for the Smithsonian. Later, he tried to replicate this success in Russian America (Alaska). The Mackenzie River project was a great success; the Alaskan failed.



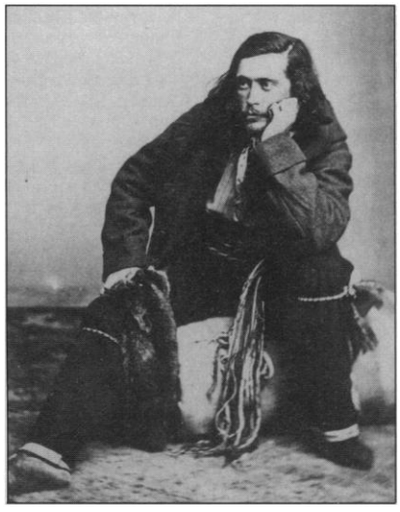
"The Royal Navy meets the Esquimaux," a drawing by John Backhouse published in John Ross's *Voyages*, 1819. [From *Science and the Canadian Arctic*; Metropolitan Toronto Library]



"Inside the Unifiler House," from E. L. Moss's *Shores of the Polar Sea* [From *Science and the Canadian Arctic*; Fisher Library, University of Toronto]

Although Lindsay's narrative is worth reading just for its detail of field practice and life as of about 1860, her conclusions make this book. One reason the Mackenzie River project succeeded was that the collectors profited from it: "specimens were a commodity." The collectors earned books, alcohol, and social status. The Russian-American venture failed for lack of the institutional support given freely by the Hudson's Bay Company. Moreover, the socioeconomic system of the Hudson's Bay Company, well established along the Mackenzie, had no counterpart in Alaska.

Trevor Levere's book exceeds Lindsay's in historical scope. The century he has chosen is the formative one for arctic science in the Western Hemisphere, from John Ross's 1818 expedition in search of the Northwest Passage to Vilhjalmur Stefansson's search for new arctic lands in 1914–1918. Levere's vision, however, stretches from Francis Bacon to the recent past. This preserves perspective when he descends into the detail of British Admiralty expeditions like those of John Franklin. In history it is essential to tell the story, providing the opulent complexity that comes from extensive reading in the primary printed sources and in the manuscripts. Levere does this. Some readers may revel in these narrative particulars; others may get lost in them. All readers should remember the overarching themes of the book at these times.



"Robert Kennicott (1835–1866), posing for a studio photograph after his return from the north." [From *Science in the Subarctic*]

One of these themes is the nature of a science dependent on fieldwork, expeditions, collecting networks, or observations extended over both time and space. Most sciences of the Earth are of this type. Although zoology, geology, glaciology, and geomagnetism have differentiating characteristics, they share a reliance on extensive databases that separates them from, say, classical physics. Levere examines this theme from many angles. Debates among historians of science on the nature of field science often center on "Baconian" or "Humboldtian" methodologies, their perceived qualities, advantages, limitations. Levere does not reify but instead recounts what Halley and Cook and others did. He repeatedly returns to what was done in hydrography, meteorology, and other "geophysical" sciences at different periods. Although it is anachronistic to speak of "geophysical" sciences for much of this period, it nevertheless provides a useful tool of historical analysis. These types of sciences require much more historical—and contemporary—attention.

The main theme unifying this book, however, is the relation of arctic science to arctic politics, specifically to questions of colonialism, sovereignty, and the national and international aspects of science. Science, as most historians now agree, cannot be separated from its social context. The prosecution of science in places requiring extensive logistical support is not possible without backing from big institutions, big money, or big government. These sociopolitical agendas, for better or worse, are part of science's story. This is certainly true of science in the Canadian Arctic. Nevertheless, as Levere states, science also has an "inner dynamic, directed through its institutions and applied through instruments and concepts to an uncompromising natural world" (p. 2). This dialectic also shapes this book.

Lindsay and Levere will edify you and engage you.

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A Technological Economy

Profits of Science. The American Marriage of Business and Technology. ROBERT TEITELMAN. BasicBooks, New York, 1994. xiv, 258 pp. $23.

Specialization of function has been changing society for many centuries, but the pace of this social transformation has quickened dramatically in the last hundred years. Specialists everyone (or so it seems), we all have our particular bodies of knowledge, our distinct forms of communication, our communities, status systems, and acknowledged leaders. So immense is the flow of information that few can expect to control the knowledge in more than one area of expertise, and indeed, we frequently find it necessary to narrow the definition of our special interest to keep any measure of control over the things we are supposed to know.

This relentless process of subdivision has created the need for intellectual brokers, for authors who can speak the languages of more than one community of specialists and help all of us understand developments beyond the boundaries of our expertise. Robert Teitelman, senior editor at *Institutional Investor*, is a very skillful intellectual broker. In *Profits of Science*, he blends ideas and literature from the history of technology, of business, of political economy, and of science to provide us with a synthetic interpretation of the "technological economy" of post–World War II America. He looks in particular at the television, transistor, computer, and pharmaceutical industries (drawing heavily in the last case on his earlier book *Gene Dreams: Wall Street, Academia, and the Rise of Biotechnology* [Basic Books, 1989]).

In each of his case studies, Teitelman describes the personalities of the leading entrepreneurs, the nature of their organizations, their relationships with the federal government, and the impact their industries or subindustries had on the U.S. economy. Joseph A. Schumpeter, the great theoretician of entrepreneurship, would be pleased to see how extensively this author has used his ideas. Schumpeter would not be entirely satisfied with this book's conclusions, if only because Teitelman is not as positive as he was about the long-term ability of capitalistic markets to clean out inefficient organizations, regardless of their size or degree of market power. But the father of entrepreneurial analysis would have to be happy with a book that applauds "creative destruction" and locates the sources of innovation in clever individuals, rather than groups.

As this suggests, Teitelman is not con-