Having spent two full years and made many visits in Copenhagen during the 1930s, I found the book most revealing, since Bohr did not tell much about his dealings with the sources of financial support to us young collaborators. Still, as an eyewitness of this period I feel that this book has the usual shortcomings of an account taken from written sources. On another occasion I wrote of Bohr "acting, talking, living as an equal in a group of young, optimistic, jocular, enthusiastic people, approaching the deepest riddles of nature with a spirit of attack, a spirit of freedom of conventional bonds . . . that can hardly be described." Here I miss a sense of that incredibly inspiring atmosphere at Copenhagen. For example, Aaserud does not mention at all the characteristic evenings of "comic physics" that took place at the yearly conferences.

I also think that the main title of the book is misleading. We did not feel at all that Bohr "redirected" science at Copenhagen. The shift that occurred in the period Aaserud is concerned with came as a natural development of fundamental physics, just as Bohr's interest in quantum electrodynamics around 1930 was a natural step forward from atomic quantum mechanics. That was no "redirection" either. True enough, Bohr undertook the natural expansion of quantum mechanics to nuclear problems later than it could have been done. His enthusiasm about the complementarity between the wave and the particle nature of the electron led him to believe that the riddles of nuclear structure and of biology should also be understood by broader complementarity relations. He thought that life phenomena were complementary to the laws of physics and chemistry, since any atomic analysis of life necessarily would destroy it, just as any attempt to localize an electron as particle destroys its wave properties. This idea turned out to be wrong, but it had decisive though indirect effects on life sciences that are not mentioned in the book; it brought Max Delbrück to switch from physics to biology, to become one of the founders of molecular biology. This influence is unconnected to the biological activities of Hevesy for which Rockefeller provided the funds.

Around 1930 Bohr had the strange idea of giving up the law of conservation of energy in order to resolve some of the nuclear problems. Pauli contemptuously attacked this as "the Copenhagen heresy." But it took only a few years for Bohr to recognize that the nucleus is an ordinary quantum mechanical system. No new complementarity was necessary for its understanding. Indeed, Bohr contributed much to this understanding—for example, the concept of compound nucleus and his analysis of fission with J. A. Wheeler. In my view these ideas and the experimental work of O. R. Frisch, H. Kopfermann, and others at Bohr's institute should not be considered a "redirection" of research but a logical continuation of the application of quantum mechanics to newly discovered phenomena.

Niels Bohr "takes a symbolic first step toward expanding the institute, 1935/36." [From *Redirecting Science*; Niels Bohr Archive, Copenhagen, courtesy of American Institute of Physics Niels Bohr Library]

George Hevesy, around 1935. [From *Redirecting Science*; Niels Bohr Archive, Copenhagen, courtesy of American Institute of Physics Niels Bohr Library]

The detailed accounts by Aaserud of Bohr's negotiations are a testimony to Bohr's uncanny ability to get what he wanted from the various foundations. We young collaborators admired his incredible ability to lead research and at the same time to provide the necessary funds—and, last but not least, to provide us Hitler refugees with jobs. Every year Bohr traveled to America and England to "sell his refugees."

Aaserud's book is an invaluable source of information and of documents that prove that Bohr was not only an inspiring physicist and philosopher but also a cunning negotiator who knew how to make use of his great reputation for the benefit of science.

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The High-Latitude Oceans

Polar Oceanography. Part A, Physical Science. WALKER O. SMITH, JR., Ed. Academic Press, San Diego, CA, 1990. xviii, 406 pp., illus., + plates. $69.50

Polar oceanography differs from the oceanography of lower latitudes in several significant respects. A fundamental physical difference is that the upper layers of the polar oceans are stratified by salinity rather than by temperature. The logistics of oceanographic measurement are also quite differ ent in the polar regions, where the climatic environment is harsh and oceanographic sampling generally requires the drilling of a hole through several meters of ice. Consequently, polar oceanographers are even more data-limited than their colleagues in lower latitudes. Polar oceanographers have heretofore been unable to turn to a book in which our current knowledge is synthesized. *Polar Oceanography* is a successful attempt to fill this void in the scientific literature, and it will be welcomed by researchers, instructors, and students of the high-latitude oceans. However, as Smith notes in his preface, our knowledge of polar oceanography is expanding so rapidly that such a synthesis might be considered premature. The contributors to *Polar Oceanography* responded to this concern by appropriately citing the knowledge gaps and research needs in their respective specialties.

Polar Oceanography is a two-volume compendium of contributions by different authors. Part A, under review here, includes chapters on physical oceanography of various scales and on the relevant meteorology, sea ice physics, remote sensing, and modeling. The surveys of the physical oceanographic topics are state-of-the-art. The contributors extensively cite the results of the more recent field programs, such as MIZEX (Marginal Ice Zone Experiment) and CEAREX (Co-ordinated Eastern Arctic Experiment), in providing especially informative reviews of mesoscale eddy generation (Muench's chapter), water masses and currents (Carmack), the structure of sea ice (Gow and Tucker), and surface roughness properties (McPhee, R. A. Brown). The book also contains comprehensive overviews of high-latitude remote sensing (Shuchman and Onstott) and ice-ocean modeling (Häkkinen).

The material is presented somewhat unevenly; the chapters devoted primarily to boundary-layer physics (Brown on meteorology, McPhee on small-scale processes in the ocean) will be followed only by planetary boundary layer specialists or by readers with strong backgrounds in turbulence theory. The key points nevertheless emerge; for example, McPhee emphasizes the fact that vertical gradients are concentrated across a very thin layer of the ice-ocean interface where molecular effects can dominate. Portions of the chapter on remote sensing also contain highly specialized material on radiative transfer. All three of these chapters contain many mathematical expressions that some readers will regard as having been "pulled out of the hat." On the other hand, Gow and Tucker's primer on sea ice and Häkkinen's survey of modeling will appeal to a wide-ranging audience. Both chapters should be required reading for global climate modelers who hope to treat sea ice processes realistically in simulations of climatic change. Carmack's chapter on large-scale physical oceanography is also a very readable summary of the limited available information on the subsurface waters of the polar oceans. The maintenance of the Arctic halocline emerges from this chapter as one of the key scientific problems in polar oceanography.

Some topics seem to have received surprisingly little attention. For example, radiative fluxes and cloud effects are primary determinants of the surface energy balance over polar oceans, yet these topics are covered in little more than a page of the chapter on meteorology. The biological importance of radiative penetration into the upper ocean also argues for greater coverage of this topic, especially since primary productivity in the Arctic Ocean is light-limited. Surface albedo, which is a key parameter for coupled atmosphere-ice-ocean models, also receives little attention. Interannual variability of the various oceanographic quantities is mentioned on only a few occasions; however, the scarcity of measurements of the subsurface ocean is clearly a contributing factor in this regard.

The various chapters do not seem to be as closely interwoven as they might have been. Separate but (in some respects) similar boundary-layer treatments are presented in chapters 1 and 6 for the atmosphere and ocean, respectively; overlapping surveys of the major ocean currents appear in the chapters authored by Carmack and Muench; and remote sensing is given a section in the chapter on meteorology as well as an entire chapter of its own. On the other hand, there is little bridging of the gap between the rather esoteric boundary-layer formulations in earlier chapters and the rather crude parameterizations of air and water stresses in Häkkinen's survey of large-scale models.

I was intrigued by Häkkinen's comment in the final paragraph of the volume that we are still unable to give a satisfactory answer to the simple question of why there is a stable ice cover in the Arctic. This volume may well stimulate progress toward the answer to such a fundamental question that is at the heart of polar oceanography.

The companion volume (part B, $65) covers chemistry, biology, and geology in six papers, one each on chemical oceanography and sedimentation and the remainder dealing with the planktonic and benthic biota and food webs.

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Trophic Topology

Community Food Webs. Data and Theory. Joel E. Cohen, Frédéric Briand, and Charles M. Newman, with a contribution by Zbigniew J. Palka. Springer-Verlag, New York, 1990. xii, 308 pp., illus. $79. Biomathematics, vol. 20.

This book opens with the statement that "a central problem in biology is to devise helpful concepts (e.g., genes) and tested quantitative models (e.g., Mendel's laws) to describe, explain and predict biological variation." Thus the apparent aim of the book is to demonstrate that the structure of food webs exhibits the kinds of generality we associate with genetics. Does it do so?

There are several critical postulates for the theory that arise from the authors' examination of 113 food webs given in the literature. First, predation is the only link between species (cannibalism, parasitism, and fluxes through detritus are ignored). Second, there are no food cycles (A eats B eats C eats A is not permitted). Third, any species (more strictly a "trophic" group of species) has only a few predators and prey—an average of two of each. This last postulate, based on the food web data, is central, since it states that complexity at the individual level is independent of the complexity of the whole web. This is quite distinct from the expectation from randomly assembled webs, where the number of links from any node (species) would be proportional to the total number of species.

These results build on the work of others such as Stuart Pimm and Robert May. They are used as the basis for a "cascade" model (a random acyclic digraph), which takes the form of a strictly upper-diagonal matrix the probability of whose elements' being 1 (rather than 0) is inversely proportional to the number of species. Using this model, the authors deduce the proportions of basal, intermediate, and top species in any web; the average and variance in the lengths of food chains; and other characteristics. These deductions are compared with observations from the 113 webs.

There are several criticisms that have been made of the authors' approach: the results are tautological; the regressions are unconvincing; the relations portray the inadequacies in the data; and so on. These detailed points are discussed in the book, and the reader must judge. The more general criticism would be that this form of analysis is not relevant to the "real" problems of ecology such as energy flow, size structure, stability, or patch dynamics. Does this topological approach illuminate other views about ecological systems?

I return to the major empirical conclu-