BOOK REVIEWS

The Swedish Scene

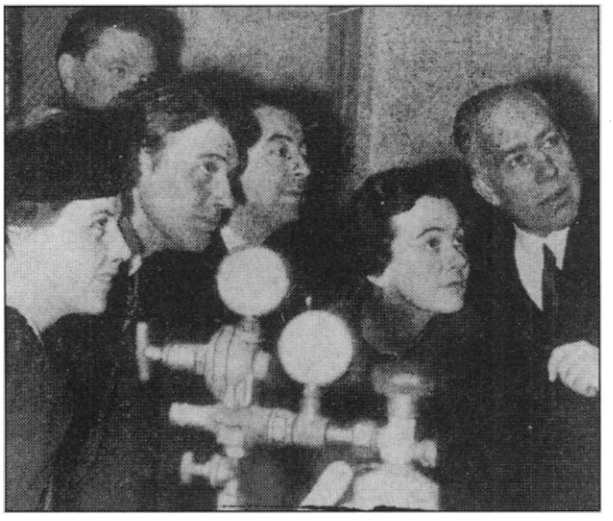
Center on the Periphery. Historical Aspects of 20th-Century Swedish Physics. SVANTE LINDQVIST, Ed. MARIKA HEDIN, and THOMAS KAISERFELD, Assoc. Eds. Science History Publications/U.S.A. (Watson), Canton, MA, 1993. lvi, 516 pp., illus. $49.95. Uppsala Studies in History of Science, vol. 17.

When the British philosopher John Theodore Merz in the early 1900s surveyed European scientific thought in the 19th century—which he termed the "scientific" century—he distinguished among the "scientific spirits" of individual nations. More recently, the American sociologist Edward Shils's center–periphery duality, according to which "societies have a center to which their members orient themselves and which influences their conduct in a wide variety of ways," has been applied in delineating scientific developments. Citing Shils, the editor of the book under review combines these two approaches, construing the place of Sweden in the world of science as that of a "center on the periphery."

This volume, embodying specific national traits at least as much as does the pursuit of science itself, provides impressive evidence of the continued increase in activity in the history of science in Sweden. Even when limiting the topic to Swedish history of physics in this century, Lindqvist (with two associate editors) has been able to enlist as many as 16 Swedish scholars as contributors. In addition, three Americans and one Dutchman contribute papers, and the librarian at Lindqvist's institution, the Stockholm Royal Institute of Technology, has compiled an extensive, if not comprehensive, bibliography of the subject.

After Lindqvist's introductory essay, which includes a lively excursion on the Swedish author Harry Martinson's 1941 meeting with Niels Bohr and Martinson's subsequent attitudes toward the significance for humanity of the discovery of nuclear fission, the book is divided into four parts. It is consistent with the tradition of Swedish scholarship in the history of science, which has tended to be dominated by the perspective of "history of ideas and learning," that neither Lindqvist's contribution nor any of the following eight essays making up the book's first two parts touches on the theory and practice of physics as such. I find part 1—Cultural and Social Histories of Science—the least satisfactory in the book; although Suzanne Gieser in particular introduces interesting material on the relationship between the respective traditions of Swedish philosophy and physics, the contributions on the whole tend to presume rather than document such a relationship.

Pointing to the second half of the previous century as the "golden age" of Swedish polar research, Urban Wråkberg, in the first paper of part 2—The Institutions and Politics of Big Science—asks why this area of research has declined in Sweden in our century. Among several factors Wråkberg stresses in particular that the large industrialized nations have gained from polar research's becoming more routine and heavily organized (becoming, that is, more like Big Science) and that Norway's independence from Sweden in 1905 placed Svalbard, a major location for Swedish polar research, under the Norwegian flag. In the second paper Sven Widmalm deals with a topic more commonly associated with the term "Big Science": the question of whether Sweden should participate in "CERN II," the building of a Super Proton Synchrotron under the auspices of CERN. Considering the intellectual and experimental activity of high-energy physics as transnational, Widmalm argues that at the contextual level, especially with respect to the political decision-making process, its development does allow a national approach. He thus presents a case study of how, from the first proposal in 1963 to the final decision in 1971, the Swedish high-energy physics community was able to turn the tide in its favor, in relation both to the Swedish government and to other fields of Swedish physics with which it competed for funds. According to Widmalm, this success owed itself to what he calls the "hierarchical" ideology, namely the view that high-energy physics constitutes the most fundamental of a whole hierarchy of scientific pursuits. In contrast to Wråkberg's and Widmalm's meticulously researched case studies, Thorsten Nybom's essay rather takes the form of a partial contribution to the national debate about who is responsible for the supposedly dysfunctional Swedish system of science councils. In short, the essays of part 2 take on a wide variety of topics and approaches, and it can be questioned whether Wråkberg's and Nybom's essays belong to the history of physics at all.



Literary figures from Sweden visiting the laboratory of Niels Bohr (extreme right) in Copenhagen, as shown on the front page of the Danish daily *Politiken*, 19 January 1941. The visitors from neutral Sweden had been invited to German-occupied Denmark as part of a "Swedish Week" intended to assert the nations' cultural solidarity. During the week "the Swedish authors gave readings of poetry and prose to eager Danish audiences." When the poet Harry Martinson (fourth from right) returned home he told a friend, "Oh yes, now I know about the atom. The atomic nucleus is like a great wind-whipped oak tree, and the electrons circle it like black crows." [From S. Lindqvist's introduction to *Center on the Periphery*; Royal Library, Stockholm]

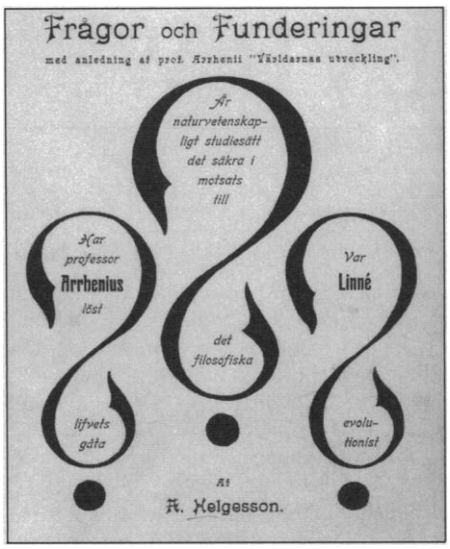
In contrast to the first half of the book, the last two parts do deal with the theory and practice of physics, mostly from a biographical perspective. Three of the five authors in part 3—International Networks of Communication—describe Swedish physicists from the illuminating combined perspective of their recognition at home and abroad. The earliest case, presented by Paul C. Hamilton, is that of Janne Rydberg, establisher of the Rydberg spectral series of the chemical elements, which was to become crucial for Bohr's formulation of his atomic theory in 1913. Born in 1854, Rydberg had published this work by the time he sought the vacant physics professorship at the University of Lund in 1897. After a drawn-out battle Rydberg was refused the post on the grounds that his results were based on the experiments of others. Rydberg's successful appeal to the foremost foreign physicists in the field was no doubt

instrumental in his appointment in 1901 as extraordinary professor at the same university.

The next physicist dealt with, David Enskog, whose biography is laid out by Mats Fridlund, was born 30 years after Rydberg. Having successfully defended his experimental licentiate thesis in 1911 at the University of Uppsala, he decided to turn to theoretical physics while working as a high school teacher. In 1917 Enskog was the first student to defend his doctoral thesis under the tutelage of Uppsala physicist Carl Wilhelm Oseen, in which work he solved Ludwig Boltzmann's integrodifferential equation for the velocity distribution of gas molecules. Enskog thus unraveled "what had been one of the central problems of the kinetic theory of gases for over 40 years," subsequently obtaining international fame for this work, which has come to be known as the Chapman-Enskog theory. At the time, however, Oseen found Enskog's thesis lacking in mathematical stringency and gave him such a low grade that he had practically no chances for a continued university career, whereupon Enskog accepted an appointment as senior master in a provincial state grammar school. Only when his British colleague Sydney Chapman intervened on his behalf in 1930 was Enskog secured a professorship at the Royal Institute of Technology.

Another generation later, Bengt Edlén (1906–1993) studied in Uppsala with the renowned x-ray spectroscopist Manne Siegbahn, who in turn had served as Rydberg's assistant in Lund. Thus trained in the spectroscopy of the extreme ultraviolet, Edlén's prospects for a spectacular career seemed poor until he in 1941 applied his expertise to establishing the source of the coronal emission lines from the sun, which had baffled theorists ever since these lines began to be experimentally established more than 60 years earlier. This work arose to a great extent from Edlén's close contacts with and encouragement from the international physics community. The American historian Karl Hufbauer concludes that it was the international attention to Edlén's work that in the end clinched his 1943 appointment to a professorship in Lund.

The contributions of Hamilton, Fridlund, and Hufbauer, then, all represent cases of the tension between the Swedes' own criteria for evaluating work in physics and their high esteem for international judgment, which tended to be based on different criteria. The University of Uppsala in particular carried a tradition of emphasis on experimental precision, which Hamilton explains in terms of Uppsala's philosophical tradition, represented by



" 'Some Questions Caused by Prof. Arrhenius's "The Development of Worlds." Has Professor Arrhenius Solved the Mystery of Life? Is the Scientific Method Certain as Opposed to the Philosophical Method? Was Linnaeus an Evolutionist?' In 1907, the clergyman Anders Helgesson published this strongly anti-Darwinian, religiously conservative reply to [Svante] Arrhenius's own version of Darwinism." [From O. Amelin's paper in *Center on the Periphery*; Royal Library, Stockholm]

Christopher Jacob Boström (1797–1866). Just as I remarked with regard to part 1, the relationship between philosophy and physics requires more serious scholarship. In contrast to those of the three authors just mentioned, Thomas Kaiserfeld's contribution does not aim to place Swedish physics in its national context. Basing his investigation on the rather limited documentation constituted by the correspondence between Munich theoretical physicist Arnold Sommerfeld and Siegbahn—particularly from 1917 to 1925—Kaiserfeld infers instead that the work of these two was mutually beneficial and concludes that the Lund/Uppsala–Munich connection constituted an "invisible department" of physics.

The essays of part 4—Science as Applied Technology—seek to understand Swedish science from the perspective of the history of technology. Thus, the Dutchman Boelie Elzen argues, taking the currently in vogue SCOT (sociological construction of technology) approach, that the development of the ultracentrifuge must be construed in relation to a variety of technical needs and not as a linear development from the first such device developed by Uppsala physical chemist Theodor (or "The") Svedberg in the early 1920s. Likewise, Mikael Hård, contrasting his work to David Edge and Michael Mulkay's description of the emergence of British radio astronomy, seeks to explain the building of the Onsala observatory outside Gothenburg by the entrepreneurial engineer Olof Rydbeck as a case of "technological drift" and subsequent research there as motivated by questions of engineering rather than of astronomy. American historian Lily E. Kay's discussion of the electrophoresis apparatus developed in the 1930s by Svedberg's former doctoral student Arne Tiselius returns attention to the motivations and activities of science as such. Locating the "Tiselius apparatus" within the "protein network of the Rockefeller Institute, the University of Uppsala, and a handful of prestigious laboratories at Columbia, Harvard, Yale, Cornell, and Wisconsin," Kay considers Tiselius's Uppsala facility up to the late 1940s as an elitist institution among several others that were able to secure the enormous—and expensive—device only by generous support from the Rockefeller Foundation. Instead of placing Tiselius in Swedish science, then, Kay locates him in an "invisible department" of biochemistry on a substantially larger scale than that proposed by Kaiserfeld. As in part 3, the individuals dealt with in part 4 are too few to make up a full picture, especially as the essays only touch on the activity of physics as commonly understood. Besides, as Lindqvist himself points out in his introductory essay, the biographical approach has its own limitations.

By treating the Swedish activity as an equal partner in a broader international effort, Kaiserfeld and Kay question implicitly the center–periphery duality and echo Swedish historian of science Elisabeth Crawford's insistence that national approaches should instead be treated as complementary to one another. As a native of Norway working in Denmark, I cannot help pointing out more generally that the book would have profited from a more comprehensive inclusion of work in other nations with which Swedish physics interacted, not only such genuine "centers" as Germany and the United States but especially the other Scandinavian countries. To remain with the metaphor of the book's title, the periphery certainly comprises not one but several interconnected centers.

As Lindqvist himself makes clear in his introductory essay, the book's intention is not "to give a full account of the history of Swedish physics in the twentieth century [but to] assemble in one volume . . . as many of the historians working in the field as possible." Nevertheless, an international readership with limited knowledge of the Swedish scene would have profited from at least a cursory guide to Swedish physics institutions, professors, activities, and achievements—not to mention a subject index and translations of the titles of publications in Swedish listed in the bibliogra-

phy and notes. I find it amusing that the section (by far the largest) of the bibliography listing biographies includes full coverage of Norwegian Vilhelm Bjerknes, Hungarian George de Hevesy, and Austrian Lise Meitner, the last of whom, incidentally, receives the largest number of entries of all. Since all three of these figures spent parts of their working lives in Sweden, I take their inclusion to reflect an effort at completeness rather than an instance of Swedish cultural imperialism.

Although the individual essays for the most part are both illuminating and well written, as a presentation of history of Swedish physics in the 20th century the book is uneven and contains glaring omissions. It could, for instance, have drawn more effectively on the substantial scholarship in the history of the Nobel Prize institution, and the emphasis is on experimentalists at the expense of theorists. The coverage is further diluted by the stretching of the definition of physics. At the same time, the volume documents impressively the present vigor and potential of this field (not least given the overall youth of the Swedish contributors) and gives reason to hope that a more synthetic historical presentation of Swedish physics is within sight. This impression is strengthened by the fact that instead of presenting a framework for the specific contributions Lindqvist in his introductory essay sets an agenda for future scholarship in the history of Swedish physics. One can also hope that this spreading of the Swedish gospel—sponsored by the Swedish Council for Research in the Humanities and Social Sciences and by the Swedish Science Research Council—will serve to encourage agencies of other small countries to support similar enterprises, which, taken together, in turn may prepare for an improved national as well as international approach to the history of modern physics.

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Psychology via Physiology

The Neuroscience of Animal Intelligence. From the Seahare to the Seahorse. EUAN M. MACPHAIL. Columbia University Press, New York, 1993. xxviii, 506 pp., illus. $45 or £35. Animal Intelligence.

In 1904 the *Journal of Comparative Neurology* became the *Journal of Comparative Neurology and Psychology* and a psychologist, Robert M. Yerkes, joined the editorial board. "It is our aim," the new editor announced bravely in the first issue, "to bring together anatomical, physiological, and psychological facts in such a manner that their relations may appear. Thus, it is hoped, the specialists in structural works will be impressed by the importance of the functions of the organs which they study, while at the same time those whose chief concern is animal behavior will see more clearly that they cannot work to advantage until they know *what* is functioning." The collusion was short-lived. By 1911 the *Journal of Comparative Neurology* was itself again, and the *Journal of Animal Behavior* had been launched. The change was attributed to an increasing volume of behavioral research, but it was clear that separate paths were to be taken. The prevailing view among psychologists was that neurologists had much to learn from them but nothing to teach them. Psychological facts are no more accessible to physical and chemical analysis than to deep-sea soundings, one authority declared, and another said that as far as he was concerned the head could be filled with cotton wool.

Euan Macphail's contrary view is that the definition of cognitive functions is one of the main goals of behavioral neuroscience, which he thinks of as "doing psychology with the aid of physiological techniques" (p. 25). He is ever cognizant of that goal in this industrious review of a vast contemporary literature on sensitization, habituation, associative learning, and short-term memory in favorite vertebrate and invertebrate species, the contributions of which to psychological theory he is led, unfortunately, to overstate. It certainly is not to work on the hippocampus, for example, that we owe the distinction between place and response learning; nor to work on the cerebellum an appreciation of the limitations of the stimulus-substitution account of classical conditioning; nor to work with vertebrate spinal preparations the suspicion that classical and instrumental conditioning can be understood without reference to "high-level motivational and cognitive capacities" (p. 155).

Aplysia (the seahare of the subtitle) and *Hermissenda* (another gastropod mollusk) figure prominently in two chapters on associative learning in "simple systems." Because of its restricted sensory and motor properties, *Aplysia* is hardly an ideal subject for behavioral research on learning, and *Hermissenda* much less so. Macphail suggests that the behavioral changes induced in *Hermissenda* by pairings of light and rotation may not be typical of conditioning in gastropods or even in *Hermissenda* itself, and that Pavlovian experiments have not demonstrated anything more than "the facilitation of preexisting excitatory connections" in either animal (p. 113). A curious claim is that work on punishment shows *Aplysia* to be "capable of learning in tasks that may involve instrumental conditioning and that do not [merely] involve strengthening of a withdrawal response" (p. 129); although punishment is operationally instrumental, it can be understood in the same way as aversive classical conditioning. An equally curious claim is that contingency (as opposed to contiguity) is important for conditioning in *Aplysia*—that the animals "somehow assess the probability of a UCS [unconditioned stimulus] in the presence of the CS [conditioned stimulus] relative to its probability in the absence of the CS" (p. 132); only someone who still holds to the discredited contingency theory of vertebrate conditioning, unaware of the deficiencies of the experiments that gave

Vignettes: Discrimination

You probably think it's easy being a bioenergeticist, spending day after day interconverting kilocalories and kilojoules and trying to think about the reaction changes of entropy along metabolic pathways. Well, I don't want to complain, but those of us working in the thermodynamic foundations of biology don't get a lot of respect.

—*Harold J. Morowitz*, in *Entropy and the Magic Flute* (Oxford University Press)

Atoms are nice, atoms are fundamental, but they're not chemistry. Chemistry is about molecules, the fixed but transformable way in which atoms get together for a while.

—*Roald Hoffmann and Vivian Torrence*, in *Chemistry Imagined: Reflections on Science* (Smithsonian Institution Press)