less serious than at least two other Russian crises of this century, when there was much greater reason to worry about the fate of science. Historical experience can support only a few very general predictions: that the more centralized the future Russian political system is, the more it will tend to preserve the Academy as the leading scientific institution, and that a more decentralized and democratic Russia will probably give more preference to the universities. The state remains almost the only source of support for science. Alternative sources can emerge only gradually, along with the development of the civil society.

Alexei Kojevnikov
Institute of the History of Science and Technology,
Moscow 103012, Russia

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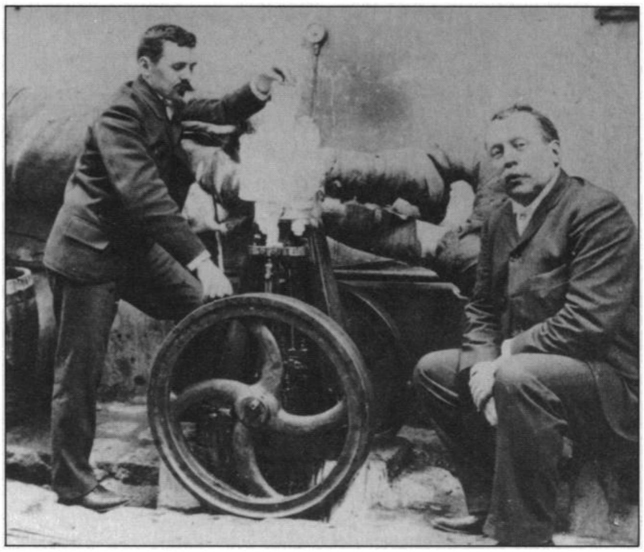
The Physics of Cold

History and Origins of Cryogenics. RALPH G. SCURLOCK, Ed. Clarendon (Oxford University Press), New York, 1992. xxiv, 653 pp., illus. $165 or £95. Monographs on Cryogenics.

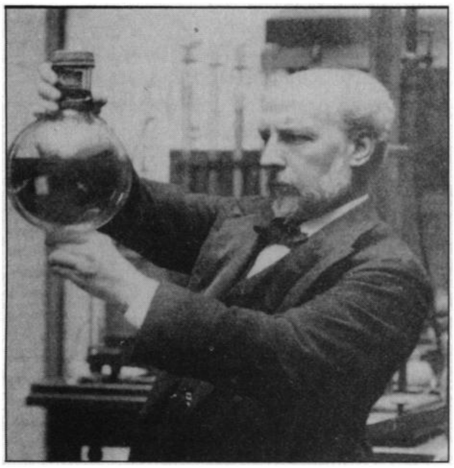
Superconductivity. Its Historical Roots and Development from Mercury to the Ceramic Oxides. PER FRIDTJOF DAHL. American Institute of Physics, New York, 1992. xiv, 406 pp., illus. $55.

Cold has been a rather unappealing state of affairs in our Western culture. Nevertheless, many people have been intrigued by it. From the time some poor folk brought ice from the mountains wrapped in dried grass so that the Roman emperors could enjoy their wine chilled in the summer to our days of high-temperature superconductivity, its wonders have been publicly displayed and its mysteries privately pursued. The history of the subject that the master of experimenters, Robert Boyle, found "the most difficult" to work at has been studied very little, and any such attempt is very welcome. So are these two books, though they also represent missed opportunities.



"Georges Claude (on left) and his first air liquefier," around 1902. With improvements, the French apparatus was able to achieve by 1906 a specific production of 0.52 liter per kilowatt-hour, exceeding those of Hampson in Great Britain and Linde in Germany. [From F. M. Dennery's chapter in *History and Origins of Cryogenics*]

Works in the history of the physical sciences should, by definition, be works of *history*. Simplified presentations of abstruse subjects or chronological narratives of published papers do not necessarily qualify. These of course may be very useful undertakings, but truly historical works have a different aim: to pose and trace out answers to questions about how things happened in an interpretative framework that also poses *whys*. This rather pedantic and perhaps self-indulgent observation is set forth to underline my ambivalence about these two books. What there is in the books is interesting—and in some parts the account of developments is quite thorough. But both books leave out so much significant material related to the story they purport to tell that they are seriously incomplete as chronicles, nor do they present any overall argument to support their selectivity.



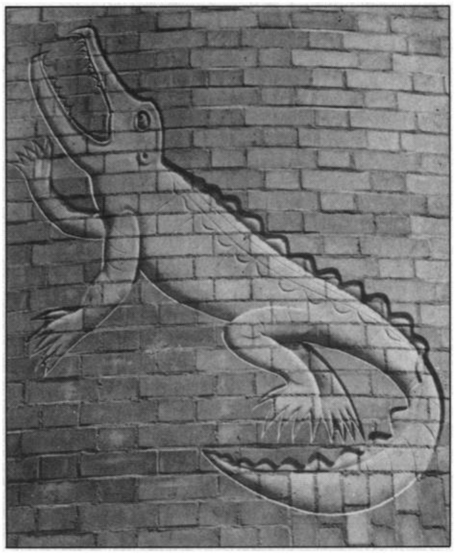
James Dewar and the vacuum flask he invented in 1892. "Sir James Dewar/Is a better man than you are/None of you asses/Can liquefy gases." [Quotation from D. Shoenberg's chapter in *History and Origins of Cryogenics*; picture courtesy AIP Emilio Segrè Visual Archives, W. F. Meggars Collection]

The compilation edited by Scurlock is an attempt to present the development of cryogenics in universities and industry in various countries of Europe and Asia and in the United States during the past hundred years—an awesome undertaking indeed. The amount of information presented by the 20 contributors is staggering, but amid all this information it is impossible to discern the story line, and one wonders what argument the information supports, what narrative it helps to unfold. Dates, names, details of machines, experimental results are all there and—remarkably for any book—with no apparent mistakes. Most of the papers about the development of cryogenics in the various universities and countries, with the exception of the three about the United States, do not add anything new to what has already been written on their subjects. Most of the papers about particular companies resemble leaflets and technical reports circulated by the companies themselves. No archival material from the companies has been used in writing their "histories," there are no interviews with the key persons, and, most important, there is no presentation of the economic and social issues relevant to the establishment and running of such companies. The development of home refrigerators, for example, with its lasting effects on the everyday habits of millions of people, is thoroughly ignored.

In the preface Scurlock characterizes the volume as "a fairly comprehensive coverage of all the developments in cryogenics since 1877" apart from "a few gaps, notably the former USSR." I do not think it is unfair to say that this is like writing the history of electromagnetism or quantum mechanics without considering Clerk Maxwell or Schrödinger. The work in the former Soviet Union was of such importance in the development of low-temperature techniques, the discovery of new phenomena, and the formulation of theories that I do not think a proper assessment of the history of cryogenics can be made without discussion of it. Almost all the contributions of the Soviet scientists in low-temperature physics were made during the Second World War, and many people not caught in the whirlpool of the Lysenko case came to regard cryogenics as the paradigmatic case of Soviet science. Scientists in the Soviet Union were proud that they had become pioneers in a difficult area with no external help during the most perilous period of their recent history. In this light it is important to understand the team culture and the collaborative efforts, especially in Moscow, the workings of the Party and the relative autonomy with respect to the Party decisions accorded to some people like Piotr Kapitza, the different views on "big science" held at the time in the Soviet Union, and relations with industry and the army. But this is not the only gap. Canada, for example, is also absent, and this is another serious omission, since the University of Toronto was the first place outside Leiden to achieve (in 1923) liquefaction of helium and functioned for 10 years as a cryogenic laboratory before Oxford, Cambridge, Munich, Breslau, and Moscow came on the scene. Canada and John McLennan deserve a close examination also because of the possibilities provided for large-scale helium extraction from the natural gas reserves and the potential of all this in military use for the British Empire in the years after World War I.

After H. Kamerlingh Onnes discovered superconductivity in 1911, the conventional wisdom among physicists—including Einstein and Bohr—was that the magnetic properties of superconductors could be determined by the Maxwell equations. It was reasoned that their magnetic behavior depended on whether the conductor was first placed in an external magnetic field and the temperature then lowered to make the sample superconducting or whether the external magnetic field was switched on after a conductor became a superconductor. Since in one case the field was trapped in the superconductor and in the other case it could not penetrate it, the phenomenon was considered irreversible and hence it was thought impossible to use thermodynamic methods for a theoretical understanding of it. And though quantum mechanical considerations were very successful in accounting for "normal" electric conductivity, an argument was put forth by Felix Bloch to the effect that there could be no theory of superconductivity. In 1933 Meissner and Ochsenfeld in Berlin discovered that superconductors—in defiance of what was expected of them on the basis of extrapolation of the Maxwell equations—were diamagnetic. This discovery changed the whole outlook on superconductivity, allowing the use of thermodynamic methods and, more significantly, changing the focus of the problem to be solved. The attempts were concentrated on understanding the magnetic behavior of superconductors rather than infinite electrical conductivity. Thus superconductivity, 22 years after its discovery and as a result of the newly found effect, began to be theoretically understood. Meissner's experiments are beautifully discussed in Dahl's book. Dahl is the first to use Meissner's archive, which includes his laboratory notebooks and correspondence, and this material makes for a fascinating presentation. Both the original experiments by Kamerlingh Onnes that led to the discovery of superconductivity and the series of experiments by De Haas at Leiden are thoroughly treated by Dahl. De Haas's hesitancy, so characteristic of Leiden's physics, to pursue Laue's suggestions and in a way anticipate the discovery of the Meissner effect is also comprehensively presented. Dahl is at his best when he deals with the experiments themselves, though his frequent use of graphs reprinted directly from the published papers does not always have the desired effect.

The last chapters of the book give the impression of having been hastily written to include the "latest developments," one of them being devoted to superconducting technology at the particle accelerators and the other to high-temperature superconductivity. Here the book is at its weakest. Adequate consideration of virtually any science and technology after the 1960s requires dealing with questions of international rivalries, government policies, academic politics, patents, co-authorships, Nobel Prizes, and the like, and these chapters do not even attempt to delineate the issues around which such a narrative would have to unfold.



Bas-relief of a crocodile carved by Eric Gill on brickwork at the entrance of the Royal Society Mond Laboratory in Cambridge, England. "Crocodile" was Peter Kapitza's nickname for Rutherford. [From D. Shoenberg's chapter in *History and Origins of Cryogenics*]

Vignettes: Questions of Standards

In the field of American higher education, prescriptive dicta concerning academic conduct seldom exhibit much staying power. Some are meant to be ephemeral, to answer a transient public criticism with an immediately placating response. Some are designed for longer duty but are quickly reduced by the swift pace of events to a remnant of a changing *geist* and then to a relic of a forgotten past. Especially short is the life expectancy of pronouncements on the proper scope of academic freedom and the proper terms of employment of faculty personnel.

—*Walter P. Metzger*, in *Freedom and Tenure in the Academy* (William W. Van Alstyne, Ed.; Duke University Press)

College professors' relative freedom to select and shape course material does not mean that textbook censorship does not affect college education. When students have spent twelve years reading books based more on market forces than on scholarly excellence, they may not come to college prepared to do college-level work. The increasing use of short sentences and simple words—often called "dumbing down"—in elementary and secondary school textbooks has generated a great deal of print since the mid-1970s, but the watering down of ideas is at least equally dangerous.

—*Joan DelFattore*, in *What Johnny Shouldn't Read: Textbook Censorship in America* (Yale University Press)

The integrity of academic intellect is not endangered by competing discourses of social inquiry. The risk now is entirely the opposite. Academe is threatened by the twin dangers of fossilization and scholasticism (of three types: tedium, high tech, and radical chic).

—*Thomas Bender*, in *Intellect and Public Life: Essays on the Social History of Academic Intellectuals in the United States* (Johns Hopkins University Press)

To understand the development of superconductivity one cannot, moreover, limit the focus almost exclusively to the experimental developments, as is done in this book. The liquefaction of helium and Leiden's "physics culture" cannot be properly assessed unless seen as part of Kamerlingh Onnes's theoretical agenda relating to the possible phenomenological ways of extending van der Waals's theory. Neither *The General Theory of Fluids*, written in 1881, nor *The Equation of State in Series*, written in 1901, is discussed. The Bardeen-Cooper-Schrieffer theory of 1957 is treated in less than a page with superficial details such as the date they mailed their manuscript. The Londons' theory is presented in a more detailed manner, but nowhere does one find a discussion of the notion of "macroscopic" quantum phenomena. The approach is mentioned a couple of times and at one point is attributed to both Fritz and Heinz. Yet in the only joint paper the two wrote there is no intimation of such a view about superconductivity. The first such suggestion was made by Fritz London in 1935 during the meeting of the Royal Society of London on low-temperature phenomena and was fully developed by him in his *Nouvelles Conceptions de la Supraconductibilité* in 1936. Fritz London's idea was that both superconductivity and superfluidity could be regarded as condensations in momentum space and therefore could be regarded as having macroscopic wave functions. The idea of accommodating the possibility of macroscopic quantum phenomena within the framework of quantum mechanics has been one of the most incisive contributions to the development of both quantum mechanics and low-temperature physics. The "macro" attitude had a long period of gestation and a difficult time getting accepted. These ideas have, in fact, their origin in Fritz London's doctoral dissertation—with Alexander Pfander, one of the founders of phenomenology—as well as in his early work in quantum theory, and specifically transformation theory. And, as is evident from his correspondence with Fritz London, these ideas had a decisive effect on the development of John Bardeen's own ideas about superconductivity.

Dahl's book has quite a few inaccuracies. "Onnes" is properly referred to as Kamerlingh Onnes. He was not, as stated, present at the Chicago Conference of 1913, having been prevented from attending by ill health, even though from the published papers it appears that he was there (in fact, Kamerlingh Onnes never visited the United States). Uhlenbeck's reminiscences about Kamerlingh Onnes's snobbishness toward the Maxwell equations were related to Max Dresden and not to Kramers. It is not the case that in the same issue of *Nature* in 1933 in which Lindemann announced the liquefaction of helium at Oxford Kapitza announced the liquefaction of helium at Cambridge. What was announced from Cambridge was the official opening of the Mond Laboratory, and helium was liquefied there about a year later, in 1934.

Despite their deficiencies, these two books remain welcome additions to the growing literature about the history of low-temperature physics in bringing together a wealth of information about its various aspects.

Kostas Gavroglu
Department of Physics,
National Technical University of Athens,
GR-157 80 Athens, Greece

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