

the last twenty-five years it has achieved great prominence as a crumb emulsifier in bakery (as calcium stearoyl-2-lactylate). Traditionally it has been made by a variety of fermentation processes, usually leading to the S(+)-lactic acid. Since the Second World War these have been supplemented by petrochemical-based synthesis (as by the addition of hydrogen cyanide to acetaldehyde to produce lactonitrile). Such syntheses of course yield the racemic mixture of acids, an unacceptable ingredient for edible products. As the tale unfolds it raises all manner of issues currently important in the history of technology: trustworthiness of food additives, pollution, quality control, changing raw materials, the impact of war and so on. Familiar to the business historian will be EEC export refunds, anti-trust legislation, cartels, dumping, and some quite bizarre tax anomalies (British import regulations charged 23 per cent duty on one isomer but not the racemic mixture!).

All these and many other themes are encountered in an account that is, at times, infuriatingly discursive and punctuated by a series of technical *Intermezzi* whose rationale is sometimes hard to discern. Historians will be put on their guard by persistent misspellings such as 'Gillespie' and 'Kékulé', by remarks of dubious accuracy about chemistry before 1850, and by several howlers for which the prize must go to the assertion that 'In 1911 Avogadro introduced the word molecule' (p. 63). Nor will chemists be too happy with reductions of Cr(VI) to 'chromium trioxide' (p. 118), with structures of maltose and sucrose interchanged (pp. 275, 278), with a confused account of enantiomerism, and with a persistent refusal to use modern nomenclature for stereoisomerism or conformational structures for disaccharides.

Nevertheless for all its faults the book is likely to be of considerable value to many historians of chemistry, pure and applied. It offers a case-history, or rather a series of case-histories, to illustrate major issues of historiography and will massively provide useful source-material for a long time ahead.

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FINN AASERUD. *Redirecting Science: Niels Bohr, Philanthropy and the Rise of Nuclear Physics*. Cambridge: Cambridge University Press, 1990. Pp. xiii + 356. ISBN 0-521-35366-1. £35.00, \$47.50.

This book describes and explores changes in the foci of research of Niels Bohr's Institute for Theoretical Physics in Copenhagen from the mid-1920s to the late 1930s. The author divides the period into three main phases. Firstly, there is the phase terminating in the late 1920s, when work was concentrated on the theoretical development of quantum mechanics, backed by spectroscopic research into the atom. Then, from the late 1920s to the mid-1930s, and spurred on by Bohr's complementarity argument, interest shifted to increasingly speculative debates on the 'lessons' to be drawn from quantum mechanics for 'a scientific understanding of the atom, the atomic nucleus, and life' (p. 101). Finally, from about 1934 onwards, Bohr launched a concerted experimental and theoretical research programme into the structure of the nucleus. The scientific productivity of the Institute marched in step with this. After reaching a peak of forty-seven publications in 1927 the output from the Institute dropped to seventeen in 1933, whereupon it climbed back to forty in 1937, some three quarters of the papers then being devoted to nuclear problems.

Aaserud's main concern is to explain the re-orientation of the Institute's work to experimental and theoretical studies in nuclear physics in the second half of the 1930s. His hypothesis, which is argued for convincingly, if somewhat laboriously, is that it was due to a combination of what he calls scientific and extra-scientific factors. On the one hand, there was Bohr's growing interest in, and excitement over, the dramatic new discoveries about the content and structure of the nucleus made between 1932 and 1934. However, in Aaserud's view, these alone cannot explain the Director's change of emphasis. What was needed for that was the arrival in Copenhagen of two old friends and refugees from Nazi Germany, James Franck, an experimental physicist, and George Hevesy, a radio chemist. Their move to Bohr's institute, and the subsequent acquisition of equipment like an

accelerator, was made possible, even forced on Bohr, by a change in the Rockefeller philanthropy's funding policies.

Until the early 1930s, Bohr's main supplier of money from this source was the International Education Board. This philanthropy aimed to equip and expand selected centres of excellence doing basic science 'for its own sake'. It also provided fellowships for young scholars to visit such centres on a temporary basis. However, in the mid-1930s the Rockefeller Foundation rethought its funding programme in response to the refugee problem. It now insisted on helping established professors and in supporting programmes in well-defined fields of research, particularly the application of physics and chemistry to biology. This new emphasis, argues Aaserud, paved the way for two men as senior as Franck and Hevesy to come to Copenhagen. It also enabled Bohr, who suitably tailored his grant application, to seek funds for a research programme that combined theoretical and experimental research into nuclear structure.

By showing that a new research programme at one of the most important scientific institutes in the world was triggered, and pushed forward, by social and financial considerations, this book delivers yet another blow to the tired old idea that scientific knowledge is driven by its own internal, inexorable logic. It also throws valuable light on Bohr's activities and strategies as a fundraiser and institution builder. From both these points of view it can be strongly recommended. At the same time the book is marred by the repeated, and ultimately irritating need to align the developments at Bohr's institute to the so-called Copenhagen spirit. This concept is introduced in the prologue to the book through the reminiscences of veteran physicists who spent some time during their youth working with the master. It means something like having a 'free-wheeling discussion' of theoretical or philosophical matters. Aaserud does not comment on the vagueness and even downright contradictoriness in the use made by the physicists of this term (and in their reactions to the spirit at work). What is more, he adds to the confusion by stretching its meaning so much that in his hands it can mean anything from doing speculative philosophy to organizing an informal

physics conference. As a result, it becomes virtually devoid of any analytical value.

This book is extensively annotated and has a very fine index.

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ATOMIC ENERGY FOR MILITARY PURPOSES, with a new foreword by Philip Morrison and an essay by Henry DeWolf Smyth. Stanford: Stanford University Press, 1990. Pp. xvi+324. ISBN 0-8047-1721-4, \$39.50 (hardback); 0-8047-1722-2, \$12.95 (paperback).

HERBERT F. YORK. *The Advisors: Oppenheimer, Teller and the Superbomb*, with a historical essay by Hans A. Bethe. Stanford: Stanford University Press, 1989. Pp. xiv+201. ISBN 0-8047-1713-3, \$32.50 (hardback); 0-8047-1714-1, \$8.95 (paperback).

The relations of science and war, and especially the history of nuclear weapons, are the stuff of myths. No matter what the quantity of serious history done we are drawn back to creation and fall stories, goodies and baddies, and the immortal formula $E = mc^2$. It is refreshing to find republished two important books by participants in the history of the nuclear age – Smyth's book started life as a quickly-written narrative of the Manhattan Project or rather the 'Manhattan Engineer District' responsible for the 'development of substitute materials'; like the scientific work, it was done very secretly. After the bombs on Japan exploded, and careful vetting, it was launched into the world as surely the longest press release in history. It was then quickly published in the United States and Britain. As well as firmly establishing the usage 'atomic', rather than 'nuclear'; it also misleadingly gave pride of place in its first chapter to Einstein's famous formula, establishing the icon for ever. The book was released, cleverly, to hide the real secrets by revealing in great detail what was not secret. The British were nevertheless appalled; their own press release, republished here, was much shorter. But the 'Smyth Report' itself was alleged to be a secret which was improperly leaked – by witchhunters in the MacCarthyite period.