Andrew Ede: *The Rise and Decline of Colloid Science in North America, 1900-1935: The Neglected Dimension*, Aldershot: Ashgate, 2007, 208 pp. [ISBN: 978-0-7546-5786-6]

In the author's words, the main narrative arc of this book is centered on "The rise and degeneration of colloid research" in the US and Canada during the first third of the 20th century (p. 3). This is a tightly focused case study, but Ede seeks to demonstrate that it has implications beyond its limited compass. He has been partly successful, but I believe more could have been done to widen its appeal.

In 1900 North America was on the periphery of international science, including chemistry. US and Canadian chemists were heading to Europe for doctoral training and then, imbued with a zeal for research, returning home with advanced degrees and problems to investigate. Colloid chemistry seemed a promising field because it was still young, with plenty of research space that could be colonized at relatively low cost. In addition, many of these newly minted PhDs had studied with Wilhelm Ostwald, whose son Wolfgang was an international figure in colloid chemistry. The colloid chemistry–North American connection "took".

It took so well that by 1920, according to Ede, "colloids were the hottest topic in American science, whether it was chemistry, physics or physiology" (p. 2). In 1923 the First National Colloid Symposium took place, and a proposal was submitted to the National Research Council for a National Institute for Colloid Chemistry. The field appeared to have broken through to the front ranks of scientific research and to have justified its claim to being a distinct subdiscipline. Yet by 1935, only a dozen years later, it was being written off as a scientific backwater whose claims to disciplinary autonomy were disparaged and/or ignored. What had caused such a catastrophic collapse?

There were a variety of factors: methodological timidity; the contingency of experiment; overweening ambition; and poor public relations. A majority of American colloid chemists adhered to a methodological ideal of simplicity that bordered on the simplistic. They resisted calls for quantification and eschewed overt theorizing, insisting that a theory of the colloid state would arise naturally from a network of qualitative observations on simple experiments. In order to sharpen their observational acuity, they adopted some new instruments – most significantly the ultramicroscope and the ultracentrifuge.

Despite their disclaimers, these researchers did have theoretical commitments. In the period under discussion some thought that colloidal behavior could be explained solely on the basis of existing physical and chemical theories and laws. They were in a minority, however; the majority held that colloids represented a unique state of matter, one whose particles "are so small that they can no longer be recognized microscopically, while they are still too large to be called molecules […] The World of Neglected Dimensions" (p. 7; the quote is from Wolfgang Ostwald). Moreover, a sizeable number of colloid chemists believed colloid chemistry to be the indispensable basis for an understanding of life and for the creation of a scientifically based medicine.

Few biologists or medical doctors accepted or even recognized these assertions, and when the claims were shown to be hollow, the reputation of colloid chemistry suffered a severe blow. The field was also being undermined from within, as its powerful new instruments yielded results incompatible with the predominant assumption, especially the alleged uniqueness of colloids. Ede has symbolized this situation whereby the chemists' research findings were destroying the basis of their own field by the ancient symbol of the Ouroborous, the worm that devours its own tail. These considerable difficulties were ex-

acerbated by a generational split as the older adherents, led by Wilder Bancroft, found their qualitative approach disdained by their younger, quantitatively oriented colleagues. Furthermore, Bancroft's emphasis on the utility of colloid chemistry did not endear the subject to those chemists eager to burnish their credentials in "pure research".

Ede tells the story competently and pays attention to important contextual factors: intra- and interdisciplinary politics; funding decisions; institutional priorities; and the boost that World War I gave to American economic, political, and scientific standing. In addition, he connects elements of his narrative with several larger issues in the history and philosophy of science. For instance, he makes the important point that in the absence of any agreed upon theory of the colloid state, "there were for colloids no anomalous results that might then have become the focus of investigation" (p. 185). Unfortunately, this important consequence, which bears upon an issue of interest to non-specialist readers, is pointed out only two pages before the conclusion. In general, more thought could have been given to attracting such an audience. I finished the book with the unhappy feeling that it would find few readers outside the small circle of those interested in the history of physical chemistry in the 20th century.

The text is replete with equations and diagrams of instruments. This is all to the good – history of science should examine in detail the tools of scientific practice. However, that examination should describe their significance as well as their function, which has not always been done. For example, the author notes that those chemists who did not accept the uniqueness of colloids, such as Jacques Loeb, maintained that most colloid phenomena could be explained with the aid of the Donnan equilibrium. This topic is mentioned early in the text and gets seven entries in the rather skimpy index. Yet its mathematical formulation does not show up until p. 142, 80% of the way through the text. Had it come earlier, accompanied by a written description of its significance, it would have helped the reader grasp one central strand in the debate over the status of colloids. On pp. 113-114 an equation for calculating the molecular weight of a colloid from ultracentrifuge data is developed. All its seven parameters are defined but nothing is said about the efficacy, reliability, ease, and expense of obtaining molecular weights in this way compared with other methods. Analogously, an equation on p. 110 contains a parameter describing "the distance the boundary moved", but there is no explanation of what boundary is being referred to.

I think that potential non-specialist readers (and perhaps specialists as well) would want to know if the events described in this book constitute a one-off or whether they resemble other moments in the history of science. After I had read about a half dozen pages I began thinking about analogies between Ede's account and the rise and decline of physical organic chemistry in the US. The study of organic reaction mechanisms became established in the UK in the 1920s. While there had already been related work in the US by then, the British thrust stimulated a strong response on the western side of the Atlantic. While American chemistry as a whole was far stronger in 1930 than in 1900, in traditional organic it lagged behind its European, and especially German counterparts. Mechanistic studies provided an open field where ambitious American organic chemists could take advantage of their countrymen's strengths in physical chemistry and make their mark on the international stage. Their success was much longer lived than that of their colloid colleagues – by the end of World War II Americans dominated the subject and it was attracting a large proportion of the top organic students. Yet by the 1980s the best and the brightest were looking

elsewhere. Physical organic chemistry was no longer the hot topic, and its decline, like that of colloid chemistry, could be traced in part to its obsessions, in this case the nonclassical ion problem. Yet the denouement of these two developments could hardly have been more different, one the victim of its failures and the other the victim of its successes. I do not doubt that Ede could think of other (and perhaps better) comparisons; had at least one been included, it would have enriched the discussion and widened its relevance.

The publisher has not helped the author's cause. The considerable number of grammatical errors, missing words, and typos suggest that no one other than the author has read the final manuscript. In Chapter 6 there is reference to a Figure 7.3; it is not to be found in either Chapters 6 or 7. A text such as this which contains a lot of tables and figures presents numerous problems of page layout; in this case, many of the solutions are remarkably infelicitous. For example, Table 5.1 is labeled "Graduate Students in selected categories, 1924-35". However, the entries start at 1935 and end at 1924. Furthermore, the table (which is not very long) is split over two pages and requires that one turn the page to get the remainder of the data. This is not the only example of poor design. It is a shame that the author's exacting research has not been matched by a comparable effort on the part of his publisher.

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