WRITING THE HISTORY OF MODERN CHEMISTRY*

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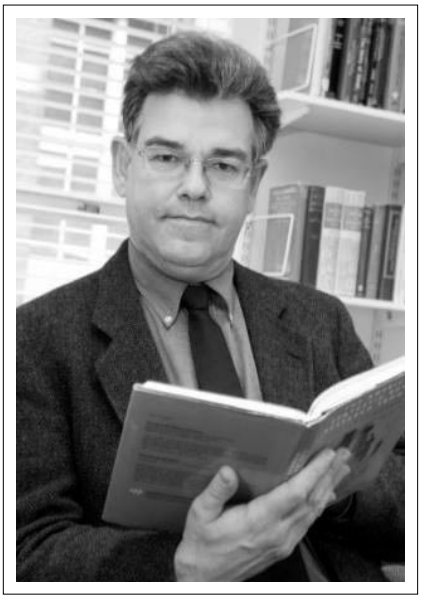
Significance of Twentieth Century Chemistry

Before discussing the history of "modern chemistry, "we need to define what modern chemistry is. After all, as late as 1954, Arthur J. Berry wrote a book entitled *From Classical to Modern Chemistry* which stopped in the early twentieth century (1). By modern chemistry I mean chemistry in the twentieth century (the same definition as the Commission for the History of Modern Chemistry of the International Union of History and Philosophy of Science). One could argue that it should be chemistry after 1945 but this would narrow the field too much, although it is remarkable that we can still describe chemistry a century ago as "modern." The twentieth century was a period of immense growth in chemistry, however we measure it. Using that incomparable source of statistical information, *Chemistry In America*, we find that the number of chemists in the USA rose twelvefold from 9,000 in 1900 to 110,000 in 1970, the number of papers published rose from 3,940 in 1913 to 106,552 in 1980 and the doctorates conferred in the USA rose from only 69 in 1900 to a peak of 2,224 in 1970 (2). Turning to the chemical industry, we find that the number of chemists employed in industrial research laboratories in the USA rose elevenfold from 3,830 in 1921 to 42,800 in 1960 (3). These chemists were very productive: 52,411 US chemical patents were taken out in 1961-5, against 4,001 in 1896-1900 and the output of "chemicals and allied products" in the USA grew 33-fold between 1899 and 1957 (compared with 10-fold for total manufacturing) (4).

Relative Insignificance of the History of Modern Chemistry

Yet when we turn from chemistry to the history of chemistry, we find a different picture. Even the last three decades of the twentieth century, the history of modern chemistry has been overshadowed by three periods which have been more popular with historians of chemistry: alchemy and chymistry, the Chemical Revolution and the nineteenth century. Indeed it could be argued that the last two or three decades of the nineteenth century have been neglected compared with the earlier part of that century. Taking the nineteenth century as our benchmark, for simplicity, and examining the number of papers in the leading journal *Annals of Science* between 1970 and 1986, there were only 2 for the twentieth century, compared with 16 for the nineteenth century. If we look at the number of papers in *Ambix* between 1986 and 2000, there were 24 for the twentieth century against 58 for the nineteenth century, a ratio of 41:100. We might have expected the situation to be better in the case of biographies, as the remembrance of more recent chemists would be fresher in the collective memory. The situation is better but not by much. Of the biographies reviewed in *Ambix* between 1970 and 2003, 12 were about chemists mostly active in the twentieth century against 21 for their nineteenth century counterparts. And this bias is reflected in general histories of chemistry. It is difficult to make accurate estimates as it is not easy to allocate individual pages to one century or the other, but if we look at three recent examples by William Brock, Bernadette Bensaude-Vincent and Isabelle Stengers, and Trevor

Levere, we find that the percentage ranges from about 15 to 30%, with Brock making a particular effort to cover the twentieth century (despite being the former director of a Victorian studies center) (5). The situation regarding books is much the same for the history of industrial chemistry although as a genre one might have expected a bias toward more recent histories. Of the 66 books about the chemical industry recorded in the British Library OPAC as published between 1970 and 2003, 22 (33%) were recorded as being twentieth century. Of the 52 books on "industrial chemistry" reviewed in Ambix up to 2003, 36 (69%) had significant content about the twentieth century. This higher figure is partly a result of the "long" nineteenth century, as several of the books went up to 1914.



Dr. Peter J. T. Morris, photo by Jennie Hills

Problems of Writing the History of Modern Chemistry

Even if we assume historians of chemistry have a personal bias in favor of the chemistry of earlier periods, the relative lack of material about modern chemistry suggests that there must be something problematic about writing its history. My personal experience has confirmed that this is the case. Although the chemistry of earlier centuries has its own difficulties, it is universally recognized that twentieth century chemistry is, technically speaking, very advanced, requiring a chemical training of some kind. This problem is compounded by the ever-increasing use of jargon and highly stylized writing in chemical publications. This trend is strikingly demonstrated by *Accounts of Chemical Research*. When I first read it in the mid-1970s, some chemical knowledge was required but it could be easily read by a nonspecialist. When I subscribed to it briefly two decades later, I found it completely unreadable. There is a vast amount of written material, especially in archives of the chemical industry, but there is a lack of personal correspondence and other personal material compared with the nineteenth and early twentieth centuries, and there is an increasing lack of paper material thanks to telephone and emails. There is also the issue of the destruction of material shredded by archives trying to keep their holdings within bounds, put into the trash when factories are closed or thrown away by their families (6). This demonstrates the importance of preserving our chemical heritage and finding archives for this important material.

Key Features of Writing the History of Modern Chemistry

What are the main features of writing the history of modern chemistry? Based on my own experience, I would stress the importance of interviews and oral history. This is not only a matter of finding out the details of what happened, by whom and when. Indeed interviews of elderly chemists are often an imperfect way of doing this, although they may also be our only source of information. Their main value lies in giving us the broad picture on one hand and the subtle relationships and interconnections so rarely captured by physical documentation on the other. A person's personality often provides the key for understanding why something happened and not something else and this is best provided by oral history as this often provides an assessment of their character by those who worked with them. At the same time the historian has to be acutely aware of the pitfalls of oral history. Not only are elderly chemists attempting to recall, usually without documentary assistance, what happened fifty or more years ago, but they are open to all the partiality and personal bias of any form of autobiography. Self-justification and self-glorification can never be completely absent, but I have usually found my interviewees to be as objective about their past experiences as one can ever hope to be in these circumstances.

If it is not possible to interview participants, correspondence offers an alternative, and in writing letters my correspondents often thought more carefully about what they were saying than they might have done in an interview and used physical documentation while they were writing the letter (and sometimes enclosed a copy of this documentation with the letter). The advantage of the interview is that you can ask the questions more directly and follow up with supplementary questions which is difficult to do in correspondence.

Once the basic research and writing have been done, it is crucial in my experience to have this work reviewed

by the chemists involved or who were at least familiar with the developments being studied. This review can show up all kinds of confusions and misunderstandings and is extremely valuable in improving our understanding of the history of chemistry. The dual importance of interviewing and review by the chemists involved means that the historical research should be done while the leading actors are still alive if this is possible. Once these chemists have passed on, it becomes much more difficult to reconstruct what actually happened, as I know well to my cost.

Any historian of modern chemistry—and especially the historian of the chemical industry—has to be able to scan huge quantities of written material, seeking out the truly informative documents and at the same time be able to tease useful information out of historically uninteresting documents (formal minutes, laboratory reports etc).

Having established these basic points, I will now illustrate them by a number of case studies of research I have carried out over the last three decades. They range from the history of industrial chemistry to biography, from the USA to Germany, and from organic synthesis to chromatography. If they do not form a completely representative sample of the history of modern chemistry, they are at least a varied one.

History of Synthetic Rubber in IG Farben

When I came to this topic in the late 1970s, there were a few histories already available, but they were very diverse and often turned out to be wrong, the major exception being the booklets produced by BASF's archives and which I was fortunately able to obtain while I was working in a nursing home in Ludwigshafen in the summer of 1977. The key archives were evident from the outset, namely the Bayerarchiv in Leverkusen, the IG Zentralarchiv at Hoechst near Frankfurt, and the British Intelligence Objectives Subcommittee collection which was then at BL Boston Spa (now stored at the Imperial War Museum at Duxford). The papers of the war crimes trial, US vs IG Farben, were crucial and were fortunately available—at a price—on microfilm. I also found useful material at Chemische Werke Hüls in the Ruhr and at the Imperial War Museum (the Speer archives).

The Bayerarchiv kindly put me in touch with several useful interviewees, notably Heino Logemann, Curt Duisberg, and Claus Heuck. I did not feel able to travel to a remote part of the Alps in winter to interview Albert Speer, although he was very insistent, and unfortunately he died before we were able to meet. To my disappointment, the key actor Otto Ambros refused to meet me although the Bayerarchiv tried to persuade him. However Speer and Ambros corresponded with me and also Franz Broich—formerly at Schkopau and Hüls—who was particularly informative about the manufacture of butadiene from monovinyl acetylene, a process which hardly appears in the physical documentation at all.

As I gathered information from the trial records and the various archives, I created a "card" index (actually A5 slips of paper) which collated documents from different sources by date. In this way I was able to reconstruct runs of correspondence and link letters to meetings, and meetings to subsequent events. Eventually I successfully reconstructed the history of synthetic rubber in Germany and developed a strong argument about the relationship between IG Farben and the Nazi regime, but I would like to have done more on the scientific aspects of its development, for example, the development of redox polymerization. The feedback about my thesis—which was completed in 1982—was generally positive from both participants (including Otto Ambros) and historians (such as Peter Hayes) (7).

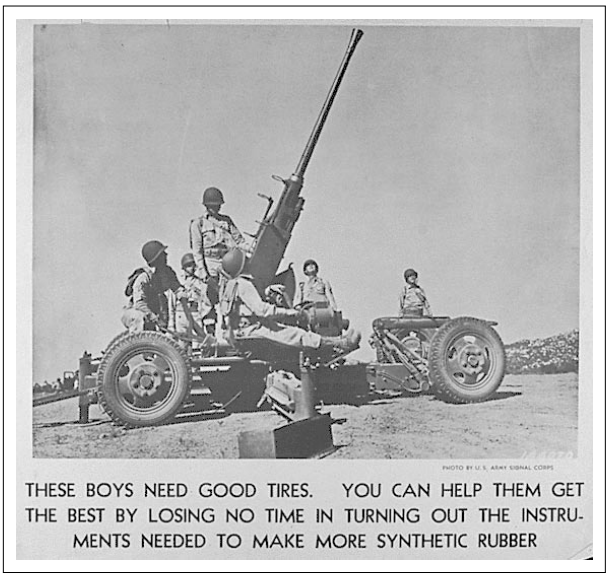
Buna exhibit at the Four-Year Plan Exhibition in Berlin, 1937, courtesy of the author

Synthetic Rubber in America

By contrast with the IG case, there was already a strong historical literature when I began my work on the synthetic rubber research program in America, and Herbert and Bisio's book was published just after I started in 1985 (8). Once again, the major archives were soon clear, namely the US National Archives, the University of Akron archives, and the AT&T archives. I also used the

archives of the University of Illinois and Goodrich, but not Firestone or Goodyear. I interviewed some participants, including an oral history of Paul Greer, a program administrator who died recently at the age of 101, but this was not a major aspect of my research; but my interviews did confirm that I was on the right track.

The main source of information for my book was the technical literature. The card index—and it was a card index this time round—in this project tracked the literature rather than letters and minutes. My major difficulty was constructing a strong argument. Because of a lack of information and a certain reticence on the part of some participants, the personal aspects of the rubber program were lacking to some extent. I feel I produced a successful reconstruction of the history of the research program, even if it was perhaps rather brief, but I have had very little feedback (9). A group of rubber industry retirees said that they had read my book during a reunion dinner and they were going to send me their comments, but they never did.



Courtesy U.S. National Archives

R. B. Woodward and Organic Synthesis

During my Edelstein fellowship in 1991 I was invited by Arnold Thackray to work with Otto Theodor (Ted) Benfey on a book about Robert Burns Woodward (10). This arose out of an exhibition about Woodward (with an accompanying booklet) put together by Ted and Mary Ellen Bowden (11). Ted edited the Cope lecture and I wrote the introductions to Woodward's most important publications. In order to do this, I interviewed and corresponded with colleagues of Woodward (Gilbert Stork, David Ollis, Claude Wintner, Ray Bonnet), but it was mainly the result of library research. Fortunately the Science Museum Library is particularly strong on twentieth century books and journals up to about 1970. Not being near Harvard, and not having the funds to travel there, I made no use of archives at all. *Chemical Reviews* and *The Alkaloids* series published by Academic Press were particularly useful.

In this case, I would particularly emphasize the importance of the feedback on my work by experienced chemists, including Gilbert Stork, David Oillis, and Stephen Mason. It also illustrated the value of having a major science library close at hand. I am glad to say that our book was well received with no criticism of my introductions to Woodward's papers, but the format is inevitably limited as a historiographical technique. It was however a fairly rare example of a historical book that appealed to active chemists. I once met a young chemist who had become a venture capitalist at a dinner in Cambridge who had enjoyed reading it very much.

My Research on Walter Reppe

This is a good example of a less successful project. It was planned as a continuation and expansion of my earlier work on synthetic rubber, but the existing historiography was very limited. To my disappointment, I found that the archives were also of limited value; I used the archives at BASF and the Deutsches Museum. Hence, this project rapidly became very dependent on a few key books and documents, never a good idea. Furthermore, I only carried out a couple of interviews, and neither of those was particularly useful, partly because of language difficulties. This was an interesting topic because of the light it shed on the development of the organic chemical industry, but Reppe himself was not an interesting person. To make matters worse, personal information about Reppe was very limited and I did not try to contact his family. It is very doubtful if the final product could have been constructed as a biography and would have really only worked as a history of industrial research with Reppe as the central (but rather shadowy) character. In the end, this project was abandoned because of changing priorities at the Science Museum, and I published what I had gathered as a paper in *Determinants* and as an entry in the *New Dictionary of Scientific Biography* (12). While it has not been completely wasted, this case study does show the difficulties facing the historian of modern industrial chemistry if the main actor is no longer alive.

The ECD and the History of DDT Analysis

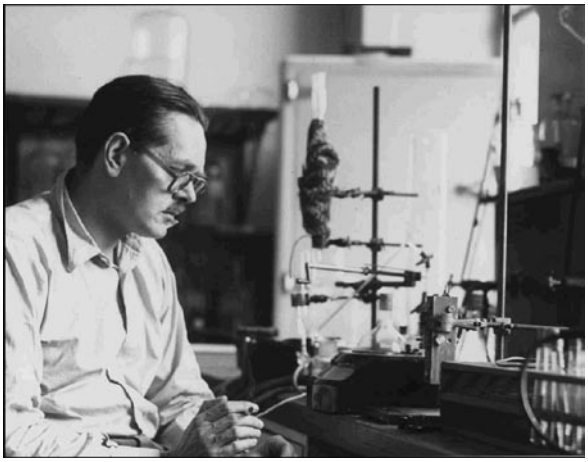
This project in the late 1990s stemmed from a need to show the importance of the electron capture detector (ECD) to justify putting it on display in the Making of the Modern World Gallery at the Science Museum and from a desire on my part to explore the contribution of chemists to the development of environmental protection. I was struck by James Lovelock's claim in various publications, most recently in *Homage to Gaia*, that his development of the ECD helped Rachel Carson to write her famous book *Silent Spring* (13). Given the timing I found this claim very surprising, and I soon showed that this was not in fact the case (14). It had the effect, however, of making me focus on the use of the ECD to detect pesticides rather than its later and more celebrated use to detect CFCs in the South Atlantic. The gap between the events and the information gathering had been about 40 years in the case of synthetic rubber and it was about the same here, but to my surprise hardly any participants had survived or were available for interview except Lovelock himself, who has been very helpful, I am glad to say. There was also a problem of distance; I had no funds to visit California and in any event there were no obvious archives, which is not to say there are none at all. I did not try to use the Shell archives and there was no material at National Institute for Medical Research at Mill Hill in north London, where Lovelock worked in the 1950s.

The historiography of pesticides and pesticide analysis is still developing, for example Edmund Russell's book *War and Nature*, and while this was of some use, I was largely dependent on technical literature and Lovelock's autobiographical writings (he very kindly lent me a manuscript version of *Homage to Gaia* before it was published) (15). An obscure book by a journalist Rita Beatty, *The DDT Myth,* was very useful in setting the scene for me (16). This once again shows the importance of having access to a first-rate library. A copy of the *Pesticide Manual* I had bought by chance in a charity shop was also very helpful (17). Fortunately the internet was now available as a major source of information.

Another major problem was the lack of suitable chemists to review my findings, which shows the need to develop links with the relevant expert community. The outcome was successful but perhaps lacks depth, and there is no doubt that it would have benefited from archival research (18). There was also a marked lack of feedback afterwards, which reflected the lack of survivors in this field and repeated my experience with the American synthetic rubber research.

Writing the Biography of Archer Martin

Fortunately my research on early chromatography in the Lovelock project paid dividends when I received an unexpected commission from the *Oxford Dictionary of National Biography* to write the entry on the Nobel Laureate Archer J. P. Martin. I was able to draw on three important contacts I had obtained through my research on DDT analysis: Leslie Ettre, James Lovelock, and Edward (Ted) Adlard. Through Adlard I made contact with Martin's family, which was crucial for the project's success. There were no archives, but once again I made extensive use of the internet, for example, genealogical indices. I also carried out a "meta-analysis" of the multiple obituaries and biographies of Martin that were available. The final product was greatly improved by an exhaustive revising process, whereby successive drafts were critiqued by Leslie Ettre, who also contributed many recollections of working with Martin. In the event, the final entry was very successful. It immediately became the biography of the month when it was published online in January 2006, and led to a commission to write the entry on Archer Martin for the *New Dictionary of Scientific Biography*.



Archer J. P. Martin in his laboratory at Mill Hill, early 1950s, courtesy of the National Institute for Medical Research

The Life of Derek Barton

I was also invited to write the entry on Sir Derek Barton. My interest in Barton arose from a more general interest in the history of organic synthesis and from the

proximity of his beloved Imperial College to the Science Museum. Our paths could have easily crossed at some point but—for better or worse—I never met the great man (nor did I ever meet Reppe or Martin). In contrast to Martin, there were a number of books available, Barton's autobiography *Gap Jumping*, the very useful *Bartonian Legacy* edited by Ian Scott and Pierre Potier, an entry in the *Oxford Dictionary of National Biography* by Noel Coley, and the obituary in the *Biographical Memoirs of the Royal Society* by Steven Ley and Rebecca Myers (19). In the circumstances, as time was short, archival research was not really necessary; and I made less use of the internet than in the case of Martin, except to track down his son whom I decided not to contact anyway.

Writing the actual entry was more a matter of developing a fresh approach to Barton's life and career than trying to find new material. In the time available, I was not able to get behind the façade of Barton's autobiography and to discover what really drove Barton to become a great chemist. Various drafts were read by his former co-workers and students, David Widdowson, William Motherwell, and Anthony Barrett, and I found to my cost that Barton's circle was fiercely protective of his reputation. Nevertheless, I believe that my final version was a balanced account of his life, without saying anything much that was really new.

What Lessons Can We Draw?

What are the conclusions I would draw from my own experience of writing the history of modern chemistry? I have found that it is possible to write the history of modern chemistry, but I have also discovered that it is necessary to write about it in a way that is accessible to a broad audience: chemists, historians, journalists, and the educated public. The audience for this subject is already tiny; any failure of communication—for instance by using jargon or a lot of chemical terms—reduces it to zero. It is therefore necessary to employ a certain rhetoric to capture the reader's attention. When I switched from the history of industrial chemistry to the history of chemical instrumentation, it took me several years to develop a new rhetorical style. One of the biggest problems for me has been the lack of feedback, especially my work on American chemistry for some reason.

I sense that my work has been reasonably successful with historians of chemistry as demonstrated by the 2006 Edelstein Award, but I feel my publications have had little impact outside our community. This is a problem for all of us. How much of our work is read by mainstream historians, chemists, or journalists? In my experience, with the marked exception of the American Chemical Society and its publications, journalists and publishers assume only scientists can write the history of (modern) science. I often wonder if they know we exist. I suspect they are aware (dimly) of our existence, but they worry about irritating scientists whom they perceive—probably correctly—as their main audience as historians of science are seen as somehow being "anti-science" as a result of the so-called "Science Wars" (20).

Future of the History of Modern Chemistry

Ignoring the fact that our "modern chemistry" will soon become "old chemistry," what is the future for the history of modern chemistry? At the beginning of this paper, I pointed out the importance of chemistry and the chemical industry in the twentieth century. Despite recent improvements, the historiography of the period does not reflects its significance. I am concerned that the backlog is mounting while the number of historians working in this field is decreasing, particularly outside the USA. I am also worried that the raw material for this research, the documentation and the oral histories, are not being kept, while libraries and archives are actually being broken up or at least pruned. For without this material how can we ever write the history of modern chemistry?

However, there are some signs that the situation may be slowly improving if we compare the present day with two decades ago. Between 1981 and 1985, there was 1 paper in *Ambix* about twentieth century chemistry compared with 18 for the nineteenth century, a ratio of 6:100 (cf. 41:100 for the period 1986-2000). Between 2001 and 2005, there were 16 papers in *Ambix* about twentieth century chemistry compared with 13 for the nineteenth century, a ratio of 123:100. Twenty-six percent of the history of chemistry entries in the British Library OPAC were twentieth century in the period 2000-2004, compared with 4% in 1980-1984 (in fact just one book: A. S. Travis, *The High Pressure Chemists*) (21).

So how do we make the situation even better? My own work shows that it is possible to write the history of modern chemistry to a high professional standard. Given the advanced knowledge of chemistry required for writing the history of this period, we need to attract more chemists into this field. They can be either retired chemists or young chemists who have decided to pursue a career as a historian or curator, but this will be a difficult task given the low status of history in today's chemical community. We need to develop ways of attracting chem-

ists into the history of modern chemistry (which may appeal to them more than the history of earlier periods of chemistry) and find ways of training them which are effective, quick and at the same time, appealing.

If the supply side gives me concern, so does the demand side, Sadly, college libraries are sharply reducing orders for books in order to pay subscriptions for online journals. There is little evidence that individuals buy many (or any) books on the history of chemistry, and the price is often prohibitive. Is it worth writing the history of modern chemistry if no one is reading it? This is a very good question and needs to be taken seriously.

Nevertheless, I do believe there is a future for the history of modern chemistry. Many chemists are enthusiastic when they encounter it, not least because it addresses the question raised by any community or profession of "how did we get here?" to a much greater extent than, say, the history of nineteenth century chemistry (not that I have anything against the history of nineteenth century chemistry, I hasten to add). The web offers a way of introducing the history of chemistry to audiences that have hitherto not been aware of our work. Ever-increasing digitization of journals and books has greatly increased our access to printed material. I only wish science journals before a certain date were open-access in the same way that medical journals have recently been made available. Amazon sells books that would not be available in any "real" bookshop, often at a discount. All this must be good for minority subjects such as our own.

I would like to conclude this paper by saluting the sterling work done by many people in this area, especially Arnold Thackray for making the history and preservation of the heritage of modern chemistry the main focus of the Chemical Heritage Foundation; Jeffrey Seeman for his seminal series of autobiographies by leading organic chemists which have underpinned much of my recent work; and Christoph Meinel for setting up the Commission for the History of Modern Chemistry, which has promoted the field by holding regular conferences.

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Division of the History of Chemistry of the American Chemical Society

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