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Who **does** write better books in history of chemistry?

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EDITOR'S LETTER

Dear Readers,

I wish to draw your attention to three articles and two reviews in this issue that I consider to be slightly outside the main fare of the journal, namely scholarly articles and book reviews in the history of chemistry.

The articles in question are all from distinguished contributors, and, I hope you will agree, exhibit high quality and interesting material. Seymour Mauskopf considers the question "Do Historians or Chemists Write Better History of Chemistry?" in an informative historiographic essay. Pierre Laszlo combines first-hand observations and opinions with scholarly research to describe "How an Anglo-American Methodology Took Root in France" in mid-20th-century physical organic chemistry. William Jensen and Julia Kuhlmann present a translation of Leopold Pfaundler's 1867 paper, "A Contribution to Chemical Statics." Publishing translations of primary historical documents, including "forgotten classics," is one of the functions originally envisioned for the *Bulletin*. Prof. Jensen's most recent contribution along these lines, a translation of an 1873 paper by August Horstmann, appeared in this journal two years ago (1). A companion article on Pfaundler's work will appear in the next issue. Historiography of chemistry, represented by Prof. Mauskopf's contribution, is another subject that has graced the pages of this journal from time to time, one which is certainly relevant to the mission of the *Bulletin*. Of the three articles I've mentioned, Prof. Laszlo's is closest to the standard scholarly article in history of chemistry, but it is a bit unusual in that its style and content include some aspects of memoir.

The reviews to which I draw your attention are of media that are somewhat different from the usual texts on history of chemistry for scholarly or general audiences. Theodor Benfey drew my attention to a radio play, *Nuclear Reactions*, by Adam Ganz, based on the Farm Hall transcripts of German nuclear scientists immediately after the Second World War. His review includes a couple of his own reminiscences of that period of history, information on how to access the play, and, of course, a description of the play. From its title alone, Lauren Redniss's book, *Radioactive: Marie & Pierre Curie, A Tale of Love and Fallout*, might seem to be an ordinary biographical text or perhaps a novel. But one can judge from its cover that it is an extraordinary illuminated book. Read Tami Spector's review to find out more.

As I continue to learn the job of editor, I remain open to suggestions on how best to fulfill the mission of the *Bulletin* to attract and disseminate high-quality articles and reviews in the field of history of chemistry. In my first year, my main job has been to try to do no harm to what William Jensen and Paul Jones built. In the future, I look forward to exploring additional ways for the *Bulletin* to serve its readers and authors. Please stay tuned.

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1. W. B. Jensen, "Introduction to the English translation of 'The Theory of Dissociation,' a Forgotten Classic of Chemical Thermodynamics," *Bull. Hist. Chem.*, **2009**, *34*, 73-75; A. Horstmann, "The Theory of Dissociation," W. B. Jensen, Trans., *Bull. Hist. Chem.*, **2009**, *34*, 76-82.

DO HISTORIANS OR CHEMISTS WRITE BETTER HISTORY OF CHEMISTRY? (1)

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This paper was the response to a challenge set me as the senior historian of chemistry (in age) serving on the ACS National Historic Chemical Landmarks Committee. The challenge was to reflect on the history of chemistry in terms of the question posed in the paper's title. Although there is a "tongue in cheek" quality to the question, it also has its serious side and it challenged me. I have to say that, until two weeks before my talk, I had no idea how I would respond. Then I had my epiphany and the result is the following historiographical reflection.

Let me state at the outset that I am primarily an historian rather than a chemist. Although I was only one credit shy of completing an undergraduate major in chemistry at Cornell, in fact, I was graduated with an official major in history and received "highest honors" for a senior thesis on twelfth- and thirteenth-century canon law! But I did encounter Henry Guerlac in two courses and subsequently did my graduate study in the history of science at Princeton (1960-1964) under Charles Coulston Gillispie, with a field in the history of chemistry and a dissertation that combined history of late eighteenth-century-early nineteenth-century chemistry with related areas in the history of physics. In my graduate studies in the history of chemistry, I did my first reflective survey of the field as it had developed down to about 1960. I had not done a sequel until this challenge was posed.

In retrospect, I see that the principal obstacle I faced was definitional. I think I know how to define a "**chemist-historian**" (i.e., a chemist who researches and writes the history of chemistry) but I was less sure regarding an "**historian of chemistry**." Because of the technical nature of the field, virtually all of us who do history of chemistry have some background in chemistry, as my own example illustrates. Some have considerably more

than I have. Alan Rocke, for example, did graduate work in chemistry. Arnold Thackray worked as a chemical engineer. Lawrence Principe has a joint appointment in the history of science and in chemistry and teaches chemistry. Does this make them chemists doing history of science or historians of science doing history of chemistry?

My epiphany was the realization of the following definition: an "**historian of chemistry**" is an "historian of science" doing the history of chemistry. By "historian of science," I mean someone who has (a) received training in the history of science and (b) holds some full-time position related to the history of science—be it an academic position or one in some institution promoting history of science (e.g., a museum).

This immediately provides a structure to my historiographical reflection. Until the mid-twentieth century, virtually all the people writing history of chemistry were chemists (one gigantic exception—Hélène Metzger), for the simple reason that there were virtually no trained historians of science (more on this and on Metzger below). By, say, 1960, history of science programs were emerging and training historians of chemistry, among other fields. These scholars (the Thackrays, Rockes, Principes—and Mauskopfs) became the norm although some chemists continued to display an active and abiding interest in the history of chemistry as witnessed by the Dexter-Edelstein Award tradition, activities of the ACS Division of the History of Chemistry (HIST), and the National Historic Chemical Landmarks Committee.

I shall discuss the work of a somewhat idiosyncratic group of **chemist-historians**—idiosyncratic in being of personal interest. Then I shall turn to the development of the history of science as a disciplinary field in mid-twentieth century and offer some conclusions.

Chemists and the History of Chemistry

The nineteenth century was already replete with historical activities of chemists. When I was in graduate school in the early 1960s, the most comprehensive standard historical reference work for chemistry was still Hermann Kopp's *Geschichte der Chemie* (2). I can still remember waiting with bated breath for the first published volume of J. R. Partington's *A History of Chemistry* (3) to see whether it would really supplant Kopp. (It did, but primarily as an immense reference work in my view.) Kopp's history of chemistry is merely the most prominent of historical productions by chemists including such notable productions as Thomas Thomson's *The History of Chemistry* (4), Albert Ladenburg's *Vorträge über die Entwicklungsgeschichte der Chemie in den letzten hundert Jahren* (5), Jean Baptiste Dumas' edition of the works of Lavoisier (first four volumes, followed by the remaining two edited by Eduard Grimaux) (6), and the works of Marcellin Berthelot on medieval chemistry and alchemy (7) to name a few. Since I mention Berthelot, I ought for the sake of equity mention his opponent, Pierre Duhem's *Le mixte et la combinaison chimique: Essai sur l'évolution d'une idée* (8), a work which I consulted with great profit a few years ago while writing on the historical background to Proust's law of definite proportions.

In connection with my work on Proust, I would also like to mention an historical study by a chemist that I came upon as a graduate student and found both quite amazing and inspirational. I refer to Ida Freund's *The Study of Chemical Composition: An Account of Its Method and Historical Development* (9). Freund (1863-1914) had an extremely interesting life and career—in some respects comparable as pioneer woman scientist to her more famous contemporary, Marie Curie. Born in Austria, Freund was both orphaned and seriously injured at a young age (losing a leg in a bicycle accident). Initially educated in Vienna, she was taken to England by an uncle (a member of the Joachim string quartet) where she was able to enter Girton College, Cambridge, in 1882 and take the natural sciences tripos, obtaining a first class degree with a specialty in chemistry. She spent the rest of her life teaching chemistry and doing research at Newnham College. The basis for *The Study of Chemical Composition* was a third year lecture course that Freund devised for her women students reading chemistry (10).

There were active historically-minded chemists in the United States at about the time Duhem and Freund were publishing their pioneering historical studies on chemical composition. One famous one was Edgar Fahs Smith, chemist, historian, and, best known today

for the magnificent historical collection in chemistry at the University of Pennsylvania. Probably less known is his Tarheel contemporary, Francis Preston Venable (1856-1934). Son of a professor of mathematics at the University of Virginia (and aide-de-camp to Robert E. Lee in the Civil War), Venable was trained in chemistry at the University of Virginia and he was offered the chair in chemistry at the University of North Carolina at Chapel Hill in 1880. Going overseas for advanced training, he earned a doctorate at the University of Göttingen and then returned to UNC to carry out important chemical research. Most notable was his identification of calcium carbide. In 1893 he received the Mary Ann Smith Professorship, the first endowed chair at UNC. In 1899 he served as Vice President of the Chemistry section of the AAAS and in 1905 President of the ACS. From 1900 to 1914, he was President of UNC (11).

Despite a busy schedule—to put it mildly—Venable had time both to collect historically important works in chemistry (which now constitute the core of the Venable Collection at UNC) and to carry out writings in the history of chemistry. He published two major historical studies (or three, depending on how you count): *A Short History of Chemistry* (1894 and subsequent editions), *The Development of the Periodic Law* (1896), and an expanded version of the *Short History* under the title *History of Chemistry* (1922). (12)

The prefaces to these books are interesting in giving some clue as to why a Tarheel chemist would be so astonishingly pioneering in historical studies. That of his *Short History of Chemistry* gives a context or origin similar to that of Freund's book and even prefigures the motivation behind the *Harvard Case Histories in Experimental Science* of more than fifty years in the future. I quote from Venable's preface:

This History has been written because of a conviction, from my own experience and experience with students, that one of the best aids to an intelligent comprehension of the science of chemistry is the study of the long struggle, the failures, and the triumphs of the men who have made this science for us. The work is based upon a course of lectures delivered for several years past to my classes in the University of North Carolina. The effort has been made to systematize and digest the material on hand so as to render it available for those desiring a general knowledge of the subject. (13)

Venable was very familiar with the literature in the history of chemistry that had built up in the course of the nineteenth century.

I would like to know a lot more than I do about the details of the success of this book—who bought it and how it was used—for *Short History* went through a number of editions. Venable wrote in the preface of the sequel of 1922 that, although the *Short History* “had passed through a number of editions, there has been no attempt to bring it up to date nor to revise it in any way:”

It has now been entirely rewritten on a changed plan of arrangement and made to cover the great progress in the science which has taken place since it first appeared. (14)

The Prefatory Sketch to *The Development of the Periodic Law* is also interesting in that Venable provided a *philosophical* rather than pedagogical motive for writing the book:

The reproach that chemistry is not, in the fullest sense, a science will continue just so long as chemists content themselves with taking together the straws of facts, gleaners many of them in a harvested field, and neglect the ‘weightier matters of the law.’ The gathering of facts is good, gleaning is good, but contentment with such gains means stagnation. The task has been undertaken in the hope of arousing interest in this matter and of aiding in the further development of the still incomplete system. (15)

The result was Venable’s most ambitious historical monograph, running to almost three hundred pages of text.

It is clear to me that Francis Venable was a **chemist-historian** who merits more study.

Activity of chemist-historians did not slacken in the first half of the twentieth century. One of the most useful results of such activity, in my opinion, is Tenny L. Davis’ *The Chemistry of Powder and Explosives* (1943), with its wealth of historical material (16). This, too, I believe, was the fruit of a course on the subject that Davis taught at MIT. These decades also witnessed the first historical publications of J. R. Partington and the complete oeuvre of Hélène Metzger (1889-1944), who died tragically in a Nazi concentration camp (17). Metzger, trained as a crystallographer but unable to obtain an academic position and able to support herself privately, treated history of chemistry as a species of intellectual history very much as part of the milieu of French historical and philosophical studies being carried out by her contemporaries such as Gaston Bachelard, Émile Meyerson, and Alexandre Koyré.

What they did—particularly Metzger and Koyré—was to historicize their subject matter. Rather than the orientation of Venable on “the long struggle, the failures,

and the triumphs of the men who have made this science for us,” Metzger attempted to get in the mindsets of her seventeenth- and eighteenth-century protagonists with as little reference as possible as to whether they were ultimately “right.” It is significant that, early on in *The Structure of Scientific Revolutions*, Thomas Kuhn cites Metzger (along with Meyerson and Koyré), as having shown him “what it was like to think scientifically in a period when the canons of scientific thought were very different from those current today (18).”

Emergence of History of Science as an Academic Discipline

All of the above belongs to the “archaic” period of the history of science as an academic discipline. During the first half of the twentieth century, the history of science began to emerge as an academic discipline but only slowly. There were few journals in the field before the 1940s (*Isis*, 1913; *Annals of Science*, 1936; *Archives internationales d’histoire des sciences*, 1947) and few academic positions. George Sarton, for example, who was the progenitor of the field in the U.S., never had a regular position at Harvard although he spent his entire American career there. Rather, he was a Research Associate in the Harvard Department of History, his financial support coming from the Carnegie Institution (19). The first American to receive a Ph.D. in the history of science was I. B. Cohen, who received his degree at Harvard in 1947 in the Program in the History of Science and Learning instituted by James Bryant Conant in 1936 (20).

Mention of Conant suggests that chemists—and the history of chemistry—were important in the genesis of the history of science as an academic discipline. The next history of science program to be officially instituted was at the University of Wisconsin in 1941 and the young scholar, Henry Guerlac, was invited to form it up (21). Guerlac had majored in chemistry at Cornell and done graduate work there and at Harvard in chemistry before switching to earn a Ph.D. in European history. Although not technically an alumnus of the Harvard history of science program, he had done coursework with George Sarton and a dissertation on science and the military school at Mézieres in the *ancien régime*.

Guerlac started up the department and then left in 1943. He did not return after the war but, instead, was hired by his alma mater, Cornell, to begin history of science there. By the late 1950s, Cornell had a flourishing program and by the 1960s, his graduate students were pursuing research on the subject on which he had come

to focus in his own research: Lavoisier and the Chemical Revolution.

At Wisconsin, the history of science program was resumed after the war. At the same time, a young chemist, Aaron Ihde, returned to his alma mater, Wisconsin, to accept a tenure-track position in the chemistry department. In 1946, Ihde was able to manifest his interest in the history of chemistry by instituting (or better resurrecting) an undergraduate course. By the end of the 1940s, Ihde was playing a central role in developing an undergraduate liberal arts education at Wisconsin and emphasizing the history of science.

Ihde strengthened his connection with the history of science (and history of chemistry) by spending the year 1951-52 at Harvard teaching in a course Conant had instituted on "case histories in experimental science" co-taught with the chemist, Leonard K. Nash, and Thomas Kuhn. Although he apparently was not formally added to the faculty of the history of science program until 1957, he in fact was the mentor for the first Ph.D. completed in that program; Robert Siegfried received his degree with a dissertation, appropriately enough in the history of chemistry, in 1953. Of course, it has also to be mentioned that Wisconsin was concurrently developing the leading program in the nation in the history of pharmacy.

So the 1950s can with a good deal of justice be considered the first decade when history of science emerged as an academic discipline. It was also, of course, the decade when the Dexter Awards began. I doubt that anyone then was conscious of a connection but we can certainly make one now: the development of the academic discipline of the history of science was heavily influenced by, indebted to, and focused on the history of chemistry. The tradition of chemist-historians continued and would be joined by the first group of historians of science with a focus in the history of chemistry. By the early 1960s, Cornell, under the tutelage of Henry Guerlac, and the University of Wisconsin, under the leadership of Aaron Ihde, emerged as centers for training "historians of chemistry." By the early 1970s, these universities had been joined by the University of Pennsylvania's Department of the History and Sociology of Science founded by Arnold Thackray. I should mention that a parallel development was taking place in England, particularly at University College London, where Douglas McKie joined the nascent Department in the History and Method of Science in 1925, remaining one of only two permanent department members until 1946. McKie's biography of Lavoisier appeared in 1935 and monographs of some of

his students on topics in eighteenth-century chemistry also were published already in the 1930s (22).

1950s and 1960s: "Heroic Age" of the History of Chemistry.

In the 1950s and 1960s, the influence of professional history of science began to become evident in the history of chemistry. I shall discuss a number of seminal publications of these decades.

The Historical Background of Chemistry by the biochemist, Henry M. Leicester (1956) (23). In my terminology, Leicester was a **chemist-historian**.

Looking over this book now, in relationship to Venable's venerable *History of Chemistry*, there are interesting parallels and differences. The major parallel is the topic and period covered, which are roughly the same despite the over fifty years that separated the two histories. Both works, moreover, give much more attention to the development of inorganic chemistry than organic, even in the nineteenth century.

But there are significant differences, which come out in Leicester's preface. Firstly, although a **chemist-historian**, Leicester recognized the professional changes then taking place, Leicester naturally employed the term, "historians of science." Moreover, his story was no longer primarily one of "failures" and "triumphs" of individual men, as it was for Venable but rather:

It is clear that the full story of such developments involves not only the personalities and intellects of the scientists themselves, but also the social and economic conditions which surrounded them and the philosophical ideas to which they are exposed.

He noted that such a comprehensive history would involve a massive project but also, "As yet, such a volume is lacking in the history of chemistry." Leicester did not propose to carry out the project in this work but rather "the development and interrelation of chemical concepts." (24)

A good marker of their differences in historical perspectives is found in their treatment of the phlogiston theory. Venable rather irritably dismisses it (25): "Since the theory was false, it obscured or twisted facts and necessarily retarded progress." Leicester, by contrast, gives a much more nuanced and positive account (26): "In this field [combustion], the phlogiston theory supplied an excellent explanation for the known facts." (27)

Harvard Case Histories in Experimental Science, James Bryant Conant, General Editor, Leonard K. Nash, Associate Editor (1957) (28). Conant and Nash were **chemist-historians**.

This two volume set the fruit of the course that Conant had instituted and with which Ihde had been associated in the early 1950s. In aim, the publication is not unlike Venable's: to enlighten the science student and the general public about the nature of science.

But this work deeply reflected the new perspectives of the history of science in much the ways that Leicester's book had. A general conceptual structure was established at the outset, which, while holding to an empiricist and progressivist view of scientific change, nevertheless took full cognizance of the complexities involved in the origins and ascendancy of new scientific concepts. Moreover, each case study was heavily interlarded with quotations and sometimes long excerpts from original sources to give the reader a real historical flavor of the narrative.

Not surprisingly given the two editors, four of the eight case studies can be said to fall in the domain of the history of chemistry (29). At least two ("Overthrow of the Phlogiston Theory" and "The Atomic-Molecular Theory") still regularly show up in history of science course syllabi, and the "Overthrow of the Phlogiston Theory" became the basis for the treatment of the Chemical Revolution in Thomas Kuhn's *The Structure of Scientific Revolutions*.

Lavoisier—the Crucial Year: The Background and Origin of his First Experiments on Combustion (1961) by Henry Guerlac (30), **historian of chemistry**

Although there were other important Lavoisier scholars before Guerlac and contemporaneous with him (McKie, Maurice Daumas), this book was a defining one in the development of the history of science generally because it represented the archival-based, detailed studies of science that came to dominate the history of science in the 1960s. Moreover, it was an interrogation of "origins;" scientific "advance" could no longer be assumed to be natural or inevitable but, like all historical events, had to be thoroughly explained. Guerlac, through his students, dominated the history of chemistry in the 1960s and 1970s, with the focus being on Lavoisier and his work.

The Development of Modern Chemistry (1964) by Aaron Ihde (31), **chemist-historian (?) historian of chemistry (?)**

It is difficult to know how to categorize Aaron Ihde—as a chemist-historian or as primarily an historian of chemistry. In *his* preface, he articulates something like Leicester's vision of what the history of chemistry should be, emphasizing especially its relationship to industry:

I have sought to give proper attention to the part played by individuals without making the account a series of biographical sketches. At the same time I have attempted to place chemistry in the framework of the times. It has influenced human life in major ways, particularly in the nature of industrial and agricultural activity. At the same time, the growth of chemistry has been influenced by human affairs—political, economic, and social. These interactions I have sought to reveal. (32)

Moreover, in contrast to Leicester, Ihde carried his historical narrative down well into the twentieth century. In the twentieth-century sections, Ihde covered not only the substance of chemistry but industrial and agricultural chemistry as well. Moreover, indicative of what soon would be rising environmental concerns, Ihde concluded his massive history with a section titled "Nonprofessional Problems Created by Chemistry." Here, he discussed various kinds of environmental problems and hazards created by chemical activities: nuclear waste, industrial waste and hazards, environmental chemical hazards. He began this section with eloquent and prophetic words:

These problems demand the best wisdom of the world's leaders, and they will be resolved only very gradually, even where there is good will and a sincere desire for their solution. Chemists can help in their solution but will need the collaboration of the best minds in many other fields. Perhaps chemists can be of greatest service if they will become more conscious of the results of their activities and use their influence to delay the introduction of new products and new processes until they can be sure the advantages outweigh the disadvantages. (33)

The Development of Modern Chemistry was, in an important way, Janus-faced. Along with Partington's multi-volume set, it was the last and most ambitious of the synoptic narratives of history of chemistry. Yet it did represent the beginnings of a different kind of history of chemistry. Although its core was the history of chemical theories, there were now considerations of chemical education, industrial chemistry, and environmental and ecological problems.

Conclusion

Although only one of these five authors was clearly an **historian of chemistry**, all the others played important roles in the development of the history of science. Given the comprehensive influence of the history of science on all of them, and the obvious blurring of my categories for most of them, I might now want to declare the question, “Do historians or chemists write better history of chemistry?” to be irrelevant and non-informative.

What I can say as an historian—and an historian of science—is that Guerlac’s *Lavoisier—the Crucial Year* became paradigmatic of the nature and style that monographs in the history of science were to assume, and this included those in the history of chemistry.

By contrast, Leicester’s and Ihde’s histories represent the last and possibly the greatest exemplars of a genre of historical writing practiced by **chemist-historians**: the general narrative of the history of this scientific discipline (34).

The decades when these works were published (and perhaps the 1970s as well) marked the high point in the productions of works in the history of chemistry—by both **chemist-historians** and **historians of chemistry** as I have defined them. Particularly in the history of science community in America, which I know best, history of chemistry moved from centrality in the 1960s to a much more marginal position by the 1980s, and many of the actors of the earlier decades moved on to research in other fields. It is difficult to say why. Possibilities include the negative image that chemistry bore by the 1970s, the fact that increasing numbers of young scholars entering the history of science lacked the technical knowledge of their forebears (necessary in pursuing history of chemistry), and perhaps the fact that chemistry did not deal with the increasingly fashionable “existential” issues of our origins, destiny, and purpose as did biology (and geology), physics, and psychology. But these are speculations.

Among the small but intrepid cadre of **chemist-historians** in HIST, interest in the history of chemistry was maintained and, with the appearance of the Beckman Center for the History of chemistry and its successor, the Chemical Heritage Foundation, the history of chemistry has at last found an institutional home.

In recent years, there has been a resurgence of interest in the history of chemistry but the foci of research are somewhat different from earlier decades. Firstly, there is now an interest in very recent developments, such as molecular biology and genomics. Secondly, at the other

temporal end, there has been a real increase [was “take up”] in the study of alchemy or “chymistry” of the sixteenth and seventeenth centuries. Thirdly, there is a good deal of interest in the history of the development of industrial research in chemistry and chemical industry. Finally, and more generally, scholars are interested in chemistry as “material culture:” the pursuit of chemically-related crafts such as pharmacy, metallurgy, and the making of products such as perfumes.

All of these foci in one way or another emphasize a feature of chemistry that often was underplayed in earlier writing both of chemist-historians and historians of chemistry: chemistry has always been as much about the making of products as it has been about discovering and scientifically explaining natural knowledge. It has always contained both craft and scientific components. In contemporary research in the history of chemistry, the science of chemistry is being recognized in its full extent.

References and Notes

1. Presented at the 233rd National Meeting of the American Chemical Society, Chicago, IL, March 27, 2007, HIST 29.
2. H. Kopp, *Geschichte der Chemie*, 4 Vols., F. Vieweg und Sohn, Braunschweig, 1843-1847.
3. J. R. Partington, *A History of Chemistry*, 4 Vols., Macmillan, London, 1961-1970.
4. T. Thomson, *The History of Chemistry*, 2 Vols., Henry Colburn and Richard Bentley, London, 1830-1831.
5. A. Ladenburg, *Vorträge über die Entwicklungsgeschichte der Chemie in den letzten hundert Jahren*, F. Vieweg und Sohn, Braunschweig, 1869.
6. J.-B. Dumas and E. Grimaux, *Oeuvres de Lavoisier*, 6 Vols., Imprimerie imperial, Paris, 1862-1893.
7. M. Berthelot, *Les origines de l'alchimie*, G. Steinheil, Paris, 1885, is one example.
8. P.-M.-M. Duhem, *Le mixte et la combinaison chimique: Essai sur l'évolution d'une idée*, C. Naud, Paris, 1902.
9. I. Freund, *The Study of Chemical Composition: An Account of Its Method and Historical Development*, Cambridge University Press, Cambridge, England, 1904.
10. M. B. Ogilvie, “Freund, Ida (1863-1914), chemist,” Oxford Dictionary of National Biography, <http://www.oxforddnb.com/index/101048490/Ida-Freund>. There has recently been an uptake of interest in Freund. See B. Palmer, “Ida Freund: Teacher, Educator, Feminist, and Chemistry Textbook Writer,” *IPSI BGD Transactions on Internet Research*, 2007, 3, 49-54, <http://www.internet-journals.net/journals/tir/2007/July/Paper%2012.pdf>; M. Hill and A. Dronsfield, “Ida Freund, the First Woman Chemistry Lecturer,” *Roy. Soc. Chem. Historical Group Newsletter*, January 2011, <http://www.chem.qmul.ac.uk/rschg/Newsletter/NL1101.pdf>.

11. M. Bursey, *Francis Preston Venable of the University of North Carolina*, Chapel Hill Historical Society, Chapel Hill, North Carolina, 1989.
12. F. P. Venable, *A Short History of Chemistry*, D. C. Heath, Boston, 1894 [1901]; *The Development of the Periodic Law*, Chemical Publishing Company, Easton, Pennsylvania, 1896; *History of Chemistry*, D. C. Heath, Boston, 1922.
13. Venable, "Preface," *A Short History of Chemistry* (1901), no pagination. The preface is dated in the text as June, 1894.
14. Venable, "Preface," *History of Chemistry*, no pagination. The preface is dated in the text as June, 1922. I used the London edition (same date and publisher).
15. Venable, "Prefatory Sketch," *The Development of the Periodic Law*, 1.
16. T. L. Davis, *The Chemistry of Powder and Explosives*, Wiley, New York, 1943.
17. Her major works in the history of chemistry were: *La genèse de la science des cristaux*, Alcan, Paris, 1918; *Les doctrines chimiques en France du début du XVII^e à la fin du XVIII^e siècle*, Presses Universitaires de France, Paris, 1923; *Newton, Stahl, Boerhaave et la doctrine chimique*, Alcan, Paris, 1930. For recent scholarship, see G. Freudenthal, Ed., *Études sur Hélène Metzger*, Brill, Leiden, 1990.
18. T. S. Kuhn, "Preface," *The Structure of Scientific Revolutions*, 3rd edition, University of Chicago Press, Chicago, 1996, viii.
19. For a thoughtful article on Sarton and his role in developing the history of science as a professional field, see A. Thackray and R. K. Merton, "On Discipline Building: The Paradoxes of George Sarton," *Isis*, **1972**, 63, 472-495. For an entire issue of *Isis* devoted to Sarton and the development of the history of science in the United States, see: "Sarton, Science, and History," *Isis*, **1984**, 75, 1-240.
20. J. Dauben, M. L. Gleason, G. E. Smith, "Seven Decades of History of Science: I. Bernard Cohen (1914-2003), Second Editor of *Isis*," *Isis*, **2009**, 100, 4-35.
21. The history of the inception and development of the Wisconsin program is based on V. L. Hiltz, "History of Science at the University of Wisconsin," *Isis*, **1984**, 75, 63-94. Before 1940, Edward Kremers (1864-1941) had promoted the teaching of the history of pharmacy and professional activities in the history of science generally. For Henry Guerlac, see M. Boas Hall, "Eloge: Henry Guerlac, 10 June 1910-29 May 1985," *Isis*, **1986**, 77, 504-506.
22. W. A. Smeaton, "History of Science at University College London, 1919-1947," *Brit. J. Hist. Sci.*, **1997**, 30, 25-28.
23. H. M. Leicester, *The Historical Background of Chemistry*, Science Editions, Wiley, New York, 1965 [first edition, 1956].
24. Leicester, "Preface," *Historical Background*, v.
25. Venable, *History of Chemistry*, 34.
26. Leicester, *Historical Background*, 123.
27. I have not treated J. R. Partington in this article but I will make a comparative reference to his *A Short History of Chemistry* here. This work has the virtue of attention to primary sources, quoted extensively. I would judge its evaluation of the phlogiston theory to be mid-way between that of Venable and Leicester: "Stahl inverted the true theory of combustion and calcination.... He neglected the quantitative aspects of chemical change, disregarded what was known of gases, and paid little attention to atomic theory. On the other hand,... his theory linked together a large number of facts into a coherent body of false doctrine, suggested new experiments, and led to discoveries." J. R. Partington, *A Short History of Chemistry*, 3rd ed., Macmillan, 1957 [1965 reprint], London, 88.
28. J. B. Conant and L. K. Nash. Eds., *Harvard Case Histories in Experimental Science*, 2 vols., Harvard University Press, Cambridge, MA, 1957.
29. Case 2: "The Overthrow of the Phlogiston Theory" (Conant); Case 4: "The Atomic-Molecular Theory" (Nash); Case 5: "Plants and the Atmosphere" (Nash); Case 6: "Pasteur's Study of Fermentation" (Conant).
30. H. Guerlac, *Lavoisier—the Crucial Year: The Background and Origin of his First Experiments on Combustion*, Cornell University Press, Ithaca 1961.
31. A. J. Ihde, *The Development of Modern Chemistry*, Harper & Row, New York, 1964.
32. Ihde, *Development*, xi.
33. Ihde, *Development*, 733.
34. Not quite the last; a major general narrative of the history of chemistry, written by a distinguished historian of chemistry, appeared in 1992: W. H. Brock, *The Fontana History of Chemistry*, Fontana Press, London, 1992, published in the US as *The Norton History of Chemistry*, W. W. Norton & Co., New York, 1993.

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CHEMISTRY IN ENGLISH ACADEMIC GIRLS' SCHOOLS, 1880-1930

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Introduction

Accounts of curricula in English girls' schools in the late nineteenth and early twentieth century rarely mention science, particularly chemistry. Science is not addressed in either of Kamm's books: *Hope Deferred: Girls' Education in English History* (1) and *Indicative Past: A Hundred Years of the Girls' Public Day School Trust* (2). In Turner's *Equality for Some: The Story of Girls' Education*, he remarks (3): "Even in schools where science was taught, it was unusual for a pupil to acquire an adequate grounding for advanced study." In Avery's *The Best Type of Girl: A History of Girls' Independent Schools* (4), the comment is made: "In the privately owned schools there was on the whole a marked absence of science before the 1950s," while subsequent discussion in the book supported that thesis. Thus there is a clear implication that science (including chemistry), was of marginal relevance to English girls' schools until the mid-twentieth century.

An exception to this viewpoint was given by Manthorpe in a chapter in Walford's *Private Schooling of Girls: Past and Present*. Her focus was on the socio-historical perspective, rather than on the science. However, in the concluding points, Manthorpe notes (5):

The North London Collegiate School for Girls acted as a model for many of those new schools. From the beginning, science education was included in the curriculum of these schools, and most often one or more branches of the physical sciences were taught as well as mathematics.

The widely-accepted view of a lack of grade-school science education for girls was also at variance with the backgrounds and life-stories of early-twentieth century British women chemists in our own research (6).

Methodology of the Study

To confirm our contentions, we have extended our studies and report here on our research. The time period which we explored was approximately 1880 to 1930. Our starting point was the published histories of the individual academically-oriented schools. Though most schools made no more than a passing reference to science facilities, a few, such as the two histories of King Edward VI High School for Girls, Birmingham, and that of St. Swithun's School, made especial note of science teaching at the respective schools. A second source of information was the contemporary magazine, *Girl's Realm*, which published a series of articles titled "Famous Girl's Schools" and these, too, noted the science facilities at some of the schools (accessed at the British Library). A third source was that of contemporary education journals, in particular, *Journal of Education* and *School World* (accessed at the University of Cambridge Main Library).

The fourth, and most interesting, source was that of high school student magazines. These were typeset and often published from the school's inception. From our searches, these magazines are not generally available, the sole surviving set of issues often being held in the

specific school. Over the period of interest (1880-1930), such magazines tended to be very “academic” with club and society reports and students’ accounts of their lives, travels, and experiences. (In subsequent decades, such magazines became literary-oriented with fiction, poems, and so on.)

In our analysis of women who became an Associate or Fellow of the Royal Institute of Chemistry or a Fellow of the Chemical Society (6), a notable proportion came from certain high schools. Of the schools which produced the most women chemists to-be, we ascertained that six had archives containing a complete set of their student magazines for our period of study. These were: King Edward VI High School for Girls, Birmingham (*The Phoenix*); North London Collegiate (*Our Magazine: North London Collegiate School for Girls*); Cheltenham Ladies College (*Cheltenham Ladies’ College Magazine*); Manchester High School for Girls (*The Magazine of the Manchester High School*); Croydon High School for Girls (*Croydon High School Magazine*); and Mary Datchelor School, East London (*Datchelor School Magazine*). We are grateful to each of the first five schools for access to their archives and also to the Clothmakers Guild, for access to the archives of the long-closed Mary Datchelor School.

The Science Education of Girls

It was in 1869 that the feminist educator, Lydia Becker, made the case for the education of middle-class girls in science (7):

... many [married middle-class] women might be saved from the evil of the life of intellectual vacuity, to which their present position renders them so peculiarly liable, if they had a thorough training in some branch of science, and the opportunity of carrying it on as a serious pursuit.

By the 1880s and 1890s, chemistry was being discussed as a specific component of a middle-class English girl’s liberal education. As an example, in an 1884 article on science teaching in girls’ schools published in the *Journal of Education*, the anonymous author stated (8):

With a small amount of material, and with no more space than is afforded by an ordinary school-room, much may be done to make Chemistry, as it should be, the basis of all Natural Science teaching.

Where a chemical laboratory, however small can be obtained, it becomes possible, as well as desirable, that Chemistry should be taught more thoroughly and practically.

Take, for instance, such a course of Chemistry as that prescribed for the London Matriculation Examination. The facts and phenomena should be taught first by the aid of experiments performed by the teacher. This should then be followed by practical lessons, in which the pupils themselves perform the experiments. The simple gases – Oxygen, Nitrogen, Hydrogen, &c. – can be prepared by a class of pupils without difficulty in a moderately sized laboratory, and students who have themselves actually performed such experiments acquire a knowledge of the laws of chemical reaction, and of the properties and constitution of matter which would be impossible without such means.

In a definitive study of education for girls published in 1898, there was a chapter on *The Teaching of Chemistry* by the woman chemist, Clare de Brereton Evans (9). In that chapter, Evans argued that junior, as well as senior, girls needed exposure to practical chemistry:

For success in examinations it is now necessary to have a certain amount of practical knowledge of chemistry and examination classes are therefore given some practical training, but this reform still remains to be extended universally to the junior classes, which need even more than the senior ones that the teaching should be objective: a child may learn and repeat correctly a dozen times that water is composed of oxygen and hydrogen, and the thirteenth time she will assure you that its constituents are oxygen and nitrogen; but let her make the gases herself, test them and get to know them as individuals, and mistakes of this kind will become impossible.

Queen’s College, Harley Street, founded in 1847 (10), seems to have played a pivotal role, directly and indirectly, in the wider acceptance of science as part of a girl’s education. Though the initial focus was on the secondary education of governesses, the aims grew rapidly broader as the nineteenth century progressed. In particular, the Queen’s College curricula from 1848 to 1868 (11) contained a course on Natural Philosophy which included a chemistry section covering the facts and classifications of chemistry, illustrated by experiments performed by a University chemistry lecturer.

One of the many Queen’s College graduates to attain fame was the educator Frances Buss (12). It was arguably her experience at Queen’s College that led her to include a very strong science program when she subsequently founded the North London Collegiate School. Buss included chemistry in the curriculum, as was noted by Watson, the school’s biographer (13):

Robert Buss [Frances Buss’s father] made a memorable science teacher as Annie Martinelli, an early

pupil later remembered: 'His talents were simply wonderful. His Chemistry series was marvellous, especially for smells and explosions.'

The turning point for the wider population was the Report of the Schools Inquiry Commission (the Taunton Commission) of 1868 (14) which provided, in general, a damning indictment of girls' education in England. In the Report, Professor T. H. Green, Assistant Commissioner for Birmingham, recommended that a girls' school was needed on the outskirts of every considerable town which would give girls an education similar to that provided in the best boys' grammar schools. Responding to this proposal, the Shirreff sisters (15) organized the "National Union for the Improvement of the Education of Women of All Classes" which led to the organization of the Girls' Public Day Schools Company (GPDSC). Under the auspices of the GPDSC, girls' schools were founded across England during the 1870-1890 period, with the majority being in the greater-London area. These schools were modeled on the North London Collegiate School and hence incorporated science as part of the curriculum. Other independent girls' schools also introduced strong science programs (16), including Manchester High School for Girls and King Edward VI High School for Girls, Birmingham.

Chemistry Teachers

We contend that it was a generation of powerful Headmistresses (17) who promoted science at their schools and, in the process, hired some highly qualified women chemistry teachers. For example, Buss hired Grace Heath (18) as the science teacher at North London Collegiate School in 1888. Heath had been the first woman chemistry student with the famous British chemist, Henry Armstrong, at the Central Technical College (later part of Imperial College, University of London). Sadly, Heath died in 1895 before she was 30 years old. Buss's successor as Headmistress, Sophia (Sophie) Willcock (Mrs. Bryant), was determined to maintain the School's science reputation. Willcock hired Rose Stern (19). Stern had been educated at King Edward VI School and, while there, had been elected as the first woman Student Member of the Institute of Chemistry.

At King Edward VI School, the first Head was Edith Elizabeth Maria Creak. The School biographer, Winifred Vardy, noted (20): "To Miss Creak belongs the honour of being a pioneer in the teaching of science to girls. Though her own training [at Newnham College, Cambridge University] had been mathematical and clas-

sical, she seems to have foreseen the value of scientific knowledge for women." Creak hired two dedicated and enthusiastic science staff, Lizzie Davison and Alice Celia Slater, upon the School's opening in 1883. Like Creak, they were both Newnham graduates. Another School biographer, Rachel Waterhouse, commented (21):

Miss Davidson and Miss Slater took charge of science, each stayed for thirty-one years at the School, and to them belongs almost all the credit for the great scientific successes achieved by Edwardians during the whole of that period.

While Vardy quoted a former student (20):

Miss Davison also used to take the XIIIth Class, little girls of 8 to 10, in the principal gases. "She did all the demonstrations, which according to modern ideas was bad," writes a pupil, "but she made it so interesting that I used to be impelled to tell my small brother all about it each week, and what she taught us *sticks*."

Buss's long-time friend, Dorothea Beale (22), also a graduate of Queen's College, became the second Principal of Cheltenham Ladies' College. Beale made an equally inspired choice of hiring Millicent Taylor (23). Taylor had been a student at Cheltenham Ladies' College, returning as chemistry teacher upon completing her B.Sc. in chemistry at University College, Bristol (later the University of Bristol). Croydon High School likewise hired one of its own outstanding former students, Kathleen Mary Leeds (24). Leeds had been one of the first women chemistry graduates from the prestigious Royal College of Science (also later part of Imperial College, University of London) but, like Heath, Leeds died young, in 1921 at age 26.

It was the second Principal of Manchester High School, Sara Burstall, who emphasised science at that school. Burstall commented that by the 1920s (25): "We had ... four specialist teachers on the staff, all first class honours graduates in chemistry, physics, botany and zoology, and many Old Girls were students in universities or science graduates".

At Leeds Girls' High School, the first Head Mistress, Catherine Kennedy, was so keen for her girls to take chemistry that she arranged in 1876 to take them to the Yorkshire College of Science (later the University of Leeds) where they were taught by Professor Edward Thorpe (26). One of the six girls that year won a prize, and others were equally successful in subsequent years. The arrangement continued until science laboratories were added to the school in 1883.

A significant number of girls were studying chemistry at these science-active schools. For example, at Mary Datchelor School in East London, an issue of the *Datchelor School Magazine* of 1901 proudly reproduced the Report of the chemistry section of the high school examination administered by Cambridge University (27): "In the Upper VI. [class] the section on Physical Chemistry studied had been thoroughly mastered, and a large number of girls scored over 90 per cent of the marks." Some of these students proceeded to university to study chemistry. For example, in 1908, two students from Mary Datchelor School entered the (women's) Royal Holloway College of the University of London, to study chemistry (28): "... She [Edith Hancock] is entering for an Honours Degree in Science, taking Chemistry as her special subject. Phœbe [Routh] is also reading for Honours in Chemistry ..."

School Chemistry and Science Clubs

We found that some of the science-active schools had student chemistry clubs or science clubs with chemistry sections. North London Collegiate School (NLCS) had a Chemistry Club from the early part of the twentieth century. The School had a hand-written magazine, illustrated with glued-in photographs. Called *The Searchlight: NLCS Student Magazine for Science*, it gave a record of Chemistry Club activities. The social events were also reported in the (printed) school magazine, *Our Magazine: North London Collegiate School for Girls*, such as (29): "On Thursday, July 11th [1912], Miss Stern, Miss Drummond [junior chemistry teacher] and the Science Sixth gave a party in the Old Laboratory. We drank tea out of beakers, and stirred it with long glass wands. ..."

We know more about the Science Club at King Edward VI School, which was not formed until 1923 and survived through the remainder of the 1920s. It had a strong chemistry focus, with the first meeting involving the reading of papers on the famous early chemists, Priestley, Scheele, Cavendish, and Lavoisier (30). At a meeting in 1928, students gave presentations on the topics of "Chemistry in the Service of Man," "Industrial Chemistry," "Flame and Fuel," and "Synthetic Chemistry" (31). While in 1929, two students gave "Chemical Magic" demonstrations. The student magazine, *The Phoenix*, reported (32):

They succeeded, among other things, in 'Turning water into wine,' producing a miniature snow-storm, and charming a beautiful serpent out of an ordinary crucible. Judging by the inquiries afterwards as to

how they did it, this last feat seems to have been their greatest triumph.

School Chemistry Laboratories

The construction of a chemistry laboratory was a mark that the girls' school was serious about the subject. Several of the articles on famous girls' schools in *Girls' Realm* highlighted the chemistry laboratory, often with a photograph, and usually showing sophisticated glass distillation apparatus and earnest-looking girls performing chemical analysis. For example, in a report on Mary Datchelor School, Whyte, a regular contributor to *Girls' Realm*, wrote in 1901 (33): "Upstairs two well-equipped laboratories for chemistry and physics are included amongst the class-rooms."

Other contemporary reviewers of prominent girls' schools were equally sure to mention the chemistry laboratories. In an article in *Girls' Realm* in 1900 on North London Collegiate School, Hill reported (34): "Beyond is a chemical laboratory well fitted up and large enough for twenty-four girls to work together at one time." The construction of science laboratories at Cheltenham Ladies' College was one of Beale's priorities. This was accomplished as noted in a description of the school in an 1899 issue of *Girls' Realm* (35): "in the Science Department there is a laboratory for physics and two for chemistry ..." while a new Science wing was added in 1904. A report in *School World* (36) on the new laboratories described in detail the dedicated chemistry lecture room with a fully-equipped demonstration bench, an elementary chemistry laboratory, an advanced chemistry laboratory, a chemical preparation room, two storerooms for chemicals and equipment, and a chemistry study room (though it should be noted these extensive facilities were also used for an external B.Sc.(London) in chemistry offered at the College).

St. Swithun's School acquired a chemistry laboratory in 1895, following a tour by the Administrative Committee during which students deliberately left an open flask releasing chlorine gas in a classroom to highlight their lack of facilities. The hazards of practical chemistry seem to have been taken as an integral part of the work. At St. Swithun's, one of the students taking the chemistry practical examination in 1897 reminisced (37):

In those days a 'don' was in charge in cap and gown. An enterprising examiner had given red phosphorus as the unknown substance. About ten minutes after we had commenced a nervous candidate dropped a glowing match on the 'unknown'—result, a wild

flare and we all 'knew'. Hardly had the invigilator extinguished this when it was discovered that a pile of dusters was on fire; this in turn was extinguished. Then suddenly the bottom came out of a medicine bottle improvised to contain sodium hydrate, devastating a varnished table and all the candidates' papers. Wearily our friend came for a third time to the rescue, remarking, 'My life is insured—I only hope yours are!'

While in her report for *Girl's Realm* in 1901 on Bedford High School, Whyte implied that the inherent dangers of practical chemistry were a valuable part of the educational experience (38):

Practical chemistry is among the best modern educational improvements. It teaches things which go much deeper into our consciousness than mere words could ever go. It teaches consequences—the stern, certain consequences of doing quite the right or the wrong thing. It never makes a mistake, or slurs over a little bit of carelessness, or pretends everything is right when everything is quite the reverse. And for girls who have to go through with life, it is not a bad thing to learn when young to expect the natural consequences of an action, even to the correct or incorrect testing of a compound or simple liquid.

Some of the school chemistry laboratories seem to have been of high quality. A group of students at the Manchester School for Girls visited Cambridge University in 1901 and pronounced (39):

The Newnham Chemistry Laboratory was inspected on Monday morning, but was agreed to compare very unfavourably with the Chemical Laboratory of the Manchester High School, whatever the standard of work might be.

The End of an Era

The first two decades of the twentieth century was marked by a debate whether "real" science or domestic science was more appropriate for a girls education (40). The academically-oriented independent girls' schools, such as the GPDSC schools, seemed to have been immune to the issue. This rise of domestic science seems to have been more of an influence on state schools, particularly those teaching girls from the "lower classes." Nevertheless, the fervor for science education for girls seems to have abated by the 1930s.

As early as 1912, the Headmistress of Sacred Heart School, Hammersmith, described how the educational reforms of the later decades of the nineteenth century had emphasized the teaching of natural science. Nevertheless, in her view, the enthusiasm for laboratory science for girls was coming to an end. She added (41):

So laboratories were fitted up at great expense, and teachers with university degrees were sought after. The height of the tide seemed to be reached in 1904 and 1905... Then disillusion seems to have set in and the tide began to ebb. It appeared that the results were small and poor in proportion to expectation and to the outlay on laboratories. ... The links between this teaching and after life did not seem to be satisfactorily established.

Yet this explanation has to be considered in context. By the 1930s, attitudes to women in science had changed (42). Those charismatic pioneer Headmistresses, fired with the fervor of the need to match or exceed boys' schools and to provide a springboard to university, had retired or died (43). As Hunt has commented (44):

In the 1920s and 1930s it was fashionable to accuse girls' secondary schools of neglecting the 'feminine' side of their pupils' development. The Victorian pioneers (and Miss Buss and Miss Beale were frequently cited on these occasions) were supposed to have adopted a model of 'liberal education' and in doing so had 'assimilated' the 'boys' curriculum' and ignored the needs of femininity in their schools. The result, said the accusers, was that girls' education was a 'slavish imitation' of boys' (and by definition, therefore, inappropriate for girls).

To the new pragmatic generation of Headmistresses, chemistry and its associated laboratory work was no longer the high-priority item that it had once been. It is therefore no wonder that we found that the late 1920s-early 1930s period also coincided with the demise of most of the science and chemistry clubs at girls' schools.

Commentary

We have endeavored to provide evidence that chemistry was a significant component of the curriculum at some academically high-achieving English girls' schools in the late 19th and early 20th centuries. In our view, the impetus came from forceful Headmistresses who saw science, and chemistry in particular, as being crucial to their goal of the schools' recognition for academic excellence. To this end, they hired some of the earliest—and most outstanding—women chemistry graduates. Some of the schools possessed modern chemistry laboratories and at several schools, enthusiastic students founded chemistry clubs or science clubs with chemistry sections to pursue their interests. As described above, the enthusiasm declined by the late 1920s and chemistry at girls' schools appears to have waned.

References

- J. Kamm, *Hope Deferred: Girls' Education in English History*, Methuen & Co., London, 1965.
- J. Kamm, *Indicative Past: A Hundred Years of the Girls' Public Day School Trust*, George Allen & Unwin, London, 1971.
- B. Turner, *Equality for Some: the Story of Girls' Education*, Ward Lock Educational, London, 1974.
- G. Avery, *The Best Type of Girl: A History of Girls' Independent Schools*, André Deutsch, London, 1991, 254.
- C. Manthorpe, "Science Education for the Public Schools for Girls in the Late Nineteenth Century," in G. Walford, Ed., *The Private Schooling of Girls: Past and Present*, Woburn Press, Portland, OR, 1993, 56-78.
- M. F. Rayner-Canham and G. W. Rayner-Canham, *Chemistry Was Their Life: Pioneer British Women Chemists, 1880-1949*, Imperial College Press, Singapore, 2008.
- L. E. Becker, "On the Study of Science by Women," *Contemporary Rev.*, **1869**, 10, 386-404.
- "Science Teaching in our Public Schools, IX. Girl's High Schools," *J. Educ.*, **1884**, 6(new series), 352-353.
- C. de Brereton Evans, "The Teaching of Chemistry," in D. Beale, L. H. M. Soulsby, and J. F. Dove, Eds., *Work and Play in Girls' Schools by Three Head Mistresses*, Longmans, Green, and Co., London, 1898, 310.
- R. G. Grylls, *Queen's College 1848-1948*, Routledge & Sons, London, 1948, 16.
- S. C. Gordon, "Studies at Queen's College, Harley Street, 1848-1868," *Brit. J. Educ. Studies*, **1955**, 3(2), 144-154.
- J. Kamm, *How Different from Us: a Biography of Miss Buss and Miss Beale*, Bodley Head, London, 1958, 237.
- N. Watson, *And Their Works do Follow Them: the Story of North London Collegiate School*, James and James, London, 2000, 16.
- S. Fletcher, *Feminists and Bureaucrats: a Study in the Development of Girls' Education in the Nineteenth Century*, Cambridge University Press, Cambridge, 1980, 19.
- E. W. Ellsworth, *Liberators of the Female Mind: the Shirreff Sisters, Educational Reform and the Women's Movement*, Greenwood Press, Westport, CT, 1979.
- J. Howarth, "Public Schools, Safety-Nets and Educational Ladders: the Classification of Girls' Secondary Schools, 1880-1914," *Oxford Rev. Educ.*, **1985**, 11(1), 59-71.
- N. Glenday and M. Price, *Reluctant Revolutionaries: A Century of Headmistresses 1874-1974*, Pitman Publishing, London, 1974.
- "In Memoriam: Grace Heath," *Our Magazine: North London Collegiate School for Girls*, **1895** (20 July), 60.
- "Death," *North London Collegiate School Magazine*, **1954**, 3, 43.
- W. I. Vardy, *King Edward VI High School for Girls Birmingham 1883-1925*, Ernest Benn, London, 1928, 26.
- R. Waterhouse, *King Edward VI High School for Girls 1883-1983*, no pub., 1983, 23.
- E. Raikes, *Dorothea Beale of Cheltenham*, Archibald Constable & Co., London, 1908.
- W. Baker, "Millicent Taylor 1871-1960," *Proc. Chem. Soc. (London)*, 1962, 94; and 'D.T.', "In Memoriam: Millicent Taylor, D.Sc.," *Cheltenham Ladies' College Magazine*, 1961, 144.
- 'E. M. L.' and others, "In Memoriam: Kathleen Mary Leeds," *Croydon High School Magazine*, 1921 (July), (32), 1.
- S. A. Burstall, *Retrospect and Prospect: Sixty Years of Women's Education*, Longmans, Green, and Co., London, 1933.
- H. M. Jewell, *A School of Unusual Excellence: Leeds Girls' High School 1876-1976*, Leeds Girls' High School, Leeds, 1976, 47.
- "Headmistress's Summary of Examination Reports," *Datchelor School Magazine*, **1901**, 13(1), 16.
- 'C. E. R.', "A Look in at Holloway College," *Datchelor School Magazine*, **1908**, 22(3), 5.
- 'Notes by One Who Was There,' "Science Tea," *Our Magazine: North London Collegiate School for Girls*, **1912** (December), 89.
- M. W. Richards, "Science Club," *The Phoenix*, **1923**, Winter (31), 24.
- J. Forsyth, "Science Club Report," *The Phoenix*, **1928**, Summer (40), 22.
- M. W. Richards, "Science Club Report," *The Phoenix*, **1929**, Summer (42), 29.
- C. G. Whyte, "Famous Girls' Schools. XII – The Mary Datchelor School," *Girls' Realm*, **1901-02**, 4, 268.
- E. M. Hill, "The Frances Mary Buss' Schools," *Girl's Realm*, **1899-1900**, 2, 595.
- E. Raikes, "The Ladies' College, Cheltenham," *Girl's Realm*, **1899-1900**, 2, 121.
- M. Taylor, "The New Science Wing, the Ladies' College, Cheltenham," *School World*, **1905**, 7, 222.
- P. Bain, *St. Swithun's: A Centenary History*, Phillimore, Chichester, 1984, 10.
- C. G. Whyte, "Famous Girls' Schools. IX – The Bedford High School," *Girls' Realm*, **1900-01**, 3, 868.
- M. McNicol, "A School Journey to Cambridge," *Magazine of the Manchester High School*, **1901**, July, 48.
- C. Manthorpe, "Science or Domestic Science? The Struggle to Define an Appropriate Science Education for Girls in Early 20th Century England," *Hist. Educ.*, **1986**, 15, 195-213.
- J. E. Stuart, *The Education of Catholic Girls*, Longmans, Green and Co., London, 1914, 120.
- M. F. Rayner-Canham and G.W. Rayner-Canham, "Women in chemistry: participation during the early twentieth century," *J. Chem. Educ.*, **1996**, 73, 203-205.
- J. S. Pedersen, "Schoolmistresses and Headmistresses: Elites and Education in Nineteenth Century England," *J. Brit. Studies*, **1975**, 15(1), 135-162.
- F. Hunt, "Divided Aims: The Educational Implications of Opposing Ideologies in Girls' Secondary Schooling, 1850-1940," in F. Hunt, Ed., *Lessons for Life: the Schooling of Girls and Women, 1850-1950*, Blackwell, Oxford, 1987, 1.

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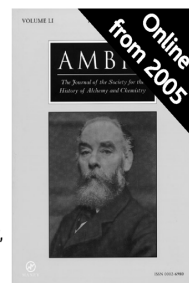
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HOW AN ANGLO-AMERICAN METHODOLOGY TOOK ROOT IN FRANCE

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Abstract

French organic chemistry had a strong nationalistic bent in the immediate aftermath to World War II. It continued to bask in the glow of the pre-World War I Nobel prize awarded jointly in 1912 to Victor Grignard and Paul Sabatier. In addition, the influence of the two mandarins then in power, Charles Prévost at the Sorbonne and Albert Kirmann, a Dean in Strasbourg who would be called upon as vice-director at the École normale supérieure in Paris, saw to it that the only theory of organic reactions, admissible in the classroom and in the laboratory, was Prévost's. As Mary Jo Nye has shown, a wall was erected against penetration of the ideas of the British school of Ingold and Hughes. Mechanistic chemistry, as was being vigorously studied by the contemporary Anglo-American physical organic chemists, was *persona non grata* in France. Publication by Bianca Tchoubar, in 1960, of *Les mécanismes réactionnels en chimie organique* opened a breach. The irony was for Dr. Tchoubar, a militant member of the Communist Party and a lady of fierce opinions, to have become a propagandist for the Anglo-American school of mechanistic studies. Truth for her overruled political propaganda. Her little book was revolutionary in the French context of the times. Together with the GECCO (*Groupe d'étude de chimie organique*) summer conferences pioneered by Guy Ourisson after his return from Harvard, it ushered in the new ideas.

This historical essay, based on an in-depth study of Tchoubar's book, will include a portrait of this remark-

able woman scientist. It will delve at some length into the renewal of French science initiated by De Gaulle's government after his return to power in 1958. The tension in the French scientific establishment of the sixties reflected two opposed versions of nationalism, the one conservative, Malthusian, inner-directed, the other forward-looking, eager for the recovery of national status, seeing a strong French science as a means for asserting national identity and independence from the two world power blocs.

Introduction

As I started my scientific career as an organic chemist in 1961, I learned by word of mouth that I urgently needed to get myself a copy of a little book. Why was that book thus advertised in somewhat hushed and conspiratorial tones? What was it about? Who wrote it? What purpose did it fulfill? My historical study aims at answering these questions.

French Science and its Insularity

My story is framed within French academic science at the beginning of the 1960s. To some extent the student revolt in 1968 aimed at casting off the straightjacket that French universities wore against change, and that students were determined to destroy. After World War II and for the next two decades, university professors had attempted to maintain their hegemony over French science. It remained insular to the extreme.

André Weil, a leading mathematician in the twentieth century who spent most of his career at the Institute for Advanced Study in Princeton, in 1955 published a little pamphlet, entitled "Science française?" denouncing the isolation and bad habits of French science (1). Each scientific discipline was ruled by a few professors in Paris. These mandarins enjoyed baronial privileges. They saw to it that their former graduate students were appointed to professorships in the provinces. They were intent, not at all in being part of the international community of scholars, but in maintaining what they felt to be essential Frenchness in the universities. Their teaching was extremely conservative. Not only were English-language textbooks totally ruled out, textbooks were not the norm. Their cost was an excuse. Likewise, most libraries lacked the funding to subscribe to foreign periodicals and purchase scholarly books. To some extent, the insularity was enforced by poverty. Professors supplied mimeographed lecture notes for rote learning by the students. The mimeographed words derived in turn from a single sacred text, Grignard's *Traité de chimie organique*, which dated back to 1935 (2). Grignard, a Nobel Prizewinner, had saintly status.

French Opposition to the Anglo-Saxon World

Before I turn to the topic of the little book which proved to be highly influential, let me briefly describe the political context. It can be given the general label of hostility to Anglo-Saxon dominance (3). Such negative feelings had been stoked during World War II, as well as the years of collaboration of the Pétain-led French State with Nazi Germany, by anti-English and anti-American propaganda within France (4). With the return of De Gaulle to power in 1958, his push for French independence from the Cold War American foreign policy (5) to some extent refreshed that public mood (6).

In between, after the Liberation of France predominantly by Anglo-American forces, there were the years of the Marshall Plan and of accruing benefits to French science, such as Fulbright Fellowships which began in 1948. There is quite a bit of truth in the saying that no good deed goes unpunished!

Hostility by the French toward the Anglo-Saxon world (7) was neither universal nor uniform. For instance, the monthly magazine edited by Jean-Paul Sartre, *Les Temps Modernes*, showed a fascination for American cultural life, for jazz as an art form and for writers such as John Dos Passos (8). However, it also embraced wholeheartedly the concept of *négritude*, as proposed

by the two great Black poets of the French language, Aimé Césaire and Léopold Sedar Senghor: segregation was the unforgivable sin in which the United States was mired (9).

Even though jazz, movies and popular music created considerable interest in the US, very few French people had traveled there. The two main drawbacks were the cost, since air travel had not yet turned to mass transportation, and language, since only a small percentage of the French were conversant in English. However, there was intense curiosity about the US, as shown, among other cultural artifacts, by the successes of a movie such as François Reichenbach's *L'Amérique insolite* (1960) and of books such as Claude Julien's *Le Nouveau Nouveau Monde* (1960) (10).

Last but not least, French chauvinism was an element in the tale I am recounting, that of a sudden breakthrough when the dam gates burst and French organic chemistry received an infusion of new ideas from the Anglo-Saxon world.

Professors Prévost and Kirmann

Back to organic chemistry as it was being taught in French universities and pursued in their poorly equipped laboratories. In 1960, within the French university system, highly hierarchical, just like the country itself, a good approximation to absolute power was wielded by two men who, in addition, were close personal friends, Charles Prévost (1899-1983) (11) and Albert Kirmann (1900-1974). They had been classmates at the *École normale supérieure* (12).

Prévost had held a professorship at the Sorbonne since 1937. During the 1930s, he and Kirmann had both recognized the importance of reaction mechanisms and of physical methods for the advancement of organic chemistry. Prévost, influenced by tautomerism and allylic rearrangements, focused on the intervention of ionic species in organic reactions. He came up with a theory of mechanisms in organic chemistry, and he coined terms such as *synionie* and *métaionie* for some of the relevant phenomena (13). Kirmann had pioneered use of Raman spectra for the characterization of organic molecules.

During World War II, Kirmann was deported and spent three years in a concentration camp. In terms of his scientific drive, he came back a broken man. At the beginning of the war, he was a professor on the faculty at the University of Strasbourg. After the Liberation, Kirmann resumed his position in Strasbourg, where shortly

afterwards he was elected Dean of the Faculty of Sciences. In 1955, Professor Kirmann received a call from his *alma mater*, the *École normale supérieure*, as head of the chemistry laboratories and as the assistant director of the whole institution, both administrative posts. During his tenure (1955-1970), Kirmann encouraged research on Grignard reagents. Such work was outside of the mainstream of international organic chemistry at that time, but Kirmann saw it as a patriotic duty to continue mining the vein which Grignard had discovered at the beginning of the century.

With respect to reaction mechanisms, Prévost and Kirmann were disappointed by the total lack of impact of Prévost's theoretical ideas in the international literature. Accordingly, they saw to it that the whole corpus of mechanistic work initiated by Ingold and Hughes in the UK during the 1930s was totally absent from the French curriculum in organic chemistry. Since both these professors held sway over appointments of organic chemists in French universities, their embargo on the "new" Anglo-Saxon theories of organic chemistry was nearly total. When Micheline Charpentier-Morize referred to the possible existence of a pi-complex to explain reactivity, during her doctoral examination, Prévost had this comment: "Madame, if I have one reproach for you, it is that you know the modern theories too well." (14)

French Backwaters

What I have just described for organic chemistry can be generalized in other sub-disciplines within chemistry. Quantum chemistry was practiced by a small team around Raymond Daudel. The emphasis was in celebrating the cult of Louis de Broglie and his *mécanique ondulatoire*. Until the early sixties, there was little penetration in France of either Pauling's valence bond theory or Mulliken's molecular orbital theory.

Polymer chemistry was likewise stifled. It was ruled by Georges Champetier, a professor at the Sorbonne. Charles Sadron, a polymer chemist from the University of Strasbourg, was intent upon building a research institute on polymers, in the American manner, and thus sought an appointment in Paris, either at the Sorbonne or at the Collège de France. Champetier repeatedly blocked those attempts. He did not want to accept competition and wanted to continue to hold French polymer chemistry hostage.

To judge by chemistry, French science at mid-twentieth century was hobbled by its insularity, by some igno-

rance of not-so-recent developments in the Anglo-Saxon world, and also by the positivistic reliance on facts alone and the steadfast refusal to indulge in interpretations relying upon electronic and quantum-mechanical theories.

Forces for Change

How did the badly-needed renewal come about? From the fringes and the periphery. From the provinces. From abroad. From both the written and the spoken word.

In 1950, Max Mousseron, a professor in Montpellier, organized an international colloquium on molecular rearrangements and the so-called Walden inversion, and it had some impact on the French participants. The Parisian professors though, frowned upon it because the initiative had not come from them. It remained more or less still-born, in spite of publication of the various contributions in the *Bulletin de la Société Chimique de France* (15).

In 1946, Marc Julia, the scion of a prestigious scientific family, after graduating from *École normale supérieure*, departed for London and the Imperial College of Science and Technology. He worked there in the laboratory of Sir Ian Heilbron and in 1948 obtained a Ph.D. in physics. After his return to France and a doctorate in chemistry, he obtained academic appointments in Paris. In 1959, he published a small book, in French, *Mécanismes électroniques en chimie organique* (16).

Another alumnus from *École normale supérieure* who also went abroad for graduate study was considerably more influential in launching a Renaissance in French organic chemistry. In 1950, Guy Ourisson went to Harvard University and worked in the laboratory of Louis Fieser. He obtained his Ph.D. in 1952, in only two years. In 1959, the year of the publication by Marc Julia of his little book, Ourisson set about to establish yearly meetings in France on the model of the Gordon Research Conferences which he had attended in the US. The first such meeting, known as GECCO, short for *Groupe d'étude de chimie organique*, was held in the summer of 1960. These yearly meetings, which served the purpose of a summer school, were an extremely efficient means for disseminating the "new" ideas on reaction mechanisms and their importance, within the French organic chemical community (17).

Bianka Tchoubar (1910-1990)

But the main actor of the renewal may well have been Mademoiselle Bianka Tchoubar (18). Of Russian Jew-

ish origin, from a small sect persecuted by fellow Jews as well (the Karaites who had originated in Babylon), she had emigrated to Paris, where she spent the rest of her life. She never married. She started her scientific career in Marc Tiffeneau's laboratory at the School of Medicine in Paris, under the supervision of Jeanne Lévy after Tiffeneau died in 1945. After her *Diplôme d'études supérieures* (1932), she had obtained her doctorate (1937) and immediately started running her own research group. During World War II, she was a fearless Resistant. She found a laboratory to host her continued career in chemistry at the Institut de biologie physico-chimique, where Edgar Lederer invited her after she gave a series of seminars in 1957-58 about the electronic theories of organic chemistry. She then followed him when he was appointed director of the newly built Institut de chimie des substances naturelles, in Gif-sur-Yvette, in 1961.

Bianka Tchoubar was memorable, a character almost out of a cartoon. Not only did she look indomitable, she gave an impression of being belligerent towards any and all. She was unkempt, obviously not giving a damn about her physical appearance. A chain smoker, she went everywhere with a cigarette held between her nicotine-stained fingers and was thus an accident waiting to happen with her habit of walking with a lit cigarette into a laboratory with its highly flammable solvents.

She was likewise totally innocent of risk when driving a car, a small Citroën *Deux-Chevaux*. There are numerous stories. One of them was witnessed by Michel Vilkas, another French organic chemist. He was once summoned to court for causing an accident. He arrived a few minutes early and heard the judge say to the person in front of him, "You, once again, Mademoiselle Tchoubar! But you are a public danger!"

For lunch, she would eat a sandwich at her desk, and wash it down with glass after glass from a nearby, highly visible bottle of red wine. There would also be on her desk a copy of the issue for that day of *L'Humanité*, the newspaper of the French Communist Party, of which she was a devout militant.

More likely than not, whenever you wanted to put a question to Tchoubar, as she was universally known, you would find her engaged in a scientific argument with one of her coworkers. It took the form of a shouting match, in which scholarly references were hurled as if they were insults. I have to engage in some subtlety here: Tchoubar constantly demonstrated in action that science thrives on discussion. She would never attempt to win an argument by asserting her authority by being a group

leader, or even from scientific experience. Yet, she hated losing *any* argument and she relished them. She was a formidable woman of science.

The audience for Tchoubar's little book

What about the group of people who, within France, were likely to be attracted to a chemistry renewed by mechanistic ideas of Anglo-Saxon provenance? They were predominantly from outside the university system dominated by Professors Prévost and Kirrmann or Champetier. They often belonged to Centre national de la recherche scientifique (CNRS).

CNRS was founded in the late 1930s. After the war years, it started to grow, both with the building of various Institutes and with the hiring of research personnel. The administration of CNRS was considerably more flexible than that of French universities. There was much less of an insistence that a person joining the ranks should show French degrees for both secondary and higher education.

Accordingly, during the aftermath of World War II, CNRS laboratories became a haven for refugees, typically well-educated Jews having survived the Holocaust and anti-Semitism in Eastern Europe. Bianka Tchoubar was from an earlier generation that had survived the Communist revolution in Russia and the subsequent civil war.

In addition to CNRS, there were other centers of active scientific research outside the university system. The Pasteur Institute in Paris was one, as well as the nuclear energy research centers, within the Commissariat à l'énergie atomique (CEA). CEA had two major research facilities, a central one in Saclay, south of Paris, and another in the provinces, in Grenoble, the Centre d'études nucléaires de Grenoble (CENG). André Rassat, a former co-worker of Guy Ourisson, and Didier Gagnaire, also an alumnus of *École normale supérieure*, started a joint laboratory at CENG in 1961, in the field of mechanistic organic chemistry (19).

Was there consciousness within these various subgroups of sharing a distaste for academic chemistry, as it was being taught and pursued in the universities? Was there consciousness of a need for reform and revitalization? You bet.

The GECO summer school was one of the providers of such hope and of some education in the new mechanistic ideas. Another was provided by attendance of Alain Horeau's lectures at the Collège de France in Paris. Alain Horeau, a pharmacist by training, was appointed in 1956

to a chair in organic chemistry at the Collège de France. He scheduled his lectures on Saturday mornings, which made attendance easier for all those of us who had regular jobs on weekdays.

A small lecture hall, holding about 60 people, was jammed—I am tempted to say with “groupies”—every Saturday morning. What was the attraction? Professor Horeau spent considerable time preparing his lectures and they were meticulous. Whereas the rule for a professor at Collège de France is to present his own research, Alain Horeau gave it a flexible interpretation. Each lecture was devoted to a separate chapter on organic stereochemistry. Horeau saw to it that it was totally up-to-date, with respect to international research progress; and that it was nourished with a comprehensive bibliography on the entire topic. The handouts summarized the various parts in each lecture and also provided an extremely useful bibliography.

Such informal gatherings of avid minds as I have described, with both GECO and the attendees of Horeau's lecture, were the fertile ground in which to sow the seeds of change. I will turn now to Tchoubar's little book and to its reception.

Claim

Bianka Tchoubar's little book (20) brought reaction mechanisms in organic chemistry to the fore. Henceforth, organic chemistry in France became up to date in its concerns and methodology. Establishing a reaction mechanism became the new paradigm, overturning the former paradigm of only adding to the catalog of existing compounds using standard reaction pathways from the books (21).

Her book was near-ideal for this purpose. It was very clearly written and it was short. This format made it possible to insert the book in the pocket of a laboratory coat—as everyone was wearing at the time. Thus, a worker at the bench, watching a transformation and waiting for it to be completed, could read or read again some paragraphs and thus become gradually initiated into the new language, with its curved arrows signifying motion of chemical bonds between atoms, as electronic pairs.

An essential feature of this guide was its brevity. It is just a little over 200 pages but, as mentioned already, in a small format, $4\frac{1}{4} \times 6\frac{1}{2}$ in (10.8×16.2 cm).

The short bibliography at the end enabled readers to complement it with other readings, some of which were translations into French of key textbooks.

Counterclaim

Tchoubar's little book was only a minor factor in the changes that finally overtook French science, French chemistry in particular, at the beginning of the 1960s (22). It was part and parcel of a much more general trend. Orthodoxies were being questioned the world over. Pope John XXIII (Angelo Roncalli) started the *aggiornamento* of the Catholic Church with the Vatican II Council in 1962. Even Communist China went through an upheaval during those years with the Cultural Revolution that Mao began in 1964.

To return to France, modernization began in every sector. Politically, the short-lived government by Pierre Mendès-France in 1954 put an end to the colonial war in Indo-China and was the portent for retreat by France from its colonial empire overseas (23). Culturally, French life underwent seismic changes. In music, Pierre Boulez, in the same year 1954, launched the *Domaine Musical* concerts which initiated French ears to contemporary music of various kinds, from the Dodecaphonic to concrete and electronic compositions. In the movies, the New Wave of directors such as François Truffaut, Jean-Luc Godard, Eric Rohmer, Agnès Varda, Jean Eustache, Jacques Rivette, Claude Chabrol, totally changed the style. All these changes began at the turn of the sixties. In the humanities, the teaching of literature was also upended, the new criticism launched with the publication of the essay by Roland Barthes, *Sur Racine*, in 1963. As an alternative to Marxist and Freudian interpretations of cultural phenomena in general, structuralist theory became the new orthodoxy, in the same period from the late 1950s into the early 1980s. Launched by the anthropologist Claude Lévy-Strauss as a rediscovery of the contribution of linguists from the Prague Circle such as Roman Jakobson, it engulfed other influential Parisian intellectuals such as the political thinker Louis Althusser, the psychoanalyst Jacques Lacan and philosophers such as Jacques Derrida and Jean Piaget, for example.

To focus once again on French science and its former isolation from the Anglo-Saxon world, it also underwent seismic changes during the early sixties. Young French scientists went abroad, to the United States in particular, for postdoctoral stays where they were initiated into the new, post-World War II science: solid-state physics, nuclear magnetic resonance, mass spectrometry, and, within chemistry, topics such as conformational analysis and modern stereochemistry, bio-organic chemistry, organometallic chemistry, quantum chemistry, etc. Conversely, lectures by British and American chemists became very

frequent in locations such as Lederer's Natural Products Institute in Gif-sur-Yvette, or the Institut de chimie in Strasbourg. Later on, during the 1970s, there were even, as a rather earthshaking development, appointments of foreign, English-speaking scientists within the French system. After some stillborn attempts to bring such scientists to France—Bernard Belleau or Martin Karplus (1972 and 1974), in the Paris area—later decades saw for example Enrico Clementi in 1991 and Martin Karplus moving to the University of Strasbourg in 1996, and, even more significantly, the British Nobel Prize winner Derek Barton taking over in 1978 from Edgar Lederer the directorship of the Institute in Gif-sur-Yvette.

A process of acculturation

Is it legitimate to frame this episode into, more generally, a history of acculturation? Indeed, it shares defining features with other kinds of acculturation, political or religious.

Perception of conflicting goals is one feature. We realized, in the early sixties, we had a choice between conformity if enrolled among graduate students of Professors Prévost, Kirmann, or another of their ilk, with the promise of a university position later on; or performing original research outside the orthodoxy without any hope of such a job afterwards (a jaundiced view which for many of us turned out to be unduly pessimistic).

Another perception goes with the onset of acculturation, that of a feeling of inferiority and outdatedness. Among French organic chemists in the early sixties, we knew that we lacked both knowledge and competence regarding the mechanisms of chemical reactions. We also knew where to look for what we needed—in the published work of British and American colleagues.

An essential feature of acculturation, whatever its kind, is a place of worship, so to speak. Those locales were provided to us by attendance of a GECO meeting in the summer and by Professor Horeau's lecture hall in the winter. In other words, the new religion was imparted to those who were already converts and only to those.

Acculturation means in addition assimilating a new knowledge. It consisted of, as already mentioned, the whole body of mechanistic results which British and American chemists had amassed. Bianka Tchoubar's little book served as a fine introduction to this new knowledge.

Beyond that, we were encouraged into acquiring a competence, that of digging out such mechanistic information ourselves: it entailed careful study of the accurate kinetics of a chemical transformation, exacting if rather

tedious experimental work which we had to learn how to carry out. This is also part and parcel of any acculturation.

Acculturation needs mediators, special people trading in the imparting of the new knowledge and competence; people whom one may wish to call "brokers" in that they do not own their stock in trade, merely distribute it. That was, typically, Bianka Tchoubar's function.

The last feature of acculturation I wish to mention is best conveyed by quoting I. Prothero's book *Artisans and Politics in Early Nineteenth-Century London*, "... what historians mean when they speak of 'the rise of the working class' is artisans becoming politically active." (24) In our case, such a division into social groups applied to *group leaders* as distinguished from the *rank-and-file*. The former, not the latter, were the subjects of the acculturation I have described.

Is this the end of the story, a stereotypical happy ending? Anglo-Saxon ideas henceforth permeated French chemistry which became up to date and regained its footing within worldwide science. I cannot deny the existence of such conventional wisdom. I also believe it to be unduly optimistic and largely mistaken.

In truth, the acculturation I have dealt with unfortunately did not include mentalities and institutional aspects. Over the years, CNRS assumed most of the unfortunate characteristics of French universities: patronage, a view of knowledge as compartmentalized in like manner to plots of lands owned by individuals, avoidance of competition, insufficient funding, lifelong appointments in fact if not in principle. But that is another story: one should never underestimate the ability of any conservative social system for self-preservation.

Conclusion

I have set down this account more as personal memoir than as history. A take-home lesson is the very unpredictability of this story, the irony, the necessity perhaps also, of a bigger-than-life immigrant from Russia bringing American and British ideas to the French.

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References

1. A. Weil, "Science française?" *Nouvelle Revue Française*, **1955**, 3(25), 97-109. It is also published, together with a commentary of mine, on the BibNum site: <http://www.bibnum.education.fr/scienceshumainesetsociales/histoire-de-lenseignement/science-francaise>
2. V. Grignard, *Traité de chimie organique*, Masson, Paris, 1935.
3. C. Bismut, "Les Anglo-Saxons ont-ils tort?" *Politique étrangère*, **1996**, 61(2), 397-406.
4. R. O. Paxton, *Vichy France: Old Guard and New Order, 1940-1944*, Columbia University Press, New York, 2001; A. Tasca and D. Peschanski, Ed., *Vichy 1940-1944: Quaderni e documenti inediti di Angelo Tasca*, Editions du CNRS and Feltrinelli, Paris, 1986.
5. I. Wall, "France in the Cold War," *J. European Studies*, **2008**, 38(2), 121-139.
6. That mood is still very much alive. See for instance P. Chaillou, "L'américanisation de la justice. Présenté comme moderne, le système accusatoire serait une régression pour la France, où l'accès au droit est déjà inégalitaire," *Libération*, 2 mai 2003, not to mention a flurry of newspaper and magazine articles, following Dominique Strauss-Kahn's arrest in New York, during the late spring of 2011.
7. A. Sauvy, "Le Malthusianisme Anglo-Saxon." *Population*, **1947**, 2(2), 221-242; P. Roger, "Cassandra's Policies: French Prophecies of an American Empire from the Civil War to the Cold War," *J. European Studies*, **2008**, 38(2), 101-120.
8. J.-F. Sirinelli, "Pas de clercs dans le siècle? Sur Sartre, Camus et quelques autres," *Vingtième Siècle. Revue d'histoire*, **1987**, 13(13), 127-134.
9. J. Flower, "The American Dream — or Nightmare. Views from the French Left, 1945–1965." *French Cultural Studies*, **2009**, 20(1), 47-64.
10. C. Julien, *Le Nouveau Nouveau Monde*, Julliard, Paris, 1960.
11. M. Blondel-Mégrelis, "Charles Prévost (1899-1983)," in L. Lestel, Ed., *Itinéraires de chimistes: 1857-2007, 150 Ans de chimie en France avec les présidents de la SFC*, Société française de chimie et EDP Sciences, Paris, 2008, pp. 445-448.
12. M. J. Nye, *From Chemical Philosophy to Theoretical Chemistry: Dynamics of Matter and Dynamics of Disciplines, 1800-1950*, University of California Press, Berkeley and Los Angeles, 1993.
13. C. Prévost and A. Kirmann, "Essai d'une théorie ionique des réactions organiques. I." *Bull. Soc. Chim. France*, **1931**, 49, 194-243; C. Prévost and A. Kirmann, "Essai d'une théorie électronique de la chimie organique. II." *Bull. Soc. Chim. France*, **1931**, 49, 1309-68; A. Kirmann and C. Prévost, "Tautomérie anneau-chaine et la notion de synionie," *Bull. Soc. Chim. France*, **1933**, 53, 253-260.
14. M. J. Nye, "National Styles? French and English Chemistry in the Nineteenth and Early Twentieth Centuries," *Osiris*, **1993**, 8, 30-49.
15. For instance, M. J. S. Dewar, "A Review of Π Complex Theory." *Bull. Soc. Chim. France*, **1951**, 18, C71-79. See J. A. Berson, "A Missed Turning Point for Theory in Organic Chemistry: Molecular Orbitals at Montpellier in 1950," *J. Phys. Org. Chem.* **2005**, 18(7), 572-577.
16. M. Julia, *Mécanismes électroniques en chimie organique*, Gauthier-Villars, Paris, 1959.
17. J.-M. Lehn, "Notice sur la vie et l'œuvre de Guy Ourisson, lue par Jean-Marie Lehn lors de la séance statutaire en région de l'Académie des Sciences à Strasbourg les 27 et 28 mai 2008," *Académie des Sciences*, 2008, http://www.academie-sciences.fr/activite/conf/seance_270508_Lehn.pdf.
18. J. Jacques, "Bianka Tchoubar (1910-1990)," *New J. Chem.*, **1992**, 16, 7-10.
19. M. Soutif, "La connivence entre physiciens (à Grenoble) de 1950 à 1975," *Revue pour l'histoire du CNRS*, **2000**, 2, 16, <http://histoire-cnrs.revues.org/1439>.
20. B. Tchoubar, *Les mécanismes réactionnels en chimie organique*, Dunod, Paris, 1960.
21. M. Charpentier-Morize, "Perrin, père de l'atome et destructeur de la chimie moderne." *Recherche*, **1997**, 295, 94-99.
22. P. Lelong, "Le général de Gaulle et la recherche en France," *Revue pour l'histoire du CNRS*, **1999**, 1, <http://histoire-cnrs.revues.org/481>.
23. A. Chatriot and V. Duclert, *Le Gouvernement de la recherche: histoire d'un engagement politique, de Pierre Mendès France à Charles de Gaulle (1953-1969)*, La Découverte, Paris, 2006.
24. I. J. Prothero, *Artisans and Politics in Early Nineteenth-Century London: John Gast and His Times*, Dawson, Folkestone, UK, 1979.

About the Author

Pierre Laszlo, who was born in Algiers in 1938, is a French physical organic chemist. After obtaining his Ph.D. with Edgar Lederer, he was a post-doc at Princeton University with Paul von Ragué Schleyer (1962-63). After his D. Sc. (1965), he returned to Princeton as an assistant professor (1966), accepting in 1970 a call from the University of Liège, Belgium as a full professor. He taught there until 1999, but, starting in 1986, held a joint appointment at the *Ecole polytechnique*, in Paris, also until 1999. Taking early retirement in that year, he has remained active since as a science writer (*Salt, Grain of Life*, 2001; *Citrus, A History*, 2007) and science historian.

CELEBRATING OUR DIVERSITY. THE EDUCATION OF SOME PIONEERING AFRICAN AMERICAN CHEMISTS IN OHIO

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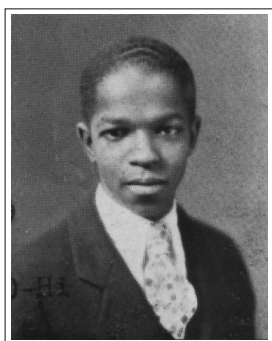


Figure 1. Thomas Nelson Baker Jr. Photo Courtesy of the Oberlin College Archives.

Thomas Nelson Baker Jr. may not be a household name, but he clearly represents the many African Americans that have achieved great success in higher education during a volatile racial period in our history (1). A native of Pittsfield, Massachusetts, Baker began his undergraduate studies at Oberlin College (Oberlin, Ohio) and earned his B.A. and M.A. in chemistry in 1929 and 1930, respectively. Baker completed his M.A. thesis entitled, "Some Esters of Anthranilic Acid" under the direction of his research advisor, Prof. Alfred P. Lothrop.

In 1931, Baker began graduate work at The Ohio State University (OSU, Columbus, Ohio) where he studied organic chemistry. Professor Melville Wolfrom, a well-known carbohydrate chemist, served as Baker's graduate advisor during his tenure at OSU. In order to support himself and his family, Baker taught at various Historically Black Colleges and Universities (HBCUs),

namely Tougaloo College (Mississippi), Talladega College (Alabama) and Virginia State College (now known as Virginia State University), where he taught from 1932-1972.

According to the acknowledgment section of Baker's dissertation, he received two fellowship awards to complete his graduate education (2). Baker received a fellowship from the General Education Board (1937-38) and a University Fellowship from OSU (1938-39). Established in 1902 by John D. Rockefeller and Frederick T. Gates, the General Education Board was a philanthropic effort to support higher education and HBCUs (3). Baker earned his Ph.D. in chemistry in 1941 from OSU with a dissertation entitled, "The Molecular Size of Glycogen and of Mannan A by the Mercaptalation Method (4)." It is believed that Baker may be the first African American to have earned a Ph.D. in chemistry from The Ohio State University. Baker later became a full professor at Virginia State College in 1944 and he served as chair of the department until his retirement in 1972. In addition to teaching responsibilities, Baker sang in the choir and played the oboe. He died on April 27, 1977.

Although Baker's academic and professional achievements are quite fascinating, his family background clearly emphasizes the importance of higher education. His three siblings Edith, Ruth, and Harry also attended Oberlin College and later pursued teaching careers in music and English. His father, Thomas Nelson

Baker Sr., born a slave in 1860 in Virginia became the first African American to earn a Ph.D. in Philosophy (1903, Yale University). Baker Sr. also attended Yale Divinity School and later served as a minister in Pittsfield, Massachusetts. Unfortunately, he died a year before his son completed his graduate studies at OSU (5). His death notice reads, "The Rev. Dr. Thomas Nelson Baker, a Negro born in slavery upon whom Yale University conferred a Ph.D. degree in 1903, died Feb. 24 after being overcome by fumes in his home in Pittsfield, Mass. He was 80 years old." (6)

In 1997, George Yancy published a fascinating article on the background and influence of Baker Sr. during the 20th century (5). Yancy concludes, "There is still a great deal that remains unexplored that is of importance in the life and philosophical worldview of Thomas N. Baker. For example, as a child, for Baker, what was it like growing up in Northhampton County, Virginia? How did his parents help him to deal with racism? Having been born enslaved and having inherited the culture of racist ideology, what impact did this have on Baker's self-esteem and self-conceptualization? What was it in the nature of Baker's character that provided him with such an inexhaustible desire to learn?"

The achievements of Baker Sr. surely had a profound impact on the success of his four children and future generations—grandson Thomas Nelson Baker III earned his Ph.D. in chemistry from Cornell University in 1963 and later worked in the petroleum industry. In 1936, Thomas Nelson Baker Jr. married Ruth Modena Taylor, who was an Assistant Professor of English at Virginia State College. They had two sons, Thomas Nelson Baker III and Newman Taylor Baker. After his first wife died in 1961, he later married Geneva J. Baker in 1964. Geneva Baker was an Associate Professor of Applied Art at Virginia State.

However, Thomas Nelson Baker Jr. is not the only first from OSU. In 1933, Ruth Ella Moore, a native of Columbus, Ohio, became the first African American woman to earn a Ph.D. in bacteriology (4, 7). Moore also earned her B.A. and M.A. at OSU, in 1926 and 1927, respectively. She would later have a very successful career in the Department of Bacteriology at Howard University, an HBCU in Washington, DC. In 1965, Robert Henry Lawrence Jr., a flight instructor in the United States Air Force and the first African American astronaut earned his Ph.D. in chemistry from OSU. Lawrence died in a tragic flight training accident in 1967. In January 2000, a room dedication ceremony was held in his honor at OSU (8). Lawrence earned his Ph.D. during the Civil

Rights Movement and dedicated his dissertation (9) to the on-going struggle for human rights:

This work is dedicated to those American Negroes who have spent their lives in the performance of menial tasks struggling to overcome both natural and man-made problems of survival. To such men and women scientific investigation would seem a grand abstraction. However, it has been their endeavors which have supplied both the wherewithal and motivation that initiated and helped sustain this effort.

Historically, after completing doctoral studies from a majority institution, the most common employers for African American Ph.D. scientists were HBCUs (4, 10). Obviously, employment opportunities for African American chemistry professors has improved over the years, but recent reports continue to suggest low participation of African Americans within the chemical sciences (11-13).

There are many questions that remain about Thomas Nelson Baker Jr. Why did he and his three siblings attend Oberlin College instead of Hampton Institute (now known as Hampton University) like their parents Rev. Dr. Baker and Elizabeth (Lizzie) Baytop Baker? The selection of Oberlin College was likely a logical choice considering that at one time Oberlin was known as an "active spot" for the Underground Railroad (14). Moreover, Oberlin College also had well-documented history regarding African Americans pursuing higher education. In 1862, Mary Jane Patterson became the first African American woman to earn an undergraduate degree in the United States (15). Thus, it is reasonable to assume that the Bakers knew their children would receive a quality education at Oberlin College.

Thomas Nelson Baker Jr. likely decided to pursue a Ph.D. at The Ohio State University due to the encouragement of Professor Alfred P. Lothrop, who served as his research mentor at Oberlin College. In his book, *Negroes in Science*, Jay indicates that OSU was one of the leading producers of African American Ph.D. scientists (10). According to Baker's college records, only one copy of his transcript was sent to Ohio State in May 1930. He didn't apply for graduate admission at other institutions such as Iowa State University (16), Wayne State University, or the University of Chicago, which were also known to produce African American Ph.D. scientists during Baker's time. It is also unknown if Baker and Moore actually knew each other because they would have overlapped in the early 1930s at OSU during their graduate studies.

In summary, Baker, Moore, and Lawrence represent the many heroes of science whose achievements are

seldom recognized and celebrated in science classes or textbooks. Their achievements during a volatile time in U.S. history are remarkable. They are significant because of their contributions to the field of science. However, they also represent the proud history and legacy of graduates from institutions in the state of Ohio.

Acknowledgements

The author would like to thank Mr. Ken Grossi, Oberlin College Archives, for his contributions to this manuscript.

References and Notes

1. C. V. Woodward, *The Strange Career of Jim Crow*, 3rd ed., Oxford, New York, 1999 [first edition 1955].
2. T. N. Baker, "The Molecular Size of Glycogen and of Mannan A by the Mercaptalation Method," Ph.D. diss., The Ohio State University, 1941.
3. The Rockefeller Archive Center – General Education Board, <http://www.rockarch.org/collections/rockorgs/geb.php>
4. H. W. Greene, *HOLDERS of Doctorates among American Negroes*, Meador, Boston, 1946.
5. G. Yancy, "Thomas Nelson Baker: The First African-American to Receive the Ph.D. in Philosophy," *Western J. Black Studies*, **1997**, *21*, 253-260.
6. "Deaths: Thomas N. Baker," *Advance*, April 1, 1941. [The author viewed a newspaper clipping at Oberlin College Archives. No further bibliographic information about the newspaper was available.]
7. M. C. Brown, *The Faces of Science: African Americans in the Sciences: Ruth Ella Moore: Bacteriologist*, https://webfiles.uci.edu/mcbrown/display/ruth_moore.html.
8. D. Lore, "OSU Ceremony Will Honor Nation's First Black Astronaut," *Columbus Dispatch*, January 21, 2000.
9. R. H. Lawrence, "The Mechanism of the Tritium Beta-ray Induced Exchange Reactions of Deuterium with Methane and Ethane in the Gas Phase." Ph.D. diss., The Ohio State University, 1965.
10. J. M. Jay, *Negroes in Science: Natural Science Doctorates, 1876-1969*, Balamp, Detroit, 1971.
11. D. J. Nelson, A National Analysis of Diversity in Science and Engineering Faculties at Research Universities, <http://chem.ou.edu/~djn/diversity/briefings/Diversity%20Report%20Final.pdf>.
12. W. Pearson Jr., *Beyond Small Numbers: Voices of African American PhD Chemists*, Elsevier JAI, Boston, 2005).
13. S. N. Collins, "African Americans and Science," *Chem. Eng. News*, *87*(43), Oct. 26, 2009, p. 3.
14. Oberlin College Heritage Center, <http://www.oberlinheritage.org>.
15. EOG Electronic Oberlin Group, Mary Jane Patterson, <http://www.oberlin.edu/external/EOG/OYTT-images/MJPatterson.html>.
16. S. N. Collins, "The Gilman Pipeline. A Historical Perspective of African American Ph.D. Chemists from Ohio State," in P. A. Thiel, Ed., *Chemistry at Iowa State University: Some Historical Accounts of the Early Years*, Iowa State University, Ames, IA, 2006, 126-148.

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INTRODUCTION TO THE ENGLISH TRANSLATION OF “A CONTRIBUTION TO CHEMICAL STATICS” BY LEOPOLD PFAUNDLER

A Forgotten Classic of Chemical Kinetics

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The central importance of Leopold Pfaundler's pioneering 1867 paper on the application of the kinetic theory of heat to chemical reactions and the desirability of a long-overdue English translation of the same have already been commented on in the introduction to the recently published English translation of August Horstmann's equally important paper of 1873 dealing with the first application of the second law of thermodynamics to the theory of chemical equilibrium (1). Since this same introduction also reviewed the current status of English-language translations of classic chemical papers in general, and a paper dealing with both Pfaundler's life and the context of his contribution will appear in the next issue of the *Bulletin* (2), all that remains for this introduction is to deal with the technicalities of the translation process itself.

As with the earlier translation of the Horstmann paper, Dr. Kuhlmann, who is a native German speaker, first produced a literal translation, which Dr. Jensen then extensively revised and edited in order to make the phrasing and sentence structures more acceptable to the English reader. As always, he prefers a looser translation which places more emphasis on clarity than on literal accuracy, and any defects in the final translation resulting from this process should be credited to Dr. Jensen alone.

At first we thought that the translation would be simple and straightforward, since Pfaundler's German

is quite easy to read in the original. However, this expectation soon proved unfounded, since the simplicity of Pfaundler's German was dependent on conventions unique to the German language which, when literally translated into English, resulted in a nightmare of pronoun ambiguity. In order to avoid the resulting confusion, we have found it necessary to convert many of Pfaundler's pronouns into the corresponding nouns and to make many of his implied meanings explicit. The larger of these interpolations are indicated within the body of the translation by enclosing the amplifications in square brackets, though many single word clarifications have been left unmarked as these would have generated too much editorial clutter within the text.

In yet other cases we encountered ambiguities due to Pfaundler's word choices. A frequent example was his use of the phrase “quantity (*Menge*) of molecules,” when it is obvious that he meant the “number of molecules,” and indeed sometimes even explicitly stated this in a later clause within the same sentence, or his use of the adjective “maximum” when he meant threshold or upper limit. Also, like Clausius, Pfaundler does not use the term “kinetic energy” in his paper, but rather refers to the *lebendig Kraft* of the moving molecules. Since a direct English translation of this term as “living force” seems awkward to the ear of the English reader, we have instead chosen to use the original Latin term for this concept—*vis viva*—which is how it is normally referred to in most

histories of mechanics. Similarly, we have modernized Pfaundler's chemical nomenclature and have translated *kohlensauren Kalkes* and *Kohlensäure* as calcium carbonate and carbon dioxide respectively.

In addition to these translation problems, there are also some severe organizational problems with Pfaundler's paper. As originally conceived, the paper was explicitly divided into three parts, in addition to a separate introductory paragraph and an unmarked conclusion. However, after completing the initial draft of his paper, Pfaundler encountered a recently published paper by H. W. Schröder van der Kolk criticizing Henri Sainte-Claire Deville's work on dissociation which Pfaundler felt compelled to comment on, not least because he felt that his own theory of dissociation both clarified and refuted most of Schröder van der Kolk's objections. But rather than attach these comments as an addendum to the end of his paper, Pfaundler chose to insert them as a separate section at the end of Part I, thereby interrupting the flow and organization of his original manuscript. Yet further confusion resulted from Pfaundler's decision to insert a lengthy addendum to the addendum as a footnote, placed not at the end of the original addendum, but at the very end of the entire paper. Other minor problems result from Pfaundler's footnoting and referencing procedures. Most of these are placed at the bottom of the pages in question, but are separately numbered for each page, whereas others are embedded within the body of the text itself. In addition, the citation style for a given journal often varies from page to page.

Since our goal is to make the translation as accessible to the modern reader as possible, we have chosen in the translation to correct these organizational problems by transferring the addendum (which is of only minor interest to the modern reader) to the end of the paper and by also transferring all of the references and notes (both those within the text and at the bottom of the various pages) to the end of the paper, where they have been

standardized and renumbered sequentially. Lastly, the various sections resulting from these rearrangements, as well as the original unmarked conclusion, have also been labelled and renumbered sequentially. We have further taken the liberty of merging most of Pfaundler's single-sentence "island" paragraphs with either the preceding or succeeding paragraphs, where they would have normally been placed by most modern writers.

One final problem involves Pfaundler's use of chemical equations. In Parts I and II of his paper, he writes them, as we do today, using linear compositional formulas for the various reactants and products. However, in Part III he suddenly reverts to writing them using type formulas for the reactants and products. Since these involve curly brackets and placement of one symbol above another, they create severe layout problems for the modern computer. Since many of the type formulas in Part III also appear as linear formulas in Parts I and II and it is obvious that Pfaundler clearly understood the equivalency of these two notations, we have chosen, for reasons of both consistency and typographical convenience, to use linear compositional formulas throughout. The only place where this change results in a loss of clarity is in Pfaundler's discussion of his postulated collision complex, ABCD, for a double decomposition reaction, where the type formula more clearly indicates the feasibility of alternative modes of decomposition than does the linear formula. For this reason, a reproduction of Pfaundler's original type formula for this complex will appear in the commentary which will be published in the next issue of the *Bulletin* (2).

References and Notes

1. W. B. Jensen, "Introduction to the English Translation of 'The Theory of Dissociation' by August Horstmann: A Forgotten Classic of Chemical Thermodynamics," *Bull. Hist. Chem.*, **2009**, *34*(2), 73-75.
2. W. B. Jensen and J. Kuhlmann, "Leopold Pfaundler and the Origins of the Kinetic Theory of Chemical Reactions," *Bull. Hist. Chem.*, **2012**, *37*, to be published.

PRIMARY DOCUMENTS

“A CONTRIBUTION TO CHEMICAL STATICS”

Leopold Pfaundler

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It seems to me that the theory, which I shall develop in this paper, is able to provide an explanation for some chemical facts for which no suitable hypothesis has yet been found. Among these facts are the phenomena of dissociation, the so-called mass-action effect, reciprocal and predisposing affinity, the equilibrium state between opposing reactions, and several other related phenomena.

I. Theory of Dissociation Phenomena

The observation of certain exceptions to the law of vapor densities initially resulted in the hypothesis that those compounds, which showed these exceptions, decompose in the vapor phase. The numerous experiments of Sainte-Claire Deville, Pebal, Würtz, Wanklyn, and of Robinson and Than have confirmed the hypothesis proposed by Hermann Kopp, Cannizzaro and Kekulé. Furthermore, they also prove that this decomposition is often incomplete—in fact, that it is only partial over a wide range of temperatures, such that, within this range, each degree of the temperature [scale] correlates with a different degree of decomposition. The majority of chemists view this partial decomposition as an entirely adequate explanation of the [observed] irregularities in the vapor densities. *However, it does not explain the partial decomposition itself.*

In my opinion, the following two essentially different ideas can be formed concerning the state of a compound, AB, whose vapor has begun to decompose. Either all of the AB molecules experience the same change (a

loosening of their bonds [and] an increase in the distance separating their components), and therefore pass into a state which is intermediate between their original state and that of complete decomposition; or this change impacts the individual molecules unequally, such that, for example, only a portion of them are completely decomposed, whereas the remainder remain undecomposed.

At first glance, the former, rather than the latter, assumption seems to have a higher probability, even though it is inconsistent with the results of the experiments of Deville, Pebal and Würtz. Even if it were possible to explain the increase in the volume of the vapor as a consequence of the resulting relaxation of the connection between components A and B, it would still not be possible to understand how something other than a complete regeneration of the original compound could occur after cooling. Nor would cleavage by diffusion be understandable in this case. Eventually, even with this process, the final temperature-induced transition from the state of highest relaxation to the state of complete separation would have to occur in a single bound, whereas experience shows that the change in the vapor densities is continuous.

The second assumption explains the observed facts completely, but involves something which is difficult to imagine. One cannot quite conceive why, at the same temperature, a certain number of evidently identical molecules will decompose, while the remainder remain intact. If it is the temperature which determines their

degree of decomposition, and this is the same for all, then all of them must suffer the same change, since identical causes must produce identical effects. I will now try to resolve this difficulty. It will be shown that it is not the correctness of the conclusions that is at fault, but rather that of the premises.

Deville has already emphasized the analogy (1) which exists between the partial decomposition of compounds below the actual decomposition temperature and the evaporation of liquids below the boiling temperature. This very same concept occurred to me while reading Clausius's paper, "On the Form of Motion Which We Call Heat" (2), and led me to investigate whether, as a consequence of the similarity between these phenomena, an hypothesis, like that used by Clausius to explain evaporation, might also be useful in explaining dissociation. I found that his hypothesis was readily applicable to a certain class of dissociation processes. By way of contrast, it was not as directly applicable to the dissociation of vapors, though it is easy enough, using the same fundamental ideas, to construct an alternative hypothesis, which, in my opinion, completely explains this phenomenon as well.

Let us first look at the process of vaporization and its explanation, as given by Clausius. If one heats a liquid in a closed space, a portion of it evaporates—that is, a certain number of molecules on its surface are transferred to the space above until it contains a certain characteristic number. As long as the temperature remains constant, this number remains unchanged. At this point one might ask why all of the molecules at the surface of the liquid do not change into vapor, since their temperature is the same as those that are already in the vapor. This point is crucial for the analogy. Those who are content with the explanation that further evaporation is inhibited by the partial pressure of the vapor, might also be satisfied, when it comes to the dissociation of a compound, with the explanation that the partial pressures of the vapors of the separated components inhibit further decomposition of the compound as long as the temperature remains constant. If this is increased by a certain amount, a further number of molecules will decompose until the increase in the partial pressure of the released components is in equilibrium with the force of decomposition.

I think this explanation is still insufficient, since—apart from the fact that it still remains to be investigated whether one can talk of a partial pressure in this situation similar to that present in evaporation and whether this would have a similar impact with respect to inhibiting the

separation of chemically bonded molecules as it has with respect to those bound by cohesion—the difference in the behavior of the individual molecules is still unexplained. One has to look further into this matter and consider the nature of *partial pressure itself*. This has been done by Clausius. According to his theory, the equilibrium which ensues when the vapor pressure has reached its maximum is due to the fact that an equal number of molecules are now leaving the surface of the liquid for the space above it as are simultaneously returning from the vapor to the liquid surface.

As representative of the general dissociation phenomena to which this hypothesis may be immediately applied, I choose the decomposition of [solid] calcium carbonate. When heated in a closed space, this undergoes a dissociation, beginning at a certain fixed temperature, which means that a number of its molecules, whose internal motions have exceeded [the allowed] maximum, decompose. The molecules of liberated carbon dioxide gas are moving in the space [above the solid] in a rectilinear fashion and will increase until the number reabsorbed per unit time is as great as the number expelled per unit time. If the temperature is slightly decreased, then the number of molecules that recombine will exceed the number being expelled and the material will *absorb* carbon dioxide. If the carbon dioxide molecules in the space [above the solid] are now displaced by air (or some other indifferent gas), the expulsion of the molecules of carbon dioxide does not stop, because its cause has not been eliminated, but the absorption of the molecules does, since they are being removed [by the air flow]. Therefore the calcium carbonate *evolves* carbon dioxide in the air stream at the *same* temperature as it absorbs carbon dioxide in its absence. The calcium carbonate and carbon dioxide behave in the air stream in a manner similar to that of a hydrated substance that is being dried (3).

I will now pass to an explanation of the *dissociation of vapors* and, for that purpose, will hypothesize that, in the vapor of a partially decomposed [gaseous] compound at constant temperature, *as many molecules are being cleaved as are being recombined by the [molecular] motions*. This manner of explanation necessarily implies that *not all of the molecules simultaneously experience the same state of motion*, just as the explanation of evaporation by Clausius postulates that the states of motion of the molecules on the surface of the liquid are unequal. According to the mechanical theory of heat, such an inequality is highly probable.

The process of decomposition for a compound AB may therefore be thought of in the following fashion: As long as the compound has not yet decomposed, all of the molecules will have the composition AB. They will move in a rectilinear fashion. Furthermore, the components of these molecules will also move relative to each other. However, this movement of the components (as well as the rectilinear motion) is not of equal magnitude for every molecule because, even if they were momentarily equal, they would not remain so as a result of their [mutual] collisions and their collisions with the wall [of the container]. Only the average *vis viva* [i.e. kinetic energy] of these motions remains unchanged at constant temperature and in a certain ratio to the *vis viva* of the rectilinear motion of the molecules. But in the individual molecules it will sometimes be larger and sometimes smaller.

If the temperature is now increased, the *vis viva* of both [kinds of] motion increases. As a result, it may happen that the increase in the internal motion of those molecules, for which, at this instant, the [internal] motion already happens to be quite large, will then become so large that it will result in a complete separation of the components A and B. It is impossible for this separation to happen to all of the molecules at the same time. Rather it must occur first for those whose internal motion happens to be larger than the rest. These separated components, which are now free molecules themselves, now possess rectilinear motion as well. Meanwhile a new selection of previously undecomposed molecules will attain the upper limit for their internal motions, and will, in turn, also decompose. This will happen to equal numbers per unit time and continuously increase the number of dissociated molecules. However, these will, in part, collide with one another. Not all of these collisions will result in the dissociated molecules recombining, but rather only those whose states of motion are such that, when the dissociated compound is reformed, the resulting combined motions of its components are no greater than that required for the original separation. Hence, it necessarily follows that, at a given constant temperature, the free molecular fragments will continue to increase until the number of reuniting molecules per unit time becomes as great as those produced per unit time by cleavage. From this point on an *equilibrium* between decomposition and recombination will dominate, provided that the temperature remains constant. But if this increases, the number of dissociating molecules must also increase, while the number of reuniting molecules will initially decrease. The equilibrium can only be restored when the number of molecules, A and B, in the unbound state is large enough that as many recombine as decompose.

If the temperature continues to increase, one will finally reach the point where all of the molecules decompose without being able to recombine. At this juncture the dissociation phase will finally terminate in one of complete decomposition.

If during the dissociation phase, an opening is made in the wall of the container, or the walls are porous, both the undecomposed and decomposed molecules will pass through in a rectilinear fashion, but since their speeds are inversely related to the square root of their masses (4), the dissociation fragments will diffuse faster than the undissociated molecules and, among the former, the lighter faster than the heavier. Based on this, the experiments of Pebal and Deville may be explained and it also leads to the conclusion that it should be possible to use diffusion to gradually increase the [degree of] dissociation of the remaining material in the container without increasing the temperature (5). The same result could be obtained using a chemical medium to absorb both of the components, *or only one* of them. (Therefore an analysis of the gas mixture is not feasible without a chemical interaction between the absorbing material and the compound.) The fact that decomposition can only occur gradually seems to me to provide the correct explanation for why many reactions require a certain period of time for their completion.

If one cools down a partially or completely decomposed vaporous compound, the process [of decomposition] will generally be reversed. However, it is conceivable, especially with rapid cooling, that the separated components will pass over into a state in which they can no longer recombine before they have had time to reunite. This explanation has already been employed by Deville.

II. Theory of States of Equilibrium Between Reciprocal Reactions, Explanation of Mass Action, etc.

It has been frequently observed that a compound AB is decomposed by material C at the same temperature as compound BC is decomposed by A. Likewise, it is a known fact that reactions of the form $AB + CD = AD + BC$ may become reciprocal at the same temperature, whether one decreases the amounts of the compounds to the left of the equal sign or increases those to the right. As a matter of fact, the affinity of a material is a function of its mass.

At that period when the principle of definite proportions was not as certain as it is today, this and similar

facts provided a great deal of support for the theory of Berthollet. They still form a dark chapter in the theory of affinity. The arguments which one can deduce from them in opposition to presently accepted theories are, it seems to me, perhaps silenced by the overwhelming number of supporting arguments, but not altogether eliminated.

The correlation of these facts with those of dissociation and the generality of this phenomenon were first specifically remarked on by Adolf Lieben in his paper: "On the Vapor Densities Known as Abnormal" (6). There he cites the same example of calcium carbonate, which I used earlier; then the facts concerning the decomposition of water, which we owe to Deville; and, finally, the results of the beautiful experiments of Berthelot and Péan de Saint-Gilles concerning the formation and decomposition of compound ethers [i.e., esters], which are, without doubt, of greatest importance for the subject under discussion. Related to this are the recently published and equally interesting discoveries of Berthelot concerning the equilibrium between the opposing reactions for the synthesis and decomposition of hydrocarbons, which Berthelot also compares to dissociation.

All of these facts allow for a single explanation formulated with the help of an hypothesis based upon Clausius' theory of the [three] states of matter and which consequently replicates his theory of evaporation. Let us assume there are equal numbers of the molecules of three gases, A, B and C, in a closed space. Furthermore, at room temperature, the gases A and B are combined in the form of the gaseous compound AB. Initially two kinds of molecules are moving in this space: AB and C. Now, if the temperature is progressively increased, a number of AB molecules can, as shown earlier, decompose, which means that the compound AB will enter into a state of dissociation. Then the separated molecules, A and B, will, like the others, move in a straight line within the [available] space and will occasionally encounter the molecules of C. Let us assume that substance B has an affinity for C—thus the molecules of B and C can combine on colliding provided that the sum of their motions does not result in a state of motion which makes their attachment impossible.

However, in this case the following process is likely: Even before the temperature has reached a level sufficient to induce the dissociation of AB, the same result can be initiated by the influence of molecule C. Let us examine a molecule of the substance AB, which, because of the high temperature, has already acquired sufficient motion of its components that it is close to decomposition,

and which now encounters a molecule C. The external motion of both molecules is now completely or partially converted into internal motion by the impact. The result now depends on whether the affinity is or is not strong enough, given this enhanced internal motion, to keep all three bodies together. If not, then the components are repelled again, which means a part of the internal motion is once again converted to external motion. Apparently the mode of separation now depends on how the internal motion is distributed among the individual parts. If the internal motion of the original AB molecule was already very large prior to impact, and was further increased by the impact, then the cleavage of the transient ABC molecule to form A and BC is more likely than to form AB and C. Therefore, a certain definite portion of the AB molecules which collide with the C molecules will react according to the equation $AB + C = A + BC$. Here we have a dissociation process which is different from pure dissociation; but also equally different from a complete chemical decomposition in which all of the molecules are decomposed at once. The peculiarity of our process consists in the necessity of only *partial* decomposition.

Besides AB and C molecules, we now have those of A and BC. Provided that even the most favorable combination of motions fails to create a net motion capable of decomposing BC, the reaction will now, in the course of time, proceed to completion without a further increase in temperature (i.e., until all of the molecules of AB have encountered molecules of C under conditions favorable for decomposition) and will terminate in the completion of the equation $AB + C = A + BC$. If this is not the case, but rather at some other temperature the motion of the components within even a few BC molecules increases to the point that (with the simultaneous assistance of the affinity of A for B) it causes their decomposition, then the process must stop at a certain composition of the mixture, provided that the temperature is held constant. Indeed, decompositions still constantly occur, but they will now be compensated by an equal number of recombinations. Within a certain temperature range, changes in temperature will only affect the reciprocal proportion of the different molecules and a certain [composition of the] mixture will correspond to a certain degree of temperature. It matters which of the combined molecules is most affected by the increase in temperature and approaches the upper limit for its internal motions faster.

Now we want to investigate how the process has to proceed if one adds, without an increase in temperature, more of gas AB to a gaseous mixture of AB, BC, A and C. Thereby the equilibrium between recombination and

decomposition must be disturbed, since the number of decomposing AB molecules increases in a manner proportional to the amount present. As a result of this, the number of free molecules of C likewise decreases. More molecules of BC are formed, but more than before are decomposed as well. Equilibrium is only possible at a different composition which contains fewer molecules of C. The larger the amount of gas AB becomes, the smaller that of gas C will be become. This reaction would also be promoted by the removal of the molecules of A, as that would have the result that the accumulated molecules of BC are no longer decomposed by the molecules of A and hence the molecules of C will no longer regenerate. Therefore, if we implement both methods at once—supply of gas AB and removal of gas A—gas C will completely disappear without the need of a higher temperature, as required previously when an equilibrium between decomposition and recombination dominated.

The reverse result will occur when, in the mixture of AB, BC, A and C, we decrease the number of AB molecules, or increase the molecules of A, or both simultaneously. The decrease in AB will result in a decrease in the decompositions of AB and the formation of BC; hence more free molecules of C will remain. An increase in the molecules A will cause an increase in the decompositions of molecule BC, whereby molecules of C are released. Therefore, this reaction can only end with the complete isolation of gas C.

If the three substances, A, B and C, and their compounds are gases, as assumed so far, it is perhaps easy to add arbitrary amounts of each individually, but not to remove each individually, if one cannot use chemical methods. Therefore, one will rarely, if ever, succeed in bringing a reaction to completion solely by changing the proportions. However, one can approach completion to an arbitrary degree through addition of the appropriate gas.

It is different when one of the substances is a liquid or a solid. One example of this case would be the reaction of copper, water vapor and hydrogen (7). If one directs water vapor (AB) over glowing copper (C), hydrogen (A) and copper oxide (BC) are formed. If one directs hydrogen (A) over copper oxide at the same temperature, water vapor (AB) and copper (C) are formed. Here water vapor, in the first case, and hydrogen, in the second case, were added in excess to a limited amount of copper or copper oxide, and the emerging (by)products were simultaneously removed as gases. But if a limited amount of water vapor is heated in a closed tube with a limited amount of copper, only a part of the water is going to be decomposed and a part of the copper is going to be

oxidized and, for each degree of temperature, there has to be a certain ratio between the amount of water vapor, hydrogen, copper and copper oxide, at which there is an equilibrium between the oxidations and the reductions. The same occurs if one directs hydrogen over iron oxide and, conversely, water vapor over iron. Zinc, tin, cobalt, nickel, uranium and cadmium behave similarly (8).

If one directs hydrogen chloride gas over glowing silver, silver chloride and hydrogen form—conversely silver chloride is reduced by hydrogen. Zinc, tin and iron behave similarly towards carbon dioxide and carbon monoxide gas. These reciprocal reactions occur at the *same* temperature, as shown by specially designed experiments, (Gay-Lussac, Regnault).

Also related is the observation that many substances, formed by reaction with a gas, can only be distilled or stored in an atmosphere of the same gas (e.g., sulfur chloride in chlorine gas). Conversely, the escape of hydrogen bromide facilitates the action of bromine on organic substances in sealed tubes. Cases of predisposing affinity also belong here and are satisfactorily explained in a similar way. I would be able to multiply my examples indefinitely, but I believe that those given so far are sufficient to illustrate the proposed hypothesis and facilitate its application. It is applicable whenever a partial decomposition occurs. Moreover, the presence of the latter is revealed by a number of characteristics, among which are: the influence of time on the progress of a reaction; the incompleteness of a reaction when occurring in a closed space; reversibility; the necessity of excess reactants; the acceleration of a reaction by removal of products, etc.

III. The Relation of Williamson's Theory of Exchanges to the Proposed Hypothesis and Its Application to the Case of Double Elective Affinity

Already some sixteen years ago Alexander Williamson proposed an hypothesis concerning the nature of decomposition in his paper on the "Theory of Etherification," which is to some extent related to the one just presented. There he developed the view that "in an aggregate of molecules of every compound, there is an exchange constantly going on between the elements which are contained in it." Williamson's hypothesis has little to do with the theory of dissociation phenomena which I have developed in Section I, but is related to my method of explaining mass action and reciprocal affinity. This

explanation is based on the assumption of the simultaneous occurrence of opposing reactions in keeping with [the operation of] simple and double elective affinities. These reactions may also be interpreted as *exchanges*, whence the similarity of both hypotheses. However, they also differ substantially on several points:

Firstly I do not assume that *every* compound undergoes a partial decomposition (exchange), but rather only some compounds—though perhaps a great many—and these only above a certain temperature limit (which, of course, in many cases may be so low that we are only aware of the compound when in a state of partial decomposition).

Secondly my hypothesis includes the essential assumption that, within certain temperature limits, *not all* molecules are subject to decomposition (exchange) *at the same time*.

Thirdly I do not base my opinions entirely on the “motion of atoms,” but rather on differences in the *momentary states of motion of individual molecules* and view this as the basis for the possibility of simultaneously opposing reactions.

Fourthly I would like to assign the merit of greater universality to my hypothesis since all partial decompositions—even those which occur by heat alone without the intervention of another body (dissociation)—may be explained from the very same point of view, whereas I will now demonstrate that Williamson’s hypothesis cannot explain this latter mode of decomposition and was never intended to do so.

I will review these points in reverse order and will begin with the fourth one, which, it seems to me, most easily illustrates the relation between both view points.

The following schemes give an overview of three groups of reaction, along with their counter reactions, on whose simultaneous occurrence the phenomena to be explained are based (9):

I. Partial decomposition by means of heat alone (dissociation):



II. Partial decomposition by means of so-called simple elective affinity:



III. Partial decomposition by means of so-called double elective affinity (10):

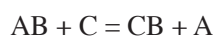


Williamson’s hypothesis is restricted to the explanation of reactions II and III and does so by the simple assumption that atoms (or groups of atoms) A and C constantly change places. The ensuing state of equilibrium is a simple result of the number of exchanges of A with B being equal to the number of exchanges of B [*sic.* C] with A. If we now try to apply this same manner of explanation to case I, it seems to work there as well. One simply needs to assume that the A within the compound is constantly interchanged with the A found in the free state. The state of equilibrium is thereby explained.

If the hypothesis is supposed to be correct for all three cases, it must not only explain the phenomenon of equilibrium, but also those phenomena which occur when the equilibrium is disturbed. These disturbances occur when:

- 1) One or more products of the reaction are removed.
- 2) The temperature is changed.

Experience shows that, when the equilibrium is disturbed by the removal of the products, the partial reaction changes into a complete [reaction] and the reciprocal reaction ceases to function. In the case of schemes II and III, this agrees with Williamson’s hypothesis; as may be seen if we consider scheme II:



If we remove all of the free A or CB that is formed or both, then the reverse exchange of A in place of C is no longer possible, though the exchange of C in place of A in AB can still occur and must lead to completion of the reaction. Conversely, the removal of AB or C or both results in completion of the reciprocal reaction. As with the above, so Williamson’s hypothesis also completely works for the reactions in scheme III. *But it no longer works* as an explanation for the disturbance of the equilibrium in scheme I, for, if this equilibrium also depended only on exchange, it would not be clear how just the removal of A or B or both would give the results that are, in fact, observed. We have the scheme:



If, for example, we now remove all of the free A, the exchange with the bound A will stop. The same is true for B. Its removal could only result in a cessation of the reaction and not in its completion, which is, however, what actually occurs, as shown by experience.

This situation clearly occurs in the specific example that I used earlier to illustrate the phenomenon of dissociation. We heat calcium carbonate in a closed tube. Carbon dioxide is formed. If we keep the temperature fixed at a constant value, the amount of carbon dioxide will also stay constant. This state of equilibrium can now be explained by both hypotheses. According to the hypothesis of exchange, free carbon dioxide molecules constantly switch position with bound molecules, which are, in turn, set free. According to the other hypothesis, it is assumed that the number of released carbon dioxide molecules is equal to the number taken up by the quicklime per unit time, *although the absorbed [molecules] do not necessarily substitute for the released [molecules]*. Therefore, according to the first hypothesis, every single release is necessarily coupled to an uptake, whereas, according to the second hypothesis, each release is independent of any given uptake, though the total number of both is constrained by the requirement of equality. This distinction seems to be negligible, but it immediately becomes crucial when we look at the following process. We direct air (or some other inert gas) through the tube and displace the carbon dioxide. Instantly fresh carbon dioxide is released. Now the first hypothesis is no longer sufficient because, in that case, one would need to assume that the air switches positions with the bound carbon dioxide, which is not the case. In contrast, the second hypothesis corresponds completely [to the facts], since, according to it, the combinations and decompositions are independent of one another [and] the first are easily reduced or eliminated by removal of the carbon dioxide, while the latter continue.

If one had caused the generation of carbon dioxide using [another] gas, capable of chemically combining with the chalk, then the difference between the two hypotheses would have remained undetected. Only the circumstance that the generation [of carbon dioxide] is also possible using an inert gas proves that only the second hypothesis can be correct.

One would reach the same conclusion on trying to explain the disturbance of the equilibrium caused by a change in temperature. According to the exchange hypothesis (11), the exchanges in cases II and III would become more frequent in one direction than in the opposite, until, as a result, the relative numbers of the different molecules had changed to such an extent that, once again, equal numbers of opposing exchanges are produced. From this point on, equilibrium would be established once more. [For these cases] the exchange hypothesis is sufficient. For case I it is not sufficient, as

one may be convinced after brief consideration. This may again be demonstrated using the previous example. The fact that calcium carbonate releases more carbon dioxide upon increasing the temperature of a closed space can only be explained by the assumption that the number of detached molecules becomes greater than the number which are simultaneously absorbed. This is not possible using a simple exchange. If one wished to maintain this [mechanism], one would have to consider two processes side by side—the exchange and the decomposition. However, it is simpler to assume that the individual combinations and decompositions are, in general, independent of each other. Thus the concept of an exchange requiring a pairwise coupling of both processes may be abandoned.

One could restrict the exchange hypothesis to cases II and III, for which they were devised by Williamson, and use the second hypothesis to explain case I. However, it seems to me more expedient to extend these hypotheses until they apply equally to all three cases, and this is most easily accomplished if one replaces the narrow conception of exchange with the broader conception of simultaneous individual and independent decompositions and combinations.

More important than the difference discussed above is the one found in *point three*. Williamson assumes an alternating transfer of the molecules [*sic.* atoms] in opposite directions, and hence an opposite movement of the same, without stating a cause for how these opposite effects are brought about. I find this cause, as already stated many times, in the momentary differences in the states of motion of individual molecules, as assumed in the hypothesis of Clausius. I have already explained the reactions found in schemes I and II, now I will attempt to explain the transfer found in scheme III.

We have molecules of AB and CD in a given volume. Depending on whether they are gaseous or liquid, they move throughout this volume in a linear or an irregular, but progressive, direction (external motion). Furthermore, their components move relative to each other, but are bound to a common center of gravity (internal motion). If one does not change the temperature, the sum of the *vis viva* of both motions will remain constant. Even the sum of the *vis viva* of the external motion alone, like that for the internal motion, will remain constant, since Clausius has proven that they must be in a constant ratio to one another. However, the external motion, as well as the internal, must be very unevenly distributed among the individual molecules. Therefore we have the following limiting cases:

- 1) Molecules possessing the maximum external and internal motion.
- 2) Molecules possessing the minimum external and internal motion.
- 3) Molecules possessing the minimum external and maximum internal motion.
- 4) Molecules possessing the maximum external and minimum internal motion.

Between these limiting cases, there exists, of course, all possible intermediate cases. The maximum for the internal motion is determined by the magnitude of the affinity. We do not know how the magnitude for the external motions is limited—indeed, it seems to me that the existence of such a maximum is not yet proven. However, this does not affect our method of explanation.

Upon the collision of two different molecules, the external motions can be increased at expense of the internal, or the internal at expense of the external, or, as a limiting case, both may remain unchanged. Among the diverse results of such a collision, the following cases should be stressed:

1) Two molecules, whose external and internal motions are very large, meet in such a fashion that, in the next moment, the external motions are completely, or for the most part, converted into internal motions which exceed the upper limits in both molecules. As a consequence, a separation into four parts, A, B, C and D, occurs.

2) Two molecules, whose external and internal motions are very small, collide. Here it is possible that the resulting internal motions are not only too small to split both of the original molecules, but also to prevent their permanent combination. An aggregated ABCD molecule results.

3) Two molecules collide under such conditions that the resulting internal motion is too small to split the molecules, but large enough to prevent a permanent connection. Hence they fly apart like elastic spheres and AB and CD remain as AB and CD

4) The molecules collide under such conditions that the interplay of the internal motions of the components of the transient double-molecule induce its splitting in a different direction. AB and CD collide and momentarily form ABCD. If the impact was—as we wish to assume in the simplest case,—linear and central, the whole system will continue to initially move in accordance with the

redistribution of various quantities of motion, the lost external motion having been transformed into internal motion. Now it depends on the magnitude of the affinity of A, B, C and D for one another and, at the same time, on the previously existing internal motions of the components of AB and CD, as to whether the split due to the increased internal motions occurs in the direction of AB/CD or in direction of AC/BD. The larger the internal motions of the molecules prior to collision, the greater the preparation for the separation of A, B, C and D and the easier it is for a split in the direction AB/CD to occur. One can see that, in general, the best conditions [for a double decomposition] are in those given earlier under limiting case 4.

In this manner it becomes obvious that, in addition to the affinities, the mode of decomposition further depends on the state of motion, and that, consequently, even those reactions that are apparently opposed by affinity may occur (reciprocal reactions).

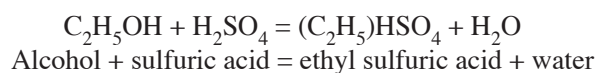
The first two of the four cases listed earlier require a larger difference in the states of motion of the individual molecules than do cases three and four. Hence it is highly likely that these are not fully achieved in many processes for which the difference is not large enough. This assumption may be made for all those reactions for which there is no basis for assuming the presence of the [product] molecules ABCD, A, B, C or D in addition to the molecules AB, AD, BC and CD. But it is also possible that one will find examples whose explanation makes this assumption necessary (12).

Now I come to the difference between the two hypotheses indicated in point 2. It is self-evident from the above. Although the collision of the molecules is random, they must, according to the principles of probability, result in regularity when the number of impacts becomes extremely large, such that the number of impacts resulting in decomposition always corresponds to the same fraction of the [total] number of impacts under the same circumstances. Therefore, in addition to those molecules that are decomposed (whose parts are exchanged), there will always be those that rebound without decomposition, which means, as I stated in point 3, that *not all of the molecules are being decomposed at the same time.*

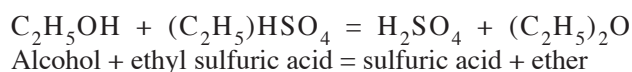
Finally, in point 1, I have described as essential to my hypothesis the assumption that a partial decomposition (exchange) does not occur at every temperature. When one considers that, according to the theory of heat, absolutely no motion of the molecules exists at -273°C , it is also apparent that for a considerable number of

degrees [above zero] the motion may be so small that it does not exceed the upper limit for internal motion and so induces no decomposition. For this reason there must be an upper temperature limit at which the reactions under consideration first begin. If, for a compound, this is situated higher than the temperature at which we are able to examine it, we will not be able to observe any hint of a decomposition (exchange), and in *this sense* I wish it to be known that Williamson's assumption that "in an aggregate of molecules of every compound, there is an exchange constantly going on between the elements which are contained in it," is restricted in the manner just described.

This also agrees with experience. As evidence, I will cite the same example which led Williamson to the discovery of his hypothesis, namely the formation of ether. This process divides into two reactions, each of which may be reversed. The first is given by the equation:



and the second by:



Both reactions show features that correspond to partial decomposition. In particular, both remain incomplete if one does not remove the products and they may also be reversed. A mixture of sulfuric acid and alcohol never forms so much ethyl sulfuric acid that some portion, not only of the alcohol but also of the sulfuric acid, does not remain unreacted. This is because the products formed—ethyl sulfuric acid and water—continuously give rise to the opposite reaction. These reciprocal reactions can become dominant if the proportions are favorable. It is known that ethyl sulfuric acid changes back to sulfuric acid and alcohol when boiled with water. This last reaction is reduced by removal of water (or ethyl sulfuric acid) (13)—hence, the less water added, the more ethyl sulfuric acid is formed initially.

By continuous removal of water and addition of alcohol the [first] reaction must go to completion in one direction. By addition of water and removal of alcohol it will go [to completion] in the other direction. At a certain ratio of the initial reagents, [the question of] whether the equilibrium between the opposing reactions will correspond to a greater or lesser degree of decomposition will depend on the temperature. Hence there must be a temperature value at which the reaction of the sulfuric

acid with the alcohol has not yet begun, at which the internal motions of the molecules—even those in which it is at a maximum—is insufficient, even with support from affinity, to cause a reaction. It is still unknown just how low this temperature value is, but the circumstance that dilute sulfuric acid only forms ethyl sulfuric acid upon heating, suggests that it cannot be very low.

For the second reaction between the alcohol and the ethyl sulfuric acid the same relationship occurs. On reversal, ether and sulfuric acid result in ethyl sulfuric acid and alcohol. Hence even here, if the ether cannot be removed, the degree of conversion must remain fixed at equilibrium, where both opposing reactions occur side by side with the same frequency.

Now, if the water as well as the ether are continuously removed by distillation during production of the latter, both processes will go to completion in one direction, as in both the reaction favoring ether formation outweighs the reverse reaction. If both processes occurred at all temperatures, ether would form at all temperatures sufficient to remove the ether and water by distillation. But this does not happen because only the alcohol distills off below 126°C [at which temperature] the second reaction, at least, cannot have yet begun. This single example will serve for many additional examples that could be quoted in support of the statement that (partial) decomposition (exchange) is correlated with a certain temperature value. I believe that I have now sufficiently discussed the relationship between my proposed hypothesis and the exchange hypothesis of Williamson.

IV. Summary and Conclusions

The assumption that atoms are in a state of rest was first challenged by physicists. However, for quite some time their work attracted little attention and was nearly forgotten. As far as I know, Williamson was the first chemist who—independent of physical arguments and based on chemical facts alone—rejected the assumption of static atoms. His inspired theory of ether formation was accepted, but his simultaneous, and more important, presentation of his theory of the continuous exchange of elements remained almost unnoticed.

The epoch-making papers by Krönig, and especially those of Clausius, abolished the assumption of static atoms forever. Sooner or later the triumphant progress of the mechanical theory of heat had to attract the attention of chemists and invite attempts to apply the highly fruitful assumptions of this new theory to the explication of as yet

unexplained chemical phenomena. The present work is such an attempt. Starting with the theory of evaporation proposed by Clausius, I first attempted an explanation of dissociation. Generalization of this approach allowed a transition to reciprocal reactions and the mass action effect. The results of my approach clearly show that the theory of gases as given by Krönig is insufficient and that the more elaborate theory of Clausius is quite indispensable.

It was of great interest to me that volume 101 of this journal contained a paper by Clausius [entitled]: "On the Electrical Conductivity of Electrolytes," in which the fact that very small currents can cause decomposition is explained by postulating that the parts have previously been in partial conversion. Here Clausius refers to Williamson's paper. It now seems to me that my method of explanation agrees even better with the theory of electrolysis. The increase in the conductivity of liquids with temperature may be related to their increasing dissociation. Those that do not conduct are unaffected by dissociation at the temperature in question. However, these are mere assumptions. In the near future, I will amplify this communication with some ideas concerning the constitution of mixtures and solutions, which are related to the above topic.

V. Addendum (14)

Only after completion of this contribution did I discover the critique of Deville's theory of dissociation by Dr. H. W. Schröder van der Kolk (15). Although I now believe that the most important objections which it raised have been refuted by the above arguments, in order to ensure a complete resolution of this interesting and important matter, I will allow myself, even at the risk of repetition, to add the following [remarks].

I completely agree with Schröder van der Kolk that the extensive measurements of flame temperatures by Deville are open to several objections. Likewise, I think his opinion that the temperature of the flame—even without the assumption of dissociation—should be lower than the calculated values is reasonable. Thus one argument in favor of the theory of dissociation is no longer applicable.

However, in spite of this objection, I have to retain the assumption of dissociation and the belief that there is a profound analogy between this and evaporation. I was led to this opinion independently and without knowing that Deville had already mentioned it earlier, for the reasons discussed above.

It appears to me that the disagreements between Schröder van der Kolk, on the one hand, and Deville and myself, on the other, are focused on the following point: According to the former, it is solely the absorption of heat from the decomposition of the initial molecules which serves to "cool" (16) the neighboring molecules and thus accounts for the *delay in the decomposition of all of the molecules* (and which, for lack of time, also prevents it).

According to the opposing view, this retarding influence is certainly present and is sufficient as an explanation for many partial decompositions; but in addition to this retarding influence there is, for compounds undergoing dissociation, yet another cause, which, within certain temperature limits, not only delays the *complete decomposition* but altogether prevents it, however long the temperature is maintained. In opposition to this Schröder van der Kolk then raises a most important argument, which I will directly quote, while retaining only the most important parts:

It seems to me that this theory appears to contain an inner contradiction. Water vapor is decomposed at [temperature] T through simple heating. This decomposition gradually progresses and will be complete at constant temperature T provided that a sufficient amount of heat is supplied. This temperature T may change with the pressure, but, in any case, is always the same at the same pressure. At a lower temperature decomposition apparently cannot happen, otherwise it would not be T , but a lower decomposition temperature [that would correspond to the dissociation temperature]. Indeed, the author (Deville) says that in this case the decomposition is only partial; *but if it occurs partially, then it must also be possible for it to become total as soon as the decomposition is viewed as a function of only the temperature*, as is the case with Deville.

This is the very same difficulty which I pointed out at the beginning of my contribution and which I think I have eliminated by the necessary assumption of unequal states of motion for the individual molecules.

According to the mechanical theory of heat, the temperature is proportional to the average *vis viva* of the molecules. If one transfers this concept of temperature to individual molecules, one could argue that the temperature of the individual molecules is unequal, although the parts of the body to which they belong have attained a mutual equilibrium of temperature. The temperature of the body is the average temperature of all its molecules.

The decomposition is now a result (function) of temperature, and hence it is now possible that, within

certain limits of the average temperature of the body, it extends only to that portion of the molecules which have exceeded a certain temperature limit. The circumstance that the initial decomposition of a molecule depends only on its *internal* motions requires, in turn, that one distinguish between the internal and external temperature of the individual molecules. This has led me to not employ this extension of the term “temperature,” not least because the phrase “internal and external motions” expresses the meaning much better than the phrase “internal and external temperature.”

In resolving this *internal* contradiction of the theory of dissociation, it seems to us of greater importance to prove by experiment that partial dissociation, even at temperatures produced by sufficient heat input over long periods, does not proceed to complete dissociation (except in the cases presented by me, e.g. by diffusion). Likewise, I consider this evidence as already provided by the often mentioned experiment with calcium carbonate, and hence it seems unnecessary for me to explore additional reasons for partial dissociation. Another proof are the so-called abnormal vapor densities (17) measured using the method of Gay-Lussac, which, as I have convinced myself by experiment, result in constant numbers with prolonged heating so long as the temperature of the vapor remains constant.

In the end Schröder van der Kolk expresses the view (page 507) that, based on the molecular theory of chemical compounds, it should be possible to develop a [theory of] partial decomposition related to mass action. I would be delighted if I have succeeded in this paper in making a contribution to the foundations of such a theory.

VI. References and Notes

- I take this from the abstract of his paper “On Dissociation in Homogeneous Flames” in *Chem. Centralblatt*, **1865**, p. 662, since the original paper is not available to me.
- This journal, Vol. 100, p. 353.
- In time I will show that sulfuric acid may be dried with air just as one is accustomed to drying air with sulfuric acid.
- Krönig already derived the principle of diffusion from his theory of gases (this journal, Vol. 99, p. 320). I briefly mention this because in the widely known monograph of Paul Beis on *The Nature of Heat*, page 150, it is erroneously stated that Krönig did not state this correlation.
- Two things follow from this: Firstly, that the two methods of vapor density determination by Gay-Lussac and by Dumas should show different results under the same circumstances, since diffusion is impossible with the first, but possible with the second. Secondly, that with the second method, the resulting values would not only be dependent on the temperature, but also on the duration of the experiment. (See note 14).
- Bull. soc. chim.*, **1865** p. 90. The remark in question reads: “In fact one frequently finds that it appears to be the general state of affairs that, when a body is decomposed, the presence of the products of the decomposition exercises an influence on the progress of the reaction. There is a tendency to establish a chemical equilibrium between certain proportions of the original products [i.e. reactants] and the products of their decomposition. When in the course of time equilibrium is attained, the decomposition is arrested, etc ... A result which one may express in general terms using the specific case of ammonium chloride, etc...”
- However, I will later show that, technically speaking, this example belongs to the case of double decomposition discussed later.
- Gmelin, Vol. I, p. 118.
- But it should not be said that this scheme includes all reactions requiring a similar explanation. If anything, more complicated ones can exist. The three cases listed are sufficient, since the more complicated ones may be referred back to them.
- The following reactions could be considered as special instances of case III:

IIIa $AB + CC = AC + CB$ and $AC + CB = AB + CC$
 IIIb $AA + BB = AB + AB$ and $AB + AB = AA + BB$
 Scheme IIIa will be assumed to be the correct one for many reactions, for which scheme II appears to occur. The same goes for scheme IIIb, which can be assumed in place of the often occurring scheme I. For example, the instances cited by me in Part I [of this paper] as special examples of case II would come under IIIa, since, free hydrogen is considered to be HH rather than H.
- Williamson did not mention the influence of temperature on the rate of exchange. However, it should behave as indicated.
- For example, the development of so-called condensed compounds at high temperatures. This journal, Vol. 131.
- Lieben assumed that, in order to remove the disturbing influence of the products of a reaction on its progress, one had to remove *all* of the resulting products. According to the proposed hypothesis, the removal of only one is necessary, as this is sufficient to make the reverse reaction impossible. This is in keeping with experience.
- Supplement to the addendum. Since the molecules of gaseous bodies under increased pressure are closer together, they will collide more often. As a result of this, reactions based on partial decomposition reach an equilibrium or end faster than under otherwise identical conditions. It seems to me that the dissociation of *vapors* can progress further, before equilibrium is reached, at lower pressures than at higher [pressures], because the number of decompositions, which decreases for compounds in the first case and increases in the second, remain equally great. Hence, when determining the vapor density according to [the


method of] Gay-Lussac, one will increase (although less severely) the [degree of] dissociation (and hence the error in the molecular [weight] determination) by a decrease in pressure, as well as by an increase in temperature, if the vapor is already in a state of dissociation, Only by decreasing the pressure at a temperature low enough that dissociation has *not yet occurred*, can one can avoid error when determining the density.

15. This journal, Vol. 129 p. 481.
16. I would substitute "hinders greater heating."
17. In particular, those leading to values that do not exhibit a simple relation to those that are calculated.



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BOOK REVIEWS

Nuclear Reactions. By Adam Ganz. A 45-minute radio drama based on the Farm Hall Transcripts. Performed on the BBC Radio Afternoon Theater 15 June 2010. Producer Eoin O. Callaghan. © Adam Ganz. The play may be streamed or downloaded from the URL* <http://www.rhul.ac.uk/mediaarts/news/nuclearreactions.aspx>

Well before the end of World War II, one of our lecturers at University College, London, was teaching us about nuclear chemistry and mentioned nuclear fission, a fact that had been discovered by Otto Hahn and Lise Meitner in Germany in 1939 but was quickly hushed up once war had been declared. It became clear that the fission produced more neutrons than the number needed to cause it, and that therefore a chain reaction could be started which also would produce a significant amount of energy. If, the lecturer commented, such a system could be incorporated in a bomb, an explosive of vastly greater energy could be harnessed than anything known until then. At the moment he mentioned it, a powerful detonation caused by a mine went off at the end of the pier in Aberystwyth, Wales, a seaside resort to which UCL's chemistry department had been evacuated for the duration of the war.

In August 1945 nuclear bombs were dropped on Hiroshima and Nagasaki killing tens of thousands of Japanese civilians. President Harry Truman announced it to the world. I wandered the streets of London in a daze, wondering if I wanted to spend the rest of my life pursuing a discipline that could be used to cause such vast suffering.

We soon learned that the American “Manhattan Project,” as it became known, had been initiated through a letter signed by Albert Einstein in 1939 and carried by his friends to President Roosevelt informing him of the likelihood that Nazi Germany was developing such a weapon and that it was imperative that the Allies create it first. *Nuclear Reactions* begins with a different document, one drafted by Otto Robert Frisch and Rudolf Peierls in England a few months later which calculated that the “critical mass” could be quite small and could be delivered by plane rather than having to be of the mammoth size assumed until then.

How close did Germany come to producing a nuclear weapon? Did some of its nuclear scientists try to slow down the effort? If Hitler had the bomb would he have used it—on London?

As World War II was ending, the Allies asked the Dutch-American physicist Samuel Goudsmit to head a search which has become known as Alsos (Hellenic for Grove, the name of the military head Leslie M. Grove of the Manhattan Project) for the key German scientists who might have been involved in the German nuclear project, in order to intern them. It is generally believed that one concern was to prevent them from falling into Soviet hands. At least ten were found and were taken to Farm Hall near Cambridge, England, where they were treated very well—and were bugged. British Intelligence officers listened to and recorded their conversations, among them the father of Adam Ganz, who last year created a BBC

radio play *Nuclear Reactions* which was aired on June 15, 2010. Ganz is a filmmaker on the faculty of Royal Holloway, University of London. His father, a philologist, was rounded up and put in a concentration camp after the 1938 Kristallnacht, but managed to flee Germany before the war started and after the war became a professor at Oxford and an authority on Jacob Grimm. During the war he was briefly interned by the British but then recruited for British Intelligence. Among the Farm Hall inhabitants, from July 1945 to January 1946, were Werner Heisenberg, Carl Friedrich von Weizsäcker, Otto Hahn, Walther Gerlach, Max von Laue, and Kurt Diebner. Most of the recorded conversations were in German. They were made public and translated but we do not know if other parts were withheld. Here is an excerpt:

Diebner: I wonder whether there are microphones installed here.

Heisenberg: Microphones installed? (*laughing*) Oh no, they're not as cute as all that. I don't think they know the real Gestapo methods; they're a bit old fashioned in that respect.

In the play the two secret listeners are of opposite sex, the woman a charming young person who says she had been a student in Göttingen before the war. In reality both listeners were male but it was suggested to Ganz that the change would make it easier to follow the conversations. At one point she asks if torture would be used but was told the British wouldn't resort to that, and they were confident sufficient information would be gained without. During their internment they were at times visited by British scientists, among them P. M. S. Blackett with whom some of them had had cordial relations before the war. One question they asked him was whether it was true that Faraday was confronted with the possible use of poison gases such as chlorine as a weapon of war.

Von Weizsäcker: Is it true what they say about Faraday and poison gas?

Blackett: Yes it's absolutely true. He was asked if it would be possible to develop poison gas for the British in the Crimean War. He said it was perfectly possible, but he was damned if he was going to do it. He meant it literally of course; he wasn't the type to swear. Very religious man.

The play begins in the plane carrying the internees to England and there already the worry about their personal future (whether they would be considered war criminals) and the future of Germany (whether agrarian or allowed to re-industrialize) surfaces.

The play records the BBC announcement of the dropping of the Hiroshima bomb on 6 August 1945,

an event that caused considerable surprise among the Germans. They had not expected it for several years and found it hard to fathom that the Allies had mobilized 180,000 individuals for the bomb's development. They decided that Hitler would never have agreed to a project of such magnitude when success was by no means certain and he was looking for quick results. In retrospect it is almost amusing to think that President Roosevelt agreed to launch the Manhattan Project because he was convinced by Einstein and others of the imminence of a German nuclear weapon.

In November 1945, The Swedish Academy announced the award of the 1944 chemistry Nobel Prize to Otto Hahn. Not knowing his whereabouts they could not congratulate him. He learned about it from the announcement in the British press. His fellow Farm Hall inmates, as can be heard in the play, celebrated the occasion with speeches, toasts, and song, and he was encouraged to write to the Academy that he accepted the award but could not attend the festivities. He received the award from the Swedish King in 1946. Lise Meitner is mentioned. She worked with Hahn in Berlin on bombarding uranium with neutrons in the hope of creating transuranic elements but had to leave—first to Holland and then Sweden—because of her Austrian Jewish background. Hahn wrote to her that he had found barium in the experiment and she (together with Frisch) was the first correctly to interpret the result as due to fission, the splitting of the uranium atom behaving as a liquid drop. There has been much criticism that she did not share in the Nobel Prize—or receive her own.

Neither Hahn nor von Laue had anything to do with the Nazi atomic weapon project. Von Laue, who had received the physics Nobel Prize in 1914 for his work on X-ray diffraction by crystals, had made his anti-Nazi views known during the war but was left alone perhaps because of his fame. It is reported that the creation and use of the nuclear weapon depressed Hahn greatly. But it seems he recovered his sense of humor. He collected some cartoons relating to the nuclear announcement—both English and German—and Adam Ganz sent a copy of the set to this reviewer. Here are descriptions of five of them:

-The first shows two very disappointed little boys in a shop selling chemistry sets. Big sign: Sorry NO uranium.

-A mother to her little girl who just heard about the bomb on the radio: "It's a new kind of bomb, darling, for the benefit of mankind."

-Wife to her lazy husband lounging on the sofa: “Now if science could only find a way to split you and release your energy.”

-The beginnings of nuclear splitting: Adam in the Garden of Eden trying to crack a coconut between two rocks.

-German nuclear scientist after Hiroshima: “I’d feel better if I had taken over dad’s herring shop.”

In January 1946 the Farm Hall residents were allowed to resume civilian life in Germany. Some of them later made major contributions to the rebuilding of German science and to its reintegration into the world scientific community.

This play no doubt will be of intense interest to those who remember that period and the news at the time of

the Farm Hall internments. Given the recent Japanese Fukushima-Daiichi triple catastrophe, the promise and hazards of nuclear energy have once again become of current interest, and I imagine that a presentation of some of the earliest beginnings of the human nuclear experiment will be widely welcomed.

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* Should this prove difficult and you would like a CD mailed to you from London, please email MediaArts@rhul.ac.uk, with Nuclear Reactions CD in the heading; or write Department of Media Arts (Nuclear Reactions CD), Royal Holloway University of London TW20 0EX, UK. Be sure to send your mailing address including your country.

Radioactive: Marie & Pierre Curie, A Tale of Love and Fallout, Lauren Redniss, It Books (HarperCollins), New York, 2010, 208 pp, ISBN 978-0-06-135132-7, \$29.99.

Even before opening *Radioactive: Marie & Pierre Curie, A Tale of Love and Fallout* its title and cover tell you that you are about to engage with a unique text. The words Radioactive and Fallout viscerally evoke the dangers of the nuclear age that began, in part, with the Curies’ discoveries, while metaphorically encapsulating the mystique and passion of the life that they had together. The cover art foretells the visual nature of the text to come—a luminescent, almost tactile, graphic biography.

Opening the book confirms the inklings of intrigue promised by the cover. From the red ghostliness of the flyleaf and the blue chemical wash of the title pages it becomes immediately apparent that Lauren Redniss has individually crafted each page to evocatively reiterate the book’s “radioactive” themes; thematics that include the Curies’ discovery of radioactive elements, their and their culture’s fascination with these element’s extraordinary properties, the curative and destructive nature of nuclear radiation, as well as the Curies’ personal and professional loves and losses. Yet, even before reading any of the text, it calls out to be looked at—perused, like a beautiful coffee-table book. One particularly striking visual aspect

of the book is the use of cyanotype printing, a method some readers will be familiar with from grade-school projects using blue “sunlight” paper. As Redniss explains at the end of the book, using this chemical process has multiple resonances with the text’s narrative. It captures “what Marie Curie called radium’s spontaneous luminosity,” mimics the “photographic imaging that was central to the discovery of X-rays and radioactivity,” and, in its production of Prussian blue upon exposure of the paper to sunlight, yields a compound that is a “safe and effective treatment for internal contamination by radioactive cesium and radioactive thallium” (pg. 199).

The story of Marie Curie’s life has been told many times before, so it is not the telling, but the way it is told that matters. Unlike Barbara Goldsmith’s *Obsessive Genius*, which seeks to reveal the truth behind the myth of Madame Curie, Redniss revels in the imaginative space generated by Marie and Pierre’s lives and work. Linking together science, spiritualism, desire and death, Redniss’s history of the Curies is a book unlike any other that I have read; fragmentary, associative, and compressed, she creates a lyrically combusive whole. This is an artistic rendering of the Curie’s lives and not a comprehensive biography. The narrative, although chronological, is held together in a loose web of information, quotes, images and asides that reinforce the visual and cultural power

of the book: page thirty-eight contains only the statement, “Two years and two months into their marriage Marie gave birth to a six-pound baby girl. They named her Irene;” page forty-three, a reproduction of an X-ray image of Wilhem Röntgen’s wife’s fingers and wrist; and page eighty-three, a brief but devastating recounting of the bombing of Hiroshima from survivor Sadae Kasaka. Even the chapter titles play a part in weaving the metaphorical interconnections Redniss seeks to make, highlighting the way in which scientific language encapsulates and creates meanings within and beyond the boundaries of science. The first chapter “Symmetry,” a key conceptual framework underlying much of modern physics, is laid out so that the story of Pierre and Marie’s early lives, in the form of biographical fragments and quotes from their collective papers, mirror one another on each of the facing pages of the chapter, while the last chapter, “Daughter Elements,” a term borrowed from the vocabulary of nuclear physics, tells the story of Marie’s death and the carrying on of her work by her daughter Irene Joliot-Curie.

One might imagine that such a fragmented biographical narrative might be difficult to follow, but somehow Redniss is able to strip bare the details and distill the essential moments of the Curies’ passionate personal and scientific life while simultaneously providing an abbreviated, yet exhilarating history of radioactivity in the twentieth century. She captures how serendipity, hard work, and genius coalesced in their scientific work, how magical radioactivity seemed at the time of their discoveries, and how shockingly dangerous, in retrospect, the experiments they carried out were. *Radioactive* reverberates with energy and captures the cataclysmic cultural changes—nuclear energy, war and medicine—wrought by their (and other’s) groundbreaking science; a powerful and unique book, for historians, and non-historians alike.

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Nothing Less Than an Adventure: Ellen Gleditsch and Her Life in Science. Anne-Marie Weidler Kubanek, Crossfield Publishing, Montreal, Canada, 2010, 185 pp, ISBN 1452842132, \$19

In the celebration of the outstanding contributions of Marie Curie, it is widely overlooked that there were other women active in the field of radioactivity during that period. Ellen Gleditsch was one of those forgotten women radiochemists. This definitive book by Kubanek finally brings recognition of the contributions of Gleditsch to the English-speaking world. In addition to researching correspondence of Gleditsch with contemporary scientists, Kubanek painstakingly tracked down and interviewed surviving relatives, friends, and former students of Gleditsch. Kubanek has woven their commentaries into this fascinating biographical study.

Gleditsch was born in 1879 at Mandal in southern Norway. Having a fascination with science from an early age, she excelled in school, particularly mathematics. Had she been a boy, Gleditsch would have progressed to university; instead, her father found her a position as

a pharmacy assistant. After qualifying as a pharmacist, Gleditsch tried to obtain a university education and, in this quest, she was aided by a chemistry professor at the University of Oslo, Dr. Eyvind Bødtker. Bødtker hired her as a laboratory assistant and he encouraged Gleditsch to publish her research. Bødtker visited Paris and pestered Marie Curie to accept Gleditsch into Curie’s research group. Initially very reluctant, Gleditsch’s publication in the *Bulletin de la Société Chimique* plus a promise by Bødtker that Gleditsch was so tiny that she would not take up any significant room in the lab, persuaded Curie to accept her.

Arriving in Paris in 1907, Gleditsch was given the task of recrystallizing the mixture of barium and radium salts in order to concentrate the radium. In addition, Curie asked her to check the claim by Sir William Ramsay that copper, in the presence of radiation, was transformed into lithium. Gleditsch showed that the lithium came from contamination of the reaction vessel, and not from any element transmutation.

Leaving Paris in 1912, Gleditsch applied in 1913 to work for a year with Bertram Boltwood at Yale. Boltwood

sent a discouraging reply to Gleditsch, but by that time, Gleditsch had already embarked on a ship for the United States. Upon her arrival at Yale, Boltwood agreed to have Gleditsch work with him, and subsequently they became good friends. While at Yale, Gleditsch established a precise value for the half-life of radium. She also worked on the atomic mass of lead, which had been shown to differ from one mineral source to another. As meticulous as ever in her measurements, Gleditsch's results provided key evidence for Soddy's discovery of isotopes.

Returning to Norway, Gleditsch obtained poor-paying, low-status positions at the University of Oslo. In June of 1916, Curie requested Gleditsch to return to Paris to work at the radium extraction factory. Gleditsch undertook the perilous wartime crossing from Norway, first to England where Ernest Rutherford had obtained a security pass for her, and thence to France. Gleditsch worked at the factory until Christmas before returning to Norway. After the War, in 1920, Curie asked her to return to Paris to run the research centre while Curie was on a tour to Brazil. The friendship and contacts between Gleditsch and Curie continued for the remainder of Curie's life.

At the University in Oslo, Gleditsch's teaching commitments mounted while she endeavored to continue research. Finally, in 1929, against considerable opposition, Gleditsch was appointed Professor of Chemistry.

In the 1930s, as the political situation deteriorated in Europe, Gleditsch offered haven to as many fleeing scientists as she could. Though Lise Meitner had fled to nearby Stockholm, Kubanek points out that, surprisingly, Gleditsch had little contact with Meitner, even though Gleditsch visited Stockholm periodically to see her long-time friend and colleague in radiochemistry, Eva Ramstedt. Throughout the Second World War, Gleditsch was active with the Norwegian resistance movement.

After formal retirement, Gleditsch continued with lecture and laboratory work, while expanding her diverse other interests. Despite having been exposed to so much radiation and suffering from periodic bouts of anemia, Gleditsch lived an active life until her death in 1968 at age 88.

This short review has focused upon Gleditsch's activities within radiochemistry. Kubanek's book has gone beyond this, giving a true sense of Gleditsch's life in the context of a woman scientist in early twentieth-century academia. In addition, there are interesting insights into the workings of the Curie laboratory. Kubanek should be congratulated for having filled a missing piece in the early history of radioactivity.

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The First Miracle Drugs: How the Sulfa Drugs Transformed Medicine, John E. Lesch, Oxford University Press, Oxford & New York, 2007, xii + 364 pp, ISBN 0-19-518775-X, \$29.95

John Lesch accomplishes a great deal with this exhaustively researched and well-written narrative about a chapter in the history of science and medicine that has received surprisingly little attention. *The First Miracle Drugs* superbly explores the historical importance of sulfa drugs, persuasively showing how they sparked an expansion of pharmaceutical research and production, and "at the same time effected a significant change in the direction of medicine." (p. 7)

Developed in the 1930s, and used extensively until the mid 1940s to treat common bacterial infections (and

particularly effective against streptococcal infections), sulfa drugs rather quickly were overshadowed by penicillin and other antibiotics. What Lesch reveals, however, is how sulfa drug research, application, and chemical theories were instrumental to advances in twentieth-century biomedicine.

First, sulfa drugs demonstrated the power of chemotherapeutic agents, initially developed by Paul Ehrlich during his quest for an anti-syphilitic agent in the early 1900s. Based on the proposition of chemotherapy—namely that chemical compounds introduced into the host organism could destroy disease-causing microorganisms—sulfa drugs represented a successful and dramatic step forward in one of the most important pathways in medical therapeutics. On Christmas Day 1932, the Ger-

man I.G. Farben company applied for a patent for Prontosil, unveiling a breakthrough drug that opened a “new era in the chemical treatment of bacterial infection.” (p. 61) Lesch adds fresh material and analysis to the pivotal biography of Gerhard Domagk, who was awarded the Nobel prize in 1939 (although the Nazis prevented him from receiving it at that time) for the development of Prontosil, the first sulfonamide azo compound.

Second, sulfa drugs fueled expansion of pharmaceutical industries in Western Europe and the United States, and seeded growing international networks of scientists, physicians, and researchers. Lesch adroitly analyzes the complexities of pharmaceutical research and development in the context of Nazi Germany, carefully considering the activities and stances of I.G. Farben’s research manager, Heinrich Horlein. After looking closely at the historical record, Lesch concludes that Horlein, who was tried and acquitted for war crimes at Nuremberg along with several high-level I.G. Farben executives, only joined the National Socialist party for “tactical accommodation rather than ideological conviction,” and cites several instances of actions Horlein took to protect German science from Nazi influence (p. 109). Furthermore, Lesch traces the uptake of sulfa drugs, and the dynamics of their eventual development in France, Britain, and the United States. For example, the United States was the last to embrace sulfa drugs. It was not until 1936 that two Johns Hopkins University medical researchers attended the Second International Congress on Microbiology in London in 1936 and learned about Prontosil and the second generation Prontylin, which they then tested in experimental and clinical trials across the Atlantic.

Finally, Lesch illuminates the compressed yet intense years from the mid 1930s to the early 1940s when several types of sulfa drugs were the most effective therapies against an array of infectious diseases including pneumonia, gonorrhea, meningitis, bacillary dysentery, hemolytic streptococcal infections, and wounds and

burns. Although their efficacy was uneven, and particularly weak for wound infections, sulfa drugs “carried the main burden of treatment and played an important role in prevention throughout World War II.” (p. 249) Yet once penicillin entered the battlefield in January 1943, proving its effectiveness for all conditions except for bacillary dysentery, penicillin and subsequent generations of antibiotics soon eclipsed sulfa drugs. Their resounding success helped to erase awareness of the significant role of sulfa drugs in combating what until the 1930s had been humans’ greatest bacterial threats.

The First Miracle Drugs is a significant achievement. Not only does it restore the oft-forgotten role of sulfa drugs, it also provides insight into the *modus operandi* of prominent pharmaceutical industries during the pivotal decades of the 1930s and 1940s. With no chip on his shoulder, Lesch evaluates the strengths and limits of key players involved in the research, development, and clinical application of sulfa drugs. Furthermore, in the book’s concluding chapters, Lesch shows how attempts to explain the limits of sulfa drugs, namely the Woods-Fildes Theory, helped to generate the contemporary antimetabolite concept, which guided innovative research in medicinal chemistry (above all, for tuberculosis and leukemia) in the second half of the twentieth century.

In sum, Lesch’s book is model scholarship for the history of science. He weaves together sufficient scientific mastery of chemistry with an ability to explain the significance of seemingly small details at the laboratory bench, thus demonstrating the larger significance of the story of sulfa drugs to biomedicine, scientific knowledge networks, and the politics of war and disease during the World War II era. One hopes that *The First Miracle Drugs* will receive a large readership despite the fact it is published only in cloth and at times requires some basic knowledge of chemistry.

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Boyle: Between God and Science, Michael Hunter, Yale University Press, New Haven, 2009 (paperback 2010), 400 pp, ISBN 9780300123814, \$28.

As readers of this journal will know well, Robert Boyle has never been far from the center of studies of the scientific revolution in England. But in the past couple of decades there had been a remarkable amount of new information about him and his world brought to light, as well as great number of new interpretations of him and his work. Michael Hunter has been close to the heart of this revival of Boyle studies, since he has not only contributed to the interpretative literature but has been instrumental in editing and publishing some previously unknown works of Boyle, a new edition of all his works, an edition of his correspondence, and the catalogue and electronic edition of his papers (called the “work-diaries,” at www.livesandletters.ac.uk/wd/index.html). The achievements are enormous, and make it possible to reconstruct the way Boyle put together his written works in ways that would never before have been thought possible. In many of these endeavors Hunter has also had expert assistance from collaborators, many of whom have themselves also contributed substantially to the recent flow of books and articles. Clearly, no one knows the historical remains of Boyle and his associates, and the arguments about them, better than Hunter. His summary in a compact and readable biography is therefore most welcome. It will be undoubtedly remain the authoritative “Life and Works” of Boyle for the foreseeable future.

Hunter’s life of Boyle is structured according to the common biographical approach, from family history and birth, to productive years, death, and legacy. The chapters are not only chronological but thematic, since Hunter organizes each around a topic that he believes best characterizes Boyle’s doings and writings for each period of his life. A “Bibliographical Essay” arranged according to the themes of the chapters, and containing a good deal of guidance to secondary sources not mentioned in the notes, follows, as does a record of “Boyle’s Whereabouts, 1627-1691;” the index is substantial. While no single biography can do justice to all aspects of a life and works, the scrupulosity of Hunter’s record will provide an open and accurate guide for anyone looking for further illumination.

But the interest of the work goes beyond Hunter’s main agenda of giving an honest and careful accounting of a major figure. For it is Boyle’s own scrupulosity that organizes Hunter’s judgments about him. Whether Hunter was drawn to Boyle because he saw a kindred spirit in him, or whether the years of work on the Boyle

papers had their effect on Hunter, a deep sympathy between author and subject is evident. It is the adult Boyle’s pre-occupation with casuistry (the examination of conscience, on which Hunter has previously written authoritatively), which he places at the center of his assessment, with Boyle’s interest in experimentalism a kind of related careful and exacting examination of God’s creation—hence the title of the book. Hunter notes the young Boyle’s reports of his frequent moments when he allowed his self-described melancholic imagination free reign (which he called his “raving” and “roving wildness”), although Hunter wonders not whether he was a day-dreamer but “mildly autistic” (p 35). He also mentions Boyle’s enthusiasm for the new romantic literature coming from France, and later on the time spent with witty and fashionable men and women; he also remarks that it was Descartes’ *Passions of the Soul* (1649) that was the only work of the famous philosopher that he read more than once (p 106). Boyle’s refusal to marry is touched on gently, but whether this was due to the death of his mother when he was three years old or a domineering father, sexual dislikes, uncertainties, or modesty—as a boy, he was taken on a tour of the brothels of Florence but later said he had not indulged the flesh—a broken heart, deep affection for two of his sisters, a religious or philosophical commitment to celibacy, the lack of necessity of a younger brother to carry on the family, or the self-conscious preference for remaining free of entanglements, or all of the above, is left an open question. So, too, Boyle’s apparent deep interest in witchcraft and demonology, as well as in transmutational alchemy and other mystical attachments, and the various personal contacts he seems to have had with adepts, are also touched on. But since much of Boyle’s notes on such topics were destroyed by previous biographers as an embarrassment, Hunter has little evidence to work with. Parts of the life, and even perhaps of the work, therefore, remain shrouded in silence. Although Hunter says more than once that it is unfortunate that so little about these matters can be said, and that he is making his best effort to move beyond Boyle the experimental natural philosopher, he sticks closely to the evidence and refuses to speculate. Despite his best intentions, then, Hunter’s biography remains that of the Boyle known to the history of science. He does not give voice to the absences or attempt a reappraisal. Boyle’s relationships with his family and estates, colonial and proselytizing projects, and even laboratory assistants are carefully drawn, but never dominate the life of the mind engaged in exposing nature to view. Given the enormous amount of paperwork on that latter subject which remains, one

can well imagine that most of Boyle's energies must have been given to that pursuit. We hear distinct voices from the self-conscious remains of the Boyle archive, catching only distant echoes of a few others.

Hunter's Boyle will therefore be open to further interpretation. For example, I think it would be possible to bring Boyle's medical interests closer to the center of the life. Hunter shows that Boyle's later personal account dated his early interest in natural philosophy to 1640, when he was in Geneva on his Grand Tour and read Seneca's *Natural Questions* (pp 49-50). But Hunter also shows that the work-diaries indicate that he started experimenting only in 1649, mentored by a number of medical practitioners who took an interest in chemistry. His last works were also devoted to medicine, and in between he learned anatomy and physiology by dissecting with William Petty in Dublin in 1653 or 1654, while (as Hunter shows clearly) his most famous early work, the *Usefulness of Experimental Naturall Philosophy* (1663) was organized according to the genre of medical textbooks known as the *Institutes*. During the 1660s and 1670s Boyle was often linked to the apothecaries and chemists who were fighting their public wars of

liberation against certain older traditions of medical physic, while in this period, too, the famous "English Hippocrates," Thomas Sydenham, associated his own work with Boyle's. But given Hunter's care to stick to the stated evidence of the Boyle papers rather than to pursue other hints, speculation about these and other associations which might illuminate some parts of Boyle's political and intellectual agenda is declined.

Hunter's Boyle therefore remains a rather aloof, exacting and industrious corpuscularian, the investigator of nature for its own sake, or rather for how it might support belief in the true God in the face of both doubt and sectarianism. One will find no Boyleian hidden agendas or conspiracy theories here, only an intelligent, earnest, open and non-doctrinaire member of the Anglo-Irish establishment. While it will not be the last interpretation of Boyle, then, Hunter's version of his Life and Works can be counted on for its full and scrupulous treatment of the evidence as we have it. Hunter's own integrity and discretion gives the work an enduring strength. Boyle himself would surely have been pleased with it.

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Atoms in Chemistry: From Dalton's Predecessors to Complex Atoms and Beyond. Carmen J. Giunta, Ed., ACS Symposium Series 1044, American Chemical Society, Washington, DC, 2010, vii + 116 pp, ISBN 978-0-8412-2557-2, \$150.

Anniversaries are difficult to pinpoint, since discovery and publication may be separated by several years, and the history of chemistry is rich in multiple discoveries. Who discovered the composition of water? Who discovered oxygen? These discoveries were contested originally, then by generations of chemists and historians. But there is no question that the chemical atomic theory, according to which each element was indecomposable, and characterized by atomic weight, was the invention or discovery of John Dalton, and made sense of his laws of multiple and definite proportions. There is more than one possible answer to the question of when he invented his atomic theory, but he first published his own detailed

statement of the atomic theory in 1808, and 2008 saw the ACS Symposium celebrating the bicentenary of that publication, followed in 2010 by the publication of this slender volume.

Carmen Giunta's introduction notes that unlike Dalton's atoms, today's chemical atoms are divisible; that atoms of the same element may exist as isotopes having different weights; that some elements are far from permanent, thanks to radioactive decay; but Dalton, were he alive today, could still take comfort from the fact that our atoms, like his, are discrete.

Scanning probe microscopy and manipulation enable us to "see" and to place individual atoms. This volume doesn't extend to nanotechnology, but it still covers a huge range. As William B. Jensen points out, atomism was seeping into chemical thought for almost two centuries before Dalton. One could argue for a longer pedigree, looking at medieval notions of least particles.

Gravimetric atomism was Dalton's invention, but there were problems in its application, resolved following Cannizzaro's solution at the Karlsruhe conference. Jensen notes that atoms prior to the 20th century were assumed to be spherical, whereas once electrical atomism was developed, the shape of atoms and electron orbitals became important for an understanding of bonding.

Leopold May looks at atomism before Dalton, in India, ancient Greece, Arabic alchemy, and medieval Europe. He gallops through the period from the 16th to the 18th century, which is fair enough since Jensen covers that period. There is, however, much recent literature that May doesn't mention, including studies of medieval atomism (William Newman's contributions here are notable), and more generally of atomism within alchemy and early chemistry or chymistry.

David Lewis looks at a century and a half of organic structures. Kekulé, Couper and Butlerov all distinguished the physical structure of molecules, which they regarded as unknowable, from chemical structure deduced from bonding affinities. There are some nice insights in this chapter, including the observation that Couper broke ranks from the theory of types, and that his excellence as an experimental chemist gave ammunition to critics who for years, ignoring his detailed instructions, failed to reproduce some of his key results. Chemical structure became physical structure only after 1874 and the work of Le Bel and van't Hoff.

William Brock's chapter, revisiting the atomic debates, is also a delight. He notes that chemists may have been agnostic about physical atoms, but that conventional atomism characterized and was essential to their chemistry. He notes, with Alan Rocke, that physical and chemical atomism increasingly provided mutual support by consilience. Benjamin Brodie's calculus of chemical operations was, as Kekulé pointed out, based upon initial assumptions, which, if altered, produced different results. It was therefore arbitrary in a way that

chemical atomism was not. Kekulé also objected to Brodie's approach because it led to unnecessarily complex consequences. Chemists learned their chemistry using the atomic theory, and they tended to ignore those who, like Ostwald, opposed that theory. Even Ostwald, with his dynamic theory, made use of chemical atoms; and after the discovery of Brownian motion, he confessed the error of his ways, and adopted chemical atomism.

Carmen Giunta looks at the period from the 1890s to the early 1930s, in which the compound nature of atoms was elaborated and the electron, proton, and neutron were identified experimentally, and radioactivity and isotopes were first understood. Here were answers to problems that had bothered chemists for many decades. Atomic weights came close to integral values in a pattern that demanded explanation; but some atoms, like chlorine, had far from integral weights. J.J. Thomson worked with cathode rays, and determined the charge and the mass of the electron. Rutherford and Geiger explored the scattering of α particles, and in 1911 Rutherford published an account of the scattering of α and β particles, along with an account of the structure of atoms. Protons, as Giunta notes, were observed long before they were named, whereas the opposite was true of the neutron.

Gary Patterson looks at the physical evidence for atoms, from the kinetic theory of gases and van der Waals forces; from spectroscopy (Faraday to Geissler), leading to an understanding of internal vibrations; cathode rays (Crookes); scattering (Becquerel, Rutherford, and Perrin); X-ray diffraction; atomic spectroscopy; radioactive decay; and mass spectrometry. Each experimental technique not only reinforced a sense of the reality of atoms, but also deepened understanding of the nature of atoms. We have come a long way from Dalton.

The final chapter offers "A Selection of Photos from Sites Important to the History of Atoms." Jim and Jenny Marshall offer an engaging travelogue.

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Reaction! Chemistry in the Movies, Mark Griep & Marjorie Mikasen, Oxford University Press, New York, 2009, x + 340 pp, ISBN 978-0-19-532692-5, \$49.95

Mark Griep is a chemistry professor at the University of Nebraska-Lincoln, where he has received a College Distinguished Teaching Award. Marjorie Mikasen, his wife, is an artist, with works in private and public collections. They have also “enjoyed watching movies for as long as we remember.” With the aid of an Alfred P. Sloan Foundation Public Understanding of Science and Technology grant they have produced an informative, entertaining, and potentially useful book on the portrayal of chemistry and chemists in motion pictures.

The organization of the book is topical and historical. There are five chapters on what they call the “dark side” of chemistry. The themes explored are Jekyll and Hyde, invisibility, chemical weapons, bad companies, and addictions. These are followed by five chapters on the “bright side,” whose themes are inventors, forensics, chemistry classrooms, good researchers, and drug discovery. Each chapter begins with an analysis of the chemistry in the theme and then summarizes a number of movies dealing with the theme, including one that is chosen as the “archetype.” The movies (over 100 with plot descriptions, another 50 or so dealt with more briefly) range in age from *The Invisible Thief* of 1909 to *United 93*, a 2006 reconstruction of the story of the hijacked plane brought down by its passengers on September 11, 2001.

The book is a fine historical survey of the movie industry’s use of chemistry and chemists; there are indices not only of movie titles, but also of actors, directors, screenwriters, and “special chemical effects.” But the authors have aimed at much more than this. There is real chemistry in every chapter, in some cases quite a lot. The

discussion of invisibility-producing concoctions starts with the imaginary ingredient “monocaine” from 1933’s *The Invisible Man*, moves to a discussion of –caine as a suffix signifying a local anesthetic, and from there to a detailed discussion of cocaine including its chemistry, its production, and its addictive properties. The “bad companies” chapter discusses hexavalent chromium (*Erin Brockovich*, 2000) and methyl isocyanate (*Bhopal Express*, 2001) as well as DDT and the book *Silent Spring*. In the “bright side” chapter on inventors there is material on polymerization reactions of various types (*The Man in the White Suit*, 1951; *The Absent-Minded Professor*, 1961, and its remake, *Flubber*, 1997). There is also mention of a very brief clip from 1897 showing *Mr. Edison at Work in his Chemical Laboratory*. The chapter on forensics discusses possible structures for the imaginary toxins and other reagents in its movies, but also includes factual material on the limits of detection of substances. Biographical films on real chemists including *Dr. Ehrlich’s Magic Bullet* (1940), *Madame Curie* (1943), and *Edison, the Man* (1940) are included in the “bright side” chapters.

The authors designed this book to be a resource for high school and college chemistry teachers. There is an appendix, “How to Use This Material in the Classroom;” the “References” section includes hundreds of books and research papers. But the book is also just plain fun, and a worthwhile read for anyone interested in movies, how chemists are perceived by the general public, or the broader area of science and society.

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Periodic Tales: A Cultural History of the Elements from Arsenic to Zinc, Hugh Aldersey-Williams, Ecco (HarperCollins), New York, 2011, xvii + 428 pp, ISBN 978-0-06-182472-2, \$29.99.

Periodic Tales is an engaging book about the uses and images of many chemical elements in the wider world outside the chemical laboratory. As such, it is relevant to chemists of all specialties interested in how chemistry and its building blocks construct the materials of art, craft, and power, and how the uses of these materials are associated with various elements.

It is *not*, however, a cultural history of the elements—despite its subtitle. It is neither so scholarly nor so systematic as the subtitle suggests. Surely, the *Bulletin* ought to review a cultural history of the elements; however, it might well choose not to review a book entitled *Periodic Tales: The Curious Lives of the Elements*, which is the name of the same work published in the United Kingdom by Penguin's Viking imprint. The author's prologue gives a more accurate picture of the book's content than does its subtitle, namely an impressionistic and idiosyncratic collection of stories about how selected inhabitants of the periodic table are seen outside the world of chemistry.

The book is aimed at an audience of readers who have some interest in chemistry but who need not have technical expertise in it. Most of Aldersey-Williams's tales describe properties of the featured element to give a jumping-off point for musings on the element's reputation or cultural meaning. For example, gold's color, luster, density, softness, and relative inertness are related to its uses as ornament and money (but not tools or weapons) and its associations with royalty and the sun. Arsenic's reputation as a poison, intentional ("inheritance powder") or otherwise, of course, is due to its toxicity. Cadmium, mercury, and lead are also highly toxic, but the author focused on other properties and other resonances for these elements. Cadmium is discussed as a base of pigments widely used by painters; mercury as a liquid mirror that mysteriously combines fluidity with the properties of metals; and lead as heavy, dull, grey, and plodding.

The author begins his prologue by describing a "periodic table" whose boxes contain artists, film stars, and astronomical objects. He notes that the periodic table has become a cultural icon that transcends chemistry. Toward the end of the prologue, he exchanges the chemists' organizational scheme for one he describes as anthropological: he treats individual elements under the five general headings of power, fire, craft, beauty, and

earth. Each category, fairly broadly construed, includes some unsurprising entries and some seemingly arbitrary ones. For example, gold, iron, carbon, and plutonium are categorized under power, and fittingly enough, as they illustrate economic, mechanical, thermodynamic, and military power. Examples of elements that could be included under more than one of these categories include silver (treated under craft, not power (economic) or beauty) and carbon (which could just as easily have been treated under fire, particularly considering that the carbon essay devotes considerable space to charcoal). Hydrogen would also be easy to include under power or fire, but it is not treated at all. Thallium would be difficult to fit under any of the categories, so fire seems no more arbitrary than any other.

The chapter entitled "Nightglow of Dystopia" is an excellent example of how engaging, wide-ranging, and idiosyncratic are the tales collected in this book. The chapter begins with sodium's yellow light, and how several authors describe sodium streetlamps as a detail in urban dystopian settings. It moves on to how sodium was discovered, not because of its characteristic glow but as a result of Humphry Davy's electrochemical experiments. After a few paragraphs on Davy, including a mention of his talent as a poet, Aldersey-Williams moves to some alkali metals that *were* discovered because of their characteristic light emission, namely cesium and rubidium. Many of the elements mentioned so far in the chapter are used in fireworks. After mentioning the elaborate display in London for which Handel wrote music and depictions of fireworks in works of Dickens and Thackeray, the author describes his visit to Britain's last remaining manufacturer of display fireworks. Aldersey-Williams and the managing director lament that Britain's biggest day for fireworks falls in dank November (Guy Fawkes night) and that "control of big displays" as passed to "people who hate fireworks." Still, to the spectator in the proper mood, the sights, sounds, and even smells of fireworks can be magical.

The author writes quite engagingly when describing properties, uses, reputations, and resonances of elements or their compounds. But he is even more engaging when he steps into the story as a participant or an explicit observer. For example, he described his unsuccessful attempt to reproduce the isolation of phosphorus from urine and his successful isolation of iodine from seaweed. In these cases, or when he muses on his own encounters with the element in question, his essays verge on memoir.

Superficially, then, one might see a resemblance between *Periodic Tales* and Primo Levi's *The Periodic*

Table (Il Sistema Periodico), both of which invoke elements and the periodic table as a springboard for idiosyncratic story-telling. But the resemblance ends there: in this book, the aspect of memoir is like frosting, a treat where it appears, but not the main course; the elements come first. A more apt comparison is to Sam Kean's recent book, *The Disappearing Spoon and Other True Tales of Madness, Love, and the History of the World from the Periodic Table of the Elements*. Both are collections of vignettes about the elements intended for a

general readership who have some interest in chemistry. *Periodic Tales* is about images, uses, and reputations of elements—a broad topic, to be sure, but more focused than Kean's book. In addition, the glimpses Aldersey-Williams provides of himself differentiates his book from *The Disappearing Spoon*, which has no hint of memoir.

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