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August Kekulé and the Stereochemistry of Benzene

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The Cover...

... shows the famous caricature of August Kekulé done by the artist Carjat in 1860 during a visit to Heidelberg. It is reported that "Die Karikatur machte Kekulé viel Spass und er zeigte sie gelegentlich seinem Freunden." The work of Kekulé and his successors on the stereochemistry of aromatic compounds is the subject of the article in this issue by Leonello Paoloni.

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THE 1991 DEXTER ADDRESS

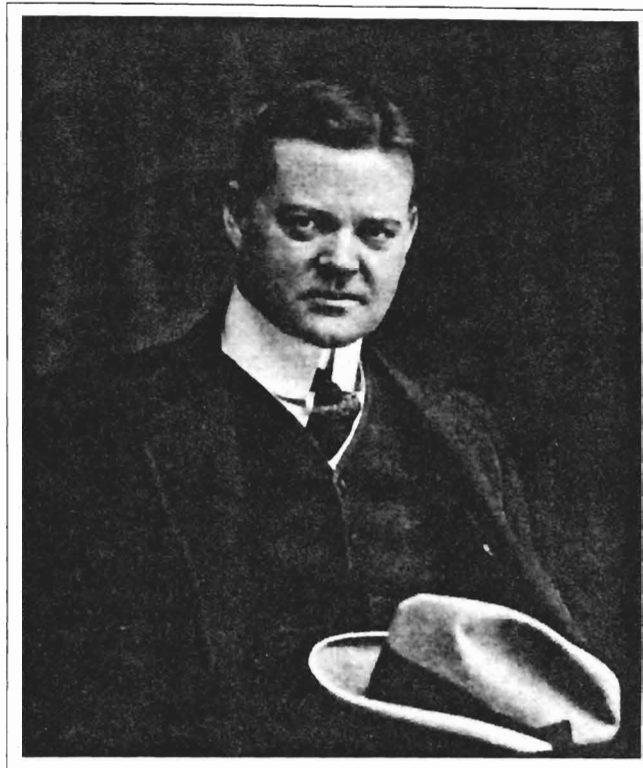
Herbert Hoover and Georgius Agricola: The Distorting Mirrors of History

Owen Hannaway, Johns Hopkins University

In my address today I wish to take a comparative look at the lives and careers of two men who lived more than 300 years apart, but whose names have become inextricably linked in the English-speaking world of the history of science and technology. They are Herbert Hoover, mining engineer, organizer of international relief efforts during World War I and President of the United States of America; and Georgius Agricola, 16th-century humanist, physician, and author of a number of works in Latin on the phenomena to be found underneath the earth. What linked these two men was a book - one of Agricola's books, namely, his *De re metallica* of 1556 that Herbert Hoover had translated into English and published in 1912 in a sumptuous edition, which in its typeface, quality of paper and vellum binding sought to come as close as possible to the magnificent standards of the original masterpiece of Renaissance printing that was published by Froben in Basel (1). Not the least of the glories of the original, and the translation, were the hundreds of woodcuts that illustrated the book. These depicted the miners and smelters, tools and machines, techniques and processes of the mines of Saxony and Bohemia in the mid-16th century where Agricola had lived, worked and made profitable investments. By 1912, the year the translation was published, Herbert Hoover had made successful investments, too, and was, in fact, close to the apex of his career as a mining engineer and consultant who, from his base in London, had interests that literally spanned the globe from Western Australia, New Zealand, Burma, China, and Russia to North America. He was a wealthy man and had succeeded with some years to spare in his self-professed ambition of making a fortune by the age of 40 (Hoover was 38 in 1912).

An interest in mining was obviously a mutual concern of Hoover and Agricola. But why should a highly successful mining engineer of the early 20th century be concerned with a book written in the middle of the 16th century? An attempt to answer that question requires us to look at Herbert Hoover's career and its trajectory. For this I am greatly indebted to the first volume of George H. Nash's *Life of Herbert Hoover*, although I shall depart somewhat from Nash's interpretation of the significance of the Agricola translation for Hoover (2).

Orphaned in childhood, Herbert Hoover acquired from the Quaker relatives who brought him up on the frontier of Iowa and Oregon, a strong sense of duty and responsibility, as well as initiative and a well developed work ethic. The place that first gave him opportunity to exploit these attributes in the larger world was Stanford University in whose very first class Hoover enrolled in 1891. There is evidence that already when



Herbert Hoover at age 40, two years after publishing his translation of Agricola's *De re metallica*

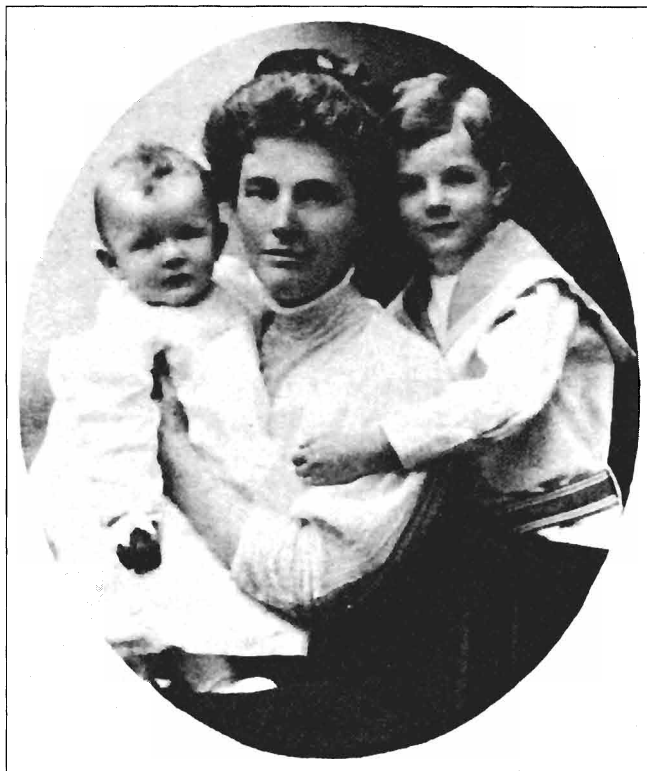
he went to Stanford he had the intention of becoming a mining engineer, but there being no curriculum in that subject, he followed the courses for mechanical engineering in his first year. In the second year, however, there appeared on campus Stanford's new professor of geology, John C. Branner. Hoover at once found his subject and his most influential mentor. Probably the most important educational experiences for Hoover's subsequent career were the summer jobs that Branner was able to arrange for him doing field work as an assistant for the United States Geological Survey. Hoover got both course credit and pay for these expeditions on which he developed his skills at making quick but accurate assessments in the field. During term time, in addition to his studies, Hoover began to refine those managerial and executive skills for which he later became famous as treasurer of various student societies (3, 4).

In May 1895, Herbert Hoover graduated from Stanford University with an A.B. in Geology. At several social events before graduation, he was accompanied by a young woman named Lou Henry, who just that year had come to Stanford to study geology with Professor Branner. Herbert Hoover later married her and she would play an important part in the translation of Agricola (5). But these steps lay in the future: Hoover had first to set the course of his career. The summer immediately following graduation found Hoover back in the

Sierras working for the U.S. Geological Survey under the direction of Waldeman Lindgren as he had done in summers before. He found himself torn between the attractions of academic geology on the one hand, and his old ambition to be a mining engineer on the other. At one point he contemplated doing post-graduate work at either Columbia or Johns Hopkins where he hoped to make a specialty of "mining geology" (6).

At any event, when the summer season of geologizing for the U.S. Survey ended in October, Hoover took himself off to the Nevada City gold mines where for two or three months he labored as a miner both at the face and in pushing hand carts filled with ore. This was Hoover's harsh introduction to practical mining; but it was scarcely mining engineering. Hoover's dilemma was that he knew what he wanted to be, but he did not know how to get there. In the end it was a suggestion by an experienced mining engineer named George Hoffmann that directed him to Louis Janin, who was a mining consultant operating out of San Francisco. Janin was the director of one of the many consulting firms which gave managerial, technical, and financial advice and assistance to mining interests. Hoover began here on the ground level as an office boy, but his expert knowledge of the geology of some California gold mines derived from his work with Lindgren enabled him to provide his boss with some convincing testimony in a law suit involving one of his clients. In short order Janin sent Hoover off as assistant manager of a mine in New Mexico which was quickly followed by spells in Colorado, Arizona, Nevada, and Wyoming. Hoover was quickly being initiated into the profession of a mining engineer by being one (7).

This business of mining consultancy and mining engineering was an international one, as events in Hoover's career soon showed. Late in 1896 Louis Janin was approached by a representative of the London-based firm of Bewick and Moreing to recommend a mining inspector who would look out for their current and prospective interests in the gold fields of Western Australia. Janin nominated Hoover for the job in spite of his youth (Hoover was only 22 at the time). In March 1897, he set out for Australia by way of London where he was interviewed by one of the senior partners of his new firm, C. A. Moreing. Passing muster, he continued on his journey to Western Australia where he arrived in May (8). The gold fields of Western Australia were at this time undergoing a transition. An initial phase of boom and speculation that was encouraged by the presence of alluvial gold on or near the surface, was giving way to mining at depths that required more geological knowledge, more technology for draining and crushing, and, hence, more rational use of labor and capital. In these conditions, Bewick and Moreing could not have chosen a better man than Hoover to evaluate their current and prospective mine holdings. For the first several months of his stay in Australia he traversed the rugged outback evaluating mines and making recommendations to his firm. Among the properties most strongly recommended by Hoover was the mine known as



Lou Henry Hoover with her sons, Allan (left) and Herbert Jr. (right), circa 1908

"Sons of Gwalia". After lengthy inspections, Hoover felt this mine would return well on investment over a period of time if modern equipment were employed to mine and mill the low grade ore, but he was insistent that the firm must have control of the design and purchase of the equipment. Bewick and Moreing paid close attention to all of Hoover's recommendations.

They secured an option to buy from the original owners, floated stock on the London market, and when the time came to begin operations, they made Herbert Hoover the superintendent of the mine. At age 23, Hoover had the chance to put the theoretical principles that lay behind his mine evaluations into practice. The chief of those principles was that the key to long-term profitability in any mining enterprise was to keep working costs low. This in turn meant the most efficient use of machinery and labor. Hoover was able to implement his principles so successfully at "Sons of Gwalia" that he not only won the confidence of his employers, but he also established his reputation internationally as a mining engineer (9).

In the fall of 1898, the senior partner of the firm, C. A. Moreing, invited Hoover to leave Western Australia for China. Moreing had ambitious schemes afoot there, including the building of an ice-free port near the vast Kaiping coal fields and assisting the Chinese to develop their gold and other mines in the province of Chihli. Hoover was tapped to be the company's

on-site representative in both schemes. Tired by the harsh conditions of the Australian outback, Hoover quickly accepted the offer. But he insisted that he be allowed to stop over in the United States before proceeding to China. The purpose of the stop was so that he could marry Lou Henry to whom he had proposed by cable from Australia (10).

This is not the time and place to discuss in detail the tumultuous two years the Hoovers spent in China. Suffice to say they were punctuated by the Boxer Rebellion and in the course of them, Herbert Hoover was introduced to a new world of international finance and political negotiation that went far beyond his experience in Australia. Although there were no successes like the "Sons of Gwalia" mine, Hoover had obviously moved to a different level of operation with Bewick and Moreing which merited him the reward of a senior partnership in the firm (11).

With the new status came a new location - the Hoovers moved to London where they established their home between 1901 and 1914. Not that traveling ceased, for Hoover still logged thousands of miles to far-flung parts of the globe to inspect mining properties for the firm. This continued even after he parted company with Bewick and Moreing and set up on his own as a mining consultant in 1908. The Hoovers remained in London during these years principally because London was the center of the world's mining finance, and it was increasingly to this side of the business that Herbert Hoover turned his attention. But London also provided amenities not found in the Australian outback or the coal fields of Northern China. There were theaters and concerts to attend, museums to visit, and books to buy and read (12). These were new kinds of interests for Herbert Hoover and he went about them with the same methodical efficiency that he brought to everything else he did. Around 1904 he set himself an ambitious program of reading in history, government, politics and economics, perhaps already anticipating the career in public life which he hoped to undertake once he had made his fortune by age 40. Books of this sort were something new for Hoover, whose education at Stanford had focused largely on scientific and technical subjects (13). Hoover's professional success had built upon that educational base, but it had been supplemented most significantly by practical experience.

This experience is clearly manifest in a book by Hoover that appeared in 1909 titled *Principles of Mining: Value, Organization and Administration: Copper, Gold, Lead, Silver, Tin and Zinc* (14). The book derived from a set of lectures that Hoover delivered, first at Stanford and then at the Columbia School of Mines, and its theme is firmly rooted in Hoover's own career. The work is essentially about the relationship between mining and finance and in the course of it, Hoover spells out the professional responsibilities of the mining engineer. He argues that the latter is distinctive since "the most dominant characteristic of the mining engineering profession is the vast preponderance of the commercial over the technical

in the daily work of the engineer" (15). It is in the domain of the commercial that the profession of mining engineering should exert its moral authority by giving informed accounts of the state and prospects of mines, thereby curbing speculation in mining shares and encouraging informed, long-term investment. Only in this way could the reputation of the industry be kept high enough to attract the capital it needed (16). To this end, a very large part of the *Principles of Mining* is devoted to the valuation of mines (which had also been a large part of Hoover's professional career). This consultative role for the mining engineer reflected, as Hoover made explicit, the new scale of capitalization in mines that saw the formation of companies floating shares on the public exchanges. Thus, the mining engineer became in Hoover's eyes the moral gatekeeper of mining shares on the stock market (17).

This is not to say he ignored the organization and administration of the mine in his book *Principles of Mining*, as both these factors were important in the valuation of mines. And here again he waxes eloquent on the moral duty of the mining engineer. The very essence of the profession is to direct men - mining engineers are officers in the great industrial army who usually serve far from civilization. In these circumstances, their duties extend beyond mine management; to them falls "the responsibility of example in fair dealing and good government in the community" (18).

What will strike the contemporary reader as odd about the contents of *Principles of Mining* is the relatively small part science and technology play in it. Technology, in fact, does not appear to be a word in Hoover's vocabulary; instead he speaks of science, and science applied to industry. "To the engineer falls the work of creating from the dry bones of scientific fact the living body of industry" (19). The principal science that the mining engineer applied to industry was geology (20).

To this period in Hoover's life belongs the translation of Agricola's *De re metallica*. Work on this translation began around 1906 and it went to press at the end of 1912; thus the *Principles of Mining* was published right in the midst of the work on Agricola. One might expect that there would be some reflection of one book in the other. George Nash, Hoover's biographer, has linked the two together by suggesting that both represent efforts to raise the professional image and status of the mining engineer (21). Clearly the Hoovers' translation of *De re metallica*, produced as it was in a handsome, vellum-bound folio, and handprinted on paper specially made in Scotland to resemble the original, was meant to impress. In fact, in every way the translation was made to resemble the masterpiece of the printer's art that Froben had produced in 1556 (22). The Hoovers' book as a physical object was a magnificent symbol of the dignity of mining. But if we penetrate to the contents of the translation itself and try to understand how these elevated the professional dignity of the mining engineer, we find some surprises.

Before discussing the text of the translation, it is important

to understand Herbert Hoover's role in the translation. The title page appears unambiguous: *Georgius Agricola De Re Metallica translated from the first Latin edition of 1556 ...* by Herbert Clark Hoover and Lou Henry Hoover ... But there is a problem with this; Herbert Hoover did not know the languages to carry out that translation. (Besides the original Latin version, *De re metallica* had been translated into German and Italian, neither of which Herbert Hoover knew) (23). In fact, the initiative for doing the translation in the first place seems to have come from Lou Henry Hoover, who wrote to J. C. Branner, the Hoovers' former geology professor at Stanford, that she could not find an English version of *De re metallica* in the British Museum so she thought she would undertake a translation herself (24). The bulk of the English text that appeared may well have been by Lou Henry Hoover, although we know that from 1908 on the Hoovers hired a number of people to do translation and bibliographic work for them. As the project moved on, Herbert Hoover seems to have defined a bigger role for himself in it, and that was to provide technical commentary by way of footnotes. This may have been behind the advertisement in *The Times of London* in July, 1911 for "a Lady Secretary who can translate Latin and German with facility and has done work at the British Museum" (25). If one looks at the footnotes to the translation of *De re metallica*, one can see the necessity for that facility in languages and familiarity with a great library, because frequently incorporated within them were lengthy passages of translation either from other works by Agricola or from treatises by classical authors. Those footnotes carry a weight of scholarly authority that their alleged author did not possess. On the other hand, let us appreciate the Hoover translation for what it was - a magnificent piece of industrial scholarship in which the master engineer, Herbert Hoover, mobilized and managed a team of translators and bibliographers to produce a work of enduring scholarship in the space of only six years.

This efficiency in the production of the translation contrasts with the protracted labor of 25 years that Georgius Agricola spent on writing the original. In fact, Agricola never saw his own book published as he died in 1555, one year before it appeared. It is scarcely imaginable that Agricola would have hired translators to speed up the production of his humanist Latin. To Agricola, the language of his book was everything; to Hoover, the language of *De re metallica* was a matrix from which the meaning had to be extracted. To understand this better, we need to look at aspects of Agricola's life and career.

Georg Bauer, who would later Latinize his name as Georgius Agricola in order to underline his identification with classical antiquity, was born in Glauchau in the Duchy of Saxony in 1494 (26). He was the son of a dyer and woolen draper, which were important trades in that cloth manufacturing town. His family obviously had ambitions for him beyond the family trade, for he entered the University of Leipzig in 1514. This was an important period in that university's



Georgius Agricola (1494-1555), born Georg Bauer

development which saw the introduction into the curriculum of the new humanism that we associate with the Renaissance, namely, the appreciation of the literature and culture of classical antiquity through a renewed study of the ancient languages of Latin and Greek. So infected was Georg Bauer by the new learning that after graduating he went to Zwickau where he introduced Latin and Greek into the municipal school. It was about this time that he adopted the name Georgius Agricola, and, indeed, he might have spent the remainder of his days in Zwickau happily teaching Latin and Greek, had not disruption caused by the preaching of some radical religious reformers undermined the peaceable conduct of the municipal school (27). Agricola decided to return to the University of Leipzig, this time to study in the higher faculty of medicine. Even here his humanist predilections showed, as he shortly set off for Italy - the home of Renaissance humanism - where, among other things, he worked in the Aldine Press as an editor of some of the Greek texts of the ancient physicians Galen and Hippocrates. Thus in his study of medicine, Agricola was also able to keep up his study of ancient languages as well.

This required much more ingenuity after Agricola returned to Germany around 1526. His first position was as town physician and apothecary to Joachimsthal, which at that time was a boom mining town on the Bohemian side of the mountain range known as the Erzgebirge (i.e., in modern Czechoslovakia). Here Agricola came into daily contact with mines and mining officials; but in such an environment, how was he to keep alive his humanist interest in ancient languages? His solution to this was a project to identify those mineral species that the ancients employed in the preparation of their medications. This involved the identification of the words used by the ancient authors in their texts with the substances that could be inspected first hand in the mines and workings of Joachimsthal.

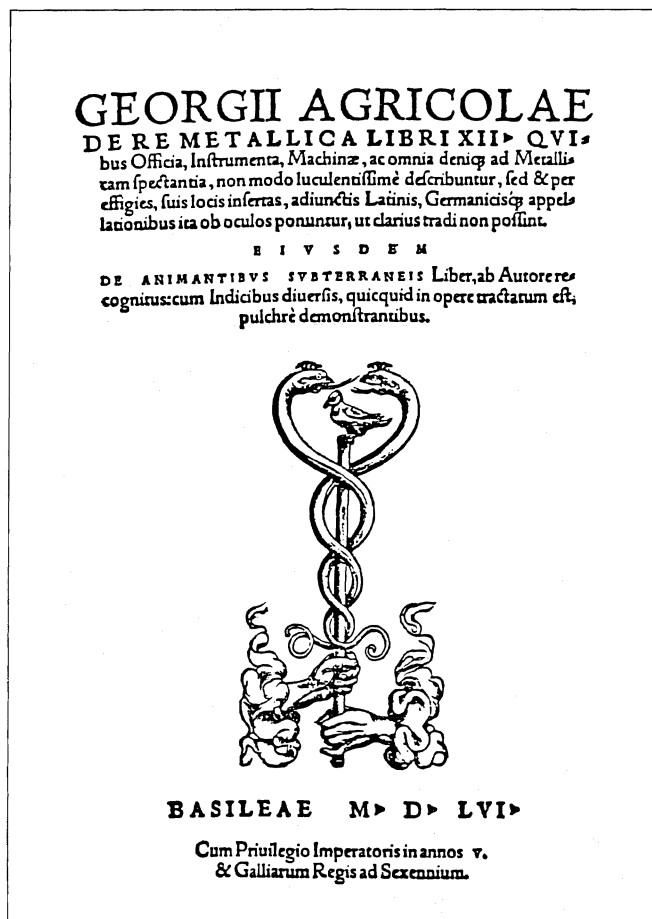
thal. It was a good example of that classic humanist enterprise of linking words to things and vice-versa. Quickly, however, this project took on broader dimensions as Agricola's interest moved beyond the names and identities of therapeutic species to embrace the whole phenomenon of mining, its products and its operations. This change in the project is clearly seen in Agricola's first publication on mining, the *Bermannus* of 1530 (28). This short work takes the form of a dialogue conducted in Latin between a mining expert and two physicians. The trio make a tour of the Joachimsthal valley in the course of which they debate not only the names and identities of minerals, but also discuss the machines and technology of mining.

The *Bermannus* is in a sense a prologue to *De re metallica* which was, in fact, the subtitle given to the dialogue. But the great work itself would be 25 more years in the making and much would happen to Agricola in that time. In the first place, he moved back to his native Saxony and the town of Chemnitz, which was still within the same silver mining region of the Erzgebirge where Joachimsthal was located. Agricola made very successful investments in the mines and with this wealth came increased responsibility in the community. He became

a town councilor and was appointed Burgomeister on several occasions. He also served on legations of Duke Maurice and Albertine Duke of Saxony (29). Throughout this crowded private and public life, he continued to work on the book that would become *De re metallica*, and most astonishingly of all, he found time in between to compose five other works which dealt comprehensively with all natural phenomena occurring beneath the earth. Together they comprise a kind of natural philosophy of the underworld on the model of Aristotle: *On the Origin and Causes of Subterranean Phenomena; On the Nature of Those Things that Flow from beneath the Earth; On the Nature of Stones; On Ancient and Modern Metals* (30). In these works can be seen the full range of Agricola's humanism - it consisted of attempting to describe and understand events and phenomena with the same sensibility as that of the ancients, and that meant expressing those experiences and thoughts in the language of the ancients.

Agricola's reaching back to the world of antiquity contrasts sharply with Herbert Hoover's projection of Agricola's writings forward into the progressive movement of science. This is most clearly seen in that section of the introduction to the Hoovers' translation entitled, "Agricola's Intellectual Attainments and Position in Science" (31). The question Herbert Hoover asks in this section is what advances did Agricola make in the sciences of geology, mineralogy and mining engineering. Hoover is not unsophisticated in the answers he gives. He is aware of the danger of reading modern concepts into Agricola's terminology; he understood that Agricola's ideas were deeply indebted to the ancients, especially the Aristotelians; and he recognized that as regards geology and mineralogy, the important texts were *On the Origins and Causes of Subterranean Things* and *On the Nature of Stones* (long translated passages of both appear in Hoover's footnotes to the translation of *De re metallica*). Hoover depicts Agricola trying to break away (not always successfully) from the confines of ancient thought whereas Agricola in fact was attempting to reestablish dialogue with the ancients. In accounting for the limited advances he made in the sciences of geology and mineralogy, Hoover says that Agricola can scarcely be blamed for not seeing forward "to the atomic theory and our vast fund of chemical knowledge" that was now available (32). In the end, Hoover makes his broadest claims for Agricola in the realm of scientific methodology and offers an interesting comparison between him and Paracelsus (33):

The wider interest of the members of the medical profession in the development of their science than that of geologists in theirs, has led to the aggrandizement of Paracelsus, a contemporary of Agricola, as the first in deductive science. Yet no comparative study of the unparalleled egotistical ravings of this half-genius, half-chemist, with the modest sober logic and real research and observation of Agricola, can leave a moment's doubt as to the incomparably greater position which should be attributed to the latter as the pioneer in



The title page of the first edition of *De re metallica*

building the foundation of science by deduction from observed phenomenon.

What is interesting and surprising to note here is that the highest pedestal on which Hoover places Agricola is science (not engineering); that the sciences to which he believed Agricola made his most important contributions were geology and mineralogy, not mining engineering; and when Hoover makes his comparison of the adulation accorded Paracelsus by the medical profession with the neglect shown - not by the profession of mining engineering - but by the profession of geology. If the Hoover translation was intended to elevate the status and dignity of the profession of mining engineering, it did not do so by celebrating Agricola as a model mining engineer; rather it claimed him to be one of the founders of modern scientific method.

What is surprising in this is that Agricola has, in fact, much to say about mining and its virtues in terms that should have appealed directly to Herbert Hoover. Agricola in the first book of *De re metallica* is at pains to argue that mining is a respectable and rewarding activity. Why was this necessary? Fundamentally, it stemmed from Agricola's posture as a humanist, seeking to understand and write about his own world with the sensibilities of the ancients. To accomplish this, he had to choose, as any humanist would have done, a classical model for his book. The model Agricola adopted was the treatise on agriculture by the 1st century A.D. Roman author, Moderatus Columella, entitled *De re rustica* (34). One can see how Agricola's title echoes that of its ancient precedent. But there was a problem with this choice in that Columella (like other ancient writers) celebrated agriculture as the only honorable and virtuous way to accumulate wealth. The first book of *De re metallica* is largely devoted to overcoming this deeply held conviction of the ancients that the economic well being and political virtue of the free-born citizen was rooted in the ownership and cultivation of land. Mining, on the other hand, was an activity carried out by slaves in the ancient world. Agricola mounts many arguments against this position, but one is of particular interest in relation to Herbert Hoover.

Agriculture was virtuous and natural to the ancients because it represented a renewable and (fairly) predictable method of acquiring wealth; the growth of crops and the raising of livestock were natural processes that followed the seasons of the year; the rhythms of agriculture were in harmony with those of the cosmos. On the other hand, other forms of economic activity, such as trade and mining, were unnatural and unpredictable - or as the ancient Romans would have put it, were subject to the goddess "Fortuna". "Fortuna" was luck or chance, and while every human life and endeavor was subject to it, it had to be combated by means of virtue. The charge that mining is too much subject to "fortuna" is the one that Agricola is at most pains to refute in his first book of *De re metallica* (35). Now recall what Herbert Hoover thought was the

primary role of the mining engineer: it was to solidify confidence in mining as representing a reliable return on investment. The technical expertise of the mining engineer was meant to end short-term speculation (i.e., fortuna) in mines and introduce to them long-term investment (i.e., virtue). Agricola, like Hoover, advocates technical knowledge acquired on-site as the best antidote to rash speculation in a mining enterprise; but he also recognized that investors can devise their own strategies to outwit "fortuna". We would call it diversification of one's portfolio.

Here is what Agricola has to say about it (36):

When a man owns mines but is ignorant of the art of mining, then it is advisable that he should share in common with others the expenses, not of one only, but of several mines. When one man alone meets the expense for a long time of a whole mine, if good fortune bestows on him a vein abundant in metals, or in other products, he becomes very wealthy; if, on the contrary, the mine is poor and barren, in time he will lose everything which he has expended on it. But the man who, in common with others, has laid out his money on several mines in a region renowned for its wealth of metals, rarely spends it in vain, for fortune usually responds to his hope in part.

Thus had Herbert Hoover made his fortune. What is surprising is that Hoover, the author of *Principles of Mining*, makes no comment whatever on Agricola's lengthy defense of the virtue of mining as an economic activity and on his suggestions as to how income from investment can be stabilized.

One last aspect of Agricola's *De re metallica* is worth comment on in relation to Herbert Hoover. As I have hinted at, but not yet made explicit, mining in Agricola's day was a business of share capital; that is, there already existed by the first half of the 16th century companies of shareholders who financed and derived income from mining operations. This was how Agricola, too, had made his substantial fortune. The introduction of share capital into the finance of the silver workings of Central Europe arose in part from technological innovation; namely, the introduction of the cupellation process, whereby the silver in argentiferous copper ores could be extracted by smelting with lead. This technique made profitable the mining of these copper ores at much greater depths than before; but this in turn meant much more capital to build the drainage and ventilation engines necessary for mining at such depths. It was this need for capital that gave rise to shareholding companies in the Central European silver mines in the 16th century (38). Not only was Agricola the beneficiary of investment in such companies, but the illustrations in *De re metallica* gave graphic testimony of the transformation in the scale and complexity of the workings that such mining brought to the regions he was familiar with. Once again this should recall Herbert Hoover in the gold fields of Western Australia, where he superintended a similar transformation in mining practice that saw the necessity of increased capitalization of

mining operations in order to make possible the profitable exploitation of low-grade ores at depth. But once again, Hoover is silent about Agricola's description and depiction of the new technology of drainage and ventilation that was at the service of a new kind of mining very close to that of Hoover's own experience (38).

Why was Hoover oblivious to those aspects of Agricola's work that most reflected Hoover's own experience and seemed to support his own version of the *Principles of Mining*? One obvious reason is that he did not know much about the conditions of mining in Agricola's time and therefore was not in a position to recognize the really novel elements and arguments in *De re metallica*. Hoover's historical footnotes to the translation are of a chronological kind, identifying who first knew of such and such a substance or process. They usually begin with a scriptural or classical source, liberally quoted, and go up to the 16th century and beyond. In fact, it is a form of history rather close to that which Agricola himself wrote in his *On Ancient and Modern Metals*, although I do not believe Hoover ever had that text translated.

And, finally, why in a work that was intended to elevate the standing of mining engineers, was Agricola not celebrated for enunciating some of the principles of mining as Hoover understood them? Why does Hoover praise Agricola primarily as a geologist and a scientist? I believe we may find the answer to that question if we see the Hoover translation less as a manifesto for engineering and more as a labor of love. The heart of this work lies at Stanford and not at varied mining fields around the globe. It was there that the young Herbert Hoover had majored in geology; it was in the halls of the geology department that he first met his wife and "fellow" translator, Lou Henry; and both were students of the professor of geology, John C. Branner. The Hoover translation of *De re metallica* is, in fact, dedicated to Branner, and in elevating Agricola's standing amongst geologists, I rather think its translators were paying a joint tribute to their old teacher. There was another sense in which the Hoover *De re metallica* led back to Stanford. The translation represented an envoi to mining engineering for Herbert Hoover. He had made his fortune and it was time in that well-planned life to move on to a career in public service. The first place he sought to exercise that role was on the Board of Trustees of Stanford University to which he was elected in the fall of 1912, just as the sheets of *De re metallica* were coming from the presses (39). There could scarcely be a more legitimating symbol for a trustee of a major university. Many could claim to be rich, but how many amongst them could also claim to have produced an enduring work of scholarship.

References and Notes

Acknowledgment: The research for this address was generously supported by the National Endowment for the Humanities, an independent federal agency.

1. The title page of the translation reads: *Georgius Agricola De Re Metallica translated from the first Latin edition of 1556 with biographical introduction, annotations and appendices upon the development of mining methods, metallurgical processes, geology, mineralogy and mining law from the earliest times to the 16th century* by Herbert Clark Hoover and Lou Henry Hoover, The Mining Magazine, London, 1912. The title page of the original reads: *Georgii Agricolae De Re Metallica Libri XII Quibus Officia, Instrumenta, Machinae, ac omnia denique ad Metallificam spectantia, non modo luculentissimè describuntur, sed & per effigies, suis locis insertas, adjunctis Latinis, Germanicisque appellationibus ita ob oculos ponuntur, ut clarius tradi non possint. Eiusdem De Animantibus subterraneis Liber, ab Authore recognitus; cum Indicibus diversis, quicquid in opere tractatum est, pulchre demonstrantibus*, Froben, Basel, 1556.

2. G. H. Nash, *The Life of Herbert Hoover; the Engineer 1874-1914*, Norton, New York, NY, 1983. This is the first volume of a projected three-volume life of which the first two have already appeared.

3. *Ibid.*, pp. 26-41.

4. Hoover's own recollections of his Stanford years are recorded in H. C. Hoover, *The Memoirs of Herbert Hoover: Years of Adventure*, Vol. 1, Macmillan, New York, NY, 1951, pp. 16-24.

5. Reference 2, pp. 37-39; reference 4, Vol. 1, p. 23. Lou Henry, an athletic and attractive young woman, was the daughter of a Monterey banker and had trained as an elementary school teacher. She was captivated by geology after hearing an extension lecture given by Professor J. C. Branner and came to study the science with him at Stanford. There seems little doubt that her first encounters with Herbert Hoover were within the confines of the geology department at Stanford; in Professor Branner's office, in the geology laboratory and at the geology club. Geology, Professor Branner and Stanford University were amongst the oldest and strongest bonds of their union.

6. Reference 2, p. 44. This indicates a residual pull towards academic geology, perhaps born out of emulation of his teachers; but this was in conflict with his ambition to make a fortune at an early age.

7. *Ibid.*, pp. 42-50.

8. *Ibid.*, pp. 50-52. The London interview with C. A. Moreing was not without its tension. Bewick and Moreing had stipulated to Janin that they wanted someone over 35. Hoover was only 22. To disguise the discrepancy, Hoover grew a moustache and beard. There is a splendid photograph of him in London on his way to Australia, newly hirsute and sporting a top hat and morning coat that quite transformed the chubby, boyish looks of the Stanford graduate. (See *ibid.*, p. 93.)

9. *Ibid.*, pp. 52-87. For a discussion of mining in Western Australia around Hoover's time there see Geoffrey Blainey, *The Rush that Never Ended: A History of Australian Mining*, 3rd ed., University of Melbourne Press, Melbourne, 1978, pp. 161-207. Hoover is specifically mentioned on p. 202 and the illustrations between pp. 206 and 207 include a photograph of the Sons of Gwalia mine with a portrait of Hoover - still sporting a moustache - as an inset.

10. Reference 2, p. 86.
11. Nash discusses the Hoovers' activities in China and their legal fallout in great detail in *ibid.*, pp. 167-222.
12. *Ibid.*, pp. 496-502.
13. *Ibid.*, p. 292.
14. H. C. Hoover, *Principles of Mining: Value, Organization and Administration:: Copper, Gold, Lead, Silver, Tin and Zinc*, McGraw-Hill, New York, NY, 1909.
15. *Ibid.*, p. 185.
16. This is a recurrent theme in *Principles of Mining*; see, for instance, *Ibid.*, pp. 2-3, p. 52; p. 184 and p. 192.
17. *Ibid.*, p. 52.
18. *Ibid.*, pp. 192-3. The last chapter entitled "The Character, Training, and Obligation of the Mining Engineering Profession", (pp. 185-193) elaborates on this and related themes.
19. *Ibid.*, p. 193.
20. *Ibid.*, pp. 21-22 and p. 19.
21. Reference 2, pp. 475-495.
22. Reference 4, Vol. 1, pp. 118-119.
23. Hoover had no working knowledge of any language but English.
24. Reference 2, pp. 490-491.
25. Nash indicates that at times three or more hired translators may have been at work on *De re metallica* and its footnotes. *Ibid.*, pp. 491-492. Hoover in his *Memoirs* does not acknowledge the part played by these translators.
26. The standard life of Agricola is H. M. Wilsdorf, *Georg Agricola und seine Zeit*, VEB Deutscher Verlag der Wissenschaften, Berlin, 1956. See the same author's article on Agricola in C. C. Gillispie, ed., *Dictionary of Scientific Biography*, Vol. 1, Scribner's, New York, NY, pp. 77-79. In what follows, I have made extensive use of H. Prescher, *Georgius Agricola, Persönlichkeit und Wirken für den Bergbau und das Hüttenwesen des 16. Jahrhunderts* [Kommentarband zum Faksimiliedruck 'Vom Bergwerck XII B ücher Basel 1557], Acta humaniora der VCH Verlagsgesellschaft mbH, Weinheim, 1985.
27. For a good general account of the Reformation in Zwickau, see C. Karant-Nunn, *Zwickau in Transition, 1550-1574: The Reformation as an Agent of Change*, Ohio State, Columbus, OH, 1987.
28. G. Agricola, *Bermannus sive de re metallica*, Froben, Basel, 1530.
29. See Prescher, reference 26, pp. 60-76.
30. These works were published together with a second edition of the *Bermannus* as G. Agricola, *De ortu et causis subterraneorum Lib. V; De natura eorum quae effluent ex terra Lib.iii; De natura fossilium Lib.X; De veteribus & novis metallis Lib.II; Bermannus, sive De re metallica Dialogus, Interpretatio Germanica vocum vel metallicae addito Indice foecundissimo*, Froben, Basel, 1546.
31. Reference 1, pp. xiii-xv.
32. *Ibid.*, p. xiv.
33. *Ibid.*, p. xv.
34. L. J. M. Columella, *On Agriculture*, (with a revision of the text and English translation), 3 Vols., Harvard University Press, Cam-

bridge, MA, 1941-1955.

35. Reference 1, pp. 5-8; 12-13; 20-22; 23-24.
36. *Ibid.*, p. 27.
37. J. Nef, *The Conquest of the Material World*, World Publishing, Cleveland, OH, 1967, pp. 42-52.
38. Agricola's description of drainage and ventilation machinery is contained in Book VI of *De re metallica* to which there are very few footnotes in the Hoover translation.
39. Reference 2, p. 527.

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STEREOCHEMICAL MODELS OF BENZENE, 1869-1875

The Conflicting Views of Kekulé, Koerner, Le Bel and van't Hoff

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The stereochemical models of benzene proposed by Joseph Achille Le Bel and Jacobus Henricus van't Hoff in 1874-1875 were related to the well-known controversies on its constitution which began soon after the publication of August Kekulé's papers of 1865 and 1866. The models examined by van't Hoff in his booklet *La chimie dans l'espace* (1875) were based on a tetrahedral model for the carbon atom which Kekulé himself had described in 1867. However, the first to use Kekulé's model for a stereochemical treatment of the six-carbon benzene nucleus was Wilhelm Koerner in a paper published in 1869. The connections between these events have not received the attention they deserve.

The development of stereochemistry during its first decade was outlined by van't Hoff when he published the second edition of his booklet in 1887 (1). His historical introduction, however, was limited in scope: it reprinted both his and LeBel's 1874 papers, Johannes Wislicenus' preface to the 1877 German translation of *La chimie dans l'espace*, and the violent critique of Hermann Kolbe. After citing some more favorable notices by chemical authorities, van't Hoff concluded by quoting textbooks which presented his theory. At the end of Part IV of the work he devoted only one page of comment to aromatic compounds, opening with the statement that his theory (1):

... has nothing new to offer but a preference for Kekulé's hexagon as compared to the conceptions which assume three-dimensional arrangement of the six carbon atoms, as for example, the equilateral triangular prism.

The ring hypothesis for benzene and its derivatives was mentioned again at the very end of the book, referring to the strain theory that Adolf von Baeyer had recently proposed.

The role played by Kekulé's 1867 proposal of a tetrahedral carbon model was fully acknowledged in the first truly historical account of the development of stereochemistry which opened Carl Adam Bischoff's and Paul Walden's 1894 volume, *Handbuch der Stereochemie* (2). They discussed the stereochemical formulas for benzene suggested by Baeyer in 1888 and by Joseph Loschmidt in 1890, described structural models proposed from 1865 to 1892. However, they ignored the ideas of van't Hoff and Le Bel and failed to mention Koerner's work.

A survey of modern historiography regarding stereochemical models of benzene began with a paper by van Klooster who, on the occasion of the centenary of van't Hoff's birth, recalled (3):

... the fact that Kekulé, shortly after his appointment at Ghent in 1858, had already represented the carbon atom at the center of a tetrahedron and that Kekulé's assistant Koerner in 1869 had constructed a space model of benzene with these tetrahedra.

More recently, Koepfel discussed stereochemical benzene models but mentioned neither Koerner's model nor the interpretation of the benzene structure offered by Le Bel and van't Hoff (4). A discussion by Snelders compared the ideas of Le Bel and van't Hoff, but made no reference to their differences with regard to benzene structure (5). The distinction between the stereochemical approaches of Le Bel and van't Hoff has also been considered by Weyer, who carefully examined the earliest stereochemical literature, but without reference to benzene (6).

Ramsay has also studied these topics. Referring to the tetrahedral models for the carbon atom, he wrote (7):

There is no evidence that van't Hoff or Le Bel were aware of these models. Since it can be shown that a number of chemists were using tetrahedral carbon models before 1874, it is perhaps surprising that van't Hoff's proposal of the tetrahedral carbon atom was considered such a revolutionary idea by chemists.

Ramsay then quoted Koerner's model in these terms (7):

To explain the ortho/para substituent effect, he proposed in 1869 a "space-filling" structure of benzene [see figure 3].

We will discuss this model below, and therefore postpone our comments on its "space-filling" character. For the moment

we simply remark that the main purpose of Koerner's paper was to prove that the hydrogens of benzene were chemically equivalent. After explicitly rejecting Kekulé's representation, he proposed an alternative structure for the six-carbon benzene nucleus because it satisfied that condition.

Grossman has provided the most accurate distinction between the theoretical approaches chosen by van't Hoff and Le Bel in their 1874 papers, though without extending his analysis to the benzene case (8). He convincingly argued that they put forward "two very different theories", and suggested that van't Hoff extended his theory after reading the more general stereochemical formulation given by Le Bel. We shall show that this seems indeed to be the case.

The purpose of this article is, first, to outline the sequence of events mentioned above with respect to the early development of stereochemical models for benzene, and their different evaluation by Kekulé and Koerner. In his 1874 dissertation, van't Hoff developed his theory of molecular structure by using elements of Kekulé's tetrahedral carbon atom model of 1867. We shall then show that in writing the chapter on the constitution of benzene in his 1875 booklet, he recognized and eventually used the more general geometric approach taken by Le Bel in 1874. Finally, we shall argue that Le Bel's 1874 discussion of benzene reflects his inadequate understanding and incorrect formulation of the assumptions made by Kekulé in putting forward the theory of the constitution of aromatic compounds.

I. The criteria adopted by Kekulé for representing the chemical properties of a substance through its molecular constitution are described in a note at the end of a paper where he proposed a constitutional formula for mesitylene which was intended to explain its synthesis from acetone (9). Kekulé first explained the advantages of representing multivalent (*mehrwertig*) atoms as aggregates of univalent (*einwertig*) atoms. These are the "sausage" symbols that he used from 1859 to 1866 in his textbook (figure 1). Ironically, Kekulé was offering this explanation at the very moment he chose to abandon his "sausages" and to follow different criteria, starting with a representation of mesitylene (figure 2C) which he also adopted for the benzene ring in the second volume of his textbook (10). Let us examine his line of reasoning through his own words (11):

Crum Brown, Frankland, Hofmann and others have used in drawings and models a representation of a different kind. Atoms are represented as circles or spheres, and affinities [valence bonds] as lines or bars coming out of them. It can easily be shown that this procedure, even if only as a drawing, is not as complete as the one I have used. Several ratios of combination cannot be represented unless the lines corresponding to the affinity units are arbitrarily stretched or bent as necessary. When applied to a model, the procedure has yet another disadvantage: it moves only apparently into space, while as a matter of fact all goes on in a plane. The model therefore does not provide

anything more than a drawing does. These faults can in some measure be avoided, both in the drawing and in the models ... by choosing the length of the lines expressing the affinities so as to make their endpoints equidistant, as shown in Fig. 2A, so to have $ab = cd = ef$. It is then possible to bond the atoms to each other either by one or by two affinity units [figure 2B]. This mode of representation, sufficient in the most frequently encountered cases, does not however allow bonding with three affinities of another carbon or nitrogen atom. But even this fault can be avoided, at least in the model, when the four affinities of carbon, instead of lying in a plane, are arranged so as to come out of the atomic sphere in the direction of tetrahedral planes. In this way the length of the metal wires corresponding to the affinity units are chosen so that their endpoints are always equidistant ... Such a model allows us to represent bonds with 1, 2, or 3 affinity units, and I believe it does all that a model should do.

It appears from these comments that Kekulé worked out his theory by thinking of atoms and molecules as three-dimensional objects. However, he refrained from using such models explicitly in his published papers and books for more than eight years. One explanation of this attitude could be that Kekulé wanted to protect himself against criticisms regarding the unnecessary assumption of atoms in space, a damning charge that would have hindered widespread acceptance and diffusion of his new ideas on the constitutions of organic compounds. However, the concreteness of atoms and molecules in Kekulé's mind (12) is clearly evident from the prescriptions for the models he gives in the sentences quoted above.

Kekulé's model is essentially the same as van't Hoff adopted in his stereochemical theory of molecular structure. The first application to benzene, however, was made by Koerner in 1869, and then by Kekulé himself in 1872. Let us review these circumstances, starting with Koerner's model.

This model is described in a paper Koerner wrote at the insistence of Stanislao Cannizzaro to report on the work he was then doing in Palermo to demonstrate the equivalence of "the six hydrogen positions" and to work out a procedure for establishing the relative position of the substituents in the

Acetamid.



Figure 1. Symbols introduced by Kekulé in the first volume of his *Lehrbuch* (reference 10, Vol. I, pp. 160-165) to represent molecular constitutions. They describe polyvalent atoms as representing "die Anzahl der chemischen Einheiten", i.e., the number of valences, through the union of univalent atoms. These choices are briefly explained in the footnote, p. 159-160.

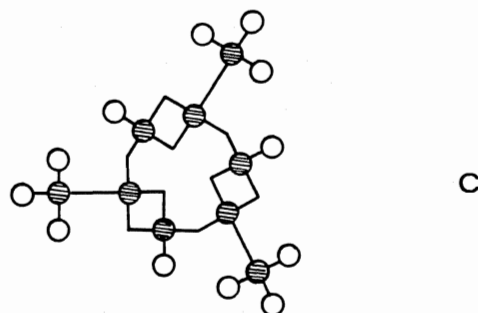
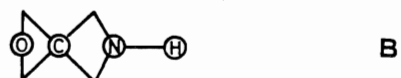
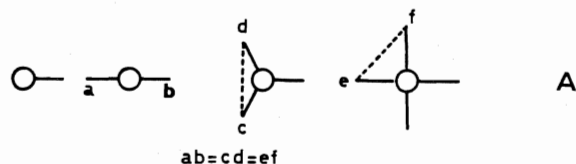


Figure 2. (A) Atomic models where "the length of the lines expressing the affinities is chosen so as to make their endpoints equidistant." (B) Their application to the case HNC(O)H taken as a model for bonds established between atoms with 1, 3, 4, and 2 "affinity units". (C) The constitutional formula for mesitylene built according to the same prescription. (From reference 9)

benzene ring. The model, shown in figure 3, is described as follows (13):

This arrangement, as can be seen in the two images, assumes the twelve atoms to be in four parallel planes; the hydrogen atoms 1,3,5 and 2,4,6 respectively are situated in the two extreme planes; the carbon atoms a,c,e and b,d,f occupy the two intermediate planes. Such an arrangement has the greatest possible symmetry; it leads to six absolutely equal hydrogen positions and to three disubstituted isomers, but requires an additional assumption to explain the formation of addition products ...

This model, equivalent geometrically to a flattened octahedron with hydrogen atoms attached to each of the six vertices, was introduced by Koerner with an explicit rejection of Kekulé's assumption of alternating double bonds. After reviewing the literature for the previous four years, he concluded that this assumption (14):

... seems to me no longer justified ... and I prefer another which allows a simpler explanation of the facts so far known. This idea, which I conceived long ago, is shown in the figure, and is very close to that discussed by Claus and more recently by Ladenburg because each carbon atom is regarded as *directly connected with three other carbon atoms*, thus explaining the great stability of benzene and its derivatives [italics in the original].

The statement by Koerner that he had conceived such a model "long ago" most likely refers to the time he spent in Ghent as an assistant to Kekulé (1866-67), and it is the first evidence of disagreement with his former mentor. Koerner surely knew of Kekulé's 1867 paper discussed above, for it was published while he was in Ghent, but he does not quote it. However, Koerner's superior at Palermo, Cannizzaro, cited Kekulé's paper in his report (15) read on 13 June 1869 to the Council of the Istituto Tecnico (the governing board of the *Giornale*), where he explained why he had asked Koerner to write the paper and recommended its publication (16).

In 1869 Kekulé published a review paper on benzene in which he examined the various structural theories that had been proposed. Intending to answer the criticisms aimed at his original proposal, he suggested guidelines for assessing the different models (figure 4), including his own, in general terms and on equal grounds (17):

Assumption [i.e. structure] No. 1 is what I have preferred; Claus has discussed assumptions No. 3 and 5, and has favored No. 3; No. 5 is still supported by Ladenburg; conversely, Wichelhaus recommends No. 4, as had been done earlier by Staedeler. The benzene formula proposed by Carius is close to scheme No. 5, while that proposed by

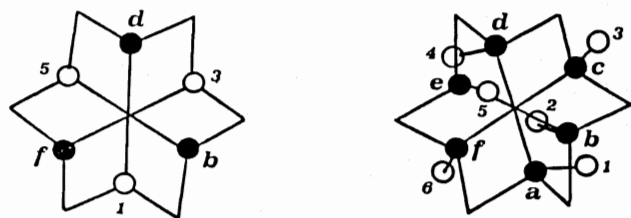


Figure 3. The stereochemical model of benzene proposed by Koerner in 1869, reference 13, p. 242. The carbon atoms are the black spheres identified by letters. In the first image the hydrogen atoms 1,3,5 are eclipsing the carbon atoms *a, c, e*, while the hydrogen atoms 2,4,6 are eclipsed by the carbon atoms *b, d, f*, as required by the tetrahedral model of carbon. The six carbon atoms are arranged as a flattened octahedron, and fill the space satisfying the condition that Kekulé characterized as "une forme plus condensée" in his 1865 paper, and referred to as "die Verdichtung...welche die aromatischen Verbindungen oder den ihnen gemeinsamen Kern charakterisiert" in his 1866 paper.

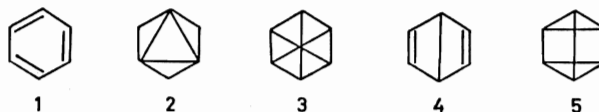


Figure 4. Representation of the bonding schemes used by Kekulé to discuss the constitution of benzene in 1869 and 1872. (References 17 and 21).

Kolbe is to No. 3, provided one leaves out the conception of chemical bonding between carbon atoms, which surely cannot be the case.

Before proceeding to explain why he had preferred, and still preferred, structure 1, Kekulé commented (18):

I admit that for a long time scheme No. 3 seemed to me particularly promising, and later on I found much of beauty in No. 5, although from a viewpoint different from that of Ladenburg.

Kekulé did not specify here what this viewpoint was. Indeed, his paper appears to be little more than an introduction to that immediately following, on the condensation products of aldehydes, where he makes explicit his future research program on benzene (19):

To establish definitively the structure of benzene by obtaining it synthetically in such a way that the nature of the synthesis shall leave no doubt about the type of bonding between the carbon atoms.

The crotonaldehyde condensation had been chosen as an exemplar reaction producing a carbon-carbon double bond (20).

The reason why Kekulé had "found much of beauty" in the Ladenburg model is stated in the 1872 paper where he proposed the assumption of oscillating valence bonds (21). This paper opens with an introductory section in which benzene models were discussed along the same lines as in the 1869 paper (sometimes with the same wording), but with the addition of bibliographic references and models proposed by Paul Havrez and by James Dewar. Here are the relevant points (22):

It has to be accepted that the atoms of a polyatomic molecule are arranged in space so as to best obey all possible attractive forces. The planar arrangement of several atoms shall not be excluded even if it may appear unlikely. Indeed one could be induced to confer a larger probability to conceptions of the constitution of benzene which lead to a regular spatial arrangement of the six carbon atoms.

At this point Kekulé referred to the space modeling procedure (described in the 1867 paper) "... recommended long ago for visualizing our representations of atomic linkages ..." (23), and gave his views on the proposals of Claus and Ladenburg (24):

Schemes [i.e., structures] No. 3 and 5 can be transformed into models in two ways: one form of scheme No. 5 is a triangular prism with the six carbon atoms as its vertices; the most beautiful form of scheme No. 3 resembles a somewhat flattened octahedron lying on a triangular face.

Of course, Kekulé discarded these models, and gave his preference to the constitutional formula as a representation of chemical properties, because scheme No. 1 is in better agreement with the formation of benzene from acetylene and with the synthesis of mesitylene from acetone; it correlates more clearly with the chemical properties of naphthalene and anthracene; and it offers a simpler explanation of addition reactions. To overcome the objections against the assumption of alternating double bonds he now put forward the idea of an oscillation of valence bonds, having given up hope for demonstrating the constitution of benzene synthetically by means of aldehyde condensation.

Nevertheless, there can be little doubt that Kekulé had a definite view of spatial modeling of atoms and molecules, and that this view had a suggestive influence on those who first considered a flattened octahedron (Koerner) or, as we shall see, a prism (van't Hoff) as possible spatial forms for the benzene nucleus (25).

II. The 1874 paper by Le Bel sought to identify two general principles which determine "... the relationship between the atomic formulas of organic substances and the rotatory power of their solutions". He adopted a geometrical criterion for examining molecules having the formula MA_4 , where M is "... a simple or a complex radical combined to four monoatomic

(univalent) atoms A" (26).

Le Bel's first principle is that when a solution of such a substance shows rotatory power, its molecular constitution corresponds to a substitution of three atoms A by univalent atoms or groups R, R' and R'' so that (27):

... the set of the radicals R, R', R'' and A, reduced to material points differing among themselves, forms in itself an edifice not superimposable on its (mirror) image, so that the residue M cannot re-establish its symmetry.

The second general principle states that when one, two or three substitutions in MA_4 give "only one and the same chemical isomer", and its disubstituted derivatives do not have rotatory power, then (28):

... we are forced to assume that the four atoms A occupy the vertices of a regular tetrahedron whose symmetry planes are the same as those of the total molecule MA_4 .



August Kekulé (1829-1896) posing (front row center) with a group of students, circa 1865. Wilhelm (Guglielmo) Koerner (1839-1925) is second from the left in the back row.

When these principles are applied to benzene (29):

... the geometric assumptions which account for isomers in the aromatic series ... consist in placing the six hydrogen atoms either on the six equivalent vertices of a rhombohedron (*rhomboëdre*) or on those of a vertical pyramid (*pyramide droite*) with an equilateral triangle as a basis. A very easy geometric discussion shows that, in each case, two different substitutions will give one asymmetric and two symmetric isomers.

These geometric references are anomalous and require interpretation. Le Bel does not give any reference to the

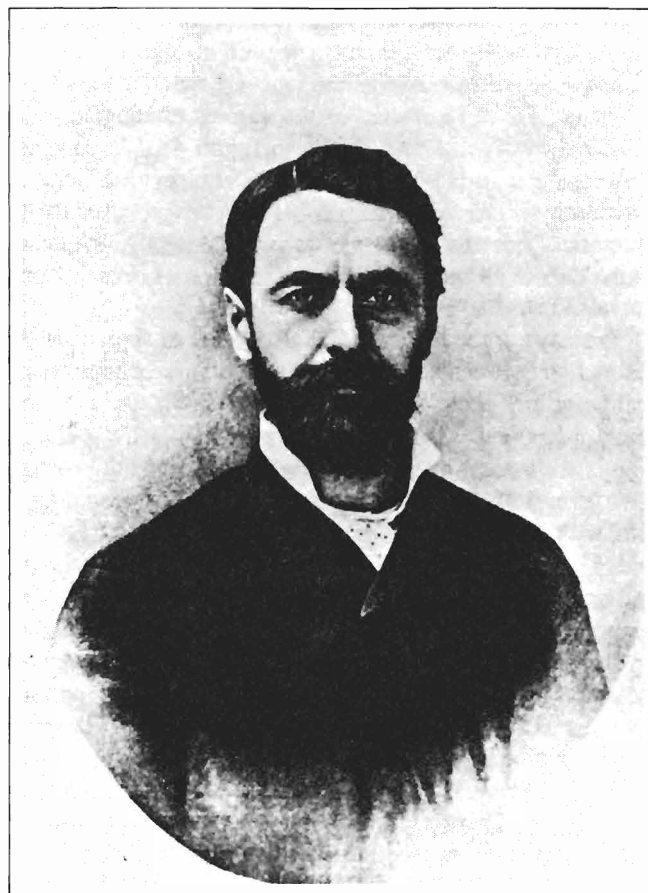
rhombohedral and pyramidal “hypothèses géométriques”, merely commenting that these had “already been discussed elsewhere (*ailleurs*)”. This seems to imply he is referring to a recent source familiar to all French readers. However, we have been unable to find in French chemical journals of the years 1869-1874 any discussion or paper on a geometric interpretation of isomerism in the aromatic series where terms such as rhombohedron or pyramid were used (30).

Considering that in 1874 Le Bel was working in the laboratory directed by Adolphe Wurtz at the *École de Médecine*, we have checked some entries in the *Dictionnaire de chimie pure et appliquée* then being published under Wurtz's direction. The entry “Série aromatique”, written by Alfred Naquet for volume 1 (1869), fails to stress the essential difference between substitution and addition reactions (31). The entries “Hydrocarbures” and “Isomérisie”, written by Wurtz himself for volume 2 (1873), never use the terms chosen by Le Bel in the sections treating aromatic compounds. The constitution of benzene preferred by Wurtz is one of those based on “the inequality of the atomicities” (i.e. either No. 2 or No. 4 as shown in figure 4), while he qualifies the hexagon, which he uses only as a matter of convenience, as (32):

... the graphic representation of Mr. Kekulé, who has numbered in a certain way the attacking points of the elements or groups which can substitute the hydrogens of the benzene nucleus.

Le Bel no doubt attached this same meaning to “l'hexagone de M. Kekulé”, which he cited and illustrated on the same page of his paper, while discussing the optical activity of *p*-cymene derivatives, a point we shall come back to later. Finally, the last part of Le Bel's statement referring to the asymmetric, optically active isomers derived from benzene by disubstitution, shows that he applied the terms *rhomboèdre* and *pyramide* to three-dimensional geometric forms meant to show the constitution of the C_6 nucleus. Indeed, he had followed the same line in his discussion of the possible optical isomers derived by substitution from ethylene, assuming that its four hydrogen atoms were situated at the base corners of a square pyramid. Moreover a *triangular pyramid* has only four vertices, not six, and therefore Le Bel must have meant something else; it was either a slip of the pen or a geometrical blunder. These considerations suggest that Le Bel actually intended the *triangular pyramid* mentioned in the case of benzene to be a *triangular prism*, where the six hydrogen atoms can take equivalent positions. This is the Ladenburg formula.

Le Bel's reference to a rhombohedron is a more difficult case. There is in the French literature of that period an example where the expression *hexagone rhomboédrique* and the term *rhomboèdre* were used to describe the constitution of benzene as a hexagon having three shorter sides alternating with three longer sides, as shown in figure 5 (33). This however cannot be the case with Le Bel, because such a planar disubstituted



Joseph Achille Le Bel (1847-1930)

geometric arrangement would never give asymmetric, optically active isomers. It seems therefore that he really meant to discuss benzene in terms of the six-rhombus faced parallelepiped shown in figure 6. Its eight vertices have different symmetry properties with respect to the trigonal rotation axis, falling into different sets: two are apical and six peripheral to the

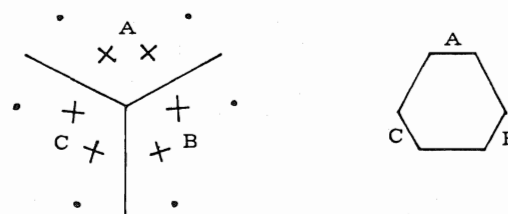


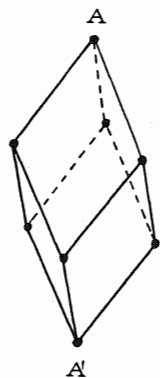
Figure 5. The *rhombohedral* “graphic formula of benzene” proposed by G. D. Hinrichs in 1875, reference 33, p. 49. Here the dots represent “the monovalent hydrogen” and the plus signs “the quadrivalent carbon” atoms. A, B, and C correspond to the shorter bonds, originating from three acetylene molecules and “representing the Berthelot synthesis” of benzene.

trigonal axis. It seems most likely that Le Bel meant to refer to the six equivalent vertices and to neglect the other two (34), as he had done with the apex of the square pyramid in the case of ethylene. He did not discuss the various binding schemes critiqued by Kekulé and shown in figure 4. We conclude therefore that in his 1874 paper Le Bel either exhibited an inaccurate knowledge of the binding schemes proposed for the aromatic C_6 nucleus, or intended to avoid taking a definite stand in the debate on the molecular constitution of organic compounds (35).

We have argued that Le Bel also failed to appreciate the distinction between substitution and addition reactions, a crucial point in Kekulé's arguments for the constitution of aromatic compounds. This is clearly apparent in Le Bel's discussion of the optical activity of camphor and turpentine. Camphor (figure 7) is described as a benzene *substitution* derivative, with the substituents given as CH_3 , $(C_3H_7, H)''$, O'' , H_2 , and H_2 . *Para*-cymene is optically inactive because "the methyl and propyl radicals are in the positions 1 and 4 of the Kekulé hexagon, i.e. in a symmetry plane of benzene" (36). Turpentine oil derives from *para*-cymene by *substitution* of two hydrogen atoms of the aromatic nucleus by "two H_2 groups", and the resulting product will be optically inactive or active, depending on whether the *substitution* has occurred in positions which are or are not "symmetric with respect to the plane passing through 1 and 4" (37). There is indeed a confusion in the meaning of the term *substitution* as used here by Le Bel with respect to the meaning given to it by Kekulé, who meant the replacement of each hydrogen atom by another univalent atom or group. Aromatic substitution, as defined by Kekulé, implies that reaction products have the C_6 benzene nucleus with six unchanged affinities, while on the contrary their number increases in addition reaction products. In Le Bel's interpretation of the constitutions of turpentine oil and camphor, the atoms or groups on the C_6 ring saturate respectively eight and ten affinities (38).

III. In September 1874 van't Hoff published the outline of a general theory for interpreting "the chemistry of carbon compounds" and correctly predicting the number of their isomers

Figure 6. The rhombohedron as a six-rhombus faced polyhedron. The A and A' vertices are on the trigonal axis; the other six vertices are geometrically equivalent with respect to any $2\pi/3$ rotation or any $2\pi/6$ rotation-reflection (S_6 operation), and can represent the homotopic carbon atoms of the benzene nucleus.



Jacobus Henricus van't Hoff (1852-1911)

by expanding "structural formulas into space". His fundamental assumption consisted (39):

... in imagining the four affinities of the carbon atom directed towards the vertices of a tetrahedron having the carbon atom at its centre.

The most relevant consequence of this assumption was explained thus (40):

In the case in which the four affinities of a carbon atom are saturated by four univalent groups differing among themselves, it is possible to obtain two, and only two, different tetrahedra which are mirror images of each other, but which can never be thought of as being brought into coincidence; i.e. one obtains two isomeric structural formulas in space.

In this, and in his subsequent paper, van't Hoff thus unveiled his "asymmetric carbon atom". The difference between Le Bel's and van't Hoff's approaches has been already pointed out in the introduction. We can summarize it here by saying that Le Bel related the optical activity of dissolved substances to an intrinsic asymmetry of their molecules, which caused

optical isomers to exist in the form of non-superimposable images; van't Hoff, on the other hand, outlined a theory of molecular structure of the same substances, and in 1874 related their optical activity *only* to the presence of an asymmetric carbon atom in their molecules. We also drew attention to Grossman's suggestion that van't Hoff extended his theory *after* reading the more general formulation given by Le Bel (41). We shall now argue that this was indeed the case by analyzing how the two men differed in dealing with the structure of benzene and aromatic compounds, and we will see that it is even possible to establish approximately when van't Hoff incorporated Le Bel's more general approach into his theory.

In his 1874 paper van't Hoff cited in Part 1, Section a (carbon compounds that contain at least one asymmetric carbon atom) camphor, borneol, camphoric acid, turpentine oil and menthol, specifying structural formulas based on those proposed by Kekulé (42). This list also contains the optically active substances discussed by Le Bel under the heading "série aromatique" and assumed for them the same constitution as that in figure 7. The difference is that van't Hoff cited them all together as "corps aromatiques" in the opening of Part 2 (compounds with double-bonded carbon atoms), but he did not discuss them again, nor did he introduce the aromatic series as such, because Part 2 focussed on the isomerism of maleic and fumaric acids and their analogues.

Van't Hoff sent a subsequent condensed version of the 1874 French paper, written after reading Le Bel's article, to Arthur Henninger, an acquaintance of both men (43). This was published after the 5 March 1875 meeting of the *Société Chimique de Paris* (44). As we learn from the *Procès-Verbaux* of the Society, the manuscript was discussed on 19 March 1875, and criticized by Marcelin Berthelot, chairman of that meeting (45). The definition of the asymmetric atom now had wording slightly different from that of the 1874 Dutch and French versions, and the paper lacked figures. With respect to our present discussion there are two points of interest: first, the list of the six-membered ring compounds has disappeared, all being now reduced to the mention of "cymène inactif dérivé du camphre actif" in Part 1; second, in the closing passage, after referring to the paper by Le Bel, van't Hoff makes the following comments (46):

In this fine work the author treats the matter that I have outlined in the first part of the present paper from a somewhat different viewpoint, nevertheless, I wanted to present it all because it forms a whole. It is evident that *concerning aromatic compounds, Mr. Le Bel has discussed the asymmetry problem in the most general terms (dans toute sa généralité), whereas I only treated a special case* [italics added].

The difference in the approach taken by Le Bel was therefore soon clear to van't Hoff. But it is hard to believe that he had not noticed at least some of Le Bel's misunderstandings of

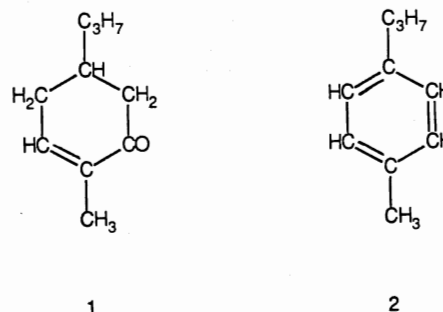


Figure 7. The relationship between the constitution of camphor 1, and *p*-cymene 2, as referred to in the 1874 papers by Le Bel (reference 26, p. 345) and van't Hoff (reference 39, p. 447). Both considered camphor to have the constitution proposed by Kekulé in reference 42, which is not the modern one.

Kekulé's benzene theory pointed out in our previous discussion of Le Bel's paper. The statement above is also untrue, because van't Hoff had not discussed a special case of aromatic compounds, but only mentioned, in a rather different context, the same substances that Le Bel considered as belonging to the "série aromatique". The simplest way of explaining van't Hoff's curious comments is to suppose that he intended to avoid a marginal polemic with respect to a topic which at that time was very controversial and which divided the chemical scientific community. As a 22 year-old graduate he was looking for approbation by the same community and seeking a position. He was also surely attempting to provide chemists with concepts that were easily understood and could help to solve the problem of correctly predicting the number of isomers of carbon compounds.

It is therefore reasonable to assume that, stimulated by what he referred to as the generality of Le Bel's treatment, van't Hoff felt that he should not, and could not, avoid a full discussion on the structure of benzene and aromatic compounds. During this attempt he must have perceived the essential value of the principles given by Le Bel for discriminating between the hexagonal and the prismatic models of the C_6 nucleus. This is why van't Hoff soon acknowledged the generality (i.e. perceived the greater value) of Le Bel's viewpoint on aromatic compounds, and went on to test his claim of "a very simple geometric discussion" for disubstituted benzene derivatives. Moreover, van't Hoff must have seen how useless it would have been to argue against the *rhomboèdre* or the *pyramide droite à base de triangle équilatéral*, the geometric forms Le Bel had chosen to exemplify the application of his principles to aromatic isomers. Indeed, he avoided doing so both in his short paper sent to the Paris Chemical Society in March 1875, and in the longer discussion on benzene he was preparing with the use of his tetrahedral carbon models. Some feelings of indebtedness to Le Bel must have remained in van't Hoff's mind

for a long time, because he knew that in his original formulation of the structural theory he had failed to grasp that the root of optical activity was in molecular asymmetry. This could be the reason (among others) why the 1887 (second) edition of the *La chimie dans l'espace* bears the dedication "à M. J. A. LE BEL. En témoignage de ma respectueuse affection" (47).

It seems therefore most likely that while, or after, writing to Henninger in March 1875, van't Hoff was already working on aromatic compounds for the booklet to be published, at his own expense, in Rotterdam with the date of May 1875, and to be sent soon after to the most eminent chemists of his time (48). Here his discussion of benzene is very different, and it is quite surprising with respect to what he had said in his 1874 papers and in the short communication sent two months before to the Paris Chemical Society. Let us examine it.

Chapter IV, the last of *La chimie dans l'espace*, consists of four pages devoted to aromatic compounds, and two pages devoted to some concluding remarks. We shall analyze now the part concerning the "série aromatique", which opens with the following proviso (49):

The application of my assumption to aromatic compounds faces a difficulty because current theory has not yet decided on the constitution of benzene, the fundamental substance of the series. Therefore I shall limit myself to the following points:

1. Examination of the number of substitution derivatives predictable by the hexagon proposed by Mr. Kekulé and the prism proposed by Mr. Ladenburg.
2. My viewpoint, applied to the two symbols (*aux deux symboles*), does not allow predicting a difference in the number of the substitution products.
3. For addition products my viewpoint allows isomers not predicted by the present symbols (*par les symboles actuels*).

These introductory points show first of all that van't Hoff, dealing separately with the substitution and the addition products of benzene, had a full understanding of the theory of aromatic compounds and knew the current state of the controversies regarding their constitution; secondly, that he chose to evaluate only two of the proposed structures in terms of spatial arrangement of molecules built with tetrahedral carbon atoms, and avoided taking a stand by explaining the reasons for his choice. His results are collected in figure 8.

The consequences van't Hoff drew from the hexagonal benzene formula are the same as those of Kekulé. The predictable isomers are "only one for the compounds C_6R_6 and C_6R_5X ; three for $C_6R_4X_2$ and $C_6R_3X_3$ " (50). Moreover he added that since a strict application of the "symbole de M. Kekulé" requires, as noted by Ladenburg, the substitution in (1,2) to be distinguished from that in (1,6), in this instance the isomers predictable for $C_6R_4X_2$ become four.

The consequences drawn for the prismatic spatial model are different from those of Ladenburg (who based them on his

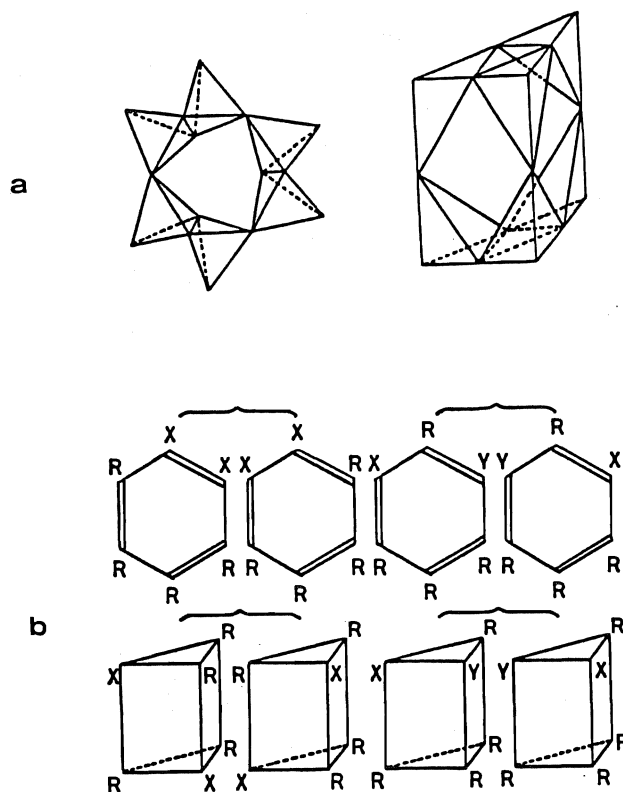


Figure 8. The molecular structures used by van't Hoff to discuss aromatic compounds: (A) the models of Kekulé and Ladenburg; (B) the counting of the $C_6R_4X_2$ and CR_4XY isomers (48).

representation), because now one case of $C_6R_4X_2$ gives two non-superimposable mirror images, and therefore four isomers altogether. Van't Hoff's isomer counting was apparently based only on his 1874 tetrahedral carbon structural theory, because he did not state in this same context that two of these four isomers are expected to be optically active. We shall return to this point after considering the other case of disubstitution.

Van't Hoff's discussion of the C_6R_4XY case follows the same lines. Kekulé's hexagon, strictly considered, predicts five isomers; and the same number is found for the Ladenburg prism because non-superimposable mirror images are now produced in two cases. In the next step van't Hoff took into account that (51):

... answering the criticism by Mr. Ladenburg, Mr. Kekulé has introduced in his hexagon a mode of motion that cancels the increase of the isomer number consequent to the fixed double-bond position.

In this instance the hexagon and prism space models lead to different predictions, because the isomers of the Kekulé bond-

ing scheme are now reduced to three for any disubstituted benzene derivative, while those of the Ladenburg bonding scheme remain unchanged. Finally, van't Hoff compared his deductions with the known results regarding the isomers of nitrobenzoic and nitrotoluic acids, and concluded that the available experimental evidence did not yet allow a choice between the hexagon and the prism models to be made.

The first comment of van't Hoff on the possibility of optical isomers among benzene substitution products comes in at the end of this long discussion on point 1 of the list made at the beginning of the chapter (52):

I shall *add* that an isomerism such as the one due to the difference shown in the Fig. 8, could (*pourrait*) produce optical activity according to what was said on pp. 20-22 (of Chapter II, ref. 49): the active cymene is perhaps its realization, and this is for me the *third case* of optical activity" [italics added].

This is a rather ambiguous statement because Chapter II of reference 48 concerns the optical activity of compounds containing asymmetric carbon atoms. In the pages referred to above we find stated as "the first cause of optical activity: the compound contains asymmetric carbon atoms". The possibility of other cases of optical activity is seen then as depending on the link between molecular constitution and optical activity as explained in the section titled "Physical interpretation of the rotatory polarization of dissolved active substances". The thesis put forward by van't Hoff is an extension to active molecules of the interpretation of the rotatory polarization of crystals given by Karl F. Rammelsberg (53) and is thus reported (54):

I was suddenly (*au premier abord*) struck by the relation between Mr. Rammelsberg's views on active crystals and my own observation on active molecules. According to this scientist the rotatory polarization in solids coincides with the existence of two crystalline forms ... *one of which is the mirror image of the other*. [italics added]

The crucial point of its extension by van't Hoff is that (55):

... the arrangement of the molecules in the active crystal (is) fully analogous to the arrangement of the atomic groups as I have assumed in the active molecule.

Rammelsberg's assumption of an arrangement of molecules along "positive and negative helices ... recalling the comparison made by Mr. Pasteur", as a possible explanation of the mirror hemihedral forms found in active crystals, is adopted also by van't Hoff. In fact, he goes on to show that the atomic groups arranged around the tetrahedron of an asymmetric carbon atom are always located along a helical path.

It is surely correct to discuss the optical activity of the prismatic models of disubstituted and trisubstituted benzene

isomers in such terms, i.e. by distinguishing between right and left helical arrangements now introduced as an elaboration of ideas previously expressed by Rammelsberg, Pasteur and John Herschel. In fact, these authors were already quoted by van't Hoff at the end of his 1874 paper (39), and were mentioned again in his shorter communication to the Paris Chemical Society (44), because the extension of their assumptions was meant to link the asymmetric carbon atom to the "physical interpretation of the rotatory polarization of dissolved active substances". But now van't Hoff uses this quotation to emphasize the *third case* as a consequence of the tetrahedral spatial arrangement which generates the asymmetric carbon atom, or to stress the general and unifying value of this key concept of his new structural theory, proposed for correctly counting isomers numbers. To show that his own theory was self-contained, sufficient in itself to deal with the cases of optical activity known or predictable for saturated (the first case), ethylenic (the second case), and aromatic compounds (the third case), seemed perhaps to van't Hoff the best way either to prevent any possible future claim of having used somebody else's ideas, or to hide the modifications arising from his reading of Le Bel's paper.

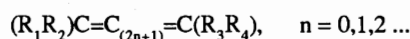
More decisive evidence that something had changed in van't Hoff's mind after reading Le Bel's paper can be found in the conclusion of Chapter IV. Its last part opens with a correction of the "règle générale" that van't Hoff had given at the start of Chapter II, where the "règle" reads (56):

Any carbon compound whose solution causes a deviation of the polarization plane, contains an asymmetric carbon atom.

The reason for the correction is thus explained (57):

... during this work we have come to predict the possibility of optical activity in *two more cases*; and these cases refer to *unsaturated* (carbon) *compounds* ... In order for my observation to keep all its value the sentence "any carbon compound" has to be replaced by "any *saturated* carbon compound" [italics added].

Of the "two more cases" of possible optical isomers mentioned above among the "combinaisons non-saturées" of carbon, one is obviously that of the aromatic compounds, in their prismatic form, characterized before as "the third case" (52). The other is that of the allenes, where the optically active cases are given in terms of the general formula:



and qualified as "the second case" at the beginning of the Chapter III. The prediction of their optical activity is done in these terms (58):

The four groups will form the vertices of an asymmetric tetrahedron;

therefore according to the reasoning done at pp. 20-22 [of Chapter II, reference 48] it is possible to predict optical activity ... This I shall call the *second case* ... R_1 has to be different from R_2 , and R_3 different from R_4 , without requiring R_1 and R_3 or R_4 to be different [italics added].

Their prediction as mirror images had been made in Chapter I and their cardboard models (numbered XX and XXI for the case $R_1 = R_3$, $R_2 = R_4$) prepared, and distributed, for counting them as isomers. At the beginning of Chapter III they became the first example of unsaturated optically active molecules, labeled as the second case (59). If we leave aside the reference to Chapter II of reference 48, whose meaning has been discussed above, it is evident that this case strictly adheres to the first principle stated by Le Bel and quoted earlier (27):

... the set R_1, R_2, R_3 , and R_4 , reduced to material points, forms in itself an edifice not superimposable to its mirror image, while the residue M cannot re-establish its symmetry.

It is hard to imagine that this rather obvious connection had escaped the attention of van't Hoff. His theory of atomic bonding in space in fact put him in the position of being able to predict when the structure of the group M allowed or forbade molecular symmetry; and of appreciating the different consequences of having M as C=C, or as C=C=C, etc. Therefore it is no surprise that he made these predictions explicit, adding the two cases based on unsaturated bonding to the prediction of optical activity based on the saturated asymmetric carbon atom.

It seems probable that the correction of the rule given in Chapter II was inserted in the concluding part of Chapter IV when van't Hoff could no longer revise the galley-proofs of the preceding text. This last chapter, devoted to aromatic compounds, must have been started at about the time he sent Henninger the communication to be read before the Paris Chemical Society. It must have been more time-consuming than he had foreseen, because 19 figures out of the total 55 (17 in the last plate) refer to the discussion of aromatic compounds.

It is more difficult to conjecture whether the application of the principles stated by Le Bel was first made to the sequence of ethylenic bonds which corresponds to the allenes, and then extended to aromatic compounds. The remark made before in footnote 59, and the order of the chapters in the book would suggest that this was the case. But the expository sequence of the book does not have to be the same as the sequence of van't Hoff's thoughts. Reading Le Bel's paper must have stimulated a complex interplay of thinking, model making, and writing which could make an interesting historical investigation (60). At any rate, we hope we have helped to establish the role that the discussion of stereochemical models of benzene played in the development of van't Hoff's ideas and in his relationship with Le Bel during the years 1874-75.

How did Le Bel feel about all this? He lived in a cultural

context where the connections between crystal morphology, molecular structure and optical activity had an established tradition. This link became evident in succeeding years. Le Bel's attempts to test the optical activity expected with the disubstituted derivatives of the prismatic structure of benzene are first reported in the *Procès-verbaux* of the meeting of the Paris Chemical Society on 23 June 1882: the mould cultivation of a dilute solution of toluidine (1-methyl, 2-aminobenzene) hydrochloride failed to show optical activity (61).

Le Bel never published an explicit revision of his "pyramidal" or "rhombohedral" geometric assumptions, while he consistently maintained that "the regular tetrahedral form of CR_4 molecules is not among the necessary and sufficient conditions for justifying the rotatory power" (62). He explained in 1890 what in 1874 had given to his "demonstrations une forme particulière et en apparence moins simple", causing him to reject the idea that the four affinities, or valencies, of carbon were directed along the four trigonal axes of a tetrahedron. The reason given is based on the assumed congruence between molecular form and crystal form, one of the axioms on which Bravais had worked out his theorems: "because the assumption of the regular tetrahedron requires the CR_4 substances to be cubic and the CR_3R' substances to be rhombohedral" (63). While this prediction is verified by Cl_4 and Cl_3H , it is not by CBr_4 . Le Bel prepared this, and found that its crystals were birefringent to parallel polarized light, and therefore could not belong to the cubic system, in agreement with his theory. Then, in the conclusion he added the following comment (64):

... benzene and its derivative C_6Cl_6 could even fail to crystallize in the hexagonal system (they actually make hexagonal crystals) and allow nevertheless understanding the correctness of the rules derivable from the Kekulé hexagon.

A year later Le Bel made a new "examen analytique" of the "relations between rotatory power and molecular structure". He adopted the name "stéréochimie" for defining what van't Hoff had called "la chimie dans l'espace", spoke of "théorie du carbone asymétrique", and proposed an axiomatic formulation to show that the tetrahedral model was not needed for explaining the optical activity of saturated carbon compounds. In the section discussing "corps aromatiques" he accounted for the failure of all attempts to obtain optically active isomers in terms of "une vérification nouvelle de l'hypothèse de Kekulé". How this was to be understood he made clear in a footnote (65):

... the results on benzene and its derivatives agree with the structural assumption of either a rigid regular hexagon or "a closed chain having a certain mobility".

This referred once more to the rejection of the single and double bond alternation in the benzene ring as interpreted by

van't Hoff in figure 8.

These ideas and feelings about the role of the unsaturated carbon compounds in the early events of stereochemistry seem to have remained in Le Bel's mind until near the end of his long life. He referred to them again on 22 December 1924, on the occasion of the 50th anniversary of the birth of stereochemistry, an international gathering convened in Paris by the French Chemical Society for the joint celebration of "notre illustre collègue J. A. Le Bel et la mémoire de J. H. van't Hoff" (66). In the closing speech of the ceremony Le Bel said (67):

Van't Hoff proposed a very interesting theory of ethylene which seemed to justify the existence of four poles: two carbon atoms, figured as two tetrahedrons, would become bound through two poles by their polar attraction, which stabilize the molecule while the four hydrogens form a rigid rectangle, explaining the maleic-fumaric isomerism and some other facts. On the other hand, the four poles are in absolute contradiction with the results derived from the hexagon theory of Kekulé, where the two ortho compounds (1,2) and (1,6) are identical, while according to van't Hoff the compound having a double bond in (1,2) has to differ from that which has a single bond in (1,6) (sic: to read (1,2)). In spite of this contradiction the majority adopted the tetrahedron [italics added].

A very representative group of that majority was in fact present in the audience, and he chose to take this occasion to distinguish again his own theory from that of van't Hoff. These feelings surely must have been very deep in his mind and heart. At the roots of these feelings is the treatment of the optical activity given by van't Hoff for unsaturated compounds and the stereochemical models of benzene.

References and Notes

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- O. B. Ramsay, *Stereochemistry*, Heyden, London 1981, pp. 62-68, quotations from p. 65. See also O. B. Ramsay, "Molecular Models in the Early Development of Stereochemistry: I. The van't Hoff Model. II. The Kekulé Models and the Baeyer Strain Theory" in O. B. Ramsay ed., *Van't Hoff-Le Bel Centennial*, American Chemical Society, Washington, D.C., 1975, pp. 74-96. The purpose of Koerner's paper has been stressed also in a paper by J. M. McBride, "Completion of Koerner's Proof that the Hydrogens of Benzene are Homotopic. An Application of Group Theory", *J. Am. Chem. Soc.*, 1980, 102, 4134-4137. Ramsay also states, p. 65, that Koerner, after being a student in Kekulé's laboratory in Ghent, "returned" to Palermo. This is not accurate, because he moved to Palermo for the first time in December 1867, as recorded by R. Anschütz in his memorial paper, *Berichte*, 1926, 59, A, 75-111, see p. 87. Comments on Koerner's structure of benzene can be found on p. 91. Other comments and additional biographical information have been published recently by H. W. Schütt, "Guglielmo Koerner (1839-1925) und sein Beitrag zur Chemie isomerer Benzolderivate", *Physis*, 1975, 17, 113-125.
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- Reference 9, p. 217.
- Reference 10, Vol. I, p. 157-158.
- W. Koerner, "Fatti per servire alla determinazione del luogo chimico nelle sostanze aromatiche", *Giornale di scienze naturali ed economiche*, 1869, 5, 212-256, quote on p. 242.
- Ibid.*, p. 241.
- S. Cannizzaro, "Rapporto del Prof. Cannizzaro detto nella seduta del 13 giugno 1869", *Giornale di scienze naturali ed economiche*, 1869, 5, 208-211, see p. 210.
- Perhaps Palermo in 1869 was one place where spatial representations of carbon compounds were considered acceptable in a publication. Cannizzaro and Koerner are to be praised for this. A few months previously Emanuele Paternó, then a chemistry student and also working in Koerner's laboratory, had published some models using the tetrahedral carbon atom for discussing the possible isomers of $C_2H_4Br_2$, in *Giornale di scienze naturali ed economiche*, 1869, 5, 117-122. This paper later became a case for arguments on priorities for such an idea, and Paternó in 1913 published a personal letter by Adolf Lieben, dated 25th June 1869 from Turin, trying to discourage

both Cannizzaro and Paternó from publishing "... speculations about the true position of atoms in space ..."; the facsimile is reproduced in the *Gazzetta*, 1913, 43, 501-503. Some other details on the 1869 work by Paternó are given in the book by Ramsay quoted in reference 7.

17. A. Kekulé, *Berichte*, "Ueber die Constitution des Benzols", 1869, 2, 362-365, quote on p. 364.

18. Reference 17, p. 364.

19. A. Kekulé, "Condensationsproducte des Aldehyds; Crotonaldehyd", *Berichte*, 1869, 2, 365-368, quote on p. 365.

20. The interpretation of the synthesis of mesitylene proposed in 1867, reference 9, was itself an assumption, and not yet a demonstrated synthetic pathway.

21. A. Kekulé, "Ueber einige Condensationsproducte des Aldehyds", *Ann. Chem.*, 1872, 162, 77-124. This paper was an implicit admission of failure to establish the nature of the benzene carbon-carbon bond unambiguously, as announced in 1869, reference 19.

22. *Ibid.*, p. 82.

23. *Ibid.*, p. 82.

24. *Ibid.*, p. 83.

25. Although it is not possible to know if Koerner ever discussed his model with Kekulé during his stay in Ghent, there can be little doubt that the model derives from the tetrahedral arrangement of the carbon atom affinities put forward by Kekulé in 1867, reference 11. Moreover, it has to be pointed out that van't Hoff in 1872 studied under Kekulé in Bonn, and in 1873 was in Wurtz's laboratory before returning in Holland for his Ph.D. in Utrecht under E. Mulder.

26. J. A. Le Bel, "Sur les relations qui existent entre les formules atomiques des corps organiques et le pouvoir rotatoire de leurs dissolutions", *Bull. Soc. Chim.* (Paris), 1874, 22, 337-347.

27. *Ibid.*, p. 338.

28. *Ibid.*, p. 339.

29. *Ibid.*, p. 345.

30. The 1869 papers of Kekulé discussed above, reference 17 and 19, were summarized by A. Wurtz in the *Bull. Soc. Chim.* (Paris) 1869, 12, 465-467, and in *Ann. Chim. Phys.* 1871, 23, 324-328. These summaries do not report Kekulé's discussion of the different constitutional formulas proposed for benzene. The 1872 paper, reference 21, apparently was never summarized in French journals. (We have particularly sought a French source because Le Bel was above all reporting to the French chemical community, although, being Alsatian, i.e., German speaking, it cannot be excluded that he simply referred to Kekulé, reference 21).

31. This same failure is also to be found in the discussion by Le Bel, as we shall see. A. Naquet made such a distinction quite clearly only in the third edition of his book: *Principes de chimie fondée sur les théories modernes*, Vol. 2, Savy, Paris, 1875, pp. 496-504. Its first edition had been published in 1865, and the second in 1867, too early for Kekulé's theory to be reported in his textbook.

32. A. Wurtz, *Dictionnaire de chimie pure et appliquée*, Vol. 2, Hachette, Paris, 1873, pp. 142-152, quote on p. 150.

33. G. D. Hinrichs, "Sur la structure des molécules de la benzine et du térébène", *Compt. Rend.*, 1875, 80, 47-51. Gustav D. Hinrichs

(1836-1923) interpreted the molar heat of benzene vapor assuming that the absorbed or lost heat goes into molecular rotational motion. By comparing the value of the inertial momentum calculated under such an assumption with the value corresponding to the distribution of the C and H atomic masses on a rhombohedral hexagon, Hinrichs concluded that the *forme générale* of benzene is a *rhomboèdre*. His paper was communicated by M. Berthelot at the meeting of the *Académie des Sciences* held on 4 January 1875, i.e. about three months after the publication of Le Bel's paper, reference 26. In their historical review of stereochemistry C. A. Bischoff and P. Walden (reference 2, p. 15) refer to Hinrichs, Professor of Physics, Chemistry and Mineralogy at the State University of Iowa, as a precursor of research on atoms in space, quoting a 1867 lithographed paper titled *Atommechanik oder die Chemie, eine Mechanik der Panatome*. Hinrichs quotes the book *The Principles of Chemistry and Molecular Mechanics*, Davenport, Iowa 1874, which however is not mentioned in his obituary note published by Charles Keyes in *The American Mineralogist*, 1924, 9, 5-8. A copy of Hinrich's book can be found in the Smith Collection at the University of Pennsylvania.

34. However, this constitutional scheme contradicts the second part of the statement by Le Bel concerning the *hypothèses géométriques* on benzene, reference 26, because its unequal disubstitution leads to one symmetric (para) and two asymmetric (ortho and meta) isomers.

35. The two alternatives are really not mutually exclusive: Le Bel stresses his intention to keep his analysis within a geometrical frame, and probably considers the debate on constitution devoid of real value in terms of an understanding of molecular reality. In a footnote on the first page of reference 26, he quotes A. Bravais' *Etudes Cristallographiques*, Gauthier-Villars, Paris, 1866, pp. 194-236, where crystals are also discussed as assemblages of polyatomic molecules. Le Bel's predilection for Bravais' crystallographic approach suggests that he was much more confident in Bravais' geometric considerations extended through rotatory power, which constitutes the core of his paper, than in the perspective of the structural bonding debates. This is in line with the fact that he paid more attention to molecular symmetry than to molecular structure, and with his choice of geometric forms for discussing the possible optical activity of ethylene and benzene derivatives.

36. Reference 26, p. 345. This statement remains true both for the hexagon as Kekulé's representation, and for the six equivalent vertices of the rhombohedron as the geometric form chosen by Le Bel.

37. *Ibid.*, p. 346.

38. The discussion of their optical activity by Le Bel remains quite ambiguous, because the carbon atoms of the benzene ring after what he calls *substitution* belong to the MA₄ type, but are not examined in terms of his first principle. At the same time the symmetry plane through the positions 1 and 4 of the hexagon coincides with the plane passing through the para positions of the rhombohedron as discussed above, note 36.

39. J. H. van't Hoff, "Sur les formules de structure dans l'espace", *Archives Néerlandaises des Sciences Exactes et Naturelles*, 1874, 9, 445-454. This text is the French translation of a pamphlet in Dutch

summarizing van't Hoff's dissertation. A similar French text was published again in 1887 as part of the historical introduction in reference 1, pp. 13-17. For other details on the Dutch text and on his doctoral dissertation see the articles by H. S. van Klooster, reference 3, H.A.M. Snelders, reference 5, and J. Weyer, reference 6.

40. *Ibid.*, p. 446.

41. A comment on the paper by R. B. Grossman, reference 8, is in a letter which I have sent to the Editor of *J. Chem. Educ.*, 1991, 68, 797-798.

42. A. Kekulé, "Ueber einige Körper der Camphergruppe. Constitution des Camphers", *Berichte*, 1873, 6, 929-934. The quotation of Le Bel is not from this original paper, but from its summary published in the *Bull. Soc. Chim.*, (Paris), 1873, 20, 558-559. Van't Hoff's comment does not have a bibliographic reference. Obviously the formulas both referred to differ from the correct modern formulas of these substances, while turpentine oil owes its optical activity mainly to α -pinene.

43. Arthur Henninger (1850-1884) was then a student at the Ecole de Médecine directed by Wurtz, which van't Hoff had visited in the middle of 1873. A joint communication by Henninger and Le Bel (assistant to Wurtz), published in *Compt. Rend.*, 1874, 79, 480, reports on the building of a distillation apparatus.

44. J. H. van't Hoff, "Sur les formules de structure dans l'espace", *Bull. Soc. Chim.* (Paris), 1875, 23, 295-301.

45. M. Berthelot, "Extrait des procès-verbaux. Séance du 19 mars 1875", *Bull. Soc. Chim.* (Paris), 1875, 23, 338-340. This is the heart of the criticism by Berthelot: "Without denying in general the interest of formulas of this type, more rational than the usual planar formulas, it must nevertheless be considered that a satisfactory representation of chemical compounds cannot be achieved without introducing into them the notion of the rotational and vibrational motions of each single atom and group of atoms within the molecule", quote on p. 338.

46. Reference 44, p. 300-301.

47. Reference 1, p. 3 (printed as shown).

48. J. H. van't Hoff, *La chimie dans l'espace*. P. M. Bazendijk, Rotterdam, 1875. The preface of this book looks like a letter to the chemical community outside of the Netherlands, regretting that Dutch colleagues had not discussed his theory, but instead "just inserted its French translation in the *Archives Néerlandaises*", see here reference 39. In Chapter I, a footnote on p. 7 has a list of nine "savants étrangers" to whom van't Hoff sent a copy of the book and a set of cardboard models, asking them to express "un jugement". According to H. S. van Klooster, the same complaint lies behind two of the 32 Statements put by van't Hoff in his Dissertation, and to be defended publicly during its discussion. Statement No. 7 reads: "There should be a committee appointed to act on matters of a chemical nature". See reference 3, p. 1096.

49. *Ibid.*, p. 38. The discussion of the structural assumptions made by van't Hoff regarding addition products clearly distinguishes between the two proposals: the Ladenburg prism can give molecules which contain a three-membered ring, whereas the Kekulé hexagon allows for molecules containing the six-membered ring. The discus-

sion of their structure leads van't Hoff to predict isomers which today we would characterize as conformational. A discussion of this is beyond the scope of the present paper.

50. *Ibid.*, p. 38.

51. *Ibid.*, p. 39. The Kekulé paper referred to is reference 21.

52. *Ibid.*, p. 40.

53. K. F. Rammelsberg, "Ueber die Beziehungen zwischen der Circularpolarisation, der Krystallform und der Molecularconstruction der Körper", *Berichte*, 1869, 2, 21-37.

54. Reference 48, p. 20.

55. *Ibid.*, p. 21. Almost the same words were used by van't Hoff in the summary reference to Rammelsberg's work (53) made at the end of the 1874 paper, p. 454 of reference 39, and of its March 1875 reduced text, p. 300 of reference 44.

56. *Ibid.*, p. 18. Here van't Hoff put camphor, borneol and camphoric acid as concluding examples of this rule, and then quoted Le Bel's paper of reference 26 as "having just made the same observation and proving its correctness", p. 19.

57. *Ibid.*, p. 42.

58. *Ibid.*, pp. 29-30.

59. *Ibid.*, pp. 13-14. This circumstance suggests that van't Hoff changed his mind between the writing of Chapters I and III. A reference to the mirror image isomers $(R_1R_2)C=C(R_1R_2)$ is made in the last footnote of Chapter I, reference 48, p. 16, footnote appended to this peculiar sentence: "but I do not want to annoy my readers by considering matters which *I shall absolutely not need in the continuation*" [italics added]. At the time of writing this chapter he probably had not yet decided to discuss benzene and aromatic compounds. It is worth noting that attempts to test the predicted optical activity of substituted allenes, started in 1903, all failed until 1935 when a successful separation of optical isomers was first reported by Peter Maitland and William H. Mills, "Resolution of an Allene Hydrocarbon into Optical Antipodes by Asymmetric Catalysis", *J. Chem. Soc.*, 1936, 987-998.

60. The use of cardboard models "pour faciliter la représentation" during the elaboration of the theory, was reported by van't Hoff in a footnote of Chapter I, *ibid.*, p. 7.

61. "Extrait des procès-verbaux. Séance du 23 Juin 1882", *Bull. Soc. Chim.* (Paris), 1882, 38, 98. This report is also quoted by van't Hoff in the page devoted to aromatic compounds in the 1887 edition of his booklet, reference 1, p. 81. The "dédoublément" procedure had been worked out by Pasteur. The failure of these repeated attempts later led Le Bel to favor the hexagonal structure of benzene. He never wrote a paper on these experiments, but only mentioned them on the occasion of discussions at the meetings of the Paris Chemical Society, e.g., see the *Procès-verbaux* of 28 November 1890. They are quoted again in a lecture given on November 1891 to the Paris Chemical Society, see p. 615 of reference 65 below.

62. J. A. Le Bel, "Sur les conditions d'équilibre des composés saturés du carbone", *Bull. Soc. Chim.* (Paris), 1890, 3, 788-796.

63. *Ibid.*, p. 790. See the comment made in footnote 35 and Bravais' papers there quoted.

64. *Ibid.*, p. 796.

65. J. A. Le Bel, "Les relations du pouvoir rotatoire avec la structure moléculaire", *Revue scientifique*, 1891, 48, 609-617. Aromatic compounds are discussed on p. 615. This paper, very relevant for understanding the distinction Le Bel meant to stress between his own theory and the structural theory of van't Hoff, again gives evidence for those aspects of Le Bel's ideas mentioned in footnote 35, still persisting in 1891.

66. "Le cinquantenaire de la théorie du carbone asymétrique", *Bull. Soc. Chim. (Paris)*, 1925, 37, 341-356.

67. *Ibid.*, pp. 353-4. The relevant part of the original text reads: "... les deux corps ortho-1.2 et 1.6 sont identiques tandis que d'après van't Hoff le corps 1.2 où il y a double liaison entre les carbones doit différer du corps 1.6 où il y a la liaison simple." (I am very grateful to Prof. J. Michael McBride for pointing me out the logical incongruence originating from the misprint: the last 1.6 must read 1.2).

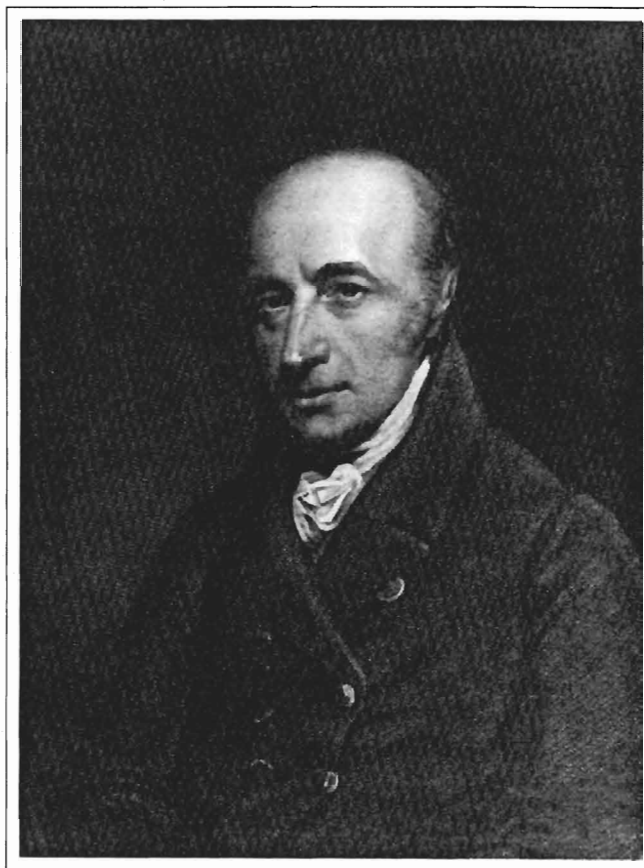
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SOME EARLY CHEMICAL SLIDE RULES

William D. Williams, Harding University

An article by George Bodner in the Winter 1990 issue of the *Bulletin* described a rare chemical slide rule designed by Lewis C. Beck and Joseph Henry - their little-known "Improved Scale of Chemical Equivalents" (1). The reader is urged to review that description. The present paper attempts to place this slide rule in context by describing its origins, as well as some of its predecessors and successors.

The concept of "A Synoptic Scale of Chemical Equivalents" was first presented in 1814 by the English chemist, William Hyde Wollaston (2). Chemical substances were arranged on a scale with distances proportional to the logarithm of their equivalent or combining weights, much as the value of π was marked on the scales of the more conventional slide rules of recent memory. A logarithmic slider, numbered from 10 to 320, allowed quick calculation, via the method of direct and inverse proportions, of the weights of substances reacting with one another, the quantity of products, or the relative proportions of elements in a compound. Wollaston's original design, measuring 12 by 2.5 inches, was marketed in



William Hyde Wollaston (1766-1828)

London that same year. A contemporary called it "an instrument which has contributed more to facilitate the general study and practice of chemistry than any other invention of man" (3). It accelerated the acceptance of Dalton's atomic theory and promoted chemistry as a mathematical science.

Only a few of these original slide rules are still in existence. Some are in European museums (4). Only two are known to have survived in the United States and both are located at Harvard University. They are described as (5):

Pine; paper labels. L of each 12 in. Inscr. on face: Chemical Equivalents; lists of elements and compounds; Published by W. Carey, 182 Strand, Jan. 1, 1814. On slider: graduated, numbered scale. On back: By Special Appointment / [arms] / Thomas Jones, / (Pupil of Ramsden.) / ASTRONOMICAL / and / philosophical / INSTRUMENT MAKER / To His Royal Highness / The Duke of Clarence / 62 Charing Cross, / LONDON.

Drawings and discussions of the Wollaston slide rule appeared in several early American chemistry texts (6). Wollaston used oxygen = 10 as his equivalent (atomic) weight standard, but his choice was not accepted by all of his contemporaries. Thomas Thomson observed that Dalton, Philips,

Henry and Turner preferred hydrogen = 1 as their standard, while he, Wollaston, Berzelius and "the greater number, if not the whole, of chemists on the continent" used oxygen = 1 or 10 (7). The 1818 edition of William Henry's *Elements of Experimental Chemistry* retained the original plate and explanation of Wollaston's scale in the Appendix, but used H = 1 in the text. Henry explained: "To reduce them [Wollaston's weights] to the standard adopted in this work, multiply by 7.5 and divide by 10" (8). Although Andrew Ure's 1821 *Dictionary of Chemistry* praised the concept of the scale, he concluded that it "is actually better adapted to the hydrogen unit than to the oxygen" (9). He also observed that such a scale would agree with Davy's system of proportions and Gay Lussac's theory of gaseous combination. Gradually H = 1 became the accepted standard since it avoided most fractional values. By 1830 most textbooks used the hydrogen standard.

Other chemical slide rules were also marketed in England. An improved version was published by David B. Reid in Edinburgh in 1826. It was issued with a 40-page pamphlet which discussed the concepts of definite proportions, gave examples for using the rule, and printed a long table of equivalent weights that could be used with the scale. This scale used H = 1 as the standard, but explained that the numbered slide did not start at 1 because the logarithmic distance would require a much longer slide. When hydrogen was used in a calculation, the slide was set at $10H = 10$ and then the answer was divided by ten. Similarly, carbon used $2C = 12$ (the accepted atomic weight of C at this time was six) and divided the answer by two. The Wollaston rule had also used this technique, but with different numbers. The dimensions of Reid's slide rule are not given in the pamphlet and it is not known if any examples still exist (10).

In 1841 William T. Brande, whose textbook used H = 1 as standard, recommended a chemical equivalent slide rule and accompanying table "made by Newman, No. 122 Regent Street, and Palmer, 60 Newgate Street" (11):

The form of this [Wollaston] instrument, which I recommend to the student, is a box-wood scale, about two feet two inches long, consisting of a movable slider with a logometric [sic] line of numbers upon it, and a corresponding series of numbers upon the rule itself; upon the rule the simple substances are also arranged, each opposite to its respective equivalent ... to avoid perplexing the scale with a multiplicity of terms, a separate table accompanies it, containing a copious list of compound equivalents.

The "Newman" in question was probably John Newman, who was the official instrument maker for the Royal Institution, where Brande was Professor of Chemistry, and the co-inventor of the Newman-Brooke oxyhydrogen blowpipe described by Ross in a recent issue of the *Bulletin* (26). It is not known if this slide rule was the same as the Reid instrument or whether any copies still exist.

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Wollaston's chemical slide rule as it appeared in his 1814 paper. This plate was reproduced in several introductory texts in the first half of the 19th century.

British-made slide rules were imported to the United States, but the Beck-Henry slide rule was the first to be manufactured in America. Produced in Albany, NY in 1827, it also used $H = 1$ as the standard. Fortunately some details of its development have survived.

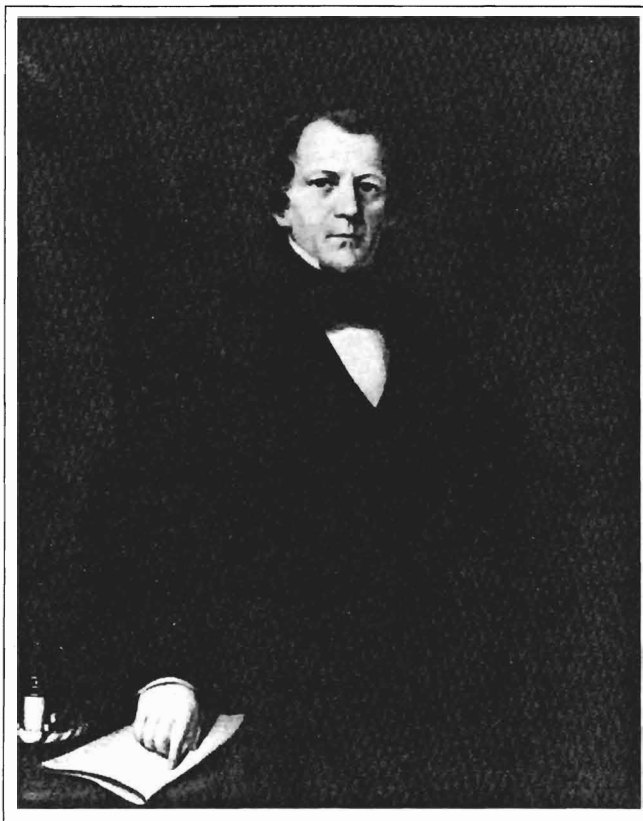
Joseph Henry (1797-1878) is noted for his contributions to electromagnetism and magnetic induction. He was immortalized when the present-day unit of inductance was named in his honor. As the first Secretary of the Smithsonian Institution, Henry established policies that resulted in the Institution's evolution into its present status as a world-renowned organization. Though his wide interests encompassed all sciences, he chose, in his own words, to confine his personal research efforts "to a course of study and investigation intermediate to pure Mathematics on the one hand and the more detailed parts of Chemistry on the other" (12).

From 1819 to 1822, Henry attended the Albany Academy, where he excelled in mathematics and science. He studied chemistry under T. Romeyn Beck using the 1816 edition of Samuel Parkes' *Chemical Catechism*, and performed so well that he was hired as Beck's lecture assistant in chemistry for the following academic year (1823-24). He then worked at various surveying and teaching jobs, including private tutor for the children of Stephen van Rensselaer, the wealthy patron of science. In 1826 Henry returned to the Albany Academy as Professor of Mathematics and Natural Philosophy. During the following six years he conducted many of his electrical experiments. Before he began that area of research, however, he and Lewis C. Beck collaborated in producing their own "Scale of Chemical Equivalents".

Lewis Caleb Beck (1798-1853) graduated from Union College in 1817 and obtained a medical license in 1818. He practiced medicine in Schenectady, St. Louis, and Albany before turning to teaching as a career in 1824. He taught chemistry and other sciences at Berkshire Medical Institution and Rensselaer Polytechnic Institute and was at Vermont Academy of Medicine in 1827 when work on the Scale of Equivalents began. He had undoubtedly met Joseph Henry while visiting his brother, T. Romeyn Beck, Principal of the Albany Academy and Henry's mentor in chemistry.

Henry may have gotten the idea of making the scale when he visited John Torrey at West Point in June 1826. He noted in his journal that "Dr. Torrey intends making one of Wallostons [sic] scales of chemical equivalents & to use Mr. Thompson's new atomic [sic] numbers" (13). Beck or Henry may have seen the newer Reid slide rule mentioned above, since a copy of Reid's *Directions* pamphlet can be found among Henry's surviving papers. The quotations below, however, strongly suggest that Henry and Beck struggled through their own design problems.

It is not clear who suggested the idea of manufacturing the scale, but Beck, in his autobiography, explained the division of labor and some of the complications (14):



Lewis Caleb Beck (1798-1853)

During this year (1827) Mr. Joseph Henry, then a Professor in the Albany Academy (now Secretary of the Smithsonian Institution) & myself, published "A Scale of Chemical Equivalents," which we constructed under the direction of Dr. Wollaston, the celebrated English Chemist. Our scale was nearly twice the length of Wollaston's & contained the names of more than double the number of substances. It was also more neatly made.

Mr. Henry arranged the divisions on the slider & furnished the account of the Mathematical Construction. I arranged all substances according to their atomic weight, & made the entire copy for the engraver, a work which of course required great accuracy.

The scale was engraved on Copper at considerable expense. One great difficulty in the way of accuracy of the Scale, was in attaching the printed part to the wood work, in such a manner that the exact ratios should be preserved in every position of the slider. The scale was at length engraved on bank paper & put on by Mr. Wilson, a globe maker, who was accustomed to this kind of work. Still many of them were, upon trial, found to be inaccurate & these were laid aside.

There were published first some 6 or 8 dozen of these Scales, & being considerably in demand, we prepared a Second edition. In this several new substances were introduced. But Wilson had, I think died in the mean time & we were obliged to employ another person to complete the work. He did not succeed very well - he, indeed, had no idea of the accuracy required, & the result was, that many of them were

useless. Out of about 150, perhaps not more than 100 were fit for use.

From the expense & trouble which attended the construction of these scales, they were necessarily sold at a considerable price. I scarcely know whether the sales were sufficient to meet expenses, but this did not at that time enter into our calculation.

Unlike previous chemical slide rules, the Beck-Henry rule placed a printed "Description and Use" paper pasted on the back of the rule. Evidently Henry was overseeing the manufacturing process in Albany. In a letter of 15 April 1827, Beck sent Henry a suggested explanation to be put on the back of the scale. He continued (15):

You can add another illustration of its utility in analysis. One mistake I have noticed. Sulphate of Potash should be 88, I believe instead of 89 ... Look at this if you please, & correct the mistake, if it is one. By all means sign our names to the above "guide board" or any other which you may adopt.

The instrument was on the market at least as early as 11 September 1827, when Henry wrote a dealer (16):

I send by the bearer Mr. Robinson one Dozen Chemical scales. We cannot afford [to sell] them for less than one dollar & twenty five Cts



Joseph Henry (1797-1878)

a piece by the wholesale. You can therefore fix your price accordingly.

On 21 September 1827, Henry wrote Beck (17):

As to the scale I have sent one doz. to Philadelphia and half a doz. to Dr. Hadley, also left one at Webster's and another at Dr. Meggs'; the woodwork of 1-1/2 doz more is completed and before they are varnished I will correct iodine with a pen. Finally the copy right [sic] which you concur in thinking necessary shall be secured.

Further errors were mentioned in a letter from Henry to John Torrey on 4 October 1827 (18):

I am much obliged to you for your notice of the errors on the scale. Sulphur according to Thompson should have been 125 instead of 152 as it is on the scale. Silica was taken from Brand's tables (32) but I agree with you in thinking that silicon would have been better. These errors shall be corrected before any more impressions are taken from the plate and in the mean time should you discover any more will you be so good as to give us further notice.

The completed slide rule measured 18.5 by 2.75 inches and the slide scale was graduated from 8 to 330 (The scale began with oxygen, which on the H = 1 scale had at the time an accepted atomic weight of eight). Only one of these instruments is known to survive, a "second edition", dated 1828, at Transylvania University, Lexington, KY as reported in the article by Bodner mentioned earlier (1). Silliman's *American Journal of Science* reported both "editions" of the slide rule, noting that it was (19):

... strongly recommended by the adoption of hydrogen as unity ... we are happy to see it thus brought within reach of all students of chemistry in this country. It is justly observed that it is founded on the most important fact in the science, namely, that all bodies unite, chemically, in weights, or multiples of weights, that have the same constant ratio to each other.

Beck also made passing reference to the scale in his own 1831 text, *A Manual of Chemistry* (20):

The greatest number of chemists call hydrogen unity and therefore oxygen 8. This is much the most simple, and has been adopted in the scale of equivalents constructed by Professor Henry and myself.

The absence of further correspondence about the scale in the Henry papers suggests that the project ended in 1828. Both men continued their distinguished careers. Henry was Professor of Natural Philosophy at Princeton from 1832 to 1846 and director of the newly organized Smithsonian Institution from 1846 until his death in 1878. Beck continued to teach chemistry until his death in 1853, holding professorships at Vermont

Academy of Medicine, Rutgers, New York University, Middlebury College, Fairfield Medical College, Castleton Medical Academy and Albany Medical College.

Yet a second chemical slide rule was also marketed in the United States. This was announced in *Silliman's Journal* in 1829 (21):

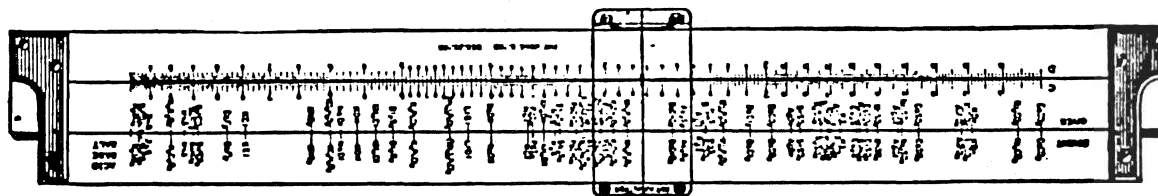
Messrs. Hedge & Co. of that place [Middletown, Conn.] have just commenced the manufacture of Wollaston's Scales. The one before us, says a writer in the *American Sentinel*, is the most finished specimen of workmanship of the kind we have ever yet seen; and the first attempt in box wood, to our knowledge, in this country. The Scale is 21 inches in length, by 3 and 2-10ths in breadth. The graduation is done by machinery, and is executed with a degree of beauty and accuracy we have never seen equaled ... Great care has been taken in the plan of arrangement of the chemical substances. The elementary bodies, metals, and metallic oxides, are arranged on one side of the

alternate substances that would contain the same quantity of active ingredient (22).

The popularity of the chemical slide rule, at least in conjunction with the teaching of elementary chemistry, seems to have waned by the second half of the 19th century. This was certainly implied by the comments of Benjamin Silliman, Jr. on the Beck-Henry scale in his 1874 survey of the history of chemistry in America (23):

While in Albany with Dr. Beck, he [Henry] devised and published an improved form of Wollaston's sliding scale of chemical equivalents, in which hydrogen was adopted as the radix, a contrivance which is hardly known, even by name, to the present generation of chemists.

However, it was revived again in the period 1910-1950 in the form of the "Ashley Chemist's Slide Rule". This was designed for the use of analytical chemists involved in repetitive routine



The "Ashley Chemist's Slide Rule", circa 1914

slide, by themselves - the names of the metals are printed in larger type ... greatest care has been taken in consulting the latest tables of Drs. Henry, Thomson, and others so as to correct the errors of former tables.

The scale, thus improved, was made at the suggestion and under the superintendence of Doct. J. Barratt ... The apparatus for effecting the graduations, is of a novel and ingenious kind, and is the invention of Mr. Hedge ...

There are none manufactured in this country or elsewhere, that can compare with them, either in cheapness, in style of finish, in the number of subdivisions, or in accuracy of graduations ...

To the best of my knowledge no example of the Barratt-Hedge slide rule has survived.

A novel circular slide rule for equivalents appeared in the 1828 American edition of J. A. Paris's *Pharmacologia*. The plate on the inside of the front cover of this volume shows a circular logarithmic scale graduated in grains. In the center is an inner rotatable disk, 4.25 inch in diameter, marked with the names of common medicinals. Labeled the "Medicinal Dynamometer and Scale of Equivalents", this circular slide rule allowed the user to calculate the quantity of active matter contained in any given weight of a medicinal or the weight of

analytical procedures and carried the usual C, D, and CI scales of a regular slide rule for multiplication, division and reciprocals, as well as two scales of gauge points, marked by chemical formulas, corresponding to the molecular weights of the common reagents and precipitates used in gravimetric and volumetric analysis (24). A quick survey of laboratory supply catalogs strongly suggest that the chemical slide rule did not survive beyond the 1950s (25). But, if per chance, it did, there is little doubt that, like the mechanical slide rule in general, it did not succeed in weathering the onslaught of the portable electronic calculator in the 1970s.

References and Notes

Acknowledgment: The author would like to thank Dr. William B. Jensen for bringing the Greenaway booklet in reference 4, the Faraday book in reference 6, and the materials in references 5, 24, 25 and 26 to his attention, as well as for the information in the final paragraph of this article and on the identification of Henry's *Syllabus* in reference 12.

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10. D. Reid, *Directions for Using the Improved Sliding Scale of Chemical Equivalents; with a short Explanation of the Doctrine of Definite Proportions*, Maclachlan and Stewart, Edinburgh, 1831, 40 pp. (First edition was 1826, Edinburgh, publisher unknown.) This pamphlet was included as Part II of Reid's *Elements of Practical Chemistry...*, Maclachlan and Stewart, Edinburgh, 1830, pp. 408-442.

11. W. Brande, *A Manual of Chemistry*, John W. Parker, London, 1841, Vol. I, p. 237.

12. N. Reingold, *The Papers of Joseph Henry*, Smithsonian Institution Press, Washington, DC, 1972, Vol. 1, p. 435. Henry's other activities in chemistry were minor. In 1832 he collaborated with T. R. Beck and Lewis C. Beck in a public lecture course in chemistry at Albany, but his contribution emphasized physics while Lewis Beck taught the chemistry. In 1833 he taught John Torrey's summer chemistry course at Princeton while Torrey was in Europe. The *National Union Catalog* contains an erroneous listing, citing Joseph Henry as the author of *Syllabus of a Course of Lectures on Chemistry* (40pp., no date, no place, no publisher). This imprint was actually the work of William Henry. A comparison of the *Syllabus* with Henry's

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18. *Ibid.*, p. 199.

19. [B. Silliman], "Intelligence and Miscellanies," *Am. J. Sci.*, **1828**, *14*, 202-203; **1830**, *18*, 385. The quotation is from the 1830 citation.

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25. Thus, for example, the 1914, 1929 and 1937 catalogs for the E. H. Sargent Co. of Chicago all carried the Ashley chemical slide rule, but it was no longer offered in the 1967 catalog. It was also listed in the 1919 catalog of the Scientific Materials Co. of Pittsburgh, the 1928 catalog of the Wilkens-Anderson Co. of Chicago, and the 1936 catalog of the Eimer and Amend Co. of New York, but it is missing from the 1912 Eimer and Amend catalog and the 1967 catalog for Fisher Scientific of Pittsburgh. The 1936 Eimer and Amend catalog also carried the "Nestler chemical slide rule".

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NOTICE: If any of our readers know of other locations of additional examples of any of the chemical slide rules mentioned in this article, both the author and the editor would appreciate hearing from you.

THE CHEMICAL NEWS, 1859-1932

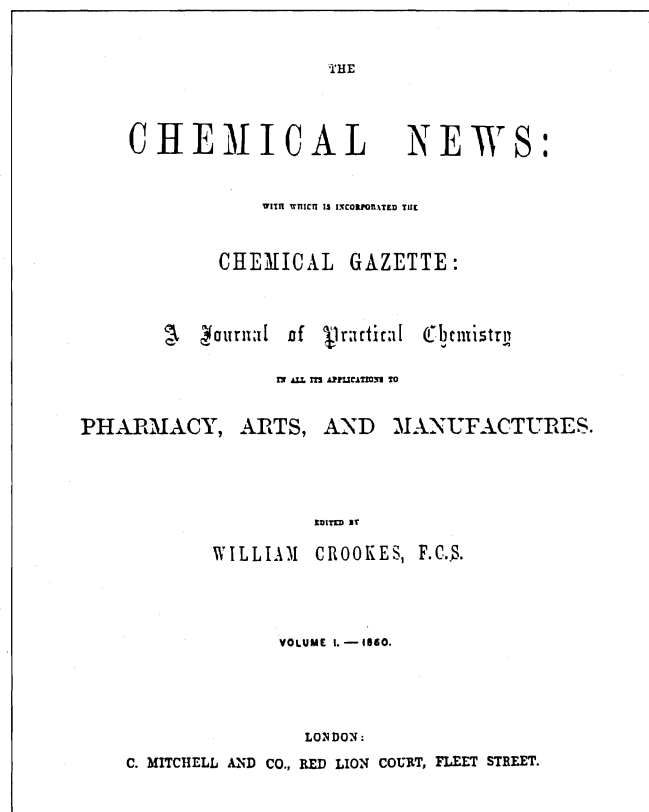
William H. Brock, University of Leicester

The Chemical News and Journal of Physical Science (With Which is Incorporated the Chemical Gazette). A Journal of Practical Chemistry in All Its Applications to Pharmacy, Arts and Manufactures, was first published on Saturday, 10 December 1859, price 3d. What was distinctly new about the periodical was its weekly character and, as such, together with the weekly general organ *Scientific Opinion* (founded November 1868, collapsed 1870), it paved the way for Norman Lockyer's *Nature*, which began in November 1869, exactly ten years later. The last issue of *Chemical News*, the 1781st issue, appeared in October 1932, thus completing almost 70 years of continuous service to the chemical community.

As the subtitle, "with which is incorporated the *Chemical Gazette*" implies, *Chemical News* was not the first commercial chemical journal in English. Commercial journals, published for the profit of editors and proprietors, are to be distinguished from the publications of chemical societies like the Chemical Society of London which had issued irregular *Memoirs* and *Proceedings* between 1841 and 1848, and which since 1848 had issued a regular *Quarterly Journal* printed by London's leading science printer, Richard Taylor.

Unlike the French and Germans with the 18th-century established periodicals, Crells' *Chemisches Journal* (f. 1778) and Lavoisier's *Annales de chimie* (f. 1789), Britain had, apart from an ephemeral artisan's journal, *The Chemist* (2 Vols. 1824-5, edited by Thomas Hodgkin), no special chemical journal until the mysterious Charles and James Watt launched the monthly medically- and pharmaceutically-oriented *Chemist; or Reporter of Chemical Discoveries and Improvements* in January 1840. With some tantalizing breaks in continuity, this journal lasted until 1858 (1). Until the 1840s, therefore, both British and Continental chemical news was published in general commercial journals, such as *Nicholson's Journal* (which had collapsed in 1814), Thomson's *Annals of Philosophy* (which collapsed in 1826) and in the long-surviving *Philosophical Magazine*, which since 1822 had been owned, published, printed and edited by Richard Taylor.

The Watts' *Chemist*, which seems to have been aimed at a readership of pharmacists, chemical analysts, chemical manufacturers and inventors (2), was almost immediately challenged by *The Annals of Chymistry and Pharmacy* (1842-3) (3), but, unable to compete with Watts, this closed in February 1843. A greater challenge was Jacob Bell's *Pharmaceutical Journal* (f. 1841), which quickly became the house organ of the Pharmaceutical Society which was established in 1841. Both these journals were opposed in their turn from 1846 by an offshoot of the *Medical Times* (1839-85) called the *Pharmaceutical Times*, which was edited by the genial Bohemian journalist, Gustav Strauss. The *Medical Times* itself had been



founded by T. P. Healey to criticize Wakley's *Lancet* (4). Not surprisingly, therefore, like so many Victorian commercial medical journals, *The Chemist*, *Pharmaceutical Journal* and *Pharmaceutical Times* were extremely quarrelsome organs and served more as factional weapons of propaganda than as journals containing serious chemical, medical and pharmaceutical information. They make entertaining reading and are important sources for the social historian of science; but for serious chemistry, the historian, like the 19th-century chemist, must look to Taylor's *Philosophical Magazine* and its sister publication, *The Chemical Gazette*.

With the tremendous expansion of organic chemistry in the 1830s, and with William Francis, Taylor's illegitimate son, who was educated at Berlin and with Liebig at Giessen, producing large numbers of translated chemistry papers for the journal, *Philosophical Magazine* simply could not cope. Hence, in 1842, Francis, together with Henry Croft (who soon migrated to Toronto to be its university's first Professor of Chemistry), founded the serious and important bimonthly, *Chemical Gazette*. This was published and printed by Taylor.

The *Chemical Gazette, or Journal of Practical Chemistry in all its Applications to Pharmacy* was very much the 25-year old Francis's pride and joy. Unfortunately, on his father's death in December 1858, Francis found, on inheriting the printing house, that he simply could not cope with editing three high-quality science journals like the *Chemical Gazette*, the

Philosophical Magazine and the equally important *Annals of Natural History*, as well as running a printing house which printed 90% of the transactions of London's learned societies together with the examination papers of the University of London and the textbooks of many large London publishing houses. And so it came about that in November 1859, Francis sold the copyright of the *Chemical Gazette* to the 27-year old William Crookes, whom he would have known through their mutual interest in photography and their joint membership in the convivial "B Club", composed largely of Liebig's and Hofmann's former British pupils.

Crookes (1832-1919) was, like Norman Lockyer, the founder-editor of *Nature*, both a scientist of considerable originality and a journalist with sound commercial sense. Like Lockyer, he never held an academic appointment (his one attempt to exploit his editorship of *Chemical News* to obtain a Chair at the Royal Veterinary College in 1860 was unsuccessful), so his journalism, analytical consultancy work, and many commercial ventures were his only means of support.

During the mid-1850s, when the technical development of photography was almost entirely in the hands of chemists, Crookes had served as editor of a number of photographic journals in Liverpool and London. Indeed, an agreement with the new weekly *Photographic News* in 1858 to give them first refusal of any articles he wrote on photography for two years, meant that Crookes was initially prevented from covering photography personally in *Chemical News*. Otherwise his target audience, including the medical profession, was pretty universal (5):

While Chemistry, in all its various branches, Scientific and Analytical, Technical, or in its relations to Agriculture will ... form the principal subject treated of, the Medical Profession will not fail to find recorded in its pages, every new discovery relating to Toxicology, Materia Medica and Pharmacy.

On purchasing the copyright of the *Chemical Gazette* from Francis and arranging for Taylor and Francis to print his first issue, Crookes bought space in *The Athenaeum* to announce his intentions (6):

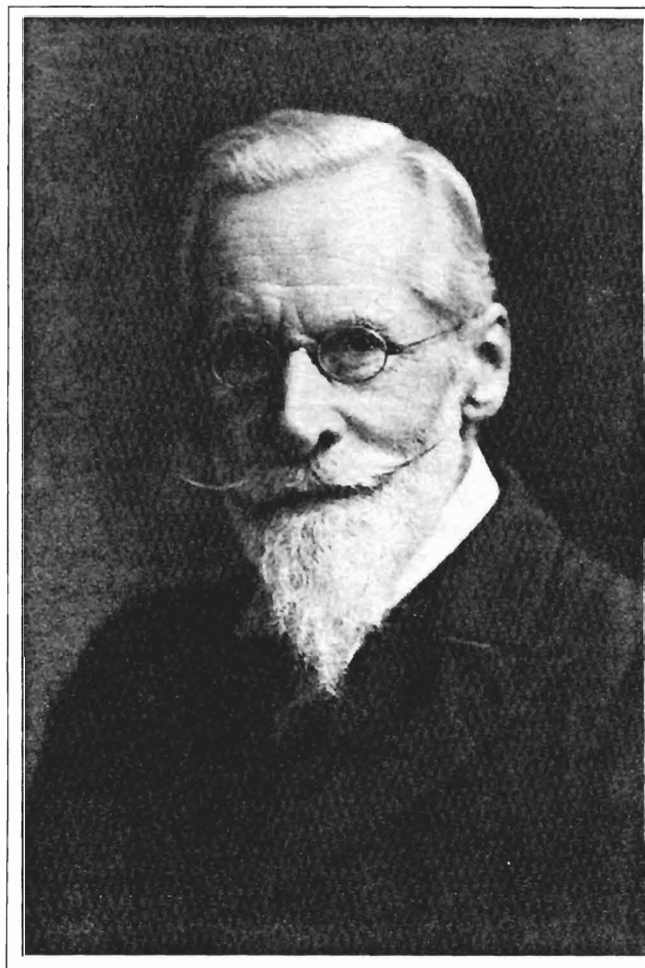
A New Scientific Periodical on Saturday next, the 10th December, 1859, Will be Published, price Three pence, No. 1 of

THE CHEMICAL NEWS

A Weekly Journal Devoted to Every Branch of Chemical Science and an Advocate of the interest of the Chemist & Chemical Manufacture (6).

Barring the failed mechanics' *Chemist* of 1824-1825, all previous chemical journals had been monthlies. Could Crookes find enough chemical news to sustain 24 columns of small type

per week (i.e., 12 pages of double columns)? He evidently had little difficulty and, in fact, one of the characteristics of *Chemical News* was its trick of serialization, which was both necessary for variety of coverage and to sustain interest from week to week. We may read Faraday, Frankland, Hofmann, Odling, Pasteur, Wurtz and, of course, Crookes himself, in parts like a Dickens novel. To these serials Crookes added the customary reports of scientific meetings at the Chemical Society, Pharmaceutical Society and other societies, frequently giving information of discussions, which are not available elsewhere, from his own shorthand notes. There were also abstracts of articles from foreign journals, the coverage of French literature being particularly strong because of Crookes' friendship with L'abbé Moigno, editor of *Cosmos*. Although Crookes tried to remain neutral editorially, correspondence columns were often lively and fueled by fractional differences over, for example, membership of the Chemical Society or, in the 1890s, the issue of Dewar versus Ramsay as President of the Society. The journal also noticed books and listed chemical patents.



William Crookes (1832-1919)

Once this balance had been found, Crookes made little adjustment to the weekly formula. *Chemical News* shows virtually no change until 1920. In particular, he continued the *Chemical Gazette*'s practice of including reports on toxicology, materia medica and pharmacy, saying that these features were specifically aimed at the medical reader (7).

By the end of the first year (1860), some financial details published by Crookes' biographer, Fournier D'Albe, show that *Chemical News* was just about breaking even, with a profit of little over £1 per week on weekly sales of 870 copies (8). Clearly, to make a half-yearly profit of £50, of £100 per annum, Crookes needed to double his sales. With this in mind, in January 1861, Crookes raised the price to 4d (where it stayed until 1920), and went into partnership with the publishers Griffin & Bohn and the printers, Reid and Pardon, the agreement being that he should be paid three and one-half guineas per week. This partnership did not work out, and after a complicated series of financial maneuvers and squabbling with his partners, Crookes found himself sole owner again by the end of 1862. We can judge that this was a difficult period for the journal financially, for although Faraday had allowed Crookes to print his Christmas Lectures on the Candle in 1861, in 1862 Crookes had to decline Frankland's Royal Institution Juvenile Lectures (9):

I should in the first place have to pay the shorthand writer 2 guineas for taking down each lecture. The wood engravings with which the course should be illustrated (to make it uniform with the previous ones by Drs. Faraday and Tyndall) would not average less than 20/- each Lecture. If you corrected the Proofs, I should have to pay the printer just double for composition, as they find it cheaper to reset the lectures in type from your altered proof than to alter the original type. That was on the former occasion an additional expense of 10/- to 40/- each. Lastly, there would be your own honorarium, which for four and one-half pages each lecture would be £14-5s. For the course of six (lectures) I should therefore have to pay

Shorthand writer	12 - 12 - 0
Engraver	6 - 0 - 0
Extra composition	10 - 10 - 0
Dr. F.	14 - 5 - 0
	£ 43 - 7 - 0

which would be more than double the amount the three previous Xmas courses of Lectures have cost each.

However, after this financial hump at the beginning of the 1860s, and despite some electrifying competition from John Cargill Brough's *Laboratory* in 1867, which forced Crookes to improve the quality of his paper and type, and the pirating of copy by an American *Chemical News* issued in New York (1867-70), he was eventually able to make a financial success of it. Indeed, by 1900, according to D'Albe, Crookes was

making an income of £400 per year, and already by 1869 Crookes spoke of a sales run of 10,000 copies per week, including America. Proudly issuing its six monthly index, Crookes called his journal the "*Jahresberichte* of Chemistry" (10).

The New York edition of *Chemical News* is interesting since it led to the establishment of America's first independent chemical journal. The reprint edition had included a small supplementary insert entitled "The American Chemist". Crookes refused to do a deal with its publishers, Charles F. and William H. Chandler, and insisted that their republication should cease. He then made his own arrangements to have *Chemical News* distributed throughout the American continent. Not put out, the Chandlers launched the *American Chemist* as an independent monthly publication. Charles Chandler (1836-1925) was an important figure in the foundation of the American Chemical Society (ACS) in 1876. Given his publishing activities, it was natural that the new society's early transactions and proceedings should appear in the *American Chemist* which, however, Chandler abandoned in 1877 when the ACS began its own *Proceedings* (and from 1879, its own journal as well).

Although the formula of the contents changed little until the 1920s, one can, of course, detect shifts in emphasis. Crookes himself noted how (11):

... in the early numbers of *Chemical News* many semi-popular expositions of scientific subjects were included, so that chemistry and physics might become less of a mystery to numbers who readily would have devoted themselves to the study of science, but whom circumstances debarred from following their true bent.

Like the important weekly *English Mechanic*, which had begun in 1865, therefore, Crookes found a market amongst the intelligent artisan movement for whom the Department of Science and Art were providing classes and an examination system, and, of course, their teachers. Since this audience was interested in textbooks, they were regularly reviewed, often very critically. For example, the text of John Buckminster, who worked tirelessly for the Department of Science and Art, was dismissed as full of "bad grammar, confused statements, bad chemistry and false chemistry" (12). Texts by Frankland, Hofmann and Williamson, on the other hand, received high praise. Given this new student audience, it can be no accident that the discussions of "quantivalence", that is valency, found in the *English Mechanic* in the mid-1860s are also found in *Chemical News*, where "valence" and "valency" are first used from 1866 (13). The importance of education is perhaps best seen in the student issues which Crookes began annually from 1863 (an idea which he took from *The Lancet*) (14). This is a most useful source of historical information concerning the way chemistry teaching slowly transformed from private to institutional teaching and for its demonstration of the continu-

ing importance of hospital posts for chemists.

In sum, Crookes brilliantly overcame the (15):

... difficulty of catering at once for the purely scientific reader, the practical chemist, and a still larger class of reader who, with but imperfect knowledge of the science, [sought] only for information which may be turned to account commercially.

A monographic treatment of *Chemical News* would have to discuss other changes of tone and the changing focus of interest in food adulteration, water analysis, chemical professionalization and debates over the endowment of research. What though, briefly, of its scientific content? There was, of course, fairly full coverage of the developments in chemistry: the historian can watch the way in which bracket type formulae continued in use until the 1870s - the first graphic or structural formula did not appear until 1871 and the benzene hexagon not until 1879 - and the way in which Crookes aided the standardization of nomenclature by adopting the Chemical Society's recommendations from 1880 onwards.

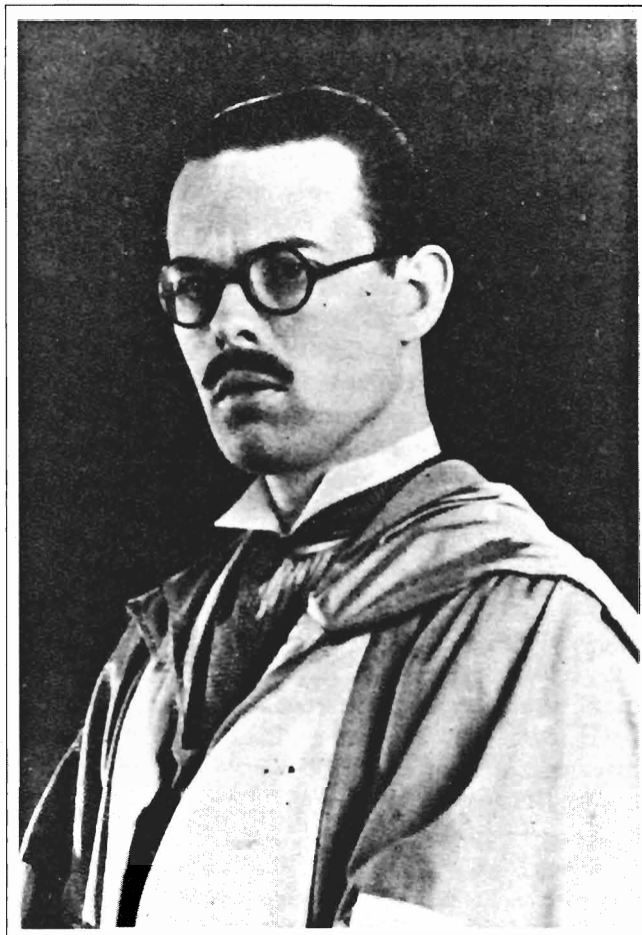
However, given that the journal was edited by William Crookes, the journal was an important vehicle for what may be termed "metachemistry". There is a strong interest in the nature of the elements and their periodicity. Historians of chemistry recall that Newland's speculative papers on "the law of octaves" first appeared in *Chemical News*, as did many other speculative papers on the evolution of elements and on Prout's hypothesis (16). And when Crookes' interests shifted to radioactivity, the journal provided an important vehicle for research on radiochemistry. Much speculative nonsense was published by Crookes on these subjects, but his aim was obviously a good one - to encourage ideas about the ultimate nature of matter. This tradition continued after his death in 1919 with contributions during the 1920s from the school-teacher, F. H. Loring.

If metachemistry was the journal's most obvious feature as far as pure science was concerned, the journal is also interesting and significant as a source for the social history of chemistry. A few random examples may be chosen.

* The absence of a pension system or benevolent fund for destitute chemists led to frequent appeals to readers to help the widows and children of minor chemical figures; for example, for the Chemical Society's librarian and editor, Henry Watts, and for the former editor of *The Chemist and Druggist and Laboratory*, John Cargill Brough.

* Correspondence in the 1860s often drew attention to the scandal of purchased degrees from German universities. Similarly, readers were warned of chemical confidence tricksters who pretended to be the pupils or friends of eminent German teachers and who, by this means, borrowed or stole money from their English pupils.

* The journal reveals the extent to which A. W. Hofmann was revered in Britain, following his return to Berlin in 1865.



H. C. Blood Ryan

In 1870, considerable publicity was given to Hofmann's appeal to British chemists for supplies of disinfectants to help in the Franco-Prussian war.

* It is a measure of the importance of analytical chemistry to Victorian chemists that advertisements for training courses at the Wiesbaden school of analytical chemistry run by Fresenius and his sons appeared regularly in the journal.

* *Chemical News* also reveals the continuing survival of alchemical speculations and aspirations. In 1865, there were rises in the price of bismuth which were attributed to a revival of alchemical experimentation. Although Crookes himself dismissed the possibility of alchemical transmutation, as befitted a metachemist, he was obviously interested in Stephen H. Emmens' pressure process for the transmutation of Mexican silver coins into gold, which received great publicity in 1897. Emmens was even allowed to advertise in the journal.

What caused this once thriving journal to collapse in 1932? By 1902 Crookes was 70 years old, and although he oversaw the journal until the day he died in 1919, from about 1906 onwards, the effective editor was Crookes' private laboratory

assistant, James H. Gardiner, who had become his research assistant when Charles Gillingham left in 1882. Gardiner became full editor on Crookes' death, until 1924, when he was succeeded by John F. G. Druce (1894-1950), chemistry master at Battersea Grammar School, and one of the last schoolteachers to do original chemistry research. Druce was also a pioneer historian of science and narrowly missed the discovery of rhenium in 1925.

Druce was probably sole editor from 1924 until 1930. During this period, *Chemical News* became much more of a secondary channel of chemical communication than a primary research journal. Although Gardiner had already changed the subtitle to reflect the fact that the journal no longer spoke for pharmacy and was more a "journal of theoretical and practical chemistry and physics in their application to engineering and manufactures", the competition from the monthly *Chemical Age* (f. 1919) and the Society of Chemical Industry's weekly *Chemistry and Industry Journal* from 1923 on, effectively shut off the receipt of primary industrial articles, as well as articles from the scintillating pen of the prolific Henry Edward Armstrong.

Nevertheless, the journal might still have survived as a general weekly review of science (along the lines of the monthly *Discovery*, founded in 1920) but for the disastrous appointment of a new editor in 1930. He was the splendidly named and honored:

H. C. Blood Ryan, M.A., D.Sc., LL.D., Vice President and Hon. Foreign Secretary of the European Branch of the Muslim Association for the Advancement of Science, Patron and Fellow of the British Radio Institution, Chairman of the National Institute of Criminology, Director of Research of the College of Pestology, etc.

He was also the founder of the misleadingly-titled Faculty of International Science in Gordon Square in the heart of the University of London.

Who exactly Ryan was is difficult to discover, though he is known to students of Nazi Germany as a "fellow traveler of the right" who translated Göring's speeches and who blamed Hitler's rise to power on Franz von Papen. He was a journalist in the 1930s and early 1940s (17). His degrees were clearly phony and he was exposed in *Nature* during the week that the final unusual 32-page issue (instead of 16 pages) of *Chemical News*, dated October 1932, appeared.

The previous issue, Friday 23 September 1932, had announced on its cover:

Owing to the complete reorganization of the policy of *Chemical News* this journal will be issued MONTHLY until further notice, the next issue being dated October 1932.

This October issue carried no printed message of demise and no reference to the scandal surrounding its editor. However, a

typed message tipped between the pages (a rare example is preserved in the Royal Institution's copy) announced:

This is the only issue which will appear monthly. Publication as a weekly will resume on 25 November No. 3782. The strong Editorial Board of this journal will continue to function as hitherto.

It was not to be. On 25 October 1932, *Chemical News, Ltd.* was wound up. Dr. Blood Ryan had led the Company into bankruptcy (18). In this ignoble manner ended one of the great international organs of chemical communication.

References and Notes

1. The second and third series, volumes 1-4 (1849-53), and volumes 1-6 (1854-58) were subtitled *A Monthly Journal ... of Chemistry Applied to the Arts ... and Medicine*.
2. "Advertisement", *The Chemist*, **1840**, 1, up.
3. Weekly for volume 1, then monthly for volume 2, Nos. 1-2 only.
4. Healey first issued a pharmaceutical supplement, (*Pharmaceutical Repository*) to the *Medical Times* on 31 May 1846. This was split off as *The Pharmaceutical Times. A Journal of Chemistry Applied to the Arts, Agriculture and Manufactures*, 5 September 1846-4 November 1848. It was then renamed *The Chemical Times and Journal of Pharmacy, Manufactures, Agriculture and the Industrial Arts*. Vols. 4-5, 11 November 1848-17 November 1849. The *Medical Times* merged with the *London Medical Gazette* in 1862 to form the *Medical Times and Gazette*.
5. Anon., "The Chemical News", *Chem. News*, **1861**, 3, 1-2.
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7. Reference 5.
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9. *Ibid.*, pp. 80-81.
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12. Anon., "Notices of Books", *Chem. News*, **1866**, 14, 176.
13. The first valence headline was 1871. See discussion in C. A. Russell, *A History of Valency*, Leicester University Press, Leicester, 1971, pp. 83-89.
14. Anon., "Address to Students", *Chem. News*, **1863**, 8, 147, 152-7, 165-6 and annually every September.
15. Anon., "To Our Readers", *Chem. News.*, **1863**, 8, 303.
16. See D. M. Knight's facsimile reprints of articles from the *Chemical News* in *Classical Scientific Papers, Chemistry, Second Series*, Mills & Boon, London, 1970.
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Drummond, London, 1943.

18. J. F. G. Druce, "The Chemical News", *J. Soc. Chem. Industry*, 1932 (16 December), 1031. Also Anon., "A Self-styled Faculty of Science", *Nature*, 1932, 130, 603-4; and "College and Faculty Dissociations", *ibid.*, 1932, 130, 692-3.

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OLD CHEMISTRIES

A "Lost" Silliman Chemistry Text

William D. Williams, Harding University

Published in 1866, *Principles of Modern Chemistry* by Benjamin Silliman, Jr. and George F. Barker, has gone unnoticed by chemical historians (1). This extremely rare text was the first American work to present the "new chemistry" of the 1860s. An examination of the volume reveals it to be a revision of the first section of Silliman's famous text, *First Principles of Chemistry*.

Silliman and Barker each authored a well-known chemistry text. Silliman's *First Principles of Chemistry* (2) went through 50 "editions" and sold 50,000 copies between 1847 and 1875 (3). Barker's *A Text-Book of Elementary Chemistry, Theoretical and Inorganic* (4) had 13 "editions" from 1870 to 1891, sold 10,000 copies the first five years, and was translated into Japanese, Arabic and French (5). Despite the prominence of these two authors, however, their co-authored text has remained "lost" (6). The following review examines this rare volume and its relationship to the two authors' well-known texts.

Modern Chemistry was published by Theodore Bliss and Co. of Philadelphia - the same publisher as Silliman's *First Principles*. Although it never mentioned *First Principles* by name, the preface of *Modern Chemistry* left no doubt that it was intended as a revision of Silliman's text. It explained that the section on physics, "which has heretofore occupied the first portion of the work", was being omitted (7). Part I of Silliman's *First Principles* was indeed titled "Physics". The preface to *Modern Chemistry* also spoke of "earlier editions" and of T. Sterry Hunt's organic section (8):

It is appropriate that the atomic system should appear in this book, since many of its leading principles have been taught in its pages for

PRINCIPLES
OF
MODERN CHEMISTRY.

ARRANGED
ON THE
ATOMIC SYSTEM OF NOTATION.

BY
BENJAMIN SILLIMAN, M.A., M.D.,
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AND
GEORGE F BARKER, M.D.,
INSTRUCTOR IN PRACTICAL CHEMISTRY IN THE YALE MEDICAL INSTITUTION.

PART I.
CHEMICAL PHILOSOPHY.

PHILADELPHIA:
THEODORE BLISS & CO.
1866

the last fourteen years. Professor T. Sterry Hunt, the author of the Organic Chemistry in the earlier editions, was one of the first laborers in this field ... while the progress of fourteen years has changed the general aspect of chemistry, the student will recognize in these pages many of the principles laid down in former editions.

Subtitled, "Part I. Chemical Philosophy", the 100 pages of *Modern Chemistry* were to replace the briefer and outdated, "Part II. Chemical Philosophy" section of Silliman's *First Principles*. The prime purpose of the new edition was the introduction of the "atomic system of notation" into inorganic chemistry as had already been done with organic. The old dualistic system of expressing inorganic formulas ($\text{CaO} \cdot \text{SO}_3$) was to be replaced by the simplest atomic formula (CaSO_4). Classification by the "theory of types" and the resulting system of "equivalence" were recommended as less complex and more easily remembered by introductory students.

The "new chemistry" of the 1860s was a reform of equal importance to Lavoisier's "new chemistry" of the 1790s. During the first half of the 19th century, facts and theories accumulated faster than they could be systematized. Earlier combining atomic weights were inconsistent with Avogadro's law of equal volumes and the Dulong and Petit relationship for

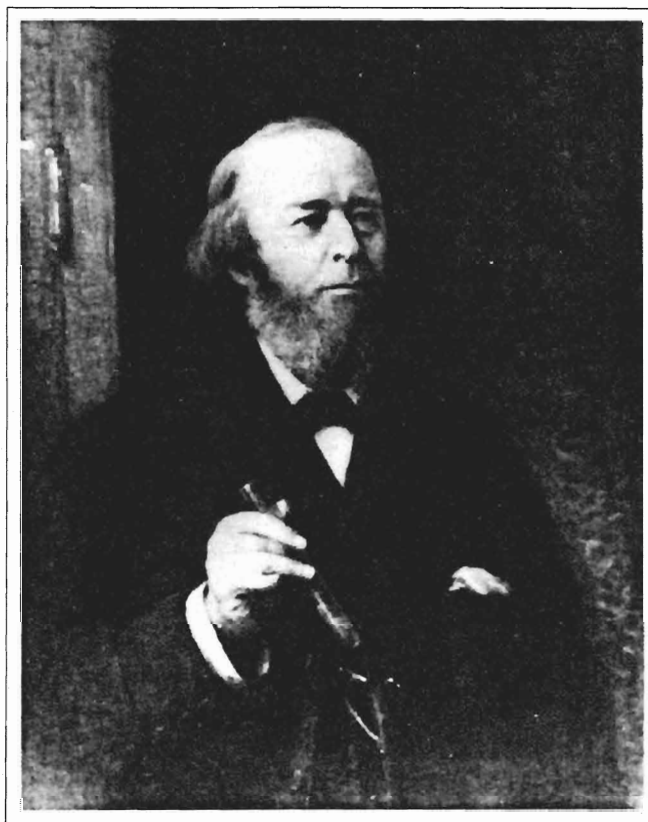
atomic heats. It was recognized that the accepted formula for water, HO (with H = 1 and O = 8), should be replaced with H₂O (with H = 1 and O = 16). In an effort to clarify inconsistent atomic weights and chemical notation, a conference of chemists was called at Karlsruhe, Germany, in 1860. About 140 prominent chemists attended. With Dumas presiding, they sought to correlate the contributions of Gay-Lussac, Avogadro, Berzelius, Dumas, Dulong and Petit, Gerhardt and others. Cannizzaro championed Avogadro's hypothesis that equal volumes of gases at the same temperature and pressure contain the same number of molecules. He argued that contradictions could be overcome by considering elemental gas molecules to be diatomic and by doubling some atomic weights. Although the delegates did not pass a resolution, Cannizzaro's recommendations were soon accepted (9).

The resulting new chemistry involved several important reforms:

- * Some atomic weights were doubled.
- * Dualistic formulas were replaced by molecular formulas. Compounds like CO₂ and SO₂ had formerly been called acids and had led to the dualistic concept of a salt as an additive combination of a base with an acid - i.e., CaO•CO₂ or CaO•SO₃.
- * A new nomenclature was needed for the new formulas. Thus carbonate of lime (CaO•CO₂) in the dualistic system became calcium carbonate (CaCO₃) in the new system and sulfurous acid (SO₂) became simply sulfur dioxide.
- * Concepts of valence and types resulted from the new organization.

These changes soon appeared in European textbooks and were widely accepted by 1865. In spite of its limited use, Silliman and Barker's *Modern Chemistry* was the first American text to explain the new system. Though several existing American texts added appendix chapters on the new system and some adopted the new atomic weights, Barker's 1870 *Text-Book* was the first widely-used American text totally based on the new system (10).

Benjamin Silliman, Jr. (1816-1885) followed the scientific path of his celebrated father, Benjamin Silliman, Sr. (1779-1864), who was the first Professor of Chemistry at Yale (11). The younger Silliman served as his father's assistant both before and after his graduation from Yale in 1837. He also joined his father in editing the *American Journal of Science and Arts*, a responsibility he continued for his lifetime. In 1842 he began an applied chemistry school that ultimately became Yale's Sheffield Scientific School, an accomplishment one biographer described as "the most important achievement of his life" (12). Except for a period from 1849 to 1853, when he was Professor of Medical Chemistry at the University of Louisville, Silliman spent his entire career at Yale. When his father retired in 1853, the younger Silliman succeeded him, teaching classes in the Academic Department and the Medical School, as well as in the Scientific School. He also became increasingly involved in industrial consulting. His 1855 report



Benjamin Silliman Jr. (1816-1885)

on Pennsylvania oil played an important role in the growth of the eastern petroleum industry. In 1864 he visited California for field investigation of mining interests. His optimistic report on California oil potential contradicted conclusions of Josiah D. Whitney, who headed the California State Geological Survey. Whitney and his former assistant, William H. Brewer, by then a Yale professor, mounted a vicious attack on Silliman that continued until 1874 (13).

The first edition of Silliman's *First Principles of Chemistry* was published in 1847. Although it had 50 so-called "editions", there were really only three different revisions - the content and pagination changing in 1848 (2nd ed.) and in 1852 (25th ed.). Around 1856, Silliman supplemented his text with two editions of a syllabus, *Synopsis of Lectures on Chemistry Delivered in Yale College*. In 1859, he wrote of desires for a major revision (14):

The Chemistry I mean to make a compound [sic] of the excellences of Fourier, Stockhardt and my own book. If the mixture is adroitly made I shall be able to concoct a better book for students than now exists.

George F. Barker (1835-1910) was a student under Silliman at the Yale (later Sheffield) Scientific School in 1856-58

and acted as Silliman's assistant the final year (15). After graduation, he served two years as assistant to John Bacon, Professor of Chemistry at the Harvard Medical School. He received his M.D. degree from Albany Medical College in 1863 while at the same time serving as professor of chemistry in that institution. After spending a year as Professor of Natural Science at Western University of Pennsylvania, he returned to Yale in 1865 as Demonstrator of Chemistry in the Yale Medical School. The 1866-67 Yale College catalog listed Barker as "Assistant to Professor Silliman".

From February 1867 until February 1868, while Silliman made another trip to California, Barker taught Silliman's chemistry classes at Yale (16). Barker apparently used *Principles of Modern Chemistry* as a supplement to Silliman's text. The copy in the author's possession contains a student's handwritten margin note, "Feb. 20th 1867". Some margin notes were trimmed when the volume was later bound. This would suggest that the original issue had a paper cover. The extreme rarity of the imprint could be explained by the fact that it was used for only one Yale class.

A contemporary biographer gave Barker credit for the book (17):

Early in the spring of 1866 Professor Barker wrote the first part of a text-book, intended as a new edition of Silliman's "Chemistry." In this book, the modern nomenclature and notation appeared in a text-book for the first time in this country. The theory of types was made use of as a basis of classification, and the book was used with the senior class in Yale College.

Any plans to continue the revision of Silliman's *First Principles* were abandoned. Barker incorporated the new chemistry concepts into his own text, brought out in 1870. Silliman's text continued to be published until 1875 with no changes from the 1852 copyright. As a result of continued attacks by Whitney and Brewer, Silliman resigned from Yale College and the Sheffield Scientific School in 1870. He retained his positions with the Medical School and the *American Journal of Science and Arts* until his death. Barker was Professor of Physiological Chemistry and Toxicology in the Yale Medical School from 1867 until 1873, when he moved to the University of Pennsylvania as Professor of Physics. His subsequent career was as physics professor and researcher, industrial consultant and expert court witness.

Silliman, in his 1874 *American Contributions to Chemistry*, omitted any mention of *Modern Chemistry* under his own name or under Barker's name. He did, however, insert an interesting comment about his *First Principles* in which he claimed that "the fundamental ideas of the so-called 'New Chemistry' were first distinctly brought out in a text-book in the organic portion prepared by Dr. Hunt" (18). This claim was very similar to the sentence about Hunt in the preface of *Modern Chemistry* quoted above.

Modern Chemistry contained five chapters:

- I. Introduction (matter, force, province of chemistry, laws of chemism, composition of matter)
- II. Nomenclature (simple and compound radicals, binary and ternary compounds, basicity of acids)
- III. Combining weight (law of combination, atomic and molecular weights, equivalence of radicals)
- IV. Notation (symbols of radicals and compounds, theory of types, reactions, stoichiometry)
- V. Chemical Physics (cohesion, crystallography)

Although the preface stated that "we have placed a series of problems at the end of such sections as require them", no problems were present in the volume. Indeed, chapter heads were continued on the same page as the last line of the preceding chapter in such a manner as to suggest printing from unfinished galley type. The 45 figures were all in the discussion of crystallography.

A comparison of *Modern Chemistry* with Silliman's *First Principles* and Barker's *Text-book* reveals very little similarity with either. The format of numbered paragraphs and the degree of complexity were similar to Silliman's text, but no identical passages have been found. *Modern Chemistry* was



George F. Barker (1835-1910)

much more detailed than the brief sections on theory and crystals in *First Principles*. Barker's *Text-book* had a more elementary, outline format and also failed to exhibit any identical passages with *Modern Chemistry*. A full-page "Electro-chemical Series" was identical, but the discussions were not the same.

It is not clear how Silliman and Barker shared the authorship of *Modern Chemistry*. The preface used the plurals "our" and "we" in referring to the authors. At that time, however, Silliman was heavily involved in the California oil controversy and other consulting work. Since Barker was more interested in the new chemistry, he probably did most of the writing - a method quite common with junior authors of today's texts.

References and Notes

1. B. Silliman and G. F. Barker, *Principles of Modern Chemistry arranged on the Atomic System of Notation. Part I. Chemical Philosophy*, Theodore Bliss & Co., Philadelphia, 1866, 100 pages. The *National Union Catalog* lists only one holder, Columbia University. The author has obtained a microfilm of that volume. Louis Kuslan, a knowledgeable Silliman biographer, states that he has seen an uncataloged copy in the Yale archives (personal communication).
2. B. Silliman, Jr., *First Principles of Chemistry for the Use of Colleges and Schools*, H. C. Peck & Theodore Bliss, Philadelphia, 1850, 480 pages.
3. B. Silliman, *American Contributions to Chemistry*, Collins, Philadelphia, 1874, p. 96.
4. G. F. Barker, *A Text-Book of Elementary Chemistry, Theoretical and Inorganic*, C. C. Chatfield & Co., New Haven, 1870, 342 pages.
5. W. Miles, *American Chemists and Chemical Engineers*, American Chemical Society, Washington, D.C., 1976, pp. 23-24.
6. The title is not listed in H. C. Bolton's *A Select Bibliography of Chemistry* or other bibliographies. It is not among any list of publications in Silliman biographies. The only source located which does mention the title is E. Thomson, "George Frederick Barker", *Proc. Am. Phil. Soc.*, **1911**, *50*, xxiv, in a list of publications by Barker.
7. Reference 1, p. vi.
8. *Ibid.*, p. [v]. Fourteen years earlier would have been 1852, which agrees with Hunt's last revision of the organic section for Silliman's *First Principles*.
9. For details of the Karlsruhe congress, see: A. J. Ihde, "The Karlsruhe Congress: A Centennial Retrospect", *J. Chem. Educ.*, **1961**, *38*, 83-86; A. Findley, *A Hundred Years of Chemistry*, Duckworth and Co., London, 1948, pp. 40-45; A. J. Rocke, *Chemical Atomism in the Nineteenth Century*, Ohio State, Columbus, OH, 1984, Chap. 10; and C. De Milt, "Carl Weltzien and the Congress at Karlsruhe", *Chymia*, **1948**, *1*, 153-169.
10. C. Eliot and F. Storer's *A Manual of Inorganic Chemistry* (Printed for the Authors, Boston, 1867) used the new system, but retained some of the dualistic formulas; J. P. Cooke incorporated the new system in writing his *First Principles of Chemical Philosophy* (Welch, Bigelow, and Co., Cambridge, 1868); J. Porter's *Principles of Chemistry* (A. S. Barnes, New York, 20th edition, 1868) added a 77-page section at the end of his former edition. He explained in the preface: "To meet the demand for a text-book containing the later theories of Chemical Philosophy, the publishers have extended the former edition so as to embrace a new part giving a complete exposition of the modern theories, together with numerous exercises in the new nomenclature."; H. E. Roscoe's *Lessons in Elementary Chemistry* (Wm. Wood, New York, 1868), the first American edition of the 1867 British work, used the new system throughout; S. W. Johnson published a five-page pamphlet, *Chemical Notation and Nomenclature, Old and New ...* (John Wiley & Son, New York, 1871), which was to accompany his edited American editions of Fresenius' texts; J. Johnston's *Manual of Chemistry* (Charles Desilver, Philadelphia, 8th edition, 1871) placed a 12-page appendix on the new system in his unchanged former edition; J. D. Steele's *Fourteen Weeks in Chemistry* (A. S. Barnes, New York, 1868) was revised to contain the new chemistry in 1873, but the old version was also continued for several years.
11. Reference 5, pp. 438-440; reference 14, pp. 159-205; E. H. Thomson in C. C. Gillespie, ed., *Dictionary of Scientific Biography*, Vol. 12, Scribner's, New York, 1975, pp. 434-437.
12. J. P. Cooke, "Benjamin Silliman", *Proc. Am. Acad. Arts Sci.*, **1885**, *20*, 523.
13. For a discussion of the oil controversy, see: G. T. White, *Scientists in Conflict: The Beginnings of the Oil Industry in California*, Huntington Library, San Marino, 1968. Whitney and Brewer claimed that Silliman was compromising scientific precision for lucrative consulting fees. When an oil sample proved to be fraudulently "salted" with eastern oil, the attack upon Silliman was intensified. Several Silliman biographers have concluded that he was too trusting of his associates and that his brother-in-law may have been responsible for the deceit.
14. Quoted in L. I. Kuslan, "Benjamin Silliman, Jr.: The Second Silliman", in L. G. Wilson, *Benjamin Silliman and His Circle*, Science History Publications, New York, 1979, p. 179.
15. Reference 5, p. 23; Thomson, reference 6, pp. xiii-xxix; Anonymous, "Biographical Notice of George F. Barker", *Pop. Sci. Monthly*, **1879**, *15*, 693-697; E. F. Smith, "George Frederick Barker", *Am. Jour. Sci.*, **1910**, *180*, 225-232; Anonymous, "George Frederick Barker", *Sci. Am.*, **1887**, *57*, 231-232.
16. Silliman's 1867 California trip is widely documented. He left in time to be in California by early March and in August he wrote Yale President Woolsey that he would not be able to be back for the fall term and asked that Barker take his classes. (Reference 13, pp. 131 and 135.)
17. Reference 15, *Pop. Sci. Monthly*, p. 694.
18. Reference 3, p. 96.

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BOOK NOTES

A History of the International Chemical Industry. Fred Aftalion (Translated by Otto Theodor Benfey), University of Pennsylvania Press, Philadelphia, PA, 1991. 435 pp. Cloth (Typeset), \$39.95. Paper, \$14.95.

This volume, the latest in the Chemical Sciences in Society Series of the Beckman Center, is a welcome attempt to fill a serious void. Few books have been written on the history of the chemical industry, and the majority of these are now out of print. For an enterprise which has played such an important role in the development of modern society, this lack of books is surprising.

Spitz, in his 1988 book, *Petrochemicals*, dealt with the complexity of this topic by providing an overview, then covering only a few representative episodes in depth. Aftalion not only provides a broad analysis of the industry but also reviews hundreds of specific companies and products. Despite the diversity and number of topics covered, this is a very readable book, which is an indication of the quality of both the original text and the translation. The narrative is presented in historical sequence, interspersed with numerous short sections on the main products, new processes, and company reorganizations during each period.

The first industrial chemicals were mainly inorganic substances used in the production of soap, glass, textiles, or fertilizers. Soon, better trained chemists and more systematic methods resulted in improved production processes and synthetic substitutes for some of these basic materials. The author points out that by the beginning of World War I, Germany had significant advantages in chemistry over both England and France. In Germany, the strong demand for organic chemicals to make synthetic dyes had encouraged industrial expansion; there was a good working relationship between universities and chemical companies, and adequate funding was available for research. Thus, the chemical industry was important for the war effort of all of these countries but was especially so for Germany.

After World War I, cartels and pricing agreements discouraged international competition. In Germany, the United States, and England, consolidation produced giant corporations, such as I. G. Farben, Du Pont, and ICI, but the lack of such mergers in France left its chemical companies small and fragmented. Here, as throughout the book, Aftalion describes not only the activity in these major industrial nations, but also in countries such as Belgium, Japan, Italy, and the Soviet Union, whose contributions are sometimes neglected.

The international chemical industry played a key role in World War II. Synthetic rubber, catalytic cracking to obtain aviation fuel, synthetic polymers, and large-scale production of penicillin are only a few of the vital contributions that Aftalion discusses. This was also the period during which the

U.S. solidified its position as the world leader in petrochemicals. By the war's end, this country had established a lead in the chemical industry which lasted over 25 years.

The 1973 oil embargo and the ensuing higher cost for petroleum feedstocks were a severe blow to a business that already suffered from over-capacity for many major products. Aftalion tells about this period of intensive reassessment and reorganization from the perspective of someone who has been personally involved with the French chemical industry from 1951 to the present. He evaluates many of the developments during this time, ranging from the effects of leveraged buyouts in the U.S. to the problems that large multinationals encountered in trying to emphasize specialty chemicals. His analysis of current issues is a particular strength of the book.

Unfortunately, this book lacks the structure that historians of chemistry would normally expect. No references are provided, nor is there a general bibliography. Two helpful indices include names of individuals and companies, but the lack of a general index will be an inconvenience to readers who wish to review events related to a specific country or product. These omissions are especially regrettable in an otherwise good book.

This is a well-written, comprehensive work in a field where too few resources are available. It should be valuable to chemists and others who wish to learn more about the history of the chemical industry. Historians of science will be particularly sensitive to its shortcomings but may still find it to be useful. *Harry E. Pence, Chemistry Department, SUNY-Oneonta, Oneonta, NY 18320.*

Chemistry. Hélène Metzger (Translated by Colette V. Michael), Locust Hill Press, West Cornwall, CT, 1991. xv +151 pp. Cloth (Typeset), \$25.00.

This book is an English translation of a short history of chemistry written by the French historian Hélène Metzger in 1926 and first published in 1930 as part of the French historical series *Histoire du monde*. The translator has added additional explanatory notes and an index and has supplemented Metzger's original bibliography and biographical appendix (as well as dispersing the latter throughout the text in the form of footnotes). Aaron Ihde of the University of Wisconsin has also provided a new foreward.

While I am delighted to have this work available in English, there are, in my opinion, a number of problems connected with both the mechanics and motivation for this translation. The first and most serious of these is the decision of the translator, Colette Michael, to opt for "a literal translation in order to keep the style conveyed by the author". Despite her claim that changes were made whenever "the rendering in English became so awkward that it detracted from the meaning", the resulting English syntax does exactly that at every turn. Elabo-

rate run-on sentences separated by endless semicolons, unusual verb tenses, uncommon word choices, and curious inversions of word order abound on every page, not to mention incomplete and incorrect translations.

As an example of the incomplete category, one encounters on page 129 the sentence:

With these speculations, and these and similar experiments, chemists were thus invited to orient themselves on the road indicated by Berthollet's static but which nobody had thought to travel.

Whatever the original French, there is no way to render this into clear English with out expanding the word "static" into "static approach to chemical equilibrium". Likewise, on page 60 we read, concerning the use of the pneumatic trough, that:

To prevent these gases from escaping into the atmosphere, the experimenter collected them in a vessel he plugged with care as soon as it was blown ...

Presumably this refers to the inflation of gas bladders but instead suggests the quaint image of a glass blower standing at the collection end of a pneumatic trough and providing freshly blown bottles which are then sealed prior to collecting the gas.

Examples of the mistranslation category are even more numerous: on page 8 we are informed that in the 17th century reagents "were of all shapes" (rather than the containers in which they were stored); on page 10 we are told that 17th century chemists were concerned with the alkalis "sodium and potassium" (rather than soda and potash), whereas on page 92 we are told that the symbol K stands for potash (rather than potassium); on page 16 alchemy is "swept out of the science of mechanical philosophy" (rather than swept out of science by mechanical philosophy); on page 46 affinity tables allow chemists to "predict what would happen if certain reagents were mixed for the first time", but apparently not for the second or third time (rather than allowing chemists to predict, for the first time, what would happen when reagents were mixed); on page 69 Lavoisier explains combustibility by "invoking the chemical structure of the combustible substances themselves" (rather than their composition); on page 111 the discovery of scandium, gallium and germanium is "prophetic of Mendeleev's table" (rather than the table being prophetic of the elements); on page 121 the unitary theory becomes the advocate of the idea that all compounds are the result of addition reactions (rather than substitutions); on page 130 the Scottish chemist Thomas Thomson becomes William Prout's "collaborator" (rather than corroborating Prout's hypothesis); and on page 136 J. W. Gibbs become the author of several "phase rules" (rather than "the" phase law).

The sensation that one is reading the instructions to a Japanese VCR, rather than an important work on the history of chemistry, is further reinforced by a variety of spelling and

typographic errors. For example, Thomsen is spelled Tomson on page 130, Kolbe is spelled Kalbe on page 123, benzene is spelled benzine on page 124, and the number 0 is used as symbol for oxygen on pages 107 and 108. Indeed, Tomson is dutifully reproduced in the index, whereas both Kalbe and Kolbe are missing, as is the term "electrochemical dualism", despite the fact that it is the subject of an entire section of the text. In short, Metzger would have been better served by a freer translation and some decent editing. A popular history is, after all, not a work of literature, and clarity rather than style must surely be the prime objective.

Even more curious, however, is the motivation for this translation. Metzger wrote any number of works that virtually all historians of chemistry would agree deserve to be translated. Most notable are her *Les doctrines chimique en France du debut du XVIIe siecle à fin du XVIIIe siecle* (1923) and her *Newton, Stahl, Boerhaave et la doctrine chimique* (1930). Even her doctoral thesis, *La genèse de la science des cristaux* (1918), and her volume on the philosophy of science, *Les concepts scientifiques* (1926), are not without current interest. Given all this, why was this particular book, which is surely her weakest and least important work, chosen instead? The answer perhaps lies in the observation that this translation forms Volume 1 of a projected series entitled "Women in the Sciences" and that the choice had more to do with length and the interests of an audience totally unrelated to historians of chemistry - but in these halcyon days of political correctness one belabors the obvious only at great risk.

Suffice to say that as a history of chemistry this work is of questionable importance. Originally intended for a lay audience, the treatment is extremely brief and impressionistic, and most of the quotations are not properly referenced. The most satisfactory portion of the book is Part I, which is largely a summary of Metzger's more detailed works on 17th and 18th century chemistry. Here, despite the brevity and awkward English, the brilliance of Metzger's interpretive genius manages to shine through, though I question whether this would be apparent to a layman without any previous background in the history of chemistry. If anything, these flashes of brilliance merely underscore the desirability of having these more substantive works properly translated. Part II of the book, which deals with 19th century chemistry and which terminates around 1870, is much less satisfactory. It quickly degenerates into a list of names, most of which are French (indeed, the entire book is decidedly biased toward the contributions of French chemists) and into vague generalized statements about theoretical trends. The treatment of 20th century chemistry is, of course, nonexistent.

Having said all of this, I must now confess to a great sense of guilt. Translations of works in the history of chemistry are so rare these days that one hates to discourage any attempt, however ill-conceived. This is especially true of the work of French historians, where not only Metzger's important books

still await the pen of the translator, but also several by Pierre Duhem, including his 1902 study of chemical composition, *Le mixte et la combinaison chimique*, and his 1893 history of chemical thermodynamics, *Introduction a la mécanique chimique*. William B. Jensen, Department of Chemistry, University of Cincinnati, Cincinnati, OH 45221.

The Historical Development of Chemical Concepts. Roman Mierzecki, Kluwer Academic Publishers, Dordrecht, and Polish Scientific Publishers, Warsaw, 1991. xi + 281 pp. Cloth (Typeset), \$129.00.

This book is an English translation of a Polish history of chemistry first published in 1985 and now appearing as Volume 12 of Kluwer's history of chemistry series, "Chemists and Chemistry". The text is organized around the history of selected topics, rather than being a continuous chronological account, and is supplemented with five tables summarizing significant dates and events.

The book is divided into five chapters, the titles of which somewhat imperfectly reflect their contents: 1. "Division of the History of Science" (i.e., a survey of significant historical periods); 2. "The Element" (i.e., the evolution of the concept of a chemical element); 3. "The Elementary Particle of Matter" (i.e., the evolution of the atomic-molecular theory); 4. "The Structure of Chemical Compounds"; and 5. "Capacity of a Substance for Transformation" (i.e., the history of chemical kinetics and thermodynamics). Despite the awkward chapter headings, the quality of the English translation is generally good, though the quality of both the typography and illustrations leaves something to be desired. As implied by the chapter headings, the book is directed primarily at chemists, rather than historians of science, and tends to focus on the internal evolution of chemical concepts, rather than on external sociological and cultural factors.

Unlike many recent histories of chemistry, the book makes use of extensive quotations from the primary literature, much of which the author actually appears to have read. However, I must confess that I do not always agree with his interpretations and in some cases rather curious errors have crept in. Thus, on page 179, we are told that Charles Gerhardt was an advocate of the idea that a chemical formula should represent the actual physical structure of a molecule, when in fact he was the leading advocate of exactly the opposite view.

However, the most serious problem with this otherwise interesting book is that it has been parochialized relative to the history of Polish chemistry. No less than 20 Polish chemists and alchemists appear in the index and one repeatedly comes upon statements that will raise the eyebrows of most chemists and historians outside of Eastern Europe and Russia, such as the claim that:

Wladyslaw Natanson, a professor at the Jagiellonian University in Cracow, is considered to be one of the founders of the modern thermodynamics of irreversible processes.

Likewise, 15 index entries and five continuous pages of text are devoted to the writings of the 18th-century Polish chemist, Jędrzej Sbuadeck, while Avogadro merits only four index entries and one page of text, and Irving Langmuir rates only two index entries and one sentence of text. Had this material been collected into a single chapter (or appendix) on the evolution of chemistry in Poland, it would have been a valuable contribution, but by dispersing it among the various chapters and substituting quotations from obscure Polish chemists and textbooks for the more standard and historically more significant references, the author produces a distortion which would largely preclude the use (outside of Poland, at least) of this book as a text in a standard history of chemistry course, had this fate not already been guaranteed by the outrageous price. William B. Jensen, Department of Chemistry, University of Cincinnati, Cincinnati, OH 45221.

Nineteenth-Century Attitudes: Men of Science. Sydney Ross, Kluwer Academic Publishers, Dordrecht, 1991. xi + 235. Cloth (Typeset), \$69.00.

This book is a collection of seven essays (all but one of which are based on previously published articles) dealing with various topics related to the history of natural philosophy and chemistry in the first half of the 19th century, and forms Volume 13 of Kluwer's history of chemistry series, "Chemists and Chemistry". In assembling the essays for republication, Ross has incorporated numerous revisions and has added an epilogue and index. In addition, all of the essays have been reset in uniform type and are well illustrated with portraits and diagrams. Indeed, both the typography and the quality of the illustrations are above average.

The titles of the seven essays are "Scientist: The Story of a Word"; "The Story of the Volta Potential"; "The Search for Electromagnetic Induction: 1820-31"; "Faraday Consults the Scholars: The Origin of the Terms of Electrochemistry"; "Herschel and Hypo"; "Herschel on Faraday and Science"; and "Herschel's Marginal Notes on Mill's *Liberty*". The essays are both well-written and scholarly, if somewhat anti-quarian in flavor, and the choice of subject matter appears in many cases to have been dictated by original documents, memorabilia, and books personally collected by the author over the years. Those who love old books, and especially books on science, are sure to find at least one, if not several, essays that will be of interest. William B. Jensen, Department of Chemistry, University of Cincinnati, Cincinnati, OH 45221.

A History of Lactic Acid Making: A Chapter in the History of Biotechnology. H. Benninga, Kluwer Academic Publishers, Dordrecht, 1990. xxi + 478 pp. Cloth (Typeset), \$79.00.

This book, which forms Volume 11 of Kluwer's history of chemistry series, "Chemists and Chemistry", is a detailed history of the lactic acid industry from the 18th century to the present. In order to provide an adequate technical and historical context, a sizable portion of the text is also devoted to the chemistry and bacteriology of lactic acid and to the history of chemistry in general. At times the book has a tendency to lapse into the format of a textbook/review article, and both Benninga's technical emphasis and his method of presentation will undoubtedly appeal more to chemists than to those historians who are primarily interested in the managerial, marketing and political aspects of the chemical industry.

Previous case studies of industrial chemistry have tended to focus on either heavy inorganic chemicals (e.g. soda, sulfuric acid, etc.) or on petrochemicals and dyes. Consequently Benninga's case study of lactic acid as representative of the fermentation industry is a welcome addition to the literature on the history of industrial chemistry. Indeed, the most fascinating portion of the book deals with the challenge presented by the petrochemical industry to traditional fermentation methods in the period between 1945 and 1960.

The weakest aspect of the book is the background sections dealing with the general history of chemistry. Many of these are of poor quality and questionable relevance, and much of this material could have been eliminated without damaging the book as a whole. An even more serious problem is the English, which is marginal in many places and badly in need of a good editor. On the other hand, both the typography and illustrations in the book are excellent and, from the technical point of view, at least, Kluwer is to be congratulated on having produced a quality product.

Contrasts in Scientific Style: Research Groups in the Chemical and Biochemical Sciences. Joseph S. Fruton, American Philosophical Society, Philadelphia, PA, 1990. ix + 473 pp. Cloth (Typeset), \$40.00.

Readers interested in the history of biochemistry will need no introduction to the work of Joseph Fruton. Beginning with his 1972 book, *Molecules and Life: Historical Essays on the Interplay of Chemistry and Biology*, and proceeding through the editions and supplements of his *Bio-bibliography for the History of the Biochemical Sciences since 1800*, and, most recently, his book of essays, *The Skeptical Biochemist*, Fruton has established himself as one of that most rare of breeds - a practicing scientist who is also an extremely well-read and sophisticated practitioner of the history and philosophy of science.

In the book under review, Fruton turns his attention to the question of the definition of research groups and styles in science, drawing his examples from 19th century chemistry and biochemistry. To what extent do the students and postdoctoral assistants of a famous scientist truly constitute a research group? What percentage of the students continue on in the same area of research as their mentor? To what extent do they study the same problems? To what extent do they diversify? To what extent do these percentages reflect the managerial style of the mentor, his ability to obtain funding, his laboratory skills, his abilities as an educator, etc.? These are but some of the questions raised by Fruton and addressed in his study of the research groups of Justus Liebig, Felix Hoppe-Seyler, Willy Kühne, Adolf von Baeyer, Emil Fischer, and Franz Hofmeister. In addition to an extensive bibliography and name index, Fruton has also included seven appendices (covering over 145 pages) giving brief biographical accounts of the students and assistants for each research group.

This is an innovative and very interesting approach to the question of the role of intellectual mentorship in science and should be of interest not only to sociologists and historians of science, but to young scientists on the verge of establishing their own research groups, or even to those all-too-numerous chemists whose historical interests run no deeper than the establishment of their own personal chemical genealogies.

Atti del III° convegno nazionale di storia e fondamenti della chimica. Ferdinando Abbri and Franco Crispini, eds., Brenner, Cosenza, 1991. 402 pp. Paper (Typeset), L. 35.000.

This volume is a collection of 32 papers presented at the 3rd National Congress on the History and Foundations of Chemistry held at the University of Calabria in 1990. All but three of the papers are in Italian and about half deal with the history of Italian chemistry. Compared to the somewhat feeble attempts to preserve the proceedings of many symposia on the history of chemistry held in the United States and elsewhere, this is an impressive publication that others might well take as a model.

Some Recollections of Gap Jumping, Sir Derek H. R. Barton. American Chemical Society, Washington, D.C., 1991, xxiv + 143 pp. Cloth (Typeset), \$24.95.

Sir Derek Barton's contribution to the J. I. Seeman series "Profiles, Pathways, and Dreams" provides a fascinating kaleidoscope of the life experiences, research interests, intellectual accomplishments, and personal philosophy of one of the key players in the "Golden Age" of organic chemistry. A primary goal of these books is to provide the reader with a clear understanding of each author's evolution as a researcher. The emphasis on evolution is particularly significant because so

much of the lastingly important science produced by the various contributors to this series is obviously more than simply the result of intelligence (or luck) in having picked the right scientific problem to investigate. Just as important is the scientist's advancing sophistication in knowledge and understanding as the research project matures. Ultimately, a collection of key insights required for a breakthrough solution must come within the investigator's grasp. From the perspective of the historian of science, as well as the working bench scientist, the three crucial elements in a major scientific advance are as follows: (1) defining exactly what is the key question that should be asked, (2) deciding how the problem could best be first approached and (3) being flexibly pragmatic in order to develop fresh, new pathways of attack if the first assault fails. Because aspects of these processes are often intense personal reflections of an individual's intellectual style, a carefully crafted discussion - high in candor, precise in its analysis, and factually accurate - can provide a powerful lesson in inspired creativity as exhibited by that particular scientist. Sir Derek's efforts to show us how he has made his own breakthroughs have left us an extremely valuable legacy.

As with many books in this series, the title of the volume is quite illuminating. By "gap jumping", Barton refers to the ability of a researcher to bring together two disparate points of view and then to bridge the gap. This juxtaposition and fusion has some of the elements of a "grand synthesis", as the phrase is used by Kuhn to describe one crucial stage in scientific revolutions. While "gap jumping" is perhaps best illustrated by Barton's melding together aspects of chemical physics and steroid reactivity in order to develop conformational analysis, the contribution that led to the 1969 Nobel Prize in Chemistry, he stresses the use of gap jumping over and over again in all of his major studies.

Barton takes great care to point out how important to a young scientist is the achievement of intellectual acceptance by the scientific elite. Each generation of scientists must pay its dues. Brilliant minds are able to compete with one another in an exciting duel of the intellect but only if they can simultaneously maintain grace, wit, honesty, and good manners. Science may be deeply competitive but there is no place for real viciousness (although an occasional umbrella-bashing may be in order). As a man obviously blessed with both extraordinary ability and tremendous self-confidence, Barton repeatedly emphasizes the importance of being able to carve out his own research niche and yet, at the same time, having to gain the respect of acknowledged leaders on the basis of his continued ability to produce work of the highest quality.

Barton appears to have pursued a relatively small number of interests but to have done so in such a way that every time he returned to an earlier topic, his creativity was able to add an innovative twist as a significant new embellishment. In addition to his lifelong interest in free-radical chemistry, he has also been keenly involved in elucidation of natural product

structures using degradative and chiroptical techniques, in natural product total synthesis, in the discovery of novel photochemical and thermal reactions, in uncovering biosynthetic pathways, and, of course, in stereochemistry.

Barton's intellectual curiosity has been constantly stimulated by the obvious one-to-one correlation of molecular properties with molecular structure. On a modern level, in exploring a reaction that is known to succeed with a simple prototype, the first stage of such an investigation would involve a systematic examination of what influence structural modifications might have on the facility of that reaction. Typically, as an assumed first approximation, one can examine separately both the electronic as well as the steric effects induced by these systematic variations. Resonance and inductive features, when taken together, often provide useful predictive information regarding electronic influences on chemical reactivity. For instance, practical organic chemistry of aromatic materials - such as the logically planned, and financially lucrative, synthesis of dyestuffs - involves application of these electronic considerations. While classical stereochemistry was one of the earlier intellectual triumphs of organic chemistry (as the work of giants such as Pasteur, LeBel, and van't Hoff attests), certain kinds of steric effects were not nearly so easily understood. In spite of the well-accepted tetrahedral carbon atom, when Barton first became an active researcher most practicing chemists were more or less satisfied by visualizing cyclohexane rings as essentially planar. While in some situations this approximation works reasonably well, to explain the detailed chemistry of the steroids, such a model was obviously unsatisfactory.

When functionalized steroid reactivities first began to be explored in the 1940s, there occurred a unique historical phase characterized by the exciting concurrence of several key elements: (1) the isolation of important, biologically active molecules, (2) the uncovering of useful, but somewhat inexplicable, chemical behavior especially in regards to product distributions, (3) the availability of new physico-chemical techniques, and (4) the presence of several unique individuals. Thus the medicinal promise of the steroids (e.g. cortisone) was of such transcendent importance that it had attracted the efforts of hundreds of organic chemists who now possessed powerful new tools (e.g., efficient column chromatography and, soon thereafter, infrared spectroscopy) to help them understand the progress of functional transformations and to purify previously intractable mixtures. Data was produced at an astounding pace. Finally, there were present on the scene several enormously talented individuals who each contributed to the unique, almost revolutionary, development of this field.

A psychohistorian could engage in an intriguing analysis of the roles played by Fieser, Woodward, and Barton. Fieser, an eminent steroid chemist with an encyclopedic grasp of factual organic chemistry, was an acquaintance of Barton's. Fieser influenced Barton's work both by devising a semi-workable

explanation for some of the observed steroid steric effects and by helping to secure for Barton a temporary post at Harvard as a sabbatical replacement for R. B. Woodward. Fieser was a traditional organic chemist whose forte was not mechanistic subtlety. On the other hand, Fieser knew a great deal of chemistry and thereby helped direct Barton's attention to many "anomalies" (e.g., gaps in understanding). Woodward, of course, was the genius who acted as a catalyst in unraveling the basis of conformational analysis. Woodward's mechanistic sophistication and clarity of vision were crucial in helping Barton to crystallize his thinking in the now classical *Experientia* paper which was the basis for his Nobel Prize. Repeatedly, Barton pays homage to the significance of Woodward in helping to gain acceptance by the chemical community of Barton's abilities. Barton even goes so far as to describe the exact process by which Woodward eventually communicated to Barton that Barton had now qualified as a trusted member of the inner circle of organic chemists. How tragic that Woodward, whose impact appears over and over in this autobiographical series, died so prematurely. Despite the fact that he is beyond contributing his own volume, he has, in effect, become a ghostly spirit hovering over the "Golden Age".

Barton's research evolution has a spectacular way of fitting together. His first major independent academic study, a systematic compilation of molecular rotation differences, gave him an opportunity to correct previous errors in the literature as well as to assign empirically new structures for a wide range of natural products. Besides indulging and challenging his lifelong curiosity about natural products, the very nature of this investigation would inevitably have sensitized Barton to the three dimensional features found in complex natural products. If, as Pasteur asserted, "Chance favors the prepared mind", then from the very beginning of his independent career Barton has been making the necessary preparations by continuously meditating about structure and stereochemistry. The intellectual challenge of trying to understand just why a particular organic structure could elicit so specific a biological response had to be endlessly appealing. Later, after Barton had developed conformational analysis, his natural product investigations moved towards those materials that might have been formed by radical coupling reactions. Here was a marvelous exercise that allowed the marriage of free-radical chemistry (the field that had formed the basis of his Ph.D. research) with structure elucidation of complex materials. Barton's biosynthetic insights, beginning with his correction of the structure of Pummerer's ketone and extending to his theory regarding the origin of the morphine alkaloids, were all logical consequences of careful, but straightforward, consideration of possible free-radical pathways. Such a feat no doubt was, in an aesthetic sense, highly satisfying to Barton. His style of combining projects that involved structure elucidation, stereochemistry, and synthetically useful free-radical chemistry is nowhere better illustrated than in his creation of a brand new approach

that made the key steroid metabolite aldosterone accessible from simple starting materials. Only an individual intimately involved in stereochemical thinking would see, in the aldosterone precursor utilized in his successful synthetic route, the possibility of a putative cyclic transition state involving an interaction through empty space of components separated by several atoms.

Much of Barton's later work has involved the direct invention of new organic reactions. Why would Barton want to "invent" synthetic transformations? Just as there is one type of knowledge that comes from the human mind's ability to participate in pattern recognition, so too, there is another kind of understanding that emerges from being able to predict reactivity.

If we can imagine a unique interaction of various reagents and intermediates that are able to accomplish a useful and non-obvious transformation, then we certainly must know something about the underlying chemistry. The collection of established organic chemical reactions did not spring into being all at once. Instead, a rich history has taken place wherein serendipity and personality played major roles. This is best illustrated by the fact that there are literally several thousand known reactions that are immortalized by the name of the individual(s) who first uncovered their novel chemistry. Before the advent of modern spectroscopic and diffraction techniques, structure elucidation by wet-chemical techniques was both an art and a science. In the application of relatively simple reagents, such as concentrated aqueous sodium hydroxide or molecular bromine, to a complex natural product, much could be learned by systematic examination of the resulting molecular debris. In large measure, the simplicity of the reagents used in the distant past often helped make the chemical behavior more clearcut since one was more likely to generate low to moderate concentrations of a single reactive intermediate that could then react with the molecule's remaining functionality. In the post-World War II period, these classic structure elucidations became much less popular. As a consequence, a major path by which totally unprecedented chemistry might be observed was in danger of extinction.

As Barton's chemical abilities increasingly matured during the 1960s and the 1970s, his attention turned more and more towards crafting new reactions by the logical analysis of intermediate generation and predictable chemical behavior. While his aldosterone synthesis is arguably the best example of such an invention, his neat approach to highly hindered alkenes via selenoketones and his long series of valuable new synthetic methodologies that employ free radicals have also had a very significant impact on organic chemistry. The rebirth of free-radical chemistry during the 1970s and 1980s is in no small measure a direct outcome of Barton's contributions. To have moved from having been a major contributor of stereochemical insights to becoming a "Thomas Edison" of new synthetic tools is merely one more indication of the breadth and dedica-

tion of this unique scientist. *John Belletire, Department of Chemistry, University of Cincinnati, Cincinnati, OH 45221.*

LETTERS

The Faraday Issue

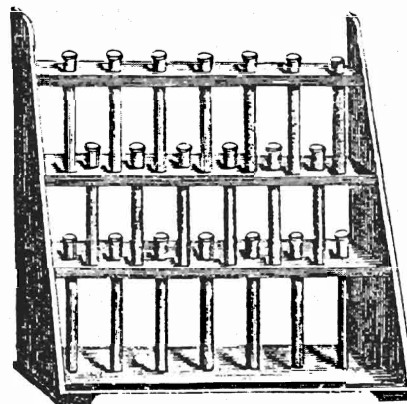
* My hearty congratulations on the Faraday number of the *Bulletin*. It's splendidly put together, a very good read and will be very useful. *William H. Brock, University of Leicester*

* In my article "Michael Faraday and the Art and Science of Chemical Manipulation" (No. 11, Winter 1991) I noted that Faraday's description of his home-made test tubes and test tube rack in his book *Chemical Manipulation* strongly suggested that "neither was commercially available at the time" and that one was "witnessing the incipient stages of their introduction" into the chemist's laboratory. Though I also added the caveat that, on the basis of this evidence, one could not "definitely assert that Faraday was the inventor of the test tube and test tube rack." Since then, Dr. Derek Davenport has donated a copy of volume 2 of the 1814 American edition of Frederic Accum's *System of Theoretical and Practical Chemistry* (Kimber and Conrad, Philadelphia) to the Oesper Collection. Figure 6 of Plate II of this volume clearly shows a test tube rack and test tubes, which Accum describes as:

... a test rack or wooden stand, containing glass tubes, for examining small portions of fluids, by the action of reagents or tests, or for dissolving small quantities of earths, or metals, by means of heat over a candle or lamp.

This plate may date back to the first London edition of 1803 or to the second London edition of 1807, and the rack and tubes were almost certainly offered for sale by Accum as part of his business as an apparatus dealer, though I am unfortunately unable to verify either of these surmises as I do not have access to earlier editions of Accum's text nor to his apparatus catalogs. Even if these earlier leads should prove dead ends, the 1814 reference cited above predates Faraday's book by over a decade. *William B. Jensen, University of Cincinnati*

* With every issue I'm increasingly impressed with the vitality, quality, and visual attractiveness of the *Bulletin*. The editorial staff is doing an outstanding job and in the process are making membership in the History of Chemistry Division a pearl of great price. You've put extraordinary value in our dues assessment. Our composition has always tended toward senior age chemists and small college academics. Neither of these groups can physically or financially turn out in great numbers at national meetings, so it has long devolved on the newsletters and bulletins to be the glue that holds us together.



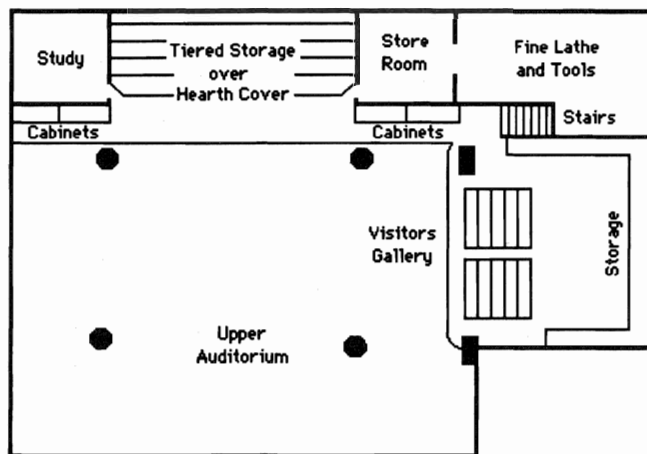
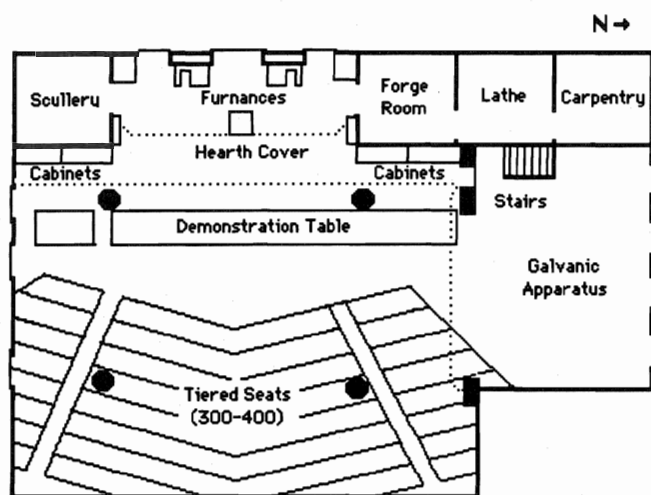
Frederic Accum's 1814 test tube rack

In your hands the *Bulletin* has become a very sticky adhesive. Congratulations! And from this long-time member ... many thanks! *Ned D. Heindel, 1993 ACS President-Elect*

Robert Hare's Lecture Hall

* I was delighted to receive Issues 10 and 11 of the *Bulletin* with the articles on Faraday and the superb historical and pictorial celebration of the art of the lecture demonstration. It's so sad to see what we once had and no longer have. An opportunity to use facilities like Robert Hare's laboratory and lecture hall at the University of Pennsylvania Medical School around 1830 (figure 10) would attract me back to teaching in an instant ... The article - especially the last paragraph - should be required reading for anyone who is involved with education in science in any way - teachers, students, parents, architects, administrators at all levels, perhaps not least of all "education presidents." *Henry A. Bent, Pittsburgh, PA*

* I enjoyed the paper on chemical lecture halls which appeared in Issue 10 of the *Bulletin* (Fall 1991). I suppose you will have recognized that figure 10 of Robert Hare's lecture hall is printed backwards. The figure used is from the second edition of Hare's *Compendium*. An earlier version, published in the *American Journal of Science*, 1830, 19, 26, did not have the electrical apparatus fastened to the front of the balcony. This earlier engraving was also used in some 1828 copies of Hare's *Compendium* (actually published or bound in 1830 as they contain a publisher's catalog dated October 1830). Both of these sources also printed a "Description of the Laboratory and Lecture Room." It is interesting to take the picture and the description and try to reconstruct the original floor plan [see figures on page 46]. The second edition of the engraving appeared in the 1836 edition of the *Compendium*. A closeup of the details of the electrical machine is given in Hare's *A Brief Exposition of the Science of Mechanical Electricity ...* (1835 and 1840). *William D. Williams, Harding University*



Dr. Williams' reconstruction of the floor plan of Robert Hare's lecture hall and laboratory at the University of Pennsylvania Medical School: (Top) ground floor, (bottom) upper floor.

AWARDS

The Dexter Award

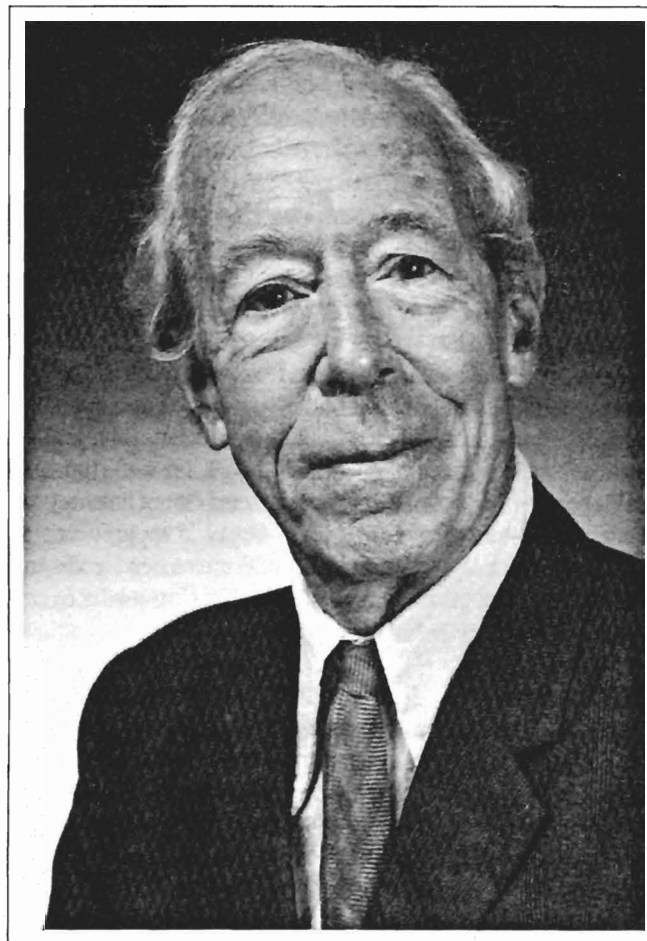
The 1992 Dexter Award for outstanding accomplishment in the history of chemistry has been awarded to Dr. John T. Stock of the University of Connecticut for his work on the preservation of early chemical instrumentation and the history of electrochemistry. The award, which consists of a cash prize of \$2000 and an engraved plaque, was presented at the Fall National ACS Meeting in Washington, DC in August.

Born in Margate, Kent, England, on 26 January 1911, Dr. Stock received a general B.Sc. from the University of London in 1936, followed by a B.Sc. in chemistry in 1941, a M.Sc. in electrochemistry in 1945, a Ph.D. in analytical chemistry in 1949 and a D.Sc. in 1965. After holding several industrial

positions, he joined the staff of Norwood Technical College in 1946. In 1956 he was appointed Associate Professor of Chemistry at the University of Connecticut, becoming full Professor in 1959 and Professor Emeritus in 1979.

Author of over 250 articles and reviews in analytical chemistry, electroanalytical chemistry, and the history of chemistry, Dr. Stock has also published two textbooks, an advanced monograph on amphoteric titrations, and several pamphlets on historical instrumentation for the London Science Museum: *The Development of the Chemical Balance* (1969) and *The Development of Instruments to Measure Electric Current* (1983). He has also co-edited, along with M. V. Orna, two historical volumes based on HIST symposia: *The History and Preservation of Chemical Instrumentation* (Reidel, 1986) and *Electrochemistry: Past and Present* (ACS Books, 1989). Dr. Stock's Dexter address will be published in Issue 14 of the *Bulletin*.

The Division would at this time like to solicit nominations for the 1994 Dexter award. Nominations should include a complete vita for the nominee, consisting of biographical data, educational background, awards and honors, publications,



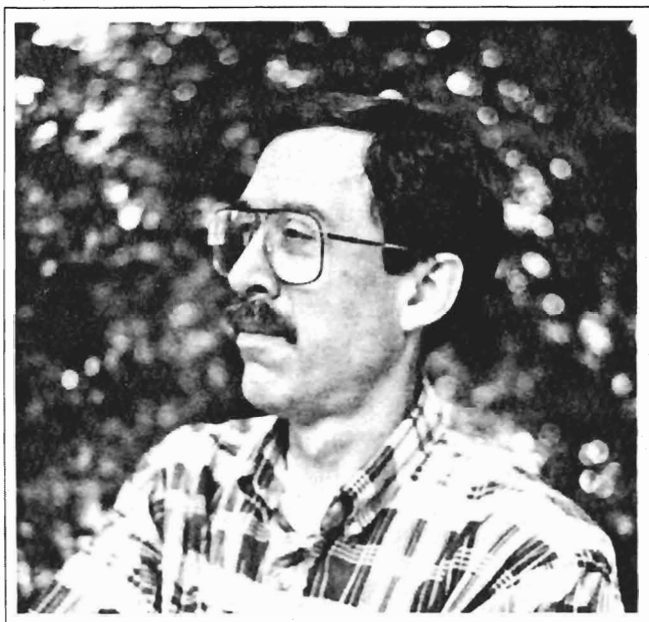
Dr. John T. Stock

presentations and other services to the profession; a nominating letter summarizing the nominee's achievements in the field of the history of chemistry and citing unique contributions which merit a major award; and at least two seconding letters. Copies of no more than three publications may also be included, if available. All nominations should be sent in triplicate to Dr. William B. Jensen, Chairman of the Dexter Award Committee, Department of Chemistry, University of Cincinnati, Cincinnati, OH, 45221 by 10 January 1994. It should be emphasized that the award is international in scope and that nominations are welcomed from all quarters. Previous winners have included historians and chemists from Germany, France, Holland, Hungary, and Great Britain.

The Outstanding Paper Award

The 1992 Outstanding Paper Award has been given to Dr. Alan J. Roche of the Department of History of Case Western Reserve University for his paper, "Between Two Stools": Kopp, Kolbe and the History of Chemistry", which appeared in the Fall 1990 (No. 7, 19-24) issue of the *Bulletin*. The award, which consists of \$100, a certificate, and \$150 worth of books from Kluwer Academic Publishers, was presented to Dr. Roche in absentia at the Fall National ACS Meeting in Washington DC.

An internationally-known authority on the history of 19th-century German organic chemistry, Dr. Roche is the author of the books *Chemical Atomism in the Nineteenth Century: From Dalton to Cannizzaro* (1984) and *The Quiet Revolution: Hermann Kolbe and the Science of Organic Chemistry*, due to be published by the University of California Press in 1993. Since



Dr. Alan J. Roche

receiving the award he has also been appointed as an Associate Editor of the *Bulletin*.

The Edelstein International Fellowship

The 1992-1993 Edelstein International Fellowship in the History of Chemical Sciences and Technology has been awarded to Dr. Kostas Gavroglu of the National Technical University of Athens. Dr. Gavroglu will divide his fellowship year between the Beckman Center for the History of Chemistry in Philadelphia and the Edelstein Center for History and Philosophy of Science, Technology, and Medicine in Jerusalem, Israel.

EVENTS OF INTEREST

* William D. Williams has prepared a catalog of the Williams - Miles Rare Science Collection. Containing over 1500 volumes published between 1600 and 1900, the collection, which has a heavy chemical emphasis, is housed in the rare book room of the Brackett Library at Harding University in Searcy, Arkansas. The collection is a combination of the personal libraries of William D. Williams and Wyndham D. Miles. Williams, Professor of Chemistry at Harding University since 1954, has been collecting old chemistry books for about 20 years. He is a regular contributor to the *Bulletin*, where he runs the "Old Chemistries" column. Miles gathered his collection of rare books over a 40-year span as a science historian for several governmental agencies in Washington, DC. Author of nearly 100 articles on the history of chemistry and winner of the 1971 Dexter Award, Miles is currently completing the second volume of his biographical dictionary, *American Chemists and Chemical Engineers*. Noted chemists are well represented in the collection. There are six volumes by Robert Boyle, 25 by Joseph Priestley, three by Lavoisier, 16 by Sir Humphry Davy, 40 by Justus Liebig, 12 by William Henry, ten by Thomas Thomson, and four by Michael Faraday. While the bulk of the titles are 19th century, there are eight 17th-century titles and 27 18th-century titles. The collection is particularly rich in early American chemistry, containing over two-thirds of all chemistry titles published in the United States prior to 1880. Many titles are present in almost all of their variant editions. There are 25 volumes by Robert Hare, 29 by John Comstock, 28 by Jane Marcet, and 14 by Edward Turner. Of special interest is the second full-sized chemistry book published in the United States: James Tytler's 1791 *System of Chemistry*. Though the primary focus of the collection is chemistry, it also contains works on other sciences, pharmacy, medicine, some early journals, and some non-science works by famous chemists. The rare volumes are supplemented by modern reference works. A small collection of antique laboratory apparatus is also on display, included hand-lettered glassware and pre-World War I chemicals from Germany. Persons interested in

a more detailed list can obtain a photocopied booklet of titles, authors and dates by sending \$3.00 to Dr. William D. Williams, Department of Chemistry, Harding University, Box 602, Searcy, AR 72143.

* Dr. William B. Jensen, Department of Chemistry, University of Cincinnati, Cincinnati, OH 45221 is trying to locate portraits of Ludwig Ferdinand Wilhelmy (1812-1864) and Leopold Pfaundler (1839-1920). Wilhelmy was a German physicist who is generally credited with being the first to quantitatively describe the kinetics of a chemical reaction using a differential equation, and Pfaundler, an Austrian physicist, was the first to theoretically rationalize the rate of a chemical reaction using the concepts of collision frequency and critical activation energy.

* The editors of the *Oxford Dictionary of Scientific Quotations* are looking for important and/or amusing quotations by chemists or about chemists and chemistry for inclusion in the forthcoming dictionary. Send your favorite examples (with proper scholarly citations) to Dr. William H. Brock, Department of History, University of Leicester, Leicester, LE1 7RH, England.

* David Trapp, a chemistry teacher at Sequim High School in Sequim, Washington, has developed a set of *Hyperstudio* stacks for the Apple IIGs computer dealing with the historical development of the periodic table. The project was an outgrowth of the Workshop on the History of Chemistry sponsored by the Beckman Center for the History of Chemistry in July of 1991. The program utilizes the features of color hypermedia to capture the history surrounding the periodic table from Greek roots to the present. Organized around time lines are key ideas, charts and tables, a few maps, and many images of the chemists. The user can move rapidly to any time period via the timeline, browse forwards or backwards between concepts, or study pictures and charts. Individuals or small groups can study history at their own pace or an entire class can view images of chemists on a television monitor. Interested persons can obtain two 3.5" disks containing the stacks by sending \$10.00 to David Trapp, 159 Holland Road, Sequim, WA 98382.

* The Royal Society of Chemistry and the American Chemical Society will sponsor the 7th BOC Priestley Conference at Bucknell University in Lewisburg, Pennsylvania on 24-27 June 1994. The theme of the conference will be *The Chemistry of the Atmosphere* and will include an historical symposium on "The History of Ozone". The conference will also include a reception and private tour of the Priestley home and laboratory in Northumberland, Pennsylvania. For further information contact the ACS Meetings Department, 1155 16th Street, NW, Washington, D.C., 20035, (202) 872-6286.

* Travel grants are available from the Beckman Center for the History of Chemistry to enable interested individuals to visit Philadelphia to make use of the Othmer Library, the Edgar Fahs Smith Collection, and other associated facilities. The

grants, which may be used for travel, subsistence, and copying costs, will not normally exceed \$500. Applications should include a vita, a one-paragraph statement on the research proposed, a budget, and the addresses and telephone numbers of two references. Deadlines are 1 February for grants covering the period April-June, 1 May for July-September, 1 August for the period October-December, and 1 November for the period January-March. Send applications to Lisa Kazanjian, The Chemical Heritage Foundation, 3401 Walnut Street, Suite 460B, Philadelphia, PA 19104-6228, (215) 898-4896.

IN MEMORIAM

Raymond Benedict Seymour (1912-1991)

Raymond B. Seymour, Distinguished Professor of Polymer Science at the University of Southern Mississippi and an active member of the ACS Division of the History of Chemistry, died of melanoma on 15 November 1991 at Hattiesburg, Mississippi. Born in Brookline, Massachusetts, on 26 July 1912, Seymour held a number of responsible positions in both industry and academe before coming to Mississippi in 1976 after his retirement as Professor of Chemistry at the University of Houston. The author of more than 40 books, more than 2000 articles, and the holder of more than 45 U.S. patents and more than 150 foreign patents, he received numerous local, national, and international awards and was in great demand as a speaker and symposium chairman. During his later years, his interest turned to biography and the history of chemistry and of polymer science; in his own words, "After spending over 50 years educating students throughout the world and providing products for better living, I am now recording information on other pioneers in science so that future students may learn by reading the accomplishments of those profiled in these biographies."

Historical books that Seymour edited or authored include *History of Polymer Science and Technology* (Marcel Dekker, 1982); *History of Polyolefins: The World's Most Widely Used Polymers* (with Tai Cheng) (Reidel, 1986); *History of Polymeric Composites* (VNU Science Press, 1987); *Profiles of Eminent American Chemists* (with Charles H. Fisher) (Litarvan, 1988); *Pioneers in Polymer Science* (Kluwer, 1989); *Organic Coatings: Their Origin and Development* (with Herman F. Mark) (Elsevier, 1990); and *Manmade Fibers: Their Origin and Development* (with R. S. Porter) (Elsevier, 1993). Symposia at ACS National Meetings that he chaired or co-chaired include "History of Polymer Science and Technology," 179th, Houston, TX, March, 1980; "Eminent Southern Chemists," 181st, Atlanta, GA, March, 1981; "Eminent Chemists from the Northeastern U.S.A.," 182nd, New York, NY, August, 1981; "Eminent Chemists from the Western U.S.A.," 183rd, March, 1982; "Eminent Chemists from the Midwest,"

184th, Kansas City, MO, September, 1982; "History of Polyolefins," 189th, Miami Beach, FL, April, 1985; "History of Polymer Composites," 192nd, Anaheim, CA, September, 1986; "History of Organic Coatings," 198th, Miami Beach, FL, September, 1989; and "Manmade Fibers," 201st, Atlanta, GA, April, 1991.

George B. Kauffman, California State University, Fresno

FUTURE MEETINGS

Denver ... 28 March - 2 April 1993

* *General Papers.* Contact M. D. Saltzman, Department of Chemistry, Providence College, Providence, RI 02918, (401) 865-2298.

* *Coordination Chemistry Centennial.* Contact George B. Kauffman, Department of Chemistry, California State University, Fresno, CA 93740, (209) 294-2581.

Chicago ... 22-27 August 1993

Four copies of abstract by 15 April 1993.

* *General Papers.* Contact M. D. Saltzman (see address above).

* *C. K. Ingold, 1893-1970. Master and Mandarin of Physical Organic Chemistry.* Contact M. D. Saltzman (see address above) or Derek Davenport, Department of Chemistry, Purdue University, West Lafayette, IN 47907, (317) 494-5465.

San Diego ... 13-18 March 1994

Four copies of abstract by 1 December 1993.

* *General Papers.* Contact M. D. Saltzman (see address above).

Washington DC ... 21-25 August 1994

Four copies of abstract by 1 May 1994.

* *General Papers.* Contact M. D. Saltzman (see address above).

Anaheim ... 2-7 April 1995

Four copies of abstract by 1 December 1994.

* *General Papers.* Contact M. D. Saltzman (see address above).

* *Archaeological Chemistry.* Contact M. V. Orna, Depart-

ment of Chemistry, College of New Rochelle, New Rochelle, NY, 10805, (914) 654-5302.

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PARTING SHOTS

Gibber, Jabber, or Just Geber?

William B. Jensen, University of Cincinnati

As an avid reader of the older chemical literature, it has long been my custom to keep a file of interesting and/or curious quotes, especially if they relate in some manner to the teaching of chemistry. Until recently my favorite was Tobern Bergman's (1735-1784) contention, which appeared in his 1779 essay "Of the Origin of Chemistry", that the history of chemistry could be divided into three distinct historical periods: "the mythologic, the obscure, and the certain" (1). Believing in a pseudo-Haeckelian version of education in which the mental develop-

ment of the individual student is a sort of weak recapitulation of the historical development of the subject being taught, I have often amused myself with the observation that the majority of my Freshmen manage to survive a full year of 20th-century introductory college chemistry without every leaving the mythologic stage. Some, I will admit, may even have reached the obscure stage, but few, if any, have passed beyond to the age of certainty (I'm certain).

More recently, however, I have stumbled on a serious contender for Bergman's position of favor, found, not in the chemical literature, but in the writings of the famous 18th-century lexicographer, critic and poet, Dr. Samuel Johnson (1709-1784). For in his great *Dictionary of the English Language* we read, under the entry for the word "gibberish" (2):

Gí bberish - [Derived by *Skinner* from *gaber*, Fr. to cheat; by others conjectured to be formed by corruption from *jabber*. But as it was anciently written giberish, it is probably derived from chymical cant, and originally implied the jargon of *Geber* and his tribe] Cant; the private language of rogues and gypsies; words without meaning.

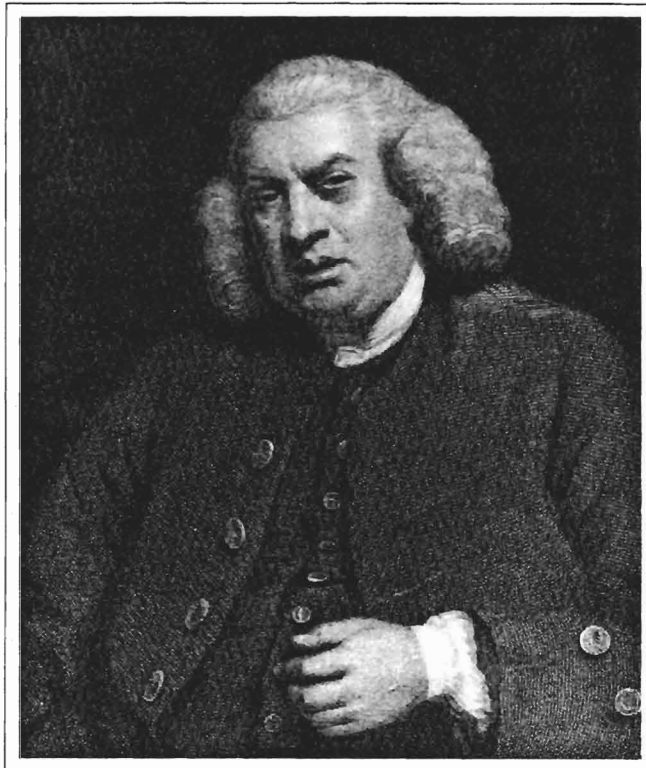
Chemical cant! The private language of rogues and gypsies!! Words without meaning !!! The nightmare vision of 300 Freshmen nodding their heads in gleeful agreement was overwhelming. The ponderous authority of Dr. Johnson had put the linguistic seal of approval on something they had suspected all along.

I immediately consulted other dictionaries in an attempt to confirm or disaffirm this dreadful revelation, but both the *Oxford Unabridged* and *Webster's Unabridged* were silent on the matter. However, Edwin Radford's small volume, *Unusual Words and How They Came About*, fully endorsed Dr. Johnson's assertion (3):

... Despite the contrary opinions of other etymologists the writer holds that this word for unintelligible conversation is derived from Geber, the Arabian alchemist of the eleventh century, who wrote in mystical jargon in order to avoid the death penalty for sorcery, which he might have incurred from the Ecclesiastical authorities of his day had he written, plainly, such heretical opinions. It seems a farfetched theory to our mind to connect it with *gibber*, a variant of *jabber*. *Gibber* did not come into use for many years after *gibberish*.

Aside from the fact that Radford's grasp of the history of chemistry - which places an 8th century Arabian alchemist in the 11th century and has him hiding from the wrath of the Catholic church - is itself an excellent example of gibberish, what exactly is the evidence for this chemical derivation?

In the first place the "Geber" in question is not the 8th century Islamic alchemist Abu Musa Jabir ibn Hayyan, whose actual historical existence is still a matter of dispute, but rather an anonymous 13th-century European author who wrote under



Samuel Johnson (1709-1784)

the pseudonym of Geber (4). Usually referred to as the "pseudo-Geber" in the history of chemistry literature, his writing style is certainly in keeping with Johnson's description of gibberish as "chymical cant", though hardly a strong contender when it comes to alchemical literature in general. Thus, in a not untypical example, from his *Of the Investigation or Search for Perfection*, we are told that (5):

... because these Imperfect Bodies are not reducible to *Sanity* and *Perfection*, unless the contrary be operated in them; that is, the Manifest be made Occult, and the Occult be made Manifest: which operation, or Contrariation, is made by Preparation, therefore they must be prepared, Superfluities in them removed, and what is wanting supplied; and so the known Perfection inserted in them. But Perfect Bodies need not this preparation: yet they need such Preparation, as that, by which their Parts may be more Subtiliated, and they reduced from their *Corporality* to a fixed *Spirituality*.

It is not improbable that Johnson was directly familiar with the writings of the pseudo-Geber through Richard Russell's 1678 English translation of his collected works, as both Boswell and Mrs. Thrale testified that Johnson was quite interested in chemistry and took delight in performing simple chemical experiments. Indeed, Boswell informs us that it was Johnson's translation of a biography of the famous Dutch chemist Her-

mann Boerhaave (1668-1738) for the *Gentleman's Magazine* in 1739 that first awoke in him "that love of chymistry which never forsook him" (6, 7). But beyond the observation that this hobby certainly sensitized Johnson to the possibility of chemical word origins in a manner not duplicated by the training or interests of most later etymologists, little more can be said, and one is left to follow one's personal preferences in the matter. As for me, I choose to believe Johnson.

But what of my 300 demonically grinning Freshmen? On thinking it over, I suspect that the word gibberish is a bit too esoteric for most of them to use on a regular basis and that I should be more concerned about the origins of such phrases as "that sucks" - undoubtedly derived from early Greek writings on hydrostatics and the theory of the siphon.

Notes and References

1. T. Bergman, "Of the Origin of Chemistry" in *Essays, Physical and Chemical*, Mudie and Fairbairn, Edinburgh, 1794, p. 4.
2. S. Johnson, *A Dictionary of the English Language*, Vol. 1., Offor et. al., London, 1820.
3. E. Radford, *Unusual Words and How They Came About*, Philosophical Library, New York, NY, 1946.
4. In a debate that took place in the 1920s and 1930s, the British historian E. J. Holmyard maintained that Jabir was an actual 8th century alchemist and that the Latin works of Geber were an imperfect



An imaginary portrait of Geber

translation of as yet undiscovered Arabic originals. The opposite position was taken by the German scholar J. Ruska, and Ruska's pupil, Paul Kraus, finally came to the conclusion that the entire Arabic Jabirian corpus was actually fabricated in the 9th century by members of an Isma'elite religious sect. I am unaware of whether this debate has ever been satisfactorily resolved.

5. E. J. Holmyard, ed., *The Works of Geber Englished by Richard Russell, 1678*, Dent, New York, NY, 1928, p. 5, a reproduction of the 1678 edition.

6. E. R. Atkinson, "Samuel Johnson's 'Life of Boerhaave'", *J. Chem. Educ.*, 1942, 19, 103-108.

7. D. J. Swaine, "Samuel Johnson's Interest in Scientific Affairs", *J. Chem. Educ.*, 1948, 25, 458-459.

NOTICE TO ALL SUBSCRIBERS AND AUTHORS

As you have undoubtedly noticed, it has been nearly six months since you received an issue of the *Bulletin*. The reason for the delay involves an extensive reorganization of our production and editorial procedures. The increasing size of the issues and the increasing number of library subscriptions have made it difficult, even with our already erratic schedule, for a single person to handle all aspects of the editing, layout, reviewing, printing and distribution. Consequently the Chemical Heritage Foundation has agreed to come in as a third sponsor of the *Bulletin for the History of Chemistry*, along with the Oesper Collection in the History of Chemistry of the University of Cincinnati and the Division of the History of Chemistry of the American Chemical Society.

Beginning with this issue all production and distribution aspects of the *Bulletin* will be handled by the Chemical Heritage Foundation under the supervision of our Associate Editor, Dr. James J. Bohning. As indicated on the subscription form on the back cover, all new subscriptions and all claims for missing or damaged issues should be directed to Dr. Bohning at The Chemical Heritage Foundation, Suite 460B, 3401 Walnut Street, Philadelphia, PA 19104-6228. All matters relating to manuscripts, book reviews and letters to the editor should continue to be sent to Dr. William B. Jensen, Editor, *Bulletin for the History of Chemistry*, Department of Chemistry, University of Cincinnati, Cincinnati, OH 45221-0172.

In order to catch up on our publication schedule we hope to publish issues 12 through 16 during 1993. Because of its large size, the special Faraday issue (No. 11), mailed in July, will count not only as the last issue of the 1991 year but as the first issue of the 1992 year. This issue (No. 12) will count as the second issue for 1992 and issue 13 as the third. Issues 14, 15 and 16 will constitute the 1993 subscription and will hopefully be completed by early spring of 1994 - an event that will finally put us back on schedule. We appreciate your patience. *William B. Jensen, Editor*

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