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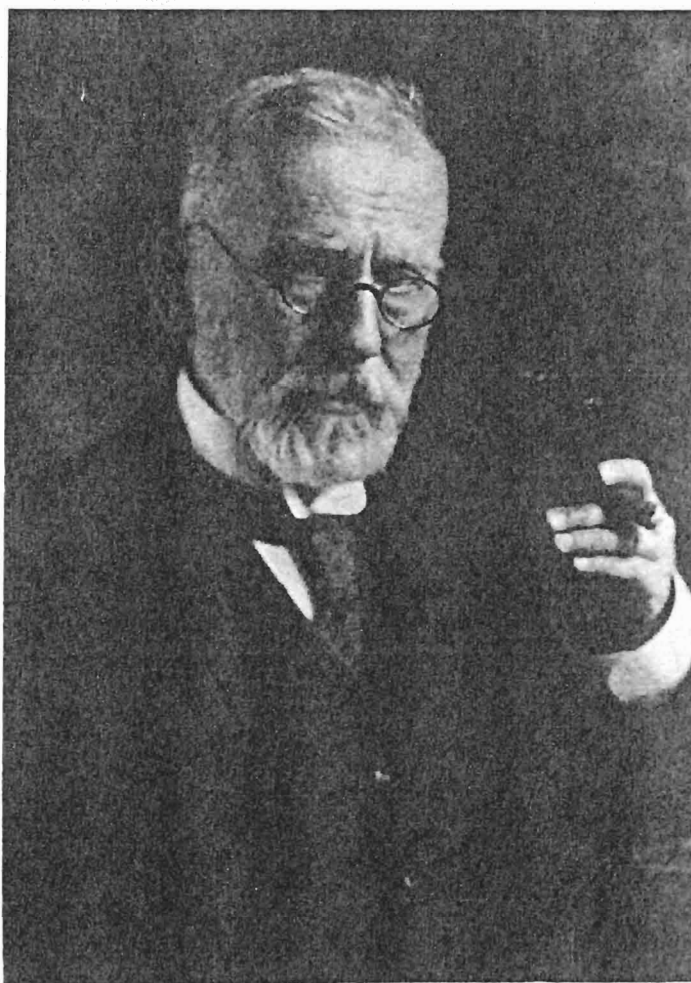
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BULLETIN FOR THE HISTORY OF CHEMISTRY

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THE 1997 DEXTER AWARD ADDRESS A LANGUAGE TO ORDER THE CHAOS

Bernadette Bensaude-Vincent, Université Paris X

The article "chemistry" in Diderot's *Encyclopedia* presented chemists as "a distinct people, still very few, with their own language, their laws, their mysteries, and living almost isolated in the midst of a greater people hardly curious of its business and expecting nearly nothing from its industry (peu curieux de son commerce et n'attendant presque rien de son industrie(1). In order to depict the miserable state of chemistry, Gabriel-François Venel, the chief writer on chemical topics in Diderot's *Encyclopedia*, established a link between the chemical community, its language, and the public perception of the discipline. Despite the deep changes that chemistry has undergone since the mid-eighteenth century, this triple connection still characterizes the identity of chemistry, if we consider chemistry as a culture developed by a specific people. In this anthropological perspective, language plays a chief role in the public perception of chemistry, an esoteric knowledge associated with powerful and obscure practices.

The public, nevertheless, is the consumer of the chemicals produced by this strange people. The chemical formulas, though far beyond the public understanding of science, are tangible and edible products which can serve to poison or to relieve pain. Any serious concern with the ambivalent public image of chemistry implies a thorough reflection on the language of chemistry. This is manifest from Roald Hoffmann's popular publications and Primo Levi's well-known stories and articles(2). The purpose of this lecture is to show that a historical glimpse of the language of chemistry can help demystify the popular demonic image of the chemist.

Let us first try to point out what is unique about chemists' language. In chemistry, as in any other science, a good command of the basic vocabulary is a precondition for an academic degree in the discipline. The chemical community, like any scientific community, shares a common jargon and patterns of argumentation and metaphors, as well a set of tacit rules that guarantee a mutual understanding when the official code of language is not respected(3).

The chemical nomenclature, however, records the chemists' unique experience of nature's diversity. Naming is the necessary activity of the intellect that is confronted with a variety of beings. As the population of substances dramatically increased in the late eighteenth century, thanks to improved analytic methods, chemists more and more needed established, systematic names for communicating and for teaching.

In the late nineteenth century, innumerable organic compounds were created by synthesis. This expanding chemical population, which is both the fruit of the chemists' creativity and a terrible burden, required subject indexing and a continuous invention of neologisms. The main problem is that the need for names always anticipates the prescribing of rules for names. To face this challenge, chemists have adopted different strategies over time. The French chemists who set up a system of nomenclature 200 years ago shaped and organized a specific world view that has been deeply transformed over the past two centuries,

Strangely enough, the chemists' changing attitude in the struggle to discipline the ever increasing multitude of chemical substances, has not attracted scholars'

attention. Whereas working chemists are extremely concerned with their language and fond of stories behind the names in use, few historians of chemistry have ventured into this domain(4). Maurice P. Crosland's classic *Historical Studies in the Language of Chemistry*, first published in 1962, remains the major reference on the nomenclature that was set up at the time of the chemical revolution(5). Later reforms of the chemical nomenclature are known to us only thanks to chemists who were active participants in the reforms(6). Their historical accounts most often emphasize the role of individuals and the difficult consensus. So omnipresent remain the difficulties of naming, that the past still belongs to the chemists' memory rather than to official history. It is also strange that chemical language has been virtually unexplored by philosophers over the past decades, despite the fashion for the philosophy of language(7).

This lecture is an attempt to outline the underlying philosophy of language in the evolution of chemical nomenclature. In contrasting the *Method of Nomenclature* presented two centuries ago at the Paris Academy of Sciences by four French chemists, Guyton de Morveau, Lavoisier, Berthollet and Fourcroy, with the more recent big reform decided at a Conference in Liege in 1930 by the Commissions on Nomenclature of the Union Internationale de Chimie Pure et Appliquée, I will point out a tension between two rival strategies of controlling the linguistic practices of chemists.

An Ambitious Reform

Though Venel complained that chemists were isolated and misunderstood because of their language, he was expecting a revolution, a hero, a new Paracelsus, who would be bold enough to sweep away all prejudices against chemists and to promote chemistry among phi-

losophers. Far from rejecting the artisan's aspects of chemistry, Venel presented the dual nature of chemical practices—both science and arts—as a major strength of chemistry. He proudly claimed that chemistry “held in its body a dual language, the popular and the scientific(8).”

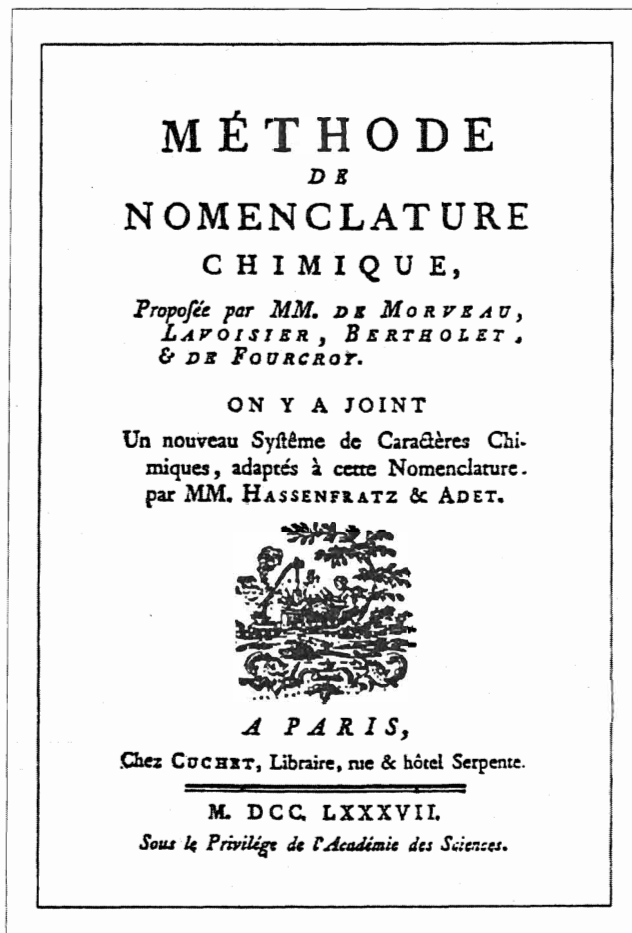
The expected revolutionary hero came twenty years later, but he did not promote the kind of chemistry that Venel advocated. Antoine-Laurent Lavoisier achieved

a revolution in chemistry which has been often presented as the origin and premise of the major reform of nomenclature published in 1787. It is thus assumed that the systematic language was the outcome of a major theoretical change. It was a kind of baptism following the birth of modern chemistry, a radical break with the sins of prescientific chemistry. Most often, the nomenclature reform is still presented as Lavoisier's personal achievement. It is hardly mentioned and only as a kind of anecdotal detail that it was a collective enterprise, whose initiative did not belong to Lavoisier.

In my view, this common interpretation obscures the real conditions of the systematization of language which can only be identified if we take into account the various attempts at systematizing names that had been made before the chemical

revolution. Throughout the eighteenth-century chemists had been increasingly dissatisfied with their language and some of them like Pierre-Louis-Joseph Macquer or Torbern Bergman made timid attempts at systematizing. Motivation for the increasing concern for reforming the language was based on four major reasons, all belonging to “normal” science and independent of the chemical revolution.

1) Enlightenment chemists wanted to rid themselves of the alchemical heritage of names full of mythological references. They had the feeling that they were par-



Method of Nomenclature, 1787

ticipating in a kind of renaissance of chemistry and wanted a language more adapted to this branch of the philosophy of nature. They were encouraged in this direction by the initiatives of natural scientists, especially Carl Linnaeus, who had introduced a systematic nomenclature in botany. Following Linnaeus, the Swedish chemist Torbern Bergman (1735-1784) made several attempts at renaming salts and minerals(9).

2) Because eighteenth-century chemists had better analytical procedures (solvent extraction in addition to distillation), they were able to distinguish between various substances and complained that one name referred to different substances, or, symmetrically, that various names were used for one single substance. Exchanges between chemists all over Europe coupled with an intense activity of translations made these flaws particularly visible. The main impulse came from the chemistry of salts which provided the first basis for a systematic nomenclature.

3) It is important to emphasize the role of classification as a condition underlying all nomenclature. Eighteenth-century chemists developed a consistent classification of salts around the distinction between middle salts, double salts, triple salts...(10). In 1744 and 1755, Guillaume-François Rouelle introduced the generic term "base" for all alkalis and redefined salts as the products of the action of an acid on a base. He then classified salts into three groups: neutral salts, acid salts, or basic salts according to the proportion of the reagents(11). This classification made possible a binomial nomenclature of chemical salts first outlined by Pierre-Joseph Macquer (1718-1784).

4) Finally, new names were badly needed for novel, recently identified substances. A host of new metals recently isolated, such as cobalt and vanadium, had been named after Swedish deities. By contrast, with the development of pneumatic chemistry in the 1760-70's, a dozen newly identified gases or "aeriform fluids" were given systematic names. Some of them expressed one characteristic property: "fixed air" for carbon dioxide because it was fixed in solids; "inflammable air" for hydrogen; or Feuerluft, the name given by William Scheele to the future oxygen. Joseph Priestley, who isolated the same substance, named it after its supposed composition and chose the term "dephlogisticated air." In short, systematic names were gradually introduced in the course of the 18th century, but chemists lacked general agreement within their own community on the principles of naming.

The main initiative for a new nomenclature came from Louis-Bernard Guyton de Morveau (1737-1816),

a lawyer and a well-known chemist from Dijon. In 1782 Guyton authored a bold project for reforming the entire chemical nomenclature(12). His reform was based on the assumption that denominations should reveal "the nature of things," like the botanical nomenclature set up by Carl Linnaeus, although Guyton chose Greek rather than Latin etymologies (presumably because of his



Guyton de Morveau

strong opposition to the language of the Jesuits). Guyton's general principle was: simple names for simple substances and compound names for chemical compounds which express their composition. When the composition is uncertain, Guyton proposed, an arbitrary and meaningless term is to be preferred. In itself, the project of making an artificial language for chemistry, breaking with the traditional language forged by the users of chemical substances over centuries, was ambitious and revolutionary. However, Guyton was extremely modest. He clearly stated in the beginning of his memoir, "I know that it is only through convention that the value of terms can be fixed, and I am further than anyone from the pretension of changing them by the authority of my opinion(13)." His reform was clearly designed to reach a consensus among European chemists.

However, when he arrived in Paris in January, 1787, to submit his project of a systematic nomenclature to the Paris Academy of Sciences, he found the chemistry "classe" divided by the controversy over phlogiston chemistry. He met with Antoine-Laurent Lavoisier (1743-1794), Claude-Louis Berthollet (1748-1822), and Antoine-François de Fourcroy (1755-1809), all three partisans of the antiphlogistonist theory. They "converted" him to the new doctrine and persuaded him to revise his project accordingly. In a few weeks, the four

of them transformed Guyton's earlier outline of a new language into a weapon against phlogistonists(14). His initial project had thus been deeply changed during the six months he spent in Paris. The word "phlogiston" was eliminated, while terms such as "hydrogen" (generator of water) and "oxygen" (generator of acids) reflected Lavoisier's alternative theory. Lavoisier also provided a philosophical legitimization for the new language by referring to Condillac's philosophy of language(15). He assumed that words, facts, and ideas were, so to speak, three various faces of one single reality and that "une langue bien faite est une science bien faite." Linguistic customs and chemical traditions carried only errors and prejudices. By contrast, a language proceeding from the simple to the complex would keep the chemists on the track of truth. The language of analysis that Lavoisier and his collaborators promoted, a "method" rather than a "system," was said to reflect nature itself. Actually, nature was identified with the products of chemical manipulations performed in the laboratory. The name of every compound was the mirror image of the operations of its decomposition.

Like most nomenclatures, this one was based on an implicit classification. Instead of the traditional naturalists' taxonomic categories of genus, species, and individual, the chemists' classification was structured like a language with an alphabet of thirty-three simple substances distributed into four classes: 1) "simple substances belonging to the three realms of nature" (including caloric, oxygen, light, hydrogen, and nitrogen; 2) "nonmetallic oxidizable and acidifiable simple substances;" 3) "metallic oxidizable and acidifiable substances." 4) "earthy salifiable simple substances." This classification was a compromise between the old notion of universal principles and the definition of element as a unit of combination. The simple substances only made up the first column of a synoptic table summarizing the whole system(16). Tables were a favorite means of representation, which Foucault depicted as the center of knowledge in the "classic era(17)." However, the table displayed at the Academy of Sciences in 1787 and

the tables published in the second section of Lavoisier's *Elements of Chemistry* differed from the previous "tables of relations" used by the eighteenth-century chemists(18). Affinity tables represented a condensation of knowledge painstakingly acquired through thousands of experiments. Lavoisier's tables incorporated empirical knowledge but were rather aimed at ordering the material world like a language, an analytical language modeled after Condillac's *Logic*. The grammar of this language derived from a dualistic theory of combinations. It was implicitly assumed that chemical compounds, whatever their mineral, vegetable, or animal origin, were formed by two elements or two radicals acting as elements.

While Lavoisier pretended that the new language mirrored nature, many of his contemporaries objected that such terms as oxygen were theory-laden rather than mere expressions of well-established facts. From all over Europe, chemists tried to discuss the reform and to improve a number of names. Alternative proposals were made for oxygen, because Lavoisier's theory of acids

was not widely accepted, and for *azote* (from a + zoion = not for animals), because many other gases besides nitrogen are not fit for animal life. The French chemists led an intensive campaign of persuasion by involving Madame Lavoisier in translations and entertaining; they created their own journal, the *Annales de chimie* in 1789. Finally, thanks to many translations of the textbooks written by Fourcroy, Chaptal, Lavoisier, and Berthollet, the French nomenclature was widely accepted by 1800. Adoption implied various strategies of linguistic adaptations. A number of chemists resented the French hegemony in a domain which, in principle, should be universal. German and Polish chemists chose to translate the French-Greek terms into German (for instance, *Sauerstoff* for oxygen and *Wasserstoff* for hydrogen), whereas English and Spanish chemists simply changed the spellings and the endings of the terms.

Thus the long term project of reform of the nomenclature which mobilized the chemical community through the course of the eighteenth century played a



Antoine-Laurent Lavoisier

key role in Lavoisier's revolutionary strategy. It encouraged his project of eradicating past and present knowledge in order to found chemistry on a *tabula rasa*. Moreover, as Lavoisier pointed out in his *Elements of Chemistry*, the analytical language, inviting the chemical student to proceed from the-simple-to-the-complex facilitated the teaching of chemistry.

It is important, however, to reconsider this reform in the broader perspective of the *longue durée* and, more importantly for our present purpose, to appreciate its impact on the discipline of chemistry. It must be noted



Claude-Louis Berthollet

that the new language, forged by academic chemists, prompted alienation between them and the dyers, glass-makers, pharmacists, and manufacturers who were more concerned with the terms inherited from their own traditions. Certainly compositional names, as well as the constitutional formulas that were later derived from them, provided significant information for chemists whose main goal was to determine the nature and the proportion of the constituents of inorganic and organic compounds. Nevertheless, these names deprived the pharmacists of knowledge about the medical properties implicit in many traditional terms. Thus the new nomenclature contributed to the subordination of pharmacy to chemistry and, more broadly, to the redefinition of chemical arts as applied chemistry(19).

The chemical language built up by the four French chemists was an integral part of Lavoisier's attempts to

promote and legitimize a new practice of chemistry. Analytical procedures controlled by the balance displaced and discredited experimental results based on qualitative data, whereas phenomenological features such as odors, colors, taste, and appearance, were discarded from the nomenclature. For instance, the "white lead" and the "Prussian blue" used by dyers became, respectively, "lead oxide" and "iron prussate." "Stinking air" was renamed "sulfuretted hydrogen gas." The new language not only ignored the chemists' senses, but it also deprived the chemical substances of their history by banishing all reference to their geographical origins or to their discoverers.

In fact, the principles of the new language were never strictly applied. First, in the domain of inorganic chemistry, a decisive break occurred in the early nineteenth century when, after isolating chlorine, Humphry Davy (1778-1829) established that some substances—hydrochloric acid, for instance—exhibited characteristic acidic properties even though they did not contain oxygen. Oxygen should have been renamed, but custom prevailed over the imperative of systematization. Over time, as many elements were isolated with the help of electrochemistry, odors and colors were restored into the nomenclature. For instance, chlorine, bromine, and iodine were coined after the Greek terms *chloros* meaning green, *bromos* meaning stink, and *iodes* for violet. As regards the vegetable and animal bases and acids, Lavoisier and his colleagues confessed that they were forced, in the manner of old chemists, still to name them after the substances from which they were obtained. In fact, their method of nomenclature did not apply to the realm of organic chemistry. The medical virtues of plant materials, the geographical origin, and even the mythological tradition still provided bases for naming the active principles isolated in the early nineteenth century as exemplified by the word "morphine," named after Morpheus, god of dreams, or by the name "strychnine" for the active principle extracted from the "bois de couleuvre" (*strychnos colubrina*)(20). Even geographical data resurfaced; the term "benzene," for instance, reminds us of the resin produced by the bark of a tree native to Sumatra and Java with the name *Styrax benzoin*; "gutta percha," a gum which played a crucial part in the development of the electric telegraph, was named after the Malay *getha percha* tree in 1845. Nineteenth-century nationalism pervaded chemical language: gallium, discovered by a French chemist, and germanium, another element predicted by Mendeleev, were followed by scandium and polonium. Even the banished Latin language resurfaced with the alphabetical symbols, ini-

tials of Latin names, that were introduced by Berzelius (1779-1848) in 1814(21).

To summarize, the first reform of the chemical nomenclature was certainly a revolutionary enterprise aimed at creating an artificial language, breaking with the past of the discipline. The nomenclators acted as legislators in the name of rationality, in strict conformity with the Enlightenment belief in the authority of reason. However, the construction of the new language of chemistry was not the result of Lavoisier's revolution in chemistry. Rather than being inspired by a radical theoretical breakthrough, it was tactically used as a weapon in the revolutionary process.

Moreover the ideal of systematization pervading eighteenth-century chemistry, which presided over the creation of an artificial language for chemistry, remained an ideal often contradicted by daily usage. It is this tension between the ideal general rules and the constraints of daily use which seems to me a characteristic feature of the creation of the language of chemistry. But what about the later reforms? How are we to characterize the philosophy of the twentieth-century reforms of language?

An Endless Process

In the twentieth century, the reform of nomenclature is no longer synonymous with a single and extraordinary event. Rather it has been a continuous process of revision and an integral part of what is called "normal science." The language of chemistry is no longer a national or a transnational issue in the hands of a few motivated individuals(22). It is an international enterprise, fully integrated into the process of internationalization of science which developed in the late nineteenth century. The Commission for Nomenclature, first coordinated by the Union of the chemical societies, later became a permanent institution, a sub-section of the Union internationale de chimie pure et appliquée (UICPA), created in 1919, with French as its official language. Germany, Austria, and Hungary were excluded because of

the boycott of German science by the allied nations after the *Traité de Versailles*. After the interruption of its activity because of World War II, the International Union was re-established as the International Union of Pure and Applied Chemistry (IUPAC). The two commissions on nomenclature appointed as early as 1921 were much more than a simple by-product of the internationalization of science. As emphasized in a number of studies, the commission acted as a driving force, though the concern for international coordination never completely abolished national rivalries(23).

The new strategy of the chemical community concerning nomenclature was gradually set up with the institution of international disciplinary conferences in the nineteenth century. The Karlsruhe Congress of 1860 can be seen as the first international conference of chemists aimed at ruling

over names and formulas. However, it was a single extraordinary event motivated by a climate of crisis. The chaos of formulas and of atomic weight values that hindered mutual understanding was described as a threat to the advancement of chemistry. Rather than the expression of an organized international community of chemists, the Karlsruhe Congress of chemists was due to the initiative of August Kekulé, who managed to mobilize a hundred colleagues from all over Europe(24).

By contrast, the reform of nomenclature became a feature of normal science in the late nineteenth-century with the institution of regular international conferences. Following the first International Conference of Chemistry held in Paris in 1889, a special section was appointed under the leadership of Charles Friedel (1832-1899), who was in charge of preparing a set of recommendations to be voted during an international conference on chemical nomenclature held in Geneva in April, 1892. The rules were aimed at coordinating the individual attempts made at systematizing the nomenclature of organic compounds. For instance, Williamson introduced parentheses into formulas to enclose the in-



Antoine-Francois de Fourcroy

variant groups as in $\text{Ca}(\text{CO}_3)$, for example, and proposed the suffix *ic* for all salts(25). Hofmann introduced the systematic names for hydrocarbons with suffixes following the order of the vowels in order to indicate the degree of saturation: *ane*, *ene*, *ine*, *one*, *une*(26). A great confusion once again reigned in the language of chemistry. Instructions were given in the various scientific journals which sprang up in the late nineteenth century. The aim of the Geneva Nomenclature was mainly to standardize terminology and to make sure that a compound would appear under one single heading in catalogs and dictionaries. The Commission for Nomenclature felt sufficiently authoritative to propose an official name for each organic compound. Official names were to be built upon the molecular structure and should be as revealing of constitution as were chemical formulas. Names were based on the longest continuous chain of carbons in the molecule, with suffixes designating the functional groups and prefixes denoting substituent atoms. Sixty-two resolutions were adopted by the Geneva group, which only considered acyclic compounds.

However, once again the ideal of systematization was contradicted by daily practice. The official names were never applied in practice although they are still included in modern textbooks because they provide governing principles. After WWI two permanent commissions were set up by the UICPA. The Commission for the Nomenclature of Inorganic Chemistry appointed the Dutch chemist W. P. Jorissen as chairman, and the Commission for the Nomenclature of Organic Chemistry also appointed a Dutch chemist, A.F. Holleman, as chairman. The choice of leaders representing a minor linguistic area clearly indicated an attempt towards a universal language that would not reflect the hegemony of a nation. In 1922, both commissions formed a working party with representatives of various linguistic areas for preparing the rules. Not only educators but also journal editors were invited to join. Following regular meetings in 1924, 1927, 1928, and 1929, the working party in charge of organic chemistry issued reports that were publicized for submission to criticisms and then amended before the final vote in Liège(27). The working party in charge of inorganic chemistry also met several times before issuing the final rules at the Tenth Conference of the UICPA in Rome in 1938. The new regime of naming was thus characterized by a long process of negotiations that allowed for the coinage of new terms familiar to chemists before their official adoption and for a consensus before the final vote(28). Whereas the Geneva Conference, presided over by Friedel, was dominated by the "French spirit and the French logic(29)," the Liège

rules codified suggestions by American chemists, particularly A.M. Patterson, who was directly connected with *Chemical Abstracts*. The Germans, though excluded because of the boycott, were consulted and finally invited to Liège(30).

The style of the Liège nomenclature is quite different from that of Geneva. No more official names. The committee report, unanimously adopted in Liège conformed to the linguistic customs of *Beilstein* and of *Chemical Abstracts* with minor corrections. Rule 1 reads as follows: "The fewest possible changes will be introduced into the universally adopted terminology." Liège, however, broadened the scope of the Geneva nomenclature. Rules were set up for naming the "functionally complex compounds," *i.e.*, those bearing more than one type of function. In the final vote it was decided that both the official Geneva names and the Liège nomenclature could be used. The ideal of systematization thus gave way to a more pragmatic strategy. Flexibility and permissibility were considered as the most efficient means to favor a general adoption of the standard language in the daily practices of chemistry, whether it be in textbooks or journals, in the classrooms or the factories. Since Liège, this pragmatic attitude has prevailed in all successive revisions, in Lucerne (1936), in Rome (1938), and after World War II in Paris (1957). The current nomenclature is by no means as systematic as that the 1787 reformers had envisioned. Trivial names—not referring to the structure of the compounds—coexist with systematic names, conforming to the rules. In fact, both in inorganic and organic chemistry, a majority of names are semi-trivial, *i.e.*, a mixture of anecdote and of constitution(31).

In conclusion, what can be retained? Since 1787, it has been tacitly assumed that chemical compounds are formed like words and phrases out of an alphabet of elemental units, whose combinations allow building up an indefinite number of compound words, according to a complex syntax. Whatever the identity of the basic units—elements, radicals, functions, atoms, ions, molecules—the linguistic metaphor still inspires contemporary chemists. "Chemistry is structured like a language." This assertion, paraphrasing what the French psychoanalyst Jacques Lacan stated about the unconscious, is the main feature of the chemists' language policy(32).

This brief survey of two major reforms of language reveals deep institutional changes in the chemical community. In 1787, the reform of language was achieved in less than six months by a small group of four chemists clearly identified as French scientists. In 1930, a

permanent commission of delegates from various linguistic areas prescribed dozens of rules aimed at standardizing the nomenclature of organic compounds.

The evolution of chemical language over the past century can be described as a retaliation of daily users against legislators. More precisely, the imperative of systematization gave way to the imperative of standardization. This more modest attitude, prevailing up to the end of the twentieth century, reveals two deep changes in the culture of chemistry. The chemists confronted with the increasing difficulty of keeping up with systematic names for extremely complex compounds have renounced their ambition of submitting the molecular world to their ideal of rational systematization. Definitely for modern chemists, the real is by no means rational. As in Diderot and Venel's times, chemists are less 'architects of matter' than dusty laborers trying to discipline a jungle of mixtures, a field certainly controlled by laboratory experiments but still at the mercy of unexpected circumstances.

Twentieth-century reforms also betray a changing attitude towards the chemical heritage received from the past. Clarence Smith, a member of the working party for the Liège nomenclature, suggested in 1936 that, "Could we but wipe out all existing names and start afresh, it would not be a very difficult task to create a logical system of nomenclature. We have, however, to suffer for the sins of our forefathers in chemistry"(33). This statement, contrasting with the revolutionary attitude of 1787, brings us back to the feeling of belonging to a damned people as expressed by Venel. Like Venel in 1753, we might want to promote the "sapientia chymica," a chemical wisdom.

ACKNOWLEDGMENT

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FUTURE ACS MEETINGS

Spring 1999 — Anaheim, CA

Fall 1999 — New Orleans, LA

Spring 2000 — Las Vegas, NV

Fall 2000 — Washington, DC

Spring 2001 — San Francisco, CA

Fall 2001 — Chicago, IL

THE DELHUYAR BROTHERS, TUNGSTEN, AND SPANISH SILVER

Lyman R. Caswell and Rebecca W. Stone Daley, Texas Woman's University

The brothers Juan José and Fausto Delhuyar y Lubice were leaders in the transfer of European chemical and metallurgical technology to colonial Spanish America in the late eighteenth century. Juan José Delhuyar was the first person to isolate tungsten metal, and is recognized as its co-discoverer. Fausto Delhuyar organized the Royal College of Mines (Real Seminario de Minería) in Mexico City, the first technical college in the New World, and served as its first director(1).

The lives of the brothers form a study of contrasts. The elder brother, Juan José, was a brilliant chemist and metallurgist; but, shy and reserved in personality, and without political skills, he was unsuited for the administrative position assigned to him. His efforts to improve the silver mining industry of New Granada ended in failure. On the other hand, Fausto, the socially adept younger brother, possessed great self-confidence and leadership ability. He succeeded as an administrator and educator in colonial Mexico, and, after Mexican independence, as a public servant in Spain.

Juan José was born on June 15, 1754, and Fausto on October 11, 1755, in Logroño, Spain. A sister, María Lorenza, was born on August 8, 1757. The parents were Juan Delhuyar Surrut (2), a surgeon, and Ursula Lubice

Sarrasti (3), who were of French Basque ancestry. Other than brief attendance at a Jesuit academy, nothing is known of the brothers' early education. With the intent of becoming physicians, in 1772 the brothers went to Paris, where they studied medicine, surgery, chemistry, mathematics, physics and natural history. Juan José returned to Spain in 1777. He was accompanied by Angel Díaz Castellanos, who had been a fellow student in Paris and who married Juan José's sister, María Lorenza. Upon his return, Juan José joined the Royal Basque Society of Friends of the Country (Real Sociedad Bascongada de los Amigos del País), which had been chartered by King Charles III in 1765 for the purpose of promoting the applied sciences in the service of Spain (4). This society had been founded by a group of Basque intellectuals under the leadership of the Count of Peñaflores.

Among the goals of the Basque Society was the modernization of the Spanish iron industry, most of which was located in the Basque provinces of northern Spain. At the same time the Navy Minister González de Castejón sought advice for improvement of the quality of Spanish cannon, which were of poorer quality than those made elsewhere in Europe. At the suggestion of a Basque naval officer, José de Mazarredo, Castejón consulted the Basque Society in



Juan José Delhuyar y Lubice

this matter. The two goals were combined. The Basque Society was to select two technically trained persons, one to visit the British cannon works at Carron in Scotland, the other to study mining engineering and metallurgy at the Freiberg Mining Academy (Bergakademie Freiberg) in Saxony and visit the cannon factories in Germany and Austria. The Basque Society selected Juan José for the latter mission. He left Spain in April 1778 to study at Freiberg on a fellowship provided by the Society. He also had secret instructions from the Navy to visit the cannon factories of northern Europe to learn as much as possible about the technologies of cannon manufacture in use there. The Navy would reimburse the Basque Society for his expenses. It was understood that he would be supervisor of the Spanish cannon works upon his return to Spain.

As part of the drive to improve Spanish technology, the Basque Society founded the Patriotic Seminary (Seminario Patriótico) in Vergara in 1777. This was the first technical college in Spain, and the first to have chairs of chemistry and mineralogy. These two professorships were to be financed by the Navy, while the Basque Society financed the chair of physics.

Two members of the Basque Society who were in Paris, Javier María Eguía and Antonio María de Munibe y Areyzaga, the son of the Count of Peñaflores, were asked to recruit faculty for the Patriotic Seminary. They nominated their friend and fellow student, Fausto Delhuyar, for the chair of mineralogy. Hilaire-Marin Rouelle, with whom Fausto had studied chemistry, supported his appointment. Louis-Joseph Proust, who later established the Law of Definite Proportions, received the chair of chemistry. Fausto accepted his appointment in December, 1777, with the condition that he first take advanced studies in geology and mineralogy in Germany and Austria. The Basque Society sent him from Paris to join his brother in Freiberg to study with Abraham Gottlob Werner, who at that time was Europe's leading geologist. In addition to their studies at Freiberg, the brothers visited mineralogists and mines in Germany and Austria. Among the persons whose acquaintance they made was Baron Ignaz von Born, Court Counselor in the Austrian Department of the Mint and Mining

(Hofrat bei der Hofkammer in Münz und Bergwesen). At a musical evening at Born's home in Vienna, Fausto met his future wife, Juana Raab de Moncelos, daughter of an Imperial Councilor (Consejero Áulico de Imperador). Following the conclusion of their studies at Freiberg, the brothers toured the mining districts of Hungary, including a visit to the Imperial Royal Mining Academy (Kaiserliche Königliche Bergwesens-Akademie) at Schemnitz (now Banská Štiavnica in Slovakia). Fausto returned to Spain in October 1781, and initiated his course in mineralogy at Vergara the following year.



Fausto Delhuyar y Lubice

In March, 1781 Juan José wrote to the Count of Peñaflores that he had learned that tests made at the Hamburg proving grounds showed that Swedish-made cannon were superior to the British cannon from Carron. In December, Juan José used this information to justify a trip to Sweden, where he took an advanced chemistry course with Torbern Olaf Bergman at the University of Uppsala (5). In July, 1782, accompanied by Charles André Hector de Virly, president of the *Chambre de*

Comptes of Dijon, France, Juan José visited the laboratories of Carl Wilhelm Scheele at Köping. At this time Scheele and Bergman were investigating the composition of a mineral called in Swedish *tung sten* ("heavy stone"), whose modern name is scheelite. Chemically it is calcium tungstate. Scheele had prepared tungstic acid, from which Bergman had obtained tungsten(VI) oxide. Both Scheele and Bergman suspected the presence of a new element in these materials, but neither of them had been able to isolate it. Among Juan José's assignments in Bergman's laboratory at Uppsala had been the task of repeating this work.

Juan José returned to Spain in October, 1782. He recommended to the Navy that the Spanish method of hollow casting of cannon be abandoned. Instead the cannon should be cast solid and the barrel bored out, following the method used in Germany and Sweden. Upon receiving this report, the Navy Minister, Marqués González de Castejón, fired Juan José. Castejón placed the management of the munitions factories under a naval officer, Antonio Valdés, with orders to continue hollow casting. The Navy refused to reimburse Juan José for the expenses of his trip to Sweden (6). Temporarily

out of a job, Juan José joined Fausto at Vergara in the analysis of a mineral from the Zinnwald, near the border between Saxony and Bohemia. This mineral, which had the German name *wolf rahm*, or "wolf foam," is known today as wolframite, and is a mixed tungstate of iron(II) and manganese(II). From this mineral the brothers prepared substances which Juan José recognized as identical with the tungstic acid and tungsten(VI) oxide he had prepared in Uppsala. The training in smelting techniques, learned at Freiberg, enabled the brothers to reduce the oxide and obtain tungsten metal, which neither Bergman nor Scheele had been able to do. Their method, reduction by powdered carbon in an air-tight crucible, produced pellets of tungsten. The Delhuyars proposed the name *wolframium* for the new metal. The international symbol W for tungsten derives from their name. The brothers' report of the new element, entitled "Chemical Analysis of Wolfram, and Examination of a New Metal, Which Enters into its Composition," was published in the *Extractos* of the Basque Society in 1783 (7). A French translation was published in 1784 in the *Mémoires* of the Academy of Toulouse, of which Fausto was a corresponding member. Translations of this report were published in Swedish in the same year, in English in 1785, and in German in 1786 (8). Although the brothers shared equally the credit for the discovery of tungsten, Juan José deserves the greater share, perhaps the principal share. He had already become familiar with the compounds of tungsten in Bergman's laboratory; and it was he who had studied smelting technology at Freiberg. Unlike his brother, he had no classes to teach and he could spend as much time as he wished in the laboratory. It was Fausto, however, who communicated the news of the isolation and properties of the new metal to Bergman, in a letter of January 15, 1784 (9). By this time Juan José had departed for Cádiz to prepare for embarkation to America.

Juan José received his new appointment late in 1783. Antonio Valdés, who had succeeded Castejón as Navy Minister in March, reviewed Juan José's qualifications and recommended him to José de Gálvez, the Minister of the Indies (colonial minister). Gálvez offered him an appointment as Director of Mines of New Granada (modern Colombia). Juan José's assignment was to increase the *quinto*, the fraction of the mining production reserved for the government. This required finding ways to increase production of the mines, while at the same time holding down costs. Since the founding of the Spanish colonies in the Americas, a significant portion of the Spanish government's revenue derived from the *quinto*. During the seventeenth and eighteenth

centuries, the value of the *quinto* declined as the result of inflationary trends in the home country. This, combined with the exhaustion of the richer ores, had resulted in increased production costs in the colonies. A large part of the production costs lay in the cost of the mercury needed for the *patio amalgamation method* used to separate silver and gold from their ores (10). In this method the pulverized ore was spread on an open, paved court, acidified, and mixed with mercury. The process was labor-intensive and required five weeks to five months to complete, depending on the weather and the nature of the ore. When the managers deemed the amalgamation to be complete, the earthy residue was washed away, the amalgam was formed into cones and the mercury was distilled out, leaving "pinecones" (*piñas*) of silver sponge. Production of mercury from American mines was inadequate to meet the need. Most of the mercury had to be shipped from Europe. In times of war adequate supplies of mercury could not be obtained. A solution to the problem was suggested by the naturalist José Celestino Mutis, the Director of the Royal Botanic Expedition in New Granada (Real Expedición Botánica del Nuevo Reino de Granada). He recommended to the Viceroy of New Granada, Antonio Caballero y Góngora, that amalgamation be replaced by smelting, which could be done with charcoal manufactured from the local forests, as was done in the mining regions of Germany. Caballero forwarded the recommendation to Gálvez. The latter, noting Juan José's expertise, ordered him to smelt the ores of New Granada.

Juan José accepted the appointment without full awareness of its nature. He saw the job as a technologist's position, in which he would work directly in prospecting, assaying, mining engineering, and metal production. The colonial authorities saw it as a management job. The two viewpoints did not overlap. Juan José, moreover, went into the job without first gaining assurances of an competent support staff and an adequate budget. After a six-month wait in Cádiz for a ship to New Granada, Juan José sailed on July 26, 1784. His brother-in-law, Angel Díaz, accompanied him. During the delay, Díaz had expended their resources on entertainments and fine clothes, and they were impoverished when they sailed. Juan José's salary did not begin until his arrival in New Granada. Juan José started work at a partially abandoned *real* (mining property) at the village of Santa Ana in New Granada on March 2, 1785. His inspection of the mines and assays of ore samples resulted in an optimistic preliminary report to the Viceroy, dated April 10, 1785, in which he recommended reopening the mines (11).

A few days after Juan José sent his first report to the Viceroy, word reached Spain that a new and highly efficient amalgamation method for extraction of silver had been invented by Baron Ignaz von Born. In response to this report, Fausto wrote to Born to request more information. Born replied through diplomatic correspondence that if the King would send Fausto to him, "it would make me an infinite pleasure to instruct him in the whole of this process, and to show him everything (12)." Discouraged by poor student interest in his classes, Fausto submitted his resignation from his professorship at the Patriotic Seminary on September 20, 1785, to be effective at the end of the year. On February 6, 1786, the Indies Minister, Gálvez, summoned Fausto to Madrid to discuss the various ore treatment processes which were of concern to Juan José in New Granada. Fausto advised his brother of the Born process in a letter of February 16. He left Madrid for Vienna soon thereafter with instructions to learn the details of the new process. He was also to recruit mining and smelting technologists from central and northern Europe to bring their expertise to the American mines.

Born invited not only Fausto Delhuyar, but also mining engineers and metallurgists from all over Europe to attend a conference held in the summer of 1786 near Schemnitz, at the village of Glashütte in Hungary (now Skleno in Slovakia), where he had set up a full-scale operation of his method (13). This meeting has been called the world's first international scientific congress (14). The conference was held with the approval of the Emperor Joseph II, who expected that adoption of Born's method would enhance the market for mercury from his mine at Idria (now Idrija in Slovenia). Born's book describing his process, *On the Amalgamation of Ores*, was also published in 1786 (15). In the *Born amalgamation process*, the ore was mixed with salt, roasted, pulverized, and slurried with mercury and water in heated copper kettles, with stirring by windmill-driven paddles. Completion of the amalgamation required only 24 hours. Workup was done in the same way as in the patio process. Fausto went to Austria to learn the details of what was actually a Spanish invention. Born himself admitted that his procedure was first described in a book published in Spain almost 150 years earlier by Alvaro Alonso Barba (16), who had been director of mines at Potosí in Upper Peru (modern Bolivia). In a letter to Gálvez on October 21, 1786, Fausto expressed the opinion that the abandonment of the Barba method in America may have been due to local conditions that made it less effective than in Austria and Germany (17).

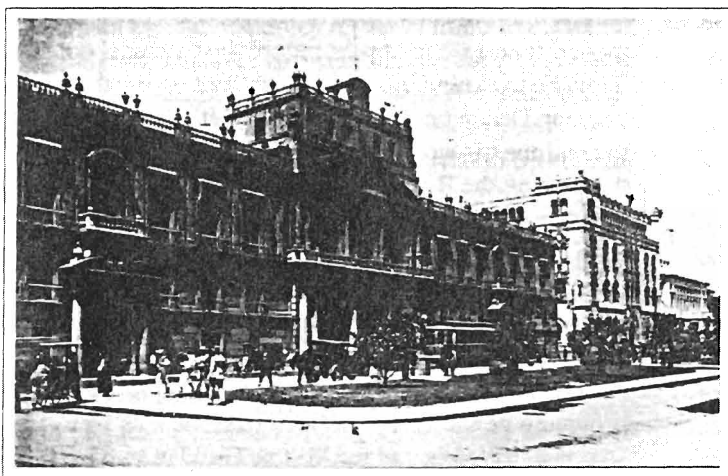
On July 3, 1786 Juan José reported to the Viceroy that he had completed preparations for smelting. Before he could begin smelting, however, he received the letter from Fausto advising him of the newly reported Born amalgamation method. Upon being advised of the Born method, the Viceroy ordered suspension of the plans for smelting. Juan José proposed a test of the Born method at the mines at Mariquita. This plan was approved by the Viceroy and subsequently by Gálvez. Juan José did not receive the full details of the Born method until nearly two years later, on June 17, 1788, when he received a copy of Born's book from his brother. In the interim a part of Juan José's attention found a very different direction. On December 1, 1788 he married María Josefa Bárbara Gaona y Lee, the daughter of a lawyer of Santa Fe (modern Bogotá). He returned with his wife to Santa Ana to make his home near the mines.

On July 18, 1786, Gálvez wrote to Fausto in Vienna to advise him of his appointment to the post of Director-General of the Royal Mining Guild of Mexico (Director General del Real Cuerpo de Minería de México). Gálvez died on June 17, 1787. His successor as Minister of the Indies was the same Antonio Valdés who had called Gálvez's attention to Juan José. Valdés allowed Fausto to delay his departure for Mexico in order to marry Juana Raab de Moncelos, whom he had met at Born's home during his earlier trip to Austria. The wedding took place on October 16, 1787, the Spanish Ambassador serving as Fausto's best man. Fausto and his wife arrived in Vera Cruz late in the summer of 1788 and proceeded at once to Mexico City (18). They were accompanied by eleven German mining technicians who had been recruited in Saxony, headed by mineralogist Friedrich Sonneschmidt (19). The Germans went to the *real* of Sobrerete, in the present-day State of Zacatecas.

Fausto found the situation in Mexico very different from that found by his brother in New Granada. Mexico City had a scientific establishment, some of whose members had already directed their attention to the practical matter of improving mining technology and productivity in the colony. In 1774 two of these men, Joaquín Velázquez Cárdenas de León and Juan Lucas de Lassaga, had submitted to King Charles III a *Representación* which described problems in the mining industry of Mexico and recommended reforms. Included in the *Representación* was a recommendation for the establishment of a College of Mines. After a nine-year traverse of the *Representación* through the Spanish bureaucracy, the King issued the Royal Mining Ordinances (Reales Ordenanzas de Minería), which were published in Mexico City on January 14, 1784. Velázquez de León

was appointed president of the Royal Tribunal General of Mining (Real Tribunal General de la Minería), the body organized to govern the Mining Guild (Cuerpo de Minería). A fund was created for the endowment of a "Metallic College" (Colegio Metálico).

Ramón Ruíz de Liceaga became acting president of the Tribunal after the death of Velázquez de León in 1786. An ugly confrontation occurred upon the presentation of Fausto Delhuyar to the Tribunal, on September 13, 1788. Fausto demanded that Ruíz de Liceaga



School of Mines in Mexico City. From M. E. Weeks and H. M. Leicester, *Discovery of the Elements*, 7th ed., Journal of Chemical Education, Easton, PA, 1968; p. 256.

surrender the chair to him, on the ground that his royal appointment as Director-General gave him authority over the Mining Tribunal. Ruíz de Liceaga refused. Angry words and insults were exchanged, and the hall had to be cleared of spectators. Ruíz de Liceaga, believing his position to be secure, appealed to the Crown. The response was prompt. On December 30, 1788, the Viceroy of New Spain dismissed Liceaga, and on January 2, 1789, Fausto sat in the chair of authority, by order of the King.

On January 12, 1789, Fausto asked the Tribunal General for all documents in their archives relative to the formation of the School of Mines. Three days later, in what can be construed as an act of bad faith on the part of the *diputados* of the Mining Tribunal, he was informed that no such documents existed. Fausto took this response as permission to proceed according to his own ideas. He prepared his "Plan for the School of Mines," which he presented to the Tribunal exactly one year later (20). The plan called for a six-year program. The first year of the curriculum was devoted to math-

ematics, beginning with arithmetic and ending with conic sections. The second year consisted of subterranean geometry, dynamics and hydrostatics. Courses in chemistry, mineralogy and metallurgy occupied the third year. The fourth year was devoted to "subterranean physics, or the theory of mountains." The students were also to study French and drawing. The final two years were devoted to internship in the mines. At the conclusion of the six years, each student was required to stand for examination before the Tribunal General and the faculty of the College. Provision was made for scholarships for twenty-five students, "none younger than fifteen years of age nor older than twenty," who were to be chosen from the various mining districts. The plan included the design of a uniform; a detailed list of the articles of clothing and bedding for each student; a schedule of the students' activities for each hour of the day, which included daily attendance at mass and two periods reserved for recreation; a schedule for daily meals, including a break for chocolate at 2:00 p. m.; and a schedule of salaries for the faculty and staff, from the rector to the scullery boy (21).

The College of Mines was inaugurated on January 1, 1792. The building initially occupied by the college was a former convent adjacent to the Hospice of Saint Nicholas (Hospicio de San Nicolás) at what is today 88, 90, and 92 Guatemala Street in Mexico City. In 1811-13, the College moved to a new building, the Palacio de Minería, on Tacuba Street. The first professor of chemistry at the college was Luis Lindner, a German mineralogist who had been a member of Sonneschmidt's team. Fausto Delhuyar taught chemistry in his place during Lindner's illness in 1795. The first course in mineralogy was initiated April 27, 1795 by Andrés Manuel del Río (22), who had also been a student of Werner at Freiberg, and who would later discover the element vanadium. The first publication of a Spanish translation of Lavoisier's *Traité Élémentaire de Chimie* was made in Mexico City in 1797, specifically for the use of the College of Mines. The identity of the translator is not known (23). In 1798 the second year of the curriculum was modified to include a full course in physics, to which instruction in calculus was added in 1802. A course in Latin was added in 1799, and courses in logic and geography were added in 1802.

Fausto's plan for the college included laboratory instruction in mechanics, electricity, optics, mineralogy, chemistry, and metallurgy. An invoice, from an agent in London, dated July 30, 1796, for the equipment for

these laboratories has been preserved (24). Items of chemical interest on the list included a variety of balances, thermometers, eudiometers, barometers, electric batteries, and "chemico-pneumatic apparatus," as well as an "electric pistol for inflammable air," and a "flask of Priestley to convert air into acid." The most expensive entry was "alembics, crucibles, mortars, evaporating dishes, flasks, jars, bottles &c. &c. &c. which cannot be specified individually in this list." Additional equipment was constructed locally. On November 29, 1803 the College purchased the instruments that had been carried by the naturalist Alexander von Humboldt in his explorations of Spanish America and commissioned Humboldt to purchase additional equipment on his return to Europe (25). During the period 1798-1811, the College of Mines admitted 92 students, and 34 of these completed the six-year course. The Mexican rebellion against Spain caused operations of the college to be suspended in 1811. Several of the graduates of the college joined the revolt, were captured by the Spanish authorities, and executed (26).

Spain recognized the independence of Mexico in 1821. Fausto Delhuyar resigned on October 17 of that year and returned to Spain with his wife and daughter. In Spain he served as Director General of Public Credit, Director General of Mines and director of the mining school at Almadén. He died in Madrid on January 6, 1833, from a blow to the head resulting from a fall on the stairs at his office.

Friedrich Sonneschmidt and his German colleagues had less success at the mines than Fausto Delhuyar in Mexico City. They found that the Born amalgamation was no more efficient than the patio method at extracting silver, and the cost of the fuel to run it in fuel-poor Mexico made it more expensive. They turned their attention to improving the patio method, at which they had greater success (27). While Fausto Delhuyar was establishing his authority in Mexico City, his brother Juan José in New Granada continued to experience frustration. A delegation of seven German miners, sent from Saxony under contract to the Spanish government, arrived at Santa Ana in December, 1788, shortly after the return of Juan José and his bride. They were not what Juan José needed. On January 2, 1789 he wrote to the Viceroy that "these fellows know nothing about the refining of ores(28)." Three of them were illiterate.

A new Viceroy, Francisco Gil y Lemos, took office on January 8, 1789. Three days later he ordered suspension of mining and extraction of metals. In order to make sure that his order was followed, the Viceroy cut off funding for the mines. Juan José was forced to issue

scrip so that the miners could buy food, and to use personal funds to pay the costs of construction of the facilities for ore processing. The Germans were left with nothing to do. Some of them became ill, and the head of their delegation died. In April the new Viceroy inspected the operations at Santa Ana. He confirmed his predecessor's order to use the Born amalgamation process and promised to reimburse Juan José for his expenses. Before this could be done, however, Gil y Lemos was reassigned in July to the Viceroyalty of Peru. His replacement as Viceroy, José de Ezpeleta, finally reimbursed Juan José at the end of November. Ezpeleta summoned Juan José to appear before the viceregal court in Santa Fe to explain his operations. Following this audience, on December 15, the Viceroy authorized Juan José to continue his work and confirmed the order to process the ores by the Born method.

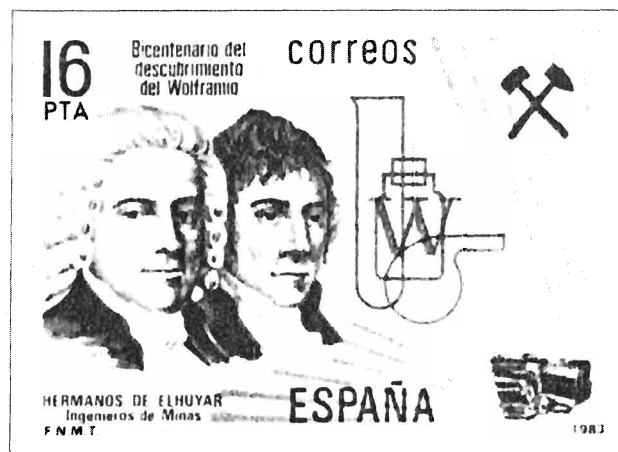
Upon their return to Santa Ana, in January 1790, Juan José and his wife were joined by his sister, María Lorenza, who had separated from her husband, Angel Díaz. Responsibility for her support fell to Juan José. Díaz, to whom Juan José had given responsibility for routine management of the mines of Santa Ana, had proven to be unreliable in this assignment as well.

Having learned of the Mining Guild in Mexico from his brother Fausto, Juan José had proposed to Viceroy Caballero that a similar organization be formed to oversee the mining industry of New Granada. Since no action was taken by either Caballero or his temporary successor Gil y Lemos, Juan José sent a renewed proposal to the new Viceroy, Ezpeleta. Not only did Ezpeleta deny the request, he also ordered Juan José to use African slaves as laborers in the mines in place of paid workers, in order to reduce expenses, a usage which was illegal under the Royal Mining Ordinances. According to the Viceroy's interpretation, these laws applied only in Mexico, not in New Granada. Once approval had been given to proceed with all operations, the delaying tactics of the Viceroy were replaced by impatience that results were not immediately produced. Juan José had difficulty obtaining supplies of the chemicals and materials sufficient to carry out the Born process on an industrial scale. Salt, like silver, was a monopoly of the Crown, and he had to depend on the Viceroy to provide it. The artisans hired to construct the Born apparatus were barely competent, and lost work time in jail as the result of fighting.

Full-scale operation of the Born process was finally initiated on July 4, 1791. The first shipment of silver "pinecones" was sent to the mint in Santa Fe on November 30, 1791, six years and nine months after Juan José started his work in New Granada. The shipment

was accompanied by instructions to the mint to assay the metal and reduce it to ingots. These instructions displeased the Viceroy. Juan José replied that he lacked the resources to carry out these activities at the mines. With operations at Santa Ana finally up and running, Juan José was able to turn his attention to other mining districts. In November 1791, on order of the Viceroy, he sent two of the surviving Germans to supervise the gold mines at Quebralamo in Popayán, as requested by the owner of these mines.

In 1793, Juan José financed and led an expedition to investigate the resources of the Páramo del Ruiz, an uninhabited plateau. He found there both wild cattle and significant mineral deposits, and filed claims to these resources. He contracted one Juan Isidro Jaramillo to supervise these claims. Jaramillo proved to be both incompetent and dishonest, and was unable to account for some of the supplies and pack animals that had been entrusted to him. To cover his own shortcomings, he spread slanders about Juan José. A confrontation occurred between Juan José and Jaramillo in the office of the mine at El Sapo in February, 1794. Angry words were exchanged. Juan José struck Jaramillo with his cane. Jaramillo stabbed Juan José, inflicting wounds to his left arm and left hand, which Juan José had raised to ward off the blow. Jaramillo fled, but was arrested a few days later at Mariquita. He was tried before the Audiencia in Santa Fe, found guilty of assault, and sentenced to four years in jail. Through his lawyer, Jaramillo sent a letter to the Viceroy, in which he accused Don Juan José of "frauds, extractions, and inlapidation" in the silver mines of Santa Ana and Lajas. The Viceroy withdrew Juan José from the direction of mines and placed him under house arrest at the town of Guaduas. Supervision of the mines of Santa Ana was assigned to Angel Díaz, the errant brother-in-law. Juan José departed from Santa Ana on August 7, 1795, leaving unfinished his research into methods for separating gold and platinum. The Viceroy's actual motivation in accepting the word of a convicted criminal and firing Don Juan José may have had a political basis. Spain was at war with revolutionary France. A Spanish translation of *The Rights of Man* and other revolutionary tracts had been clandestinely circulated in New Granada. Juan José was known to be of French ancestry, and relatives of his mother still lived in France. Juan José appealed to the Crown to review his case, on the ground that his contract as Director of Mines had been made with the King, not the Viceroy. His position was vindicated by a royal decree of January 19, 1796, in which the Viceroy was ordered to restore Juan José's rights.



Fausto (in wig) and Juan José Delhuyar on a Spanish stamp issued in 1983 to commemorate the two-hundredth anniversary of their discovery of tungsten.

Meanwhile, Angel Díaz proved to be no more competent at managing a mine than he had been at maintaining his marriage. When word of these problems reached Spain, the Minister of the Indies, Diego de Gardoqui, authorized the Viceroy to sell the mines. On November 19, 1796, the Viceroy recommended that the mines be freely ceded, with their slaves, to anyone who would obligate himself to continue their work.

Juan José's health declined from the middle of 1796. He suffered from an intermittent fever. On November 20, the day after the Viceroy's decision to abandon the mines, he had a seizure and died (29). He was survived by his wife, a son, and two daughters.

In summary, Fausto Delhuyar and his colleagues in Mexico were unable to apply the Born amalgamation method for the refining of silver ores but succeeded in reforming the Mexican mining industry and in establishing the School of Mines. His elder brother Juan José failed in his efforts to organize and improve the mining industry of New Granada, but succeeded in applying the Born method there. The availability of adequate fuel made this latter success possible; but if the authorities had allowed him a choice, Juan José would have used that fuel to smelt the ores instead. By the time of the initiation of the Born amalgamation process in New Granada, it had already been abandoned in Europe in favor of smelting (13).

ACKNOWLEDGMENT

The authors are grateful to a reviewer for calling their attention to some materials relating to the lives of the Delhuyar brothers which they had overlooked.

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- The family name of the brothers has many variant spellings, of which Delhuyar, D'Elhuyar and de Elhuyar are the most common. The brothers themselves were inconsistent in their usage. At the International Symposium for the Bicentenary of the Foundation of the College of Mines of Mexico it was decided to accept exclusively the form "Delhuyar" (Ref. 1e, p. 61).
- Ursula's family name is given as Zubice by some sources. Bernardo Caycedo, Juan José's descendent and biographer, cites contemporary documents in which the name is given as Lubice (Ref. 1b, pp. 20, 167, 185). In the marriage contract of the brothers' parents, the name is spelled Lubize (Ref. 1e, pp. 58-59). The discrepancy may be due to misreading a script capital "L" with a long upper tail as a "Z".
- Some sources refer to the "Royal Economic Society of Friends of the Country" (Real Sociedad Económica de Amigos del País). There were actually several societies with this name. They were regional societies organized on the model of the Basque Society, and with similar functions (Ref. 1d, pp. 4-6).
- Juan José's notes, in French, taken from Bergman's lectures, have been preserved and published: A. Fredga and S. Rydén, "Juan José de Elhuyars anteckningar after Torbern Bergmans föreläsningar 1782", *Lychnos*, **1959**, 161-208. An English translation of these notes has been provided by J. A. Schufle in "Torbern Bergman, A Man Before His Time," Coronado Press, Lawrence KS, 1985, Appendix I, pp. 412-464.
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- J. J. and F. de Luyart, "Análisis químico del volfram, y examen de un nuevo metal, que entra en su composición", *Extractos de las Juntas Generales celebradas por la Real Sociedad Bascongada de los Amigos del País*, Sept. 1783, pp. 46-88 (Ref. 1f, footnote 1).
- Ref. 1e, p. 211.
- S. Rydén, *Don Juan José de Elhuyar en Suecia (1781-1782)*, Imp. S. Aguirre Torre, Madrid, Spain, 1954, Anexo I, pp. 41-48.
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- (a) F. Szabadváry, "Ignaz Born und die erste internationale wissenschaftliche Tagung und Gesellschaft der Welt", *Österr. Chem. Z.*, **1989**, *90*, 109-110. (b) M. Teich, "Born's Amalgamation Process and the International Metallurgic Gathering at Skleno in 1786," *Ann. Sci.*, **1975**, *32*, 305-340.
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- Ref. 18a, p. 307.
- F. de Elhuyar, *Plan del Colegio de Minería presentado al Real Tribunal por el Director del ramo, D. Fausto de Elhuyar* (Ref. 18c, pp. 61-73), translated by Howe (Ref. 18a, Appendix D, pp. 490-500).

21. The best salaries were those of the professor of chemistry and metallurgy, the professor of mining, and the professor of mechanics, each of whom was to receive 2000 pesos per annum (Ref. 20).
22. S. Ramírez, *Biografía del Sr. D. Andrés Manuel del Río*, Imp. del Sagrado Corazón de Jesús, Mexico DF, 1891, 18.
23. *Tratado Elemental de Química*, Mariano de Zúñiga y Ontiveros, Mexico City, 1797 (Ref. 18b, pp. 109-200). Izquierdo holds the opinion that either Luis Lindner or Fausto Delhuyar, or both, were involved in the translation, since they shared the chemistry instruction at this time.
24. The complete list is reproduced by Howe (Ref. 18a, Appendix E, pp. 501-508). See also Ref. 18b, pp. 91-95 (laboratory instruction in physics), and pp. 115-119 (laboratory instruction in chemistry).
25. J. A. Ortega y Medina, *Cronología Humboldtiana. Datos de la Vida de Alejandro de Humboldt*, Anexo I to A. de Humboldt, Ensayo Político Sobre el Reino de la Nueva España Editorial Porrúa, S. A., Mexico DF, 1966, pp. lv-cxxi.
26. The College of Mines resumed operation after Mexican independence. In 1867 it became a part of the College of Engineering, which became the Faculty of Engineering of the Universidad Nacional Autónoma de México (UNAM) in 1910. The date of the founding of the College of Mines is today recognized as founding of engineering education in Mexico.
27. F. Sonneschmidt, *Tratado de la Amalgamación de Mexico*, Imprenta de d. Mariano de Zúñiga y Ontiveros, Mexico City, 1805 (Ref. 18b, p. 200). Humboldt's description of the patio amalgamation derives from the manuscript of this book (Ref. 10, p 252). Sonneschmidt found that the efficiency of the patio method was improved by treating different types of ores separately, rather than amalgamating different types indiscriminately mixed together.
28. "Esos sujetos no entienden nada del beneficio de los minerales." (Ref. 1c, p. 172)
29. According to Ref. 1f, death was due to a cerebral hemorrhage.

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BRITISH WOMEN CHEMISTS AND THE FIRST WORLD WAR

Marelene F. Rayner-Canham and Geoffrey W. Rayner-Canham, Sir Wilfred Grenfell College

The First World War is sometimes called the 'Chemist's War' as its prosecution demanded ever increasing quantities of explosives, poison gases, optical glass, synthetic dyes, and pharmaceuticals (1). As the war progressed and severe shortages of chemicals occurred, more and more women were pressed into chemical-related work. Very little has been published about the skilled women chemists who were assigned to war duties (2). They were obviously much fewer in number than the hundreds of thousands of unskilled women who worked in the explosive factories (3), though they certainly did exist. Fortunately, the Women's Work Collection of the Imperial War Museum (IWM) has a significant amount of documentary evidence on the wartime women scientists. This useful material was compiled in 1919 by Agnes Ethel Conway of the Women's Work Sub-Committee of the IWM. Conway circulated a questionnaire to universities and industries informing them that the Committee was compiling a historical record of war work performed by women for the National Archives. In particular, Conway adds: "they [the Sub-Committee] are anxious that women's share in scientific research and in routine work should not be overlooked ..." (4). A sufficient number of replies were received to provide a sense of the breadth of employment of scientifically trained women during the War.

The Availability of Women Chemists

Had it not been for the significant increase in the number of women taking chemistry degrees during the first 15 years of the twentieth century (5), it is apparent from the replies to Conway's enquiries that the British war machine would have faced a severe shortage of chem-

ists. Fortunately, there was a pool of qualified women chemists ready and willing to do their part towards the war effort. As illustration, K. J. P. Orton, the Professor of Chemistry at the University College of Wales, Bangor, commented in a report that "The demand for young women who have received a training in Chemistry, both for educational and professional work, has increased greatly during the past Session. The demand is far in excess of the supply ..." (6). There were three areas where the contributions of women chemists were of particular importance: employment in analytical laboratories; the filling of academic posts vacated by men drafted for war work or for military service; and employment in the synthesis of essential chemicals.

Analytical Chemistry

Throughout the War there was a demand for analytical chemists: some to determine purities of explosives and of their precursors; and others to analyze samples of the iron and steel used in the production of military items, such as ships and tanks. It is not surprising, then, that Sheffield, the center of the British steel industry, became the focus for training of women for this latter purpose. In a response to Conway, Fred K. Knowles of the Faculty of Metallurgy at the University of Sheffield noted that when the war started, the men in the analytical and research laboratories of the industry were barred from joining the armed forces, because of the essential nature of their occupation (7). By the Autumn of 1916, however, the demand for "cannon fodder" became so great that even these individuals were drafted. Knowles continued (7):

In these laboratories there is a large amount of routine repetition work which can be carried out by semi-trained assistants, as distinct from chemists and physicists. To meet this emergency, special one month Intensive Courses for Women were started in the Metallurgical Department of the Faculty of Applied Science, University of Sheffield: the aim being to give a training in accurate weighing, filtration, titration, general manipulation and calculations. At the end of the Course those students who passed an Examination in the rapid determination of the elements:- carbon, silicon, manganese, sulfur, phosphorus, readily found remunerative employment. The Classes commenced on the 6th November 1916, and continued practically for 2 full University years: during this time 96 women students entered for this work.

Sheffield also provided specialized courses in other areas. For example, six women were trained as analysts for coke oven laboratories (8).

Some of the steel companies welcomed the women analysts. The Chief Supervisor of the Women's Welfare Department of Thos. Frith and Sons Ltd. of Sheffield wrote to Conway to inform her that four women had worked in the research laboratory and sixteen in the general laboratory at the company, primarily on the analysis of iron and steels and in microphotography. He added (9):

I understand that this Firm was one of the first (if not the first) in the Country to employ women at such work and the results have been quite satisfactory to the Heads of the two Laboratories.

Not all companies were effusive in their praise of women chemists. William Rintoul of Nobel Explosives Company in Ayreshire reported to Conway (10):

Only routine work was entrusted to women. Our experience agrees with the generally accepted view that, in the main, women are unsuitable for the control and carrying out of research work unless under strict supervision.

At the National Physical Laboratory (NPL), too, women chemists played their part, again mainly in the analysis of iron and steel samples for the Admiralty. It is noticeable, though, that 10 of the 12 Junior Assistants at the NPL were female, while all of the Assistants, the Senior Assistants, and the Supervisor, were male (11). The reports listing women's contributions, such as that of the NPL, provide only names and assigned duties. For many of the women chemists, little other information exists. We do know more about one analyst, Ada Hitchins (12). Hitchins, a graduate of the University of Glasgow, became the stalwart research assistant to Frederick Soddy,

working with him from 1913 until 1927, except for the period from 1916 until 1921. It was in September 1916 that Hitchins left Aberdeen (where Soddy was at the time) to undertake war service in the Admiralty Steel Analysis Laboratories. When the former male occupants of the analytical laboratories returned upon the end of hostilities, Hitchins lost her position. However, the wartime analytical experience enabled her to find employment as a chemist with a Sheffield steel works until Soddy, then at Oxford, obtained funding to rehire her (13).

Academic Staff Positions

Though some especially talented women had obtained junior academic positions, such as demonstrator, prior to the outbreak of war, it was the war itself that opened up the possibility of academic advancement (14). The best documented example is that of the University of Sheffield. By 1915, Professor William Palmer Wynne (15) was the sole remaining faculty member of the chemistry department, the others having departed for war work. Wynne hired Emily G. Turner and Dorothy M. Bennett as Assistant Lecturers and Demonstrators; and Annie M. Mathews, as Demonstrator and Lecture Assistant (16). All of the women held M.Sc. degrees in chemistry from Sheffield. In 1918, Mathews left to be replaced by May Walsh for the 1918-19 year. Bennett and Turner were known as the "Tartrate Twins." Though none of the former Sheffield students that we contacted (17) were able to recall the reason for this appellation, it was probably due to the fact that they worked so much as a team, they were like mirror images. In contrast to the hiring of most women, whose positions terminated after the war, the "twins" continued in their posts. Their major responsibility was teaching, but Turner was able to co-author three research papers, two with Wynne and one with G. M. Bennett. Turner, born in 1888, stayed as Assistant Lecturer until 1952 and died in 1958. Bennett resigned her academic position in 1934 in order to marry (becoming Mrs. Leighton), but she kept her posts as Tutor for women students (1926-1947) and Warden of University Hall (1936-1947). She died on May 11, 1984, twelve days after her 100th birthday (18).

At East London College (later Queen Mary College), it was Kathleen Balls, B.Sc. (later Mrs. Stratton), who as Lecturer and Demonstrator in Chemistry enabled courses to be run through the war period (19). She and the Head, F. G. Pope, were the only members of the Chemistry Department during part of that time. At the outbreak of war, Balls was a high school science teacher, but the greater urgency for academic staff led to her re-

lease from the County School, Enfield, for the duration of the war (20). In fact, her appointment at the College stipulated the occupancy of the position as being for the length of hostilities only. Despite this limitation, she actually continued until 1924, when she submitted her resignation (21). Though the resignation was accepted, she was asked to continue as Lady Superintendent at the College. It is probable that Balls did accept, for she certainly continued to be active in chemistry at Queen Mary College, co-authoring three publications with J. R. Partington (22) between 1922 and 1936. In addition, Balls and Partington co-authored a book on chemical calculations (23).

Organic Synthesis

Prior to the First World War, the Allied nations had relied heavily on the German chemical industries for their pharmaceuticals and other fine chemicals. With the supply cut off as hostilities began, there was an urgent need to produce drugs and medications. Until proper chemical facilities could be built, a committee of the Royal Society was set up to coordinate production of these chemicals. Arthur Schuster, on behalf of the Committee, contacted all of the chemistry departments of British universities, asking them to contribute to the production of the relevant organic chemicals (24). As late in the war as 1917, the universities were still supplying chemicals for the war effort, including β -eucaine, arabinose, atropine, and butyl chloral hydrate (25).

It will probably come as no surprise to the reader of this study that the University of Sheffield was one of the participants in the organic synthesis. Wynne assembled a team of six women chemists, including Turner,

Mathews, and Walsh, to synthesize β -eucaine (16). However, the most noteworthy production was that at the Imperial College of Science, London University, in the group run by Martha Whiteley, whose biography has recently been described in this *Bulletin* (26). Whiteley was another of the women who benefited career-wise from the First World War, becoming lecturer at Imperial College in 1914. As well as the production of β -eucaine, the group also undertook research for the chemical warfare department of the Ministry of Munitions for War (27). Whiteley's seven assistants, all women, included Frances M. G. Mickelthwait, who received an M.B.E. for her contributions to the war effort (28). Mickelthwait was born in 1868 and, after a private education, attended the Swanley Horticultural College, where she gained her love of chemistry. In 1898 she attended the Royal College of Science, obtaining an Associateship in 1901. She

continued as a research student in organic chemistry until the war, being one of the most prolific women authors of chemistry publications of her time. At the outbreak of war, she came under the wing of Whiteley. After the war, she worked briefly in the research laboratory of Boots Pure Drug Company and then returned to Swanley Horticultural College, where she taught until 1921. From then until 1927, she compiled the index for the second edition



The University of Sheffield β -eucaine Team, 1915-16. Front row (left to right): M. Walsh, two unidentified women assistants, E. Foster. Second row: E.G. Turner, Professor W.P. Wynne, A. Matthews. Photo credit: Chris Lumley, University of Sheffield.

of *Thorpe's Dictionary of Applied Chemistry*, a series co-edited by her friend, Whiteley. She died on March 25, 1950 at the age of 83.

Even small colleges took part in the synthesis program. One of these was the University College of Wales, Aberystwyth, where much of the work was performed by Margaret K. Turner. Turner, too, had been hired at

the beginning of the war, in her case, with the rank of Demonstrator (29). She wrote a stirring letter to the War Committee, volunteering for additional duties (30):

I was one of the workers in the preparation of diethylamine some weeks ago and should be very glad to hear of any further help I could give. I can put all my time and energy at your service for the next 6 weeks, and am anxious to know whether the few helpers down here could not be allowed to contribute further to the needs of the country? I should be much obliged if you would inform me whether there is any other preparations we can make, as I, for one, am willing and eager to give up all ideas of holiday while there remains so much to be done .

Of all the women chemists, Phyllis Violet McKie of the University College of Wales, Bangor, seems to have been the most productive during the war period. McKie was part of the team at Bangor producing paraldehyde (31). In addition, she authored and co-authored a number of studies for the war effort, including a new method for the preparation of the explosive tetranitromethane for the Ministry of Munitions and a study of methods of preparation of saccharin and vanillin for war purposes (32). Unfortunately, we have been unable to find any information on McKie's later life.

Millicent Taylor of the Cheltenham Ladies' College was another contributor to the war effort. Taylor (33), born in October 1871, attended the Ladies' College, Cheltenham, between 1888 and 1893. It was from Cheltenham that she obtained an external B.Sc. (London) in 1893, the same year that she was appointed to the staff at the College. The following year, Taylor was made Head of the Chemistry Department, and then Head of the Science Department in 1911, a position that she held until 1919. Between 1898 and 1910 she devoted most of her spare time to research work in organic and physical chemistry at the University College of Bristol (now the University of Bristol), producing a range of papers in those fields. On weekends, she would often cycle the eighty-mile round trip (34). She received an M.Sc.(Bristol) in 1910 and a D.Sc.(Bristol) in 1911. During the war she was involved in production of b-eucaine and then, in 1917, she was appointed a research chemist at H.M. Factory, Oldbury. In 1919 she returned briefly to her post at Cheltenham but left to accept an appointment as Demonstrator in Chemistry at the University of Bristol in 1921. In 1923 she was promoted to Lecturer, a position that she held until her retirement in 1937. But this was not the end for Taylor. Upon retirement, she was given the use of a small laboratory in an army hut on the grounds of the Bristol Chemical build-

ings. She continued research in this personal laboratory until her death in December 1960, at the age of 89.

Finally, we should mention the University, St. Andrews, Scotland. The women chemists at St. Andrews worked on the production of synthetic drugs and bacteriological sugars, research on explosives and poison gases, and the improvement of industrial processes. The report sent to Conway noted that (35):

It should be stated that the whole of this work was unpaid from Government sources, the workers receiving only their University salaries, in cases where they were members of staff, or the value of their Scholarships, if they held any such distinctions. Not only so, but the demand for chemists throughout the war was continuous, so that the workers who remained with me gave up many opportunities for professional advancement. I mention these facts as an index of public spirit with which these women gave their services, services which have not received any public recognition.

Other Duties

Just as male faculty members were drafted for war duties, so were most male graduate students. Thus for the duration of the war, many researchers depended upon women students for the maintenance of their activities. For example, at the University of Edinburgh, Charles Barkla, the X-ray spectroscopist and 1917 Nobel Laureate, relied on Margaret Pirie White (Mrs. Dunbar) and Janette Gilchrist Dunlop for the continuation of his work (36). He later lost the services of Dunlop, who was "compelled by war-time teacher shortage" to become a high school teacher of mathematics and science (37). Ruth King was another wartime researcher. She was born on May 13, 1894 and graduated from East London College in 1914 (38). She was hired by the organic chemist, J. T. Hewitt, to study the synthesis of picric acid (39). At the conclusion of the war, she was appointed Lecturer in Organic Chemistry at the University College of South West England (later the University of Exeter) as well as Warden of Hope Hall, the women's residence (40). Like the other women chemists given academic positions, she was assigned a high proportion of the teaching duties. In fact, from 1919 to 1945 she was the only organic chemist in the department. King stayed at Exeter until 1955, when she took early retirement to move to Canada to help care for her aged mother. The mother died while King was crossing the Atlantic; but, undaunted, she continued to Vancouver, where she obtained a post as Lecturer at the University of British Columbia and remained until 1961.

Some women chemists were assigned specific tasks necessary for the war effort. For example, May Sybil Leslie, a graduate of the University of Leeds and a former researcher with Marie Curie (1909-11) and Ernest Rutherford (1911-12), was appointed in 1916 as a research chemist at His Majesty's Factory in Litherland, Liverpool (41). The following year she was promoted to Chief of Laboratory, having been given the task of improving the synthesis of nitric acid, a vital reagent in explosives production. Her wartime contributions were highly valued and resulted in the award of a D.Sc. degree by the University of Leeds in 1918. In the citation, it was noted that (41):

The problems she has had to solve are not only of the first importance at the present time, but have been attacked in a manner showing unusual resource as well as novel methods of procedure.

The biochemist Dorothy Jordan Lloyd, a researcher with F. Gowland Hopkins at Cambridge, was also given a specific task. On the outbreak of war, the Medical Research Committee assigned her the study of culture media for meningococcus, one of the anaerobic pathogens involved in trench diseases, and of causes and prevention of "ropiness" in bread (42). Another "draftee" chemist was Nora Renouf, a research fellow with Professor A. W. Crossley at the research laboratory of the Pharmaceutical Society. She spent the war period as a survey officer with the Fuel Research Board (32).

Many women researchers in radioactivity were drafted to operate X-ray machines at the war front. Thus Marie Curie and Irène Joliot-Curie joined the service of the French forces (43) while Lise Meitner volunteered as an X-ray nurse with the Austrian army (44). A British example was Jesse Slater, assistant lecturer in chemistry and physics at Newnham College and former researcher with J. J. Thomson. After working initially as a nurse, she was called for full-time duty as a radiographer at British military hospitals in France and later held the rank of *Officier de l'Instruction Publique* with the French army (13).

The bacterial biochemist Marjorie Stephenson took up the traditional role of nursing (45). She left her research position at University College, London, to join the British Red Cross in France and then Salonika, where she was in charge of a nurses' convalescent home and also had responsibilities for invalid diets. She was mentioned in dispatches in 1917 and awarded an M.B.E. for her war work.

The End of the War

With few exceptions, the end of the war resulted in the termination of employment for women chemists (46). The government closed the explosives factories, while the male chemists returned from their war duties and reoccupied their former faculty and research positions. The women chemists with specialized training stood the best chance of survival: for example, according to Knowles' letter to Conway, the graduates of the metallurgical analysis course at Sheffield seemed to survive (7):

That women have been an undoubted success in this branch of industry, is proved by the fact that notwithstanding so many of the men (who are now demobilized) have resumed duty, a large proportion of the women who desired to stay on have retained their positions to the present time.

This was a different opinion from that of the respondent from the Sheffield Steel Company of Thos. Frith who noted (9):

On the signing of the Armistice most of the women were replaced by returning soldiers, but two [of 16] in the General Laboratory have become so proficient that their services have been retained.

Most responses pointed to bleak post-war opportunities for women chemists in industry as Dorothy Adams described succinctly in a letter to Conway (47):

With regard to the prospects of scientifically trained women after the war my experience has led me to the conclusion that there will be practically no scope for them in industry. There is, and will continue to be for some time, a far larger supply of male Chemists than will be needed. Under such circumstances women with the same qualifications will stand the poorest chances of employment. As teachers and lecturers there is still some demand for such women, but in industry there is next to none. I have been led to this conclusion by my experience in endeavouring to obtain a fresh post myself. I do not stand alone in my opinion, Mr. Pilcher, the Registrar of the Institute of Chemistry whom I consulted on the subject told me exactly the same things as I learnt later from my own experience.

Her fears proved to be justified as the biochemist Kathleen Culhane (Mrs. Lathbury) discovered in 1922 (48). Culhane was offered interviews with biochemical companies only when she signed her application letters "K. Culhane;" and once her gender was revealed at the interview, she was denied the position. In fact, the lack

of employment prospects for women scientists became one of the reasons for the slump in women's enrollment in university science programs during the late 1920s and the 1930s (5).

In conclusion, the First World War provided a wider variety and a much larger number of employment prospects for British women chemists than they had experienced before, though they have been forgotten in most historical accounts. Unfortunately, with the closure of war-related factories and the return of male chemists from their war duties, most of the opportunities vanished; and it was to be the Second World War before the situation improved (5, 49).

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2. Neither paper cited in (1) discusses the role of women chemists, though Macleod mentions one woman in a passing comment "And so, too, left without an offer from the [Reserved Occupations] Committee, the only woman of whom we have record, Margaret Turner, of the Chemical Laboratories at Aberystwyth." In a history of early twentieth-century chemical industry, Haber notes that in Britain "The number of women in the chemical trades ... rose from 17 000 to 25 000 and their relative importance from 14 to 20 per cent between 1907 and 1924. At the former date they were especially prominent in the match and explosives trades, at the latter they also formed a large minority of the sector comprising chemicals, dyes, and drugs, specifically of the expanding pharmaceutical preparation business. Women did not become important in the offices and laboratories of chemical manufacturers until the war" [L. F. Haber, *The Chemical Industry 1900-1930*, Clarendon Press, Oxford, UK, 1971, 378].
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Professor David Knight, Durham, University, "Theoretical Improvements Accompanied by Practical Advantages: Rumford, Banks, Davy"

ERLICH, BERTHEIM, AND ATOXYL: THE ORIGINS OF MODERN CHEMOTHERAPY *

Steven Riethmiller, Virginia Military Institute

“Statistics teach that one-seventh of all human beings die of tuberculosis, and that, if one considers only the productive middle-age groups, tuberculosis carries away one-third and often more of these” (1). So stated Robert Koch (1843-1910) when he presented his paper on the etiology of tuberculosis to the Physiological Society in Berlin in 1882. It is difficult for us, at the end of the twentieth century, to understand the impact of infectious disease on humanity at the end of the nineteenth century. At that time the number of effective drugs was very small, digitalis for heart problems and morphine for pain but for infectious diseases, with the exception of quinine for malaria, there were basically none. The treatment and control of infectious diseases is, arguably, the most outstanding scientific and humanitarian achievement of the twentieth century. The untold

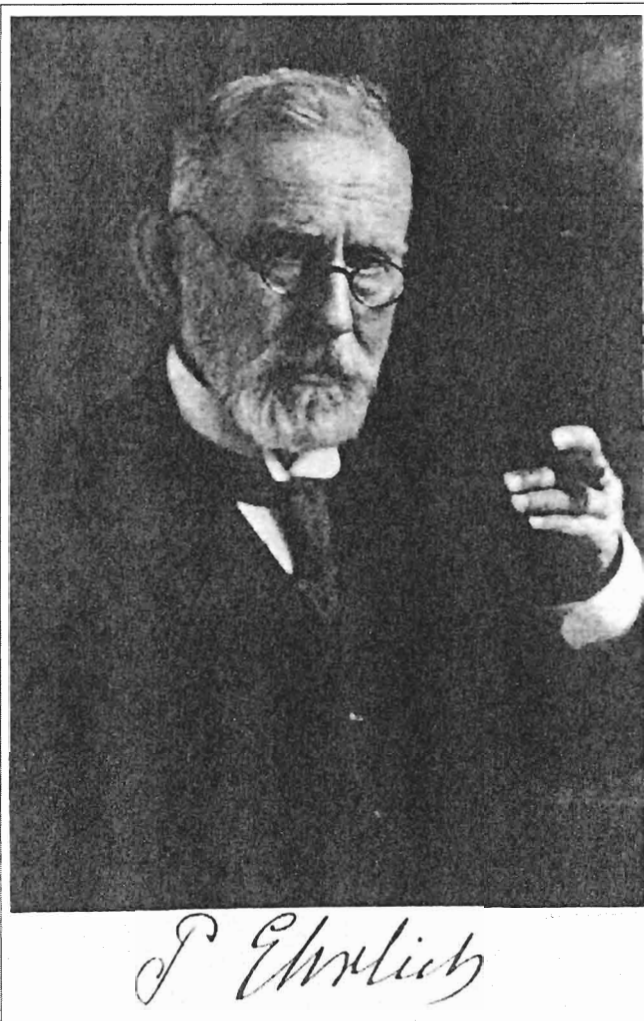


Figure 1. Paul Ehrlich (1854-1915). (Courtesy of the Rockefeller Archive Center.)

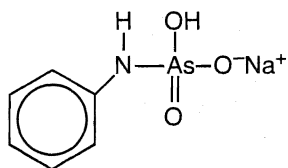
death, pain and misery caused by just two, tuberculosis and syphilis, is difficult for us to comprehend today. Perhaps nothing in the history of mankind has so affected the quality and quantity of life for all people as have chemical therapeutics. The genesis for this great humanitarian and scientific achievement has its roots in nineteenth century chemistry.

Paul Ehrlich (1854-1915), (Fig. 1), is the father of chemotherapy; in fact he coined the word and defined it as “the use of drugs to injure an invading organism without injury to the host(2).” Ehrlich was born in Strehlen, in what was then the Prussian state of Silesia. He studied medicine and pathology in Breslau and earned his MD degree from the University of Leipzig in 1878. His MD thesis was entitled “Contributions to the Theory and Practice of Histological Staining,” indicative of his

early and lifelong interest in the use of dyes to stain bacteria. Carl Weigert (1845-1910), Ehrlich's cousin and noted pathologist, had interested Ehrlich in bacteriology and the use of aniline and other dyes to stain bacteria (3). The idea of staining and perhaps killing bacteria with aniline or other dyes was a seed planted early in Ehrlich's professional life. After receiving his MD degree he worked for ten years in Berlin's Charité hospital and between the years of 1891-1899 he worked with Robert Koch in his Institute for Infectious Diseases also in Berlin. It was during this time that he developed methods for evaluating and standardizing the diphtheria antitoxin which led to his sharing the Nobel Prize in Medicine with Metchnikoff in 1908.

In 1899 Ehrlich moved to Frankfurt, where he would spend the rest of his life, and to the Royal Institute for Experimental Therapy. He was to remain at the Institute until 1906. During this period several key discoveries, which would play a fundamental role in the genesis of chemotherapy, were made. In 1903 Bruce discovered that a trypanosome was the cause of African sleeping sickness, in 1905 Schaudinn discovered that a spirochete caused syphilis. Also in 1905 Thomas discovered that an organic arsenic compound, "atoxyl," was effective against trypanosomes. This compound had been synthesized some forty years earlier by the French physician/chemist Béchamp.

Pierre J. A. Béchamp (1816-1908), (Fig. 2), was an interesting and controversial scientist. Born in the Moselle region of France, he held a doctorate in both medicine and the physical sciences. In 1852 he developed a cheap method of making aniline from the reduction of nitrobenzene and in 1863, while teaching medical chemistry at the University of Montpellier, he synthesized a compound from aniline and arsenic acid (4). This organic arsenical became



Atoxyl, according to Béchamp

known later on, because of its decreased toxicity to animal forms of life, as "atoxyl." Béchamp characterized this compound as an anilide and described some of its chemistry; however, interest in his *anilide de l'acide arsenique* languished until the turn of the twentieth century.

The use of arsenic as a medicinal drug had been known for centuries. As early as the fifth century B.C.,

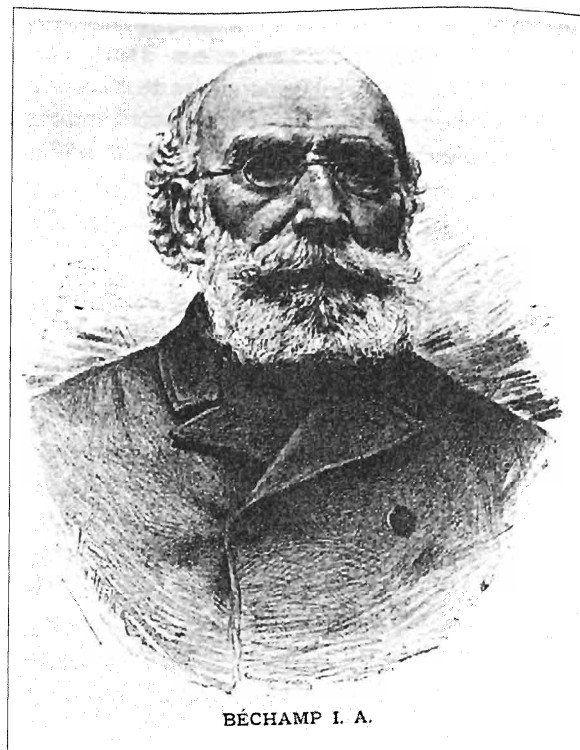


Figure 2. Antoine Béchamp (1816-1908). (Courtesy of Archives de l'Academie des Sciences, Paris.)

Hippocrates recommended using arsenic trisulfide for abscesses and when syphilis first appeared in Europe in the end of the fifteenth century it was only natural to try some of these inorganic compounds of arsenic. The high toxicity of these inorganic preparations precluded widespread usage (5). In the eighteenth and nineteenth century the work of de Gassicourt (6) and Bunsen (7) led to the discovery of organic arsenic compounds. Forty two years after Beuchamp first announced his aniline and arsenic preparation, Thomas showed that "atoxyl" was effective in the treatment of trypanosomiasis (8). *Trypanosoma brucei gambiense*, is the cause of African sleeping sickness and was at one time Africa's number one health problem. In the years around 1900 an epidemic in the Belgian Congo killed half a million people (9).

Thomas's paper apparently peaked Ehrlich's interest for he wrote to Karl Herxheimer (1861-1942), Professor of Dermatology at the University of Frankfurt(10):

August 29, 1905, Please be good enough to send me the paper on atoxyl, since I would like to read it in the original.

Thus began, with this simple inquiry, modern chemotherapy. Ehrlich became the first person to elucidate the correct chemical structure of a therapeutically ac-

tive compound and then set out to modify this structure to improve its medicinal effectiveness. There had been earlier attempts to correlate structure to effectiveness (11) but Ehrlich was the first to understand and exploit this idea. There is very little mention of atoxyl again until Ehrlich wrote to Julius von Braun, a Privatdocent specializing in organic chemistry at Göttingen(12):

November 7, 1905, Dr. Bertheim, who has given up his position at the factory, has in the meantime arrived here, and since he is free, he can start to work and be of help to me, which is very welcomed.

This is the first mention of Alfred Bertheim (1879-1914), Ph.D., Berlin in 1901. He worked as a manufacturing chemist until joining Ehrlich. After joining Ehrlich in 1905 he remained with him until the outbreak of World War I; joining the German army he was killed in an accident in Berlin on August 17, 1914 (13). Bertheim, either on his own or under Ehrlich's direction, discov-

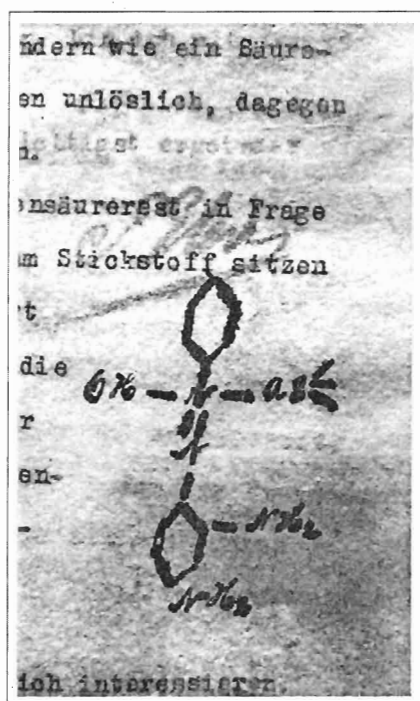


Figure 3. Sketch in a letter from Ehrlich to von Braun, November 7, 1905, Copirbuch XVIII, 186-188, box 24, Ehrlich Collection. (Courtesy of the Rockefeller Archive Center.)

ered the true nature of atoxyl. This seminal discovery led to the first truly effective therapeutic agent in the fight against syphilis. In this same letter to von Braun (12), Ehrlich discussed the chemistry of atoxyl and specifically referred to it as "arsenic acid anilide." He goes on to say (12):

I have found that if one treats atoxyl with nitrous acid one gets a diazo compound that must contain the arsenic residue. This compound couples easily to produce dyes that still must contain an acid residue. Thus, for example, the coupling product with toluylaminodiamine does not behave like the diamidoazobenzol but like an acid derivative. It is insoluble in weak acids, but easily soluble in alkalis.

He then sketched the structural formula in his copy book as he believed it to be (Fig. 3). So, on the day that he wrote of the arrival of Alfred Bertheim, Ehrlich still tried to rationalize the structure of atoxyl as that of an anilide. Since diazotization should only be possible for primary aromatic amines and not for anilides it is difficult to understand how Ehrlich could have continued to persist in this idea. Perhaps it was because he was described as a "self taught chemical investigator(14)." Scarcely one week later and only one week after Bertheim had been on the job, he again wrote to von Braun (15):

November 14, 1905, one must consider if the constitution of the atoxyl is really correct, or if perhaps it is not in fact a paraamino derivative of benzol arsenic acid.

Almost three weeks after the arrival of Alfred Bertheim, Ehrlich wrote to Ludwig Darmstaedter, a trained chemist and one who helped Ehrlich raise research funds (16):

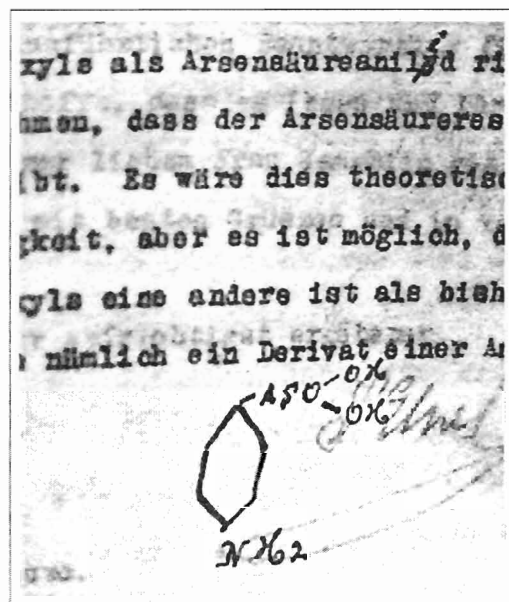


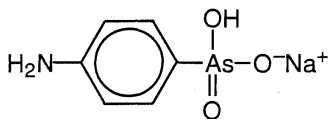
Figure 4. Sketch in a letter from Ehrlich to Darmstaedter, November 25, 1905, Copirbuch XVIII, 303-305, Ehrlich Collection. (Courtesy of the Rockefeller Archive Center.)

November 25, 1905, either the structure of atoxyl as an arsenic acid anilide is correct, in which case one would need to propose that the arsenic acid residue remains attached to the azo group. Theoretically this would be of the greatest importance, but it is possible that the structure of atoxyl is different than assumed until now, and that it is really a derivative of aminophenyl arsenic acid.



Figure 5. Copirbuch XVIII, box 24, Ehrlich Collection. (Courtesy of the Rockefeller Archive Center.)

He then sketched the proposed structure in his copy book as an amino phenylarsenic acid. (Fig. 4). Ehrlich dictated some of his voluminous correspondence to his secretary, which she typed and placed in copy books (Copirbuch in German). The cover of copy book number XVIII is shown in Fig. 5. From that time on there seemed to be no question that atoxyl was not an anilide but an arsenic acid, and he and Bertheim published a paper to that effect in 1907 (17).



Atoxyl, according to Ehrlich and Bertheim

Interestingly, this point, i.e. who discovered the true nature of atoxyl, is a most important one and was made into a major bone of contention both in the film (18) made about Ehrlich and the biography written about Ehrlich by his former secretary, Martha Marquardt (19). In one scene in the movie Ehrlich is talking to his three chemists (von Braun, Schmitz, and Bertheim) and telling them to proceed with their work as if atoxyl is an amino acid and not an anilide. Two of the three quit then and there and only Bertheim, meekly, says he will stay. The Marquardt biography describes an even more contentious confrontation (20):

Ehrlich declared firmly to the three chemical workers at the Georg Speyer Haus: 'Atoxyl is *not* an anilide

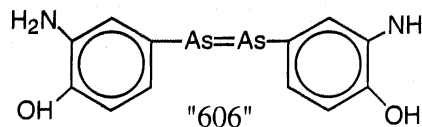
of arsenic acid. On the contrary, it contains a free amino group; I have worked on the arsenic azo-dyes, which can be made in consequence of this, for some considerable time'.

She then continues to describe the scene between Ehrlich and his three chemists which results in von Braun and Schmitz's resigning with only Bertheim willing to remain. If, as Marquardt says, Ehrlich knew that atoxyl was not an anilide he did not describe it as such, according to his own copy books, until several weeks after Bertheim's appearance in his laboratory. The facts, as revealed in Ehrlich's copy books, are on November 7, 1905, Ehrlich thinks atoxyl is an anilide and Bertheim joins him and begins working on the problem. On November 14, 1905, Ehrlich questions whether atoxyl is an anilide and finally on November 25, 1905, almost three weeks after Bertheim's arrival he draws the correct structure of atoxyl. This is well before the summer of 1906 and the famous confrontation described by Marquardt. Is this just a coincidence or did Bertheim, after hearing the facts, suggest to Ehrlich in November, 1905 that if atoxyl could be diazotized it could not be an anilide and therefore was probably an amino phenylarsenic acid? We shall never know but the elucidation of this critical fact, i.e. that atoxyl is indeed an phenylamino arsenic acid and not an anilide allowed Ehrlich and Bertheim to proceed with the synthesis of many variants of the atoxyl structure. That this was the critical discovery in the synthesis of therapeutic arsenicals was evidenced in a commemorative address made later by Bertheim (21):

A readily cleavable anilide held out no promise chemically, although a stable p-amino-phenylarsenic acid could be expected to combine in itself the numerous reactions of aniline with that of the arsenic acids and would also possess special properties deriving from the combination of both. Probably for the first time, therefore, a biologically effective substance existed whose structure was not only known precisely but also-unlike the alkaloids-was of a simple composition and extraordinary reactivity, which permitted a wide variety of modifications.

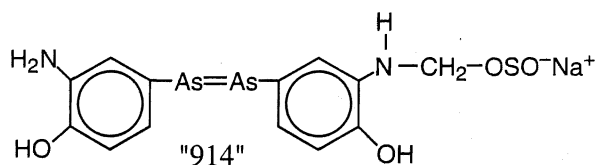
The last sentence in this statement, underlined for emphasis, is particularly important in that it points out that the genesis of modern chemical therapeutics began with Ehrlich and Bertheim. After this Ehrlich and Bertheim began a program of synthesizing various modifications of the atoxyl molecule. Eventually with the 606th compound of this series, they came upon a compound which was very effective against syphilis and was the first major triumph in the synthesis of chemotherapeutic agents.

This compound, arsphenamine or Salvarsan, was introduced into general usage in 1910.



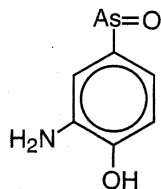
Salvarsan, arsphenamine

This compound brought world wide fame and recognition to Ehrlich. However, problems with making an injectable solution of it caused Ehrlich to seek an improved version. This resulted in the water-soluble variant Neosalvarsan.



Neosalvarsan

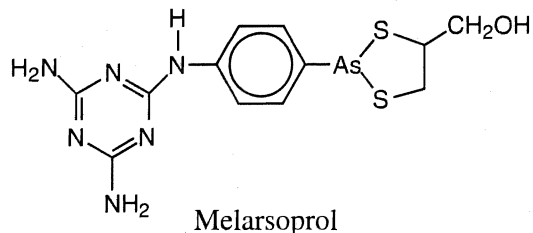
Interestingly, doubt about the proposed As=As bond in these molecules arose as early as 1921, when it was found that the per cent arsenic did not conform to the structure. Later work convincingly showed that neither Salvarsan nor Neosalvarsan was a pure substance and suggested that they were mixtures of polymeric materials containing only arsenic-arsenic single bonds. There is no evidence that an As=As bond exists in either of these compounds (22). By the 1930s it was recognized that the arsenoso relative:



Oxophenarsine, mapharsen

was really the active ingredient and, under the trade name of Mapharsen, was the drug of choice in the treatment of syphilis until the advent of penicillin in the early 1940s. Ironically, this compound had been synthesized in Ehrlich's laboratory early on and listed as compound number 5 but was thought to be too toxic to be of use (23). Even though research into arsenicals has ceased, Paul Ehrlich and Alfred Bertheim's legacy of chemical therapeutic arsenic drugs lives on. Recently an article

detailed how Melarsoprol, a compound closely related to atoxyl:



Melarsoprol

is still being used to treat sleeping sickness (9).

ACKNOWLEDGMENT

I would like to thank the Dean of VMI and the VMI Research Committee for their support of this research. I would also like to thank Dr. David DuPuy of the VMI Physics Department for his help with the computer images and Dr. Lee Hiltzik of the Rockefeller Archives Center for his help with the Ehrlich collection.

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ACADEMIA AND INDUSTRY: WHAT SHOULD THEIR RELATIONSHIP BE? THE LEVINSTEIN-ROSCOE DIALOG

Martin D. Saltzman, Providence College

What should be the proper relationship between the practitioners of pure academic science and those involved in the application of science?

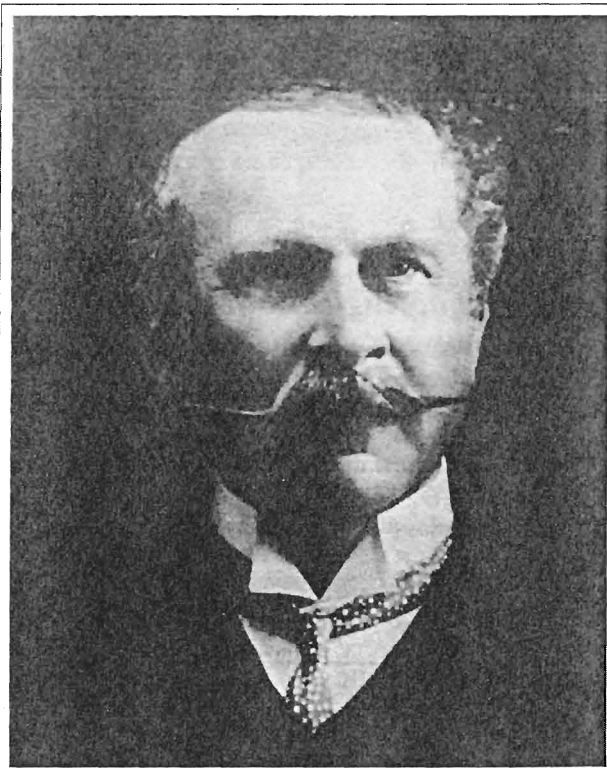
Should there be an active partnership or should applied science be of no concern at all to the academic scientist? This is an age old question which still has not been satisfactorily answered today. As we enter the 21st century are lessons of the past still relevant? In this context, it is worth considering the first major debate on the relationship of academic chemistry and industrial. This took place at the end of the 19th century and focused on the perceived needs of the British synthetic dyestuff industry, which was then in decline(1). The debate was carried on between Ivan Levinstein (1845-1916)(2) representing applied chemistry and Henry Enfield Roscoe (1833-1915)(3) representing pure chemistry.

The British synthetic dye industry had been founded in England following the discovery of the aniline dye mauve in 1856 by William Henry Perkin (1838-1907). This also marked the beginning of the modern synthetic organic chemical in-

dustry. Yet within two decades it was totally eclipsed by the German chemical industry. What led to this decline?

The reasons were complex and many(4), but for Ivan Levinstein, one of the few British survivors (despite his German origins), one of the most important was the lack of cooperation of industry and academia. Some recent observers have seen this as the culmination of a trend that had occurred in Britain with the professionalization of chemistry, begun in the 1840's with the founding of the Chemical Society (1841) and the Royal College of Chemistry in London(1845)(5). Initially there was a very visible connection between pure and applied chemistry. Perkin had studied at the Royal College for three years, and he received training in theory as well as research experience under the tutelage of A. W. Hofmann (1818-1892)(6). This was in-

strumental in his being able to make his laboratory discovery and successfully commercialize it within a very short time span.



Ivan Levinstein

A common theme in chemical instruction as it developed in Britain was that theoretical or pure chemistry had to be taught first, and then more practical or applied chemistry would follow at the end of the course of studies. By the 1870's the academic study of chemistry had almost completely displaced applied chemistry from the curriculum. The commonly accepted wisdom was that the attainment of a knowledge of pure chemistry would be enough to make it possible for anyone to apply it to industrial situations. Academic chemists had developed their own "industry" of training students and teachers and ignored the needs of the chemical industry for the most part. Numerous British chemists obtained Ph.D. degrees at the German research institutes and universities(7) beginning in the 1840's and, on their return, brought an emphasis on pure chemical research to the developing universities. This helped displace any remnants of applied chemistry from the university curriculum. The importance of a continuing relationship between academia and industry seemed to most British academics to be of little concern. Though many served as consultants, few if any connected these activities with their own research program.

By the 1860's the center of the British dye industry had moved from the London area where Perkin had established it, northward to Manchester(8) and Huddersfield, which were closer to the textile industry of Lancashire and Yorkshire. As Manchester had already grown in importance as an industrial center, and an institution of higher learning Owens College had been founded in 1851 to fulfill the perceived need for technical training. Owens would serve as the model for many of the civic colleges founded in the next few decades in major British cities. Chemistry became one of the most important disciplines at Owens, and after a keen competition, Edward Frankland (1825-1899)(9) was chosen as the first Professor of Chemistry. Frankland had impeccable credentials for the appointment, having studied with Robert Bunsen at Marburg and Justus Liebig at Giessen, and already had published a considerable body of research. In his inaugural address Frankland stressed that chemistry was an important mental discipline in and itself and would equip the holder of a certificate of proficiency for many other occupations. Frankland stressed the practical aspects of chemical training with respect to the situation in Manchester(10).

The advantages of chemistry to the chemical manufacturer, the dyer and the calico printer are almost too obvious to require comment...It is now an acknowledged fact that these processes cannot be carried on without some knowledge of our science, yet with the exception



Sir Henry Roscoe

of some few firms who have not the aid and co-operation of distinguished chemists, this knowledge is too often only superficial and sufficient to prevent egregious blunders and ruinous losses, but inadequate to seize upon and turn to advantage the numerous hints which are almost sure to be constantly furnished in all manufacturing processes. It is well known how many valuable discoveries of the highest practical importance have been made by the acute observation of a single minute phenomenon exhibiting itself during a manufacturing process and which would perhaps never have come to the cognizance of any one if the intelligent and scientific conductor of the process had not at once comprehended the reaction and chronicled the fact.

In Frankland's mind pure science and practical chemistry were mutually compatible and not exclusive of each other. Frankland left Owens in 1857 because of the uncertainty of the survival of the college. His successor was Henry Enfield Roscoe, a Liverpool native who had studied at University College, London, and obtained his Ph.D. with Bunsen at Marburg in 1853. Returning to London, Roscoe worked as a part-time teacher and consultant until his appointment to Owens in 1857. Roscoe was probably the person most responsible for the reversal of fortune at Owens in the succeeding years and the evolution of Owens into the

nucleus of the University of Manchester. Roscoe's goals can be best appreciated from this passage from his autobiography(11):

Public opinion in Manchester at that time did not appreciate the value of the higher education, and it was not understood that science could be made an efficient instrument of education, and that such an education was absolutely necessary for an industrial career. To make a school of chemistry worthy of the great manufacturing districts of South Lancashire was my ambition, and after thirty years of work I think it must be admitted that this was, to some extent at least, realized.

Robert Kargon has written of Roscoe(12):

Roscoe's own combination of energy, enthusiasm, and political skill which thrust him to the forefront of Owens professors in reviving the College. This energy and enthusiasm rubbed off on his students and the community at large where he acted as a consultant on matters of health and waste management to manufacturers.

In theory at least Roscoe maintained a strong belief that there must be a union between the science of chemistry and the practice of chemistry. However, this was not to be construed to mean that Owens College was to be a vocational school(13):

.....to the practical man the youths trained in the Chemical School of Owens College were able not only to take a more intelligent part in the operations of the various manufacturers than those who had not had such advantages, but that this education had given insight into these processes such that those thus trained were able to effect improvements or even to make discoveries of importance.

Levinstein and the Dye Industry

Ivan Levinstein was the eighth son of Levin Jacob Levinstein (1803-1865), a Berlin merchant. Four of his sons, Hugo (1832-1878), Gustav (1842-1910),

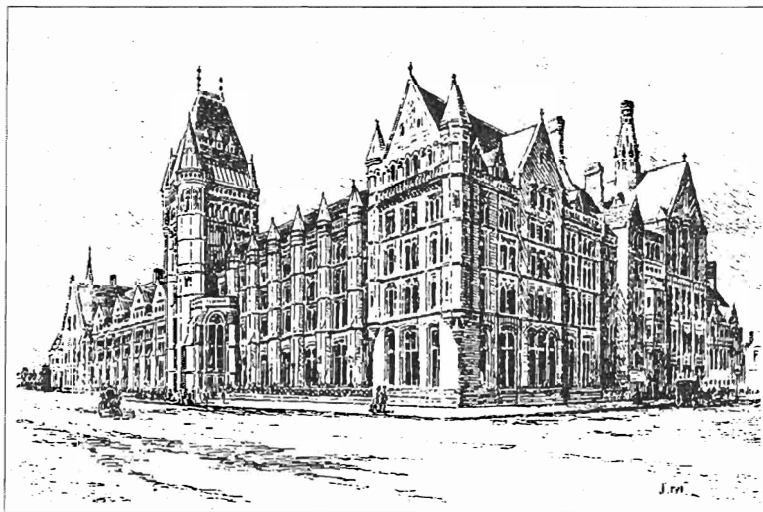
Alexander (1843-?), and Ivan would become involved in the dye business in some capacity in England during their lifetimes. Of all the sons, only Ivan had any formal chemical training. After completing secondary school studies at the Royal Prussian Gymnasium in Berlin, he studied applied chemistry at the Gewerbeinstitut Berlin. Ivan's older brother Hugo was the first to emigrate to England in 1858. In London he founded the firm of Hugo Levinstein & Co. for the purpose of manufacturing and selling natural dyes and later manufacturing aniline dyes. The family's entrance into the dye business in England may have been the result of the father's declining fortunes in Germany. Greater opportunities existed now in England and in the newly developing dye industry.

By 1864 when Ivan left Germany for England, the English dye industry was probably at its zenith, although it was about to enter into a period of very rapid decline. Ivan is supposed to have remarked that his father in 1864 had already sensed that the English dye industry was going to be challenged by competition from the continent and would not likely survive this challenge. Putting

his father's advice aside, Ivan established a chemical works first in Salford in 1864, and one year later he moved to the Blackley section of Manchester. This was to become the manufacturing site of Levinstein & Co during its existence(14). The initial products manufactured by Levinstein were aniline dyes, such as aniline red or magenta and aniline blue.

As the fortunes

of the British chemical industry began to fade rapidly in the late 1870's as because of continental competition, Levinstein spoke out whenever he could as to the causes of this decline and ways to arrest it. The media used by Levinstein were his own journal *Chemical Reviews*, which he published from 1871-1891, the Society of Chemical Industry, founded in 1881(15) and its journal, and the Society of Dyers and Colourists, founded in 1884. In addition, Levinstein was an active member of the Manchester Chamber of Commerce. Levinstein



Victoria University of Manchester

analyzed the reasons and offered solutions for the problems of the synthetic chemical industry as well as the declining competitiveness of British industry in general.

The reasons for the decline were, according to Levinstein, a combination of several factors. Germany, using the pioneering models of Liebig and Wöhler, had produced numerous first-rate centers for the training of chemists. Since there was little opportunity for many of these German chemists in the fatherland, it was only natural for many of them to migrate to Britain in the 1860's. As the dye industry grew there was the need for specialized technical trained personnel which were not readily available in Britain(16).

German chemists had made significant strides in developing structural organic chemistry and aromatic chemistry in particular, which was at the heart of the dye industry in the nineteenth century. Chemists such as Heinrich Caro (1834-1910), Carl Martius (1838-1920), August Leonhardt(1827-99), and Otto Witt (1853-1915) returned to their homeland with the experience and expertise learned in Britain. They joined dye-making firms—in particular Hoechst, Agfa, BASF, and Bayer.

A major problem that accelerated the decline in Britain from about 1865 was the fragmentation of the industry following patent litigation and growing imports of German-made coal tar dyes. Levinstein remarked in 1906, "German manufacturers were in the happy position that they had the brains of the world at their disposal without paying for them...It was therefore no wonder that the German competitors, who were unfettered and unrestricted by patents or monopolies, soon made headway by selling goods at lower prices than those charged by the patent(17)." German dye companies benefited greatly from the transfer of technology from England which allowed them to get off to a very fast start in the dye business. Coupled with the intimate relationship between academia and industry as well as a supply of chemists trained in the British dye industry spelled inevitable doom for the British. In addition British patent laws did not require foreign companies to

work their patents in Britain nor were German chemical companies compelled to license their patents to British manufacturers. Thus German dye companies patented their new products in England and freely exported them to the textile centers. Patent law would only be reformed in the beginning of the 20th century, when it was too late for the chemical industry.



Aerial view of British Dyestuffs Corporation, Blackley, 1921

Levinstein attributed much of the technical advantage the Germans enjoyed to the close cooperation between academia and industry. This is clear in the dialog between Levinstein and Roscoe carried out for the most part at meetings of the Society of Chemistry and Industry. The dialog was recorded in the pages of the *Journal of the Society of Chemical Industry*. The first annual meeting of the

Society was held July 5, 1882, at Owens College. Roscoe was elected the first President of the Society. In his presidential address he related his observations on a recent visit to several of the German and Swiss dye companies. In particular, he described the organization of one of the Swiss firms, Bindschedler and Busch of Basel. His point was to show that on the continent the first principle that guided the management was(18):

The absolute necessity of having-trained scientific chemists, not only at the head of the works but at the head of every department of the works where a special manufacture is being carried on. In this respect this method of working stands in absolute contrast to that too often adopted in chemical works in this country, where the control of the processes is left in the hands of men whose only rule is that of the thumb, and whose only knowledge is that bequeathed to them by their fathers.

Roscoe further stated that English chemists had been just as successful as their continental opposites in the initial stages of the synthetic organic chemical industry. The real reason he attributed to the decline of the dye industry was the lack of appreciation of training in pure chemistry as a prelude to its practice in the industry(18):

The Germans and Swiss, however, have been and still are distinctly before us, not only in the facilities which they possess of obtaining the highest technical training in their numerous Universities and Polytechnic Schools, but what is even more to the point, before us is the general recognition of the value and importance of such training for the successful prosecution of any branch of applied science. It is only the highest and most complete scientific training that can insure commercial success.

Levinstein became Chairman of the Manchester Section of the Society of Chemical Industry in 1883. In his inaugural address he focused on the subject of technical education and the state of the dye industry. After stating that most of the British chemical industry had managed to do very well despite difficult conditions, he then added that..."it is only that special part which deals with the production of aromatic hydrocarbon derivatives that has been neglected, and in which this country has allowed itself to remain behind others(19)." The dye industry was only in its infancy; yet it was generating enormous profits. Levinstein indicated that the three largest German dyestuff companies in 1882 had a profit of one million pounds sterling! (in today's terms over one hundred million dollars at least). Levinstein then posed a rhetorical question(19):

You will also agree with me that it is a perfect anomaly that England, the largest manufacturing nation, possessing immense accumulated wealth, with general manufacturing facilities superior to those of any other country, situated geographically in the most favored position, and, above all, possessing the necessary raw

material in quantities sufficient to supply both its own wants and the needs of the world, should be altogether beaten in an industry which had its original development in its own borders, and of which it was indeed the founder. What, then, is the cause of the fact that this country has lost and is still losing ground as regards this special industry?

The blame according to Levinstein lay squarely on the shoulders of British academics. Since the departure of Hofmann in 1865, the study and application of aromatic chemistry which was so fruitful in the early developing stages of the industry had been neglected. W.H. Perkin and E. C. Nicholson, students of Hofmann and early leaders of the dye industry, had continued their research on problems related to dyestuffs as the industry developed. However, these two chemist-entrepreneurs, who had become very wealthy, abandoned the industry while still in their prime. Continuing his Presidential address Levinstein stated(19):

But then these gentlemen, though even great organic chemists, are none of them specialists in this particular department of science, and if you desire further confirmation of my statements, I invite you to look through the literature of the last ten years, and to compare the researches and investigations undertaken by the German with those of the English professors bearing on this subject, when you will be quite as much astonished at the overwhelming amount done by the former as at the insignificance of the work done by the latter. Indeed, gentlemen, unless my memory plays me false, I do not remember, during the period referred to, with perhaps few exceptions, any important original research bearing on this subject undertaken by the English professors, and I certainly cannot call to mind any work done by them which has been of practical utility to this industry.....and whilst the Germans are constantly developing this department of chemistry, and are thus extending and cultivating year by year the ground taken from this country, the British are content with simply acknowledging the ever-increasing amount of experimental research in Germany, without making the slightest effort to overtake their opponents, save perhaps by the demand for increased expenditure on technical education or endowments for research.

Levinstein argued for the appointment to professorships of men willing to work on research that would benefit the color industry. In Germany, science and prac-



The dyehouse of Levinstein's works in Blackley, 1912

tice were intimately related, and Levinstein would have liked to see this also occur in Britain.

In the following year, Roscoe became Chairman of the Manchester section of the SCI and in his remarks he sought to reply to the criticisms put forward by his predecessor. Roscoe stated that the purpose of any course in chemical technology in his view was not to train persons so completely that they could immediately manage an alkali or alizarin works(20):

All that instruction can with any degree of success attempt is to lay a scientific foundation upon which the pupil is afterwards to build...In our case the first thing to ensure is that the student has obtained a careful and systematic grounding in the principles of the science, the application of which it will be his business to carry out.

Roscoe further stated his belief that one must know the theory, and then and only then can the student make full use of any lectures in the practice of chemistry(20):

Above all, train him in habits of precise scientific thought, thus giving him the power of...initiating improvements or new processes by a complete understanding of the old one.

The period of anyone's formal education is brief as compared to the whole of a working career. Practical knowledge can be adequately acquired during this second stage of one's life. Original laboratory research, Roscoe argued, better equips the chemist to analyze and solve the problems encountered in the works. Roscoe was fully aware that under the most ideal circumstance teachers should be employed who had both a thorough grounding in theory as well as some practical experience. In Roscoe's view this practical training became outdated fairly quickly(20):

We may indeed be well content if our students are brought so near the harbour for which they steer, that they can recognize and appreciate the value of the new lights which by degrees open out to them in practice. We may be satisfied if by careful training they are brought to know the principles of their calling, though their knowledge of detail be deficient, and even somewhat antiquated.

In concluding his remarks, Roscoe made the following plea(20):

But the great difficulty which meets us on every hand is the proneness of the English mind to look for immediate results. We are proud of being called a practical nation; let us take care that with this we unite theory or scientific knowledge, and all will be well

with us; whilst if we continue to be content with practice alone, we shall find ourselves outstripped in our industries by those who see more clearly than we do in what the real strength of a manufacturing nation lies.

This was to be Roscoe's last statement as a member of the Owens College faculty; he was elected in 1885 to Parliament as the member for the South Manchester constituency and would never return again to academic life. These remarks of Roscoe ring as pertinent today as they were in his time in the continuing debate over funding pure versus applied science and the need for academia to become more involved in practical research.

In 1886 the present state of the British chemical industry was analyzed again by Levinstein before the Manchester section of the SCI. This was a wide ranging analysis of the industry as a whole and particularly of the dye industry. With respect to the question of pure versus applied science, Levinstein again returned to his theme that teachers of technical chemistry must have practical experience to be effective(21):

.....when Professors are placed at the head of our largest technical colleges who have never in their life done an hour's practical work in any manufactory or works, and whose only qualification appears to be purely scientific attainments...Industry and science must work hand in hand, for each can learn from and benefit the other.

Levinstein further lamented the lack of training available in Britain in what would become the discipline of chemical engineering. Mechanical engineers had long been a fixture at chemical plants as they were needed to run the steam engines and pumps. The problem of scaling up a chemical process was a challenge they were not equipped to handle. So much of the industrial chemist's time was taken up with learning the mechanics of running a chemical plant, that the application of their knowledge of chemistry was greatly hindered. From 1880 to 1907 at the Manchester Technical Municipal School, an institution that Levinstein helped to found, George Davis presented a series of lectures that became the core of chemical engineering programs in Britain. Degrees in chemical engineering were not offered until 1909, however. Judging from the discussion after the paper, Levinstein had struck a very responsive note with the audience. Speaker after speaker arose to agree with the conclusions of Levinstein concerning the thrust of his argument for technical education and the lack of appreciation of scientific education in Britain as a whole. Watson Smith of Owens College stated that the managers of chemical works argue, "Give me an

ounce of practice and you can keep your ton of theory"(22). This completely subverted the role of the chemist.

Levinstein did have some success in improving technical education in Manchester since he was the driving force behind the merger of the Faculty of Technology at Owens with Manchester Municipal College to form the foundation of the present University of Manchester Institute of Science and Technology.

Well into the present century Levinstein continued his attack on the general state of education in Britain as a whole. His 1902 Presidential address to the SCI was entitled, "Education and Legislation: Their Influence on Trade and Industry." After reviewing the litany of reasons for the decline of British commerce, he made the following remarks concerning legislation(23):

Under our present system, questions of the highest commercial importance receive but scant attention either from the Board of Trade or the House of Commons. Parliament is indifferent because it is largely constituted of lawyers and of men who, having been fortunate in the choice of a father, disdain anything appertaining to trade or commerce. They regard the House as a first-class club where one meets good society; and without the most strenuous efforts from the outside, it is difficult to get the House to consider any commercial question of national importance. The Board of Trade consists of a number of distinguished officials, often overworked with routine, and hence by no means anxious for changes which mean more work still. The appointment of a Minister of Commerce, able to command the attention of the House and to organize his own department, selected especially on account of an intimate knowledge of commercial affairs, would be an immense boon to the country.

Levinstein would be greatly pleased at how the emphasis on commerce and its importance has become a key element of national policy today. The Department of Trade and Industry in the UK and the US Department of Commerce as well as Japan's Ministry of Trade and Industry are examples of what Levinstein had hoped to accomplish. Levinstein saw the need to reform general nonspecialized secondary education. What good was there in a system of technical education if the proper groundwork was not laid in prior education? He went on to cite impressive statistics concerning secondary education in Germany as compared to England(24):

The millions of money spent on technical education in this country had been in the main sadly wasted. Of 54 so-called technical schools 22 had practically

no day students, and the total number of day students taking technological courses in Technical Schools and University does not equal the number of students in one single German Technische Hochschule—that of Charlottenburg.

To restore British competitiveness Levinstein advocated a system of modern comprehensive education instead of the classical education that had existed in Britain for so long. Students in Germany obtained an education that prepared them to deal with the complexities of modern businesses. The real deficiency, however, was in general secondary education and until this was remedied, all the magnificent facilities for higher education would not be adequately used(24):

What we really want is a large number of men who have enjoyed the highest scientific training. To attain this end, the first condition is not by any means to erect an additional number of technical high schools, but to have a very large number available who have been thoroughly trained in general knowledge and are ready to benefit from the existing facilities for higher education. It is here that the "shoe really pinches." Establish a system of high-class secondary education accessible to all grades, give sufficient inducement for the boys to stay long enough at school, and you will find that efficient scientific and technical education will follow here just as it did in Germany.

These remarks again have much contemporary application in the current debate over secondary school education in the United States as compared to other parts of the world.

In many ways Levinstein can be likened to the Biblical prophets of old whose warnings were not heeded. The deficiencies that Levinstein saw in the chemical industry in his adopted country were dramatically shown during the Great War, the first conflict in which science, especially chemistry, played an important role. The inadequacies of the British chemical industry became very evident by 1915, the cause being the lack of investment in research and the failure, as Levinstein had so well documented, training of chemists for applied work by the universities. The lesson seemed to be learned to some extent, since the British were in a much better position in 1939 than in 1914. At the time of his death in 1916 Levinstein must have been saddened to see how true his prophecies had become. Without the continuous dialog between opposing viewpoints concerning the role of pure and applied science, however, change would never have occurred.

ACKNOWLEDGMENT

I am deeply grateful to Dr Anthony Travis of the Edelstein Center for the History of Science at the Hebrew University of Jerusalem for his help in the preparation of this paper.

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THE KEY ROLE PLAYED BY SUGAR IN EARLY EXPERIMENTS IN KINETICS AND EQUILIBRIA*

John T. Stock, University of Connecticut

Enormous quantities of sucrose, the common chemical name for cane or beet sugar, are consumed not only in commercial and domestic food making; in fact, this cheap and biorenewable compound has other industrial uses, an example being in the production of polyurethane foam, where the original feedstock, petroleum or glycerol, has been replaced by sucrose (1). At the other extreme, small quantities of sucrose played an important role in the early development of chemical kinetics, and of concepts such as the relative strengths of acids and bases. This is the theme of the present account.

(Friedrich) Wilhelm Ostwald (1853-1932), Fig. 1, carried out his doctoral research at Dorpat, on "The Mass Action of Water," in particular the hydrolysis of BiCl_3 , showing that the hydrolysis increased as more water was added to the solution. In his ten-year career at Dorpat, where he quickly became recognized as a promising scientist, Ostwald realized that the study of phenomena in solutions should employ effects that do not themselves alter the concentrations of the reacting substances. He became especially interested in the "affinities" (i.e., the relative strengths) of acids.

Julius Thomsen (1826-1909) had used calorimetry to compare the affinities of pairs of acids, by allowing the members of a pair to compete for alkali insufficient to neutralize both. Ostwald, having no access to the sensitive equipment used by Thomsen, made measurements of changes in volume, density and refractive index. In 1878, Ostwald summarized his conclusions as follows(2):



Figure 1. Wilhelm Ostwald

Ostwald's Table of Relative Affinities of Acids
(Monobasic) (2)

Nitric acid (taken as standard)	100	Formic acid	3.9
Hydrochloric acid	98	Lactic acid	3.3
Trichloroacetic acid	80	Acetic acid	1.23
Dichloroacetic acid	33	Propionic acid	1.04
Monochloroacetic acid	7.0	Butyric acid	0.98
Glycollic acid	5.0	isoButyric acid	0.92

During his appointment as professor at the Riga Polytechnicum, 1882-1887, Ostwald turned his attention from equilibrium methods to ones based upon the measurement of reaction velocities. After first studying the effect of acids on the rate of hydrolysis of acetamide (3) and of methyl acetate (4), he turned to measurements based on the acid-catalyzed "inversion" of the polarimetrically dextro-rotatory cane sugar into the levo-rotatory "invert sugar" (5), an equimolar mixture of the isomeric monosaccharides glucose and fructose. The polarimetry of sugar solutions and the phenomenon of inversion had been described in 1833 by Jean Baptiste Biot (1774-1862) (Fig. 2), Professor of Physics at the Collège de France, and Jean Francois Persoz (1805-1868) (6,7), who obtained his doctorate in the same year and later became Professor of Chemistry in Paris. At that time there was little interest in chemical kinetics.

A pioneer in the field of kinetics was Ludwig Wilhelmy (1812-1864), then a faculty member at Heidelberg, who in 1850 reported a study on the parameters that govern the rate of the inversion of sugar (8). The sugar-HNO₃ solution was placed directly in the polarimeter tube, and changes in the rotation were observed for a day. During this time, the temperature ranged from 14.5° to 18° C. Wilhelmy found that the rate of inversion, dx/dt , was proportional to the concentration of acid and also to the concentration of sugar. He formulated the equation

$$-\frac{dx}{dt} = M.Z.S.$$

where S is the amount of acid, Z the amount of sugar at time t , and M is a constant for this acid. Wilhelmy's paper of 1850 passed unnoticed by most later workers, until, with change of symbols, Ostwald rewrote the integrated form of equation (1) as:

$$\log \frac{b}{b-x} = \log \frac{1}{1-x/b} = a.c.t.$$

Here b signifies the initial amount of sugar and x the amount inverted after passage of time t minutes. The



Figure 2. Jean Baptiste Biot

velocity constant c applies to a particular acid, the specified concentration, a , of which remains unchanged (5).

Ostwald had invented a very effective thermostat which he later described (9). This allowed all experiments to be carried out at 25° C. Constancy of temperature was important, because observations extended over many hours. Ten-mL portions of each 40-50% sugar solution and one-normal acid solution were mixed in a small stoppered vessel. After a suitable time, the liquid was transferred to the polarimeter tube, which was water-jacketed at 25° C. Following polarimetry, the liquid was returned to the thermostatted vessel, returned to the polarimeter after further time lapse, and the sequence continued until termination of the experiment. In this manner, Ostwald determined the inversion velocity constants of more than thirty mainly monobasic acids. Further, he was able to show that the relative values obtained by this technique were in line with those from the methyl acetate hydrolysis studies. In the following year, Ostwald published a second paper on sugar inversion (10). Having been troubled by mold formation in his sugar solution, he found a cure in the addition of camphor to the solution and the use of petroleum-impregnated cork closures. However, his main aim was to show that yet another technique, electrolytic conductivity, afforded results that were in line with those obtained by inversion studies.

In 1887 Ostwald was appointed Professor of Physical Chemistry at Leipzig. He attracted many students



Figure 3. James Walker

from America and Britain. The Scot, James (later, Sir James) Walker (1863-1935) (Fig. 3) was the first of these to earn a Ph.D. under Ostwald's direction. Walker's doctoral research entailed a thorough study of the behavior of weak organic bases. His paper, "Estimation of the Affinities of Organic Bases," is a description of his accomplishments (11). Although the relative strengths of acids could be explored directly, this was not the case for Walker's weak monoacidic bases. To study these, he invoked the phenomenon of *salt hydrolysis*. On dissolution in water, the salt of a weak base undergoes at least partial breakdown into the parent acid and base, or their ions. A determination of the concentration of this acid should provide a measure of the extent of hydrolysis, which will be large with a base of low affinity. This determination cannot be performed by titration with alkali; neutralization of the acid would result in further salt hydrolysis. Walker successfully applied the acid-catalyzed breakdown of methyl acetate used by Ostwald to study organic acids.

After receipt of his Ph.D. degree in July, 1889, Walker returned to Edinburgh as assistant to Alexander Crum Brown (1838-1922), where the two worked on the electrosynthesis of organic dibasic acids, whose dissociation constants Walker then determined. In 1895, Walker returned to the area of his doctoral studies but now made use of the sugar-inversion method in this re-examination of the affinities of organic bases (12). As in the Leipzig experiments, the acid arose from the hy-

drollysis of the weak-base hydrochloride. However, this method appeared to be inferior to the conductometric, and possibly to the methyl acetate method. Unlike the latter approach, the sucrose method was applicable to a wide range of affinities. Salt hydrolysis ranged from 2% for pyridine to 97% for thiourea. Because of the uncertainties of this method, affinity constants were not reported. However, the velocity constants followed a sequence similar to that noted at Leipzig.

Joseph Ellis Trevor (1864-1941) (Fig. 4), an American student in Ostwald's laboratory, after making a successful study of double-salt formation (13), turned his attention to the measurement of the small degrees of "hydrogen dissociation" (i.e., the very low acidities) of acid sodium salts, such as sodium hydrogen maleate (14). At least in Ostwald's laboratory, the ionic theory had become fully accepted. Accordingly, the hydrogen ion was considered to be the actual catalyst in the inversion of sugar. Trevor pointed out that the solution of such a salt may contain the ionic species Na^+ , H^+ , HX^- , and X^{2-} , and therefore dismissed conductometry as a possible means of acidity determination.

A polybasic acid has at least two dissociation constants. Thus for the dibasic maleic acid, $K_1 = 1 \times 10^{-2}$ and $K_2 = 3 \times 10^{-7}$, so that the acidity of the monosodium salt is much lower than that of the free acid. Working at 25° C with various weak acids, Ostwald had needed very

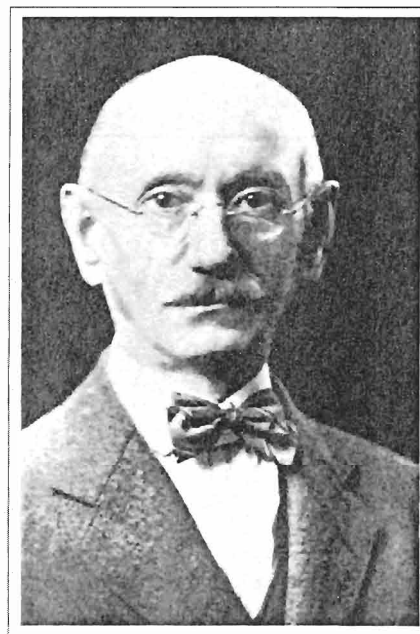


Figure 4. Joseph Ellis Trevor

long times to get extensive inversion of sugar. Trevor realized that, at this temperature, inversion by his acid salts would be impossibly slow. Knowing that the rate

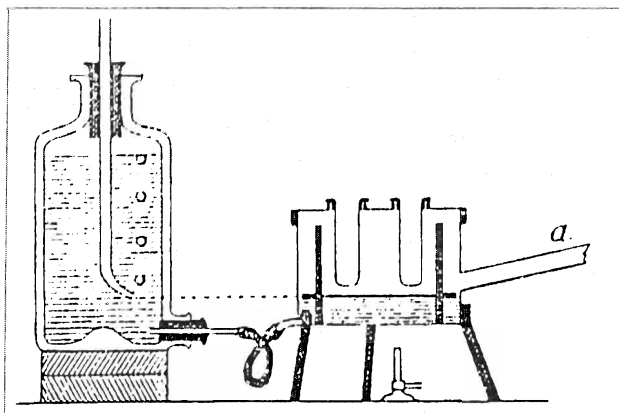


Figure 5. Trevor's steam bath

of inversion increases with temperature, Trevor made the obvious choice of working at 100°C . For this purpose, he devised the constant water level steam bath shown in Fig. 5. From a trial with $0.01N$ succinic acid, he found that the rate of inversion of sugar was about 4000 times faster at 100° than at 25°C . If inversion at 100°C solved one problem, it created another. For successful polarimetry, the solution had to be cooled down to 20°C , the temperature of the jacketed polarimeter tube. Corrections for inversion that occurred while the temperature was changing had to be elucidated. Further, since the strong acid HCl was to be used as the reference standard, a way to arrest the very fast inversion at 100°C had to be found. Trevor sterilized his approximately 13% sugar solution by boiling, then added camphor or naphthalene as preservative. He showed that the solution underwent no polarimetric change after being kept in the steam bath for 80 minutes. Inversions were carried out in steamed-out hard glass test tubes, closed by rubber stoppers, each with a short length of capillary tubing as a vent. Each tube contained 10 mL each of solutions of sugar and of the acid. The steam bath could hold about seven of these tubes. After a suitable time, a tube was transferred to a thermostated bath operating at 20°C and left there for 15 minutes. Although this period sufficed to bring the tube contents to 20°C , the acidity of many of the salts was so low that hour-long periods in the thermostat caused no further inversion. Preliminary experiments were made with lactic acid, so that a correction could be obtained for the inversion that occurred while the temperature of the reaction mixture was rising to 100°C .

Having tested his proposed technique by determining the inversion velocity constants of monosodium succinate and monosodium-*m*-phthalate, Trevor turned to the determination of the degrees of "hydrogen dissocia-

tion" in solutions of these and of other sodium acid salts. He determined the molar velocity constant of HCl , his "standard," at dilutions ranging from 200 to 3200 L per mole. In such solutions, 100% dissociation could be assumed with confidence, and the rate of inversion was low enough to allow samplings to be made with reasonable accuracy. As a sample tube was removed from the thermostat, one mL of concentrated sodium acetate solution was added. This destroyed the acidity by forming undissociated acetic acid and thus arrested the inversion. Experiments in glass tubes, confirmed and extended by runs in silver tubes, gave the mean value 17.92, with a maximum deviation of 0.6%, as the molar velocity constant of HCl at 100°C . If this value represents 100% dissociation, the degree of dissociation of another acid could be assessed from the ratio of its inversion rate to that of HCl . The acid salt solutions were prepared by titrating a measured volume, V cc, of a standard solution of the appropriate acid with NaOH , to form the disodium salt. A second V cc of acid was then added, and the resulting solution, of known concentration of the monosodium salt, was diluted as required.

Trevor suspected that the principal uncertainty in his experiments with acid salts was in the exact proportionality between inversion velocity and degree of dis-

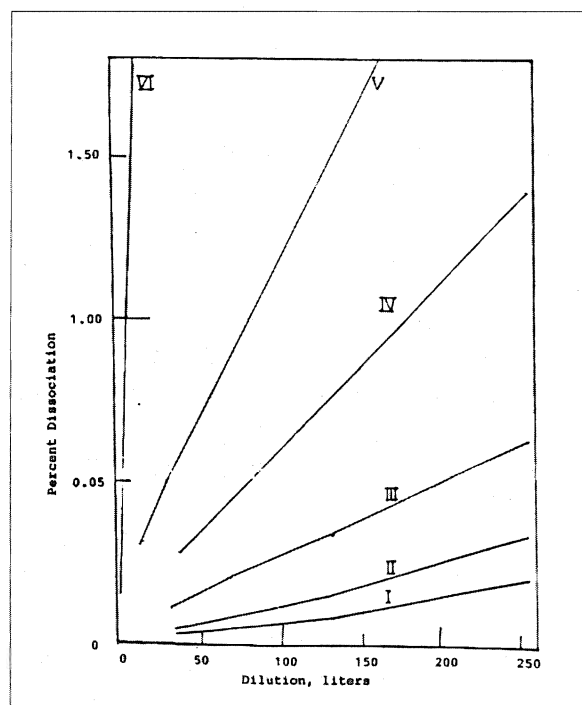


Figure 6. Percent dissociation of the monosodium salts of dibasic acids: I, suberate; II, succinate; III, citrate; IV, mesaconate; V, fumarate; VI, acetic acid (for comparison).

sociation. He attributed this uncertainty to the unknown influence of the nonelectrolyte, sugar, on the dissociation of an acid. The results of Trevor's extensive determinations are indicated by the curves in Fig. 6. These show the relationship between the degree of dissociation and the dilution of solutions of the various acid salts. For comparison, the rocket-like curve of acetic acid, the typical "textbook" weak acid, is included. Trevor believed that his value of 0.03%, at $v = 32$ L, for monosodium suberate was the smallest degree of dissociation that had ever been measured. He noted that a conductometrically obtained value of 0.04% at $v = 14.4$ had been reported for *p*-cresol (15).

Thus a common natural substance sugar played a key role in a variety of investigations which led to the development of chemical kinetics and support of the ionic theory.

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GUSTAVUS HINRICHS AND THE LAVOISIER MONUMENT

William D. Williams, Harding University

As many commemorations of the bicentennial of Antoine Lavoisier appeared (1), so there were similar efforts to honor that great chemist in the 1890's, at the time of the 100th anniversary of the observance of the French Revolution and Lavoisier's death. One of those efforts a century ago was originated by a little known American chemist, Gustavus Hinrichs, who called for a world-wide subscription by chemists to erect a monument to Lavoisier in Paris.

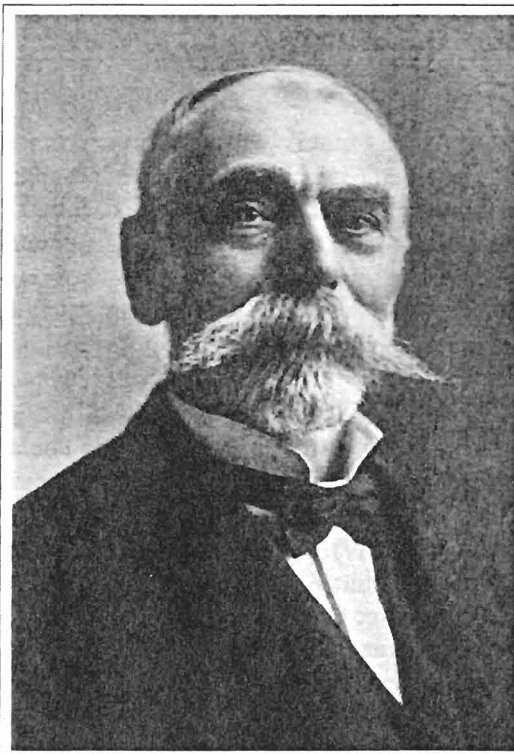
Gustavus Detlef Hinrichs (1836-1923) was one of America's earliest theoretical physical chemists. He was professor of chemistry at the State University of Iowa (1863-1885), St. Louis College of Pharmacy (1889-1903), and the Medical Department, St. Louis University (1903-1907). Fluent in five languages, he published most of his research in European journals and was as well known in Europe as in the United States. Hinrichs himself carried out very little laboratory experimentation but rather sought to interpret and interrelate data from the literature. He devised a classification of the chemical elements and a type of periodic chart which predated those of Mendeleev and developed some of the earliest mathematical relationships between molecu-

lar structures and physical properties. Seeking to identify a fundamental subatomic unit of matter, which he called the panatom, he spent 40 years evaluating methods of atomic weight determination. The author of about

300 publications, including 25 books, he also made significant contributions to chemical education, meteorology, astronomy, and geology. Although some of his concepts later proved to be incorrect, Hinrichs showed insightful originality at the frontiers of the science of his generation. He was described by Charles C. Wylie as "as versatile a man as American science has produced (2)."

On February 12, 1894, Hinrichs presented a lecture, "Centenary Commemoration of the last Days of Antoine Laurent Lavoisier," before the Writers' Club of St. Louis. He concluded his presentation with a wish that "chemists of the world would unite to erect a monument to Lavoisier (3)." Captivated by that idea, Hinrichs published a four-page "Appeal to Chemists of All Nations" (4). On the ex-

act centennial of Lavoisier's death on the guillotine, May 8, 1894, Hinrichs mailed one thousand copies of his "Appeal" to chemists, chemical journals, and science



Gustavus Detlef Hinrichs

societies. An advance copy was sent to the secretary of the French Academy of Sciences, Marcelin Berthelot (1827-1907), who immediately labeled it "a happy and grand idea"[translation by Hinrichs; see Ref. 8] and offered the cooperation of the Academy. Soon the French Academy assumed sponsorship of the project, and Hinrichs was appointed "Delegate of the Academy of Paris" to organize the American committee. Karl Remigius Fresenius (1818-1897) assembled a German committee; the Royal Society backed efforts in England, and a Russian committee responded as well.

Hinrichs selected an American committee of fourteen additional members (5): Jasper L. Beeson, Ph.D., Louisiana Experimental Station, New Orleans; Charles A. Goessmann, Ph.D., Massachusetts Agricultural College, Amherst; Eugene W. Hilgard, Ph.D., University of California, Berkeley; Richard W. Jones, M.A., LL.D., University of Mississippi, University, MS; John Uri Lloyd, Ph.D., Eclectic Medical Institute, Cincinnati; John H. Long, ScD., Northwestern University, Chicago; John U. Nef, Ph.D., University of Chicago; James M. Pickel, Ph.D., University of Alabama, Tuscaloosa; Paul Schweitzer, Ph.D., University of Missouri, Columbia; William Simon, Ph.D., M.D., College of Physicians and Surgeons, Baltimore; Edgar F. Smith, Ph.D., University of Pennsylvania, Philadelphia; Eugene A. Smith, Ph.D., University of Alabama, Tuscaloosa; Henry Trimble, Ph.M., Philadelphia College of Pharmacy; Francis Venable, Ph.D., University of North Carolina, Chapel Hill. So that every dollar subscribed would reach Paris, Hinrichs asked committee members to defray all expenses involved in collecting the money.

As money was raised from 1894 to 1899, periodic reports were printed in *Comptes Rendus* on the status of the contributions (6). A total of almost 100,000 francs was eventually collected, with France providing about half the amount. Russia sent about one-fourth of the total, including a sizable gift from the Tsar. The remaining one-fourth came from all the other countries, with the United States sending 3,054 francs (\$580) (7). In Hinrichs' final report for the United States committee, a list of about 500 contributors indicated the source of the \$580 (8). Since committee members only canvassed people in their own areas, a larger committee would have yielded a bigger total. Most individual subscriptions were between \$1 and \$10, and several groups of students pooled modest amounts. Hinrichs' personal contribution was 250 francs (about \$50). While the American combined contribution seems embarrassingly small, it should be noted that an economic depression in the 1890's reduced incomes to meager amounts. In addi-



Lavoisier Monument, Paris

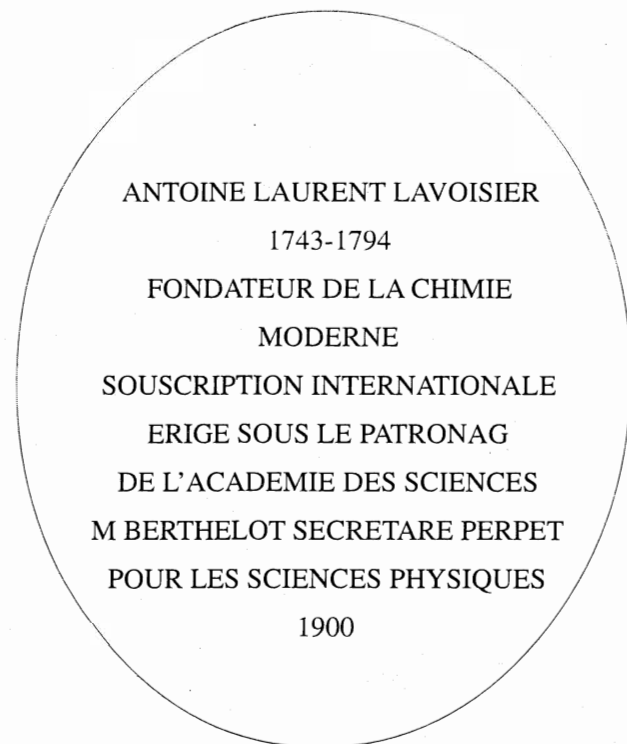
tion to the committee members, some notable American chemists appearing in the list of contributors were: John W. Mallett, University of Virginia; Alexander Smith, University of Chicago; Ira Remsen, Johns Hopkins University; Albert B. Prescott, University of Michigan; and James M. Crafts, Massachusetts Institute of Technology.

The statue was cast in bronze by E. Barrias of the French Art Institute. The figure of Lavoisier stood beside a table containing chemical glassware and a balance. It has been reported that Barrias copied a head of Lavoisier made by another sculptor at an earlier date. Later it was found that the head was that of Condorcet, rather than of Lavoisier (9). Two large bronze bas-relief scenes were set into the pedestal. In one, Lavoisier was working in his laboratory as his wife recorded the results of his experiments. In the other Lavoisier was lecturing at the Academy of Sciences to contemporary scientists Guyton de Morveau, Berthollet, d'Alembert, Condorcet, Monge, Laplace, Lamark, and Lagrange (10).

Dedication ceremonies were held on July 27, 1900. Berthelot presented a biographical sketch of Lavoisier, and H. Moissan, secretary of the French Academy committee for erecting the statue, gave details of the inter-

national cooperation. There is no indication that Hinrichs or any other American attended the dedication (11).

The monument was located at the rear of the Eglise de la Madeleine, facing rue Tronchet, near the house where Lavoisier had lived for many years. The inscription on the plaque read:



Lamentably, the monument did not survive World War II, for German occupation forces confiscated all metal statues in Paris to use as scrap for their war efforts. A 1945 photograph showed the Lavoisier pedestal stripped of the statue, the two bas-reliefs and the inscription plaque (12). The ultimate fate of the pedestal is unknown.

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BOOK REVIEWS

From Caveman to Chemist: Circumstances and Achievements. Hugh W. Salzberg, American Chemical Society, Washington DC, 1991, xix + 263 pp.

Among the many one-volume histories of chemistry that have appeared during the past decade, only a few have aimed at being "popular" histories of the subject, that is works designed not specifically for historians and chemists but also for a wider audience. Perhaps the best-known example of that genre is Bernard Jaffe's *Crucibles*, first published more than a half century ago and still in print. To quote from the preface of the work under review, "This book is not a detailed, comprehensive history. It is a narrative designed to give chemists and interested bystanders some insight into the profession." It clearly falls into the class of popular histories.

The 25 chapters of *From Caveman to Chemist* proceed from ancient technology to the divisible atom. A page count will give an idea of the relative importance given to various periods in this extended time line. The text encompasses 260 pages. The first 52 of these bring us to the end of the Hellenistic and Roman eras. A short (18 pp.) chapter on Islamic Alchemy is followed by 51 pages in three chapters on medieval and renaissance contributions to chemistry; 16 pages on the impact of printing and books; 10 pages on some of the architects of the scientific revolution—Copernicus,

Kepler, and Galileo among others; and 23 pages on Sala, Van Helmont, and Boyle, who are dubbed the first chemists. The phlogiston theory (11 pp.) is followed by Lavoisier and the chemical revolution (21 pp.). Nineteenth-century chemistry occupies most of the rest of the book, with 18 pages on the atom and molecular formulas and 25 pages on organic chemistry up to stereoisomerism. Twentieth-century chemistry is limited to a few pages in the final chapter (10 pp.) on the divisible atom. The book includes a number of illustrations, among them one in full color showing the earliest known depiction of a wearer of spectacles, emphasizing the importance of artisanship in early chemical technology. Each chapter has a short list of references, and there is a four-and-a-half-page additional reading list and a detailed index.

Hugh Salzberg has an easy style and his book is readily comprehensible to a wide readership. It stresses the contributions of individual scientists and is very much an internalist history of the subject. While there are connections to medicine, other sciences, and technology, there are few to any broader historical context. However, the task of presenting in a relatively concise way the major outlines of the development of chemistry from prehistoric times to the beginning of the twentieth century is one that this book does accomplish well. *Harold Goldwhite, Department of Chemistry, California State University, Los Angeles 90032.*

Cavendish. Christa Jungnickel and Russell McCormach, American Philosophical Society, Philadelphia, PA, 1996. xi + 414 pp. Cloth (Typeset), \$32.00.

Henry Cavendish (1731-1810), the well-known 18th-century British chemist and natural philosopher, has been the subject of three book-length biographies. The first, by the British chemist George Wilson, was published in 1851 and was commissioned by the Cavendish Society. One of several early 19th-century subscription printing clubs, the society was best known for issuing massive multi-volume English translations of Gmelin's well-known *Handbuch der Chemie*. Not only was the society named in honor of Cavendish, it also adopted a woodcut of Cavendish's famous glass eudiometer as its logo.

Wilson's biography, set in small type, was 478 pages long, of which roughly 190 pages, or about 40%, were strictly biographical, with another 104 pages or 22% devoted to a summary of Cavendish's work in chemistry and natural philosophy. The remaining 184 pages, sandwiched between the descriptions of Cavendish's chemical and physical researches, dealt with the vindication of Cavendish's reputation relative to the so-called "water controversy."

Since the end of the 18th century there had been rival claims as to who—Lavoisier, Monge, Watt, or Cavendish—most deserved the credit for having first discovered that water was a compound of hydrogen and oxygen. Matters had been brought to a head by the publication in 1846 of James Patrick Muirhead's book on *The Correspondence of the Late James Watt on his Discovery of the Theory of the Composition of Water*, and by an article published two years later by Lord Jeffrey in the *Edinburgh Review*. Both authors were enthusiastic supporters of Watt's claim and essentially accused Cavendish of out and out plagiarism or, at best, of highly questionable ethics. Wilson, and doubtless the directors of the Cavendish Society as well, felt that

these accusations had to be answered and the result was Wilson's long digression on the history of the water controversy. Though Wilson realized that this digression produced an imbalance by underrating Cavendish's work in natural philosophy relative to his work in chemistry, he felt that it was nevertheless justified. Since the attacks concerned Cavendish's chemical activities, it was only right that chemists should come to his defense.

Wilson was no amateur when it came to history of chemistry. More than one modern historian of chemistry made his reputation in the early 1960s in the debates over the true origins of Dalton's atomic theory by rediscovering what Wilson had already said about Dalton's theory a century earlier in one of his historical essays. Besides his teaching position as Professor of Technology at the University of Edinburgh, Wilson also became the first Director of the Industrial Museum of Scotland and was largely responsible for establishing the Playfair Collection of Historical Chemical Apparatus.

Not only do we owe most of our existing anecdotes of Cavendish to Wilson's zeal in interviewing the older members of the Royal Society, he also left us with an unforgettable summary of Cavendish's life and character - a summary which at times borders on the brilliant:



Logo of the Cavendish Society: Henry Cavendish's glass eudiometer for sparking gas mixtures

Morally [the life of Cavendish] was a blank and can only be described by a series of negations. He did not love, he did not hate, he did not hope, he did not fear, he did not worship as others do. He separated himself from his fellow men, and apparently from God. There was nothing earnest, enthusiastic, heroic, or chivalrous in his nature, and as little was there anything mean, groveling, or ignoble. He was almost passionless. All that needed for its apprehension more than the pure intellect, or required the exercise of fancy, imagination, affection, or faith, was distasteful to Cavendish. An intellectual head thinking, a pair of wonderfully acute eyes observing, and a pair of very skillful hands experimenting or recording, are all that I realize in reading his memorials.

Over a century would pass before a second book-length biography of Cavendish appeared in 1960. Like Wilson before him, its author, the Cambridge chemist A. J. Berry, was no stranger to historical writing and is perhaps best known for his two earlier volumes of essays dealing with selected topics in the history of chemistry (*Modern Chemistry: Some Sketches of Its Historical Development*, 1948, and *From Classical to Modern Chemistry: Some Historical Sketches*, 1954). Berry made no pretense that he had uncovered important new information concerning Cavendish. His goal was rather to produce a concise and balanced summary of Cavendish's life and work. The resulting book was less than half the length of Wilson's and was devoted almost totally to a description of Cavendish's scientific researches, with his life being compressed into a single 15-page chapter. Unlike most recent scientific biographies, which tend to focus on a scientist's hobbies, political and social activities, or personal sexual peculiarities, Berry realized that it is the scientific work of scientists which alone makes them worthy subjects of book-length biographies, and that any account which fails to address this work in detail is seriously defective.

The third book-length biography of Cavendish, and the subject of this review, was published by the American Philosophical Society in 1996 and is coauthored by the team of Christa Jungnickel and Russell McCormmach, perhaps best known for their two-volume social history of the 19th- and early 20th-century German physics community (*Intellectual Mastery of Nature: Theoretical Physics from Ohm to Einstein*, 1986). Beautifully produced in a large 8.5 x 11 inch page format with double print columns, the book logs in at 425 pages. Despite the differences in font size, its use of the larger page size and double columns probably means that it is longer than even Wilson's early volume. This 28-fold expansion of Berry's concise 15-page summary of Cavendish's life would suggest that the authors have uncovered a vast reservoir of previously unknown material concerning Cavendish, but in fact most of this size increase is a simple result of how the authors have chosen to define their subject.

In less than a paragraph, Berry pointed out that Henry Cavendish's father, Lord Charles Cavendish, was the single most important influence in Henry's life. Charles was something of a scientist in his own right, though hardly as distinguished as his son, and Henry received his scientific education by acting as his father's assistant. He also lived and worked in his father's house until the latter's death in 1783, during which he per-

formed most of his electrical researches. It is primarily this father-son connection that Jungnickel and McCormmach have sought to explore in detail and which accounts for both the title of their book (*Cavendish* rather than *Henry Cavendish*) and for the fact that the portrait on the front cover is that of Charles Cavendish rather than of Henry Cavendish.

The book is divided into four parts. Part 1 provides a brief introductory history of the Cavendish family; Part 2 deals with the life and career of Charles Cavendish, Part 3 with the education of Henry Cavendish during his father's lifetime, and Part 4 with Henry's career after his father's death. As with their earlier book, the authors spend a great deal of time exploring the social and political context of their subject. If Berry's biography errs on the side of too little context, this volume comes close to erring in the opposite direction - but not quite, since the authors do an admirable job of tying all of their pieces together and in documenting their subject.

As is the case with most modern literature in the history of science, the authors are not kind to their predecessors. Wilson's biography is described as an eccentric work which violates the fundamental rules of good biography and his brilliant analysis of Cavendish's character, quoted above, is dismissed as an unfair projection onto Cavendish of Wilson's own personal depression and obsession with religion. Likewise, Berry's biography is characterized as being little more than the kind of introductory essay that one might expect of someone who was assembling an edition of Cavendish's collected works. I think these comments are much too severe. Though the biography by Jungnickel and McCormmach is an invaluable addition to the secondary literature on Cavendish, it in no way displaces the earlier volumes by Wilson and Berry for the simple reason that each of these three volumes serves very different audiences.

Because it is the source of most of our knowledge of Cavendish, the Wilson biography will always be of interest and, with the passage of time, it has become almost as valuable for what it tells us about the water controversy and the views of early 19th-century chemists concerning the nature of Lavoisier's chemical revolution, as for what it tells us about Cavendish. On the other hand, students of the history of science, who are interested in a case study of the larger issues of the relations between science, politics, scientific organizations and class structure in 18th-century England, will find the Jungnickel - McCormmach biography of most interest, whereas chemists and teachers, who lack a pro-

fessional interest in the history of science but who are looking for a concise overview of Cavendish, will still find Berry's modest, but balanced, account the most

manageable starting point. *William B. Jensen, Department of Chemistry, University of Cincinnati, Cincinnati, OH 45221 - 0172*

Episodes from the History of the Rare Earth Elements. C. H. Evans, Ed., Kluwer Academic Publishers, Dordrecht, Hingham, MA, 1996. xxv + 240 pp.

The story of the discovery of the rare earth elements is probably the most confused and complex of any group of the elements. The history of the rare earths also illuminates more areas of chemical progress than any other group of elements. It stretches from the discovery of the first rare earth mineral in 1787 by Carl Axel Arrhenius to the unequivocal identification of the last in 1947 by Marinsky and Glendennin. The story involves the discovery of emission spectroscopy by Kirchhoff and Bunsen, the development of the periodic chart by Mendeleev, the discovery of X-rays by Röntgen, the discovery of atomic numbers by Moseley and atomic structure by Bohr and Rutherford, and, finally, work on the atomic bomb during World War II.

The book is divided into two main sections: *Discovery* and *Applications*, which, the editor states, concentrate more on historiography and contextual interpretations, rather than the highly technical. *Discovery* consists of six chapters; each authored by an experienced scholar in the field, in which are described the discovery and identification of the 17 rare earth or lanthanide elements. In chronological order, the chapters are: "What did Johann Gadolin Actually Do" by Pekka Pykko and Olli Orama; "The Discovery of Cerium—A Fascinating Story" by Jan Trofast; "Carl Gustaf Mosander and his Research on Rare Earths" by Levi Tansjö; "The Fifty Years Following Mosander" by F. Szabadvary and C. H. Evans; "Elements No. 70, 71, and 72: Discoveries and Controversies" by Helge Kragh; and "The Search for Element 61" by Jacob Marinsky.

The first three chapters are most useful because they provide access to the earliest days of research on the

rare earths through the Scandinavian literature, which is inaccessible to most because of language. The most outstanding figures of this early period were the Swedes Jacob Berzelius and his younger colleague, Carl Gustav Mosander, successively professor of chemistry and pharmacology at the Karolinska Institute. Tansjö notes that Berzelius retired from the professorship at 53 in favor of Mosander, as his wedding present to Mosander. He remarks "...that such generosity was then—and is still—not very common." In another of those strange historical connections, he notes that Mosander's great granddaughter, Carin Fock, was married to Herman Goering, and that Adolf Hitler liked her Swedish pea soup. Despite heavy academic and governmental responsibilities, Mosander expanded the rare earth family from two—cerium and yttrium—to six, the additional four being lanthanum, didymium, terbium, and erbium. This work was done without benefit of any unifying principles and by the traditional methods of fractional precipitation and crystallization. Following Mosander, very little was accomplished in the next thirty years because further progress required the development of new techniques and new concepts.

In the last quarter of the 19th century spectral analysis introduced by Kirchhoff and Bunsen and the development of the periodic table led to a new interest in identifying rare earth elements. This work resulted in the identification of all of the remaining elements, except for two: lutetium and promethium. It also led to the industrial production of rare earths by Carl Auer von Welsbach, who developed the incandescent gas mantle, using a mixture of thorium and cerium, and the automatic gas lighter, with a pyrophoric alloy of iron and mixed rare earths.

The chapter by Helge Kragh on the discovery of elements 70, 71, and 72 is a case study of contentious

priority and nationalistic disputes between the French chemist Georges Urbain and the Austrian Auer von Welsbach and their followers. Both were highly regarded by their colleagues. Auer von Welsbach was nominated ten times by Austrians and Germans for the Nobel Prize between 1918 and 1929. Urbain, on the other hand, received fifty-six nominations, almost all by French, between 1912 and 1936. Their disputes apparently led the Swedish Academy to award a Nobel Prize to neither. This period also brought physicists into the field, with the discovery by Moseley of atomic numbers from X-ray spectroscopy. This, in turn, led to disputes between the old-line chemists, such as Bohuslav Brauner, and the Copenhagen school of physicists. The chemists resented the easy solution found by the physicists to determine the number of rare earth elements.

The chapter on the search for and discovery of element 61 by one of its discoverers, Jacob Marinsky, is especially welcome and important. In the late 1950s Professor Marinsky was a member of my doctoral committee at what was then the University of Buffalo. It was he who, in part, whetted my interest in the history of the long search for 61. In this chapter he clearly describes his involvement in the research at Oak Ridge on separating uranium fission products during work on the atomic bomb in World War II. He also describes the interactions between himself, Lawrence Glendenin, Charles Coryell, and other colleagues in their use of ion exchange chromatography for the separation of the rare earth fission products and their discovery of element 61 in those products. He also explains their reasons for their claim of the discovery of 61 and the source of the name, promethium.

Part II: *Applications* consists of five chapters: "Carl Auer von Welsbach, A Pioneer in the Industrial Application of Rare Earths" by E. Baumgartner; "The History of China's Rare Earth Industry" by Wang Minggin and Dou Xuehong; "Rare Earth Elements in the Geological Sciences" by Edward Lidiak and Wayne Jolly;

"Use of Lanthanum as a Tool to Delineate Calcium Mobilization Patterns in Smooth Muscle" by George Weiss; and "Medical Uses of the Rare Earths" by C. H. Evans. These entail descriptions of various practical aspects of rare earth chemistry. Probably of most interest to chemical historians is Baumgartner's chapter on Auer von Welsbach and the industrial application of rare earths. This author treats much of the same material covered earlier by Kragh from a somewhat different viewpoint, decidedly more favorable to his fellow Austrian.

The editor states that he imposed minimal stylistic restrictions on the authors. Unfortunately, this has resulted in some interesting nongrammatical language by some of the authors whose native tongue is something other than English. There are also a number of typographical errors, which bring into question editorial and proof-reading care. A random sample of the index also disclosed a troubling number of errors. For example, a search for work by the American chemist Charles James under the eight index listings of his name led to two for Charles James, one each for James Spencer, H. L. James, A. E. James, and James Prescott Joule.

In conclusion, *Episodes from the History of the Rare Earth Elements* is a welcome and valuable contribution to this subject in the history of chemistry. In many ways it describes what science really is: a noncontinuous path, with many false turns, to the solution of a problem. The close connections between advancements in science and in instrumentation and theory are also well documented.

Episodes is an excellent source of information on the discovery of a group of elements intimately connected with the development and understanding of atomic structure and of the periodic table. The search was an integral part of the development of science and technology in the late 19th and early 20th centuries which served as the basis for the atomic bomb. *Clarence J. Murphy, East Stroudsburg University, East Stroudsburg, PA 18301-2999.*

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Paracelsus: Das Werk - die Rezeption. Volker Zimmermann, Ed. Franz Steiner Verlag, Stuttgart, 1995. 227 pp. DM 76-.

This book in German is a collection of sixteen articles celebrating the 500th birthday of Paracelsus. The birthday symposium preceding its publication was convened in Basel in 1993 with support from the Ciba Foundation. The book covers a diverse set of topics. Some are oriented toward what is medical and scientific. Others are spiritual or theological in nature. Still others focus on modern representations of Paracelsus in 20th-century literature and in the movies. All of the articles are scholarly, and the book is a must-read for any Paracelsus enthusiast. It was, however, a very difficult task for this reviewer with intermediate skills in German because of its specialized vocabulary and the liberal use of untranslated phrases in Latin, Greek, medieval German, and modern Swiss dialect. Like an American in a Berlin cabaret, I understood that the audience was laughing, but I missed many of the punch lines. The historian unprepared for the nuances of German philology might want to wait for an English translation, but that could involve a long delay.

Perhaps the best way to review this book is to emphasize that the title, *Das Werk - die Rezeption*, really states what the book is about: (1) what Paracelsus did and wrote and (2) what perceptions later generations had of his life. About half the articles deal with Paracelsus the balneologist (therapeutic user of baths), the practitioner of homeopathic medicine (syphilis and mercurials), the military field surgeon, the wandering healer, the "Luther of Medicine," and the cantankerous

rebel in the medical establishment of Basel. The other half of the articles deals with the fictionalized or rediscovered Paracelsus of later periods. To the extent that there exists a single theme and that the book is not just a compilation of unique thoughts occurring to euphoric party-goers, it is a summary of the peaks and valleys of

Paracelsus' esteem. In this sense the approach is similar to that of Jaroslav Pelikan in *Jesus through the Centuries: His Place in the History of Culture* (1985), which illustrates how widely posterity can range in its characterization of a single historical personality when given several millennia in which to reformulate. In the case of Paracelsus, we have had only five centuries for reformulation. There was a lifetime of "bombast" followed by repudiation and eclipse. A nadir was reached in 1777 when the Swiss writer and naturalist Albrecht von Haller declared, "The theories of this man are so insignificant that they scarcely deserve discussion." A resurrection attributable to the fastidious archaeology of Karl Sudhoff took place in 1911 with the assembly of 17,773 Paracelsus artifacts for exhibit in Leipzig. This painstaking historical effort was fol-



Tomb of Paracelsus, St. Sebastian, Salzburg, Austria

lowed (unfortunately) by Erwin Guido Kolbenheyer's three-part novel (*The Youth of Paracelsus*, *The Constellation of Paracelsus*, and *The Third Reich of Paracelsus*), which promoted a politically correct (for that time), popular—if historically inaccurate—conception of Paracelsus the "Ingenium Teutonicum." The trilogy published from 1917 to 1926 paved the way for the film "Paracelsus" (Script, Kurt Heuser; Direction, Georg Pabst) in 1943, which found in Paracelsus the Faust of National Socialism as magnificent in fantasy as was Paracelsus dwarfish in reality (Paracelsus was very short).

Highlights from the book include the reincarnation of Paracelsus (in the voice of Werner Rihm - *Paracelsus Basileae redivivus*) and a lament about Basel in the age of Golden Arches (McDonald's), two articles on the appropriateness of the epithet "Luther of Medicine," two articles on medical ethics of the 15th-century, and a review of German literature dealing with Paracelsus fictions.

The historian of science will find no coherent examination of what Paracelsus contributed to the basic sciences, although the book is filled with insights. In the opinion of Gundolf Keil, Paracelsus differed little from his contemporaries in the practice of medicine and regularly "cured his patients to death." Hartmut Böhme explains that Paracelsus specialized in treating the terminally ill and sought to understand a netherworld between the last flickers of life and death. This fits well with the thesis of Jolande Jacobi, that in the reading of Paracelsus science cannot be separated from theology [Jolande Jacobi, *Paracelsus: Selected Writings*, Bollingen Series XXVIII, Princeton University Press, Princeton, NJ, 1951.] Renewed appreciation for this side of Paracelsus

comes with the completion of Part II: Paracelsus Complete Works - Theological and Religious Writings by Goldammer [Kurt Goldammer, Ed., *Theophrast von Hohenheim, gennant Paracelsus. Sämtliche Werke. Zweite Abteilung: Theologische und religionsphilosophische Schriften*. Franz Steiner Verlag, Stuttgart. 7 volumes plus index, ca. 1955-1995.]

Contrary to the hope of editor Volker Zimmermann that this volume might rehabilitate Paracelsus, Gundolf Keil, who contributed three of the sixteen articles (two as *additamenta*), concludes that "Paracelsus never saw the Light of Nature." He remained entrapped in medieval thinking, and thus he strikes the modern reader as arcane and obscure. Although Paracelsus may not have led the way as a scientist, he may well be considered the founder of alternative medicine as we know it today. The light that he saw was the same one that shines in the work of Deepak Chopra, M.D., and the spiritualists of the Harvard Medical School. Physicians are not always judged by the diseases they cure. Sometimes they are judged by the spirits they lift. *Thomas W. Orme, 14001 Harpers Ferry Road, Purcellville, VA 20132-1729.*

John Dalton, 1766-1844: A Bibliography of Works By and About Him, with an Annotated List of his Surviving Apparatus and Personal Effects. A. L. Smyth, Ashgate Publ. Co., Brookfield, VT, 2nd ed., 1997, 192 pp., \$72.95.

John Dalton is a character well-known to chemists, particularly for his influential version of chemical atomism. The present volume is a revised and much expanded version of a Dalton bibliography produced by the same author in 1966. It begins with a brief overview of the long saga—both miraculous and tragic—of the Dalton archive. He recounts the archive's fate from its rediscovery bit-by-bit in the library of the Manchester Literary and Philosophical Society, through the disaster of Christmas Eve, 1940, when a German bomb set the Society's quarters alight, destroying three-quarters of Dalton's papers, to the 1979 sale of the remainder to the University of Manchester.

The majority of the book consists of well-organized and classified lists of primary and secondary Dalton materials. In the former category are classed his published works, manuscripts, notebooks, and letters. The second part lists the letters to and about Dalton, various "Daltoniana" (such as handbills and "laments" over his death), portraits, and scholarly books and articles dealing with Dalton. The third and final section catalogues surviving objects relating to Dalton: his apparatus and personal effects, medals, and so forth. The book concludes with indices of letters, names, and subjects. It is well illustrated throughout. Scholars interested in writing on Dalton, atomic theory, and related topics of the early nineteenth century will find this a valuable reference catalogue. *Lawrence M. Principe, Department of Chemistry and Department of the History of Science, Medicine, and Technology, The Johns Hopkins University, Baltimore, MD, 21218-2685.*