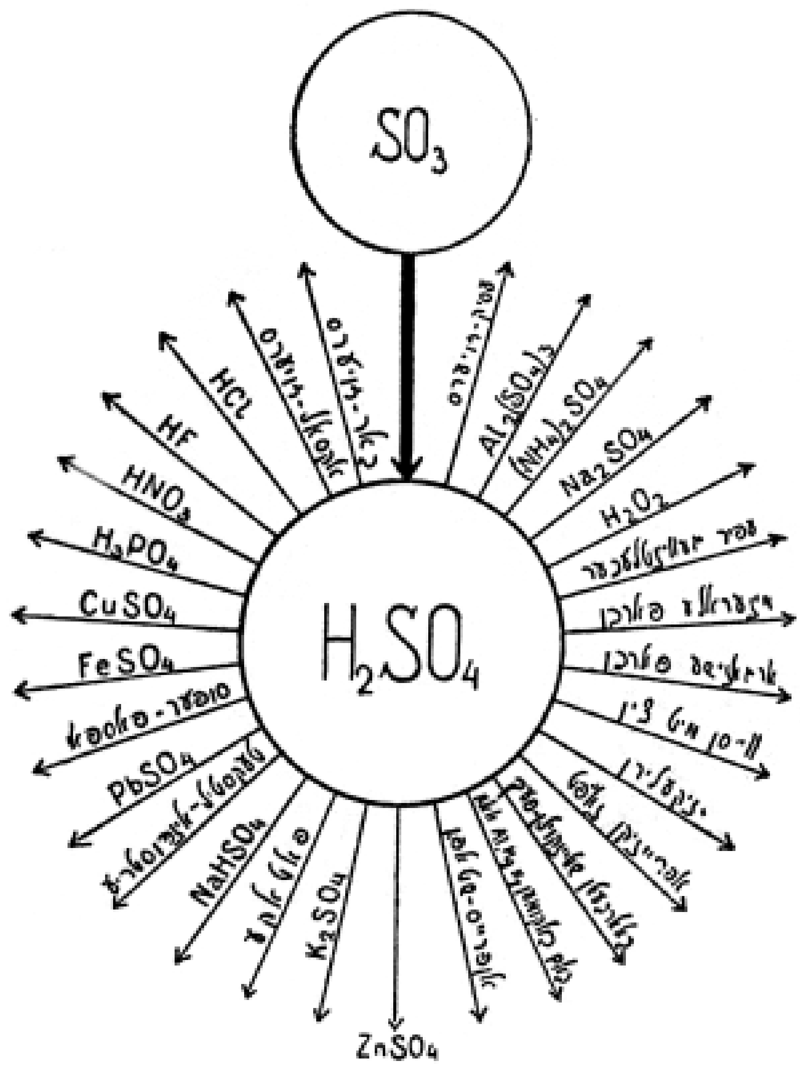
BULLETIN FOR THE HISTORY OF CHEMISTRY

Division of the History of Chemistry of the American Chemical Society

VOLUME 29, Number 1 2004



BULLETIN FOR THE HISTORY OF CHEMISTRY

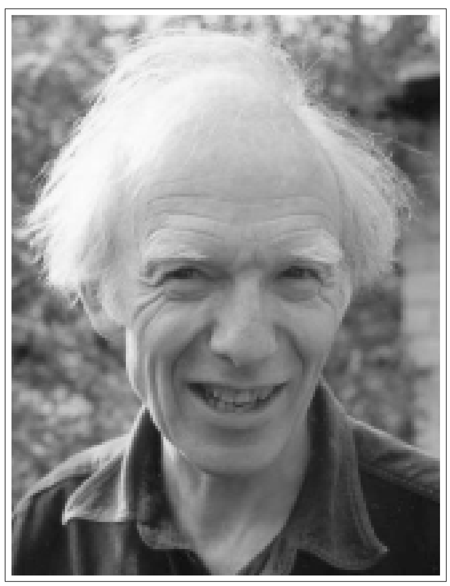
VOLUME 29, CONTENTS

NUMBER 1

The Cover…….See page 24.

THE 2003 EDELSTEIN AWARD ADDRESS* MAKING CHEMISTRY POPULAR

David Knight, University of Durham, England



David Knight

"Chemistry is wonderful," wrote Linus Pauling (1), "I feel sorry for people who don't know anything about chemistry. They are missing an important source of happiness." That is not how the science has universally been seen in our time. We would not expect to see lecture-rooms crowded out, chemists as stars to be invited to fashionable parties, or chemistry books becoming best-sellers. And yet, in the half century following the publication of Antoine Lavoisier's revolutionary book in the revolutionary year of 1789 (2), chemistry gave that pleasure to many, drew crowds, was seen as the fundamental science, and was made attractive to women as well as men, girls as well as boys, in accessible books and lectures. It was an important aspect of modernity, a science in which understanding the world went hand in hand with changing it.

Chemistry made good theater (3), if the experiments worked as they always did at the Royal Institution in London (or even if they did not) and before the heavy hand of 'health and safety' legislation was laid upon the science. Fertilizers and explosives seemed wholly beneficent in those optimistic days, the gas industry transformed urban life with well lighted winter evenings, and a bright dawn gleamed over a chemically-based society. Intellectually, the science did not demand the mathematics required for serious pursuit of the sublime science of astronomy. Chemists like Joseph Priestley thought it the ideal Baconian science in which everyone might join, for its theoretical structure was still unformed. Others in our Edelstein symposium have looked at France, Germany, and Russia, where government initiatives were crucial. Our chief focus will be Britain, a prospering society based upon patronage, where committees of interested people ran things, where there might be a black market in tickets for a chemistry lecture, where Humphry Davy was a chemical star (4), and where chemical literature was readable, and widely read (by the 1830s often on chemically-bleached paper).

One way to understand chemistry's popularity is to see it as appealing to body, mind, and spirit. Chemistry is the science of the secondary qualities, concerned with colours, tastes, smells, textures, and even sometimes (as with the 'pop' of ignited hydrogen) noises: there is endless stimulation for the senses in doing, or even watching, chemistry. The smell in particular of a laboratory is amazingly evocative, transporting one back vividly across the years. Chemistry also requires, or required, manual skills: it was necessarily experimental, and with experience the pleasure of manipulating apparatus (from weighing to blowing glass and handling dangerous substances) and getting results increased steadily. Then, in the years we are considering, chemical theory was fluid and not too recondite. There was not yet an enormous amount to learn. Journals were informal and open. The able person might move into the science and with astonishing rapidity be making serious contributions to knowledge, respected by peers. Then, like other sciences, chemistry could cast light upon God's working in nature. But chemistry was useful. Chemical natural theology therefore differed from other kinds in that the chemist sought to improve the world, whereas the astro-theologian contemplated, awestruck, the perfection of the heavens, and the physico-theologian the design evident in the eye of the eagle or the fly (5). Nevertheless, using God-given reason and manual skill to overcome pain, disease and hunger was highly significant spiritually (6).

Chemistry and the Body

Chemistry had always had connections with medicine, and academic chemistry was taught in medical schools in our period (7), during which drugs like 'Jesuits' bark' and opium, of doubtful provenance and efficacy, were analyzed and their active components prepared as white crystals (8). This meant that dosage could be controlled and effectiveness determined. Such analysis became a major research program, leading by the middle of the century to jobs in industry or in controlling pollution; though by 1840 synthesis (guided by the use of Jöns Jakob Berzelius' symbols on paper) was becoming extremely important (9) as the key to understanding chemical processes. Clearly, pharmacy was an important way in which chemistry would be useful; and with the isolation of 'airs,' notably by Priestley, a new range of chemical substances became available to the sick, or to those looking for new sensations. Thomas Beddoes, with money from the wealthy potter Josiah Wedgwood and equipment designed by James Watt, in 1798 set up in Bristol a Pneumatic Institution to treat disease with gases. Both Wedgwood and Watt (prominent members of the Lunar Society of Birmingham, with Priestley and Erasmus Darwin (10)), had sons suffering from tuberculosis, and oxygen seemed a promising treatment. Health, comfort and wealth would flow from chemistry.

In the event, these medical hopes which had focused attention on the latest chemistry were not fulfilled at that time, and before Beddoes' death in 1808 it was said that people were having to be paid to undergo the experimental treatments. But in 1799 young Davy (11), employed as Beddoes' assistant, discovered that nitrous oxide, feared by some (notably the American, Samuel Mitchill) as a deadly poison, a very 'septon,' was instead laughing gas. His subjective accounts of anesthesia remain classics; and this gas offered the pleasures of alcoholic indulgence without a subsequent hangover. Davy met the poet Samuel Taylor Coleridge, who was experimenting with drugs (having taken opium for pain relief on the suggestion of Beddoes (12)), and he and others tried the gas. In 1800 Davy published his first book, *Researches Chemical and Philosophical, Chiefly Concerning Nitrous Oxide,* in which part was devoted to the chemistry of the oxides of nitrogen, and part to the effects of laughing gas. It made his reputation, and made the breathing of nitrous oxide a craze. In 1801 Davy was appointed (by Benjamin Thompson, Count Rumford) to a position at the Royal Institution in London, and there is a celebrated cartoon by James Gillray showing the public administration of laughing gas there in the course of one of the fashionable lectures for which the Institution was celebrated (13).

Those watching this and other lectures could follow the lecturer's thought as he manipulated the apparatus in order to illustrate his exposition. And some of them at least were tempted to do the experiments themselves. In his last posthumously published book, *Consolations in Travel* (14), Davy commented upon the way in which chemists had in his lifetime come to deal with much smaller quantities, replacing furnaces by spirit lamps so that experiments could now be done in the drawing room. He also remarked that few chemists had retained through life a steady hand and a quick eye, for the laboratory was a dangerous place; but neophytes might perhaps be expected to avoid this spice of danger which made chemistry macho, and work with apparatus which might easily be contained in a small trunk or traveling carriage, and cost only a few pounds. Davy had himself, when visiting Napoleon's France to collect his prize for electrochemistry from the Academy of Sciences (accompanied by young Michael Faraday as assistant and servant), used such a box of apparatus in the preliminary work of elucidating the nature of iodine, in a race with Joseph Louis Gay-Lussac, though the research was completed in a fully equipped Parisian laboratory (15).

When he and William Wordsworth were settled in the Lake District, Coleridge had written earlier to Davy asking for advice in setting up a laboratory. Nothing seems to have come of the proposal, though Davy did punctuate some of Wordsworth's poems for the printer in London, stay with the Wordsworths at Dove Cottage, and subsequently climb Helvellyn with Wordsworth and Walter Scott. But at that time there were chests of apparatus, chemistry sets, commercially available and known as 'portable laboratories;' Brian Gee describes their development (16). They had begun as equipment for mineral surveyors or doctors testing mineral waters (doctors were already accustomed to carrying medical chests in their carriage as they visited patients, or on shipboard); but by 1800 they were being assembled by instrument makers for recreational purposes also. Thus William Henry in Manchester sold portable laboratories of different sizes at fifteen, eleven, or six and a half guineas (17). James Watt junior bought one of the top-price models, although Henry was soon grumbling at the trouble involved in assembling all the components in the provinces. London-made portable laboratories were bought by Davy's friend and physician William Babington (to whom Davy's fishing dialogues, *Salmonia* (1828) were dedicated), and by Bryan Higgins. Frederick Accum and then John Newman sold standard sets, with Accum asking £80 in his catalogue of 1817 for one suitable for 'a general course of chemical experiments.'

This would be a huge price, an investment for a would-be itinerant lecturer or an institution, at a time when Davy at the peak of his career was earning about £1,000 annually. Soon cheaper sets came onto the market, often accompanying a popular book, for texts at this time, such as Samuel Parkes' *Chemical Catechism* and Colin Mackenzie's *Thousand Experiments in Chemistry* listed many experiments to be performed, as did Michael Faraday's only book, *Chemical Manipulation* (18), which describes how to carry out processes such as weighing and bending glass tubes in ways that might still be helpful to the practical chemist. Chemistry after all could not be learned in a meaningful way from books or lectures alone. Thus Gee tells us that in 1835 R. B. Ede sold small trunks of apparatus at one and a half or two guineas (superior grade, with stoppered bottles and French-polished box) to accompany J. J. Griffin's *Chemical Recreations.* By the middle of the century, forward looking schools were beginning to teach chemistry and used portable laboratories because they did not have a purpose-built room. Jane Marcet's famous *Conversations on Chemistry* (1807), was written to help those who had heard lectures by Davy (or someone less exalted), really understand what was going on. Those dialogues, written for girls, contain experiments with illustrations of apparatus (including hands, indicating how to manipulate it) and perhaps real governesses followed the example of 'Mrs. B' in the book and used a portable laboratory with their charges (19).

Fifty years ago I learned chemistry in a school laboratory built at the end of the nineteenth century, and the experiments with which we began went back to the time of Priestley and Lavoisier. We collected gases over water, we weighed, we bored corks, then we titrated and heated as we progressed towards about the time of Robert Bunsen. The sheer sensual pleasures of chemistry enthralled us (as it did Oliver Sacks, where he vividly describes it as saving him from childhood miseries (20)); and we also (though forbidden) dissolved pennies in nitric acid and squirted each other with wash bottles. Later, doing ether distillations and handling concentrated acids and other unpleasant or poisonous substances gave that spice of danger which Davy and his contemporaries had relished. In the nineteenth century, chemistry had led the way in hands-on practice—physicists might think of it as mere advanced cookery, but chemists knew better—and anyway, cookery is not to be despised, nor are manual skills and bodily satisfactions. But we may wonder how readily available these things were in the early nineteenth century, to those who were not well-off supporters of literary and philosophical societies, athenaeums, or academies.

In 1824, when William Nicholson's informal *Journal of Natural Philosophy* had long ago been taken over by *The Philosophical Magazine* (which was also soon to swallow *Annals of Philosophy)*, a new journal, *The Chemist*, was launched, coming out weekly in octavo parts of sixteen pages, and costing 3d (about 8c), so that 80 issues would have cost a pound. It was illustrated with woodcuts in the text, rather than expensive copper-plates, many of them showing apparatus, and was aimed at working men—skilled artisans rather than laborers. It was a part of the 'march of mind,' going with Mechanics' Institutes and the Society for the Diffusion of Useful Knowledge, as more people learned to read in the Sunday Schools, and in the weekday 'monitorial' schools, founded by the churches in educationally backward England, when political reform was at last on the agenda. In his opening editorial the editor, looking for a chemical hero, was therefore critical of Davy, who as figurehead (21):

> …professes a sort of royal science. If in its pursuit he makes any discoveries which are useful to the multitude, they may, and welcome, have the benefit of them. But he has no appearance of labouring for the people. He brings not the science which he pursues down to their level; he stands aloof amidst dignitaries, nobles, and philosophers; and apparently takes no concern in the improvement of those classes for whom our labours are intended, and to whom we look for support. Amidst all the great efforts which have been lately made to promote scientific instruction among the working classes, and amidst all the patronage which these efforts have found among opulent and clever men, it has been with regret that we have sought in vain to trace one exertion or smile of encouragement bestowed on such efforts by the President of the Royal Society.

The use of the term 'working classes' was unusually early, but the message was that elite chemistry was not popular. Instead *The Chemist* recognized the difficulty working men would have in assembling apparatus. Faraday in his book was to advocate the use of ordinary household equipment wherever possible, and to advise on making a cheap and ingenious balance for those who had no access to a proper one. He also urged the reader to contrive things out of glass tubing and pieces of wood. On the first page of the first number of *The Chemist*, we find the reassuring message that many experiments may be carried on with 'a simple and cheap apparatus,' and that 'experiments conducted on a small scale have led to most of the brilliant discoveries of our times,' and noting that the galvanic battery and the blowpipe will only work on small quantities. Heat may be supplied by an ordinary fireplace and bellows, while for other operations 'a few glass retorts and phials, a small lamp and a common bason' are all that is needed. The editors promised to make a point of describing cheap and easy experiments for readers to perform and included sensible advice on cleanliness and labeling. Each number of *The Chemist* did indeed include a description of one or more pieces of equipment and gave advice on manipulation and on the recycling of damaged glassware. The journal was high-minded in rejecting advertising (many chemistry books also functioned as trade catalogues) and in paying authors, and therefore did not last very long. But it did point to the delight in chemical experiment that working men shared with the more leisured; whether their daughters got much of a look is doubtful. Chemistry was a science in which manual skills had to be developed to give bodily dexterity and sensual pleasure.

Chemistry and the Mind

What then about the mind? All science should give intellectual satisfaction, but with chemistry at this time the relatively undeveloped state of theory made it particularly exciting and approachable. Lavoisier had described his own work as a revolution, akin to what was happening at just the same time in French political life. And 'revolution,' which had meant in Britain in 1688 and in America in 1776 a return to the supposed lost liberties of Merrie England before the Norman yoke was imposed, came with the French revolution of 1789 to mean instead a new departure, an escape from the past rather than a restoration of it. Thus the new language of Lavoisier and his associates (22) was a fresh start, making the task of learning chemistry much easier for the neophyte. With its basis in the logic of Condorcet and Condillac, and thus ultimately of Locke, this new language (seen by Thomas Kuhn as a crucial feature of scientific revolutions (23)) was to be a kind of algebra, clear and free from personal, adventitious, or historical associations, incapable of metaphor or flights of fancy. With Priestley and Lavoisier, chemistry had expanded to include all three phases of matter: it was no longer a branch of cookery or pharmacy, and indeed Davy could define it as a wide-ranging and fundamental activity (24):

> Chemistry relates to those operations by which the intimate nature of bodies is changed, or by which they acquire new properties.

Chemical theory was also controversial, something which always attracts outsiders far more than calm certainty. There was argument over whether the science needed to be theory-laden, should have an international language, should be seen as static or dynamic (based upon weights or forces), and how it should relate to other sciences.

Priestley had interpreted his work in the context of the phlogiston theory; and in the lectures he delivered in Hackney (to which he fled after his house in Birmingham was sacked by rioters in 1791), he compared Lavoisier's theory of oxygen (25) to the vortices by which Descartes had sought to account for planetary orbits. This was a classic put-down for the French, because the vortices had been magisterially shown to be false by Isaac Newton in his *Principia* (1687). But to Nicholson, author and translator of textbooks, author of a dictionary of chemistry, and editor of a journal important in its day, both Priestley and Lavoisier over emphasized theory. His ideal was the sober presentation of facts, and for him theory was an add-on. He indicated this devotion to the inductive philosophy of Francis Bacon by putting theories at the end of chapters and treating them as more or less probable aids to memory and organization—generalizations rather than serious guides to the structure of the world. He explained them (26):

> In such a way, as to create in the chemical student an habit of steadily and calmly attending to the operations of nature; instead of indulging that hasty disposition for theorizing, which indeed might pass, on account of its evident impropriety, without any earnest censure, if we had not had the mortification to see it too much practiced by men entitled to the best thanks of the scientific world, and on that account possessing greater power to mislead.

Nicholson evenhandedly put down Priestley and Lavoisier and found no trouble translating between the phlogistic and antiphlogistic languages of chemistry.

Lavoisier and his team had hoped that by choosing names like 'oxygen' and 'hydrogen,' based upon ancient Greek words, they would (as Linnaeus had with his botanical Latin (27)) create an international language, used by everyone. In this they were disappointed (28), for in Germany, Russia, the Netherlands, and elsewhere the terms were translated; so the Germans have Sauerstoff for oxygen, for example. For them, Lavoisier's error in thinking that oxygen was the generator of acids is constantly before their eyes; while for English speakers, who adopted the French forms and who no longer mostly have a classical education, the words convey nothing but chemistry. It is curious that although Britain was at war with France for much of the eighteenth century, and then for over twenty years after the 1789 revolution, there should have been no trouble in taking over the language. Although the French worry about *Franglais*, English has been for centuries very permeable to French, as we see with café, restaurant, government, and many other ordinary words. 'Oxygen' was theory-laden; but when convinced that his greenish choking gas was an element, and not oxymuriatic acid, Davy named it 'chlorine' for its color, and the avoidance of theory (except that metals normally end in 'um') has become general. Similarly, the French 'azote,' (deadly), was replaced in English by 'nitrogen,' since it was a faulty description. When Berzelius eventually told his housekeeper Anna to say 'chlorine' because that was better, he was signaling that he had changed his theory of acidity from Lavoisier's to Davy's.

Lavoisier's chemistry was based upon weights and careful bookkeeping, as in his job in the tax farm where the accounts had to balance (29). Priestley had another vision, of a science based upon forces (30):

> Hitherto philosophy has been chiefly conversant about the more sensible properties of bodies; electricity, together with chymistry and the doctrine of light and colours, seems to be giving us an inlet into their internal structure, on which all their sensible properties depend. By pursuing this new light, therefore, the bounds of natural science may possibly be extended, beyond what we can now form an idea of. New worlds may open to our view, and the glory of the great Sir Isaac Newton himself, and all his contemporaries, be eclipsed, by a new set of philosophers, in quite a new field of speculation.

In his laws of motion and of gravity, Newton had gone beyond the facts of astronomy to disclose the underlying forces and the ultimate simplicity, order, and beauty of the world. But mechanics went less deeply than chemistry, if allied with electricity, could do. With the publication in 1800 of Alessandro Volta's paper on the electric pile, this alliance was cemented. Different metals immersed in water, or better in dilute acid, generated electricity; and the subsequent researches of Nicholson and then in 1806 onwards of Davy established electricity as a chemical science. The conclusion for which Davy was awarded his prize by the Parisian Academy of Sciences was that electricity and chemistry were manifestations of one power. Berzelius was to build this insight into his account of chemical affinity, as 'dualism:' every compound had its positive and negative pole. Even before Davy's great papers Friedrich Schelling (31), Johann Ritter, and others in the German tradition of Naturphilosophie had sought a dynamical chemistry in which combination was a true synthesis of opposites in a world of flux and process. Chemical logic lay behind the romantic publication of 'fragments' by Friedrich Schlegel (32). There could be many candidates standing as 'the Newton of Chemistry,' and much exciting discussion about matter and force, especially as chemists explained respiration and photosynthesis and seemed to be casting light on the vital principle itself. Here was excitement, but where the speculation was allied to and controlled by experiment.

Chemistry and Spirit

There is less to be said about chemistry and spirituality than would be the case with astronomy or natural history, where popularization was very generally in the form of praise for the Creator, and William Paley's famous *Natural Theology* was published in 1802. Joseph Priestley (who was by profession a minister) had in his *Disquisitions Concerning Matter and Spirit* (1777) put across his view that matter was active, its point atoms being centers of force. There was no reason why some suitable arrangements of such matter could not think, and Priestley therefore embraced a Christian materialism bringing together his Unitarian faith and his dynamic idea of matter. He believed that the doctrine of immortal souls had drifted into true 'primitive' Christianity from pagan Platonists; and that we were material beings, who at death came to an end. Death was not a family reunion. We did not survive it as disembodied souls. The promised resurrection of the dead would happen as in medieval wagon plays or on the wall of the Sistine Chapel: when the angel blew the trumpet, the 'sleeping' dead would by a miracle be revived and face judgment. Contemporaries frightened by the French Revolution of 1789 were alarmed by Priestley's embracing of democracy and heresy; and, except among Unitarians, whose faith according to the Darwin and Wedgwood families was a feather bed to catch a falling Christian (33), his particular form of scientific religion did not catch on. Materialism remained very much a term of abuse through the nineteenth century.

Lecturers alluded in a general way to design; and when the Earl of Bridgewater died in 1829, bequeathing £8,000 to the Royal Society to commission treatises demonstrating the goodness and wisdom of God in the creation, the eminent physician and chemist William Prout was one of the eight authors selected (34). Whereas astronomy, physiology, geology, zoology, psychology, and even the human hand all received a treatise of their own, however, chemistry was shoehorned in with meteorology and the function of digestion (35). Prout, who had identified hydrochloric acid in the stomach, used the chemical part of the book to present a version of his famous hypothesis about the nature of matter and the complexity of the chemical elements; but apart from that, the arguments for a wise creator are conventional, and the work rather dull. Although the series as a whole was a great success, his was not much commented on and never became a classic as some did.

In 1838 Mrs Hannah Acton gave £1,000 to the Royal Institution for a prize to be awarded every seven years for a work of natural theology. The first in 1844 went to a chemist at the Middlesex Hospital in London, George Fownes, whose essay was published that year. He believed that (36):

> …recent discoveries in chemistry, more especially in its relations to animal and vegetable physiology, lead to the hope that it may be possible to draw an inference of design from the chemical constitution of the earth and its inhabitants, hardly inferior in value to that derived from their physical study, although not always so obvious and striking.

Thus he was able to popularize the recent advances in organic chemistry, even what we would call biochemistry, and indicate the potential for chemical explanations of biological phenomena. There are discussions of chemical mechanisms and of organic analyses, giving a good snapshot of how things stood at this time. Justus von Liebig was the great man in this work, and the first two of his *Familiar Letters on Chemistry* (37) take up the same points: chemical natural theology was possible, indeed unavoidable. Awe and wonder at the extent and complexity of the creation and of the processes which sustain life were inevitable consequences of the serious (as opposed to the merely empirical) study of chemistry.

Those who dilate upon the wisdom and goodness of God have tended to be healthy and comfortably off. Another chemical perspective, further from easy optimism, is found in the *Religio Chemici* (38) of George Wilson, the first Professor of Technology (39) in the University of Edinburgh and a lifelong invalid. He had long hoped to write it, but it was incomplete at his death and was published posthumously by his sister. He was prepared to face up to the evil and pain in the world and was perplexed by the way in which, although we exist through the continual flux of our material components, repairs after injury or aging are never complete. The new materials reform the scars and wrinkles. The book, a collection of essays originally conceived on the model of Sir Thomas Browne's *Religio Medici* (1642) (40), also contains biographical essays valuable to the historian, reminding us that the lives of chemists then and since make the science interesting and perhaps popular.

We do not therefore need to marvel that chemistry should have been popular in those years after the French and Chemical Revolutions of 1789; it had everything, appealing to body, mind, and spirit. Whether the chemistry of our time can be made as attractive remains to be seen; but a great deal of specialization, unhappy experience of reluctant students with examinations to pass, and scientific disasters lie between us and the cheerful childhood of the science.

REFERENCES AND NOTES

* Presented at the Edelstein Award Symposium, 226th National Meeting of the American Chemical Society, New York, Sept. 9, 2003, HIST 016..

1. C. G. Gaither and A. E. Cavasos-Gaither, Ed., *Chemically Speaking; a Dictionary of Quotations,* Institute of Physics, Bristol, 2002, 118.
2. A. L. Lavoisier, *Elements of Chemistry,* tr. R. Kerr [1790], Dover, New York, 1965; also reprinted as Vol. 1 of the collection, D. M. Knight, Ed., *The Development of Chemistry 1789-1914,* Routledge, London, 1998.
3. D. M. Knight, "Scientific Lectures: a History of Performance," *Interdisciplinary Science Reviews,* **2002,** 27, 217-24.
4. D. M. Knight, *Humphry Davy: Science and Power,* Cambridge University Press, Cambridge, 2nd ed., 1998.
5. J. H. Brooke and G. Cantor, *Reconstructing Nature: the Engagement of Science and Religion,* T. and T. Clark, Edinburgh, 1999, 313-46.
6. D. M. Knight, *Science and Spirituality: the Volatile Connection,* Routledge, London and New York, 2004.
7. W. Babington, A. Marcet, and W. Allen, *A Syllabus of a Course of Chemical Lectures Read at Guy's Hospital,* Lancaster, London, 1811.
8. A. H. Maehle, *Drugs on Trial: Experimental Pharmacology and Therapeutic Innovation in the Eighteenth Century,* Rodopi, Amsterdam, 1999.
9. U. Klein, *Experiments, Models, Paper Tools: Cultures of Organic Chemistry in the Nineteenth Century,* Stanford University Press, Stanford CA, 2003.
10. J. Uglow, *The Lunar Men: the Friends who Made the Future,* Faber and Faber, London, 2002.
11. J. Z. Fullmer, *Young Humphry Davy: the Making of an Experimental Chemist,* American Philosophical Society, Philadelphia, PA, 2000.
12. A. Hayter, *Opium and the Romantic Imagination,* Faber and Faber, London, 1968, 27.
13. F. A. J. L. James, Ed., *The Common Purposes of Life: Science and Society at the Royal Institution,* Ashgate, Aldershot, 2002, 109.
14. H. Davy, *Consolations in Travel: or the Last Days of a Philosopher,* Murray, London, 1830, 247-55.
15. B. Bowers and L. Symons, Ed., *Curiosity Perfectly Satisfyed: Faraday's Travels in Europe 1813-1815,* Peter Peregrinus, London, 1991, 26-30.
16. B. Gee, "Amusement Chests and Portable Laboratories: Practical Alternatives to the Regular Laboratory," in F. A. J. L. James, Ed., *The Development of the Laboratory,* Macmillan, London, 1989, 37-59.
17. W. Henry, *Epitome of Chemistry,* Johnson, London, 1801, 3rd ed. 1803, vi, 3, 14. A guinea was one pound and one shilling (of which there were twenty to the pound).
18. S. Parkes, *The Chemical Catechism, with Notes, Illustrations, and Experiments,* Lackington Allen, London, 3rd ed., 1808; C. Mackenzie, *One Thousand Experiments in Chemistry, with Illustrations of Natural Phenomena; and Practical Observations on the Manufacturing and Chemical Processes at Present Pursued in the Successful Cultivation of the Useful Arts,* Phillips, *London,* 2nd ed., 1822; M. Faraday, *Chemical Manipulation: Being Instructions to Students in Chemistry on the Methods of Performing Experiments of Demonstration or Research, with Accuracy and Success,* Murray, London, 1827; 3rd ed. (1842) reprinted in facsimile as Vol. 5 of the collection D. M. Knight, Ed., *The Development of Chemistry 1789-1914,* Routledge, London, 1998.
19. B. Polkinghorn, *Jane Marcet: an Uncommon Woman,* Fleetwood, Aldermaston, 1993.
20. O. Sacks, *Uncle Tungsten: Memories of a Chemical Boyhood,* Alfred A. Knopf, New York, 2001.
21. Anon., *The Chemist,* **1824,** *1*, vi, vii.
22. G. de Morveau, A. L. Lavoisier, C. L. Berthollet, and A. F. Fourcroy, *Méthode de Nomenclature Chimique,* Cuchet, Paris, 1787, reprint Petrogal, Lisbon, 1991.
23. T. S. Kuhn, *The Structure of Scientific Revolutions,* University of Chicago Press, Chicago, IL, 1962.
24. H. Davy, *Consolations in Travel,* Murray, London, 1830, 247.
25. J. Priestley, *Heads of Lectures on a Course of Experimental Philosophy,* Johnson, London, 1794, 3.
26. W. Nicholson, *The First Principles of Chemistry,* Robinson, London, 3rd ed., 1796, viii-x.
27. W. T. Stearn, *Botanical Latin,* David and Charles, Newton Abbott, 1966.
28. B. Bensaude-Vincent and F. Abbri, *Lavoisier in European Context: Negotiating a New Language for Chemistry,* Watson, Canton, MA, 1995.
29. J. P. Poirier, *Lavoisier: Chemist, Biologist, Economist,* PENN, Philadelphia, PA, 1996.
30. J. Priestley, *The History and Present State of Electricity, with Experiments,* Bathurst and Lowndes, London, 3rd ed., 1775, reprinted Johnson, New York, 1966, xiv-xv, 1.
31. F. W. J. Schelling in E. E. Harris, P. Heath, and R. Stern, Ed., *Ideas for a Philosophy of Nature,* Cambridge University Press, Cambridge, 1988; D. M. Knight, *Ideas in Chemistry: a History of the Science,* Athlone, London, 1992, 54-67.
32. M. Chaouli, *The Laboratory of Poetry: Chemistry and Poetics in the Work of Friedrich Schlegel,* Johns Hopkins University Press, Baltimore MD, 2002.
33. A. Desmond and J. Moore, *Darwin,* Penguin, London, 1992, 5.
34. J. R. Topham, "Beyond the 'Common Context:' the Production and Reading of the Bridgewater Treatises," *Isis,* **1998,** *89*, 233-62.
35. W. Prout, *On Chemistry, Meteorology, and the Function of Digestion,* Pickering, London, 1834; W. H. Brock, *From Protyle to Proton; William Prout and the Nature of Matter, 1785-1985,* Adam Hilger, Bristol, 1985.
36. G. Fownes, *Chemistry as Exemplifying the Wisdom and Beneficence of God,* Churchill, London, 1844, ix.

37. J. Liebig, *Familiar Letters on Chemistry,* Taylor, Walton and Mabberly, London, 3rd ed., 1851, 1-24. This edition was reprinted in the collection, D. M. Knight, Ed, *The Development of Chemistry 1789-1914,* Routledge, London, 1998. These letters were not present in the 1845 edition, my copy of which seems to have belonged to William Kirby, the Bridgewater author.
38. G. Wilson, *Religio Chemici,* Macmillan, London, 1862.
39. R. Anderson, "'What is technology?': Education through Museums in the mid 19th century," *Brit. J. Hist. Sci.,* **1992,** *25*, 169-84.
40. G. Keynes Ed., *The Works of Sir Thomas Browne,* Faber and Faber, London, 1928, Vol. 1, 3-93.

ABOUT THE AUTHOR

Dr. David Knight is Professor Emeritus of Philosophy, Department of Philosophy, University of Durham, 50, Old Elvet, Durham, DH1 3HN, England; d.m.knight@durham.ac.uk.

THE DISCOVERY OF LECITHIN, THE FIRST PHOSPHOLIPID

Theodore L. Sourkes, McGill University

Phospholipids (1) have important functions as key elements of cell membranes. In recent years they have been recognized also as the source of important intracellular messengers, thus endowing them with more than a structural role. Their discovery at the beginning of the nineteenth century is intimately tied to the beginnings of the modern study of the chemistry of the brain. This paper traces the events leading up to the discovery of lipid-bound phosphorus (2) in the brain and some other tissues. The story unfolds through the work of six chemists, spanning more than a century, the first of whom was Johann Thomas Hensing.

Johann Thomas Hensing (1683-1726)

Hensing was born in Frankfurt/Main on August 30, 1683, into a medical family. At the age of 18 he enrolled in the Philosophical Faculty at Leipzig. His plan to study theology was interrupted by illness. Following restoration of his health, he registered as a student in the Medical Faculty. He completed his studies in Giessen where, except for a brief period in Frankfurt, he remained for the remainder of his life.

Initially Hensing was district medical officer in Giessen, but in 1712 he was appointed Privatdozent in Medicine at the University. In 1717 his status was raised to that of Professor Extraordinarius, and five years later he received the title of Professor Ordinarius of Natural and Chemical Philosophy in the Philosophical Faculty. He was fortunate in his promotions in having had Professor G. C. Möller as his mentor when beginning his medical practice. Möller had been in charge of the teaching of chemistry in the Medical Faculty, and Hensing succeeded him in this responsibility. Laboratory facilities allowed the young professor to carry out his notable study of the chemical composition of the brain, the results of which he published in 1719 (3).

Hensing chose the brain as an object of study because he recognized that organ as "truly the throne of the soul and the abode of wisdom, from whose nature the former is the recipient of the virtues of health, and the latter of brilliance (4)." To apply chemical analysis in the effort to understand such abstract conceptions was, indeed, a materialistic proposition, although Hensing could hardly have expected to end his work with a precise chemical discovery about the brain. He published the results of his study in Latin, with the title: "The Chemical Examination of the Brain and the Unique Phosphorus from which it Ignites all Combustibles." The translation of the essay is by Tower (5).

Hensing's analysis of the brain included the 'volatiles' (chiefly water), solids, and ash. Examination of the last item revealed the presence of elemental phosphorus. This was a highly original discovery, for until Hensing's work, phosphorus had been found only in excreta, from which it was prepared commercially, and in the ash of vegetable matter. Although his finding was mentioned subsequently by a few writers, there is no reference to it in the writings of the most popular authors of chemistry texts and compendia at the end of the eighteenth century. The work of the Giessen chemist probably received more attention only after it was mentioned by Johann Friedrich John (1782-1847) in his translation of N. L. Vauquelin's thesis (6) and in his comprehensive chemical tables of the animal kingdom (7). Many years later J. L. W. Thudichum (1829-1901) noted that Hensing's discovery was (8):

> The earliest distinctly chemical fact ascertained by research conducted on brain matter. … The discovery of [phosphorus there] was no doubt made by the methods of Brandt and Kunckel, the discoverers of phosphorus, and was one of the many results of the great impulse which the then marvellous productions of these accomplished apothecaries had given to the study of chemistry in the principal European countries.

Finding phosphorus in the brain was especially intriguing, for its properties, especially its light-emission (phosphorescence), suggested to some a kind of relationship to thought and the production of 'ideas.' Many years later the French physician and philosopher Georges Cabanis (1757-1808) proposed an intimate relationship between phosphorus and mental states, even implying that the element is formed in the brain. Cabanis's view exerted an influence well into the nineteenth century (9), despite the disproof of phosphorus's vital origin by John (10).

Johann Thomas Hensing, M.D. circa 1724
(D.B. Tower, Ref. 3)

Another work by Hensing bears an interesting title (11):

> Dr. Johann Thomas Hensing extends a courteous and loving invitation to the senior members and patrons of the Academy, as well as to the most excellent, illustrious and honorable citizens to [attend] the funeral solemnities of Lais—not she of Greece [i.e. a famous courtesan], but rather of the whole world—that is, of alchemy, who is thought to be the elder daughter of Chemistry, which will take place on a coming day in October, and in which he will publicly conduct chemical demonstrations.

It is noteworthy that Hensing addressed his book not to some exalted sponsor, but rather to a wide audience of readers interested in science, which was now progressing beyond alchemy.

Antoine-François Fourcroy (1755-1809)

Fourcroy was born in Paris, the son of a much respected pharmacist. He studied medicine, receiving his degree in September 1780. Shortly thereafter he was elected to associate membership in the Royal Society of Medicine. He had already chosen chemistry as his field of professional interest and was lecturing on the subject even before completing his medical course. He opened a private laboratory in which a succession of brilliant young men received their training. Fourcroy was soon appointed professor of chemistry at the Royal Veterinary School at Alfort and also at the Jardin du Roi. He was named Director of the Museum of Natural History (12).

Fourcroy investigated a large number of diverse subjects, publishing many scientific papers, often with his protégé and friend Nicolas-Louis Vauquelin (1763-1829). When Fourcroy was appointed in 1785 to the commission to oversee the removal of the Cemetery of the Holy Innocents in Paris to the Catacombs, he and the head of that body, M.-A. Thouret (1748-1810), found the opportunity to make chemical observations on some of the cadavers. They were especially impressed with the apparent state of preservation of the brain, even in corpses long interred.

Fourcroy soon realized that to obtain reliable data, it would be necessary to work with fresh brains. It is noteworthy that he eschewed the older methods that depended so largely on distillation and capturing of the degradation products for analysis. He used extraction procedures with aqueous solvents and alcohol, methods that led him to the conclusion that the brain consists of "animal pulp," (largely protein), fatty substances that he regarded as "soaps," and salts, chiefly phosphates of calcium, ammonia, and sodium (13). Among the fatty substances was a 'greasy oil,' later to be recognized as phospholipid. Neither in his "Cemetery papers" nor in his extensive compendium of chemistry does Fourcroy make mention of Hensing's work. He was apparently unaware of the prior finding of phosphorus in the brain (14).

Johann Ludwig Jordan (1771-1853)

A decade after Fourcroy's studies of brain chemistry, the German chemist Johann Ludwig Jordan undertook to repeat his work. Unlike Fourcroy, who was attracted to brain studies through rather practical considerations, Jordan expressed his interest in the composition of the brain philosophically (15):

> We must well wonder that one of the most important animal substances, in which the origin of mind and the seat of the soul have been sought, has thus far so little aroused the curiosity of chemists. Blood, bile, milk and other matters have already been worked upon so often and repeatedly that our knowledge of these is considerable, whereas we still stand almost in the dark here [i.e. with respect to the brain.] Is the brain then not less important? It would be indisputable to wish very much that chemists might agree to work upon this important subject, just as has been done for other animal substances.

Jordan was born in Göttingen on June 6, 1771. He attended the university there, eventually receiving a degree in medicine. For a short while he had a medical practice in Clausthal, but his interest in chemistry soon drew him away from medicine. He became committed to analytical work in mineralogy, and in his papers on the brain he states that he is no longer in a position to carry on the work of Thouret and Fourcroy. He ultimately was appointed Master of the Mint in Clausthal (16).

The essence of his work on the brain is as follows: the desiccated tissue, when burned in an open crucible, gives an acid reaction, which he suspected was due to phosphoric acid contaminated with sulfuric acid. An aqueous extract of brain from which the protein had been precipitated was treated with ground lime; this gave rise to ammonia, presumably released from ammonium phosphate. In another experiment he was able to isolate, with the addition of limewater, calcium phosphate.

Jordan carried out many other experiments, from which he concluded that the brain mass contains water, albumin (protein), sodium, ammonium, and calcium phosphates, and "a characteristic fatty material." This last component corresponds to Fourcroy's 'greasy oil,' and represents confirmation of the work of the French scientist. Jordan regarded his lipid extract as a distinctly animal product not encountered elsewhere. He located it in the medullary portion of the brain [*i.e.*, the white matter] and in the marrow of nerves (15).

Jordan retired in 1845 on a pension and died on May first, 1853 in Osterode, not far from Clausthal.

Nicolas-Louis Vauquelin (1763-1829)

Vauquelin has been the subject of numerous biographical articles and eulogies that describe his rise from impoverished family origins in St. André d'Héberdot, in Calvados, Normandy, to that of the élite of French science in the first three decades of the nineteenth century (17). His career began when he arrived in Paris and found work in a pharmacy, where he had the good fortune to meet Fourcroy. The senior chemist took the young man under his wing in 1784, giving him a post in his own laboratory. This was a major step in Vauquelin's professional development. Having obtained his diploma in pharmacy in 1792 and master's degree in 1795, he was invited to join the faculty of the School of Pharmacy, shortly thereafter becoming Professor of Chemistry there, and eventually its director, from the time of its reorganization in 1803 until his death in 1829. Under the French system permitting the holding of multiple posts, Vauquelin was also Professor of Chemistry at the Museum of Natural History and at the Medical Faculty, and for a time was Master of the Mint. He was celebrated throughout Europe for his achievements in analytical chemistry, as well as for his discovery of chromium and beryllium.

Following the death of Fourcroy in 1809, Vauquelin was appointed to the chair of chemistry at the Faculty of Medicine, despite the fact that he lacked a degree in medicine. His very extensive medical knowledge, together with a thesis on the subject of the analysis of cerebral matter of man and animals (18), earned him the doctorate, as well as the chair (19). He held this post until March 1822 when, following student demonstrations, the faculty was suppressed by le Comte de Villèle, the minister overseeing the medical school. A year later, the faculty was allowed to re-open, but professors considered unfriendly to the régime were excluded, among them Vauquelin, who was known to hold liberal views (20).

Before considering Vauquelin's work on the brain described in his thesis, his analysis of fish roe must be mentioned. A few years after Jordan's illuminating work on brain lipids, Fourcroy and Vauquelin reported the discovery of phosphorus in fish roe. Their preliminary experiments showed that the roe is neutral in reaction; yet the residue from its combustion is strongly acidic. The acid was characterized as phosphoric acid. The authors thought that it must have been formed during combustion. When they resorted to distillation of this fish product, they noticed elemental phosphorus condensing on the walls of the distillation tube.

In other experiments, they extracted fish roe with alcohol and obtained a "soap-like material," which contained phosphorus. The authors proudly state that (21):

> The discovery of phosphorus in a combustible state in organized bodies [*i.e.*, living matter] belongs entirely to Messieurs Fourcroy and Vauquelin.

Jordan had identified the new "fatty" substance in brain but had missed the fact that it contained the phosphoric acid he had identified.

Section IV of Vauquelin's thesis is entitled "Examination of the Fatty Matter of the Brain which is Precipitated during the Cooling of the Alcohol used to Extract this Organ." He states that the substance that he isolated was "white, solid but soft, and sticky; that it had a satiny and bright aspect, that it stained paper in the way that oil does" (18). He goes on to describe his first experiment (18):

> A portion of this material, which had been dissolved several times in alcohol in order to separate out from it the last of the animal substance [*i.e.*, protein], was burned in a platinum crucible. … The carbonized residue, washed with distilled water, rendered this fluid very acidic, with its ability to precipitate lime water. The unusual result of this procedure which, evidently indicated the presence of phosphoric acid, made me suspect that this fatty substance contained phosphoric acid in combination.

He continues:

> [I]n order to be sure about this … I diluted some [of the material] with distilled water. [The resulting emulsion] demonstrated no acidity, and did not affect litmus at all.

After describing another experiment, Vauquelin writes:

> I believe that I can conclude from these experiments that the brain substance involved here contains neither free phosphoric acid nor ammonium phosphate, and that consequently the acid which forms in the course of combustion has another origin……What is to be concluded from these experiments if not that there is phosphorus combined with fatty material in the brain and that the former is dissolved along with that fatty substance in alcohol? … One must necessarily accept that phosphorus is present in the substance of the brain, just as in the roe of fish, as discovered by Fourcroy and myself.

Finally, Vauquelin offers some words of caution:

> Although the substance we have described offers a closer relationship to the fats than to all other classes of substances, nevertheless it should not be identified with ordinary fat. It differs from fat mainly by its insolubility in alcohol, by its ability to form crystals, its viscosity, its lesser fusibility, and the black color, which it assumes on melting. Thus, while classifying it among the fatty bodies, it must be regarded as a specific and new substance.

The research that Vauquelin described in his thesis was destined to play a very significant role in the history of neuroscience, as the first complete analysis of the brain by state-of-the-art methods of chemistry. It was not only published in France, but soon appeared in translation in German and English journals (6, 22). Moreover, his extraction of 'white matter' from brain tissue with boiling alcohol, and its precipitation on cooling the solution, became the starting-point for several later investigators of brain chemistry.

Jean-Pierre Couerbe (1805-1867)

One of these investigators was Jean-Pierre Couerbe, a young French chemist hailing from the Bordeaux region. Couerbe trained in chemistry at the School of Pharmacy in Paris, working in several laboratories. For a period he was with Pierre-Joseph Pelletier (1788-1842) but left him in a dispute to work under the toxicologist M. J. B. Orfila (1787-1853) (23).

Couerbe introduced the use of ether as well as alcohol for extraction of lipids of the brain. Moreover, his was the first attempt to analyze the individual constituents making up Vauquelin's 'white matter.' Vauquelin had separated two 'fatty' fractions. Couerbe was able to separate five, one of which was cholesterol. His elemental analysis of the isolated cholesterol conforms very closely to the theoretical, a measure of the purity of his product. Thus, Couerbe demonstrated that it was a normal constituent of the brain. The other fractions were, from the present standpoint, mixtures. However, one of them, which was soluble in ether but not in alcohol or water, was saponifiable and contained phosphorus (24), and so exhibited the properties of phospholipids. This fraction he named 'céphalote' or 'brain wax.' He provided analytical data for this and the other fractions he had isolated. Although his elemental analysis of céphalote does not agree well with that for lecithin, his practice of characterizing each of his isolated fractions distinguished him as "the first to apply organic analysis to brain-products (25)."

Théodore-Nicolas Gobley (1811-1876)

Gobley was born on May 11, 1811, in Paris. He studied pharmacy there as a pupil of Pierre Robiquet (1780-1840), Vauquelin's successor. He received his diploma in pharmacy in 1835 and practiced his profession for many years. In 1842 he was appointed Professeur agrégé at the School of Pharmacy in Paris; the next year he joined the Société de Pharmacie de Paris. In 1861 he was elected to membership in the Academy of Medicine.

Theodore-Nicolas Gobley

Gobley's interests lay not only in laboratory work, but also in carrying out public responsibilities. He took time from his professional career to perform charitable work and to make social contributions as a member of the Council of Public Health of the Department of the Seine, of the Paris Commission on Unsanitary Housing, and of the Council of the Society for the Promotion of National Industry. He was assiduous in fulfilling these and the other functions he had accepted. He later became administrator, and then vice-president of the welfare offices of his district (26). In addition to these activities he was a member of many scientific societies. Tétry describes Gobley as "devoted, benevolent, and charitable, [a man] without ostentation (27)."

In his scientific work, Gobley dealt with a wide variety of subjects; but the one that concern us now was his research on the composition of hen's egg yolk, brain of several species, and carp organs. In his investigation of the lipid content of the yolk, he isolated lecithin, the first specific phospholipid to be recognized. This was in 1846 (28). He accomplished this by dehydrating the egg yolk and then extracting it with boiling ether or alcohol. Evaporation of the extract yielded an oily liquid and a soft, viscous substance. By hot filtration, the latter material was retained on the filter paper. The viscous matter was neutral to litmus, but on combustion its ash contained an acid, identified as phosphoric acid. The constituents of the viscous matter that Gobley described at that time were oleic and margaric acids (29), and a specific acid containing phosphorus, namely, phosphoglyceric acid. In addition, there was a base that he at first thought was ammonia.

In 1847 Gobley published a paper in two parts (30) comparing the chemical composition of egg yolk and brain. In it he stated that he had repeated all the egg yolk experiments with brain matter of chicken, sheep, and humans and had found the same fatty acids in the 'viscous matter' extracted from those sources as in egg yolk. However, he was unable to prepare the compound in a pure state. His work was presented to the Academy of Sciences by E. Frémy (1814-1894) who, unfortunately for Gobley, introduced his personal speculations about the composition of the lipids that Gobley had analyzed (31). Three years later, Gobley presented new work dealing with the roe of carp. It is in this paper that he gave the name 'lecithin' (from the Greek 'lekithos,' egg yolk) to what had hitherto been referred to as 'viscous matter' (32). In further work he identified this new entity also in the milt of carp (33), blood (*i.e.*, in the erythrocytes) (34), in bile (35), and even in the tissues of some lowly invertebrates, such as the sea nettle, the starfish, the sea urchin, medusa, and the sea anemone (36).

As for the basic constituent of lecithin, Gobley drew upon the finding by Adolf Strecker (1822-1871) of choline in bile in 1861-62—that is, a few years after he himself had discovered lecithin in that biological fluid (1856). Strecker, moreover, correctly deduced the structure of lecithin (37). Gobley then concluded that the choline in bile arose through the double decomposition of lecithin (38).

Gobley made numerous contributions to the chemical literature throughout his life, many based upon laboratory research, others of a literary nature such as his articles prepared for various encyclopedias. But it is his studies of animal lipids, particularly his elucidation of the chemistry of the phospholipid lecitihin, for which he is best remembered. He died at Bagnères-de-Luchon, a spa in the Haute-Garonne, on the first of September 1876, as the result of pulmonary disease.

The 130 years between Hensing's discovery of phosphorus in the brain and Gobley's description of lecithin saw many changes in chemical procedures for the isolation of natural products. The early customary methods of destructive distillation and incineration gave way to solvent extraction and other milder procedures, exemplified in this area of work by Fourcroy's use of aqueous solutions and alcohol. In Vauquelin's hands these techniques led to the recognition of organically bound phosphorus in the brain. Because alcohol was not an ideal solvent for this material, Couerbe's introduction of ether as an extractant advanced the recognition of phosphorus-bound lipid as a novel chemical entity. Gobley concluded the process by characterizing the material, giving it a specific name, and demonstrating its wide distribution in the animal kingdom.

REFERENCES AND NOTES

1. Phospholipids include phosphatidylcholine (lecithin), phosphatidylethanolamine, phosphatidylserine (the latter two being known as cephalins), phosphatidyl-*myo*-inositol, diphosphatidylglycerol (e.g. cardiolipin), and phosphatidylsphingosine (sphingomyelin). The phosphatidyl group is diacylglycerophosphoric acid. Phospholipids occur in all living matter but were first recognized in animal tissues.
2. Nomenclature of these compounds as a group has changed over the years. J. L. W. Thudichum introduced the term 'phosphatids' in 1884: "Historical Account of the Previous Researches on the Chemistry of the Brain," in J. Simon, *Reports of the Medical Officer of the Privy Council*, **1874,** *New Series, No. 3, Appendix 5*, 113-247; *A Treatise on the Chemical Constitution of the Brain*, Baillière, Tindall & Cox, London, 1884; *Die chemische Konstitution des Gehirns des Menschen und der Tieren*, F. Pietzcker, Tübingen, 1901. J. B. Leathes, *The Fats*, Longmans, Green, London, 1910, preferred 'phospholipines.' According to H. MacLean and I. S. MacLean, *Lecithin and Allied Substances*, Longmans, Green, London, 1927, an International Commission for the Reform of Nomenclature of Biological Chemistry recommended in 1923 the use of 'lipides,' hence phospholipides. E. B. Working and A. C. Andrews, "The Structure of the Phospholipids," *Chem. Rev.*, **1941,** *29*, 245-256, used the spelling 'phospholipid,' in common usage at the present time.
3. J. C. Poggendorff, *Biographisches-literarishces Handwörterbuch zur Geschichte der exacten Wissenschaften*, B. M. Israel, Amsterdam, 1970**,** 1, 1073. (Reprint ed.); K. Hufbauer, *The Formation of the German Chemical Community, 1720-1795*, University of California Press, Berkeley, CA, 1982; D. B. Tower, *Hensing, 1719. An Account of the First Chemical Examination of the Brain and the Discovery of Phosphorus Therein*, Raven Press, New York, 1983.
4. Ref. 3, Tower, p 241.
5. Ref. 3, Tower, p 224-315.
6. N. L. Vauquelin, "Analyse des Hirnmarks von dem Menschen und von einigen Thieren," *Schweigger's J.Chem Phys*., **1813**, *8*, 430-460. This is the translation by J. F. John.
7. J. F. John, *Chemische Tabellen des Thierreichs*, Maurer, Berlin, 1814.
8. Hensing, in fact, used a different procedure from that of Brandt (Ref. 3, Tower, especially p 210 ff.). It is, hence, likely that Thudichum had only the reference to Hensing's monograph but had not read it.
9. T. L. Sourkes, "An Element of Thought: Phosphorus and Mental Philosophy in the Nineteenth Century," *J. Hist. Neurosci*., **1998**, *7*, 108-124.
10. T. L. Sourkes, " Devitalising the Elements: Johann Friedrich John (1782-1847) and the Liberation of Phosphorus and Potassium from a Vital Force," *Ambix*, **2000,** *47*, 37-46.
11. J. T. Hensing, "Ad exequiarum Laidis, non Graeciae, sed magni orbis h.e. alchymiae, quae filia natu maior chemiae, habetur, solennia spectanda futuro die Octobris publice chemicas facturus demonstrationes Academiae patres, patronos atque cives excellentiss. Inclytos. ornatiss. officiose et peramanter invitat D. Io. Thomas Hensing," 1724. Prof. Faith Wallis, McGill University, kindly provided the translation of this title.
12. C. Chatagnon and P. A. Chatagnon, "Les Pionniers Français de la Chimie Cérébrale: les Citoyens. A. F. de Fourcroy (1755-1809) et L.-N. [sic] Vauquelin (1763-1829)," *Ann. Méd.-Psycholog.*, **1954,** *112*, 14-39; W. A. Smeaton, *Fourcroy: Chemist and Revolutionary, 1755-1809*, Heffer, Cambridge, 1962; G. Kersaint, *Antoine-François de Fourcroy (1755-1809). Sa Vie et son Oeuvre*, Editions du Muséum, Paris, 1966.
13. A. F. Fourcroy, "Examen Chimique du Cerveau de Plusieurs Animaux," *Ann. Chim.*, **1793,** *16*, 282-322; T. L. Sourkes, "The Origins of Neurochemistry: the Chemical Study of the Brain in France at the End of the Eighteenth Century," *J. Hist. Med. Allied Sci.*, **1992,** *47*, 322-339.
14. W. A. Smeaton, Personal communication to the author, letter of October 3, 1997.
15. J. L. Jordan, "Zerlegungsversuche mit dem menschlichen Gehirn" [Analytical Experiments on Human Brain], *Crell's Ann. Chem.*, **1803,** Part *5*, 364-369, and Part 6, 447-451.
16. A. C. P. Callisen, *Medicinisches Schriftsteller-Lexicon*, Copenhagen, 1832, Vol. 9, 515; Ref. 3, Poggendorff.

17. Ref.12, Chatagnon & Chatagnon; W. A. Smeaton, "Nicolas Louis Vauquelin," in C. C.Gillispie, Ed., *Dictionary of Scientific Biography,* Charles Scribner's Sons, New York, 1970-1980, Vol. 13, 596-598; A.Queruel, *Vauquelin et son Temps (1763-1829),* L'Harmattan, Paris, 1994.
18. N. L. Vauquelin, *Analyse de la Matière Cérébrale de l'Homme et de quelques Animaux,* Didot jeune, Paris, 1811.
19. E. Pariset, "Eloge de M. Vauquelin," *Mém. Acad. Roy. Méd.,* **1833,** *2,* 39-59.
20. P. Menetrier, "Le Centenaire de la Suppression de la Faculté de Médecine de Paris," *Bull. Soc. Fr. Hist. Méd. (Paris),* **1922,** *16,* 440-445.
21. A. F. Fourcroy and N. L. Vauquelin, "Expériences Chimiques, pour Servir à l'Histoire de la Laite des Poissons, *Ann. Chim. Phys.,* **1807,** *64,* 5-18.
22. N. L. Vauquelin, "Analysis of the Cerebral Matter of Man, and some other Animals," *Ann. Phil. London,* **1813,** *1,* 332-346. This is the translation by T. Thomson.
23. C. Chatagnon, and P.-A. Chatagnon, "La Chimie Cérébrale au Cours du XIXe Siècle en France. I. J. Couerbe (1807 [sic]–1867). II. E. Frémy (1814-1894)," *Ann. Méd.-Psycholog.,* **1954,** *112,* 364-390; D. B. Tower, *Brain Chemistry and the French Connection, 1791-1841,* Raven Press, New York 1994.
24. J.-P. Couerbe, "Du Cerveau, Considéré sous le Point de Vue Chimique et Physiologique," *Ann. Chim. Phys.,* **1834,** ser.2, *56,* 160-193.
25. J. L. W. Thudichum, *The Progress of Medical Chemistry,* Baillière, Tindall & Cox, London, 1896, 87.
26. [A. L. D.] Delpech, "Mort de M. Gobley," *J. Pharm. Chim.,* **1876,** ser.3, *24,* 328-333; Anonyme, *La Grande Encyclopédie,* Larousse, Paris, 1886-1902, Vol. 18, 1141; E. Bourquelot, *Le Centenaire du Journal de Pharmacie et de Chimie, 1809-1909; Histoire du Journal et Notices Biographiques,* Doin, Paris, 1910, 71-72; C. Chatagnon and P. Chatagnon, "L'Etude Chimique des Constituants du Tissu Cérébral au Cours du XIXe Siècle. Les Pionniers Français (III): Théodore-Nicolas Gobley (1811-1876)," *Ann Méd.-Psycholog.,* **1957,** *115,* 256-275; A. Tétry, in *Dictionnaire de Biographie française,* **1979**, Vol. 11, 395-396.
27. Ref. 26, Tétry.
28. T.-N. Gobley, "Recherches Chimiques sur le Jaune d'Oeuf," *J. Chim. Méd, Pharm. Toxicol.,* **1846,** ser.3, *3,* 320-329; "Recherches Chimiques sur le Jaune d'Oeuf [Premier Mémoire],"*J. Pharm. Chim.,* **1846,** *9,* 81-91, and 161-174.
29. Margaric acid is a C_{17} fatty acid. In effect, Gobley had a mixture of palmitic (C_{16}) and stearic (C_{18}) acids.
30. T.-N. Gobley, "Recherches Chimiques sur le Jaune d'Oeuf [Deuxième Mémoire]," *J. Pharm. Chim.,* **1947,** *11,* 409-417, and *12,* 5-12.
31. Frémy reported that Gobley had confirmed the presence of 'oleophosphoric acid,' a substance he claimed to have isolated from brain.
32. T.-N. Gobley, "Recherches Chimiques sur les Oeufs de Carpe," *J. Pharm. Chim.,* **1850,** *17,* 401-430, and *18,* 107-119.
33. T.-N. Gobley, "Recherches Chimiques sur la Laitance de Carpe," *J. Pharm. Chim.,* **1851,** *19,* 406-421.
34. T.-N. Gobley, "Recherches Chimiques sur les Matières Grasses du Sang Veineux de l'Homme," *J. Pharm. Chim.,* **1852,** *21,* 241-254.
35. T.-N. Gobley, "Recherches sur la Nature Chimique et les Propriétés des Matières Grasses Contenues dans la Bile," *J. Pharm. Chim.,* **1856,** *30,* 241-246.
36. T.-N. Gobley, "Sur la Lécithine et la Cérébrine," *J. Pharm. Chim.,* **1874,** ser. 4, *19,* 346-354.
37. A. Strecker, "Ueber das Lecithin," *Ann. Chem. Pharm.,* **1868,** *148,* 77-90.
38. Ref. 36, p 348-349.

ABOUT THE AUTHOR

Dr. Sourkes, Department of Psychiatry, Faculty of Medicine, McGill University, Montreal, Canada H3A 1A1, a biochemist, conducted research on neurochemistry and neuropharmacology for many years. He is particularly known for his basic contributions to knowledge of Parkinson's disease and its pharmacological treatment. Since retirement in 1991, he has turned to research on the history of the subjects he previously dealt with in the laboratory. His e-mail address: theodore.sourkes@mcgill.ca.

GABRIEL LIPPMANN AND THE CAPILLARY ELECTROMETER

John T. Stock, University of Connecticut

Early Potentiometry

Early workers in the field of potentiometry faced a serious problem. The type of cell commonly used in potentiometric studies could supply only a tiny current and often had a high internal electrical resistance. If the electromotive force (emf) of such a cell is to be measured accurately, the apparatus must draw essentially no current from the cell. One obvious approach is to oppose this emf with another that is exactly equal, and is generated externally. This is the basis of the "compensation method," devised by Johann Christian Poggendorf in 1841 and later improved by others (1).

The problem here lies in the detection of balance, that is, of the so-called "null point." The electromagnetic galvanometer is essentially a current-measuring device, but a galvanometer of the mirror type may serve when the cell resistance is not too high. However, this high-sensitivity galvanometer is not very convenient and is easily damaged by overload. The development of the nearly currentless capillary electrometer solved the problem. This device had the added advantage of simplicity and, with some later forms, of comparative robustness.

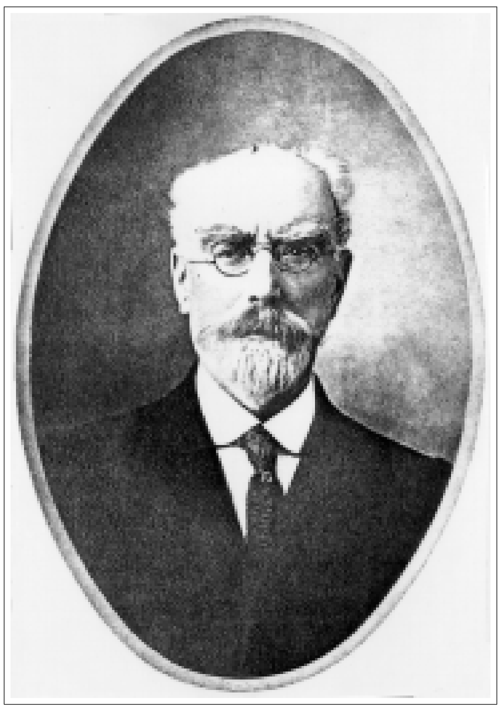


Figure 1. Gabriel Lippman, *Proc. R. Soc. London* **1922**, *101A*, i-iii.

Lippmann

The studies that led to the invention of this electrometer and to the formulation of the associated theory were not the work of a lifetime, but of that of a beginner, Gabriel Lippmann (Fig. 1). He was born near Luxemburg on August 16, 1845. His parents moved to Paris and eventually he was admitted to the École Normale. Pursuing only the topics that aroused his interest, Lippmann was not an ideal student. He failed in the examination that would have qualified him as a teacher (2). Nevertheless, his latent abilities were recognized and he was given the opportunity to study in Heidelberg, where the celebrated physicist, Gustav Robert Kirchhoff (1824-1887) was professor.

Lippmann saw a then well-known demonstration that involves a drop of mercury covered by dilute H_2SO_4. When touched by an iron wire, the drop contracts but regains its original shape on removal of the wire. The agitation of a mercury drop in contact with a voltaic cell was first observed by William Henry in 1800. Between Henry's observation and the work of Lippmann, Partington cites seven additional reports of the same phenomenon, the most important being John Draper's 1845 observation of the depression of an electrified thread of mercury confined in a capillary tube (3).

Lippmann, recognizing that the effect must be due to a connection between electric polarization and surface tension, developed the concept that led to the design of the capillary electrometer in Kirchhoff's laboratory. Having been granted a Heidelberg Ph.D. in 1873, Lippmann returned to Paris, where he obtained a second doctorate in 1875. After various appointments he became professor of mathematics at the Sorbonne in 1883, and then of physics in 1886. He held the latter position for the remainder of his life.

The Capillary Electrometer

Lippmann's publications on electrocapillarity began with a brief note in French (4). He explained that the contraction of the mercury drop was due to the electrical polarization of its surface, thereby changing its "capillary constant." Among other comments was that, if a mercury-dilute H_2SO_4 interface was formed in the capillary tip of a tube containing mercury, the observation by microscope of the displacement of the interface provided a sensitive measure of the emf applied to the system. Fortunately, while in Kirchhoff's laboratory, he had been able to use an electrostatic electrometer of the type invented by William Thomson (later, Lord Kelvin) (1824-1907) and described by him in 1867 (5). When this instrument was connected in place of the polarizing source, it could be shown that a mechanical displacement of the interface resulted in a deflection of the electrometer. The effect was found to be independent of the shape of the surface, but proportional to the change in its area. The note concluded with intriguing remarks about an electrocapillary motor that had been constructed. Almost simultaneously, Lippmann published in German a description of this motor and of the principles outlined above (6). Two years later an even more extended French paper appeared (7).

A detailed description of the motor, shown in Fig. 2, is beyond the scope of the present article. Battery power is applied alternately to the pistons of bunched capillaries that work in cylinders containing mercury. These stand in a trough containing dilute H_2SO_4. Vertical movement of the pistons leads to left-right oscillation of overhead piece *v* and hence to the rotation of wheel *R*, through the agency of crank *x*. The motor demonstrated that (i) electrical energy could be converted into mechanical energy by use of the principles that Lippmann had developed and (ii) that electrocapillary forces were by no means insignificant.

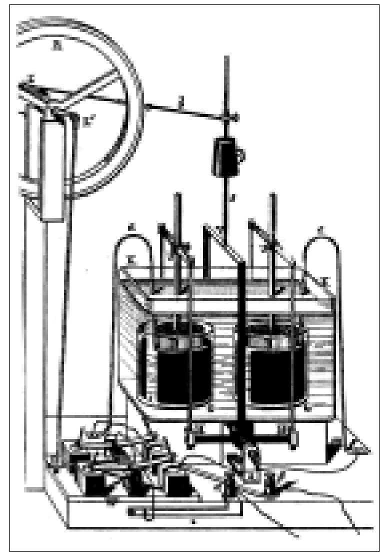


Figure 2. Electrocapillary motor; Ref. 5, p 522.

Lippmann pointed out that, hitherto, determinations of capillary constant or "tension superficielle" had not been satisfactory. He mentioned a paper by Georg Hermann Quincke (1834-1924), then professor of physics at Würzburg (8). Quincke had attempted to refute the results given by Lippmann in 1873. Quincke attributed the changes in superficial tension to the presence of impurities and concluded that capillary phenomena could not provide a measure of electrical effects. Lippmann's comment on this paper was that it showed the state of affairs when he began his work.

Both experiment and theory were used by Lippmann to establish his two "laws:"
I. The capillary constant extended at the surface of separation of mercury and sulfuric acid is a function of the electrical difference that is at this surface. II. When, by mechanical means, a liquid surface is deformed, the electrical difference at this surface varies in a sense such that the "tension superficielle" developed by virtue of the first law opposes continuation of the movement.

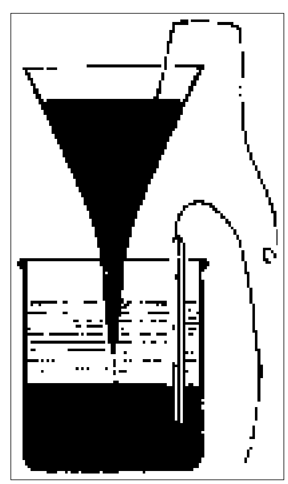


Figure 3. Electrocapillary source of current; Ref. 5, p 512

The assembly shown in Fig. 3 provided "une experience curieuse," based on the above principles. Drops of mercury fall from the very narrow tip of the funnel through dilute H_2SO_4 and into a pool of mercury. Wires a and b connect the masses of mercury to a galvanometer, which at once indicates that electricity is flowing. This flow continues indefinitely, provided that mercury from the growing pool is restored to the funnel. When a drop of mercury forms and grows, the electrical difference at its surface increases and the mercury in the funnel becomes negative with respect to the mercury in the pool. The electrical effect is enhanced when the drop of mercury reaches the pool.

Fig. 4 shows Lippmann's high-sensitivity capillary electrometer. The tip of the vertical tube A is drawn out to a very fine capillary and bent as shown. The tip dips into dilute H_2SO_4 contained in vessel B, at the bottom of which is a pool of mercury. The height of the column of mercury in A is such that the liquid interface in the capillary can be satisfactorily viewed through a microscope with an eyepiece scale. Wires a and b connect the two masses of mercury to the source of the emf to be measured, or rather to be compared with that of another source. For example, it was common practice to use the zinc-copper Daniell cell as a standard and to assign unit value to its emf. Lippmann provided several examples of such comparisons, such as of the emfs of the Daniell and of the Leclanché cell.

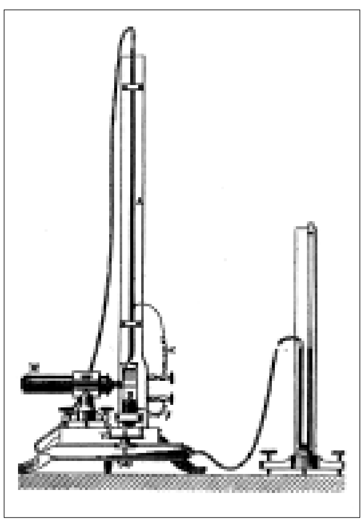


Figure 4. Lippmann capillary electrometer; Ref 5, p 532

Users of the electrometer soon became aware of the importance of providing a fresh mercury surface before the next observation; otherwise the response might have been irregular. With electrometers of the Lippmann type, pressure may be temporarily increased, so that a drop of mercury is expelled from the capillary.

In a paper of 1880, Lippmann mentioned claims concerning the sensitivity of the capillary electrometer that he had found in the literature (9). One claim indicated measurement to 1/10000th that of a Daniell cell, i.e., to about 0.1 mV, another to 1/30000th.

Almost immediately after the appearance of Lippmann's preliminary note (4), a "capillary galvanoscope" made by Werner Siemens (1816-1892) was reported (10). This device, intended for testing rather than for measuring, was obviously less sensitive than the Lippmann instrument. A 0.5-mm diameter horizontal capillary that curves slightly upwards joins two vertical mercury-containing tubes. A small drop of dilute H_2SO_4 is situated at the middle—the highest point—of the capillary. The application of an emf moves the drop to an extent indicated by an attached scale. However, the convex form of the capillary hinders extensive displacement. On breaking the circuit and short-circuiting, the drop returns to the mid position.

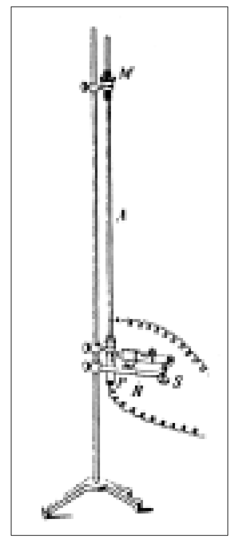


Figure 5. Ostwald's version of the Lippmann electrometer; Ref. 5, p 404

Applications

It is doubtful whether anyone appreciated the utility and simplicity of the capillary electrometer more than Wilhelm Ostwald (1853-1932). He developed the form shown in Fig. 5 while still at the Riga Polytechnicum (11). No new principles were involved, but the device was easy to construct and to use. An externally threaded brass tube is cemented to glass tube A, which contains a column of mercury. A collar M, which is screwed onto the brass tube, rests upon a small stand ring. Rotation of M then raises or lowers tube A. The capillary, which is drawn out from thermometer tubing, is cemented into the lower end of A. A second small stand ring restricts the lateral movement of this end, and a rubber stopper supports tube P. This contains dilute H_2SO_4, into which the capillary dips, and also a pool of mercury. The microscope is aligned by adjustment of screws on the platform, and electrical connections are made through sealed-in platinum wires.

Following Ostwald's move to the University of Leipzig in 1887, his students and associates made much use of the capillary electrometer, either of the original

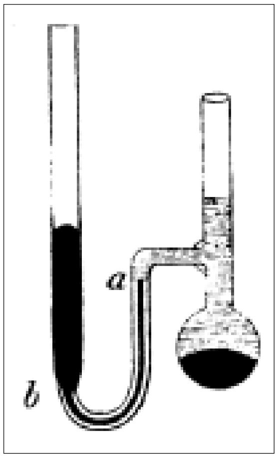


Figure 6. Compact capillary electrometer; Ref. 11, p 555.

form or of later ones. Ostwald may have suggested the more compact form shown in Fig. 6. This form was used in a potentiometric study of mercury (12) and also by other workers in Ostwald's laboratory. The application of the electrometer to acid-base titrimetry was the subject of an extensive investigation (13). Fig. 7, based on a sketch given by Max Le Blanc (1865-1943) in his paper on amalgams (14), shows a sloping-capillary high sensitivity form of electrometer. Le Blanc and also Anton Robert Behrend (1856-1926), who used this type of instrument in a study of potentiometric titration (15), attribute the device to Ostwald..

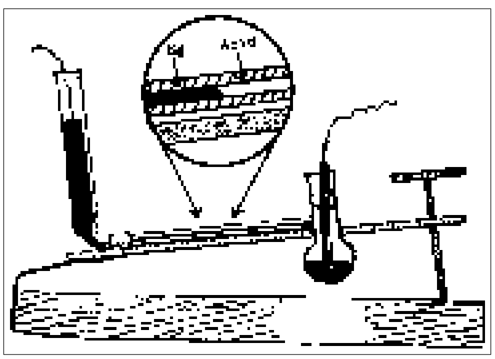


Figure 7. High-sensitivity sloping-capillary electrometer; Redrawn from Ref. 13

Robert Luther, one of Ostwald's assistants who became sub-director of physical chemistry in 1901, devised the totally sealed form of electrometer shown in Fig. 8 (16). Obviously this is easily portable and can be stored in any position. Transfer of liquid through cross tube b permits the adjustment of the position of the liquid interface in capillary c.

Historical Perspective

Practical electronics began with the invention of the vacuum triode in 1917. By then, any reference to the capillary electrometer might be expected to be one of mere mention. A brief scan of the first four decennial indices of *Chemical Abstracts* shows that this was not so. For example, the value of the capillary electrometer in numerous titrimetric analyses was pointed out in 1919 (17). Then a new form of this device was described in 1942 (18). This was mid-year of a war, so that easily-damaged galvanometers could not be repaired quickly. The device shown in Fig. 9 was intended to be a user-safe substitute for a galvanometer. A common cause of trouble with capillary electrometers in general had been the precipitation of Hg_2SO_4 within the capillary. In experiments with a Luther-type electrometer, it was found that this kind of trouble did not occur if the usual dilute H_2SO_4 was replaced by 20% $HClO_4$. This improvement was incorporated in the new device shown. An enlargement K is joined at A to capillary C. If an excessive emf is applied, electrolysis takes place in K, rather than in C. Thus the mercury-acid interface in C is so little disturbed that, after brief short-circuiting, it returns almost exactly to the initial position. Yoke I_1 is merely a support, while I_2 is a wide capillary that ensures the equalization of pressures in the vertical limbs.

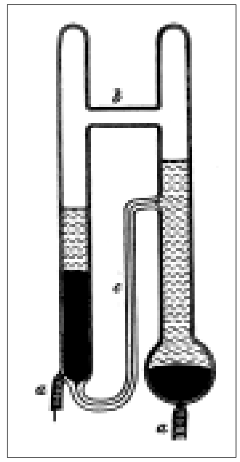


Figure 8. Luther capillary electrometer; Ref. 15, p 426

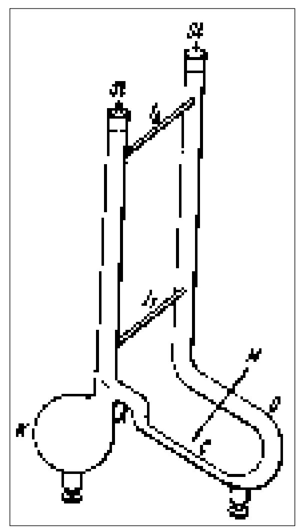


Figure 9. Uhl electrometer; Ref. 17, p 326

Lippmann did not abandon his interest in electrocapillarity after he had published the work that led to the development of the electrometer. However, his research spread to other topics, such as to the determination of electrical units and to the exact measurement of time. He had been thinking about the possibility of photography in color for some years before he published a note on this topic in 1889 (19). Thereafter, the bulk of Lippmann's publications was concerned with this form of photography. In 1891 he demonstrated a method for producing permanent color photographs. He was awarded the 1908 Nobel Prize for Physics "for his method of reproducing colors photographically based on the principle of interference." In his Nobel Lecture, he demonstrated that colors were indeed produced by interference in a nonpigmented emulsion. He admitted that the exposure, one minute in sunlight, was too slow for portraiture. With modern dyestuffs-based color photography, the necessary exposure is of course only a fraction of a second.

Lippmann died aboard ship on July 12, 1921, while returning from a visit to Canada; but by no means did interest in the development and use of the capillary electrometer die with him.

REFERENCES AND NOTES

1. E. Hoppe, *Geschichte der Elektrizität*, Barth, Leipzig, 1884, 393-396.
2. A. Schuster, "Gabriel Lippmann, 1845-1921," *Proc. R. Soc London*, **1922**, *101A*, i-iii.
3. J. R. Partington, *A History of Chemistry* Macmillan, London, 1964, Vol. 4, 707.
4. G. Lippmann, "Relation entre les phénomènes électriques et capillaries," *C . R.. Séances Acad. Sci., Ser. C* **1873**, *76*, 1407-1408.
5. W. Thomson, "Report on electrometers and electrostatic measurements," *Report of the British Association for the Advancement of Science*, Dundee, **1867**, 489-512.
6. G. Lippmann, "Beziehungen zwischen den capillaren und elektrischen Erscheinungen," *Ann. Phys.*, **1873**, *149*, 546-561.
7. G. Lippmann, "Relations entre les phénomènes électriques et capillaries," *Ann. Chim. Phys.*, **1875**, *5*, 494-549.
8. G. Quincke, "Ueber elektrische Ströme bei ungleichzeitigem Eintauchen zweier Quecksilber-Elektroden in verschiedene Flüssigkeiten," *Ann.Phys.*, **1874**, *153*, 161-205.
9. G. Lippmann, "Bermerkungen über einige neuere electrocapillaire Versuche," *Ann. Phys.*, **1880**, *11*, 316-324.
10. Anon., "Ueber ein von Hrn. Siemens construirtes Capillar-Galvanoscope," *Ann. Phys.*, **1874**, *151*, 639-641.
11. W. Ostwald, "Das Kompensations-Elektrometer," *Z. Phys. Chem. (Leipzig)*, **1887**, *1*, 403-407.
12. H. Brandenburg, "Abnorme elektromotrische Kräfte des Quecksilbers," *Z. Phys. Chem. (Leipzig)*, **1893**, *1I*, 552-576.
13. W. Böttger, "Die Anwendung des Elektrometers als Indikator beim Tittrieren von Säuren und Basen," *Z. Phys. Chem. (Leipzig)*, **1897**, *24*, 253-301.
14. M. Le Blanc, "Ein Beitrag zur Kenntnis der Amalgame," *Z. Phys. Chem. (Leipzig)*, **1890**, *5*, 467-480.
15. R. Behrend, "Elektrometrische Analyse," *Z. Phys. Chem. (Leipzig)*, **1893**, *11*, 466-491.
16. C. Drucker, Ed., *Ostwald-Luther Hand- und Hilfbuch zur Ausfuhrung physikochemischer Messungen*, Academische Verlagsgesellschaft, Leipzig, 4th ed., 1925, 426.
17. J. Pinkhof, "Das Elektrometer als Titrierindicator," *Pharm. Weekblad*, **1919,** *56*, 218-234.
18. F. A. Uhl, "Ein neues, dauernd betriebssicheres Capillelektrometer als Galvanometerersatz," *Z. Anal. Chem.*, **1942**, *124*, 324-327.
19. G. Lippmann, "Sur l'obtention de photographies en values justes par l'emploi der verres colorés," *C .R. Séances Acad. Sci., Ser.C*, **1889**, *108*, 871-873.

ABOUT THE AUTHOR

Dr. John T Stock, recipient of the Dexter Award in the History of Chemistry, is Professor Emeritus of Chemistry, University of Connecticut, Storrs, CT 06269-3060.

KHEMYE: CHEMICAL LITERATURE IN YIDDISH

Stephen M. Cohen

English, along with a few other languages (*e.g.*, German, French, Russian, Japanese, and Chinese), is the primary vehicle for transmitting chemical knowledge and discoveries today. Yet languages are not neutral carriers of information; the very act of choosing a language for instruction implies an educational, ethnic, and perhaps even a social class among the users. Given the impetus and the means, any language is capable of explaining complex scientific phenomena. This article aims to provide a modest history of the chemical literature, confined to the 20th century, written in a lesser-known language, Yiddish, now considered endangered.

Yiddish's origins, dating back about a millennium, are unclear. Linguists are still arguing over the details of where and how it started, but most would agree that medieval southern and west-central Germany are likely candidates. The language of most Central and Eastern European Jews, it flourished in the 19th and early 20th centuries, until Nazi massacres, Stalin's purges, and immigrant assimilationism markedly reduced the present number of native speakers. Yiddish is considered a "fusion language," for it has fused together medieval German dialects, some Slavic vocabulary and grammar, a Hebrew-Aramaic component, and even some words of Romance origins.

The cultural and social milieu that engendered scientific writing in Yiddish began as the 18th-century Western-European Enlightenment gradually filtered eastward into the Russian Empire during the 19th century. At that time, Jews in the Russian Empire were serfs, peasants, or poorly educated city dwellers. They were, as a rule, not educated in secular or scientific studies, nor even in the local official or semi-official languages of Russian or Polish. Universities held to strict quotas for the number of Jews allowed entry per year. Those few Jews who had traveled to Western Europe brought back with them the wonders of 19th-century natural and social science and accompanying technology (1). Simultaneously, the terrible anti-Semitism of the Russian pogroms from 1881–1897, and France's infamous Dreyfus affair (1894) increased the nationalistic desires of many Jews, culminating in Theodore Herzl's Zionist Congress in 1897. Emigration to the United States rose dramatically, intensifying after further Russian pogroms from 1903–1905. Most immigrants stayed in the largest American cities, such as New York, Boston, Philadelphia, and Baltimore. Their sweatshop working conditions in America and extreme poverty in Russia were dreadful (2). Many joined the socialist and communist movements in order to overthrow the oppressive Russian Imperial and capitalist social orders (3). To improve their lot, many Jews set up educational organizations to instruct their brethren in these new technological wonders and to bring them out of their allegedly superstitious, ignorant environment. Thus, interest among the better-educated Eastern-European Jews developed in socialism, communism, and secular studies—which included science in general, and chemistry in particular.

From this cultural milieu came Yiddish science books. Though the Eastern-European Jews fortunate enough to gain entry to local universities through the strict quota systems learned enough Russian, French, and German to communicate with their fellow scientists, some decided to write for laymen ill-versed in these languages. Many of the authors listed in this article have fallen into obscurity, and details about their lives are unknown. The first known textbook in Yiddish devoted entirely to chemistry was published in 1920 by a chemist (and a member of the American Chemical Society), philanthropist, and devotee of American history, Sol Feinstone (1888–1980) (4). Entitled *Khemye: Tsu Lezen un tsu Lernen* ("Chemistry: To Read and to Learn") (5), the goal of the work was "to give the Yiddish reader a short, popular treatment on the great chemical science, about which, until now, so little has been written in Yiddish literature." (See Fig. 1.) Basic concepts, from atoms to molecules, reactions, inorganic and organic chemistry, and even nomenclature, were covered in the 272 pages contained in *Khemye* (6). Figure 2 shows Feinstone's sketch of the Hall Process for reducing aluminum. A table of petroleum distillation products appears in Fig. 3. Discussion of reactions involving chlorine is shown in Fig. 4.

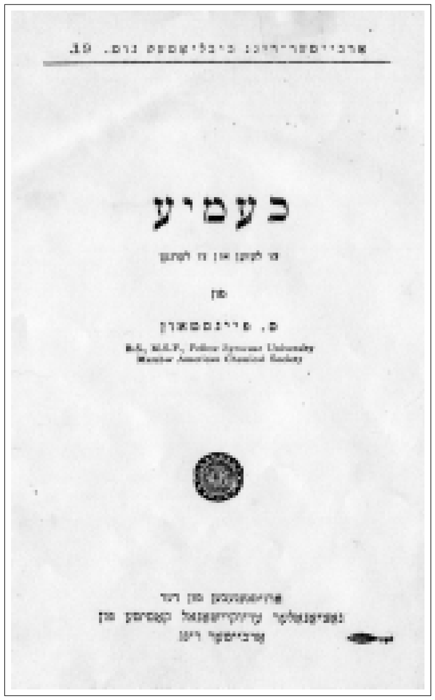
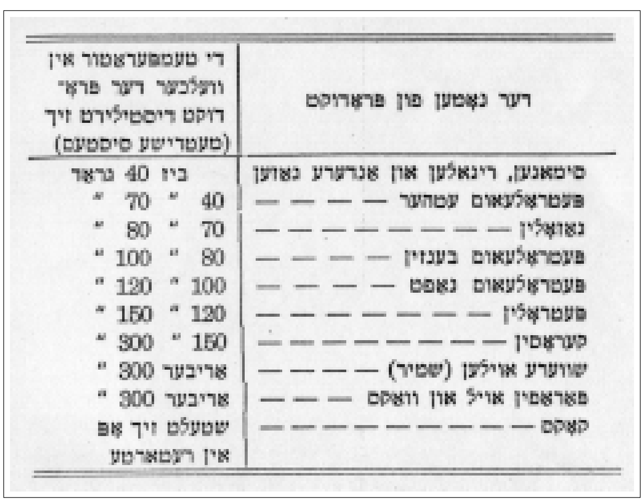


Figure 1. Title page from Sol Feinstone's *Khemye*.

Khemye was actually a part of the "*Arbeter-Ring Bibliotek*" (Workmen's Circle Library) series, published by the Workmen's Circle, a secular Jewish labor organization devoted to helping immigrant Jewish laborers fit into modern American society. Another of this series was Dr. Abraham Caspe's *Geologye* ("Geology") (7). Though not a book about chemistry *per se*, the discussion of minerals, interspersed throughout the geological topics, included their chemical composition (8).

General encyclopedias and volumes devoted to self-education in Yiddish also began to appear, such as the *Folks-Universitet* ("People's University") series (9). Major topics—presented as self-contained chapters—included in the three volumes were chemistry, physics, biology, anthropology, and history.

There were teacher's editions about science in Yiddish as well. Golomb's *Praktishe Arbet af Natur-Limed*



די טעמפּעראַטור אין וועלכער דער פּראָדוקט דיסטילירט זיך (מעטרישע סיסטעם)	דער נאָמען פון פּראָדוקט
ביז 40 גראַד	סימאָגען, ריגאָלען און אַנדערע גאַזען
" 70 " 40	פּעטראָלעאום עטהער — — — — —
" 80 " 70	נאַפטאַלין — — — — — — — — —
" 100 " 80	פּעטראָלעאום בענזין — — — — —
" 120 " 100	פּעטראָלעאום נאַפטאַ — — — —
" 150 " 120	פּעטראָלין — — — — — — — —
" 300 " 150	קעראָסין — — — — — — — — —
" 300 איבער	שווערע אוילען (שמיר) — — — — —
" 300 איבער	פּאַראַפין אויל און וואַקס — — — —
שטעלט זיך אָפּ אין רעטאָרטע	קאָקס — — — — — — — — — — —

Figure 2. Schematic of the Hall process in Feinstone's *Khemye*. The caption under the diagram reads, "The electronic flow goes into the cryolite and molten aluminum oxide (ק) through the strips (ש), splits the oxide apart and leaves the box through the wall (וו) and through (—). The molten aluminum pours out through the opening (ע)."

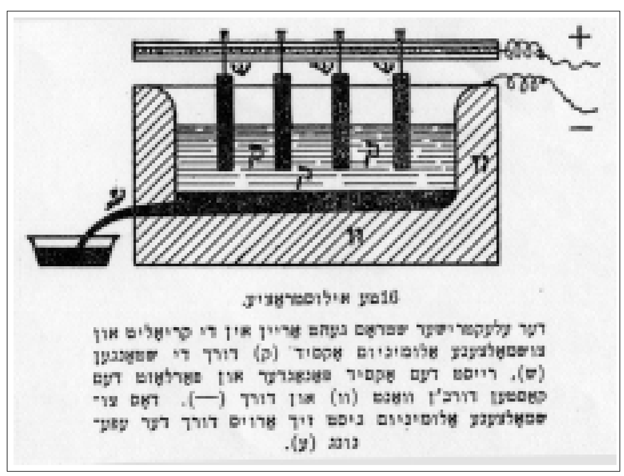
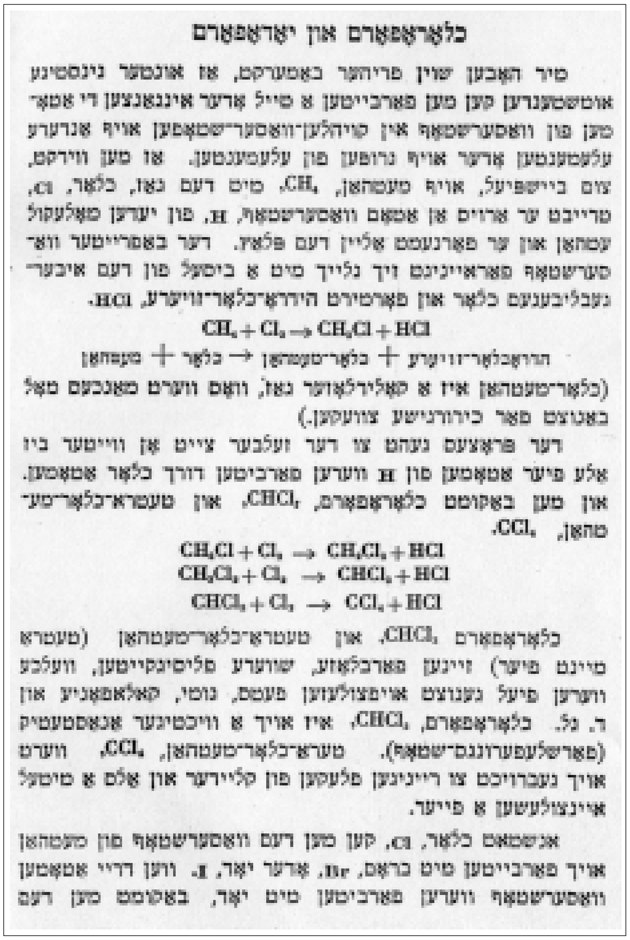


Figure 3. Table of distillation products from crude oil in Feinstone's *Khemye*. The right column is "The name of the product," and the left column shows "The temperature at which the product is distilled off (metric system)."


כלאָראָפאָרם און יאָדאָפאָרם

מיר האָבן שוין פריער באַמערקט, אַז אונטער גינסטיגע אומשטענדן קען מען פאַרבייטן אַ טייל אָדער אינגאַנצן די אַטאָמען פון וואַסערשטאָף אין קויגלען־וואַסערשטאָפן אויף אַנדערע עלעמענטן אָדער אויף גרופּן פון עלעמענטן. אַז מען ווירקט, צום ביישפּיל, אויף מעטאַן, CH_4, מיט דעם גאַז, כלאָר, Cl, טרייבט ער אַרויס אַן אַטאָם וואַסערשטאָף, H, פון יעדן מאָלעקול מעטאַן און ער פאַרנעמט אַליין דעם פּלאַץ. דער באַפרייטער וואַסערשטאָף פאַראייניקט זיך גלייך מיט אַ ביסל פון דעם איבערגעבליבענעם כלאָר און פאָרמירט הידראָ־כלאָר־זויערע, HCl.

$$CH_4 + Cl_2 \rightarrow CH_3Cl + HCl$$

מעטאַן + כלאָר → כלאָר־מעטאַן + הידראָכלאָר־זויערע

(כלאָר־מעטאַן איז אַ קאָלירלאָזער גאַז, וואָס ווערט מאַנכעס מאָל באַנוצט פאַר כירורגישע צוועקן.)

דער פּראָצעס גייט צו דער זעלבער צייט אָן ווייטער ביז אַלע פיר אַטאָמען פון H ווערן פאַרביטן דורך כלאָר אַטאָמען. און מען באַקומט כלאָראָפאָרם, $CHCl_3$, און טעטראַ־כלאָר־מעטאַן, CCl_4.

$$CH_3Cl + Cl_2 \rightarrow CH_2Cl_2 + HCl$$
$$CH_2Cl_2 + Cl_2 \rightarrow CHCl_3 + HCl$$
$$CHCl_3 + Cl_2 \rightarrow CCl_4 + HCl$$

כלאָראָפאָרם, $CHCl_3$, און טעטראַ־כלאָר־מעטאַן (טעטראַ מיינט פיר) זיינען פאַרבלאָזע, שווערע פליסיקייטן, וועלכע ווערן פיל גענוצט אויפצולייזן פעטס, גומי, קאַלאָפאָניע און ד. גל. כלאָראָפאָרם, $CHCl_3$, איז אויך אַ וויכטיקער אַנאַסטעטיק (פאַרשלעפערונגס־שטאָף). טעטראַ־כלאָר־מעטאַן, CCl_4, ווערט אויך געברויכט צו רייניקן פלעקן פון קליידער און אַלס אַ מיטל איינצולעשן אַ פייער.

אַנשטאָט כלאָר, Cl, קען מען דעם וואַסערשטאָף פון מעטאַן אויך פאַרבייטן מיט בראָם, Br, אָדער יאָד, I. ווען דריי אַטאָמען וואַסערשטאָף ווערן פאַרביטן מיט יאָד, באַקומט מען דעם

Figure 4. Reactions with chlorine, from Feinstone's *Khemye*. The heading of this section is "*khloroform un yodoform*" (chloroform and iodoform).

("Practical Work in Natural Studies") provided a guide for science teachers for laboratory experiments on the metric system, melting and boiling points of various materials, and solutions, as well as a list of necessary materials for the school laboratory (10). Fig. 5 shows Golomb's examples of a cooling curve for water. Golomb's book was a product of the Eastern-European Jewish secular schools, which were quite active between the World Wars.

Perhaps the high point in Yiddish chemistry literature and the most thorough and serious treatment of chemistry in Yiddish was Shmuel Brokhes's 305-page *Khemye: Loytn Laboratorishn Metod* ("Chemistry: According to the Laboratory Method"), written on a high-school senior level (Fig. 6) (11). This textbook was not one written for nonscientists, nor a translation of textbooks from western Europe, but an original Yiddish chemistry textbook, published in Belarus, in the former

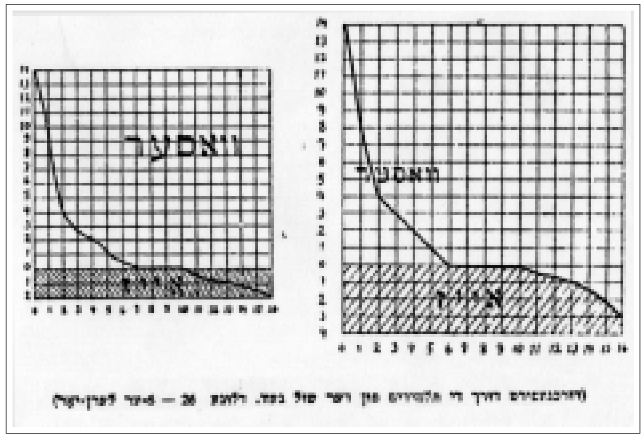


Figure 5. Sixth-year students' cooling-curves of water from *Praktishe Arbet*. The handwritten words on the upper part of the graphs are *vaser* ("water"), and at the bottom, *ayz* ("ice").

Soviet Union. Brokhes explained in the preface to the teacher that (11):

> This book is constructed according to the laboratory method, and has a technical bent. Everywhere the material is thoroughly taught, offered from practical works that the student himself has to do in the school laboratory.

Brokhes's treatment of the chemistry itself was very descriptive, practical, and nontheoretical, with several paragraphs of explanation followed by a laboratory experiment, repeated a number of times per chapter. Chapters were organized more-or-less according to the important commercial elements and compounds (See Table 1). Fig. 7 is a chart sketching the importance of sulfuric acid. The then new quantum theory was not even mentioned. A small amount of radiochemistry was discussed in a section entitled "radioactivity," beginning with Becquerel's discovery of radioactivity in 1896 and the Curies' isolation of radium. Brokhes mentioned

Figure 6. Title page from Brokhes's *Khemye*. Interspersed with the formulae for sulfuric acid, zinc sulfate, and calcium sulfate is the word *khemye* ("*chemistry*")

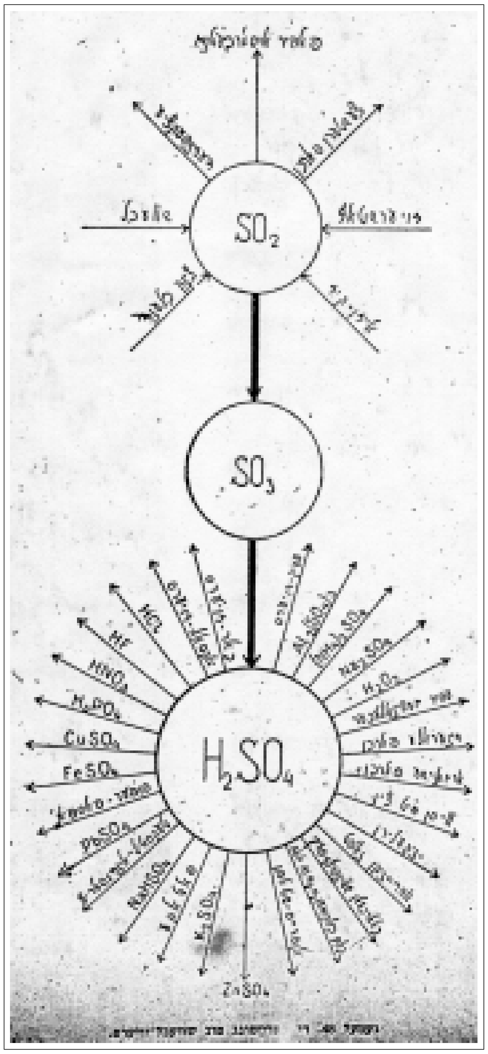


Figure 7. Chart depicting the importance of sulfuric acid, from Brokhes's *Khemye*. Arrows leaving SO_2 point to paper fabrication, dyes, and disinfectants. Arrows entering SO_2 are labeled sulfur, oxygen, iron pyrite, and zincblende.

α, β, and γ-rays, describing α-rays as having the weight of a He atom, β-rays as similar to cathode rays (without further explanation), and γ-rays as similar to light. He noted that U, Ac, and Th are radioactive, with the final decay product being Pb, and that their half-lives are insensitive to pressure, heat, and other common energy sources. Brokhes discussed isotopes of lead (^{206}Pb and ^{208}Pb) and chlorine (^{35}Cl, ^{37}Cl, and he suggests possibly ^{39}Cl) and their relationship to atomic weight. No explanation of radioactivity or isotopes based on the then incomplete knowledge of atomic structure was provided. Perhaps the subject was too controversial, and he intended the astute reader to draw his own conclusions.

Brokhes's textbook was a product of the Soviet educational system, designed to neutralize the peasants' superstitious interests in religion and to instill a materialistic sensibility. Publishing in Yiddish was a logical choice, for most Jews at the time were still poorly educated but fluent in this language. The Jews in early Soviet society were officially considered one of many "peoples" comprising the Soviet Union, so Yiddish became a government accepted medium of secular instruction for Jews for a while. Numerous technical dictionaries and textbooks in Yiddish were published in the 1930s.

The worries of World War I weighed heavily on the Soviet people, for Brokhes also explains that (11):

> [B]ecause of the great significance of chemical warfare methods in a wartime, it is necessary to give the students an idea of the most important explosives and poison gases. In this book a separate chapter (XVIII) is given about them.

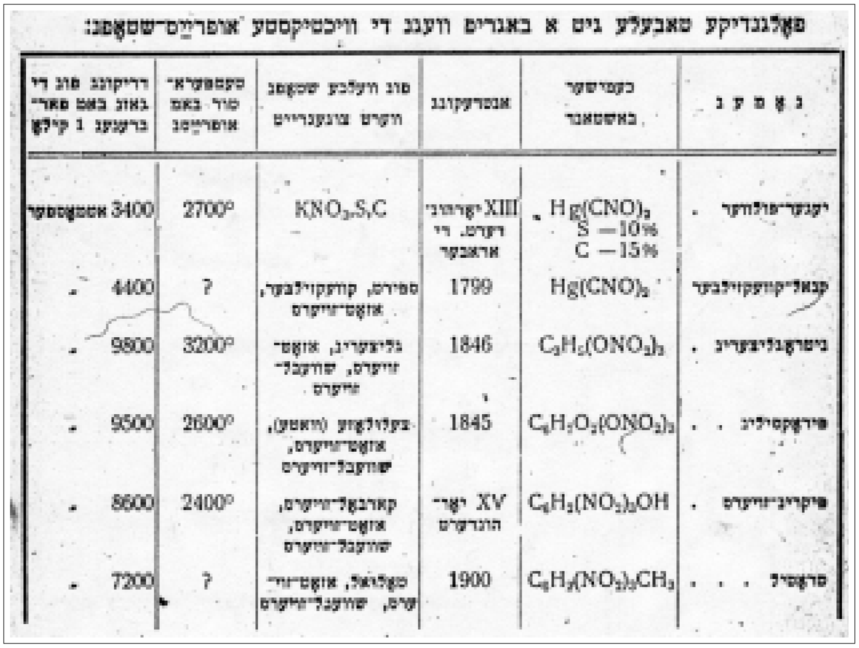
Indeed, an entire propagandistic chapter devoted to chemical warfare is provided, with the following introduction (11):

> What does each citizen of the Soviet Union have to remember? For our entire existence, the capitalists have not stopped preparing for war against us. Many facts reveal that during recent times, in concert with our victories on the socialist front from one side and with the economic crisis in the bourgeois countries from the other side, the relation of the capitalist world to us gets ever more aggravated, therefore the revolutionary ascent of the proletariat and colonial peoples of the world has to take care. It is enough to remind one of the wild hate that is driven against us by the spiritual people of all beliefs under the leadership of the Roman Pope; among them the rabbis are counted separately.

Once Brokhes got past this obligatory socialist drivel, he plowed into the chemistry of explosives. Fig. 8 is a table of various explosive compounds in Yiddish.

Chemical propaganda was by no means an isolated incident to Brokhes's textbook. A contemporaneous book, *Khemisher Kamf* ("Chemical Struggle") (12), gave detailed explanations for nonscientists of how to prepare for the predicted chemical war against socialist peoples. From this book, an illustration of how soldiers suited up for gas attacks would appear is shown in Fig. 9.

Since World War II, unfortunately, interest in Yiddish as a medium for education has severely declined, because most native speakers were killed or have died



פאלגנדיקע טאבעלע גיט א באגריף וועגן די וויכטיקסטע אופרייס־שטאפן:

דריקונג פון די גאזן באם פארברענען 1 קילא	טעמפעראטור באם אופרייסן	פון וועלכע שטאפן ווערט צוגעגרייט	אנטדעקונג	כעמישער באשטאנד	נאמען
אטמאספער 3400	2700°	KNO_3,S,C	XIII יארהונדערט, די אראבער	$Hg(CNO)_2$ S — 10% C — 15%	[illegible]־פולווער
. 4400	?	ספירט, קוועקזילבער, אזאט־זויערס	1799	$Hg(CNO)_2$	קנאל־קוועקזילבער
. 9800	3200°	גליצערין, אזאט־זויערס, שוועבל־זויערס	1846	$C_3H_5(ONO_2)_3$	ניטראגליצערין
. 9500	2600°	צעלולאזע (וואטע), אזאט־זויערס, שוועבל־זויערס	1845	$C_6H_7O_2(ONO_2)_3$	פיראקסילין
. 8600	2400°	קארבאל־זויערס, אזאט־זויערס, שוועבל־זויערס	XV יארהונדערט	$C_6H_2(NO_2)_3OH$	פיקרין־זויערס
. 7200	?	טאלואל, אזאט־זויערס, שוועבל־זויערס	1900	$C_6H_2(NO_2)_3CH_3$	טראטיל

Figure 8. Table of explosives, from Brokhes's *Khemye*. Column headings from right to left: "Name;" "Chemical composition;" "Discovery;" "From which materials is it prepared;" "Temperature upon explosion;" "Pressure of the gases upon burning 1 kilo."

without passing on the language to their children. Literature, including scientific works, continues to appear occasionally, however. The 1960s saw Sol Podolefsky's book *Di Velt fun Visnshaft, un Visnshaftlekhe Teoryes* ("The World of Science, and Scientific Theories") (13) appear, with numerous short essays on various scientific topics for the layman. Most of these dealt with astronomy, biology, geology, archeology, and cosmogony; but several touched on chemistry, including a not-so-accurate discussion on the structure of the atom (Fig. 10). Other essays explained about fire, hardness of water, diamonds, the discovery of phosphorus, and properties of oxygen and copper.

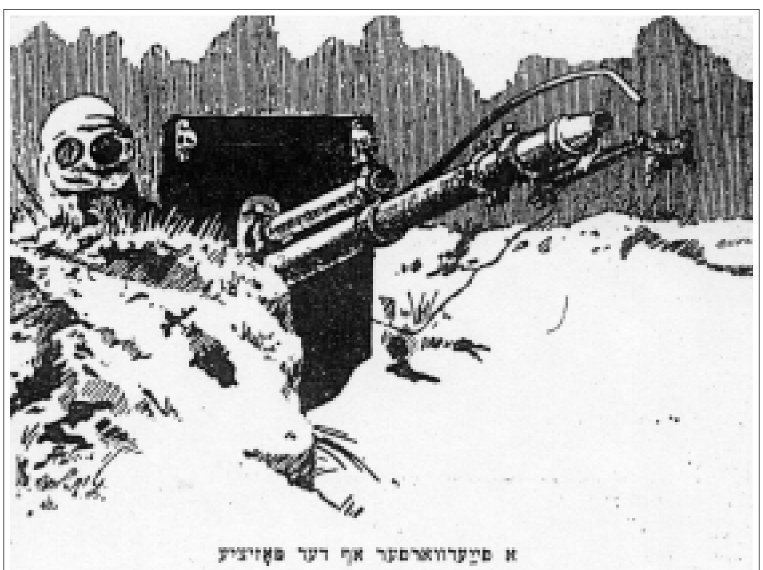


Figure 9. "A flamethrower in position," from *Khemisher Kamf.*

Recent examples of Yiddish works on chemical subjects are primarily news items. The weekly Yiddish newspaper, *Forverts* ("Forward") publishes general news on various world-wide topics, including scientific discoveries, especially when there are political implications (*e.g.*, the energy crisis, greenhouse effect, genetic engineering,). Examples of recent chemistry-related headlines appearing in the *Forverts* are shown in Fig. 11.

Last sources for Yiddish chemical terminology are, of course, various reference books. Uriel Weinreich's *Modern English-Yiddish Yiddish-English Dictionary* (14), considered the modern standard, includes a small number of relatively common terms, useful in general conversation, such as *brom* ("bromine") and *molekul* ("molecule"). Mordkhe Schaechter's recent dictionary *Trogn, Hobn un Friike*

Kinder-Yorn ("Pregnancy, Childbirth and Early Childhood") (15) includes a few chemically related terms such as *de-en-a* ("DNA"), *haytl* ("membrane"), and *tsinkzayers* ("zinc oxide"). Within the League for Yiddish's quarterly journal *Afn Shvel* ("On the Threshold") is Schaechter's language column, "Laytish Mame-Loshn" ("Proper Mother-Tongue"), in which he explains correct Yiddish grammar and vocabulary. Occasionally he includes chemistry terminology (16), such as *zayers* (acid), *zayersdikayt* (acidity), and *zayers-regn* (acid rain). On the other hand, Nahum Stutchkoff's massive *Der Oytser fun der Yidisher Shprakh* ("The Thesaurus of the Yiddish Language") (17), arranged very much like an English *Roget's Thesaurus*, includes literally hundreds of terms related to chemistry, ranging from the obsolete *doberiners triadn* ("Dobereiner's Triads") to *daltons gezets vegn teylvayzn druk* ("Dalton's Law of Partial Pressures"), as well as verbs like *filtrirn* ("to filter"), or *sublimirn* ("to sublimate"), and elemental names such as *silitsyum* ("silicon") and *bor* ("boron"). (See Fig. 12.)

דער געבוי פון דעם אטאם

דער קאפ פון א שפילקע קעפל, וואס וועגט אן אכט־טויזנטל פון אן
אונץ און די גרויס פון א טויזנטל פון א קוביק אינטש, באשטייט פון א צאל
פון 10 ביז 20 נולן אטאמען. דאס איז אזא גוואלדיקע צאל וואס מיר האבן
שוין דערויף קיין נאמען נים. יעדער אטאם האט א צענטראלן נוקלעאום, אדער
ניוטראן און א פראטאן ארום וועלכן עס דרייען זיך עלעקטראנען אין פארי־
שידענע ארביטס. די גרויס פון ניוטראן איז א סך קלענער ווי דער אטאם
אליין. אבער די ארביטס פון די עלעקטראנען זיינען גרויס לויט דער גרויס
פון אטאם.

Figure 10. The beginning of an essay on "the structure of the atom" from *Di Velt fun Visnshaft*: "The head of a pin, which weighs an eight-thousandth of an ounce and a thousandth of a cubic inch in size, consists of an amount of 10 to the 20 zeroes atoms. This is such a huge amount that we don't yet have a name for it. Each atom has a central nucleus, or neutron and a proton [sic] around which revolve electrons in different orbits. The size of a neutron is much smaller than the atom itself. But the orbits of the electrons are big according to the size of the atom."

With the help of several scientifically oriented native Yiddish speakers, I have created a modern chemistry dictionary with about 3,000 words and phrases from *absoluter alkohol* ("absolute alcohol") to *tishboyres* ("fractional number") (18). In addition, to promote the use of chemistry-related terms in the home with children, I co-authored a web-based article in the internet magazine *Der Bavebter Yid* ("The Interconnected Jew") in Yiddish on generating electricity with two dissimilar metal strips inserted into a lemon (19).

For a language that has been sequestered most of its existence in the Eastern European areas where Jews were forced to live, a legitimate question is whence did Yiddish's chemical vocabulary arise? No definitive study has been done on the etymology of scientific terminology—let alone general word origins—in Yiddish, so some speculation is offered herein. Besides coinages native to Yiddish, probably the primary source is German, for several reasons. Modern *Hochdeutsch* is quite similar to Yiddish, and it was in widespread use in the 19th century as the medium of chemical research and instruction. Furthermore, in the late 19th through early 20th centuries, a style of Yiddish usage (*daytshmerish*) suggesting higher education and social status incorporating much German general vocabulary was in vogue (20). Feinstone's *Khemye* made extensive use of such "*daytshmerisms*" ("Germanicisms"), to the point of being stilted for today's Yiddish speakers. Examples of words from German are names of certain elements, such as *vasershtof* (*Wasserstoff*, hydrogen), *zoyershtof* (*Sauerstoff*, oxygen), and *shtikshtof* (*Stickstoff*, nitrogen). The common name of carbon dioxide in Yiddish is *koyln-zayers* (*cf.* German *Kohlensäure*, carbonic acid).

די נאבעל־פרעמיעס פאר וויסנשאפט אינעם יאר 2000

ברען־מאטעריאלן פארן 21סטן יארהונדערט

נייע זון־צעלע קאן ווערן דער וויכטיקסטער ענערגיע־קוואל

Figure 11. Recent chemistry-related headlines from the *Forverts* newspaper: (top) "The Nobel Prizes for science in the year 2000," Oct. 13, 2000; (middle) "Fuels for the 21st century," July 7, 2000; (bottom) "New solar cell can become the most important energy source," Sept. 20, 1996.

A second source of vocabulary is from Slavic languages (Russian, Polish, Serbian, Czech, etc.), because of the geographic proximity. For "rust", Yiddish uses *zhaver* (*cf.* Russian *rzhavchina*). For "slaked lime," the Yiddish term is *vapne* (Polish *wapno*). "Neutral" is *neytral* in Yiddish (Russian *neytral'niy*), and "flask" is *kolbe* (Polish and Russian *kolba*).

Figure 12. Yiddish thesaurus with topic 218, “Chemistry,” starting on the lower right, with entries “chemistry; general, analytical, theoretical, physical,” continuing on the left column “practical, organic, inorganic, etc. chemistry; biological chemistry, biochemistry; microchemistry, thermochemistry, geochemistry, pharmacochemistry, electrochemistry, photochemistry, etc.; agricultural chemistry, zoochemistry, alchemy…”

The Hebrew-Aramaic component of scientific Yiddish is found in *mashmoesdikeyt* (probability [as in statistics]), *umseyder* (disorder, a combination of Germanic *um*-, “dis-,” and *seyder*, “order”), and *makhshir* (instrument, tool, apparatus). The influence of French, the language of culture that the Russian nobility admired, appears occasionally in Yiddish. Two examples are the second name of nitrogen, *azot* (Russian *azot*, probably from French *azote*), and *nivo* (“level” [height, *e.g.*, of a liquid in a container or energy of electrons], from French *niveau*).

Finally, internationalisms, generally from Latin or Greek, common to many languages beginning with the Renaissance and accelerating during the Industrial Revolution, are found throughout Yiddish, including most element names, organic and inorganic naming conventions, mathematical terms, units for the metric system, subatomic particles, and so forth.

As we have seen, Yiddish has nearly all types of chemical literature: textbooks for the serious student and layman, teacher's guides, newsworthy articles, and reference sources of terminology (21). One important and notable absence is journals in which to report original chemical research.

Considering the general utility of chemical literature in Yiddish, one might examine the population of the present speakers and readers of the language. Estimates of the number of Yiddish speakers in the 1930s ranged from 10.7–11.9 million. By contrast, Birnbaum estimated the number of Yiddish speakers had dropped to 5–6 million by the late 1970s (22). It would not be unreasonable, therefore, to guess, solely on the basis of natural attrition and general lack of transmission of the language to the youth, that the number of Yiddish speakers has been cut in half yet again. According to the 1990 United States Census, Yiddish was the 16th largest language spoken in the USA, with 213,000 speakers over age 5 (23). Though full statistical data have not been released yet, the 2000 United States Census counted nearly 179,000 people over the age of five who spoke Yiddish (24), out of estimates ranging from 5.2–6.1 million Jews in the USA (25, 26), or only about 3 % of the Jewish population. At present, most Jews of Eastern-European descent communicate among themselves in the language where they live (*e.g.*, English, Hebrew, Russian, French,), rather than Yiddish; therefore the need for discussion of matters concerning chemistry is perceived as correspondingly low. Offsetting slightly the general decline in the use of Yiddish, the religiously strict and often isolated Hasidim still use Yiddish as their everyday means of communication; they tend to have larger families. The Census showed that 36,000 children ages 5–17 used Yiddish in the State of New York alone in 2000 (27).

Today, in the European Community, the Yiddish language is under the jurisdiction of the "European Bureau of Lesser Used Languages" (28) and has gained official status as a recognized Jewish language (along with Ladino) in Israel. The Jewish Autonomous Region of Birobidzhan, established in 1934, in remote far eastern Russia near Manchuria, has used Yiddish as an official language since 1935, though no more than roughly 5,000 Jews remain there out of over 200,000 inhabitants. For the past several decades, a growing number of colleges and universities around the world have offered classes in Yiddish, catering to the small but increasing interest by the young for this thousand-year-old language with much to offer, even in science. Perhaps we will see an increase in the use of Yiddish, as well as with other minority languages, to transmit the excitement of chemistry, a universal topic, in the future. Table 2 gives the first 18 elements from the Periodic Table in Yiddish.

ACKNOWLEDGMENTS

I wish to thank the late Ezra Stone (the son of Sol Feinstone) and his son Josef Stone, along with the David Library of the American Revolution, in Washington Crossing, PA, for the opportunity to examine the Library's copies of Sol Feinstone's *Khemye*. **NOTE:** Yiddish transcription is according to the YIVO (Institute for Jewish Research, New York) standard.

REFERENCES AND FOOTNOTES

1. See, for example, many pertinent jokes about the newfangled technology in the hands of the uninitiated in I. Olsvanger, *Röyte Pomerantsn, or, How to Laugh in Yiddish*, Schocken Books, New York, 1965.
2. I. Howe, *World of Our Fathers*, Harcourt Brace Jovanovich, New York, 1976.
3. A. Eban, *My People: The Story of the Jews*, Behrman House, New York, 1968.
4. H. Harris, "Sol the Eccentric," *The Pennsylvania Gazette*, **1993**, *92*(2), 35–39.
5. S. Faynstoun, *Khemye: Tsu Lezen un tsu Lernen*, Arbeter-Ring Bibliotek No. 19, The Workmen's Circle, New York, 1920.
6. On p 6 of *Khemye*, Feinstone reproduces a title page of a book by Dr. Sheneman, *Khimiah, oder Sheydikunst ("Chemistry, or The Art of Separation")*, Freischule, Berlin, 1795, part 1. I have not been able to locate a copy of this work.
7. A. Kaspe, *Geologye*, Arbeter-Ring Bibliotek No. 11, The Workmen's Circle, New York, 1918, Vol. 1 and 2.
8. A slightly later book, a Yiddish translation of Wilhelm Ostwald's introduction to chemistry, *Szkola Chemij*, Warsaw, 1924, exists, but I have not yet been able to examine it.
9. *Folks-Universitet*, Farlag Yehudiya, Warsaw, ca. 1925, Vol. I–III (but probably originally from ca. 1906, based on the treatment of history, which ends with the Russo-Japanese War).
10. A. Golomb, *Praktishe Arbet af Natur-Limed*, Kultur-Lige, Warsaw, 1922, ershte heft (farn lerer).

11. Sh. Brokhes, *Khemye: Loytn Laboratorishn Metod*, 2nd ed., Tsentraler Felker-Farlag Fun F.S.S.R., Minsk, Belarus, 1931.
12. A. Siromatnikov, *Khemisher Kamf*, Tsentraler Felker-Farlag fun F.S.S.R., Minsk, Belarus, 1931.
13. S. Podolefsky, *Di Velt fun Visnshaft, un Visnshaftelkhe Teoryes*, 2nd ed., Rachman Publishing Company, New York, 1971.
14. U. Weinreich, *Modern English-Yiddish Yiddish-English Dictionary*, Schocken Books, New York, 1977.
15. M. Schaechter, *Trogn, Hobn, un Friike Kinder-Yorn*, League for Yiddish, New York, 1991.
16. M. Schaechter, "Laytish Mame-Loshn," *Afn Shvel*, **Oct.–Dec. 2001**, *324*, 11.
17. N. Stutchkoff, *Der Oytser fun der Yidisher Shprakh*, YIVO, New York, 1991 (originally published in 1950).
18. The Yiddish alphabet goes from ? (alef) to ? (sov). No Yiddish words start with ?, so the previous letter, ? (tov), is the last entry in a Yiddish dictionary.
19. Sh.-Kh. Koyen and L. Botvinik, "Elektrokhemye: a Bayshpil fun Visnshaft-loshn af Yidish," *Der Bavebter Yid*, **June 1, 1997**, *1*(1), found at http://cs.engr.uky.edu/~raphael/bavebter/numer.1.1/shloyme.elektro.html.
20. Though out of vogue now among secular, educated Yiddish speakers, *daytshmerish* is still found among the conservative language habits of the religiously strict Hasidim.
21. An extensive—but not complete—list of chemistry books in Yiddish can be found at http://www.ibiblio.org/yiddish/Bibliog/term-yi.htm.
22. S. A. Birnbaum, *Yiddish: A Survey and a Grammar*, University of Toronto Press, Toronto, 1979, 42.
23. http://www.census.gov/population/socdemo/language/table5.txt.
24. See http://factfinder.census.gov/, Quick Table QT-P16.
25. D. Singer, Ed., *American Jewish Year Book 2001*, American Jewish Committee, New York, 2001, Vol. 101.
26. World Jewish Congress (WJC), Lerner Publications Company, 1998.
27. E. Vaysman, "Vifl Yidish-Reders in Amerike?" *Forverts*, **Sept. 27, 2002**, *106* (31,412), 15.
28. See the European Bureau of Lesser Used Languages's web site, http://www.eblul.org/wow/.

ABOUT THE AUTHOR

Stephen M. Cohen is a Technical Writer for Jobin Yvon, Inc. He obtained his Ph.D. in physical chemistry from Rice University in 1992. His research has included surface chemistry, nonlinear spectroscopy, spectroscopy of free jets, and corrosion of aluminum. While an undergraduate chemistry major at the University of Pennsylvania, he took two years of Yiddish Language. He now raises his children in a Yiddish-speaking household, and collects scientific materials in the Yiddish language. Besides research-oriented publications and instruction manuals for scientific instrumentation, he has published works on the history of chemistry, chemical education, and a range of nonscientific topics. He can be reached at 9 Cherry Brook Lane, East Windsor, NJ 08520; drstevecohen@earthlink.net.

AN EARLY HISTORY OF CHEMISTRY AT TEXAS TECH UNIVERSITY, 1925-1970*

Henry J. Shine, Texas Tech University

Introduction

Chemistry got its start at Texas Tech with the founding of the institution. This account is not a complete history of the department, but covers its evolution from its earliest days up to the 1970s. Thus, this is the history of a department's evolution, a narrative that is sometimes anecdotal and sometimes personal. The City of Lubbock dates from the 1890s, and Texas Tech, itself, was founded only in 1923. Consequently, the history I write about is relatively new, but it is about the beginning of higher education on the frontier, in West Texas, a vast part of Texas quite different from the then better developed and better known regions of east and central Texas. I have chosen to write about the first 45 years, the period 1925-1970, when, except for the last part of 1969, the department was under the leadership of just three men, Drs. William Thornton Read, Robert Cabannis Goodwin, and Joe Dennis. Having been on the faculty of Texas Tech for about two thirds of its life (and of mine), I find it easier to write about the early days of Texas Tech than about its later and current days, for I am still part of those, and they are not "history" for me. At the same time, in writing this history, I am conscious of the personal stake that I have in it. In writing about the beginnings of my own department, it is as if I am writing about my own beginnings in academia. The Chemistry Department was one of the first departments in a newly created institution for higher learning in West Texas, Texas Technological College. In my mind, then, it is not possible to separate the beginning of the Chemistry Department from the beginning of its host, the college, itself. I shall start, then, with the founding of Texas Tech, how it got its name, and why it was located in Lubbock.

The Founding of Texas Tech

In writing this portion of this history, I have drawn heavily from the 1939 thesis for the M. A. degree in History (1) by Clifford L. Gibbs, a student at Texas Tech, and a history by Homer D. Wade (2). Texas Tech was born in 1923 after a long gestation period and a number of aborted conceptions. Movement to establish an Agriculture and Mechanical College in West Texas began in the 1890s. At that time, West Texas was viewed as stretching from the panhandle, past Lubbock, down through Snyder and Sweetwater and farther out to the country of the Pecos River. Institutions of higher learning already existed in the eastern part of the state, prominent among which were the University of Texas at Austin (UT) and Texas A and M (TAMU) at Bryan (now at College Station). There were also already a small teachers college at Canyon in the panhandle (3), two private, religiously affiliated colleges at Abilene, and a third on the way (4). But, the people and politicians of West Texas wanted a state college of their own. The movement toward an Agriculture and Mechanical College was based in part on the claim that the land and agriculture of West were different from those of East Texas, so that there was a need for a place of learning for the people of the West. Early attempts to introduce bills into the State legislature, in 1896 and 1911 failed to get beyond the committee stage. More serious movement began gathering momentum in the years 1915-1916. At that time, the most likely way of securing passage of a bill in the state legislature was to have its content included in the platform of a State Democratic Convention, and this was achieved for the West Texas A and M College in the Democratic Convention in Houston in August, 1916, when James E. Ferguson became the Party's nominee and eventually successful candidate for Governor of Texas. The likelihood of getting legislative approval for a college in West Texas was now almost certain, and, in fact, such a bill passed both House and Senate in 1917. Nothing now seemed to stand in the way of securing for West Texas its own college of higher learning. But something strange happened on the way to this forum. Although the college was approved, its location had yet to be chosen, and the competition for location was fierce. A locating committee was established, consisting of Gov. Ferguson (chair) and four other members, namely, Frank O Fuller, William P. Hobby, Fred W. Davis, and Walter F. Doughty. The committee travelled to candidate towns to assess their capabilities of supporting a college, and then met on June 28, 1917, in Gov. Ferguson's office to cast secret ballots. These, collected by the Governor, were declared by him to have chosen Abilene as the location of the new college. Abilene celebrated with great joy. Subsequently, however, other committee members canvassing among themselves, realized that at least three of them had not voted for Abilene. The Governor, it seems, had pulled a fast one over them. Much outcry and a move to undo the choice followed. Gov. Ferguson was in jeopardy with the state legislature for other reasons, too. He was impeached and deposed by the legislature, and among the charges was that of misuse of location ballots. The legislature that had created the bill to establish West Texas A and M College now threw it out in a special session. The establishment of a college in West Texas was now back to square one.

Paul Whitfield Horn, first president of Texas Technological College. Archives, Southwest Collection TTU

Out of the attempts to promote the establishment of a college in West Texas had grown, in 1918, an organization called the West Texas A and M Association, comprised of representatives from various towns in the west. This organization continued with the efforts. It failed to get a plank for its objective in the platform of the 1920 Democratic Convention, at which Pat Neff was nominated as the candidate for Governor, but the Association carried its campaign directly to the state legislature and was able to get a bill passed in 1921. It is interesting to note that the college would be for white students, coeducational, and that its courses would be prescribed by the Board of Directors of Texas A and M College. The latter limitation illustrates the influence that East Texas had in trying to obstruct the founding of a college in the west. That bill, however, was vetoed by Governor Neff, partly on the grounds that it had not been included in the Democratic platform.

Once more, the West Texas group began its efforts at the Democratic Convention in 1922. This time it was successful, and a plank for having a college as a branch of Texas A and M was included, even though that connection was not to the liking of legislators from West Texas. Again, something strange was to occur that remodeled the character of the proposed college. At the same time that bills had been introduced into the Senate (by Bledsoe of Lubbock) and the House (by Baldwin of Slaton), representatives Carpenter and Irwin from Dallas introduced a bill to create a College of Technology and Textile Engineering in Texas. Their motivation was a belief that these subjects were not yet well enough represented in the state's colleges and universities. This bill gathered so much support that West Texans were afraid that it would derail the bills for a West Texas A and M. A conference of the authors of the various bills was called, at which the Dallas representatives, supportive of the West Texas aim, agreed to the drawing up of a substitute bill combining the interests of both groups. In order to satisfy the interest from Dallas in technology, the name Texas Technological College (TTC, Texas Tech) was proposed by Carpenter for a new college to be located in West Texas. The earlier, proposed West Texas forename was dropped because that was possessed already by West Texas State Teachers College in Canyon. This bill was passed by the legislature and was signed by Gov. Neff on February 10, 1923. Thus was created the college in which the new chemistry department was to begin its life. That is how Texas Tech got its first name, which it retained until 1970 (5). There were no ties to Texas A and M. Again, a location committee was appointed, and from among 37 contending cities, Lubbock was chosen in August, 1923. That choice was surprising, because the early drives for a new college in West Texas had originated in cities such as Amarillo and Sweetwater. A Board of Directors was soon appointed, and just as soon, chose as President of TTC on November 22 Dr. Paul Whitfield Horn, President of Southwestern University in Georgetown.

Paul Whitfield Horn was charged with the formidable task of building a college from scratch. This he set about doing, first from a rented home in Lubbock and later from the president's home, one of the first six buildings to be erected on the campus. As can be seen in his voluminous correspondence (6), Horn appears to have worked single-handedly in supervising the construction of buildings and the hiring of faculty. The overall plan of the campus-to-be was designed by the architect William Ward Watkin, who had designed the plan for Rice Institute (later Rice University) in Houston and was, in 1923, the Head of the Architecture Department at Rice. Watkin's overall plan was eventually abandoned, but the early buildings at Texas Tech show a remarkable resemblance to the early buildings at Rice. The first buildings to be erected were the Administration Building (to house also all liberal arts and science departments), the Home Economics Building, the Textile Engineering Building (to house all engineering departments), a Stock Judging Pavillion, a Dairy Barn, and the president's home. These buildings illustrate the rationale for establishing TTC, namely agriculture of the region, textiles and technology, and subjects of a first-class college. It is interesting, impressive, sometimes amusing and sometimes touching to read Horn's correspondence in his endeavor. No detail escaped his eye. On May 12, 1925, for example, he wrote to Watkin that (6):

> In looking over the stone for our Administration Building today, I observed two errors in spelling. (1) The name Hippocrates, instead of being spelled as here shown is spelled 'Hipocrites.' This of course does serious injustice to the father of medicine. (2) The name Pestalozzi is spelled 'Pespalozzi.' So far as I know, no one ever bore this latter name and certainly the great schoolmaster did not.

Later, on August 31, 1927, when TTC was well on its way and more buildings were under construction, he again wrote to Watkin to propose a compromising solution to the complaint from 'ex-confederate friends' that a placque (*sic*) of Abraham Lincoln had been placed on the Administration Building, but not one of Jefferson Davis.

In the hiring of faculty, Horn both advertised and sought out academic contacts. The number of applications for faculty positions was huge, often from teachers in high schools and from other universities. A member of the Music Department at the University of Texas wrote on April 6, 1925 (6):

> The entire Music Department of the University has been eliminated by reason of the veto of the Governor of Texas and I am in consequence seeking an appointment.

A graduate student at the University of Chicago, whose work for the Ph.D. degree was "practically all completed" and who was then "reviewing for the final examinations," applied for "a position in West Texas Technological College as head of the Chemistry Department (6)." Alas, neither the music nor the chemistry applicant made the grade.

William Thornton Read, copied from La Ventana yearbook-1926, TTC

Creation of the Department

William Thornton Read, Head, 1925-1930

Insofar as positions in the Chemistry Department were concerned, Horn began with William Thornton Read, an assistant professor in the Department of Chemistry at Yale. The story of his hiring is told by Read in a 1968 letter from Read, then retired and living in Houston, to Clifford B. Jones, then President of Lubbock National Bank and former President of TTC (7). Read had grown up in Texas. He was, in fact, born on a campus, the first baby to be born (in 1886) to a faculty member at Texas A and M College (8), where his father was the first college physician from 1883 to 1891 (7). He was an undergraduate at Austin College and had earned an M. A. in organic chemistry from UT. He had gone on to Yale for his Ph.D. (1921), where he remained as an instructor and assistant professor. His mentor at UT was J. R. Bailey, who suggested to Read that he might be interested in coming back to Texas. Bailey, himself, lobbied Horn on Read's behalf, sending Horn letters of recommendation that Bailey had gathered (9). Read was unsure of his future at Yale, writing that he knew the road to promotion was long and hard and that he was older than most of his colleagues. Read knew of Horn, having met him first as a sixth-grader in Sherman when Horn was visiting his school as the superintendent of schools. Later, when Read was in his junior and senior years at Austin College, he worked as a part-time reporter for the Sherman *Democrat*, covering schools and churches, and would meet Horn for a few minutes every week in Horn's office. They became good friends. Thus, the circumstances were ideal for an offer to join TTC, and this was made by Horn on December 12, 1924 (6). The offer was for a full professorship beginning in September, 1925, at the salary of $3,750 for 9 months, that being the maximum allowed by legislative appropriation. The offer made no mention of a headship. It is not clear when Read accepted the offer and when he was offered the headship. But, it is quite clear from the many letters exchanged with Horn that Read was working on setting up the Department of Chemistry while he was still at Yale. He was corresponding with supply houses and had drawn up a budget for supplies and equipment for the future laboratories. Read, wanting to move to Lubbock in the summer of 1925, asked Horn whether he could supply a salary that would let Read assist Horn with his work. But, money was not available and Read had to stay in New Haven, where he worked on his book *Industrial Chemistry*. He arrived in Lubbock in August, 1925. Two other appointments were made in chemistry. One of these was of a full professorship for William Lamkin Ray, who had an M. A. from UT and Ph.D. from Chicago. This appointment was made by Horn, and it is clear that Ray, then on the faculty of Stephen F. Austin State Teachers College, was brought to Horn's attention by Prof. Schoch of UT (10). Ray was offered a salary of $3,600 for nine months. The second appointee was Freeman Dent Galbraith as associate professor, but no record seems to be available of who made the appointment and at what salary. Ray stayed at TTC until 1933, after which any trace of him has been lost. Galbraith stayed only for one year, having been discouraged from staying longer by Read (11). Galbraith went on to spend the rest of his academic career at Potomac State College in Keyser, West Virginia, where he had a beloved and honored career. He died at the age of 57 on April 18, 1938 (12).

Thus, when TTC opened for business in the fall, 1925, it had a chemistry faculty of three. At that time, Horn had hired a total of 44 persons. They are listed in the 4th Bulletin of TTC, issued for the opening of the College (13). It is interesting to note that the faculty members in this group were either full (24) or associate professors (9). There were no assistant professors. Two adjunct professors and six instructors were appointed, several of the latter being wives of full professors. Neither Horn nor any of the four academic deans (Liberal Arts, Agriculture, Engineering, and Household Economics) had a Ph.D. degree. Among the full and associate professors, seven had Ph.D. degrees and 17 had master's degrees. Two professors (Studhalter in biology and Qualia in Spanish), however, completed their Ph.Ds. later. Thus, the Chemistry Department was well off with two thirds of its faculty holding doctorates. The college was proud of its student enrollment, amounting to 642 men and 272 women. They comprised 730 freshmen and 184 sophomores. The Bulletin notes that this enrollment placed TTC "fifth among 15 state supported colleges in the number of students enrolled for work of collegiate grade." The college in West Texas had arrived. One wonders, though, how few students the trailing 10 institutions may have had.

The task of the three chemists was to devise and teach all of the courses. These are set out in the First Annual Catalog 1925-1926, dated April, 1925, as comprising General Chemistry, Advanced Theoretical Chemistry and Analytical Chemistry, Organic Chemistry, Physical Chemistry, Industrial Chemistry, and Technical Analysis. By January 26, 1926, the First Annual Catalog had changed, however, and the courses in chemistry had been given course numbers, were expanded, and described in detail (Table 1). The course numberings were derived from a three-term year (Fall, Winter, and Spring) that was in effect at TTC in its early days. The chemistry curriculum seems to have been a hefty undertaking for a three-man department. There were no teaching assistants, in the graduate student sense, to help with the teaching loads; but the catalogs of January, 1927, 1928, and 1929 each list by name nine or ten "chemistry assistants." Their duties are now unlikely to be

Table 1. Courses in Chemistry, 1926

Course Name	Course Numbers	Hours per week	
	CHEM-	Lecture	Laboratory
Elementary General Chemistry[a]	141, 142, 143	3	3
Theoretical and Analytical[b]	231, 232, 233	2	3
Advanced Inorganic Chemistry	234, 235, 236	3	-
Analytical Chemistry[c]	237, 238, 239	-	9
Organic Chemistry Short Course[d]	331, 332	2	3
Organic Chemistry Long Course	343, 344, 345	3	3
Industrial Chemistry[e]	336, 337, 338	3	-
Power Plant Chemistry[f]	339	-	9
Technical Analysis[g]	431, 432, 433	-	9
Physical Chemistry	441, 442, 443	3	3
Physiological Chemistry	437, 438	2	3
Mechanical Chemistry[h]	321, 322	-	6[h]
Principles of Chemical Engineering[i]		3	

[a]Divided into two categories for students with or without high school chemistry and required for students in engineering, agriculture, and home economics. [b]Designed for students who could devote a limited amount of time to the study of chemistry. [c]Consisting of qualitative and quantitative analysis. [d]For students in agriculture and home economics. [e]Divided into two groups, one of which was for students majoring in chemistry, and consisting of a study of leading chemical industries from the point of view of chemical engineering operations, the fundamental theories and principles of chemistry involved, and economic and business principles. [f]Required of students in engineering. [g]Divided into thirds, any one of which could be taken, to cover commercial methods of analysis of foods, stock feeds, fertilizer and soil, animal and vegetable oils, petroleum products, water, and fuel. [h]Chemical plant design, six hours of drawing and calculation. [i]Preparation for further courses in chemical engineering in other institutions.

known. They were undergraduates because a number of them (E. W. Camp, Jr., Marion Green, T. M. Binnion, Loy B. Cross, La Thaggar Green, G. Robert Martin, and Andrew Jenkins) went on to receive B. A. degrees in chemistry at TTC. Chemistry assistants ceased being listed in the 1930 catalog; instead, in 1931, we see for the first time the listing of teaching assistants, one of whom (Cecil H. Connell) was the department's first M. A., 1933 (14), and another of whom (Charles C. Galbraith) remained in the department for many years as an instructor without, apparently, making it to the master's level (14, 16).

The courses in industrial chemistry, power plant chemistry, and technical analysis reflected Read's own interests, for during 1916-1918 he worked at UT in the Division of Chemistry of the Bureau of Economic Geology and Technology, which had been established under E. P. Schoch in 1915, and from which Read published a bulletin on boiler waters (17). Read, furthermore, was writing a book on industrial chemistry; but that, not surprisingly in view of his workload, was not finished until after he had left TTC (18). The courses with chemical engineering content were a forerunner of the formal introduction of chemical engineering into the department in 1933, when the Department of Chemistry and Chemical Engineering was created. Until 1933, chemical engineering was an option within the Department of Mechanical Engineering (14). Apart from their teaching loads, the three chemistry hires carried college-wide committee assignments: Read on those for registration, student help, publicity, religious life among students, and course of study for liberal arts; Galbraith on student help; Ray on boarding houses (19). Read's assignment to the committee on religious life reflected, too, Horn's view of him. Read, like Horn, was a devoted member of the Presbyterian Church, a connection that Horn brought up in their early correspondence (20); and while at TTC Read was instrumental in setting up a branch of the YMCA (21).

In its first years, the Department of Chemistry was housed in the basement of the Administration Building.

The periodic table, Chemistry Building North wing, TTU, designed by former professor of physical chemistry, William M. Craig, 1926.

We can get a glimpse of the pioneering way of life there from the 1927 and 1928 catalogs, which say (19):

> Two laboratories are devoted to elementary inorganic chemistry and are completely equipped with desks, lockers, gas, water, current, hoods and all the apparatus and chemicals necessary for the course. A smaller laboratory has been provided for advanced courses. The Department of Chemistry also has a stock and preparation room, a storage room and a cellar outside the main building for certain chemicals.

The overall plan for continuing campus construction in 1927 was to add a wing to the Administration Building and to construct two academic buildings, Chemistry/Science and Engineering. But, with a shortage of legislative money, only parts of the plan could be carried out. A conflict arose about the choices. On the one hand, the president and board preferred to complete the Administration Building. On the other hand, Read, Dean W. J. Miller of Engineering, and Prof. E. F. George, Head of Physics, supported by one member of the board, H. T. Kimbro, pushed for the academic buildings. The choice of academic buildings prevailed and the Chemistry/Science Building was erected in the center of the campus, just west of the Administration Building (7). It was occupied on January 1, 1929, and the January 1929 catalog now tells us (with evident pride) that (19):

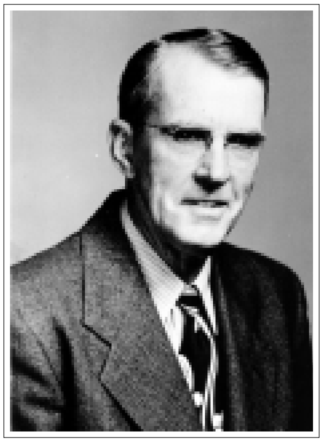
> The Chemistry Building is 240 feet long and 60 feet wide, with one wing extending back 100 feet. There are two stories, a full basement, and at the east end a low tower. Although designed primarily as a Chemistry Building, it houses for the present the Departments of Biology, Geology, Physics and Chemistry.

Read marveled that Horn bore him no grudge nor exacted any retribution for having had to give up his plan for a fine looking, completed Administration Building. Read had designed the Chemistry/Science Building with removable partitions and with all utility services in place, so that when the other science departments would move out, anticipated to be in two stages, each of two years, it would be relatively easy to convert the building into full use for chemistry. That move did not occur, however, until 1951, 22 years later. In the meantime, chemistry occupied the east end of the basement and first floor, and physics the west ends of those floors. Geology and biology shared the second floor, and geology also had the Tower Room, a beautiful room that later, under the headship of Joe Dennis, was to become a faculty lounge and conference room, in time, named "The Dennis Room."

Read stayed at TTC until 1930, when he was attracted away to serve as Dean of the School of Chemistry at Rutgers University. During his five years at TTC he set the Department of Chemistry on a sound foundation and with little increase in the number or character of the faculty. In 1926 he replaced Galbraith with William Moore Craig, an inorganic and physical chemist who was to spend the rest of his academic life at Texas Tech. Craig had been a student at Harvard, working on the atomic weight of gallium under the famous Nobel Laureate, T. W. Richards. He was an instructor at Rice when Read invited him to join TTC. While at Rice he worked with W.

New Chemistry building, Texas Technological College, 1929

W. Watkin to incorporate alchemical figures of the elements into the newly constructed chemistry building. Craig did the same for Texas Tech's building. Those symbols can be seen in what is now called the north wing of the chemistry building. On the face of that wing can be seen chiseled into the roofline stone work the symbols of the periodic table. All of Craig's classes had to memorize those symbols, and many survivors can still recite the beginning line that sounds like "Heliebibcanoff." The only other appointments to help Read in his work were of Hulda Wilde Marshall and Roxie Clark Read in 1926, and of William Mackey Slagle in 1928. Roxie Clark Read was Read's wife, who had an M. A. degree (1918) in chemistry from UT. Her appointment at TTC was negotiated by Horn in order to avoid the criterion of nepotism that prevented Read himself from hiring his wife (6). Horn arranged for the appointment to be made directly by the Board of Directors. One wonders, though, how, in 1926, Ruth B. Studhalter remained an instructor in biology while her husband, Richard A. Studhalter, was listed as Head of Biology; that appointment has yet to be researched. Here, then, is the story of the first five years of Texas Tech's Chemistry Department. Much had been accomplished on what was once cotton fields, some farmed, in fact, by Slagle himself, just west of the town. The next 20 years, embracing the Depression and Second World War saw little real growth. The numbers of faculty and students increased, surely, but the character of the department remained essentially that of a teaching one.



William M. Slagle, ca. 1949. Faculty file, TTC

Robert Cabaniss Goodwin, Head, 1930-1950

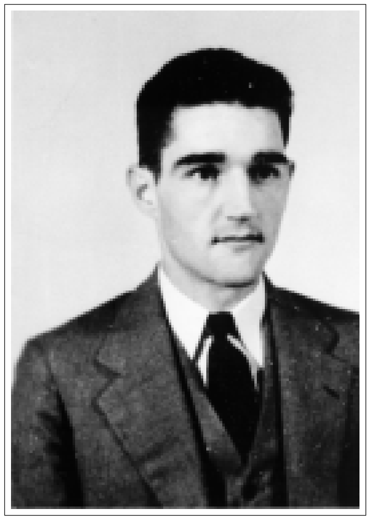
In order to replace Read as Head, Horn turned again to J. R. Bailey at UT (7), who recommended Robert Cabaniss Goodwin, then an assistant professor at the University of Florida, who had done research for his master's degree with Bailey. Like Read, Goodwin was from Texas. He was born in Brownwood in 1898, obtained a B. A. degree in English and history from Howard Payne College (1917), M. A. in organic chemistry from UT (1923), and Ph.D. from Harvard (1928), where he studied with E. P. Kohler. Goodwin's task was not an easy one in the cramped quarters of the Chemistry/Science Building and with sparse funding. The task was made no easier when chemical engineering was added to the department in 1933. Furthermore, Goodwin took on a succession of other administrative positions in addition to keeping the headship of chemistry. Thus, he was Dean of the Graduate School (1938-1945) and Dean of the College of Arts and Sciences from 1945 until 1950, when, under changing college policy he had to choose one administrative post or another. Goodwin chose to be Dean of the College of Arts and Sciences (1950-1959) and went on to become Acting President of Texas Tech (1959-1960) and President (1960-1966). While he was both head and dean, the administrative work of the department was shared with an assistant head, either Craig or Oberg. Even while in the deanships, Goodwin continued to teach the main undergraduate organic chemistry course. He returned to Gainesville, FL, in 1967 and died there in 1993, age 95.

Robert C. Goodwin, presidential inauguration photograph, 1960. Archives, Southwest Collection.

Faculty were hired by Goodwin to take care of the increasing burden in teaching. Few but Joe Dennis (1938) had much inclination or, really, opportunity to do serious research. The climate in the department can be understood from letters that Goodwin wrote to or about applicants for posts. For example, on June 17, 1940, he wrote to Prof. G. L. Clark at the University of Illinois (22):

> It may be that we shall have an opening on our staff next fall. The position will be that of an instructor with a salary of $1,800.00 for the nine-month term. The position will probably be permanent. The possibility for rapid advancement in rank and salary is slight. Opportunity for summer teaching will practically be nonexistent. On the other hand we do all we can to encourage research by the faculty. Our facilities are quite limited but are gradually improving. If complicated or specialized apparatus is not involved, we can usually provide the materials needed. Our attitude toward our instructors is that of aiding them in all ways to se-

> cure better positions—at other places if not with us.
> If you think that Mr. Rowan, about whom you wrote us earlier, would be interested in such a position, kindly have him furnish us with his credentials. Please have him include a picture and complete personal data such as race, religion, personal habits, marriage, etc.



Robert Rowan, Jr., ca. 1940. faculty file, TTC

It is notable that the position offered to Rowan, who was finishing the Ph.D. at Illinois, was that of an instructor. A change seems to have occurred at TTC since its very first permanent faculty were hired in 1925, all of whom, Ph.D. or not, were appointed as either full or associate professors. As can be seen from the letters of Goodwin and Horn, a large drop in salaries occurred, too. The TTC catalogs a few years later show that appointments in all areas were heavily at the instructor level. In the Department of Chemistry and Chemical Engineering, this applied not only to Rowan but also to Oberg (1936) and Rolf (1937), the first "senior" appointees since 1933. Later, Ph.D.s were again appointed at higher ranks, for example, Jones (1948) as associate professor and Watson (1948), Detman (1949), and Tinsley (1949) as assistant professors. Rowan did join TTC, possibly because he, too, was a Texan, who had been born in Waco and took his B. S. degree at West Texas State College (1934). But he stayed at TTC just one year, leaving, seemingly wisely, to go into industry. Goodwin wrote to Dean J. M. Gordon on June 20, 1941 that he had (22):

> …just received a telegram from Dr. Robert Rowan. He is getting married and felt it would be necessary for him to accept an industrial position where he will receive a considerably higher salary.

Eventually, in 1962, Rowan returned to academia at New Mexico State University for a long career in analytical chemistry. This example gives an idea of the situation at Texas Tech in Goodwin's years. Even Joe Dennis was hired, not so much for his research potential, but because he had the experience in biological chemistry which Goodwin wanted to establish in the department (23). It turned out, however, that Dennis would become the spearhead of research in the future.

Many of the faculty appointed in the period 1925-1950 had been educated in Texas and had some connection with UT, as is shown in Table 2. Perhaps this stemmed from a feeling of security in the department by hiring from a known locality or perhaps it was the easiest way of bringing people to a rural and academically unknown part of Texas. Some of these appointments lasted a lifetime (Craig, Marshall, Slagle, Goodwin, Dennis) and others were for brief periods. During Goodwin's headship, 28 faculty appointments were made. They and their terms of service are listed in Table 3. Seventeen of the 28 did not have doctorates and were hired, no doubt, only to help with the burgeoning post-war load of undergraduate teaching. Many were involved with laboratory supervision (24). Most of the latter type short-term appointees (Melton, Bruton, Baker, Cohea, Menaul, M. L. Bryant, Hardey, Plemmons, Work, Brock, Dodge, Crow, Hufstedler) could not be traced after their departure from TTC. Others of that period left and continued their careers elsewhere. Valerie Schneider is said to have become wealthy in local real estate and retired. Neil Berst went into industry. Charles Galbraith went into the Civil Service. Dysart Holcomb, who was, in fact, Dean of Engineering and held his faculty appointment as a chemical engineer in the department, became President of Texas Western College and later went into upper administration in industry. Fred Rolf joined the U. S. Air Force. Sam Tinsley joined Union Carbide, from which he retired in 1986 as Director of Corporate Technology. James Watson went to Northeastern Louisiana State College and later to Southeastern Louisiana State College. Gus Oberg, Oscar Southall, and Margret Stuart each died some years after retiring, Oberg and Stuart after life-long service to the department.

Frederick W. Rolf, ca. 1937. Faculty file, TTC

Joe Dennis, Head, 1950-1969

The career of Joe Dennis was particularly significant for Texas Tech. Born in Sherman, Texas, in 1911, he

Table 2. Texas Tech's Early Faculty Connections with the University of Texas

Name	Years at Tech	UT Connection
William T. Read	1925-1930	M.A., 1915
William L. Ray	1925-1933	B.A., 1918; M.A.,1920
Freeman D. Galbraith	1925-1926	B.A., 1922; M.A., 1923
William M. Craig	1926-1958	M.A., 1916
Hulda W. Marshall	1926-1947	B.A., 1909
Roxie C. Read	1926-1930	M.A., 1918
William M. Slagle	1928-1960	M.A., 1928
Robert C. Goodwin	1930-1966	M.A., 1923
Valerie Schneider	1933-1944	B.S.,1926; M.S., 1928
	1927-1930	Chem. Eng.
Joe Dennis	1938-1976	M.A., 1933
Joseph W. Melton	1941-1944	M.A., 1941
James A. Watson	1948-1951	B.A., 1940

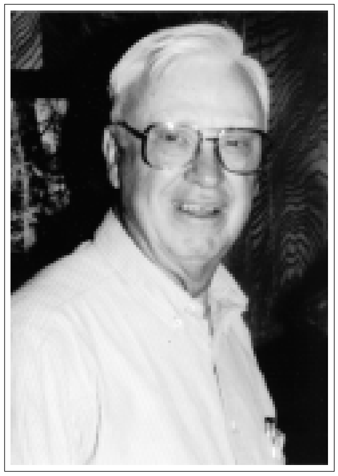
received the B. A. degree from Austin College and the M. A. and Ph.D. degrees from UT. Research for the Ph.D. was carried out at Texas Tech, itself, because Dennis joined the faculty in 1938, invited there by Goodwin, while Dennis was an instructor in the UT medical branch at Galveston. Indicative of Goodwin's willingness to help young appointees to do research, as can be read in his letter to Clark in 1940 (22), he set up a laboratory for Dennis in the attic of the Chemistry/Science Building. Certainly, there was no space anywhere else. Dennis kept that laboratory for a number of years afterward, long after he himself became head. His research was carried out during the long semesters under the long-distance guidance of Prof. B. M. Hendrix of the UT Medical Branch in Galveston. Coursework was taken in the summers in Austin under the care of Prof. H. R. Henze, and the Ph.D. in biochemistry was awarded by UT in 1942. When Goodwin chose to give up the headship of the Chemistry Department, Dennis succeeded him. Dennis had the ambition of turning the department from essentially undergraduate teaching into one in which research and graduate studies would be emphasized. He wanted to model his approach on that which, in his mind, Roger Adams had used so successfully at the University of Illinois; that is, of bringing in young people capable of developing research and teaching careers (23). He was the first of the science heads at Texas Tech to embark on such an objective. The existence of two other powerful, well-funded universities in Texas (UT and TAMU) as well as a plethora of other universities and colleges did not make Dennis's objective easy. Nor, in fact, did Lubbock's location and the general lack of the spirit for research at Texas Tech help Dennis. Nevertheless, he became dedicated to his objective. When the Departments of Biology, Physics, and Geology moved to the Science Building in 1951, relatively ample space was freed up in the, now, Chemistry Building, although there was little money to do much with it. Dennis created a carpentry shop and a machine shop in the basement and received TTC funds to employ a carpenter (Jesse Truman) to build furniture for the laboratories and newly needed offices. Dennis himself took a large part in the design of the furniture. Later, he employed an incredibly good Jack-of-all-trades, Warner Kendall, as a combined carpenter-machinist-plumber to keep the department's laboratories, shops, and building in repair. Kendall was joined in this by his equally skilled brother-in-law, Jimmy Hall, who succeeded Kendall after his death in 1976. These appointments were made in an era, apparently, when maintenance of the building was not restricted entirely to TTC's central building-maintenance departments. Hall remained with the department until he retired in 1999. Although the entire building was freed up for use by chemistry in 1951, it still proved to be too small for the growing needs in research and student laboratory space, even after a thorough renovation in 1956-7. For some years, in fact, the laboratories for general chemistry were housed in temporary war-surplus buildings that were brought to Lubbock and placed directly south of the chemistry building. In the mid 1960s, Dennis began pushing for an extension to the building, and permission to plan an addition for research was given in 1967. The plans for a small addition drawn up by the chemistry faculty were, it seems, deemed too unimaginative by one of the members of the board of directors, Harold Hinn of Plainview, Texas. It also seems that Dennis was in direct contact with Hinn (23), a practice which was then not in accord with Texas Tech policy. In any event, a magnificent addition was planned, not by the faculty alone, but by the faculty and a firm of experienced architects, Pitts, Phelp, and White, at a projected cost of the then unheard of figure of five million dollars. The "addition" was, in fact, larger (81,000 sq ft) than the original building (58,000 sq ft) (14). It was completed in 1970, about a year after Dennis stepped down from leading the department. His resignation was prompted by the change in titles that occurred in 1968. All heads of departments, who had had until then indefinite terms of office, were re-designated chairs, with terms renewable after review. Dennis's response to that was, "If I couldn't have the authority, I didn't want the responsibility (25)." He had served 17 years as head and two as chairman. In 1969, H. J. Shine was selected as chairman. Dennis retired in 1976, moved to Kerrville, Texas, in 1999, where he died on October 15, 2001. The Chemistry Department owes a great deal to his devotion to its welfare.

Joe Dennis, copied from La Ventana yearbook -1960

The building of an "addition" to the Chemistry Building has a side story that is part of the history now being recorded, although it occurred in the years 1972-1973. Plans for a medical school on TTU's campus were formulated in 1970. Because buildings for the medical school were to be erected long after the school had begun accepting students, the science departments of the school were spread out among the science departments of TTU. The new addition to the chemistry building was a prime choice because there was as yet quite a lot of unused space available. Therefore, the Departments of Biochemistry, Anatomy, and Pharmacology were placed temporarily in the addition. While it was still under construction, it became evident that not enough money would be available to build the splendid facility that had been planned and to furnish it completely, too. The department was given the choice of going without some furniture or cutting down the size of the addition. The department chose the former, arguing that there might always be a chance to get the furniture later. Each new research laboratory, therefore, was only partially furnished. After three of the medical school's science departments were housed in the addition, claims for more space in the addition were made constantly by the biochemists. Consequently, the Chairman of the Chemistry Department, this writer, made the case to TTU that the proper way of providing more space for research in the addition was to require the medical school to complete its furnishing. Because at that time the president of TTU was also president of the medical school, this was done, with a grant from medical school funds. In this way, TTU acquired a fully furnished chemistry building, and Dennis's push for an addition to the chemistry building for the furtherance of research was not only achieved but the addition was also beautifully furnished.

During Dennis's long period of leadership, 38 faculty appointments were made or initiated. These are listed in Table 3. In contrast with the trend in appointments under Goodwin's leadership, most of those of the Dennis era held doctorates. In the early 1950s, Dennis did the hiring without much evidence of input from the existing faculty. In this author's case, he was offered a position by a Texas Tech faculty member who had interviewed him at an American Chemical Society meeting, far from the Lubbock campus. Two of the appointments (Stubbs and Guerrant) were made specifically to serve as coordinators of the freshman chemistry program. Holcomb's and Bradford's appointments were as chemical engineers because they were, successively, Dean of Engineering. McPherson managed the logistics of the freshman laboratories. Most of the other appointments were for researchers who were to fulfill Dennis's hopes for building a research-oriented department. Of the 38 appointees, 13 stayed on to retirement (Adamcik, Anderson, Draper, Guerrant, McPherson, Mills, Renard, Rekers, Shine, Shoppee, Stubbs, Wilde, and Wilson). Some retired to other careers: Adamcik to a law degree from TTU's School of Law, McPherson to introduce vineyards to the region and to start a winery, Mills to become a restaurateur in Hawaii, and Stubbs to become Professor and Dean at the University of Albuquerque. Charles Shoppee, the department's first Welch

Professor, returned to Australia, to continue part-time research as Honorary Professor at LaTrobe University. Among those who left the department, David Carlyle chose high school teaching and farming in his home state; Bertha Delaney returned to nursing at a local hospital; George Estok went to St. Edward's University in Austin; Patricia Fain went to Mallinckrodt in St. Louis; James Fresco to a long career at McGill; Harry Hecht to Los Alamos National Laboratories and later to South Dakota State University; Dick Hendry to Westminster College; William Herndon to become Chairman at the University of Texas at El Paso; Irving Lipschitz to Lowell Institute; Lynn Marcoux to industry; Roy Mitchell became a wine master at a local winery and then joined TTU's College of Agricultural Sciences to teach oenology; Tom O'Brien went on leave and then to industry; Pill-Soon Song became chairman at the University of Nebraska; Don Scott went into industry with Lockheed-Georgia; Richard Thompson to the Bureau of Criteria and Standards; Fred Trusell to Marathon Oil Co.; Randolph Wilhoit went to Highlands University in Las Vegas, NM, and later to the API project at TAMU. Three members of the early faculty were untraceable (Franz, Gryder and Thoma), and two (Marx, Redington) remain in the department.



Clinton M. McPherson, on the occasion of his retirement, September, 1988

My last words about these Dennis-era appointees concern William Wesley Wendlandt, who joined TTC at the same time as the author. "W^3", as he signed himself, was an innovator in the evolving department. He began his productive research life by setting up a thermogravometric balance in his office. Eventually, he designed more sophisticated balances and went on, at the University of Houston, to become the founding editor of *Thermochemica Acta*. At TTC Wendlandt also became the first coordinator of the freshman chemistry program. Until then, almost every member of the faculty taught freshman chemistry, in classes of about 30 students. Each teacher had two or three sections and was responsible individually for course content and examinations. Wendlandt introduced the system of large classes with fewer teachers. The course content was centralized, as were examinations, which were made up by Wendlandt from questions submitted by the several teachers. From this system evolved the concept of appointing a coordinator for general chemistry (Stubbs, Guerrant) and the establishment of a Division of Chemical Education in the Department, overseen by a faculty member of the Division, but whose logistics for several thousand general chemistry students are managed by a staff appointee. Wendlandt moved in 1966 to become Chairman of Chemistry at the University of Houston, a position he held until 1972. He retired in 1991 and died on June 30, 1997.

Research grants made their appearance in the department also during Dennis's early years as head. Perhaps the first grants made by external agencies were from The Robert A. Welch Foundation (now The Welch Foundation) of Houston. These grants came about in a historically interesting way. Early in 1955, Dennis told this writer (25) he had heard about an agency in Houston that was giving grants for research in chemistry, and that, in some way, Henry Eyring, the renowned chemist at the University of Utah, was involved. An enquiry was made to Eyring, whose response (26) named the Welch Foundation, on whose advisory committee he served, and its awards of grants to the three major research institutions then in Texas, UT, TAMU, and the (then) Rice Institute. This information was followed up by Dennis, who later carried a number of research proposals from TTC to Houston. Of these, two were funded, one to Patricia Fain and the other to the author, each for two years. The author's grant, totaling $18,450, enabled him to advertise for and bring to TTC two Ph.D. students (Robert Snell and John Trisler), with research support of $200/month. This was the beginning of the department's life as a research department, and of research support by The Welch Foundation that has continued and expanded until the present day.

Thus during the years 1925-1969, the department was in the hands of three men. Under Read, tremendous progress was made, and with very few faculty and little else. The progress was relative, of course, to having started with nothing. Yet, one wonders: had Read stayed in the department, would it have had a different history? Read went to Rutgers as its dean to establish a research school of chemistry in which task he eventually felt disappointed, having soon encountered both a change in administration and the Great Depression (7). One might think that, had he stayed at TTC, he might have tried to establish a research department there. It is worth conjecture, too, had he stayed would he have been content to house the other science departments in "his" building for 22 years? Goodwin seems to have been cut from a different cloth. It is recognized that the Depression came just when he, too, began his headship of the department, so that he may well have been faced with keeping the department going at best. It is my own assessment that Goodwin was not driven (as was his successor, Dennis) to build a research department. This opinion is based partly on the character of the department during Goodwin's headship, and also on his views on my own early progress in the department, when, as a new president of TTC, he expressed his concern to Joe Dennis that my getting so many research grants, as I then had, might interfere with my teaching (25). Joe Dennis was determined to have a research department. He had to contend with holding a lone objective on a campus more inclined toward teaching. Teaching loads remained high for years. In time they were reduced for researchers; salaries for researchers, each perk solely in the hands of Dennis, were raised. In those early years, too, there was no such thing as start-up funds for research, and there were very few teaching assistantships for graduate students. On the other hand, there were no charges for supplies by the department either; what was available was free for use. I regard Dennis as the founder of our research department. He set the stage for what would come later under the leadership of various chairmen and the participation of faculty who, themselves, were there for careers in research and teaching. The history of the later times awaits its assessment and telling.

Henry J. Shine, United States Rubber Co. research laboratory, Passaic, NJ, 1953

A word about degrees is in order. In Table 3 are listed the numbers of degrees awarded, in increments of years. The numbers show that for the first 25 years of its life the department was mainly in the undergraduate teaching mode. Eleven master's degrees were awarded in that period, as compared with 460 bachelor's degrees in chemistry and chemical engineering. The slow increase in numbers of graduate degrees from 1950, when Dennis became head, can be seen. The dominance of B. A. as compared with B. S. degrees reflects the influence of premedical and other health-science students. Dennis was committed to nurturing students who were interested in medical careers, so much so, that he managed a premedical advisory committee himself for many years and then persuaded Margret Stuart into doing that, some-

Table 3. Numbers of Degrees in Chemistry and Chemical Engineering 1925-1970[1,2]

Period	B.A.	B.S.	B.S. CE[3]	M.A.	M.S.	Ph.D.
1927-1930	19					
1931-1935	46	1	15			
1936-1940	45	13	41	1	4	
1941-1945	27	16	53		1	
1946-1950	54	41	89	1	4	
1951-1955	46	29	82		9	2
1956-1960	53	30	83		7	3
1961-1965	67	51			16	8
1966-1970	80	70			22	13

[1] Taken from the compilation of Joe Dennis (14). [2] The first B.A. was awarded in 1927; the first B.S. in 1932; the first M.A. in 1933 and the last in 1949; the first M.S. in1936, the first Ph.D. in 1953. [3] Supplied to Joe Dennis by Prof. A. G. Oberg. Data for 1936 were missing. Chemical Engineering separated from Chemistry in 1959.

thing she continued until retirement. Dennis was adamant about keeping that committee in the Department of Chemistry. The premedical advisory committee later became the Health Sciences Careers Office with separately paid staff but supervised by one of the faculty—and still housed in the department. The gradual increase in numbers of B. S. degrees can also be seen in the Table. It is astonishing to see how many B. S. in Chem. E. degrees were awarded, inasmuch as very few of the faculty were teaching chemical engineering, primarily Professors Oberg and Renard after Valerie Schneider left in 1948. It is small wonder that little research in chemical engineering was carried out.

Table 4. Faculty of the Department 1925-1970[1]

Name	Highest Degree and Place		Area[2]	Began	Ended
Read, W. T.	Ph.D.	Yale	O	1925	1930
Ray, W. L.	Ph.D.	Chicago	O	1925	1933
Galbraith, F. D.	M.A.	Texas		1925	1926
Craig, W. M.	Ph.D.	Harvard	P	1926	1958
Marshall, H. W.	M.A.	TTC		1926	1947
Read, R, C.	M.A.	Texas		1926	1930
Slagle, W. M.	M.A.	Texas		1928	1960
Goodwin, R. C.	Ph.D.	Harvard	O	1930	1966
Schneider, V.	Sc.D.	MIT	CE	1933	1948
Galbraith, C. C.	B.S.	Trinity		1934	1948
Oberg, A. G.	Ph.D.	Michigan	P/CE	1936	1959
Rolf, W.F.	Ph.D.	Iowa	A	1937	1942
Dennis, J.	Ph.D.	Texas	B	1938	1976
Rowan, R., Jr	Ph.D.	Illinois	A	1940	1941
Melton, J. W.	M.A.	Texas	O	1941	1945
Bruton, B. J.	M.A.	Southwestern		1942	1944
Southall, O. C.	M.A.	TTC		1944	1962
Allen, R. T.	B.A.	-		1946	1947
Baker, E. B.	B.A.	-		1946	1947
Cohea, B.	B.S.	-		1946	1948
Menaul, M.M.	B.A.	-		1946	1949
Stuart, M.R.	M.A.	TTC	A	1946	1979
Bryant, M.L.	B.S.	-		1947	1948
Hardey, C.E.	B.A.	-		1947	1948
Plemmons, A.E.	B.S.	-		1947	1949
Work, M.L.	B.A.	-		1947	1948
Jones, P.T.	Ph.D.	MIT	P	1948	1951
Watson, J.A., Jr	Ph.D.	LSU	B	1948	1951
Brock, J.	M.S.	TTC		1948	1954
Dodge, E.H.	M.S.(CE)	Washington		1948	1951
Crow, B.C.	B.S.	TTC		1948	1949
Detman, R.F.	Ph.D.	LSU		1949	1951
Hufstedler, R. S.	M.S.	TTC		1949	1951
Tinsley, S.W.	Ph.D.	Northwestern	O	1949	1950
Holcomb, D.E.	Ph.D.	Michigan	CE	1950	1955
Berst, N.W.	Ph.D.	Penn State	O	1950	1951
Estok, G.K.	Ph.D.	Notre Dame	O	1951	1961
Lee, S.H., Jr	Ph.D.	Ohio State	O	1951	1975
Renard, J.	Ing. Chim.	Nancy	CE	1951	1970
Thoma, R.E., Jr	Ph.D.	Colorado		1951	1952
Kimball, M.D.	B.S.	-		1952	1953
Wilhoit, R.C.	Ph.D.	Northwestern	P	1953	1957
Gryder, D.Y.	B.S.	Southeastern State		1953	1954
Bryant, J.M.	B.S.	St. Mary's		1954	1955
Fain, P.	Ph.D.	TTC		1954	1957
Franz, G.E.	Ph.D.	Columbia		1954	1955
Shine, H.J.	Ph.D.	London		1954	
Tilton, P.C.	M.S.	TTC		1954	1955
Wendlandt, W.W.	Ph.D.	Iowa	I	1954	1966
Bradford, J.R.	Ph.D.	Case Western	CE	955	1959
Rekers, R.G.	Ph.D.	Colorado	A	1955	1986

Freasier, B.F.	M.S.	Texas A & I		1956	1957
McPherson, C.M.	Ed.D.	TTC		1956	1984
Tidwell, K.G.	B.S. Ed	TTC		1956	1957
Adamcik, J.A.	Ph.D.	Illinois	O	1957	1988
Hendry, R.A.	Ph.D.	Baylor	B	1957	1959
Wilson, C.E., Jr	A.B.	Missouri		1957	1967
Crosthwait, A.A.	B.S.	Eastern NM		1959	1961
Draper, A.L.	Ph.D.	Rice	P	1959	1985
Morris, M.L.	Ph.D.	Ohio State	I	1960	1961
Anderson, J.A.	Ph.D.	Oregon State	B	1961	1993
Trusell, F.C.	Ph.D.	Iowa State	A	1961	1964
Hecht, H.G.	Ph.D.	Utah	P	1962	1966
Thompson, R.J.	Ph.D.	Texas	I	1962	1968
Stubbs, M.F.	Ph.D.	Chicago	FC	1963	1968
Wilde, R.E., Jr	Ph.D.	Washington	P	1963	1991
Fresco, J.M.	Ph.D.	Arizona	A	1964	1965
Scott, D.R.	Ph.D.	Houston	P	1965	1967
Song, P.S.	Ph.D.	Cal.	B	1965	1987
Herndon, W.C.	Ph.D.	Rice	O	1966	1972
Lipschitz, I.	Ph.D.	VPI		1966	1968
Mitchell, R.E.	Ph.D.	Purdue	I	1966	1989
Delaney, B.H.	B.S.	Kent State		1967	1974
Marx, J.N.	Ph.D.	Kansas	O	1967	
Redington, R.L.	Ph.D.	Washington	P	1967	
Guerrant, W.B.	Ph.D.	N. Carolina	FC	1968	1984
Carlyle, D.W.	Ph.D.	Iowa State	I	1969	1974
Marcoux, L.S.	Ph.D.	Texas	A	1969	1974
O'Brien, T.J.	Ph.D.	Wisconsin	P	1969	1979
Shoppee, C.W.	Ph.D.	London	O	1970	1975
Mills, J.L.	Ph.D.	Texas	I	1970	1995

[1] The list of names in this Table has been drawn for the most part from a brief history of the department (14) written by Joe Dennis for the symposium that accompanied the opening of the addition to the chemistry building in 1970. There were some gaps in Dennis's list, and they have been filled by consulting the TTC catalogs year by year. There appears to be no other cumulative list of the department's faculty in the University's archives. [2] Area refers to the teaching specialty: A, analytical; B, biochemistry; CE, chemical engineering; FC, freshman coordinator; I, inorganic; P, physical; O, organic. Where a specialty is not listed, the information could not be found.

ACKNOWLEDGMENTS

The writing of this history began with an invitation by Dr. E. Thomas Strom to participate in a symposium "History of Chemistry in the Southwest" at the 1998 American Chemical Society meeting in Dallas. Strom's ambition was next to persuade each of the speakers who described a chemistry department to write up the talk for publication. I apologize to Tom Strom for being so tardy, and I thank him for his patience. Much of the information on which this history is based came from the archives of TTU's Southwest Collection (SWC), in which early bulletins, catalogs, and correspondence of P. W. Horn, C. B. Jones, anad R. C. Goodwin are housed. I thank the staff of the SWC for their help in locating letters and photographs, the latter for the ACS presentation. Mary Ellis in TTU's Office of Institutional Research was especially helpful in finding files on early faculty members, getting permission for me to read them, and for providing lists of graduates in the period of interest. In the Department of Chemistry and Biochemistry, Cheryl Blasinghame and Kathy Jones were always ready in filling gaps in faculty and student data. During 1997-1999, conversations with and letters from Joe Dennis were the sources of much of the anecdotal history. His memory was remarkable. Dr. C. M. McPherson, whose own relationship with TTU began as a student in 1938, was also very helpful in filling gaps in records and memories. McPherson's student career, interrupted by wartime service flying supply missions "over the hump" between India and China, was resumed in 1947 when he received the B. S. Chem. E. degree. My ability to hold a post-retirement appointment as Research Professor at TTU, as well as to be Paul Whitfield Horn Professor Emeritus, allowing me to continue research in chemistry and have the opportunity to write this history, is made possible by the continued support as a Horn Professor by the University and by the support of my research from The Welch Foundation (Grant D-0028).

REFERENCES AND NOTES

*Based on a paper presented at the 215th national meeting of the American Chemical Society, Dallas, TX, Spring, 1998, HIST 017.

1. C. L. Gibbs, "The Establishment of Texas Technological College," M. A. Thesis, Texas Technological College, 1939.
2. H. D. Wade, in E. H. West, Ed., *Establishment of Texas Technological College, 1916-1923*, Texas Tech Press, Lubbock, TX, 1956.
3. West Texas A & M University, so named as a branch of TAMU on September 1, 1990, an irony that would not go unnoticed by the early advocates of a State College in West Texas. Formerly, West Texas Normal College, established in 1909 by a bill from the Legislature to "establish a State Normal School for the education of white teachers west of the 98th meridian," named West Texas State Teachers College in 1922 and West Texas State University in 1963. This information is taken from the WTAMU website.
4. Hardin-Simmons University, established 1891; Abilene Christian University (1976), established as Childers Classical Institute in 1906, accredited as a junior college in 1914, a senior college in 1919, and named Abilene Christian College in 1920; McMurry University, opened in September, 1923. This information is taken from the respective websites.
5. There was a general consensus in the late 1960s that Texas Tech was no longer a technological college and was more suited to being called a university. The change to Texas Tech University was made in 1970. The name was derided by a large segment of the faculty, particularly those not in the science and technological departments, as being meaningless and misleading to the collegiate colleagues beyond Lubbock. The name was chosen mainly to retain the abbreviation Texas Tech and the double-T symbol entrenched in Texas Tech's history. The middle name Tech was described by its proponents as not connoting technology, because it was not followed by a period. The writer of this history has given up trying to educate correspondents from England that Tech is not followed by a full stop (i. e., period).
6. Papers of P. W. Horn, Southwest Collection (SWC), Texas Tech University.
7. Letter from WTR to C. B. Jones, April 29, 1968. CBJ papers, SWC.
8. R. H. Andrews, *The First Thirty Years, A History of Texas Technological College 1925-1955*, Texas Tech Press, Lubbock, TX, 1956.
9. Letter from J. R. Bailey to PWH, December 27, 1924. PWH papers, SWC.
10. Letter from W. L. Ray to PWH, September 13, 1924. PWH papers, SWC.
11. Letter from W. T. Read to F. D. Galbraith, June 8, 1926. PWH papers, SWC.
12. D. D. Downey, circulation librarian, Mary F. Shipper Library LRC, Potomac State College of West Virginia University, Keyser, WV. I am indebted to Ms. Downey for providing copies of numerous articles about Galbraith.
13. Fourth Bulletin of Texas Technological College, 1925.
14. J. Dennis, "A Brief Historical Statement Concerning the Department of Chemistry," compiled for the Symposium (15), held to commemorate the opening of the addition to the Chemistry Building. Appended to this "statement" is a list of names of all students who received bachelors, masters and doctors degrees, year by year, 1933-1970, the names of all students who received B. S. Ch. E. degrees, 1934-1959, year by year, and the names of the faculty, 1925-1970, in the year of appointment.
15. W. Shoppee, Ed., "Excited States of Matter," Graduate Studies Texas Tech, No. 2, Texas Tech Press, Lubbock, TX, 1973.
16. Annual catalogs of TTC, 1929-1947. C. C. Galbraith died January 27, 1976, in Corpus Christi.
17. T. Read, "Boiler Waters: Their Chemical Composition, Use and Treatment," University of Texas Bulletin No. 1752, September 15, 1917.
18. T. Read, *Industrial Chemistry*, John Wiley, New York, 1933.
19. Early catalogs of TTC, 1925-1926.
20. Letter from PWH to WTR, undated. PWH papers, SWC.
21. Ref. 8, p 245.
22. Faculty file on R. Rowan, Jr., TTU Office of Institutional Research.
23. J. Dennis, "Recollections Concerning the Department of Chemistry and Chemical Engineering with Special Reference to the Years 1938-1969," handwritten and undated, presented to H. J. Shine, fall 1997.
24. C. M. McPherson, conversation with H. J. Shine, May 12, 2002.
25. J. Dennis, conversation with H. J. Shine, undated.
26. Letter from H. Eyring, Dean of the Graduate School, University of Utah, to H. J. Shine, February 9, 1955.

ABOUT THE AUTHOR

Dr. Henry J. Shine, Research Professor and Paul Whitfield Horn Professor Emeritus of Chemistry, is former Chair, Department of Chemistry and Biochemistry, Texas Tech University, Lubbock TX 79409; HENRY.SHINE@ttu.edu.

NOYES LABORATORY, AN ACS NATIONAL CHEMICAL LANDMARK: 100 YEARS OF CHEMISTRY AT THE UNIVERSITY OF ILLINOIS

Sharon Bertsch McGrayne

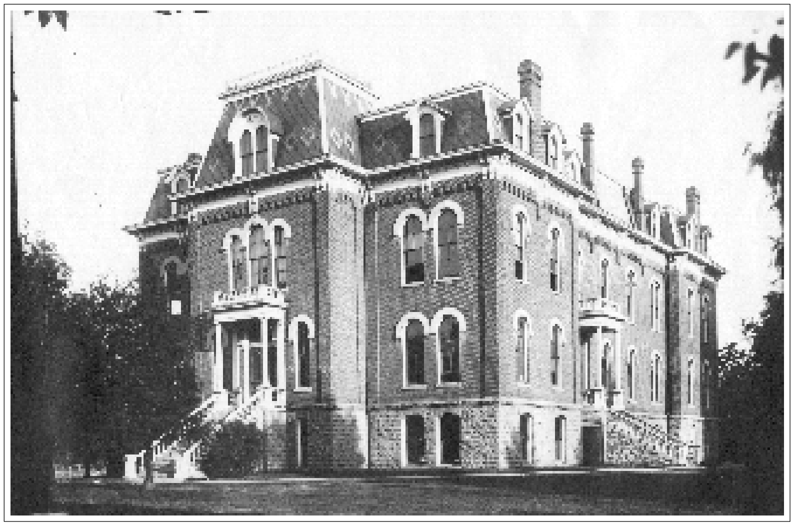
Ten Nobel Prize winners and, in the past 80 years, almost one out of every four presidents of the American Chemical Society (ACS) have studied or worked in the William Albert Noyes Laboratory of Chemistry at the University of Illinois in Urbana-Champaign (1). So have an estimated 12,000 other chemists, more than in any other structure in the United States and a significant proportion of the American Chemical Society's membership. On September 14, 2002, the ACS celebrated the centennial of Noyes Laboratory by designating it an ACS National Historic Landmark.

A. W. Palmer

The early history of Noyes Laboratory suggests several reasons for its remarkable success. Chemistry was important to the University of Illinois from its beginnings in 1867 as the Illinois Industrial University. Only a handful of U.S. institutions had even a rudimentary chemical laboratory at the time (2), yet in 1868 the school's first president declared in his first annual report, "It is especially important that an appropriation should be made to fit up, at once, a chemical laboratory (3)."

At first, the chemistry department was housed in the basement of Illinois Industrial's only building, without gas, electricity, water, or central heat. Bench space was limited so students worked in shifts. Within a decade, however, chemistry became the first department on campus to move into its own building, Harker Hall, a $40,000 structure with both gas and water. During the 1880s, when Illinois Industrial University changed its name to the University of Illinois, federal aid for land grant education doubled the university's budget; and Arthur William Palmer became the first of five chairs who–with only two breaks–would lead Illinois' chemistry department for the next century.

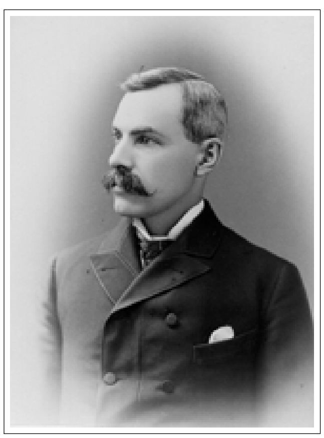
Under Palmer, Illinois' chemistry department became an interdisciplinary entity devoted in large part to helping the state of Illinois. Palmer was born in England but educated at Illinois and Harvard and in Germany (4). After a typhoid epidemic, the legislature established the Illinois State Water Survey to analyze local water supplies and appointed Palmer its first director. The State Water Survey remained in the department for 44 years (5). Illinois' unusual combination of chemistry and chemical engineering originated when Samuel Wilson Parr joined the department, and the two men divided the workload. Chemical science stayed with Palmer, and industrial chemistry-later called chemical engineering-went to Parr (6). Parr helped Illinois' bituminous coal compete with Eastern Appalachian coal (7) by developing an alloy and various processes and fuel testing devices, including the Parr bomb.



Harker Hall

Notwithstanding these successes, when lightning struck and badly damaged the chemistry building in 1896, it took Palmer four years to convince the Illiinois Legislature to appropriate $100,000 for a new structure. Palmer's E-shaped building, the western half of the structure that still exists, opened in 1902. It was supposed to provide enough space for chemistry for 25 years (8).

Despite the recent fire, Palmer's building was wooden-framed. Its fireproofing consisted of 12 inches of sand between floors and ceilings (9), and wooden beams stretched through the flues. In addition, the building's hoods never functioned properly, and Illinois chemists worked with inadequate ventilation until a major renovation in 1941 (10). Moreover, the university and particularly its chemistry department were growing quickly. Within a decade, the building was overcrowded.



S. W. Parr

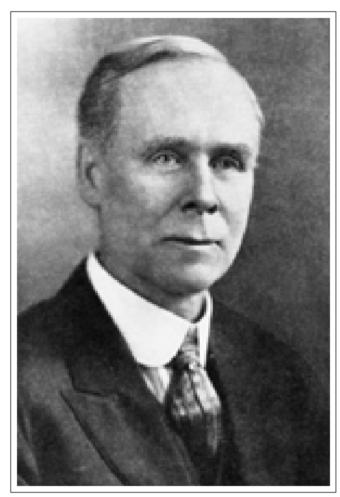
Palmer died in 1904 at the age of 43, apparently of overwork. The university hired a new chemistry head, William Albert Noyes, for whom the chemistry building was named in 1939. Noyes built Illinois' department into one of the most prestigious in the United States. He also doubled the size of its building and made it the largest and most modern chemistry laboratory in the world. How could Noyes succeed where the politically astute, workaholic Palmer could not?

First, Noyes was nationally known as the discoverer of the definitive structure of camphor, the former chief chemist at the National Bureau of Standards, the editor of the prestigious Journal of the American Chemical Society, the founder of Chemical Abstracts, and one of the founders of Chemical Reviews (11).

Second, chemical research was the basis for Germany's growing industrial and military power, and anything seemed possible with chemistry. Speakers in Noyes Laboratory dreamed of turning cornstarch into India rubber; of using the "emanations" from radium to transform copper into potassium (for fertilizer) (12); and of mining seawater for potash, again for fertilizer. An Illinois chemistry professor declared in 1916, "Don't say it can't be done, for it is being done by miles of seaweed (13)."

Most important, Noyes was asking for money to expand the chemistry building during the military buildup preceding World War I. As an Illinois chemistry department brochure explained at the time, the sudden exclusion of German products from U.S. markets "opened the eyes of the whole country to our inferior and dependent position in many lines of chemical manufacture and to the importance of establishing such industries on a better footing in America (14)."

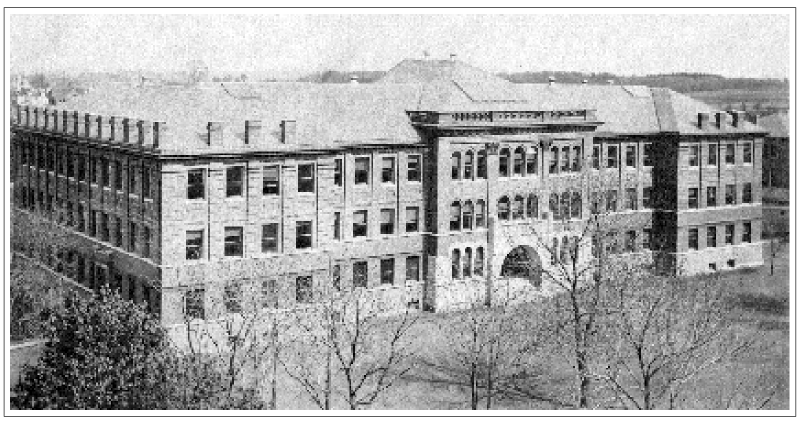
Enrollment in Illinois' chemistry courses multiplied more than six times in 15 years (15). Growth in graduate education was particularly rapid. By the beginning of World War I, more U.S. students were earning Ph.D.'s in chemistry than in any other science (16). As



W. A. Noeyes

a result, Noyes was able to secure the first state appropriation made to a state university specifically for graduate work (17) and $500,000 to enlarge the chemistry laboratory. New construction enclosed Palmer's E-shaped building to form a hollow square with almost four acres of working space-twice as much as before. The addition had many unique features: distilled water, compressed air, piped-in hydrogen sulfide, steam, a special vacuum system, and 150 electric wall plugs. It was finally fireproof, and its ventilation system was supposed to exchange the air six to eleven times an hour (18).

The addition opened for business in 1915, but its dedication was delayed a year to give the Twin Cities of Champaign and Urbana time to build a hotel. The 1916 ACS spring meeting was held in conjunction with the building's inauguration, and 729 visitors came, more than to any previous ACS convention (19). There were not enough organic chemists, however, to fill a 35-seat classroom (20).



Noyes Laboratory

The 1916 dedication of Noyes Laboratory celebrated chemistry's coming of age in the United States and the highpoint of women's participation until the late 20th century. Women made up 8 % of chemistry's instructional staff (21); but, banned from the all-male chemistry club and fraternities, women faculty and students had formed a sorority (22). At the convention banquet, the sorority served refreshments to the men. As the department grew in size and prestige under Noyes and later Roger Adams, women's participation declined markedly. During the Adams years, there was a "definite feeling," as one observer put it, that "a graduate student should have neither wife nor car (23)." The chemistry department hired its first tenure-track woman in 1985 (24).

If the Department overlooked women chemists, it chose its men well. One of Illinois' early Ph.D.'s was St. Elmo Brady, the first African-American Ph.D. chemist in the United States. Of his years in Noyes Lab, Brady said, "They began with 20 whites and one other and ended, in 1916, with six whites and one other (25)."

As chair, William Noyes also hired important men, especially Roger Adams and the nucleus of division heads who would constitute the department's establishment for a quarter century between 1926 and 1954. Under Noyes, the university hired, in chronological order, B. Smith Hopkins, Roger Adams, Carl S. (Speed) Marvel, Worth H. Rodebush, William C. Rose, and G. Frederick Smith.

As Roger Adams emphasized, military research during World War I gave chemistry a big boost. Adams figured that the Chemical Warfare Service "brought together 80 % of the chemists [in the U.S.] (26). " He himself directed a poison gas research laboratory, studied arsenic compounds, and worked out a simple way to make tear gas.

Marvel remained in Noyes Laboratory to run a financially self-supporting project called "Summer Preps." Graduate students earned summer salaries by manufacturing in Noyes' sweltering attic various organic reagents for the American military, manufacturers, and medical and university researchers. By then, Noyes Laboratory suffered from an almost complete lack of ventilation; one professor could joke that fire was not a danger because the air in Noyes would not support combustion (27, 28). Black smoke often filled the Preps room; students' hands became black with chemicals; and "a certain aroma that wafted from each worker became the mark of the preps chemist (29)."

As Speed Marvel described the work (30):

> Various government groups needed materials, especially for the new chemical warfare which had been introduced by the Germans.... Many of the requests for chemicals were for prospective chemical warfare agents, and it was quite a task to make such materials in the university laboratories which had rather poor ventilating hoods.

Rogers Adams, by then a major in the chemical warfare program (30):

R. Adams

...used us to furnish needed chemicals on a rush basis. We had many experiences with toxic materials ... Some accidents occurred because safety regulations had not yet come to university laboratories.

Such attitudes about safety were typical of generations of macho chemists. Marvel claimed, for example, that he could identify 500 compounds by smell alone (31 It was not until 1980 that Illinois' organic chemists developed, as department head Herbert S. Gutowsky reported (32):

> ..a new laboratory safety program which for the first time has been able to encourage a set of laboratory safety practices comparable to those commonly encountered in industrial chemical laboratories.

During the 1920s and 1930s, between the two World Wars, organic chemistry at Illinois entered what has been called "the Golden Age of Roger Adams." Adams, who arrived in Illinois in 1916, was a man of particular charm and force, buoyant and gregarious at the same time that he was intellectually brilliant, pragmatic, and extremely tough (33). Ten years later, he was unanimously chosen head of a very young department; only one faculty member was more than 40 years old (34).

Adams spent 56 years at Illinois, 28 of them as department head. He developed the platinum oxide catalyst that hardens liquid vegetable oils into solid fats for soap and shortening and analyzed several biologically active compounds, including a natural oil used to treat leprosy, a toxic constituent of cottonseed meal, and the active ingredients in marijuana. Adams published 425 scientific papers.

Under Adams and Marvel, Illinois became the country's largest producer of organic chemists, particularly Ph.D. chemists. Between the wars, Adams guided 21% of Illinois' Ph.D. chemists, many of whom became important in industrial research and management, in universities, and in the ACS (35).

Adams' student Wallace Hume Carothers was perhaps the single most important product of the University of Illinois' chemistry program. Carothers arrived at Illinois for graduate school in 1920 after six desperate years spent in his father's secretarial school and working his way through college. Carothers also struggled, without modern medications, with both thyroid disease and bipolar mood disorder, formerly called manic depression. Despite his health problems, Carothers earned a Ph.D. and taught for two years in Noyes Laboratory before going to Harvard for another two years and then to Du Pont's Experimental Station in Wilmington, Delaware, for nine more years. Throughout his life, Carothers maintained ties to Noyes Laboratory and the friends he made there (36).

Carothers and Marvel

While at Du Pont, Carothers conducted the first fundamental scientific research in the American chemical industry. He showed that-surprisingly-polymers are long but otherwise normal molecules held together by normal bonds. Then Carothers invented Neoprene, a synthetic rubber, and nylon, the first commercially marketable synthetic fabric and the beginning of the modern era of plastics and synthetics. Within days of arriving at Du Pont in February, 1928, Carothers outlined the research project that would start the field of polymer chemistry and culminate in nylon. In his plan, Carothers predicted that, if it succeeded, it would be an "important factor in the great success of the work in organic chemistry at the University of Illinois (37)." A few months later on March 1, 1928, Carothers submitted a detailed research plan to Du Pont and wrote at the top (38):

> Copies to: Dr. Adams: University of Illinois; Dr. Marvel: University of Illinois.

Adams and Marvel had become consultants at Du Pont (39), and at one point, Carothers wrote a friend about Adams (40):

> I think his visit will put about $5,000 worth of pep into some of the chemists around here.

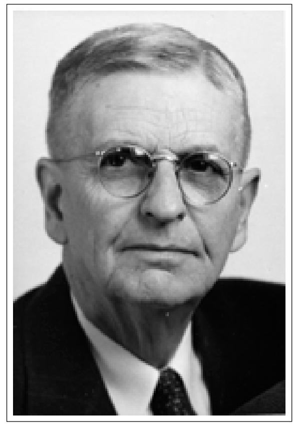
Marvel, who had been Carothers' instructor in Noyes Laboratory, went on to start at Illinois the first major polymer research program in an U.S. university. Marvel said, years later, after Carothers' suicide (41):

> I learned most of my polymer chemistry at Du Pont from Carothers.

The high point of Adams' tenure as head of the chemistry department occurred, he thought, during the 1930s when the American chemistry establishment consisted largely of Illinois and some other Midwestern state universities with a sprinkling from Harvard, Columbia, Princeton, Caltech, and a few other chemists. According to the so-called Illinois system, small groups of graduate students from different backgrounds were expected to do research "fraternally." Illinois' undergraduates went to other midwestern universities for their postgraduate education, and those midwestern universities, in turn, sent their best students to Illinois for graduate school. More undergraduates in chemistry went on to graduate school from Illinois than from any other institution in the U.S. Illinois chemistry offered social mobility to generations of able and ambitious young men of limited means for whom medical school would have been too costly (42).

Ironically, despite the reputation of Illinois' organic program before World War II, the most famous figures in the department were an inorganic chemist and a biochemist. In 1926, rare earth chemist B. Smith Hopkins announced that he had discovered a tiny amount of the long-sought Element No. 61. Hopkins named it "Illinium" for the state and university. It became a departmental embarrassment because, although it was learned after World War II that Illinium was not an element after all (43), it lived on in campus charts of the periodic table and in textbooks, including one that Hopkins edited, as late as 1956 (44).

In 1935, a decade after Hopkins' Illinium, biochemist William C. Rose discovered threonine, the last of the eight essential amino acids that people need but must get from food because our bodies cannot make them or cannot make enough of them. Rose eventually calculated the minimum daily requirement for each essential amino acid (45). Rose began his feeding experiments, involving rats and more than 42 graduate students, in 1930. Today they would not be permitted because they could have jeopardized the health of his students (46). But Rose wanted to make a synthetic diet for patients who could not digest proteins or who had to be fed intravenously. At the time there was no complete synthetic diet for them; nor was there any way to characterize and separate one amino acid from another in a protein. Feeding studies had long played a quite honorable role in science, and Rose regarded them as his only choice. In any event, the work of both Hopkins and Rose demonstrated that Illinois was far more than just an organic chemistry department, even under Roger Adams.



W. C. Rose

During World War II, Marvel coordinated from Noyes Laboratory two interdisciplinary research efforts-one of them larger than the Manhattan Project-that produced a usable synthetic rubber within a year and chloroquine, in time for its use in the Pacific against malarial mosquitoes. After the war, during the 1950s, Marvel explained that (47):

> With the new synthetic rubber program, the big surge in synthetic textiles, and the growth of the automobile industry and its needs for new materials, the chemical industry profited in the fifties and sixties. Research laboratories expanded and new ones started up. The demand for chemists reached new peaks.

Illinois, which had played key roles in the development of both synthetic textiles and rubber, was ready for the post-war growth. Within one year between 1953 and 1954, the department underwent a massive generational upheaval as Roger Adams and most of his divisional leaders retired or moved to other jobs. Nonorganic chemistry emerged from under the prewar shadow of organic chemistry, and Adams was succeeded first by biochemist Herbert Carter and then by physical chemist Herbert S. Gutowsky.

H. E. Carter

Carter not only replaced most of the department division heads with younger men but also expanded the senior faculty-because of growing enrollments-by 35 percent (48). In addition, as mathematics and quantum physics became increasingly important to chemistry, Illinois developed strong programs in inorganic, physical, and analytical chemistry to balance its organic work.

Gutowsky's application of nuclear magnetic resonance spectroscopy to chemistry is indicative of these new directions. Gutowsky said that Roger Adams hired him "accidentally" in 1948 to do infrared spectroscopy for Illinois' organic chemists (49). Gutowsky came to Illinois as a young instructor, however, convinced that the magnetic vibrations of protons could reveal what was happening inside a molecule. So Gutowsky

H. S. Gutowsky

gambled his career. With the help of a graduate student in chemistry and an undergraduate in electrical engineering, he rigged up an NMR apparatus (50). With this device, he discovered and explained the phenomenon of spin-spin coupling, which allows scientists to determine the relative locations of neighboring atoms. He also predicted and then discovered the phenomenon of chemical exchange, which chemists use to understand how atoms and molecules move (51).

Gutowsky's monumental work on NMR and Rose's essential amino acid studies are regarded as Noyes Laboratory's greatest scientific and medical discoveries. Gutowsky later chaired the chemistry department and the School of Chemical Sciences and built and staffed what he regarded as "revolutionary" service centers to operate NMR spectrometers, mass spectrometers, X-ray equipment, and computers (51).

How did the inhabitants of Noyes Laboratory become such a significant force in chemistry? Several aspects of its history are particularly striking. First, Noyes' interdisciplinary, organic, graduate orientation began before World War II. Second, leading scientists-not just skilled administrators-chaired the department and provided scientific vision. Third, Noyes Laboratory scientists not only conducted research that benefited the State of Illinois and a broad region around it; they also gave their constituents' children social and economic mobility. Finally, Noyes nurtured the long-term careers of its students, sending undergraduates on to graduate schools and placing graduate students in academia and industry.

ACKNOWLEDGMENTS

Dr. Vera Mainz made this article possible by assembling historical data about the history of Noyes Laboratory for the American Chemical Society's National Historic Landmarks Program and by answering the author's many questions. Many documents are accessible in a website

REFERENCES AND NOTES

1. Many of the documents on which this article is based are accessible in a website assembled by Dr. Vera V. Mainz at http://www.scs.illinois.edu/chem/history/. The website is designated here as MW. Other important documents remain in the Archives of the University of Illinois Department of Chemistry, referred to here as A-UIDC.
2. G. B. Frankforter, "Teaching of Chemistry in State Universities," Science 1907, 26, 703-706.
3. Anon., University of Illinois Bulletin: University of Illinois Circular of Information of the Department of Chemistry, XIII, Feb. 21, 1916, 25, 16, MW.
4. S. W. Parr, "Professor Arthur W. Palmer: Memorial Convocation," Feb. 7, 1904, A-UIDC.
5. Ref. 3, S. W. Parr, "Historical Sketch of the Chemistry Department," pp 16-29.
6. D. S. Tarbell and A. T. Tarbell, Roger Adams: Scientist and Statesman, American Chemical Society, Washington, DC, 1981, 38.
7. G. D. Beal, letter to Maynard Brichford, Oct 13, 1965, A-UIDC.
8. Ref. 5, p 26.

9. Ref. 6, p 41.
10. V. Bartow, “Developments in the Chemistry Department University of Illinois 1927-1941,” University of Illinois Department of Chemistry, 1941, 28, MW.
11. Anon., “William Albert Noyes,” University of Illinois Department of Chemistry Development during the Period 1927-1941, 1941, 5-8, MW.
12. W. McMurtrie, “Relation of Chemistry to the Industries,” Science, 1907, 26, 697.
13. W. R. Whitney, “Research as a National Duty. The Dedication of the New Chemistry Building, University of Illinois,” J. Ind. Eng. Chem., 1916, 8, 523ff, A-UIDC.
14. Ref. 3, p 31.
15. Sir W. A. Tilden, Chemical Discovery and Invention in the Twentieth Century, E. P. Dutton and Co., New York, 1916, 38, MW.
16. Ref. 3, p 30.
17. Ref. 6, p 40.
18. B. S. Hopkins, “Description of the Chemical Laboratory of the University of Illinois. The Dedication of the New Chemistry Building, University of Illinois,” J. Ind. Eng. Chem., 1916, 8, 537-547, A-UIDC.
19. V. Bartow, “The First Fifty-Five Years of the University of Illinois Section,” Typescript of a paper presented at the ACS meeting, Spring 1961, St. Louis, Unpublished, 9-10, A-UIDC.
20. C. S. Marvel, “The Priestley Medal Address: The Responsibility of American Chemists Today,” Chem. Eng. News, 1956, 34, 2168-2171, MW.
21. Ref. 3, pp 9-13.
22. Ref. 3, p 44; University of Illinois Bulletin “Special Circular of the Department of Chemistry 1916-1927,” Aug. 30, 1927, XXIV (52), 29.
23. Ref. 19, p 11; E. P. Rogers, “An Anecdotal History of Chemistry Prior to 1950 at the University of Illinois at Urbana,” Unpublished typescript, 9, 17, A-UIDC.
24. G. Girolami, private communication.
25. V. V. Mainz, MW, Appendix 1.
26. Ref. 6, p 62.
27. R. C. Fuson, “Chemistry at the U of I: A Centennial Review,” Speech given Oct. 18, 1967, at University of Illinois, MW.
28. R. Adams, “Annual Report of the Chemistry Department, 1938-39,” May 15, 1939, A-UIDC.
29. Ref. 6, pp 55-6.
30. C. S. Marvel, “My Sixty-five Years in Chemistry,” Speech given at University of Illinois, March, 1976, pp 5, 6, 9, A-UIDC.
31. Ref. 30, pp 2-4; Ref. 23, p 13.
32. Anon., Annual Report, School of Chemical Sciences, Aug. 1980, A-UIDC.
33. Ref. 6, p 38.
34. Ref. 27.
35. Ref. 6, passim, pp 5, 98, 221-228.
36. S. B. McGrayne, Prometheans in the Lab: Chemistry and the Making of the Modern World. McGraw-Hill, New York, 2001.
37. W. H. Carothers, letter to Hamilton Bradshaw, Nov. 9, 1927, 3, Hagley Museum and Library, Wilmington DE, HML 1896.
38. W. H. Carothers, letter to Charles M. A. Stine, March 1, 1928, Hagley Acc. 1784, Box 24.
39. Interview with Carl S. Marvel, May 2, 1983, by David A. Hounshell and John K. Smith, Hagley HML 1878, 1.
40. W. H. Carothers, undated letter to John “Jack” Johnson, Hagley HML 1842.
41. Ref. 39, p 7.
42. Ref. 6, pp 84-5, 95-7, 105.
43. G. D. Beal, Department History of the Decade 1916-27, 22, A-UIDC.
44. Ref. 23, p 9; P. Wood, “Illinium, the Element that Wasn’t,” News-Gazette, March 16, 1986, C1, A-UIDC.
45. H. E. Carter, “Identification and Synthesis of Threonine,” Proc. Fed. Am. Soc. Exp. Biol. [Fed. Proc.], 1979, 38, 2684-2686; D. A. Roe, “William Cummings Rose: A Biographical Sketch,” J. Nutr., 1981, 3, 1313-1320; W.C. Rose, “Amino Acid Requirements of Man,” Fed. Proc., 1949, 8, 546-552; W. C. Rose, “The Role of the Amino Acids in Human Nutrition.” Proc. Am. Philos. Soc., 1947, 91, 112-116.
46. L. Hager, telephone communication. Sept. 9, 2002.
47. Ref. 30, A-UIDC.
48. Anon., “History and Development 1951-1961.” Department of Chemistry and Chemical Engineering, 18-20, A-UIDC.
49. H. S. Gutowsky, “The Coupling of Chemical and Nuclear Magnetic Phenomena,” in D. M. Grant and R. K. Harris, Ed., Encyclopedia of Nuclear Magnetic Resonance, John Wiley & Sons, New York, 1996, Vol. 1, 360-368.
50. C. P. Slichter, telephone conversation, Aug. 30, 2002.
51. V. V. Mainz, E-mail communication, Sept. 2, 2002.
52. Dr. Vera Mainz made this article possible by assembling a massive amount of historical data about the history of Noyes Laboratory for the American Chemical Society’s National Historic Landmarks Program and by answering the author’s many questions. Many of the documents appear in http://www.scs.illinois.edu/chem/history/. All others are in the Chemistry Department’s archives.

ABOUT THE AUTHOR

Sharon Bertsch McGrayne, the author of Prometheans in the Lab: Chemistry and the Making of the Modern World (McGraw-Hill, 2001), gave the keynote address about the history of Noyes Laboratory when it was designated an ACS national historic landmark on September 14, 2002. McGrayne lives in Seattle, Washington. Her web page address is http://www.McGrayne.com.

BOOK REVIEWS

Gehennical Fire: The Lives of George Starkey, an American Alchemist in the Scientific Revolution. William R. Newman, University of Chicago Press, Chicago, IL, 2003, xxii + 386 pp, ISBN 0-226-57714-7, $27.50.

Alchemy Tried in the Fire: George Starkey, Robert Boyle and the Fate of Helmontian Alchemy, William R. Newman and Lawrence Principe, University of Chicago Press, Chicago, IL, 2002, xiv + 344 pp, ISBN 0-226-57711-2, $40.00.

Born in Bermuda in 1628, George Starkey matriculated at Harvard in 1643, and almost immediately became familiar with the theory and practice of alchemy and iatrochemistry (collectively called "chymistry") as it was practiced in New England. In 1650, Starkey emigrated to London, where he became deeply involved with a group of natural philosophers centered around Samuel Hartlib. Starkey's skill as an adept in the chymical arts in America was already known before he moved to London, but within a short time, Starkey's fame in alchemical circles increased dramatically on the publication of works under his own name and works written under the name of Eirenaeus Philalethes. Starkey explained to colleagues that he was working as an agent for Philalethes, an adept who Starkey had met in New England, and who had remained there and sent his periodic manuscripts to Starkey for publication. Philalethes' works quickly became more influential than the works that Starkey published under his own name, and Starkey crafted the persona of Philalethes so well that nobody at the time suspected that Starkey and Philalethes were in fact one and the same. More than a pseudonym, Philalethes became Starkey's alter ego, a personality so real that others actually spread stories about him, and only in 1919 did George Lyman Kittridge first suggest that Philalethes and Starkey were identical. The origins and nature of the specific chemical ideas in the Philalethan works have long remained obscure under layers of alchemical metaphor and symbolism, although as Newman notes in Foreword to the new edition of *Gehennical Fire*, Starkey was the first North American author in any subject to be widely read in Europe. And as Newman and Principe argue in *Alchemy Tried in the Fire*, Starkey deserves wider recognition as a pivotal figure in seventeenth century science, the chief mentor to Robert Boyle and a crucial link between Jean Baptiste van Helmont and Antoine Laurent Lavoisier.

Starkey's remarkable life and accomplishments are the subject of Newman's *Gehennical Fire*, first published by Harvard University Press in 1994 and recently reprinted by the University of Chicago Press with a new foreword by Newman. Harvard was only seven years old when Starkey matriculated, yet as Newman clearly shows, the physics curriculum contained an innovative and sophisticated version of the mechanical philosophy of the seventeenth century based on a "belief in the existence of minimal parts, a theory that the smallest parts of fire, air, water, and earth form an ascending scale size, and a confidence that such particles remain, bonded together, in a mixture" (p. 32). This matter theory provided Starkey with the theoretical underpinning for transmutational alchemy and medicinal chemistry. In 1644, the obscure physician Richard Palgrave introduced Starkey to alchemical laboratory practice, and Starkey became a member of the group of alchemists associated with John Winthrop, Jr., the first governor of Connecticut.

Because his contacts with the New England alchemical circle, Starkey's reputation preceded him on his emigration to England and his subsequent association with the Hartlib circle. Newman suggests that it was the nature of the Hartlib circle—steeped in millenarianism, alchemy and secrecy—that prompted Starkey to create the fictional Philalethes. Starkey carefully crafted the personality of Philalethes to make him seem a greater adept than Starkey himself, and by playing the role of Philalethes' "agent," Starkey would be in a position to dispense important and desirable alchemical secrets. Starkey combined alchemy and prophecy in a way that had a long tradition in alchemical works, and Starkey based much of Philalethes' story and personality on Michael Sendivogius. One of Starkey's primary motives was to obtain financial support from members of the Hartlib circle (including Robert Boyle), suggest that like other "alchemists," his claimed work chemistry could have been a deliberate fraud. As Newman points out, however, Starkey had diverse motives for creating Philalethes, including the need to maintain trade secrecy in the invention of dyes, perfumes, drugs, and transmutational processes, and the possible need for Starkey, as an immigrant from New England, to make himself more desirable as an apparent master of secrets for gaining access to members of the Hartlib circle.

To modern readers, the possibility of alchemical fraud is also apparent in the alchemical texts themselves, especially those by Philalethes, that are written in bizarre figurative language, with metaphors and codes to conceal their secrets. What is one to make, for example, of kings drowning in their own sweat, ravens that melt after eating venomous tumified toads, or extracting a royal diadem "from the menstrual blood of our whore" (p. 130)? The most influential interpretation of this imagery has been by the psychologist Carl Jung, who denied that they have any chemical meaning at all. Under Newman's analysis, however, Jung's interpretation has no historical basis (a conclusion that Newman and Principe have argued forcefully elsewhere). When properly decoded and placed into context, the Philalethan texts reveal an undeniably coherent doctrine of chemical practice and theory. Newman's analysis of the Philalethan texts also reveals Starkey's thoroughly corpuscular (yet vitalistic) matter theory, unique to Starkey, but clearly derived from Helmontian vitalism, Harvard matter theory, and a long tradition of corpuscular matter theory dating to the thirteenth century.

In the persona of Philalethes, Starkey proved to be influential long after his premature death in the 1665 plague of London. Most notably, Isaac Newton clearly read Philalethes closely, referring to him more times than any other alchemist in his *Index chemicus*. The published versions of Newton's matter theory, including his concept of "mediation," and "sociability" seem drawn principally from Philalethes. Newton's own "shell theory" of matter, described at length in the 31st Query of the *Opticks*, resembles closely Starkey's own particulate matter theory, and Newton expressed his theory in terms that could only be drawn from Philalethes. As Newman himself admits, Starkey's influence on Newtonian *mechanics* is still under dispute, but it seems that Newtonian *matter theory* was almost certainly shaped by Newton's reading of Philalethes and other alchemical tracts.

Newman's primary purpose in *Gehennical Fire* was a biographical study of Starkey and his chymistry to place him firmly in the theoretical tradition of western alchemy. In *Alchemy Tried in the Fire*, Newman and Principe provide a detailed discussion of Starkey's actual chemical *practice* as revealed in preserved laboratory notebooks, and place Starkey's work more broadly in context, demonstrating the influence of Starkey on Robert Boyle and later generations of chemists. Prevailing historiography has tended to make a sharp divide between Starkey/Philalethes, the last of the alchemists, and Boyle, the first of the "new chemists." As Newman and Principe make abundantly clear, such a distinction is essentially non-existent. Their argument is based on rich contextualization of Starkey's theory and laboratory practice with past alchemical practice, a detailed analysis of Starkey's notebooks, uncovering the role of chymistry within in the Hartlib circle, and the subsequent fate of Helmontian and Starkeyian principles.

Boyle met Starkey sometime in early 1651, shortly after Starkey's arrival in London. Starkey was an experienced chrysopoetic alchemist and dedicated Helmontian iatrochemist, from the provinces of the New World and of humble origins. Boyle was the author of moral and devotional treatises, with as yet little or no interest in natural philosophy, from the cosmopolitan centers of England, and born into fabulous wealth. But almost immediately upon their meeting, Starkey was tutoring Boyle in chrysopoetic alchemy and sparking Boyle's interest in natural philosophy. While Boyle would soon eclipse Starkey in fame as a natural philosopher and the founder of the "new" chemistry, Starkey would be all but forgotten as a major influence on Boyle, or as a significant natural philosopher of the seventeenth century. Starkey's rapid fall into obscurity is probably related to the impenetrability of the Philalethan texts to later readers, but more importantly, Boyle himself suppressed Starkey's influence on his chemical thought, and in his published works was silent about nearly all the sources of his natural philosophy. The reasons for this are complex, and rest in part on Boyle's own disingenuous public claims that chymical experiments would be "subordinate" to natural philosophy and provide examples for demonstrating the validity of the new natural philosophy. Yet Boyle drew much of his mechanical philosophy from the chymical tradition, including Starkey and the chymist Daniel Sennert, whose ideas Boyle also seems to have adopted while intentionally not acknowledging his influence. This practice was not uncommon in chymistry, natural philosophy, or literature of the period, in which authors freely "borrowed" works of other authors with the aim of improving on them (the practice of *imitatio*). Boyle's intentional suppression of his sources has subsequently led to the dominant and erroneous, historiographic position that Boyle provided a radical break from the "old" chemistry.

When Boyle's sources are revealed, he emerges as a chymist situated in a long theoretical and practical tradition dating to the earliest appearance of western alchemy. This tradition, as portrayed by Newman and Principe, contrasts sharply with the stereotypical image of the alchemist, who is concerned only with his own spiritual perfection, and who certainly did not regard quantitative experimentation as important. In contrast, medieval alchemists, particularly in the metallurgical and mineralogical traditions, were greatly concerned with testing and assaying materials, using specific tests, purification procedures, and exact measurement, including gravimetric analysis. Jean Baptiste van Helmont has been well known for his antipathy towards the use of mathematics in natural philosophy, yet close study of his chemical works shows that he inherited this traditional alchemical concern for quantitative measurement, including the concept of conservation of weight and matter. Expanding on another medieval tradition, van Helmont stressed the importance of the concept of "spagyria" in chymistry, meaning the laboratory separation of bodies into their components and their subsequent recombination (in later terms, analysis and synthesis). By singling out spagyria as a central method in chemistry, van Helmont then required precise gravimetric methods and the concept of mass balance that would allow him to determine when separation and combination had occurred.

The *practice* of Helmontian alchemy is vividly illustrated in Starkey's preserved laboratory notebooks, which provide a rare glimpse into the day-to-day activities of seventeenth century chymistry. Not at all what one would expect from the stereotypical alchemist, Starkey's notebooks are "orderly, methodical, and formalized" (p. 154). They allow a detailed reconstruction of Starkey's methods and his day-to-day work in the laboratory. The notebooks reveal that Starkey would systematically make conjectures about the materials and processes given in alchemical works and then test those processes in the laboratory. That is, Starkey was attempting to decode alchemical works by testing them in the laboratory, and if a conjectured process did not work, Starkey would devise another plausible interpretation and test it. Like the canonical figures in the Scientific Revolution, Starkey was subjecting claims from the chymical world to extensive empirical test. And like van Helmont, Starkey also used careful gravimetric techniques and the concept of mass balance to determine whether a given procedure was correct. Starkey never suspected, however, that the information given in his sources could possibly be wrong. If his conjectured process did not work, it must be his interpretation, and not the text itself that was in error. Starkey therefore remained firmly in the world of seventeenth century alchemy—he was certain that alchemical authors were true adepti and that their encoded secrets were facts of nature that he could replicate if only he interpreted their texts correctly.

The notebooks are remarkable in several other respects. They demonstrate Starkey's interest in commercial projects, and reveal that he fused his empiricism with the formal scholastic techniques of textual analysis and argument he learned at Harvard. They also show how Starkey dealt with the problem of secrecy and concealment in his sources. Authors would code their works in *Decknamen* (aliases), disperse pieces of a single preparation throughout the text, or omit a particular step in a process (known as syncope). Such techniques for keeping alchemical processes secret derived not only from the author's desire to keep such knowledge away from those unqualified to view it, but also from the expectation that the *reader* would enjoy puzzling out the codes and ciphers in the text. Finally, the notebooks also record Starkey's divine revelations, whose role was not to reveal knowledge by intense contemplation or fervent prayer, but by active work in the laboratory, in the Helmontian sense of "God sells secrets for sweat" (p. 201). Alchemical secrets might well be a *Donum Dei* (a gift of God), but those gifts would not arrive without actively working in the laboratory. Starkey's notebooks provide a unique insight in to seventeenth century chymical practice that Newman and Principe have not yet exhausted; they promise to publish a separate volume of transcriptions and translations of Starkey's notebooks and correspondence.

Within the Hartlib circle, Starkey quickly became one of the most sought-after members of the group. After Newman and Principe's discussion, there seems little doubt that Boyle learned chymistry from Starkey, and although Boyle's later chymical theories were thoroughly mechanical, they bear the strong imprint of Starkey and van Helmont. Starkey's influence can be traced further to Lavoisier, whose quantitative methods date to his earliest notebooks from 1764 on the study of gypsum. Indeed, there seems to be a continual development of quantitative measurement from van Helmont to Lavoisier, via Starkey and Wilhelm Homberg (1652-1715). Homberg, located at the Academie Royale des Sciences in Paris, had worked with Boyle and made use of the quantitative techniques developed by Starkey. A 1700 paper by Homberg on solubilities of metals in acids reveals processes identical to those in Starkey's chymistry (although like Boyle, Homberg is silent about Starkey as his source). Homberg therefore provides an important link from English chymistry to eighteenth century French chemistry. As Newman and Principe conclude, the recent claim that Lavoisier "borrowed" quantitative methods from physics to reform chemistry in the 1770s appears inaccurate. Lavoisier learned quantitative techniques that already had long tradition in chemistry.

Taken together, *Gehennical Fire* and *Alchemy Tried in the Fire* strongly suggest two major revisions in our understanding of early modern chymistry. First, Jung's interpretation of alchemy as a spiritual activity, so predominant in current historiography (popular and scholarly), is clearly false. Alchemists had a coherent corpuscular matter theory that was grounded in laboratory experiment, and were interested in quantitative measurement. Alchemical texts, although written in codes and metaphors, describe real chemical theories, materials and processes. Starkey's notebooks reveal a methodology that has more in common with the well-known emerging methodologies in physics in the seventeenth century than with the stereotypical alchemist, and Starkey himself (as well as van Helmont, Starkey's major influence) emerges as a major figure in the formulation and transmission of chymical knowledge in the seventeenth century. Second, if the assumption of mass balance and the use of quantitative measurement are pushed back to at least the early seventeenth century, our understanding of the Chemical Revolution must be revised. Certainly, Lavoisier's contribution is significant, but traditional historiography has emphasized his use of quantitative measurement and the concept of mass balance as the keys to the new chemistry. If these two characteristics actually date to van Helmont (and perhaps even earlier), and Newman and Principe make a very strong argument that they do, then what is Lavoisier's true contribution? Lavoisier was certainly important and pivotal, but not quite as "revolutionary" (at least in his methods), as we are accustomed to thinking. *Peter J. Ramberg, Division of Science, Truman State University, Kirksville, MO 63501.*

Ostwald's American Students. John T. Stock, Plaidswede Publishing, Concord, NH, 2003, ISBN 0-9626832-9-9, hardcover; xiv + 193 pp.

John T. Stock, Professor Emeritus, University of Connecticut, has devoted a major portion of his retirement years to a project that has culminated in the publication of this book: an account of the activities of Wilhelm Ostwald's chemistry students who were born in America or migrated there. The author's approach has been unwaveringly persistent and thorough. First, he identified the nature of each student's graduate thesis work and placed it in perspective with regard to contemporary practice and theory. Then, where the information was available, he went on to describe the ensuing professional activities of each individual as he embarked on a career in the U.S. This historical research has demanded untold hours of searching and reading and a command of the theoretical and experimental details unique to each research project. The biographical sketches, averaging 6-8 pages each, present a chronological Leipzig roll call of familiar and less well known names: M. Loeb, A. A. Noyes, H. Goodwin, W. L. Miller, W. D. Bancroft, O. F. Tower, L. Kahlenberg, A. J. Wakeman, T. W. Richards, J. L. Morgan, F. B. Kenrick, W. R. Whitney, E. Sullivan, J. E. Trevor, S. Bigelow, A. Blanchard, E. Buckingham, H. C. Jones, F. A. Lidbury, G. A. Hulett, F. W. Skirrow, H. W. Morse, and F. G. Cottrell. Most, but not all, earned the D. Phil. directly under Ostwald or one of his assistants. The book is replete with reproductions of individuals, their laboratories, and equipment and with tables and equations to describe their research. The book opens with introductory material on Ostwald and Nernst and ends with a brief "The Years Beyond." Many of the sketches have been presented by the author at national American Chemical Society meetings before the History of Chemistry Division and published in the *Bulletin for the History of Chemistry* or *Chemical Intelligencer*. It is sometimes stated—with a burst of oversimplification—that "All physical chemists ultimately trace their roots back to Ostwald." From the ambitious undertaking represented by this book, one can appreciate that many of the Americans, indeed, are Ostwaldians. *Paul R. Jones, University of Michigan.*

Herman Boerhaave (1668-1738), Calvinist Chemist and Physician. Rina Knoeff, Koninklijke Nederlandse Akademie van Wetenschappen, Amsterdam, 2002, xvi + 213 pp, ISBN 9-6984-342-0, $35.

Chemistry and medicine practiced according to Calvinist doctrine, the underlying theme of this book, is based upon the Ph.D. dissertation of Dr. Knoeff, who identifies her research mentor, Andrew Cunningham, but not the institution where she earned the degree. Her thesis is clearly stated and repeated throughout the book: that the Dutch physician, Herman Boerhaave, whose life spanned half of the 17th and 18th centuries, was motivated by his devout Calvinism, not only in his religious practice but in his approach to chemistry and medicine. His lifelong goal was the search for truth through natural philosophy (chemistry, physics, and medicine) by aligning himself to the will of God, as he understood it.

The basis for the historical study is a rich source of literature, most importantly the three dozen or so original writings of Boerhaave composed, for the most part, in Latin. It is not clear whether the author read these works in the original or only those that have been translated. The bibliography includes over 20 of Boerhaave's manuscripts residing in the Library of the Military Medicine Academy, St. Petersburg. The author had to rely on an inventory of these manuscripts published in 1959 by B. P. M. Schulte, for she was refused permission to examine the St. Petersburg collection in detail. Microfilmed copies of some of these manuscripts, available in the University of Leiden, served as original material. Another important source was the three-volume set of Boerhaave's correspondence, edited by Lindeboom. Other works from the period include original writings of Calvin, Newton, Locke, Spinoza, and Stahl. Secondary sources from the 20th century number over 200.

An introductory section provides the religious setting, with its strong Calvinist foundation, in the Netherlands as Boerhaave's career began. This is followed by four major chapters: I. Herman Boerhaave: Spinozist? II. H.B.: Calvinist; III. H. B.: Calvinist Chemist; and IV. H. B.: Calvinist Chemist and Physician: and a brief conclusion.

In Chapter I we learn of Boerhaave's strict upbringing by a Dutch Reform minister father. By age 11 he was skilled in communicating from Latin to Dutch and vice versa. He began theological studies at age 15 at the University of Leiden, where he was exposed to Cartesianism and Spinozism, topics definitely not sanctioned by the devout faculty. His education culminated in a thesis, "Distinction between mind and body" (*De distinctione mentis a corpore*). The author's answer to the chapter title is negative, that, while Boerhaave was willing to listen to diverging points of view on nature, he never deviated widely from conventional theology and so could not be labeled a Spinozan.

Boerhaave the Calvinist is depicted vividly in Chapter II. Although described as an "average" rather than "extreme" Calvinist, he nevertheless lived an exemplary life of humility, introspection, prayer, and rigid lifestyle based upon daily reading of the Scriptures. In his orations, he makes it clear that true knowledge lies in creation and not with man himself. This attitude thus shapes Boerhaave's approach to chemistry, which is covered in Chapter III. Herman's first exposure to chemistry, as it existed in the 17^{th} century, began with experimentation he carried out with his brother Jacob as part of the latter's medical studies. Even after he embarked on the study of theology, Herman continued chemical experiments. Much of his chemistry is manifested in a 1732 publication, *Elementa chemiae*, his authorized version, which had been preceded by an English text by Shaw, who had "de-Calvinized" Boerhaave's chemistry. By 1718, when Boerhaave accepted the chair of chemistry at Leiden, he had described a "reformed" chemistry, which he presented in his inaugural address. Among practices of unreformed practitioners, he mentioned misuse of chemistry in medicine and the misreading of "chemistry" in the Bible. He distinguished between "true" alchemists, whom he respected, and "vagabond" alchemists, often iatrochemists and fake gold makers, who wrote in an obscure style so as to keep their findings mysterious. By contrast Boerhaave believed in making observations objectively, always with the conviction that man could only approach nature's truths but never achieve a full understanding of them. He opined that chemistry, of all fields of natural philosophy, was best adapted for improving natural knowledge. Unfortunately, Boerhaave's chemical experiments from 1718-1735 remain buried in his manuscripts in the St. Petersburg library. The author has highlighted some of his experiments: purification of mercury, heating of a vessel of lead for 20 years; attempt at the fire-induced transmutation of lead into mercury (published in the 1730s in two articles in the *Philosophical Transactions of the Royal Society*). In spite of Boerhaave's hesitancy about explaining his observations—a presumptuous suggestion that man could fully comprehend God's creation—he nevertheless assembled a set of theories. All matter contains *spiritus rector*, in however minute amounts; alcohol is the principle of inflammability; air contains a hidden virtue, without gravity. Perhaps best known is his theory of fire, present in all bodies and the instrumental cause of all motion. This is reflected in his definition of chemistry (chymistry), which Samuel Johnson used in his dictionary: "an art whereby sensible bodies contained in vessels...are so changed, by means of certain instruments, and principally fire, that their several powers and virtues are thereby discovered, with a view to medicine or philosophy."

The author's goal in the last chapter is to offer evidence that Boerhaave's approach to medicine through chemistry was not merely mechanistic, as proposed by earlier biographers, but centered around the discovery of latent powers in nature. Boerhaave was largely self taught in medicine, unlike his training in theology. In a two-year period in Harderwijk he "bought a medical degree," a common practice of the time. He attended no lectures and had scant experience dealing with the sick but rather studied medical texts and the work of Hippocrates on his own. Once he took up medical practice in Leiden, he continued his self-education. His concept of medicine went through three phases in his career, starting with a Cartesian, then Newtonian approach, and finally, what the author calls "chemical," whereby Boerhaave considered the body to be a machine, with a specific role for each body part. His *in vitro* experiments were directed toward observations on the behavior of the humors, blood, urine, milk, and other fluids. With his theory of menstrua in mind, he monitored the effect of diet on urine and observed the coagulation of blood with alcohol. From the latter experiments, he concluded that the ingestion of alcohol might be related to hemorrhaging and brain damage. Firmly holding to the seminal principle, he considered that each form of life regenerated itself through its unique seed. This included not only animals and plants but also metals. For gout and venereal disease, Boerhaave prescribed trace amounts of mercury, "God's most wonderful creation." It is recorded that Boerhaave always accompanied a prescription with a prayer for divine blessing of his endeavor—a fitting gesture for a devout Calvinist.

The subject of this book is heavy going for one not specializing in 17^{th}-century alchemy or iatrochemistry and is made somewhat difficult by what appears to be lack of rigorous organization of the topics. Dozens of misspellings, syntactical errors, and the errant misuse of commas distract the reader from concentrating on the flow of narrative. It is disappointing that the index is limited to proper names, for the inclusion of general subject entries would enhance the book's value as a reference source. This book is one of four published so far in a series, History of Science and Scholarship in the Netherlands, and is available in the U.S. through University of Chicago Press. *Paul R. Jones, University of Michigan.*

Cohesion: A Scientific History of Intermolecular Forces. John Shipley Rowlinson, Cambridge University Press, Cambridge, 2002; vii + 333 pp, Cloth, ISBN 0-521-81008-6; $90.

Dr. Rowlinson is best known to physical chemists for his work on the theory of fluids and especially for his classic monograph, *Liquids and Liquid Mixtures*, first published in 1955 and revised several times since. Now Dr Lee's Professor of Chemistry Emeritus at Oxford University, Rowlinson has crowned a distinguished research career by writing a detailed technical history of the field in which he has made so many important contributions. Though the subject of intermolecular forces is relevant to virtually every branch of the physical sciences, its history is curiously underrepresented in most standard histories of chemistry and physics and it is a pleasure finally to have a comprehensive historical account.

The book is divided into five chapters: an introductory summary, followed by three chapters dealing with contributions stemming from the seminal work of Newton, Laplace, and van der Waals, respectively, and a concluding chapter dealing with the impact of modern quantum statistical mechanics and its role in resolving many of the long-standing problems associated with classical theories of cohesion. Though much of this material was touched on in Stephen Brush's 1983 volume, *Statistical Physics and the Atomic Theory of Matter: From Boyle and Newton to Landau and Onsager*, Rowlinson's account is more limited in its scope and hence more detailed.

I cannot praise this book enough. Though thoroughly familiar with the secondary history of science literature, Rowlinson has chosen to construct his story largely from the primary sources, thereby bypassing most of the petty squabbles, questionable interpretations, and knee-jerk revisionism that have enervated so much of the current history of science literature. His writing style is urbane and witty, and his knowledge of his subject unexcelled. Nevertheless, this is not a book for the faint of heart when it comes to making demands on the reader's knowledge of both physical chemistry and mathematics. In short, it is the kind of high-level technical, concept-oriented history that has virtually disappeared from the professional history of science literature since it fell into the hands of the social relativists. It is a pity that more professional scientists do not have the vision and the breadth of interest to write similar accounts of their own fields. *William B. Jensen, Department of Chemistry, University of Cincinnati, Cincinnati, OH 45221-0172.*

Chemical Structure, Spatial Arrangement. The Early History of Stereochemistry 1874-1914. Peter J. Ramberg, in *Science, Technology and Culture, 1700-1945,* David M. Knight and Trevor H. Levere, Ed., Ashgate Publishing Company, Burlington, VT, 2003. xxiv + 399pp, $99.95.

Stereochemistry pervades all of chemistry. A number of textbooks have been written on the subject, the most recent (*Basic Organic Stereochemistry*) being only two years old. In addition, a number of historical accounts were produced on the occasion of the van't Hoff-Le Bel centennial in 1974. However, except for two books by O. B. Ramsay and G. V. Bykov and a collection of historical essays by Ramsay, most of these accounts were written by experimental chemists rather than chemical historians (and so is this review). The points of view of these two categories of authors tend to be substantially different: an experimentalist looks at the state of the subject as it is today and then tries to unravel the historical threads that have led to our current knowledge and insight; a historian, in contrast, is more likely to look at the very beginnings of the subject (however defined) and to analyze its subsequent development. The latter is the approach taken by Ramberg.

The work of experimentalists must be judged primarily by the quality of their experimentation and only secondarily by the cogency of their explanations. And so, the work of historians should, presumably, be judged, first, by their thoroughness in uncovering and mining relevant sources, and only after that in terms of the historical connections and interpretations they provide. On the former score I would give the author high marks. He has not only extensively consulted primary and secondary literature sources by and about the principals of his account, but has also often uncovered interesting and illuminating private correspondence and lectures of these individuals. If I have any criticism here, it is about the almost exclusive use of German sources.

The introductory chapter, "Van't Hoff's Gold Mines," foreshadows the author's greater regard for van't Hoff as compared to Le Bel; more about this later. Here the author lays out his planned development of the subject. By ending his account in 1914, with the development of inorganic stereochemistry by Alfred Werner, he deliberately omits the third aspect of three-dimensional structure, the all-important subject of conformation. Perhaps a more felicitous endpoint would have been 1950, when D. H. R. Barton gave final shape to this subject.

The extensive second chapter (42 pages) deals with the historical development of organic chemistry prior to 1874. The historian of chemistry will find much of this material familiar. The author includes significant discussions of the laboratory practices and of the organization of chemical institutes. The culmination of this chapter relates to the development of the concept of constitution (connectivity)—first hesitatingly in the writings of Kekulé and Butlerov and then more clearly with Couper, Loschmidt, Crum Brown and Frankland. Yet, as Ramberg stresses, the constitutional formulas were largely "symbolic;" they expressed in the minds of the chemist the chemical behavior of the compounds in question, but did not necessarily have any bearing on the physical nature of the atoms in a molecule, which were believed to be in motion. It was van't Hoff's insight that, by explicitly disregarding atomic motion, gave the (by then 3-dimensional) formulas a physical meaning. In Ramberg's words the formulas changed from being "symbolic" to being "iconic," *i.e.*, they reflected chemical reality in the way a map represents a country.

I was surprised that Pasteur's work commands hardly more than two pages in Chapter 2, whereas Wislicenus' work (up to 1873) claims well over eight, even though his work on lactic acid, begun in 1859, ended inconclusively in 1873. In the process he seems to have influenced van't Hoff by isolating both enantiomers of lactic acid and by having explicitly ascribed the difference to the arrangements of the atoms in space. However, both events were anticipated by Pasteur years earlier when he obtained (+)- and (-)- tartaric acid in 1848, and by his conjecture, in his 1860 lecture, that their difference in rotation was due to molecular dissymmetry. (He mentions helices and even an irregular tetrahedron as examples of such dissymmetry but, absent from his horizon the just proposed ideas of atomic connectivity, was unable to be more detailed in his explanations). It is not clear whether Wislicenus was aware of the details of Pasteur's work, but van't Hoff clearly was (see "Dix Années...), and so, of course, was Le Bel.

Chapter 3 deals with van't Hoff's (curiously spelled with a capital V) initial work. Ramberg asserts that Le Bel's paper might have drifted into obscurity but for the impetus van't Hoff gave it in his many reviews, but acknowledges that the same might have happened to van't Hoff's own 1874-1875 publications. Perhaps so; for original papers to become known, it is important for the author to summarize them in review journals and book chapters. Van't Hoff did so in 1877, 1887 and later and used these occasions to expand his own horizons. (In contrast to Pasteur—see below—van't Hoff kept careful track of the stereochemical literature, even after his interests had changed to other areas.). Le Bel, though continuing to do original work in stereochemistry for some 20 years, never wrote a review. Yet, to be fair, one should compare the original 1874/75 papers: Le Bel clearly explains the existence of meso as well as chiral tartaric acid, which had been mysterious to Pasteur who spoke of an "untwisted" molecule; van't Hoff is vague on this point in 1874. Also, Le Bel clearly recognized in 1874 that synthesis of a chiral (today's nomenclature) compound from an achiral one yields a racemate *except when carried out in the presence of another "asymmetric body" or traversed by circularly polarized light.* This is a prediction of enantioselective synthesis and of the absolute asymmetric synthesis carried out by Kagan and by Calvin only 100 years later. Le Bel's failing was his scientific caution. He was not willing to commit himself, in the absence of clear evidence, to the valences (bonds) to carbon being tetrahedrally arranged, nor to the planar geometry of olefins. Here van't Hoff was clearly more successful. By empirically assuming carbon to be tetrahedral (as represented by his cardboard models) he was able to understand the *cis-trans* isomerism in olefins and the optical isomerism in allenes. The former prediction proved crucial. In the 1870's, as today, interest in optical rotation was limited; chemists' main interest was in explaining isomerism and van't Hoff's explanation of, *e.g.*, the isomerism between maleic and fumaric acid greatly enhanced the impact of his paper over the next few years.

Chapter 4 deals with the reception of the tetrahedron, 1874-1887. Of most interest here are Kolbe's oft recited violent critique (possibly helpful, because by then Kolbe was known to be an old curmudgeon) and Wislicenus' vigorous, and as it turned out crucial, support, which led to a German translation of van't Hoff's article with Wislicenus' introduction. Support from other sources tended to be lukewarm, perhaps because most contemporaries were experimentalists and thus leery of van't Hoff's purely theoretical paper. Wurtz (who had hosted both chemists) indicated "attention and interest" in the work and devoted some 41/2 pages to it in his 1881 book, *The Atomic Theory*. But shocking (though not mentioned by Ramberg) is the apparent total absence of a reaction from Pasteur. Even more shocking: Pasteur, in his 1883 lecture to the Chemical Society of Paris, barely mentioned Lebel (*sic*) and not at all van't Hoff. (The lecture is centered on Pasteur's fixed idea that optically active compounds are found only among natural products or compounds derived from them.)

The fifth chapter deals with Wislicenus' extensive work on olefin stereochemistry at the end of the 1880's. Unfortunately, Wislicenus misinterpreted the stereochemical implications of his (and others') experiments. Misled by the unquestioned *cis* addition in permanganate oxidation of maleic and fumaric acids (which yields, respectively, the known meso and racemic tartaric acids of established configuration), Wislicenus generalized that addition to olefins was *cis* and the reverse elimination of the disubstituted ethanes *syn*. Since the result of a *cis* addition followed by a *syn* elimination is the same as that of a *trans* addition followed by an *anti* elimination, configurational assignment to the two-step products was often correct, but assignment to the intermediate saturated compounds wrong (as was the assignment of the *trans* configuration to the liquid isocrotonic acid). One might have wished for a briefer treatment of the multitude of these reactions in favor of a more succinct summary of the important principles established by Wislicenus: 1) Although van't Hoff had assumed free rotation about single bonds (in order not to predict a surfeit of stereoisomers), Wislicenus hypothesized (three years before C. A. Bischoff) that some arrangements (that we now call conformations) are more stable than others or, at least, come into play in the course of elimination reactions (unfortunately, unlike Bischoff later, Wislicenus used erroneous principles in deciding which conformations were the salient ones); 2) Wislicenus was probably the first to postulate that the steric course of a nontrivial reaction could be used to correlate configurations (by postulating what we would now call the mechanisms of addition and elimination). (This approach was strenuously opposed by Arthur Michael on experimental grounds, but Michael had no theory to undergird his experiments.); 3) It also appears that Wislicenus' long 1887 paper, "On the Spatial Arrangement of Atoms in Organic Molecules..,." perhaps along with van't Hoff's publication of "Dix Années..." the same year, finally put van't Hoff's ideas over the top.

Chapters 6 and 7, referring to the work of Victor Meyer and Arthur Hantzsch, respectively, will be considered together, since they deal mainly with the stereochemistry of oximes. (However, Victor Meyer is perhaps best remembered as the originator of the term "stereochemistry".) He and his student Auwers (who, some 35 years later, corrected the configuration of the crotonic acids mentioned above) first isolated the three isomers of benzil dioxime. After convincing themselves that they were not *constitutional* isomers (not a trivial task before the arrival of spectroscopy and crystallography), they proposed a stereochemical explanation based on restricted rotation about single bonds. This explanation soon yielded to the correct one by Hantzsch and his student Alfred Werner, who ascribed the isomerism to *cis* or *trans* arrangement about the C=N double bonds, similar to that postulated by van't Hoff and corroborated by Wislicenus in olefins. Unfortunately, Hantzsch also believed in *syn* elimination (and an analogous *syn* migration in the just discovered Beckmann rearrangement), and so all oxime configurations were misassigned until Meisenheimer straightened out the situation in 1921. Not surprisingly, the absence of stereoisomerism in NRR'R" and the assumed pentavalence of nitrogen in ammonium salts caused a fair amount of intellectual confusion in those days.

The last two historical chapters, on Emil Fischer and on Alfred Werner, are probably the best in the book. Some aspects of Fischer's brilliant researches on the configuration of the sugars are, of course, well known; but Ramberg's chapter chronicles that there is much more: *e.g*. synthesis of racemic sugars by condensation of 3-carbon fragments and synthesis of L-sugars. And Fischer based his important "lock-and-key" hypothesis of enzymic action on a series of systematic fermentation experiments with sugars. Ramberg does not deal with Fischer's later work on amino acids and peptides. The chapter on Alfred Werner gives a very clear and easy to follow exposition of his pioneering work on the structure and stereochemistry of metal coordination compounds and ends with his first resolution of a purely inorganic complex. Some of this material had already been discussed earlier by George Kauffman.

The last chapter, "Conclusion," in addition to providing a summary, deals largely with the historical and epistemological aspects of the progress of chemistry in the second half of the 19th century. It brought home to me the realization that a historian of chemistry might have written a review quite different from mine! There are five appendices, translations of interesting letters and of Wislicenus' foreword to "Die Lagerung der Atome im Raume," which I found useful. So is the following bibliography, but less so the rather scanty index. This book should interest not only historians of science but anyone concerned with stereochemistry and its early development. It may also be used as a source of stereochemical problems. Thus a chemistry undergraduate interested in literature search might go through all of Wislicenus' configurational assignments, try to decide which are correct in the light of modern mechanistic insight, and then check whether they have been corrected in later investigations. Although the book is, in spots, densely written (since it brings together chemical, biographical, historical, and philosophical material), I found it interesting and stimulating reading. *Ernest L. Eliel, Department of Chemistry, University of North Carolina, Chapel Hill, NC 27599-3290.*

Chemical Discovery and the Logician's Program: A Problematic Pairing. Jerome A. Berson, Wiley-VCH Verlag, Weinheim, Germany, 2003; 194 + xiii pp, ISBN 3-527-30797-4, $ 55.

Chemical Discovery and the Logician's Program is a welcome addition to the literature of philosophy and history of science from the perspective of a thoughtful practitioner of chemistry. Its author, Jerome Berson, Professor Emeritus at Yale University, has more than 50 years of experience in organic chemistry. Not surprisingly, then, organic is the branch of chemistry from which he draws his historical examples. Berson's philosophical concern is nothing less than scientific method, in particular its formulation by Karl Popper. The book is a collection of historical cases from organic chemistry analyzed in light of Popper's "conjectures and refutations" version of scientific methodology. As the subtitle "A Problematic Pairing" suggests, the correspondence between practice and methodology is far from perfect.

Berson outlines his aims and his audience (chemists) in an introductory chapter. In the next two chapters, he introduces two important philosophical positions on scientific method: induction and Popper's scheme of conjectures and refutations. The subsequent five chapters of case studies form the heart of the book. The chemical subjects described in considerable detail include Kekulé's benzene structure (Chapter Four), the slow and gradual recognition of the occurrence of rearrangements of the carbon skeleton in some organic reactions (Chapter Six), and useful but incorrect speculations on the biological synthesis of certain alkaloids (Chapter Eight). Several other episodes from 19th- and early 20th-century organic chemistry are treated in less detail in Chapters Five and Seven. Each case study includes both historical exposition and analysis in light of philosophical principles. A very brief summary chapter concludes the book.

Berson writes explicitly in the introduction that he is a chemist writing for chemists about philosophy and history of science. He wants to see whether philosophers have anything useful to tell chemists about the practice of chemistry, in particular anything that could help chemists in the conduct of research. He disavows any intent to engage in the latest philosophical debates on scientific method, as his focus on inductivism and Popperian falsificationism makes clear. Berson explains that the logicians' program for the philosophy of science is or was an attempt to formulate scientific methodology and to show that the practice of science corresponds to the methodology. His presentation of cases suggests that such a program is far from completely successful.

The book's focus on Popper's formulation of scientific method is appropriate, for, as Berson notes, Popper's ideas resonate with practicing scientists (and educators I would add)—certainly more than the ideas of Francis Bacon, Thomas Kuhn, Paul Feyerabend, or Imre Lakatos, to name other philosophers of science mentioned prominently in the book. Berson summarizes Popper's ideas clearly and fairly, and he mentions some philosophical critiques of those ideas as well before he begins the case histories.

Kekulé's benzene structure is the subject of the first detailed case history. Berson documents Kekulé's proposal of a cyclic structure with alternating single and double carbon-carbon bonds, a structure that would today be named cyclohexatriene. Kekulé also predicted the number of distinct isomers of monosubstituted, disubstituted, trisubstituted, etc., benzenes, namely only one chlorobenzene, for example, but three different dichlorobenzenes. Berson also noted a critique raised by certain former students of Kekulé's. The number of disubstituted isomers actually implied by Kekulé's cyclohexatriene structure is four. (Two substituents on adjacent carbon atoms could have a double bond or a single bond between them.) Berson explores several possible responses to this problem, for example alternative structures that preserve the tetravalence of carbon. What Kekulé did, however, amounted to keeping both the cyclohexatriene structure and the original isomer prediction and adding a hypothesis that the carbon atoms in the ring collided with their neighbors in a way that some individuals later interpreted as an oscillation of double and single bonds. Thus, Kekulé's original formulation contained a contradiction (concerning the number of isomers predicted) that he subsequently attempted to resolve by means of a highly speculative *ad hoc* hypothesis. Needless to say, neither internally contradictory theories nor theories with *ad hoc* hypotheses have high standing in Popper's scientific method; nevertheless, organic chemists in Kekulé's day failed to regard the benzene structure as refuted.

The historical subject of Chapter Six is the eventual recognition of molecular rearrangements in organic reactions, in particular the pinacol and benzilic acid rearrangements. The main philosophical point arising from these detailed histories is that prevailing theories influence the interpretation of experiments, which, in Popper's method, could potentially refute those theories. Here the rule of minimal structural change helped prevent experimenters from recognizing rearrangements when they occurred. In the end, the fact that rearrangements sometimes occur does not so much refute the rule of minimal structural change as limit its applicability. Berson also uses these detailed cases to discuss the question of what constitutes a scientific discovery.

The last and longest historical chapter details certain early 20th-century investigations of the complicated chemistry of alkaloids. Berson's description provides many specifics on structure determination and synthesis of members of this class of plant bases. It dwells particularly on strychnine and on two giants of the field, Robert Robinson and Robert Burns Woodward. As a student in Woodward's laboratory, Berson was a witness to some of this history, and he offers some insights into the personalities of Robinson and Woodward. This account is much more than Berson's philosophical purpose requires; however, it stands on its own as recent chemical history. The philosophical point of the chapter is to raise the question of what to make of a theory, eventually refuted, which nonetheless proved to be fruitful and predictive. The theory in question is a speculation made by Woodward and endorsed by Robinson about the mechanism of biosynthesis of a group of alkaloids.

Chapters Five and Seven include less historical detail. Chapter Five examines the logical status of experimental refutations, or falsifications as they are sometimes called, in two historical instances. One involves nonvicinal hydrogen shifts, which were proposed as part of a mechanism in the racemization of camphor derivatives. The proposed shifts were eventually shown not to occur. But the proposal was resurrected in mechanisms of other reactions, despite its refutation in camphene racemization. The other instance involves the demise of a proposal that enzymes are small molecules associated with proteins rather than proteins themselves. The small-molecule hypothesis lost support after experiments that logically did not refute the hypothesis, but only rendered it less likely than had been previously thought. In Chapter Seven, Berson examines scientific efforts directed toward aims other then the refutation of theories. The exploratory phases of new fields of investigation certainly seem to resemble classical induction more than Popperian conjectures and refutations—at least until enough observations have been gathered about which conjectures can be made. Berson also identifies organic synthesis as an area that appears to be antithetical to falsificationist methodology. As he notes, a failed synthesis refutes nothing, but a successful synthesis is a powerful corroboration of its design.

Consider once more the case of benzene structure, because it is the most familiar and the most accessible case Berson treats in detail. His exposition and analysis carefully distinguish between concepts and critiques from Kekulé's time, explanations of those concepts and critiques in language 21st-century chemists understand, and later interpretations both of Kekulé's hypotheses and their underlying phenomena. I followed Berson's arguments point by point and agreed with his conclusion that the chemical community acted contrary to Popper's description or prescription of scientific method in this matter. Yet I was unsatisfied in the end. Surely Popper was right to disapprove of internally contradictory theories; but at the same time, there ought to be room for "transitional" theories, ones that despite loose ends represent a significant advance over available alternatives. In this chapter and in subsequent chapters, Berson demonstrates contradictions between method and practice, and at least implicitly endorses most of the practices analyzed; however, he makes little or no effort to improve the prescription or description of the method. He clearly states that the book was to confront certain philosophical propositions with selected instances of scientific practice, not to offer philosophical alternatives. As he correctly points out, philosophers have treated the question of scientific method for a long time without arriving at satisfactory answers.

In the end, does philosophy of science have anything useful to tell practicing chemists? Berson certainly does not present any foolproof philosophical advice for scientists. It is clear, however, that he thinks scientists and philosophers can both benefit from interacting.

I hope the book finds a readership among philosophers of science, even though the chemistry described in it presents a formidable obstacle. (I mean no disrespect to philosophers; some of the organic reactions made for pretty slow going for this physical chemist!) The cases Berson describes can provide useful data to philosophers interested in constructing or refining formulations of scientific method. As he points out, philosophical treatments of science tend to focus on physics, astronomy, and biology rather than chemistry. In this book, then, he brings selected philosophical ideas into contact with a relatively unexplored area of science. Furthermore, Berson raises several points that deserve further philosophical scrutiny, in my opinion. I mentioned one in the previous paragraph. A second example is the fact that theories often have provisos, stated or unstated, attached to them. It seems to me that chemistry is a field in which provisos about possibly confounding conditions are important (more so than in physics) but usually manageable (more so than in medicine, for example, or the social sciences, with their small sample sizes and large individual variability). *Carmen J. Giunta, Le Moyne College, Syracuse, NY 13214-1399.*

INSTRUCTIONS FOR AUTHORS

Articles of 4-12 pages, typed, double-spaced (excluding references) should be submitted as hard copy (2 copies) and also electronically in Word or WordPerfect on diskette or CD, to **Editor, Bulletin for the History of Chemistry**, at the University of Michigan. Chemical formulas, to be kept to a minimum, should be computer-generated and printed on separate sheets, with a clear indication of their location in the ms. Authors are encouraged to provide photographs (black and white glossy prints) and drawings (black ink) to enhance the publication. Include a legend for photos, drawings, graphs and credits if appropriate. Diskettes, photographs, and drawings will be returned at the authors' request.

The title of the article should be of reasonable length (up to 15 words); a subtitle may be included if appropriate. Authors should strive to make the title descriptive of the specific scope and content of the paper, bearing in mind that the title will determine entries in the subject index. Subheadings within the paper may be used if authors feel their inclusion will enhance clarity.

Format as found in recent issues of the **Bulletin** should serve as a guide. Authors should consult J. S. Dodd, Ed., *The ACS Style Guide*, American Chemical Society, Washington, DC, 2nd ed., 1997. Format the document throughout with standard 1" left and right margins. Direct quotations, which in the journal will be indented, will be repositioned by the graphic artist.

REFERENCES AND NOTES should appear at the end as part of the main document and not buried as footnotes. References should conform to the format as illustrated below. Standard **Chem. Abstr.** abbreviations are to be used (see CASSI); title of the article is in quotes. Books and journals are italicized, as are volume numbers. The year of publication of periodicals (not books) is boldfaced. Inclusive page numbers are given for an article or partial section of a book. Note the placement of commas and periods. It is imperative to recheck the accuracy of references before submitting the ms. In the text references are identified by Arabic numbers within parentheses—no superscripts.

1. O. T. Benfey, "Dimensional Analysis of Chemical Laws and Theories," *J. Chem. Educ*., **1957**, *34*, 286-288.
2. G. W. Wheland, *Advanced Organic Chemistry*, Wiley, New York, NY, 1949.
3. J. R. Partington, *A History of Chemistry*, Macmillan, London, 1972, Vol. 4, 104-105.
4. L. P. Rowland, Ed., *Merritt's Textbook of Neurology*, 8th ed., Lea and Febiger, Philadelphia, PA, 1989.
5. K. A. Nier, The Emergence of Physics in Nineteenth-Century Britain as a Socially Organized Category of Knowledge. Preliminary Studies. Ph.D. Thesis, Harvard University, 1975.
6. J. B. Conant, "Elmer Peter Kohler," *Biogr. Mem. Nat. Acad. Sci. USA*, **1952**, *27*, 264-291.

Please provide a short biographical sketch, to be included as **ABOUT THE AUTHOR(S)** at the end of the article.

Readers are encouraged to submit short notes, where appropriate, and letters to the Editor, as well as articles. We would welcome hearing from those who have an interest in refereeing papers and/or preparing book reviews.

BULLETIN FOR THE HISTORY OF CHEMISTRY

William B. Jensen, Founding Editor

The BULLETIN FOR THE HISTORY OF CHEMISTRY (ISSN 1053-4385) is published biannually (February & September) by the History of Chemistry Division of the American Chemical Society. All matters relating to manuscripts, book reviews, and letters should be sent to Dr. Paul R. Jones, Editor. Subscription changes, changes of address, and claims for missing issues, as well as new memberships, are handled by the Sec./Treas.

BULLETIN FOR THE HISTORY OF CHEMISTRY

Subscription and Membership Application

____ ACS Member at $15/year (includes HIST Division membership)
____ Associates: $20/year
____ Associates outside North America: $25/year
____ Annual Library Subscription: $36/domestic; $40/foreign

Begin subscription with the year 20 __

Name ____

Address ____

Signature ____

Return to Dr. Vera V. Mainz, HIST Secretary/Treasurer, School of Chemical Sciences, University of Illinois, Urbana, IL 61801. Checks should be made payable to the Division of the History of Chemistry, American Chemical Society.

BULLETIN FOR THE HISTORY OF CHEMISTRY
Dr. Paul R. Jones, Editor
University of Michigan
Department of Chemistry
930 N. University Avenue
Ann Arbor, MI 48109-1055

Non-Profit Org.
U.S. Postage
PAID
Ann Arbor, MI
Permit No. 144