

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 adepts 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 introduc-

tory 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 ora-

tions, 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 17th 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 17th-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 clas-

sical 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 sec-

ondary 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 dis-

symmetry. (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 than 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.*

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