BOOK REVIEWS

Chemistry and Medical Debate: van Helmont to Boerhaave. Allen G. Debus, Science History Publications/USA, Nantucket, MA, 2001. 296 pp, ISBN 0-88135-292-6, $52.

For many years now Allan G. Debus has been a tireless worker in the history of chemistry. As he points out in this volume, when he first began to study the history of chemistry, it was unfashionable; and attention was mainly lavished on the physical sciences. His own choice of research was particularly obscure in those days. He studied Paracelsus and more significantly the legacy of Paracelsus in its European context. This was a topic familiar to German historians but was virtually unknown in North America. Debus made us alive to the fact that in the sixteenth and seventeenth centuries Paracelsian chemistry flourished in the courts of Europe and had strong associations with natural magic. It became clear from Debus' work and those that followed him that this tradition, obscure and strange though it may seem, played a very important part in the scientific revolution. Debus' studies also drew attention to the strong connections between early modern chemistry and medicine. Until recently Debus' scholarship has largely remained confined to the period in which he began his work. In this book he pushes the boat out and, building on his earlier studies, writes about chemistry and medicine from Paracelsus to Boerhaave. As usual, Debus' approach is to elucidate primary texts, and any reader looking for useful accounts of the chemistry of various figures in the era covered by this book can expect to find first-hand reporting rather than derivative repetition from secondary sources.

The first three chapters of the current work largely draw on Debus' earlier books, notably *The English Paracelsians* of 1965 and *Man and Nature in the Renaissance* of 1978. In these chapters he describes the chemistry and physiological concepts of Paracelsus and those of his defenders such as Peter Severinus. Debus takes great pains to stress the medical dimensions of Paracelsus' work; quite rightly too, since it was doctors rather than those with a commercial or industrial (if that is the word) interest in chemistry who found his concepts most valuable. As usual Debus is wide ranging, describing the diffusion of Paracelsus' texts in continental Europe and England. Chapter 2 is mainly devoted to Jean Baptiste van Helmont and the new chemical medicine. Consistent with Debus' approach, he not only outlines Helmont's ideas but details responses to them, many of them by scholars who thought his work worthless. Eschewing strict nationalist approaches, he deals in Chapter 3 with Sylvius and then a number of English chemists, including Thomas Willis and Robert Boyle. Much of this chapter is given to the controversy over the place of chemistry at the Royal College of Physicians, a debate that is now fairly well known from the work of Harold Cook and others. From here, in Chapter 4, Debus goes on to take up an intriguing subject that has also been the center of recent attention; that is the uses to which the ancient Hippocratic texts were put during the scientific revolution. Hippocrates is usually considered an empiric, little bothered by theory; but Debus discovers in the writings of the German-born Otto Tachenius a figure who found in the books attributed to the Greek physician the philosophy of what was then modern chemistry. After this Debus enters relatively new territory for him. He takes on the early eighteenth-century controversies between the iatrochemists and the iatromechanists, in particular looking at debates over digestion. Displaying his customary catholic interests, he draws attention to the writings of a number of Spanish Paracelsians who until now can only have been known to a very few modern scholars. Chapter 6 deals with chemistry and medicine in the early Enlightenment and again spans a European canvas, taking in Hermann Boerhaave and Georg Ernst Stahl. This book is not simply an eclectic compilation of writings from early chemistry. Its principal theme is to show that there was a continuous chemical tradition fuelled by Paracelsian origins even after they had been repudiated. This tradition, he rightly insists, had particularly strong links to medicine. As usual Debus writes clearly and punctuates his texts with numerous quotes from primary sources. No doubt scholars will disagree with many of his interpretations, but this will remain a most useful contribution of an understanding of chemistry in this period. For many it could form a valuable introduction to the subject. *Professor Christopher Lawrence, Wellcome Trust Centre for the History of Medicine at UCL, London.*

Nationalizing Science: Adolphe Wurtz and the Battle for French Chemistry. Alan J. Rocke, MIT Press, Cambridge, MA, 2001. xi + 443pp, Cloth, ISBN 0-262-18204-1. $42.95.

"Adolphe Wurtz [(1817-84)] lived a modest life and died a modest death," (p. 376), but he was perhaps best known for his immodest and controversial claim: "La chemie est une science française" which is inscribed on Wurtz's statue outside the church of Saint-Pierre-le-Jeune in Strasbourg, in whose parsonage he was born. What could Wurtz have meant by this claim? How did he view the relationship between French and German chemistry and how did his contributions to chemistry reflect the 'dialectics' between different national styles?

This is the third and most recent book from the pen of Alan J. Rocke, the 2000 Dexter Award winner, and one of the premier historians of 19th-century chemistry. Whereas his first book, *Chemical Atomism in the Nineteenth Century*, Ohio State University Press, Columbus, OH, 1984, gave us the history of a particular scientific concept, chemical atomism, this book, as well as his earlier one entitled, *The Quiet Revolution: Herman Kolbe and the Science of Organic Chemistry,* University of California Press, Berkeley, CA, 1993, are written in the genre of scientific biography. Rocke is convinced that it is not enough for someone to make a scientific discovery or promote a particular scientific theory such as the chemical structural theory of organic chemistry advanced by Wurtz. One must also make the world take note of them. Wurtz's world is that of Parisian science: a complex social-political-scientific network.

Rocke endeavors to take commonplace notions and repeated truisms about the state of 19th-century French chemistry and give them new life by 'contextualizing' the questions. To take one example: not only were laboratory facilities meager, a common reading of the state of French chemistry, but Rocke gives reasons why this may be so, and further shows how this fact had a debilitating effect on French organic chemistry in contrast to the laboratory support for German chemistry. This comparative advantage of German chemistry gave support to Wurtz's effort in the late 1860s in asking for more government support for research facilities.

To someone unfamiliar with the history of 19th-century chemistry, I would advise the reader to begin with the "Introduction" and especially the last chapter: "A Summing Up." This will give one a sense of the flow of the chapters in the book and provide some of the historiographical considerations for the structure of the book. Rocke's Dexter award address, "Celebrity Culture in Parisian Chemistry," in the *Bull. Hist. Chem.,* **2001**, *26*, 81-91, would also be beneficial to read.

In Rocke's hands Wurtz's life serves as the focal point for a much larger narrative: the development of French chemistry and its comparative (dis)advantage over German chemistry. Is it possible to tell a grand story, a macro-history that accurately reflects the critical intellectual, social, institutional, and material factors and themes, which are inevitably interwoven and interrelated without succumbing to a hagiographic one-dimensional story about an individual? Rocke invites us to evaluate his attempt.

The book's introduction details the difficulties and challenges faced by someone examining 19th-century French science. Take, for example, the supposed simple fact of determining a person's date of appointment. The details are frequently clouded by the institutional structure of *cumul* in which an individual could hold multiple appointments at different research institutions simultaneously. In the Parisian network there were indeed many such instances. At one point in 1845 Wurtz held positions in the Faculté de Médicine, the ?cole Centrale, and Dumas's private laboratory.

In rapid review, Chapters 1 and 2 present two of the leading lights of the chemical realm: Liebig and Dumas, both of whom played an important role in the career of Wurtz. The book's next four chapters focus on Wurtz. In Chapter 3 Rocke recounts Wurtz's education, his participation in Liebig's laboratory in Giessen, and his research on hypophosphorous acid. Chapter 4 locates Wurtz in Paris, finding his way through the politics of academic appointments as evidenced in the careers of Gerhardt and Laurent. The next two chapters, 5 and 6, describe Wurtz's research: cyanic esters, amides, primary amines, and his acceptance of the theory of types as well as his use of structural theory. Chapter 7, "The Campaign," recounts Wurtz's involvement with the *Bulletin* of the Société chimique, his research on glycol, lactic acid, oxalic acid, and the events surrounding the famous Karlsruhe Conference of 1860. In Chapter 8 Rocke presents further details of Wurtz's struggles with his principal Parisian rival, Marcellin Berthelot. The next chapter, 9, describes efforts to renovate laboratory science in France. "The Atomic War," namely, the struggle between using atoms or equivalents in chemistry and its aftermath, are described in great detail in Chapter 10.

The penultimate chapter gives us a glimpse of Wurtz in his "later years." Details are provided on Wurtz's efforts to convince his French colleagues to adopt the 'modern' atomic-structural point of view, by arguing that the flowering of contemporary German chemistry was a consequence of French seeds falling on fertile ground ("chemistry is a French science"); his mature research school; his political involvement; his role as dean of the Faculty of Medicine; and some of his family life.

Why did Wurtz not receive more acceptance (or win victory) for his arguments in favor of atomic weights, atomic theory, and structural theory by his contemporaries? And why have modern historians of science paid him so little attention? This is the subject of the concluding chapter, in which Rocke describes some of the broad cultural aspects of the French chemical community: its celebrity cult, its pedagogy, institutional structure, laboratory facilities, and the causes for the general decline of French chemistry.

This is an outstanding book. It places Wurtz, his scientific ideas, and his strategies for advancing those ideas in its appropriate cultural context. Ideas do indeed become embodied or incarnate in the ebb and flow of historical events. *Dr. Arie Leegwater, Calvin College, Grand Rapids, MI 49456-4301.*

Robert Boyle (1627-1691): Scrupulosity and Science. Michael Hunter, Boydell & Brewer, Inc., Rochester, NY; Woodbridge, UK, 2000, $90.

The famous chemist Robert Boyle has received enormous attention from historians of late. His complete works (in 14 volumes) were republished during 1999-2000 in a critical edition enriched with the first publication of much material left in manuscript form by the great man. Last year, Boyle's surviving correspondence was published for the first time in its entirety, filling another six substantial volumes. When we consider the dozen or more scholarly monographs on Boyle that have appeared since about 1990, it is no exaggeration to say that the Boyle we now know seems a wholly different (and certainly a more interesting) man than the iconic "Father of Chemistry" we thought we knew previously and whom most scientists associated primarily with a simple law describing the pressure and volume of gases.

Michael Hunter has been in the vanguard of Boyle studies, as an editor of both Boyle's *Works* and *Correspondence* and as a prolific author of scholarly papers on Boyle and his Restoration milieu. The present volume is a collection of ten papers on Boyle (eight of them previously published in journals or collections) plus an introduction. The topics range widely across various aspects of Boyle's career and persona. The first paper is an important contribution (published first in 1995) on "How Boyle Became a Scientist," which examines how and why Boyle first turned to the study of natural science during his early twenties, and away from his original activities in writing devotional and moralizing tracts. Three subsequent papers deal with how Boyle's moral concerns, particularly his examination of his conscience, affected his work. We read here about his casuistical interviews with his confessor, the potential moral roadblocks to Boyle's otherwise avid pursuit of alchemy, and the seeming "dysfunctionality" which plagued Boyle and which resulted from his over-anxious, even obsessive, concerns about taking right actions. This last paper is of special interest on a wider scale, since it serves to remind historians that our objects of study do often fail to act "rationally" (at least by our definition), and so the task of judging causes for their actions requires a great deal of finesse and understanding.

Glimpses of Boyle's curious (in both senses of the word) mind are provided by two further papers. One examines how the surviving Boyle Papers (housed in over 40 volumes at the Royal Society in London) tell us something about the English philosopher's mental landscape and method of work. The other, one of the papers published here for the first time, examines the rather obsessive "apologies" that Boyle regularly prefixed to his publications. These prefaces gave excuses for what Boyle perceived (often correctly) as the imperfect or seemingly disorganized state of his text, or fended off potential charges of plagiarism, or apologized that the book was being published at all or at the present time (being either late or premature). Readers accustomed to the modern state of scholarly publication will find this study both enlightening and amusing.

Boyle was interested in medical practices and their reforms, and accordingly he published several books on the subject. But two papers here indicate how there would have been several more if Boyle had not held his thunder. In one case, Boyle suppressed a critique of the contemporary medical establishment partly on the grounds that he was an outsider to it. In the other paper, Hunter shows how Boyle's hot, youthful enthusiasm for reforming medicine and the free communication of medical knowledge cooled significantly over time as a more mature Boyle came to understand the real social, political, and economic complexities of medical practice. The penultimate paper examines Boyle's interest in magic and how his concern over his reputation made him wary of revealing the depth of these interests. The volume is rounded out by a paper on the "Dilemma of Biography:" namely, the difficulty subsequent scholars have had in constructing a biography of Boyle. Boyle's interests and activities were wide-ranging. This situation complicated matters first by making a comprehensive biography all but impossible (even to his near contemporaries), and second by ensuring that there were always topics of interest to Boyle which, to quote one eighteenth-century student of Boyle, were "not suited to the genius of the present age," and thus had to be downplayed or dismissed.

This dilemma of biography continues in modified form to the present day. How does one fit the "new Boyle" into the narrative of the "Scientific Revolution"? Most of the papers in this book, and indeed the whole brunt of recent Boyle studies, show how quite a few facile categorizations or dichotomies, such ancient/modern, scientific/nonscientific, science/magic, rational/irrational, need to be rethought and amended if we are to do justice to our historical characters by understanding them aright. *Lawrence M. Principe, Johns Hopkins University, Baltimore, MD 21218.*

Histories of the Electron: the Birth of Microphysics. Jed Z. Buchwald and Andrew Warwick, Ed., MIT Press, Cambridge and London, 2001. xi + 514 pp, Cloth, ISBN 0-262-02494-2. $55.

The discovery of the electron or, more correctly, the discovery of the suite of properties that have been attributed to the electron, initiated the electronic age of science in the early years of the 20th century. Although subdivision of the atom had been hypothesized by energeticists and spectroscopists during the 19th century, no one could have foreseen that the electron would emerge from its origins as the unit carrier of electrical charge to become the focus of early 20th century physics and workhorse of chemistry. Physicists were the first to discover, explore, and explain the remarkable properties of the first subatomic particle and needed to confront the complexities of a massy particle bearing wave/particle duality. But some chemists began to suspect that the electron held the key to atomic valence, molecular bonding, structure, and even reaction tendencies. Modern chemists now move effortlessly (maybe superficially on occasion) from discussions of electrons as negatively charged particles to others in which electron density is spatially distributed in atomic and molecular orbitals. The electron has become fundamental to chemical explanation; and consequently a book such as this, which presents the context of its discovery and theoretical interpretation, has potential interest to chemists.

Although this book's title offers little enticement to curious chemists (or to tentative reviewers), it does contain a rewarding collection of articles that have a great deal of chemical interest and offers great value for the price. The book is one of a series entitled the Dibner Institute Studies in the History of Science and Technology and is, like others in the series, a collection of edited articles drawn from workshops focussed on selected themes. The essays in *Histories of the Electron* were first presented at two meetings held in 1997 to commemorate the centenary of the electron's purported discovery by J. J. Thomson at the Cavendish Laboratory in 1897. The subtitle reflects the electron's distinction as the first microphysical particle to be discovered, with the understanding that the "microworld " is comprised of objects smaller than the wavelength of visible light. All articles but two were written by historians and philosophers of science, but historians and philosophers who know their science very well. Their studies on various aspects of the electron are grouped around four themes: the experimental discovery of the electron and its major properties; questions of priority and the nature of discovery; accommodation of the electron in nuclear physics, chemistry and electrical science; and the electron as a real entity.

The first section, entitled "Corpuscles and Electrons," contains four chapters that explore the experimental and conceptual environment in which the electron appeared. George Smith discusses Thomson's three classic papers of 1897, 1898, and 1899, which presented the results of experimental work on cathode rays and led Thomson to conclude that the rays consisted of negatively charged "corpuscles" with a very high charge to mass ratio. (Thomson avoided use of the word "electron" coined in 1891 by George Stoney for the basic unit of electrical charge). Further, the negative rays differed in fundamental ways from positive rays and were composed, he concluded, of subatomic particles. For these contributions, Thomson is properly judged to be the seminal figure in electron history; but depending on how one defines discovery, he may not be the electron's unique discoverer. Isobel Falconer demonstrates how local context affects historical analysis by comparing early British and German accounts, the former emphasizing Thomson's work and the latter that of Lorentz and Zeeman. Graeme Gooday adds that the impact of Thomson's results was unclear for several years, during which time the electro-technology of the period bounded along. After about 1910, as the particulate properties of the electron became more widely accepted, Thomson's students worked diligently to place their mentor at the center of the discovery process. Benoit Lelong writes that it was even possible to explain many of the cathode ray results by a theoretically more conservative hypothesis involving ionized hydrogen atoms, as the Frenchman Paul Villard did before converting to Thomson's interpretation. In sum, these opening chapters confirm that the discovery of the electron is not the rational, individualistic process succinctly presented in modern science texts; only the logical reconstruction of historical events makes it seem so.

The second section of the book, entitled "What was the Newborn Electron Good For?," is loosely organized around the theoretical status of the new particle and its incorporation into experimental physics. Theodore Arabatzis circumvents the claim of antirealist philosophers that it is impossible to specify compelling criteria for the discovery of an unobservable entity such as the electron by redefining discovery as the formation of consensus within the scientific community. On this view Zeeman, who obtained good values for the charge to mass ratio of the atomic component responsible for the electromagnetic splitting of the sodium D line in 1896, Lorentz and Larmor all have significant roles. Helge Kragh investigates the electron's brief life as the potential "protyle." the ultimate particle of all matter, a hypothesis that was dashed by the discovery of other subatomic particles later in the 20th century. Such particulate views of the electron were, however, intimately interconnected with explanations of phenomena in electrochemical, electrodynamical, and magnetooptical researches. The electron seemed to be as ubiquitous as the ether, but just as elusive. Ole Knudsen describes the work of one of Thomson's most successful students, O. W. Richardson, who initiated the study of thermal electron emission and extended the range of phenomena explicable by electron theory. Walter Kaiser gives another example of the theory elaboration in his report of work beginning in 1900 on electrical conduction in metals, work that began by transferring concepts from the kinetic theory of gases to an "electron gas" of charged electrons of fixed mass moving freely within a metal.

The third section, entitled "Electrons Applied and Appropriated," contains papers of greater relevance to chemists. Laurie Brown investigates the various suggestions for the location of electrons in the atom, either within or outside of the nucleus, with special emphasis on Heisenberg's theories on nuclear electrons. Lillian Hoddeson and Michael Riordan advocate that the electron's reality was reinforced for scientists and engineers when it was put to work in devices such as the vacuum tube amplifier. Mary Jo Nye suggests the electron entered chemistry in three stages— as a material particle in the valence bond, as a participant in reaction mechanisms and then in resonance theory, with the key participants being Robert Robinson, Keith Ingold, and Linus Pauling. The electronic aspects of chemical explanation are presented as an evolutionary advance because nothing important in the chemical corpus had to be discarded to make room for it. Kostas Gavroglu focuses on the developments which enabled physicists and, later, chemists to bring the quantized electron into comprehensive theories of bonding and valence. After Heitler and London's successful use of electron spin in 1927 to explain the 2-electron bond in molecular hydrogen, others such as Mulliken and Pauling extended the "quantum mechanical" methods of electronic bonding to larger molecules, thus giving the methods greater chemical utility. The chapters in this section bring the electron into its modern chemical environment where it manifests itself as a particle or wave, as need requires. But the story does not end here.

The concluding section, simply entitled "Philosophical Electrons," does what philosophy does best—render asunder the reconstructed logic of favored explanations. Peter Achinstein dismisses the sociological interpretation of discovery as consensus in place of a person-oriented, conscious, and even wilful path to discovery. By his criteria, Thomson fulfilled enough of the requirements to merit recognition as a discoverer, maybe. How surprising it is to have a philosopher leave an argument as an open question. Margaret Morrison uses the concept of electron spin to explore the problems inherent in affirming the reality of an entity independently of the ways in which it is investigated. Electron spin, so crucial to the electron's behaviour, still lacks consensus as to its physical nature. Jonathan Bain and John Norton use electron theories to dispute the philosophical tenet that, since all theories in the history of science have been false (in the sense that none has been complete), the methods of science do not generate true theories— dubbed, obviously enough, the "pessimistic induction." They argue, contrarily, for an "optimistic induction" in which science advances through a series of theories that correct the errors of predecessors while providing ever improved representations of phenomena. Such a progressionist view is likely in harmony with the history of science most of us chemists are comfortable with. The book concludes with a chapter by Nicholas Rasmussen and Alan Chalmers, in which they investigate early uses of the electron microscope in biology and physics to conclude that instruments often interact synergistically with theory. The interaction of theory and practice is normally quite complex and event-specific, so much so that "science might be much more heterogeneous and complex than philosophers have long been imagining." No chemist will dispute this claim.

This book is packed with scientific, historical, philosophical, and sociological information in each of the four sections. It helps us view, from the perspective of the 21st century, the enormous renovation of chemical thought in the previous century made possible by the discovery of the electron. In addition it provides, for those wishing it, an accessible account of various issues current in the history, sociology, and philosophy of science illustrated by a scientific example of great interest and subtlety. If you do not wish to have it for yourself, make sure your library orders it. It is quite likely that, after reading the book, you will present electron theory to your colleagues or students in a very different way. *M. C. Usselman, Department of Chemistry, University of Western Ontario, London ON N6A 5B7, Canada.*

Liebigs Lehrer. Karl W. G. Kastner (1783-1857): Eine Professorenkarriere in Zeiten naturwissenschaftlichen Umbruchs. Martin Kirschke, GNT Verlag (Verlag für Geschichte der Naturwissenschaften und Technik), Berlin, Diepholz, 2001, 450 pp, 38.50 Euro.

When Richard Wagner felt the need to create room for his music, he decided to destroy not only the reputation of Felix Mendelssohn, whose music was considered the pinnacle of musical achievement, but also all music created by Jews which he described as derivative and unoriginal. Something similar must have occurred to Justus Liebig. After having worked closely with his teacher Karl Kastner and greatly helped by him over a long period, Liebig in 1840, at age 37, let loose a diatribe against chemistry in Prussia, making fun of *Naturphilosophie*, the reigning romantic perspective on natural phenomena. Not only by implication, but by name, he singled out Kastner as a leading exponent. Ever since, Kastner has tended to be derided or ignored. Yet in his time he was considered one of the greatest German chemists, equally versed also in physics, botany, and pharmacy. Liebig chose to be his student in Bonn because of his eminence; and when Kastner moved to

Erlangen, Liebig went with him. Kastner helped him when he wanted to study in France, obtaining a stipend for him from the Grand Duke; and even after the verbal blast, Kastner continued in various ways to be of assistance.

Recent writers have begun to revise the general viewpoint and have stated their shock at Liebig's ingratitude. Thus the Liebig biographer William H. Brock in his *Norton History of Chemistry* (p 200): "In later life Liebig was rude about Kastner's chemical competence and decidedly ungracious towards him; but without Kastner's support and patronage Liebig might well have remained a small-town hardware salesman."

In his book *Karl Kastner* Martin Kirschke explores the Liebig episode in detail; but the book covers far, far, more. It reminds me of Alan Rocke's biography of Hermann Kolbe, where Rocke deliberately chose a lesser yet very able man to illuminate a period. Rocke actually begins his book with a reference to Liebig's unfair criticism of German chemistry. Kirschke makes clear that he is certainly not writing a hagiography. Instead his book lifts a forgotten man out of obscurity and uses him as a vehicle to illuminate an early chemical transformation.

The book is a doctoral dissertation submitted to the University of Regensburg, where the presence on the history-of-science faculty of Christoph Meinel and Carsten Renhardt alone suffices to indicate the blisteringly high standards expected of a doctoral candidate. The book does not disappoint. It places Kastner in his social, religious, academic, and political milieu. In the process we learn of the development of chemistry, and often of other sciences, at the universities of Jena, Heidelberg, Halle, Bonn, and Erlangen. Kastner's many books and the journals he edited are described in detail, and the reader is surprised at the emphasis Kastner placed on empirical evidence, on experimental confirmation. He was interested in commercial applications of chemistry and was the author of books on experimental chemistry and experimental physics that were widely used. And he was up to date. He discussed galvanic electricity as well as some of Humphry Davy's researches, including Davy's invention of the miner's safety lamp.

There are some fascinating aspects to Kastner's very appealing life. He experienced the French occupation and participated in the wars of liberation against Napoleon, being in charge at one point of four field hospitals. When it was all over, he was in Britain for four months raising relief funds for German widows and orphans and came home with a sizeable sum voted by the British parliament. When the Erlangen town-gown tensions had reached the point that the students, Liebig of course among them, left the university *en masse*, Kastner served as the go-between, trusted by both students and administration, to bring the students back to Erlangen. He was a very popular lecturer; and Liebig was not the only student of Kastner of interest to later historians. Another future scientist of renown was Pierre Louis Dulong of the Dulong and Petit Rule. Kastner also taught August Goethe, the son of the poet. Kastner was married and had several children but only one reached the age of 45. All his life he sought adequate laboratory space and equipment for his students and for his own research, but the authorities were slow to recognize the significance and importance of practical instruction and independent research. Only with Liebig's instructional laboratory in Giessen do we see this essential training component for all chemists adequately recognized by the authorities. Nevertheless, even during the many years Kastner had to work in his own home, he was known for his analyses of the waters of mineral springs and was sought for advice regarding their safety.

Kastner is variously labeled as representing the romantic and *Naturphilosophie* traditions and as being a Kantian. The latter designation helps explain Kastner's insistence that all theorizing should be based on observable evidence. In the absence of laboratories he used lecture demonstrations extensively. *Naturphilosophie* sought for a vision of unity, linking phenomena in the heavens, for instance, to living systems and chemical processes observable in the laboratory. It was opposed to mechanistic and reductionist views and was suspicious of mathematical formulations, looking for qualitative rather than quantitative laws.

In defense of Liebig it should be said that Wöhler and Berzelius also made fun of *Naturphilosophie.* And although in 1840 Liebig ridiculed the concept of a vital force in chemistry, he felt the need for it in order to explain various physiological processes, as Kenneth Caneva points out in his *Robert Mayer and the Conservation of Energy.*

We need to remember that Joseph Priestley clung to phlogiston ideas rather than espousing Lavoisier's system and yet found it possible to do perfectly sound chemistry using the older language. And we are finding that with the espousal of a new language certain insights of the older system are lost, only to be rediscovered many years later. Modern science has gone through a long period of searching for local order irrespective of the larger picture, in the faith that in the end all the pieces will fit together. Yet the longing of Kastner and others for a unitary, holistic view of nature is never submerged completely and is the motivation of many who make major contributions to our science. *Theodor Benfey, 909 Woodbrook Drive, Greensboro NC 27410; and the Chemical Heritage Foundation* benfeyot@nr.infi.net.

Imaging a Career in Science: The Iconography of Antoine Laurent Lavoisier. Marco Beretta, Science History Publications/USA, Nantucket, MA, 2001; xvii + 126 pp, clothbound and jacketed, ISBN 0-88135-2294-2, $29.95.

According to the author in his introduction, the only other attempt at putting together a survey of images of Lavoisier—in the author's words "iconography"—was published by Pierre Lemay in the 1930s, a "superficial but useful survey." Beretta, inspired by newer findings of references to paintings and sculptures portraying Lavoisier, undertook the project he feels Lemay and some others never accomplished: a thorough survey of the iconography of Lavoisier and an interpretation of these images as insight into Lavoisier's intellectual background and professional career. To the present author this seems a gargantuan goal for someone who must be equally competent as an historian of chemistry and art. Roald Hoffmann, in his foreward, expresses the view that we the readers have a better understanding of Lavoisier through this critique of the many renditions of Lavoisier, some verifiable and many of questionable authenticity.

To be sure, readers are presented with a generous collection of images of Lavoisier in the form of a "Select Catalogue of Lavoisier's and Madame Lavoisier's Iconography" (pp 77 – 111), many of which are reproduced in the catalogue section or elsewhere in the text. The 1806 engraving by Brossard de Beaulieu appears twice (pp 13, 29). Unfortunately some images within the text are not given their corresponding catalogue designation. The pivotal image, around which all discussion rests, is the famous and familiar portrait of M. and Mme. Lavoisier painted by Jacques Louis David in 1788. In possession of family heirs until 1925, when it was purchased by John D. Rockefeller, the portrait passed to the Rockefeller Institute in 1927 and was acquired by the Metropolitan Museum of New York in 1977. It is the sole subject of Chapter 2; any images before David are treated in Chapter 1. The significant contributions of Mme. Lavoisier as illustrator form the basis for Chapter 3, "The Chemical Revolution in Action." Not only did she provide artistic elaboration for her husband's writings, as is well known; she also created many illustrations to promote the 'new chemistry.' Beretta describes an allegorical performance, probably organized by Mme. Lavoisier, at the Arsenal in 1788 or 1789. In this staged inquisition on phlogiston, she played the role of a priestess, with Stahl as the victim. "The Iconographic Myth," the last chapter, is a description of the use of artistic renditions of Lavoisier as a means of glorifying his image in the decades after his death.

It is not surprising that Beretta, as a historian of chemistry, should defer to two recent publications by historians (M. Vidal, 1995; A. Donovan, 1996) on David's creative works, including the Lavoisier portrait. Vidal reads a wealth of information from Mme. Lavoisier's gaze directed, not at her husband, but at the artist; and Beretta seems to confirm this "evidence" that she is mediator between science and art—not merely her husband's muse. There are many examples of supposition in 'reading' meaning from the art. David might have drawn inspiration for the portrait from Tangena's 17th-century engraving of Descarte (p 40); it is "perhaps not unlikely" that *Hommage rendu à la mémoire de Lavoisier* (1807) was done by Mme. Lavosier (p 47). Beretta acknowledges several colleagues by name and three anonymous referees for their suggestions and criticisms, but a few inconsistencies have slipped by. One is the contradictory information on Fontana's instrument, variously dated as 1777 (p 37) and 1780 (p 38). In fact, Partington gives the date as 1781. One hopes this error is the exception rather than the rule. No name is attached to the translation, for which the author acknowledges financial support; but some mistakes have been overlooked ("laying;" p 34; "loosing," p 44).

Whether the treatment by Beretta measures up as to scholarly treatment or not, the book is a handsome collection of black and white and colored reproductions of many fascinating likenesses of Lavoisier, but also of a variety of other forms: Mme. Lavoisier's painting of Benjamin Franklin and her self portrait, for example. *Paul R. Jones, University of Michigan.*