BOOK REVIEWS

William Crookes (1832-1919) and the Commercialization of Science. William H. Brock, Ashgate Publishing Company, Burlington, VT, 2008, 586 pp, ISBN 978-0-7546-6322-5, £65, $124.95.

This hefty biography is a contribution to the publisher's 'Science, Technology and Culture, 1700-1945' series. Even that broad rubric scarcely encompasses the multifarious activities of William Crookes, whose proud and ambiguous motto for his escutcheon was *Ubi Crux Ibi Lux*.

The son of a prosperous tailor, Crookes received a somewhat sporadic education, the most important part of which was an apprenticeship at Prince Albert's Royal College of Chemistry under the tutelage of August Wilhelm Hofmann. Subsequently, a rather aimless year spent in Oxford completed his formal education. He is a supreme example of the autodidact. In spite of the fact that he had no formal degree and never held an academic position, he was to make major contributions to photography, chemistry, physics, agricultural science, public health, scientific journalism, and, astonishingly, spiritualism. An ambitious, flamboyant and at times ruthless man, he rose to be knighted by Queen Victoria and elected President of both The Chemical Society and The Royal Society.

To write the biography of such a varied man is a challenge, but Brock has met that challenge in superb fashion. He summarizes Crookes' principal achievements in the following words:

> He is remembered chiefly for five things: the discovery of thallium in 1861; the invention of the eye-catching and puzzling radiometer in 1875; his brilliant experimental work on cathode rays using the eponymous Crookes tubein the 1870s; his dire prediction that mankind would starve unless chemists learned how to 'fix' nitrogen; and for his seemingly unorthodox spiritualism in the 1870s.

Brock goes into all this (and much else) in meticulous detail. Indeed, for the general reader the detail at times is a trifle overwhelming, but one of Crookes' talents was to recognize signific,ance in seemingly trifling experimental observations.

To a modern reader Crookes' deep interest in spiritualism from the 1870s on is most strange. It was an interest shared by many eminent Victorians. Not all were believers—Faraday and Tyndall, for instance, were skeptical—but many scientists (Lord Rayleigh and Dewar) and other intellectuals (such as Arthur Conan Doyle) shared Crookes' passion, if not his intensity. A later wag even modified Crookes' proud motto to *Ubi Crookes Ibi Spookes*.

The last major biography of Crookes was written by Fournier d'Albe in 1923. Brock's achievement is such that there should be no need to write another for at least 86 more years. *Derek A Davenport, Purdue University, West Lafayette, IN 47906.*

A Strange and Formidable Weapon: British Responses to World War I Poison Gas. Marion Girard, University of Nebraska Press, Lincoln, NE, 2008, xii + 284 pp, ISBN 978-0-8032-2223-6, $45.

I always feel a bit apprehensive when I start to read a book that began its life as a dissertation. Perhaps it's from thinking about my own eminently unpublishable dissertation, or perhaps it's the graduate-school memory of seemingly endless rows of dissertations in the library. They seemed to collect dust and be mostly unread, except by each candidate's committee members (and sometimes not even by them). It is true that turning a dissertation into a published book is more common in the humanities than in the hard sciences, and there are many successful and interesting books that have come into being via that route. Whether Marion Girard's *A Strange and Formidable Weapon*, which is based on her 2002 dissertation at Yale, is such a book may well depend on your point of view and reason for reading it. The blurb on the inside front jacket claims that it "uncovers the history of this weapon of total war and illustrates the widening involvement of society in warfare." I found much more of the latter than the former, and in that respect the subtitle, *British Responses to World War I Poison Gas*, is more indicative of the real intent of the book.

This is not a history of chemical weapons; Girard had a different goal in mind. Chemical weapons are her vehicle for investigating larger issues surrounding the war. Two ideas central to the book are that gas is "a tool of total war and [also] of post-Great War military policy" (p 6). The term "total war" refers to the dedication of all the people and resources of a nation to a particular war effort, but Girard suggests that viewing poison gas as a "total weapon"—i.e., one that affected everyone, on the home front, as well as on the battlefield—is both a novel way of looking at Great Britain as a participant in WWI and a means of examining the effect of the war on its citizens. Since different segments of society viewed and reacted to chemical weapons differently, "[c]omparing and contrasting these views offer a wider window into total war, First World War Britain, and the mixed reputation of gas" (p 7).

Girard sets up two pairs of opposites in connection with the use of chemical weapons in WWI: (1) the Western view of the superiority of its own civilization versus the barbarity of deploying poison gases on the battlefield; and (2) the terror that such weapons could evoke, not only among soldiers at the front (at least before adequate antigas protection became available), but also among civilians at home, versus desensitization toward the horrors of poison gas by some people as they became more familiar with these weapons. Somewhat mechanically, Girard then devotes one chapter each to examining the views of politicians, the military, chemists and army physicians, industrialists, and general civilians along the spectra of these two pairs of opposites.

Chapter 3, "The Scientific Divide: Chemists versus Physicians," might be one of the more interesting chapters to many readers of the *Bulletin*. In a sense, these two groups fit together as opposite sides of the same coin. Physicians treated victims of the poison gases developed by research chemists. Girard portrays chemists as having "enjoyed a positive experience with gas" (p 76) as they played a crucial role in developing both new chemical weapons and effective antigas measures and, in doing so, enhanced the prestige of their profession. This picture of chemists working on chemical weapons in WWI bears more than a superficial resemblance to that of physicists working on atomic weapons in WWII. The author also does a good job, in just a few pages (pp 80-88), of succinctly describing the state of British chemistry at the start of the 20th century and the participation of individual chemists and scientific organizations in the war effort. Besides their research in the laboratory, looking for poison gases that might be suitable for the battlefield, chemists served on government committees that worked directly with the military. Among the list of names are such well-known British chemists as F. G. Donnan of University College, London, P. F. Frankland of Birmingham University, and W. J. Pope of Cambridge University.

I found that Chapter 5 covers a somewhat different topic from what Girard indicated. She states that "Chapter 5 analyzes general civilians" (p 12), but its title, "Gas as a Symbol: Visual Images of Chemical Weapons in the Popular Press," is a better description of its actual topic. While I accept the author's claim that people generally read newspapers and magazines offering points of view they agree with, I don't think this leads to her conclusions that "[t]he tone of the pictures also illustrates attitudes held by the British public about gas and war" (p 127) and that the "[v]isual images in journals therefore offer rich insights into perceptions of poison gas by the British public" (pp 127-8). While these images—mostly drawings and cartoons, along with a few photographs—may offer insights into what editors thought their reading public wanted, there is no discussion of the response of the British public to these images. Despite her description of a number of images that seem clearly intended as propaganda (at least it seems that way to me) and even her own admission that some of them might be propagandistic, Girard never discusses the role of these images in shaping popular opinion, maintaining instead that they captured "public sentiment about poison gas" (p 154) and "helped the British to comprehend the horrors of World War I" (p 156). These latter claims may be true, but published illustrations that reflect (and may help shape) public sentiment are not public sentiment themselves.

Chapter 6, "The Reestablishment of the Gas Taboo and the Public Debate," focuses on the postwar debate about chemical weapons both within and between different groups in Great Britain. Girard sets up the debate broadly between those who were "gas-tolerant" (including the biologist J. B. S. Haldane) and those who were antigas (including the writer H. G. Wells), though each side included individuals with a broad range of views and attitudes. The debate was carried out within the context of certain beliefs by both sides: (1) another war was inevitable; and (2) the coming war would include the use of chemical weapons, possibly against civilians, as well as against soldiers. Nevertheless, the antigas arguments eventually prevailed as public opinion came to embrace the taboo against such weapons and the hope that international treaties would prevent their use in the future.

The "Epilogue" is intended to extend the lessons about chemical weapons from WWI down to the present, but I did not find that, even in conjunction with the previous chapter, it tied together the five chapters about different segments of British society. Much of this final chapter focuses on the taboo against such weapons, which was strengthened through the public debate between the two world wars. Girard poses the question whether it is the taboo or the idea of deterrence that accounts for the fact that chemical weapons have not generally been used in wars since WWI. She does not explicitly consider the possibility that as more effective and deadly weapons were developed, the need for chemical weapons by conventional military forces diminished. While the vagaries of wind and weather would always influence the use and effectiveness of chemical weapons, there are no such problems with atomic and nuclear weapons.

As a final note, the book's origins as a dissertation are obvious in its documentation. Although it is listed at 284 pages, the text ends on page 199, and notes take up the next fifty pages, along with an 11-page bibliography. The documentation is obviously important to a scholar interested in this topic, especially since many of the notes are references to material in British archives. However, as a general reader, I became annoyed with so many references, few of which added directly to the text.

The book is a well-researched and documented scholarly work, which can obviously provide important material and references for the specialist. The general reader, however, may find it too narrowly focused on a very specific slice of the overall story of chemical weapons. *Richard E. Rice, P.O. Box 1210, Florence, MT 59833; charrice@juno.com.*

Perspectives on Risk and Regulation: The FDA at 100. Arthur Daemmrich and Joanna Radin, Ed., Chemical Heritage Foundation, Philadelphia, PA, 2007, 163 pp, ISBN 978-0-941901-42-0, $12.

This volume contains the proceedings from a one-day conference held in 2006 in Philadelphia, at the Chemical Heritage Foundation, to celebrate 100 years of the Food and Drug Administration. Attendees at the conference, one of several events held throughout the country, included people from industry, trade organizations, and the FDA. Although each person's perspective differed, there was common ground: science is the basis for decisions made by the FDA; adequate funding is necessary to continue the work of the administration; the policies and regulations set forth by the FDA allow the United States to have the "gold standard" in terms of consumer safety as it related to food, drugs, cosmetics and medical devices; and historical perspective illumines the present and points the way to the future. The book's structure reflects that of the conference itself, with an introduction, division into three sections—historical perspective, drug and medical devices, and food and dietary supplements—and a conclusion from FDA Commissioner Andrew Eschenbach. In addition, a streamlined Q&A appears after the second and third sections. A time line of the FDA at the beginning of the book is very helpful. In organization, in brevity and in content, this book provides a comprehensive survey of the many ways in which the FDA ensures the public's safety on a daily basis.

The historical section, by Peter Barton Hutt, is framed around ten events that Hutt sees as turning points in FDA history. Hutt suggests that science is the basis for the FDA and allows it to move forward. He also notes that he could have chosen many other examples for his ten "critical" events. A mix of public outcry following accidents, legal cases, and Congressional maneuvering comprise his list.

The second section focuses on drug and device regulation from industry and administration perspectives. Steve Galson explains how the Center for Drug Evaluation and Research works to ensure drug safety. With new processes initiated, he sees communication with the public, with health care professionals, with industry, and with other governmental agencies as critical to the success and continued safety of the American drug market. Ronald Krall envisions the future of pharmaceuticals as individualized, genetic-based medicine and the concomitant regulation of said pharmaceuticals as one of continual and active surveillance in this personalized medicine world. Krall offers suggestions to help keep the United States at the forefront of worldwide regulation. Daniel Schultz explores the world of device safety as this becomes increasingly complex with the creation of items that are both device and drug, such as coated stents. As the medical marketplace moves forward, Schultz notes that the personnel at the FDA who evaluate such items will need increasing resources to stay atop the latest developments in two different regulatory fields. Robert O'Holla offers a mini-retrospective of how the changes in device regulation have affected industry as he ponders thirty years of device manufacture and design and regulatory approval.

The third section considers those items we ingest through food, including dietary supplements, the latter not regulated until 1994. Robert Brackett posits that the FDA's challenge to keep our food supply safe has grown commensurate to our changing food habits: we consume items grown or produced across the world; we demand raw or organic foodstuffs; we travel globally; and all of this affects what we eat and how our bodies react. The FDA does keep our food safe, but with expedited air travel and new fruits and vegetables entering the market all the time, scientists are continually creating new tests for new items. Idamarie Laquatra advances an industry's appreciation for regulation. The extant guidelines help companies as they look to current research and the FDA's interpretation of the latest scientific studies to craft regulations relating to labeling and nutrition information. Barbara Schneeman proffers the FDA view on dietary supplement regulations, and Steven Mister counters with an industry perspective. In both cases, the authors emphasize the relationship between science and public health. Not surprisingly, they differ in what they think the FDA should do with the relevant labeling and education laws.

In the final section, FDA Commissioner Andrew von Eschenbach looks at the past and glimpses the future of all that the FDA did, does, and should do. His vision is one of solidity: to keep our food and drug supply as safe as it can be, within the financial constraints imposed by Congress and limitations of staff. As do all the other chapters, this final one emphasizes how much we have gained from the FDA's vigilance on our behalf over the last 100 years.

The editors compiled the chapters fairly quickly after the conference. The Chemical Heritage Foundation is to be commended both for sponsoring the conference and printing this book. Each chapter is a crisper version of the conference paper, and the incorporated discussion sections offer an opportunity for readers to know how the authors responded "off the cuff" to questions that arose after each of the two major sessions. In some cases, the chapter offered one version and the answers revealed current and future plans for industry or enforcement. This book is one that can be read either cover to cover or selectively, depending on one's background and interest. Given that we all depend on the FDA for ensuring the safety of our food and drug supplies, cosmetic items and medical devices, this should be a topic of interest to all. *Gwen Kay, State University of New York, Oswego*.

Collected Papers on Philosophy of Chemistry. Eric R. Scerri, World Scientific Publishing Co., Pte Ltd, Singapore, 2008, 235 pp, ISBN-13 978-1-84816-137-5, $95.

The author, Malta-born (1953) and UK-educated, received his Ph. D. in history and philosophy of science from King's College, London, in 1992. After postdoctoral appointments at the London School of Economics and at Caltech, he taught at Bradley University and at Purdue. He joined UCLA in 2000 as a lecturer in the Department of Chemistry. This book gathers a selection of his papers. It is organized in three parts, dealing with the reducibility of chemistry to quantum mechanics, with the periodic table, and with the issues of realism/anti-realism in relation to chemical education. An introduction states the general goals and traces the intellectual itinerary of the author during the period 1992-2007.

Scerri's core intuition is shared by many of us: chemistry is an autonomous science. This is a point I will return to. Before doing so, I will examine Scerri's approach and the topics he chose to study, its originality, the relevance of his ideas to chemical education, and I will note strengths and weaknesses. I will conclude with stating my views on philosophy of chemistry, its purpose and usefulness.

Scerri's approach is to bring up statements made by various historians and philosophers of science and to demolish them as mistaken. His main evidence is the periodic table of the elements. Scerri argues that the periodic table, and chemistry accordingly, were established from empirical data rather than being derived deductively from first principles stated in quantum mechanics. In terms of a general philosophy of chemistry, this is a rather narrow viewpoint. The book is underlined by a teaching of general chemistry at the high school-college level, 1950s-vintage, emphasizing electronic configurations for the elements and valency—rather than, say, coordination numbers—that has become somewhat dated. Likewise with the near-exclusive focus on the periodic table. Instead of attacking the claim by some physicists of the periodic table deriving directly and exclusively from quantum mechanics, Scerri might have performed a more useful task by analyzing, historically and philosophically, the *chemical* evidence on which the periodic table relies.

Yes, Mendeleev discovered what one might term, metaphorically, the Rosetta Stone for chemistry. As such, the periodic table is a monument of science. Its iconic status is sufficiently obvious to need no reiteration. But a distinction is essential. While reverence is amply justified, to treat the periodic system of the elements in like manner to the Ten Commandments is uncritical and, ultimately, unscientific. I am referring here to the naïve illusion, harbored by many a student—of course not by Dr. Scerri—of the periodic table as providing the answer to any exam question. While this may be axiomatically true, it is far from being pragmatically useful. One ought to keep this key distinction in mind: to endow the periodic table with talismanic value amounts to treating it as an object of magic.

Which makes it all the more difficult to examine it as a topic for historical and philosophical appraisal and discussion. Art historians, for a similar reason, steer clear of the Mona Lisa. The way to go about, with such idolized artifacts, is to treat them with levity and wit, seriously of course, but not solemnly, with a measure of disrespect. In a word, one has to first remove the veneer. At least, this is my intuitive understanding of how to treat such monuments to fit them into intellectual history. Scerri's attitude of reverential respect, while warranted may be self-defeating: the periodic table is not central to an understanding of chemistry, a point I shall now examine.

Three axioms undergird Scerri's enterprise: A. the periodic table is the concept, the organizing principle most important to chemistry; B. the main goal for a philosophy of chemistry is to critically examine the concepts at the core of the science; C. hence, if Eric R. Scerri devotes himself to studying the periodic table, he fulfills a most essential task.

While C is a winsome belief, it remains an act of faith. As such, it can be followed or it can be ignored. The other two axioms deserve closer scrutiny.

There is a host of other candidates for A. One might argue for the primacy of any of the following: A_2, chemistry is a molecular science; A_3, chemistry is subsumed by the Pauli Principle; A_4, chemistry deals with the organization of matter at the microscopic level, the way in which atoms cluster, form bonds and molecules and supramolecular assemblies, enter coordination complexes, ...; A_5, formation and breaking of bonds between atoms is at the heart of chemistry; A_6, catalysis cements the synergy between chemistry as a science and as an industry; etc.

B is also questionable. One may want to replace the emphasis on *concepts* by one on *actions*, i.e., what chemists do: B_1, purifying substances; B_2, synthesizing molecules; B_3, putting together nanometric structures; B_4, interconverting chemical entities on hypersurfaces; B_5, determining reaction mechanisms; B_6, pursuing the artificial, in its infinite variety of costumes; B_7, mastering a combinatorial artistry; etc.

I have put together these, admittedly short, lists to stress that Scerri's endeavor may not belong, as he so clearly trusts and would have us believe, at the apex of any philosophy of chemistry.

To return to the periodic table, its iconic status is undeniable. Does that justify treating it as the only worthwhile topic for philosophy of chemistry? To use a comparison, Marilyn Monroe also enjoyed iconic status. Does it make her in any way an important object of study in terms of, say, American womanhood in the Sixties? The recently departed Studs Terkel made a lasting contribution to American sociology from focusing, not on individuals with iconic status but, conversely, on ordinary men and women. Would not an analogous attitude make a lot more sense for building a genuine philosophy of chemistry?

Despite the present times being the age of hype, the author's smugness, his constant one-upmanship make a bad impression. He presents himself as a founder, if not the founder of the whole field of philosophy of chemistry. In so doing, he ignores the earlier, much earlier contributions by the likes of Emile Meyerson, Hélène Metzger, or Gaston Bachelard (except, in this last case, for the flimsiest of mentions), not to mention Hegel, who wrote an entire book on the philosophy of chemistry. Moreover, did not our ancestors, the alchemists, term themselves "philosophers," admittedly in a different sense?

Take this for instance. Scerri writes: "I am not aware of anybody other than myself who has written about the nature of the most recent density functional approaches in the philosophical literature." Assuming that the assertion is true, and turning to what Scerri has written about the density functional approach (pp 160-162), one is bound to ask "where is the beef? What philosophical questions or issues has he raised? In what way is Scerri's description of the density functional approach enlightening?"

An important point is absent from Scerri's book: how does philosophy of chemistry fit into philosophy of science? Is it exceptional and, if so, in what way? It would have been most useful had Scerri distinguished between synchronic and diachronic approaches to epistemology. The former describes deductive logic, the strategy which Paul Dirac asserted—which incidentally may be irrefutable in principle—would ultimately make chemistry a daughter science to quantum physics. The latter is illustrated, among others, by Sir Karl Popper's notion of conjectures and refutations: scientific epistemology, in that tradition, is procedure-driven.

Scerri's constant self-reference and self-assurance grate all the more that the book is marred by quite a few mistakes. Examples? Stating nitric oxide to be an unstable molecule (p 74) is a patently untrue assertion: unstable relative to *what*? Clearly, NO is stable, not unstable, with respect to its dissociation. Scerri blames the purported instability on the presence of an odd number of electrons. OClO is a highly persistent entity, yet it is a free radical, too. Dioxygen is a diradical in the ground state; does it make it unstable? It explains its reactivity, which is not the same thing. As for NO, its falsely asserted instability runs in the face of its multitudinous physiological functions.

Another questionable assertion concerns isotopes. Scerri repeats the old chestnut of isotopes having identical chemical properties (p 16), whereas primary or secondary isotope effects, from protium-deuterium substitution for instance, are ample evidence to the contrary. Each atom of carbon in a natural product has a distinct and measurable (at the 1% difference level) $^{12}C/^{11}C$ ratio, because of such isotope effects.

But let us examine the main emphasis of the book: the effect of the irreducibility of chemistry in general, and of the periodic table in particular, to quantum mechanics. As already stated, Scerri is to be commended for taking issue with Paul Dirac's statement (1929 -1930). The very course of chemistry since Dirac's famous dictum, especially during the second half of the twentieth century, has made it moot and has abundantly displayed the autonomy of chemistry from physics.

Scerri thus puts the question (p 60): "Has Chemistry Been At Least Approximately Reduced to Quantum Mechanics?" One is reminded, let me note in passing, of the theological discussions on whether the sacred wafer is consubstantial with the body of Christ.

Scerri's question is of the logical type "Does A cause B?" where A stands for quantum mechanics and B stands for chemistry. But is it a well-posed question? Chemistry is a field of science. Quantum mechanics does not enjoy an equivalent status. Quantum mechanics is a toolbox, drawing on various mathematical equations. Granted, it is an extremely powerful toolbox. It allows calculation of many observables to impressive accuracy. If I am allowed the comparison, would one even think of raising the formally identical question: "Has Astronomy Been Redefined by the Hubble Telescope?"?

But let us turn from the unfortunate wording of the title of that chapter to its content. It has considerable merit. Scerri presents in clear, succinct and rather objective manner the gist of the main quantum chemical calculations. The interesting question is, I submit, not that of the reducibility of chemistry to quantum mechanics. Instead, it is that of the chemical *insights* gained through quantum chemical calculations: one judges a tool by how efficient it is, not by metaphysical considerations as to its generative prowess. This is again, as with the (iconic status/talismanic value) a fine distinction. But—and this is a crucial point—the role of philosophy is to make such fine distinctions, not to use language and concepts loosely.

Philosophy of chemistry calls for scrutiny of what chemists actually do. This is all the more challenging that the science does not stand still. It has evolved more since 1950 than between 1789 and 1950. Textbooks lag behind by necessity and because of the conservatism of teachers. They simply fail as sourcebooks for philosophical issues and discussions.

Philosophy of chemistry deals with questions both old and new. An example of the former, reinvigorated by recent developments, is the heap of sand. Greek philosophy of Antiquity raised that paradox. It concerns meaning and naming both. When is it legitimate to name an aggregate of grains of sand a heap? If we remove a single grain, clearly it remains a heap of sand. But let us continue likewise to reduce the heap indefinitely, grain by grain. Is it still a heap when only six grains of sand, say, are left? Clearly not. Then, at what stage does the collection of grains switch from being a heap to being something else? This question, as old as philosophy itself, assumes renewed urgency nowadays with quantum dots. Those are aggregates, not of grains of sand, but of atoms. There is a critical number, of the order of magnitude of 30-50, when such a cluster, instead of displaying the usual macroscopic properties of a condensed phase, switches into an entirely different set of properties, describable by quantum theory. Even more interesting, the critical number is observable-dependent: whether one looks at the cohesive energy, conductivity, spectrum, … the crossover occurs at different aggregate sizes.

An altogether different question is that of the epistemic status of molecular *models*: what are they? They obviously differ from their homonyms, those intellectual constructs in-between working hypotheses and fully-fledged theories. They resemble more the dummies architects rely upon. A careful delineation of the two kinds of models is in order.

Any philosophy of chemistry has to address a central cognitive issue, that of the iconic language of chemistry (formulas), in relationship to division of labor between the two hemispheres of the brain, pictograms and other visual languages.

One could go on and on! Scerri has merely scratched the surface of the bounty, too often in the same spot.
Pierre Laszlo, Prades, F-12320, Senergues, France.

Fermentation: Vital or Chemical Process? Joseph S. Fruton, History of Science and Medicine Library, Vol. 1, Koninklijke Brill NV, Leiden, Boston, MA, 2006, xv + 116 pp, ISBN 978 90 04 15268 7, € 75, $98.

Fermentation is the last book written by Joseph Fruton, who died at the age of 95 on July 29, 2007, two days after the death of his wife and long time collaborator, Sophia Simmonds. Fruton had distinguished careers as a biochemist and as an historian. It is quite apparent that he had given a great deal of thought to fermentation and its importance to science and to human activity. It is also apparent that he did not intend the book to be the last word in the history of fermentation, but rather a cruise through the thoughts and actions of philosophers and scientists with respect to fermentation from the early Greeks to the mid-twentieth century.

A careful reading of the introduction is necessary to understand where the book is going and what the author is attempting to accomplish. Perhaps one sentence in the introduction (pp xiii-xiv) best describes Fruton's intentions, and perhaps excuses many of the shortcomings of the book:

> In this book, I offer a *sketch* of the usage in the Mediterranean world and western Europe of the terms fermentation and ferment (or their Greek, Latin, Arabic, or German equivalent) in alchemical efforts and in subsequent controversies about the nature of alcoholic fermentation.

The word "sketch," which I have italicized and underlined, is the operative word. Fruton is covering the mention of fermentation over time, and hitting the highlights in the nineteenth and twentieth centuries; but he apparently had no intention of presenting a coherent picture of the development of the modern theory of fermentation.

After the short introduction the book has four chapters, each covering a period in the history of mankind, and a brief conclusion. Fermentation, the action of yeast in the making of wine, beer, and bread and in the processes of digestion and putrefaction has been important as both a useful process and a subject for speculation and study from the early Greeks to the present. The first chapter, only 15 pages long, covers Aristotle to Paracelsus, a period of almost 2,000 years (400 BC to 1600 AD). Essentially it covers the mention of fermentation or ferments over this period of time. Fruton quickly takes us through some of the thought of Aristotle on fermentation, the influence of Aristotle on the alchemists, the translation of the Greek scientific literature into Arabic and eventually into Latin, the use of the terms fermentation and ferment in describing the transmutation of metals, and a little of the history of Paracelsus and the Paracelsians. The second chapter, titled van Helmont to Black, covers the 17^{th} and much of the 18^{th} centuries. It begins with Joan Baptista van Helmont, a Flemish physician and the most important of the Paracelsians, who had a strong influence on the thinking of a number of English physicians and on Robert Boyle. Fruton covers the speculations regarding fermentation of many of the premier thinkers, scientists and physicians of this period, including Francis Bacon, René Descartes, Robert Boyle, Isaac Newton, Herman Boerhaave, and Johann Bernoulli. With the invention of the pneumatic trough by Stephen Hales, it became possible to trap gases evolved in chemical reactions. This led to Joseph Black's discovery of "fixed air," carbon dioxide, which was later identified as the gas evolved during vinous fermentation. Henry Cavendish did some crude quantitative work on the fermentation of brown sugar. During these two centuries the picture of fermentation had evolved from a mystical view to a more mechanistic view.

In the third chapter Fruton covers the period from the late 18^{th} century and the classic work of Antoine Lavoisier on fermentation to the late 19^{th}-century work of Emil Fischer on the chemical structure of the sugars and the action of enzymes. In the century between the efforts of these two chemical giants, there was the work of many in discovering that yeast was a living organism and finding a host of soluble "ferments" that eventually came to be known as enzymes. Some of the best known contributors were Justus Liebig, Friedrich Wöhler, Jons Jacob Berzelius, Theodor Schwann, and, of course, Louis Pasteur. These discoveries raised the issue of vitalism versus chemical processes, although Fruton says very little of this debate.

Chapter Four, *The Buchners to the Warburg Group,* covers much of the work of the early 20^{th} century leading to the Embden-Meyerhoff-Parnas (EMP) pathway for the yeast fermentation of glucose to ethanol and CO_2. Although this chapter contains an enormous amount of information regarding the work of the Buchner brothers, Arthur Harden, Otto Warburg, Otto Myerhoff, Gustav Embden, and Jacob Parnas, and it deals with the importance of the isolation and purification of the enzymes, it does not present a logical development of the theory. This should have been a chapter in which the evidence built inexorably to a grand conclusion, and that just does not happen. We are left wondering how the biochemical community ever arrived at the final pathway.

The brief conclusion does not offer any relief. If anything, it leaves us wondering once more what Fruton had in mind for this book.

There is an extensive bibliography, a very complete index of personal names, and an almost nonexistent subject index. If nothing else, this book should prompt some enterprising young historian to write the really exciting story that the scientific work demands. *Leon Gortler, Brooklyn College*.

Max Perutz and the Secret of Life. Georgina Ferry, Cold Spring Harbor Laboratory Press, Woodbury, NY, 2008, 352 pp, ISBN 978-0-87969-785-3, $39.

One might quibble with the title as a bit melodramatic, but, in every other respect, Georgina Ferry got it right. Even the title is based on Perutz's 1936 question to British crystallographer Desmond Bernal, "How can I solve the secret of life?" Bernal replied, "The secret of life lies in the structure of proteins, and X-ray crystallography is the only way to solve it."

Perutz summoned Georgina Ferry to his bedside shortly before his death from cancer in 2002. He wanted her to write his biography, probably because she had written a biography of Dorothy Hodgkin, another Nobel crystallographer, and he knew she understood the discipline and could relate it to nonspecialists. Ferry responded with an engaging read. While the book is largely chronological in presentation, Ferry freely steps back in time with almost every chapter to develop a particular theme. The result is an insightful look at Perutz's life and work and the role he played in what was arguably the most productive collaboration of scientists in twentieth-century molecular biology. Max Perutz was born into a Jewish family of the moneyed, educated Viennese society. He was a sickly child. In fact health issues dominated his life, leading, especially in his later years, to renowned eccentricities. He did manage good health during his early adulthood, becoming an excellent skier and mountaineer.

After studying chemistry at the University of Vienna, he left for Cambridge in 1936, only two years before Hitler's *Anschluss*, the reunification of Austria and Germany. Perutz had money, so an offer for a graduate position was easily obtained. While Bernal attracted Perutz to X-ray crystallography, his cousin's husband, Felix Haurowitz, professor of biochemistry at the German University of Prague, enticed Perutz into his life-long pursuit of hemoglobin. Perutz shared the 1962 Nobel Prize in Chemistry with his former student and colleague John Kendrew—Perutz for the structure of hemoglobin, Kendrew for the structure of myoglobin. Perutz did not stop there. Perhaps his best work was developing a mechanism for how hemoglobin functions, in his words, as a molecular lung.

Ferry guides the reader though this pursuit. Much of the book follows the painstaking process of elucidating the hemoglobin structure. Perutz was criticized for selecting such a complex molecule for study. Gradually, improved methods and techniques gave better and better glimpses of the structure. Ideas came from many other sources: Linus Pauling, Francis Crick, Lawrence Bragg, John Kendrew, and Michael Rossman. In each case Perutz incorporated those ideas into the next set of experiments. One of Perutz's strengths was to take ideas, even when delivered as criticism, and put them into play. For example, when Cal Tech's Linus Pauling postulated an α-helix structure for proteins, Perutz immediately recognized support for the idea from his X-ray patterns. That led Perutz to postulate a model for the hemoglobin structure. In a seminar setting, Francis Crick completely demolished Perutz's model. Perutz was not offended and set about to devise new experiments. That reaction typified Perutz in the early years. He knew he lacked the mathematical abilities of the physicists. Crick later suggested isomorphous replacement, whereby a marker atom is incorporated into a structure without altering its three-dimensional folding pattern. Perutz used mercury atoms for a critical breakthrough in protein imaging.

John Kendrew began studying the smaller myoglobin molecule. Michael Rossman joined the group as a programming expert, which led to even better results. In fact, it was Rossman who saw that hemoglobin looked like four myoglobins. Perutz immediately began to build a model.

The Nobel Prize gave Perutz new confidence. His own best thinking appears to have been in obtaining images of oxygenated and deoxygenated hemoglobin and subsequently postulating the oxygen binding mechanism. Whereas during the structure years, Perutz lacked a certain amount of self-esteem, he now defended his binding mechanism like a bull dog.

Ferry does not limit the story to hemoglobin. During Perutz's graduate student years, Bernal left Cambridge for London, but Perutz stayed behind. To Perutz, science and Cambridge were inseparable. Sir Lawrence Bragg came to the Cavendish Laboratory and became Perutz's new champion. The *Anschluss* changed Perutz from a visiting foreign national to a refugee. His source of income evaporated as his family fled Austria. Bragg, however, succeeded in securing support for Perutz from the Rockefeller Foundation. Perutz's parents managed to get to Cambridge. He was now supporting his family too, a matter complicated because his parents were not willing to live at the level he could now afford. Financial pressures led to new health issues. To supplement his income, Perutz engaged in periodic studies of glaciers, an opportunity that arose from his crystal expertise and his mountaineering abilities.

In 1940 Perutz received his Ph. D. for X-ray studies on hemoglobin. Less than two months later, British authorities took him into custody and sent him to internment camps in England and Canada. It took nine months for family and colleagues to gain his release.

All British scientists had war work in addition to their normal scientific pursuits. Perutz's glacial experience led to an involvement with the Habbakuk project, Lord Louis Mountbatten's attempt to build aircraft carriers from hybrids of wood and ice. It was eventually abandoned.

Bragg convinced the Medical Research Council to found the MRC Unit on Molecular Structures of Biological Systems, with Perutz as head. Hugh Huxley and Francis Crick were recruited to the Unit. Bragg himself joined Perutz in madly pursuing the hemoglobin structure.

James Watson joined the Unit. Watson and Crick were interested in DNA, but Bragg forbade it. Wilkins' group in London was working on that project, and a gentlemen's agreement did not allow competition with another MRC unit. Watson had already seen data during a visit to Wilkins' laboratory that suggested a helix pattern. Things changed, however, when Richard Pauling, a graduate student with Kendrew, indicated that his father, Linus Pauling, was also working on the DNA structure. To Bragg, losing to the Americans was not acceptable, so he relaxed his edict. As fate would have it, unpublished data from Wilkins' group came to Perutz as a member of the MRC Biophysics Committee. When Watson asked to see it, Perutz showed it to him. After all, it was not marked confidential. Crick immediately saw the anti-parallel helix pattern, and the rest is history. Fifteen years later, when that story came out, Perutz was criticized for the ethical lapse. At the time, however, he was very pleased that the unit got the structure for DNA … and the secret of life.

Perutz considered 1953 the *annus mirabilis*. Edmund Hillary conquered Mount Everest, Elizabeth II was crowned, Watson and Crick solved DNA, Huxley described muscle fiber contraction, and he got definitive hemoglobin patterns. The unit was on top of the world.

That same year Bragg moved to London to become Director of the Royal Institution. The new physics chair at the Cavendish did not share Bragg's devotion to the unit. It was moved from Cavendish into a hut and placed on borrowed time. Across campus, a similar fate befell biochemist Fred Sanger, who was supported by MRC grants but lacked regular faculty status. Sanger had published the structure of insulin, which was to lead to his first Nobel Prize in 1958. Discussions between Perutz and Sanger led to requesting the MRC to build a new laboratory for molecular biology. Perutz took the lead that resulted in the MRC Laboratory of Molecular Biology and a new facility in 1962, which he headed. That fall the number of Nobel Laureates rose from one to five as the chemistry prize went to Perutz and Kendrew and the medicine prize went to Watson and Crick.

Even at the unit, Perutz had followed the "Cambridge tradition of recruiting excellent people and letting them do what they wanted." At the LMB, a canteen was built on the top floor to facilitate discussions over tea or lunch. Perutz's greatest strength was in fostering those interactions. He said, "Creativity in science, as in the arts, cannot be organized. It arises spontaneously from individual talent. Well-run laboratories can foster it, but hierarchical organization, inflexible, bureaucratic rules, and mountains of paperwork can kill it." As of 2002, the year of Perutz's death, the LMB had produced twelve Nobel Laureates.

Ferry had access to volumes of Perutz's letters. She richly used them to accent the narrative. She paints a sympathetic picture of Perutz, but in no way glosses over his shortcomings. In most respects, she leaves it to the reader to interpret Perutz.

Max Perutz is probably not among the names most people associate with the champions of twentieth-century molecular biology. However, Georgina Ferry captures the essence of Max Perutz. He was not the intellect of the Cambridge revolution in molecular biology; he was the glue that held it together. Francis Crick said, "Max wasn't a particularly quick thinker. He was a plodder, but a persistent plodder, and he had considerable insight as a result of his plodding." Perutz was not threatened by the genius of people like Francis Crick, James Watson, or Sydney Brenner; rather he reveled in them and helped develop a system that allowed science to reap their collective benefits. He was the master at understated direction.
Joe Jeffers, Ouachita Baptist University, Arkadelphia, AR 71998-0001.

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*Cathedrals of Science: The Personalities and Rivalries That Made Modern Chemistry***.** Patrick Coffey, Oxford University Press, New York, 2008, xix + 325 pp, ISBN 9780195321340. $29.95.

This book is an enjoyable presentation of the evolution of physical chemistry during approximately the first half of the twentieth century. The focus is upon the personalities and rivalries of six dominant figures, S. Arrenhius, W. Nernst, G. N. Lewis, I. Langmuir, F. Haber, and L. Pauling, who made much of modern physical chemistry. There are as well as a number of others who interacted strongly with these principals in producing much of modern physical and theoretical chemistry. The geographic playing fields are primarily the United States, Germany, and Sweden. Winning is clearly defined by the

Nobel Prize in chemistry. The rules of the game were relatively straightforward: develop an important area experimentally and present a theoretical explanation for the observations. It should be noted that of the selected six only G. N. Lewis did *not* receive the Nobel Prize. An extremely readable review of this book, which properly stresses personalities and rivalries, has been given by Sam Kean [*Chem. Eng. News*, October 6, 2008].

The competition and rivalry characterized by the personalities of the players are certainly the theme of this work. It is more difficult to place this competition into the scientific mindsets of physical chemistry that existed during the periods of development in the approximately half century 1890 -1950. Understanding and determining *chemical affinity* play a fundamental role in the evolution of useful chemical thermodynamics. As noted by Lewis and Randall "….numerous applications of thermodynamics to physics and especially to chemistry. Here the methods of thermodynamics have brought quantitative precision in place of the old vague ideas of chemical affinity and thus chemistry has made the greatest advance toward the status of an exact science…" [G. N. Lewis and M. Randall, *Thermodynamics and the Free Energy of Chemical Substances*, McGraw-Hill, New York, 1923, 1st ed., 1923, 2.]

This history of leaders of physical chemistry presents discussions essentially in terms of concepts and language of their period rather than through the hindsight of contemporary vision. I have found that having Lewis and Randall, which was published midway through the period, at hand is extremely useful in strengthening the science basis of the development, as well as the language changes that have occurred.

The educational consequences of being at the forefront of evolving knowledge are clearly developed by Coffey. The American leaders Richards, Lewis, and Langmuir all studied in Germany with Nernst. Lewis, who is the central figure of this book, is the leader in the development of physical chemistry in the United States. He moved from MIT in 1912 to become chairman of the department of chemistry as well as dean of The College of Chemistry at the University of California at Berkeley, positions he held for thirty years. In this period Berkeley became the leading institution for the education of physical chemists. The faculty grew from within at Berkeley and developed under the unique personality of Lewis. This imprint remained at Berkeley long after Lewis' death in 1946.

The first Nobel Prize was awarded in 1901 to J. H. van 't Hoff for the laws of chemical dynamics and osmotic pressure. The personality conflicts among the protagonists are probably most glaring by the role of Svante Arrhenius (who won the 1903 Chemistry Nobel Prize for his electrolytic theory of dissociation). His personality and actions in blocking the award of the Nobel Prize to Nernst for fifteen years are spelled out in detail. G. N. Lewis never received the Nobel Prize, a point which occupies a considerable part of this book. Whether this is entirely a consequence of his personality and verbal communication skills is not clear to me. The contrast between Langmuir and Lewis in this respect is quite dramatic. Langmuir is a self-taught alpine skier, while Lewis is a chain smoker of cheap Philippine cigars. Of considerable importance to Langmuir was the General Electric Research Laboratory, led by Willis Whitney, where basic research was valued. The description of the invention (discovery?) of the inert gas-filled incandescent light bulb by Langmuir is delightful and important, showing that skillfully planned basic research before full-scale industrial production is a very cost effective step.

The history of physical chemistry is a large project, with many facets. This work of Patrick Coffey will remain an essential component in this project. The emphasis is on the people who were responsible for its development. The final chapters bring nuclear chemistry and isotopes, especially deuterium to the science of physical chemistry. The presentation of the very altruistic personality of Harold Urey, the discoverer of deuterium, is especially enriching. In summary, this book by Patrick Coffey is an enjoyable read. It furthermore stimulates the desire of the reader for further professional history of physical and theoretical chemistry providing clear delineation of the science development associated with the developers. Chemistry is a rich science, frequently called the central science (by chemists) by its place between biology and physics. This book is thoroughly documented. It sets a professional standard for the further historical analysis of the evolution of physical and theoretical chemistry. It is difficult in a short review to fully expose the richness of this text. *William Klemperer, Harvard University.*