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William B. Jensen
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THE 2005 EDELSTEIN AWARD PAPER

TEXTBOOKS AND THE FUTURE OF THE HISTORY OF CHEMISTRY AS AN ACADEMIC DISCIPLINE*

William B. Jensen, University of Cincinnati

In order for history of chemistry to remain academically viable it must be teachable and, in order to teach it, one must have suitable textbooks. By history of chemistry, I am, of course, referring to a specialty course directed specifically at chemistry majors, such as was traditionally taught within chemistry departments, rather than to a general history of science course taught within a history department and directed primarily at undergraduate nonscience majors, in which the chemistry component, if any, deals with its industrial and social impact rather than with its internal conceptual and experimental development. Though specialty courses in the history of chemistry were relatively common before the Second World War, they have become increasingly rare in recent years, due in no small measure to the indifference, if not active opposition, of the American Chemical Society (1). Indeed, if current trends continue, we may soon face the irony of having a Division within the American Chemical Society which deals with a subject that has neither an industrial nor an academic presence.

Having received my training in a chemistry department which still taught a traditional history of chemistry course and holding one of the few chairs in chemistry which explicitly requires that I teach such a course, I have long been preoccupied with the problem of finding suitable textbook material. Indeed, this preoccupation has finally driven me to the extreme of writing a textbook of my own and I thought that it might be of interest to

share with you, on the occasion of receiving the Edelstein Award, some of the considerations that lay behind the decision to take this step, as well as some of the problems and lessons which resulted (2).

The Nature and Function of a Textbook

Before describing the chemical issues involved, I should say something about what I consider the nature and function of a textbook to be, as well as a little about the audience it is intended to serve. Though the history of chemistry course which I took as an undergraduate at the University of Wisconsin was spread over two semesters, I am required to cover the same material in a single quarter or in roughly a third of the time (3). My clientele consists largely of chemistry majors in their senior year, with an occasional audit by a graduate student. In addition, the course attracts a few seniors and/or graduate students from the school of education, chemical engineering, the philosophy department, and the medical campus.

Since essentially none of these students has even a rudimentary background in the history of science, let alone in the history of chemistry, the course is intended to serve as an introductory overview or survey. Consequently, the first requirement of a suitable textbook is that it must sketch the evolution of modern chemistry in the broadest possible terms, a requirement that automatically limits the space that can be devoted to discussing the de-

tailed development of individual theories and experimental techniques, the larger political and social context of these discoveries, or the various philosophical issues that were involved. Rather the book must provide a skeleton framework of significant names, dates, and key historical transitions on which this detail can be arranged at a later date, whether acquired through the independent reading of specialist monographs in the history of chemistry or the taking of more advanced courses.

Like a reference work, a good textbook should be tightly organized in order to facilitate rapid access to significant names, events, and dates. Unlike a reference work, however, it must be selective, rather than comprehensive, in its coverage. This selectivity is constrained not only by the comparative importance of the various topics, but also by the fact that few chemistry departments are willing to devote more than a single quarter or semester to a history of chemistry course.

Like a specialist monograph, a good textbook should also provide some context for these names, events, and dates by pointing out significant trends and summarizing interpretive conclusions. Unlike a specialist monograph, however, it cannot present the detailed arguments supporting these trends and conclusions nor indulge in nuanced discussions of subtle distinctions or qualifications. In the interests of clarity and brevity, these summarized, albeit oversimplified, conclusions and characterizations must stand on their own.

Like a popular history intended for the lay public, a good textbook should be readable. Unlike a popular history, however, it does not shy away from using technical terminology, equations, and formulas, or from employing various organizational and scholarly devices, such as sectional headers, summary tables, graphs, and footnotes.

In short, what I wanted was neither a reference book nor an interpretive essay, but rather a survey which took a traditional internalist approach to the history of chemistry and was explicitly targeted at readers having a basic understanding of the principles and techniques of modern chemistry, rather than at nonscience majors or the lay public.

I point out these obvious distinctions between a textbook, on the one hand, and a reference book, specialist monograph, or popular history, on the other, because the textbook appears to be a literary form that has disappeared from the repertoire of the modern historian of science. This neglect is undoubtedly connected with the kinds of questions that are of most interest to professional historians. The type of introductory textbook

which I have been describing deals with only the most rudimentary of these: namely with the questions of when certain concepts and techniques became dominant in science and which scientists played a prominent role in that rise to dominance. However, if one asks the further question of how these concepts and techniques were actually discovered, then the situation rapidly becomes more complex and topics, which in an introductory survey consume only a paragraph and which mention only two or three names, suddenly expand to the size of chapters or entire books.

Further complications arise from the fact that modern historians are seldom content to base their accounts of scientific discovery on the published record, but rather seek to discover unpublished correspondence, journals, and laboratory notes which might shed further light on these questions. These unpublished documents are frequently fragmentary, lacking a proper context, and chronologically ambiguous, thus tempting the historian to unrestrained speculation. In addition, they often contradict the published accounts. Though common sense would dictate that these published accounts represent the author's final and considered opinion on the subjects in question, whereas the unpublished documents represent preliminary drafts or paths subsequently rejected, recent historians have tended to invert this view and to claim that the unpublished documents represent the true picture, whereas the published accounts are little more than official misrepresentations. As a result, the recent literature in the history of science has been deluged with highly speculative, and often quite questionable, revisionist accounts of major scientific discoveries (4).

If one moves beyond these questions to the further question of why a given concept triumphed over its competitors and became dominant at a particular time and place, rather than earlier, later, or elsewhere, then things move from the realm of speculation into the realm of acrimony. Attempts to answer such questions range from those who believe in the scientific method and that certain concepts triumph because of their superior explanatory powers, to those who advocate a strict social constructionist approach and maintain that the dominance of one concept versus another is merely a matter of intellectual fads dictated by the larger cultural milieu. Though common sense would suggest that the true reasons probably involve a mixture of these various factors, the proponents of these extremes have again generated a vast and problematic literature which often tells us more about the individual political and philosophical biases of the authors than about the nature of science itself.

Though indulging in speculation and controversy certainly makes for a more stimulating approach to the history of chemistry than does plodding through the introductory basics, I seriously question their use with students who lack the necessary factual background to evaluate independently the cogency of the arguments being offered and feel that they are out of place in an introductory textbook.

The Problem of Selectivity

There are, of course, still numerous older histories of chemistry in print which were written by chemists rather than historians and which essentially conform to the constraints outlined above. In particular, the histories by Leicester, Ihde, and the shorter history by Partington immediately come to mind, all of which are currently available as high quality Dover paperback reprints, as well as the recent history by Fruton (5-8). Indeed, I have at one time or another used all of these as textbooks, but uniformly found them to be unsatisfactory for a variety of reasons. In common with most other older histories of chemistry, they tend to suffer from one or more of the following defects:

1. They are often heavily biographical in their emphasis, thereby sacrificing conceptual, technical, and sociological insights for anecdotal trivia.
2. They seldom provide any substantive coverage of events after 1925, thus ignoring most of the history of 20th-century chemistry.
3. They often fail to provide overview summaries of significant trends that would allow the student to put names, events, and dates into proper perspective.
4. They often focus exclusively on the historical development of concepts related to the composition and structure of the discrete, stoichiometric, molecular species typical of organic chemistry, thereby ignoring or trivializing the equally

important advances made by such fields as solid-state chemistry, phase science, quantum chemistry, chemical thermodynamics, and chemical kinetics – advances which clearly reveal that the traditional molecular mind-set of the organic chemist and the introductory chemistry textbook actually correspond to special cases of a far more general set of chemical and physical concepts.

Anyone who has attempted to write a short overview history of chemistry or has glanced through James Partington's comprehensive, multivolume reference work (weighing in at four volumes and over 3,000 pages) soon becomes painfully aware of the reasons for the first and second of these defects (9). By the second half of the 19th century, the cast of characters and topics becomes overwhelming and, by the 20th century, almost impossible to deal with. Consequently the level of coverage becomes increasingly abbreviated, especially if the author attempts to provide biographical information, however brief, on the chemists and physicists that are mentioned.

Ruthless selectivity becomes essential, though this process automatically produces an historical distortion by associating experimental and conceptual advances with only one or two selected names or dates, when in fact they were really the result of a long evolutionary discovery process and an equally long post-discovery refinement process, each of which involved the cooperative efforts of many chemists and physicists. A closely related consequence of this selectivity is that it also produces a distorted impression of the day-to-day activities of the average chemist. Only a small fraction of the chemical community is privileged to have made a significant conceptual or methodological contribution to chemistry. The vast majority spend their careers applying and refining the concepts and methods discovered by others – a characterization that even includes many activities that were later honored by a Nobel Prize. Work of this sort is absolutely essential to the progress of science and often involves great skill,



William B. Jensen
2005 Edelstein Award

persistence, and brilliance, though, in the end, the big picture often condemns it to historical anonymity.

The most natural way of applying the requisite selectivity is to let time itself act as the ultimate arbiter of what is to be included and what is to be ignored. In other words, one focuses on the origins of only those advances which still have significance to modern chemistry. This criterion has long been an anathema to professional historians, who claim that all events – even those which qualify as crank science – should be given equal and impartial treatment. To do otherwise is to commit the deadly historical sin of “Whig history.” I need hardly point out the incredible naiveté of such a position when it comes to the reasons that motivate most people to either write or read history, let alone the overwhelming impracticality of putting such a plan into practice when dealing with a broad range of topics and time periods—an impracticality which no doubt accounts for the failure of said historians to provide any comprehensive histories of chemistry themselves.

Using the present to select the past also has the shortcoming that the selection process is highly dependent on the author’s understanding of the present. What I or some other chemist might deem as historically important will vary with our current understanding of chemistry and with what we consider to be its most fundamental achievements. Indeed, it has been my personal experience that it is virtually impossible to get any two chemists to agree on just what constitutes the most important and most basic principles of chemistry and that they often mistake their areas of specialization or practical industrial applications for fundamental generalizations. This lack of consensus is largely responsible for the third and fourth of the above defects. It is difficult to formulate broad perspectives if one cannot agree on what is important, and most past histories of chemistry have been written by organic chemists with a limited appreciation of the achievements of phase science and solid-state inorganic chemistry.

In my teaching I have found that I not only have to repeatedly commit the historical sin of using the present to select the past, I also have to violate the injunction of said historical theorists against using our current knowledge of chemistry to help clarify and evaluate older theories. If possible, this taboo is even more unrealistic than the first. As even a rudimentary knowledge of educational psychology shows, people do not assimilate new information in a vacuum, but rather seek to integrate that information with their previous knowledge. Historians may have

the luxury of assuming that their readers or students are blissfully ignorant of modern chemical theory, but I do not. It is simply impossible to present outdated chemical theories and terminology to an audience of trained chemists without them automatically asking themselves “but what is really going on here?” and attempting to evaluate that theory or terminology in terms of their current knowledge of modern chemistry. Either the teacher or author can attempt to control this integration process by explicitly pointing out the differences and similarities with our current views or allow each individual to do so on their own—a process which can lead to some very bizarre distortions and misinterpretations, as repeated studies by science educators have shown (10).

In the end, it all boils down to the question of just how seriously one should take historians who claim that there is only one legitimate set of historical interests (by which they usually mean the political and sociological context of scientific discovery) and only one legitimate method of writing history, and who furthermore base these claims on the highly dubious proposition that the writing of political history can serve as a legitimate model for the writing of history of science (11). Despite their strident claims to the contrary, science, unlike politics, does progress and we really do know more about the nature of the physical universe in the 20th century than we did in the 15th century, even if we are no wiser when it comes to the motives of the human heart. Historical hindsight is simply not the culprit it is made out to be. Indeed, it can be plausibly argued that hindsight is the only thing that differentiates history from mere chronology.

Making Some Choices

Having also experimented with various organizational approaches, I eventually concluded that a simple century by century chronological approach was best for the type of survey course I had envisioned. Of course, every historian knows that the start and finish of significant historical eras seldom coincide with the turn of a decade, a century, or even a millennium. Yet there is something in the human psyche which endows these arbitrary dates with a special significance and which makes us want to pause and evaluate where we have been and where we are going. More importantly, however, I found that use of these purely conventional time divisions seemed to facilitate mastery of the names and dates required to construct our basic historical framework, whereas students found more sophisticated approaches based on significant eras or themes chronologically confusing.

In the interests of brevity I also decided to restrict my coverage per century to three themes or historical indicators:

1. Professional Development
2. Experimental Techniques
3. Conceptual Content

The category of professional development was intended to subsume the state of chemical training or education, the development of scientific societies and other professional organizations, and the evolution of a distinct chemical literature, including textbooks, monographs of various sorts, journals, abstracting services, etc.

The category of experimental techniques was intended to subsume advances in instrumentation and apparatus, the development of new experimental procedures, and the discovery of new classes of reactions and compounds. Though it might seem odd, at first glance, to include reactions and compounds in this category, the discovery of a new reaction or synthetic procedure can be as productive of new experimental results as the invention of a new instrument, and new classes of compounds may challenge existing theories of bonding and structure as effectively as quantitative data measurements.

Lastly, the category of conceptual content was intended to subsume not only theories proper, but also definitions and nomenclature – in short, all of those aspects that contribute to the organization and interpretation of experimental data.

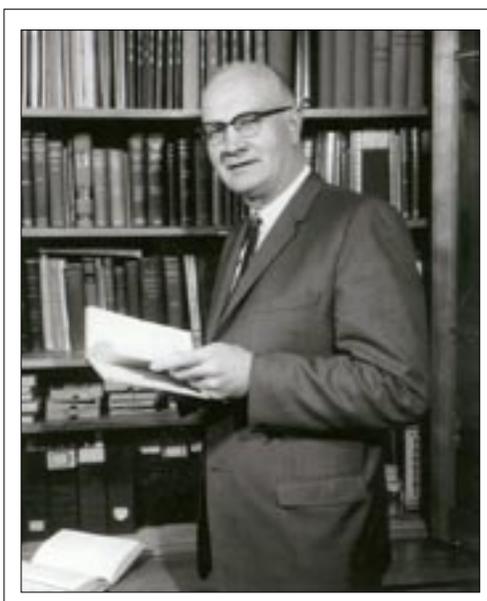
Of course, these three indicators are not completely independent of one another. New experimental techniques and theories often lead to the development of new specialties at the professional level, whereas professionalization leads to the sharing of experimental data, provides much of the driving force for funding basic research, and streamlines theory by enforcing shared standards of vocabulary and symbolism. Likewise, theory often suggests new instruments and aids in the interpretation of experimental results, whereas experiment, in turn, both confirms and challenges current theory. This interdependence means that, as one approaches the 20th century, it becomes increas-

ingly difficult to avoid some degree of repetition when separately discussing each indicator.

The necessity of brevity also required that I restrict my survey of experimental techniques and conceptual content to only the most fundamental advances common to all branches of chemistry, whether pure or applied. Consequently it was necessary to largely ignore the specific history of such applied fields as industrial chemistry, geochemistry, biochemistry, etc., most of which are the subject of an historical literature of their own. Similarly, biographical coverage was limited to names and occasionally to nationality and birth and death dates.

A final decision was to begin the survey in the 15th century and to extend it to the end of the 20th century. The choice of the 15th century as the starting point was dictated by the fact that it was essentially the latest date that could still be effectively used as a reference point for a brief overview of the technical heritage of the previous centuries, as it is only in the 16th century that we begin to see the stirrings of a significant change in this otherwise relatively flat chemical landscape. A perceptive reader will note that I used the term “technical heritage” rather than “alchemical heritage.” The reason for this is that I believe, despite the recent fad in the history of science, which purports to find the origins of virtually everything from Newton’s physics to Boyle’s atomism in the alchemical literature, that alchemy proper is not an important progenitor of modern chemistry, which instead clearly evolved out of metallurgy and pharmacy.

As mentioned earlier, much of this revisionist literature is based on so-called “imaginative reconstructions” of manuscripts, personal interactions, and chronologies, as well as on the indiscriminate use of the terms “alchemist” and “alchemy” to describe any chemical activity that suits the thesis at hand. While it is true that, etymologically speaking, the words chemistry and alchemy are one and the same, the restricted use of the Arabic form to describe only those activities dealing with the improvement or transmutation of metals is a tradition which dates back at least to the 17th century and one which I also maintain.



Aaron J. Ihde, 1968 Dexter Award

In contrast to the technical and philosophical writings of the Greeks and Romans, the genuine alchemical literature, with its rampant use of allegory, its intentional obfuscation, and its pervasive forgery of dates and names, clearly has more in common with the occult and religious literature than with the literature of secular philosophy, technology, and science. It is alchemy's commitment to these practices, rather than its belief in the transmutation of metals or the elixir of life, which clearly places it outside the history of science proper.

Adoption of this point of view leads to a critical reassessment of the nature and role of certain key protochemical documents. Thus, in keeping with the opinion of Cyril Stanley Smith, I have chosen to view the famous Stockholm and Leyden X papyri as part of a continuous tradition of practical recipe books intended for the use of metal workers and artisans, rather than as proto-alchemical documents, as they have been traditionally portrayed since the work of Berthelot at the end of the 19th century (12).

As for the often repeated claim that the alchemists, despite their obscure writings and questionable theories, developed important pieces of equipment and accidentally stumbled on many new substances, it is, in my opinion, far more likely that they either borrowed or adapted these from the metallurgical and pharmaceutical practice of their day. Though occasional consultation of the alchemical literature is useful for descriptions of common apparatus and chemicals when filling in the gaps in the technical and pharmaceutical literature, the necessity for this increasingly disappears after the 15th century.

Some Historical Lessons

Perhaps the primary historical lesson I learned from writing the book was just how difficult it was, given the necessity of ruthless selectivity, to decide just which 20th-century advances to include and which to ignore. This editing process has long been accomplished for the 18th and 19th centuries and most modern readers, unfamiliar with the chemical literature of these two periods, have no idea of the vast numbers of books and journal articles that this editing process has consigned to permanent historical oblivion. In the case of the 20th century, however, there are many readers still alive who are well aware of the literature extending back at least as far as the 1930s and an author runs the risk of violent disagreements over his unilateral choices of what to include and what to ignore. Indeed, though the book was intended to cover the entire 20th century, I found it almost

impossible to pass a reasonable historical judgment on events less than 25 years old, so in effect little is said of developments after 1980.

As already indicated, I found that surveying my fellow chemists was not very helpful in this regard, as most were so focused on their narrow research specialties that they totally lack a basis for making reasonable value judgments about what was or was not fundamental to chemistry as a whole. A more helpful approach was to look at which concepts and techniques had made it into the textbooks, as this was ostensibly an indication that the chemical community had found them important enough to pass along to the next generation. One consequence of this procedure is that the resulting survey places a much heavier emphasis on the publication of significant textbooks and monographs than has been the case with most previous general histories of chemistry.

A second historical lesson was the realization of how closely coordinated the histories of physics and chemistry have been for the last 400 years. It has usually been assumed that this close connection was a development of the late 19th century and has been fully operative only throughout the 20th century. But in fact chemistry and physics have shared the same general assumptions about the nature of matter since at least the 17th century, though chemistry has often exhibited a lag time relative to physics and has often partially modified the shared model to suit its own purposes. The change from hylomorphic models of form and matter to static hylomeric models based on the size, shape, and mechanical entanglement of discrete corpuscles is common to both in the late 17th century; the switch to static dynamical models based on short-range Newtonian interparticle forces and assorted imponderable fluids is common to both in the 18th century; the change to kinetic molecular models and the laws of thermodynamics is common to both in the 19th century; and the switch to electrical models of matter and interaction is the common denominator in the 20th century.

Some Philosophical Lessons

Perhaps the single most important philosophical lesson gleaned from writing the book was the realization of how removed these shared models of matter were from the details of day-to-day experimental work in chemistry. What I mean by this statement is best illustrated by a T-shirt which I saw during a recent visit to Oberlin College. On the front of the shirt was printed the observation:

$$\Delta S \geq 0, \text{ All the rest is mere detail}$$

But that is just the point: the devil is in the details. Formulating a nice concise mathematical statement of the second law of thermodynamics as a fundamental principle of nature is fine and good, but it tells you nothing about how to quantitatively apply this law to a specific chemical system. This requires the development of detailed application models which allow one to calculate the entropy changes for specific systems. Contradictory experimental results cannot be taken as a disproof of the second law, but are far more likely to be a disproof of the application model or one of its underlying assumptions. The same is true of the laws of quantum mechanics. The approximations used in applying these laws to chemical calculations are often quite extensive. Again, conflicts with experimental results are always assumed to be a reflection of defects in the application model and not a direct test of the laws of quantum mechanics themselves. Indeed, chemists often have to take the results of quantum chemical calculations with a grain of salt, as presumed general conclusions about the nature of chemical bonding, the details of the electronic structure of molecules, etc. derived from these calculations may not be fundamental at all, but merely artifacts of some nonfundamental assumption of the application model.

The point here is that history shows that chemists are seldom involved in the direct experimental testing of truly fundamental physical laws, but rather are largely occupied with the development and testing of approximate application models, and with questions concerning their accuracy and range of application. The models and theories of science are hierarchical in nature. Those highest in the hierarchy are seldom subject to direct experimental testing. Rather it is at the lower levels of approximate application models that the day-to-day give and take between theory and experiment, much beloved of the philosopher of science, largely takes place.

Some Pedagogical Lessons

Having tested the book in manuscript form for the last three years in my history of chemistry course at the Uni-

versity of Cincinnati, the question naturally arises as to how successful it has been. While I am perfectly happy with the amount of material, which is not excessive for a one quarter survey course, and feel that it provides a much more comprehensive overview of the development of modern chemistry than the previous texts I have tried, I must confess to some disappointments. Experience has shown that most of the chemistry majors taking the course, having survived four years of undergraduate training without being required to memorize any descriptive chemistry, are also extremely resistant to the idea of having to memorize any historical facts. Though I have made a great effort to structure the book and the exams as closely as possible around the types of experiences they have encountered in their chemistry courses, they almost

universally lack the ability to assess and master large amounts of verbal information. In this regard, the philosophy majors, though lacking as extensive a chemical background, beat the chemistry majors hands down, and the same is largely true of the engineering students who take the course. Indeed, the only students who do more poorly than the chemistry majors are the education majors.

I will resist commenting on what this says about the defects of the recommended ACS curriculum for chemistry majors or about the quality of the students which it attracts. However, I will note that the ACS has repeatedly proven to be an obstacle in other ways. Since the course covers the development of chemical kinetics,

thermodynamics, and quantum mechanics, it has physical chemistry as a prerequisite and is consequently counted as an advanced chemistry credit by the department. This is in fact the primary reason many of our seniors take the course, as they are looking for some relief from the excessive number of laboratory courses they would otherwise have to take their senior year to fulfill the ACS advanced requirements. However, despite this prerequisite and the disturbingly poor performance of our seniors in the course, the ACS has repeatedly refused to support the department's decision to count it as an advanced credit and consequently they have seriously jeopardized its continuing existence. A similar move was taken by the ACS a number of years ago when they rejected Mary



James R. Partington, 1961 Dexter Award

Virginia Orna's attempts to introduce my suggestions for how to structure an introductory chemistry course around the history of chemistry (13). Hence the source of my earlier remarks that the ACS may well prove to be the primary driving force for the demise of history of chemistry as an academic discipline.

REFERENCES AND NOTES

* As part of the Edelstein Symposium held at the 230th Annual ACS Meeting in Washington DC, 30 August, 2005, Dr. Jensen gave the talk, "An Illustrated Tour of the Oesper Collections in the History of Chemistry." Since this involved an informal commentary on more than 80 slides illustrating the Oesper Book, Journal, Print, Photo, and Apparatus Collections at the University of Cincinnati, it was felt that it was impractical to reproduce the flavor of the presentation within the usual format of the *Bulletin*, and Dr. Jensen has requested that he be allowed to substitute the above remarks instead.

1. The most recent survey of history of chemistry courses I am aware of is given in K. G. Everett and W. S. DeLoach, "Who is Teaching History of Chemistry?" *J. Chem. Educ.*, **1987**, *64*, 991-993.
2. W. B. Jensen, *Philosophers of Fire: An Illustrated Survey of 600 Years of Chemical History for Students of Chemistry*, Oesper Collections, University of Cincinnati, Cincinnati, OH, 2003. This is the manuscript version tested in class.
3. The course at Wisconsin was taught by Aaron Ihde and formed the basis of both his published text, *The Development of Modern Chemistry* (see below) and the unpublished manuscript, *The Dawn of Chemistry*.
4. For a particularly egregious example, see T. Tucker, *Bolt of Fate: Benjamin Franklin and his Electric Kite Hoax*, Public Affairs, New York, 2003, in which the author attempts to show that Franklin never performed his famous kite experiment, but rather fabricated it in an attempt to indirectly murder his rival, the British electrician, William Watson, whom he suspected of plagiarism.
5. H. M. Leicester, *The Historical Background of Chemistry*, Dover, New York, 1971. Original edition published by Wiley in 1956.
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8. J. S. Fruton, *Methods and Styles in the Development of Chemistry*, American Philosophical Society, Philadelphia, PA, 2002.
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10. See, for example, M. McCloskey, A. Caramazza, and B. Green, "Curvilinear Motion in the Absence of Forces: Naïve Beliefs about the Motion of Objects," *Science*, **1980**, *210*, 1139-141.
11. For a good critical commentary on the concept of Whig history, see A. R. Hall, "On Whiggism," *Hist. Sci.*, **1983**, *21*, 45-59.
12. C. S. Smith and J. C. Hawthorne, "Mappae Cavicula: A Little Key to the World of Medieval Techniques," *Mem. Am. Philos. Soc.*, **1974**, *64*(4), 1-128.
13. The suggestions referred to were based on the series of papers, W. B. Jensen, "Logic, History, and the Chemistry Textbook," *J. Chem. Educ.*, **1998**, *75*, 679-687; 817-828; 961-969.

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All photographs courtesy of the Oesper Collection, University of Cincinnati.

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CHARLES JAMES, B. SMITH HOPKINS, AND THE TANGLED WEB OF ELEMENT 61

Clarence J. Murphy, East Stroudsburg University of Pennsylvania

Introduction

The history of the search for and discovery of Element 61 is one of the most complex and confused of any of the elements in the periodic table. Certainly no element has been “discovered” and named more times than 61. At least seven claims for discovery were made and 61 has been named at various times illinium, florentium, cyclonium, and promethium. The story of element 61 is also intimately connected with the development of the understanding of atomic structure and of the Periodic Table, and of advances in science and technology in the late 19th and early 20th centuries. The story involves Roentgen’s discovery of X-rays and Moseley’s use of X-ray spectra to determine atomic numbers. It involves the more than one hundred-year effort to separate the rare earths and to find a place for them in the Periodic Table. Finally it involves the development of ion-exchange chromatography and research on the atomic bomb during World War II. Element 61 was named prometheum in 1946 by its discoverers Coryell, Marinsky, and Glendenin after the Titan Prometheus, who stole fire from the gods and was sentenced to eternal torment for the crime, as a warning that atomic energy could be the savior or the destroyer of humankind. The spelling was later changed to promethium by IUPAC.

The story of Element 61 also involves highly competent, careful investigators who searched for and

claimed they found an element that almost certainly does not exist in nature.

James and Hopkins

Two American chemists, Charles James (1) of the University of New Hampshire and B. Smith Hopkins (2) of the University of Illinois were involved in the controversies that surrounded claims of discovery for element 61 in the 1920s.

The conventional wisdom on Professor James’s contributions is probably best summarized by a quote from an article on Element 61 by Gould, which appeared in *Chemical and Engineering News* in 1949 (3):

When Hopkins made his announcement in March 1926, James and Fogg of the University of New Hampshire had just completed their fractionation of ytterspar and had sent the 61-rich concentrate to Cork at the University of Michigan for X-ray analysis. The results were reported in December, but by this time the controversies over the other three claims were in full swing, and the fourth entry went almost unnoticed in spite of the fact that the evidence was perhaps better than that of any other claimant. Probably contributing to this neglect was the fact that the announcement was published in a relatively obscure journal (*Proceedings of the National Academy of Sciences*)...Seven lines of the L series, falling between the corresponding lines of elements 60 and 62, were observed in the X-ray spectrogram, which accompanied the announcement.

To date, no other X-ray spectrogram of element 61 has been published, and while James' work has never been successfully repeated, neither has it been denied or repudiated.

Gould's view of James's role in the element 61 controversy has been repeated by other authors (4), or James has been ignored entirely (5).

The situation is actually much more complex as revealed by scrapbooks kept by Marion E. James, wife of Charles James (6) and recently catalogued by the University of New Hampshire Archives. They contain letters, which together with others in the University of Illinois Archives (7), shed light on the origins of James' search for element 61 and his relationships with B. S. Hopkins and W. A. Noyes.

The instigation of James' search for Element 61 is most probably a letter from Sir William Ramsay to Charles James dated February 26, 1912 (8). In this letter he points out that there are a number of wide gaps in atomic weights between adjacent known elements which may indicate a missing element. Among these gaps is one between neodymium and samarium.

Letters from Sir William Crookes (9) show that from early 1908 Crookes was analyzing rare earth samples spectroscopically in his private laboratory for James. One of these letters, written in 1913 indicates that James was searching for a new element in a sample of ytterbium. Crookes writes, referring to a letter of April 2 from James (10):

I shall be glad to photograph its (an ytterbium sample) spectrum and send you the results. I can point out to you what impurities it contains, but the actual measurement of the lines in any new element is a very tedious job. I am afraid I cannot undertake to give more than an approximate measurement (say to five figures) of any new lines.

A letter from H. E. G. Moseley to James, dated May 27, 1914, in which he requests a sample of thulia to replace the one which had been lost in the mail contains the lines (11):

I am most interested to hear of your systematic search for the missing Nd-Sm element. I have been unsuccessful in the few, rather rough, attempts to find the lines corresponding to it in the X-ray spectrum of a Nd-Sm mixture.

This indicates that James had a systematic search for element 61 underway before the publication of Moseley's second paper on atomic numbers (12), which showed

that element 61 was missing. Incidentally, Moseley apparently never received the sample of thulia or received it after his paper was submitted because the space for thulium (69), in his list of atomic numbers is vacant.

Why James published his paper on element 61 in the *Proceedings of the National Academy of Science* has been puzzling, since almost all of his 60 papers were published in the *Journal of the American Chemical Society* (13). The answer is provided in a letter to James from Arthur B. Lamb, editor of the *Journal of the American Chemical Society*. This letter dated April 26, 1926 reads (14):

Dear Professor James:
The enclosed manuscripts from Dr. Hopkins will, I am sure, interest you. Dr. Hopkins is naturally very desirous of getting them published promptly, indeed in the June number if possible. If you could give me your verdict on them promptly, I would be grateful.
Yours truly,
Arthur B. Lamb
Dictated.
Manuscripts by Drs. Hopkins and Yntema, enclosed.

James received this letter at the time he was waiting for the X-ray spectrum of his sample to be determined by Cork at the University of Michigan. He was now being asked to referee papers on the very subject he had been working on for probably fourteen years. The papers claimed discovery and proposed the name illinium for element 61 on the basis of evidence that seemed no better than that which he had declined to publish several years earlier. James apparently quickly gave a positive opinion because the two papers were published with the notations: "Received April 26, 1925; Published June 5, 1925."

Evidently James then submitted his paper on Element 61 to the *Proceedings of the National Academy of Science*, to avoid any conflict of interest. Since none of the authors was a member of the Academy, the identity of the transmitter has been a mystery. A letter dated October 25, 1925 to James from Karl T. Compton in Zürich, Switzerland supplies the answer. The letter reads in part (15):

I have transmitted your very interesting paper on Element 61 to Professor E. B. Wilson, editor of the *Proc. of the Nat. Acad.* I am sorry that the forwarding of your letter has caused some delay.

The paper was published without further delay in the December, 1926 issue of the *Proceedings*.

The Papers on Element 61 of Hopkins and James

It is perhaps useful to examine Hopkins' and James' publications to determine their experimental procedures and the reasoning which led to the conclusion that they had discovered element 61.

Hopkins' publication consists of two papers, "Observations on the Rare Earths XXII. Element No. 61 Part One. Concentration and Isolation in Impure State," by J. Allen Harris with B. Smith Hopkins (16) and "Observations on The Rare Earths XXII. Element No. 61 Part Two. X-Ray Analysis," by J. Allen Harris with L. F. Yntema and B. S. Hopkins (17). The two papers bound together under the title "Element Number 61 (Illinium)" by Joseph Allen Harris constituted Harris's Ph.D. thesis (18).

In a historical section of the first paper it is stated that since the time of Moseley's work, which definitely showed that an element should exist between neodymium and samarium, a number of attempts had been made to isolate the missing element. The authors then mention several unsuccessful attempts to isolate the unknown element from rare earth minerals. As confirmation of the existence of the element among the rare earths, they cite the paper by Brinton and James, who showed that, when the rates of hydrolysis of the rare earth carbonates were plotted against time, they were (19):

...generally spaced uniformly from praseodymium on, except after neodymium. At this point there was a distinct gap between the curves of that element and samarium.

In the Introduction, it is stated that in "early 1919 an agreement was entered into between the National Bureau of Standards and the University of Illinois for a comparative study of the arc spectra, especially in the red and infra-red regions of certain of the rarer elements and particularly members of the rare earth group." The

results were published in a series of articles in the Bureau of Standards Scientific Papers between 1921 and 1923. In one of these articles Kiess is quoted by Harris and Hopkins as stating (20):

A third table contains 130 lines of unknown origin which are common to both spectra (neodymium and samarium). These lines are of unknown origin and may belong to the missing element of order No. 61...

On the basis of this evidence L. F. Yntema conducted an extensive fractionation of neodymium and samarium materials using double magnesium nitrate salts. Because the solubilities of the double magnesium salts increase with atomic number, element 61 should concentrate in the fractions between neodymium and samarium. Examination of X-ray spectra, however, failed to show any evidence of element 61; but ultraviolet arc spectra of the purest samples of both neodymium and samarium gave lines common to both elements, which were somewhat stronger in intermediate samples.

Harris took over the project in 1923 and states (18):

...in view of the foregoing results it was considered that the logical place to search for Element 61 would be among the rare earths... Since the most extensive researchers in attempts to isolate this element had been using frac-

tional crystallization of the double magnesium salts as a means of separation, in which case it is natural to expect to obtain a concentration of No. 61 in those fractions intermediate between the neodymium and samarium and, since all had resulted in failure, three reasons as to the case presented themselves to us.

The paper goes on to detail the three reasons:

- 1) Element No. 61 might be extremely scarce, perhaps the most rare of the rare earths, and so only infinite fractionation of tremendous amounts of materials would result in separation.
- 2) Very little difference in solubility might exist between the double magnesium nitrates of either 60 and 61 or of 61 and 62, with the result that the element concentrates with one of its more plentiful neighbors.
- 3) The solubility of the double magnesium nitrate of element No. 61 might



B. S. Hopkins

be entirely unique in falling out of sequence with those of its congeners, and hence would not be found in the fractions intermediate between neodymium and samarium.

They conclude that 1 and 2 are more probable because of the extremely sharp separation between neodymium and samarium when the double magnesium nitrates are fractionated; "the lack of continuity in passing from neodymium and samarium in such a study as that made by Brinton and James; and the presence of new lines in the arc spectra in intermediate fractions." They then assume that 61 is concentrated with neodymium and 61 cannot be detected because its absorption bands are masked by the extensive absorption bands of neodymium. They also conclude that the concentration of 61 remains practically constant throughout the series of fractions and if the ratio is less than 1:1000, the detection by X-ray analysis cannot be relied upon.

At this point they refer to James's paper on the use of bromates to separate the rare earth elements (21). They note that James had shown that, when the solubilities of the rare earths are plotted against atomic number, there is a gradual decrease to europium followed by a gradual increase, as shown in a figure from James's paper. They further state:

Experience in this Laboratory has shown that the elements arrange themselves in the approximate order of solubility as follows: europium, samarium, gadolinium, No. 61, terbium and neodymium.

Thus it should be much easier to separate 61 from neodymium. Also 61 would probably concentrate with terbium, which has no interfering absorption bands, thus making it possible to detect absorption bands due to the presence of 61. They state that absorption spectroscopy is probably sensitive to one part per one hundred thousand, compared to X-ray spectroscopy that is sensitive to one part per thousand; thus it should be possible to detect the presence of 61 by absorption spectroscopy in amounts too small to be detected by X-ray spectroscopy.

In the experimental section that follows, the separation of a monazite residue donated by the Lindsay Light Company by more than 150 fractional crystallizations is described in great detail. The concluding step was the conversion of a fraction to bromates by the "James Method"

(22). After more than 70 fractional crystallizations of the bromates, a band at 5816Å was becoming stronger in some fractions while at the same time characteristic absorptions assigned to neodymium were becoming weaker. In addition, an absorption band at 5123Å, which had previously been assigned to neodymium, was much stronger than other neodymium bands.

At this point Harris and Hopkins make the following conclusion (16):

The detection of absorption bands at 5816Å and 5123Å confirmed our belief that we were dealing with a new element and increased intensity of these bands led us to hope that the new element had been concentrated sufficiently to enable us to identify it by means of X-ray analysis.

The second paper by Harris, Hopkins, and Yntema describes the construction of an X-ray spectrograph and the collection and interpretation of X-ray spectra. Of special interest is the statement (17):

A tube was also constructed on which many helpful suggestions were received from Professor Manne Siegbahn who was at the University of Illinois at that time, and to whom the investigators are indeed grateful.

Karl M. G. Siegbahn, at the time Professor of Physics at the University of Uppsala, won the Nobel Prize in Physics in 1924 for his contributions to X-ray spectroscopy. In 1924-25 he traveled extensively in the United States and Canada, delivering lectures at the invitation of the Rockefeller Foundation (23). Evidently one of his stops on his lecture tour was the University of Illinois, probably in 1925. Siegbahn designed and built vastly improved X-ray spectrographs, which allowed a large number of new series of X-radiations to be discovered.

Date	Fraction	Time of exposure	Observed wave lengths in Å.		Deviation from calculated values.
			L _α	L _β	
Dec. 16	S ₇₋₄	16 ¹ / ₄ hrs.	2.2800		+0.0023
Jan. 2	S ₇₋₄	24 ¹ / ₄	2.2800*		+ .0021
Jan. 24	S ₇₋₄	8 ¹ / ₄	2.2786		+ .0009
Feb. 10	BT ₁₁₋₁₁	12 ¹ / ₄	2.2803		+ .0026
Feb. 22	BT ₁₁₋₁₁	16 ¹ / ₄	2.2786		+ .0000
Mar. 6	BT ₁₁₋₁₁	15	2.2730		- .0047
		Av. value	2.2781		
		Value calcd. from Siegbahn	2.2777		
		Deviation	0.0004+		
Mar. 13	CT ₁₁	10 ¹ / ₄ hrs.		2.0770	+0.0040
		Value calcd. from Siegbahn		2.0730	
		* Plate rejected. Loose plate holder.			

The new precision technique developed by Siegbahn led to a practically complete knowledge of the energy and radiation conditions of the electron shells of the atoms and created a solid empirical foundation for the quantum-mechanical interpretation.

X-ray spectra of a number of fractions expected to contain element 61 gave results which were summarized in the table reproduced here (24).

Note the average of the observed wavelengths of the $L_{\alpha 1}$ lines is very close to the value "calculated from Siegbahn." Likewise the wavelength of the $L_{\beta 1}$ line is relatively close to the "value calculated by Siegbahn."

The authors also give a table of all the lines possible in the region other than 61 and offer reasons why all can be eliminated. The paper concluded with a summary in which they claim the discovery of a new element on the basis of: 1) The presence of 130 lines in the red and infrared and 5 lines toward the violet in the arc spectra which are common to both samarium and neodymium and which are stronger in intermediate fractions; 2) The presence in the intermediate fractions of absorption bands which become stronger as the characteristic bands of neodymium become weaker; 3) The presence of lines in the X-ray spectrum corresponding to the theoretical positions for $L_{\alpha 1}$ and $L_{\beta 1}$ of Element 61. On this basis they proposed:

...the name of Illinium with the symbol *Il* for this element in honor of the state of Illinois and of our university.

James's paper, with James. M. Cork of the Department of Physics at the University of Michigan as first author and Heman C. Fogg, at the time a graduate student and later a chemistry professor at New Hampshire as third author, is entitled "The Concentration and Identification of the Element of Atomic Number 61 (25). The paper opens with the lines:

In making measurements of the wave-lengths of the X-ray K emission lines for the rare-earth elements, very faint traces of lines corresponding to the K series of the element of atomic number 61 appeared on the plate with certain specimens of samarium (62) and neodymium (60).

With this line is a reference to a paper entitled "A Short Wave X-Ray Spectrograph and Some K Series Emission Wave-Lengths" (26). In this paper Cork describes the design of an X-ray spectrograph similar to one used by Rutherford and Andrade for gamma rays. Cork states that among the advantages of this spectrograph is the fact that all lines of the K series are obtained simultaneously

without crystal rotation and that the time of exposure may be greatly reduced by placing the crystal close to the source of the rays. Cork then describes the preparation of samples and the determination of the X-ray K spectra of the elements Ba (56) through Er (68). The source of the rare earth samples is not given, but they most likely came from James. A table gives the wave-lengths in X units of the K_{α} , K_{α} , K_{β} and K_{γ} lines. The lines of each series have an almost linear relationship with a break between Nd (60) and Sm (62). Included is a photograph of a plate containing the spectrum of praseodymium that contains sharp clear spectral lines of Pr as well as tin and tungsten which were used as calibration points.

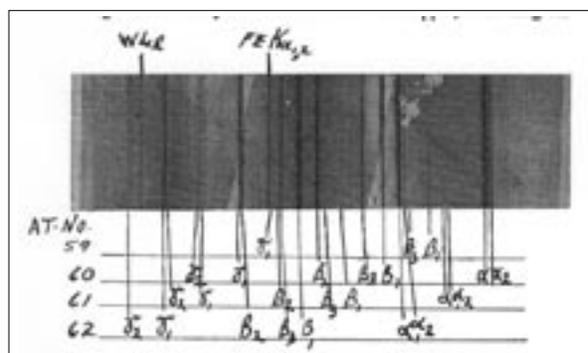
James's paper states that:

Many of the samples obtained have shown definite traces of lines where they should be expected (for element 61) in both the K and L X-ray regions but the photographic spectra have until now never been strong enough to permit photographic reproduction in a printed article and this was deemed necessary before the announcement of the discovery of the element was made.

A program, beginning in 1923 to examine large quantities of gadolinite, ytterspar, and monazite for element 61 is then described. It is stated that:

...the minerals were decomposed and separated in the usual methods of fractionation. In the case of gadolinite and monazite this Nd-Sa (sic) portion had to be put through an exhaustive fractionation before even the faintest line of 61 could be discerned. On the other hand, the ytterspar material gave faint lines after just a few crystallizations.

A reference is then made to James's 1914 article on terbium, where faint absorption lines were observed in fractions coming between neodymium bromate and less soluble gadolinium bromate, which were assigned to neodymium (27). This was shown to be a false as-



X-ray diagram from Ref. 25

sumption since further work has shown that neodymium bromate is more soluble than terbium bromate. A careful reexamination of the fractions of bromates more soluble than terbium showed a complete absence of neodymium absorption bands. From this the authors conclude that the faint absorption bands in addition to terbium in the terbium gadolinium fractions were due to the presence of element 61.

In an attempt to confirm the presence of element 61, a large quantity of Brazilian monazite sand was fractionated and the small amount of neodymium which accompanied the yttrium earths was concentrated by fractionally crystallizing the bromate and then the double magnesium nitrate salts. The neodymium fraction was expected to carry the major portion of element 61. This sample was sent to Cork for determination of the X-ray spectrum. The X-ray L spectrum was then obtained using a Siegbahn vacuum spectrograph. A photograph of the X-ray plate is shown with the various lines of elements 59-62 identified (28). They conclude that the sample contained the elements samarium (62), neodymium (60), praseodymium (59), a slight amount of cerium (58), and about 1 to 1.5% element 61. They state that, while there are more than twenty L series lines for each element, only about seven are fairly strong: the α_1 , α_2 , β_1 , β_2 , γ_1 , γ_2 lines (Siegbahn notation). All seven of the lines for element 61 lie approximately midway between the corresponding lines for elements 60 and 62, with the α_1 line being the strongest. By using the Siegbahn values for 60 and 62, the L wavelengths of the lines of element 61 are calculated as follows:

$$\alpha_1 2.289 \quad \alpha_2 2.279 \quad \beta_1 2.078 \quad \beta_2 2.038 \\ \beta_3 1.952 \quad \gamma_1 1.799 \quad \gamma_2 1.725$$

They then proceeded to eliminate all the possibilities of impurities that might give the observed lines which are attributed to element 61.

Cork had apparently obtained a Siegbahn vacuum spectrograph since he had previously determined the X-ray spectra for James in 1924 on a spectrograph similar to one designed by Rutherford. This Siegbahn instrument that could determine the entire

X-ray spectrum without rotating the sample was clearly superior to the Uhler spectrograph used by Hopkins that could only determine one X-ray line at a time by rotating the sample at the appropriate angle.

In 1949 W. F. Peed, E. J. Pitzer, and L. E. Burkhart, working at Oak Ridge National Laboratories, published the L spectrum of Element 61 in *Physical Reviews* (29). In this paper they compared the L spectrum of a sample of Element 61 isolated at Oak Ridge with the spectra obtained by James and Hopkins, as shown in the table:

It is striking that the six spectral lines reported by James and the two by Hopkins are remarkably close to those determined from an authentic sample of element 61.

Personal Relationships of James and Hopkins

Documents recently found in the archives of the University of New Hampshire and the University of Illinois reveal a previously unknown relationship between the careers of James and Hopkins. The revelation of this relationship began when a letter from W. A. Noyes, Chairman of the Chemistry Department at Illinois, to James dated March 23, 1916, was found among James's papers. In this letter Noyes offered James a faculty position at Illinois to supervise the General Chemistry program and to carry on the rare earth research at Illinois started by C. W. Balke (30).

A search of the Illinois Archives uncovered a number of interesting documents in the papers of B. S. Hopkins, W. A. Noyes, and the Departmental and Subject File of the College of Liberal Arts. One is a letter from Noyes to

Charles Parsons, Secretary of the ACS and James' predecessor as Chemistry Department Head at New Hampshire, requesting a reference for James (31). In this letter Noyes states that James is "almost the only man in the country who

could take up this work of Professor Balke and carry it on successfully without a break." There is also a letter

X-ray Diagram			
Line	This Laboratory	Cork, James and Fogg	Harris, Yntema, and Hopkins
L_{α_1}	$2287.9 \pm 0.4 \text{ \AA}$	2289	
L_{α_2}	2277.5 ± 0.3	2279	2278.1 ± 3.0
L_{β_1}	2075.4 ± 0.4	2078	2077
L_{β_2}	2037.9 ± 0.4	2038	
L_{β_3}	1951.8 ± 0.6	1952	
L_{γ_1}	1795.2 ± 0.9	1799	

Table from Ref. 29

from Noyes to Dean K. C. Babcock of the College of Liberal Arts and Sciences requesting permission to invite James for an interview (32). Unfortunately, there is no evidence what reply, if any, James made to this offer. Also among Noyes correspondence concerning a search for chemistry faculty in 1916 are letters which indicate that among those approached after James were Joel Hildebrand, Moses Gomberg, and A. B. Lamb (33). Interestingly, B. S. Hopkins was working for Balke at the time, and Noyes was writing to Babcock favorably about Hopkins in December 1916 (34). Apparently, Hopkins was appointed to the vacancy after the search outside the department had failed.

James and Hopkins were in fact friendly rivals, well aware of the other's work in rare earth chemistry, and carried on an extensive correspondence. James reviewed Hopkins's book *Chemistry of the Rarer Elements*, and Hopkins wrote James a letter stating that he appreciated the suggestions for improvements (35), to which James replied that he had liked the book very much and had recommended it to several people (36).

Hopkins visited James in New Hampshire in the spring of 1925. During the visit James took Hopkins to



Charles James

a small storage area where he kept samples of the materials he had prepared. James took down a bottle of one of the rarer earths to show Hopkins, who exclaimed, "Goodness, you are holding in your hand more of that material than exists anywhere in the world." James replied, "Oh, that's just an overflow bottle. I have others here of much larger size" (37). Related to this visit is a letter from James to Hopkins (38), in which James expresses his pleasure about Hopkins' visit and the hope of meeting again.

Apparently, Hopkins was not aware that James was searching for Element 61 before James published his paper because he does not list James in the groups who were "hot on its trail" in an interview that was published in "Eminent Albionia" in 1945 (39). James was also probably not aware of Hopkins' work until he received Hopkins' papers to referee.

That they acknowledged each other's work after publication is shown by two letters:

To be noted is the reference to Element 61, Illinium in the second paragraph. This letter was written while James

Letter from James to Hopkins (40):

August 17, 1926

Dear Professor Hopkins:

I am sorry to say that at the present time we have no man available who has specialized on rare earths.

I was interested in your work on Element 61, Illinium. We also have done some work in the past on this element. Our observations show that it occurs much more commonly in titaniferous xenotimes since the neodymium samarium fraction gives the lines immediately. We have a quantity of this material which we are working up.

Although we have examined a very large number of minerals which has been a tedious matter occupying years, we have come across nothing to equal the one mentioned above.

Yours truly,
C. James (sig)

Letter from Hopkins to James (41):

February 3, 1927

Dear Professor James:

I have just today seen a copy of your article in the December *Proceedings of the National Academy of Sciences* describing your work in the location of element No. 61. Permit me to offer my most sincere congratulation on the excellence of the work. I believe the men in our laboratory can appreciate fully how difficult the task has been and your success is very gratifying. Professor Cork's cooperation in X-ray spectrum work is timely and his photograph is splendid.

I rejoice with you in the successful outcome of the work.

With kindest personal regards.

Very sincerely,
B. S. Hopkins (sig)

was waiting for the X-ray spectrum of his sample of 61 to be determined by Cork at Michigan and after he had served as the referee for Hopkins's papers.

Hopkins wrote a letter dated February 3, 1927 to James, commenting on James's paper in the *Proceeding of the National Academy of Sciences*.

Perhaps others overlooked James's paper on Element 61 in the *Proceedings of the National Academy of Sciences*.

Dear Professor Hopkins:

We are still working on our bromate solubilities and expect to continue this work for some time yet. We are also running solubilities of some other compounds.

We are beginning to believe that the bromate method will not be used much in the future, since we have discovered a very much better process, giving great speed in separation.

Yours very sincerely,
C. James (sig.)

It is quite probable, however, that W. A. Noyes was aware of James's paper when he engaged in his polemical exchange of letters with Luigi Rolla in *Nature* in 1927 over priority for the discovery of 61 (42). Despite this James and Hopkins apparently continued to exchange polite letters concerning their research, as evidenced by a letter from James to Hopkins dated March 9, 1927 (43):

Despite the confidence shown by Noyes in his defense of Hopkins's claim for discovery of Element 61, apparently by 1928 he was having doubts about the experimental evidence. This resulted in a letter to James asking him to collaborate with Hopkins to achieve "a prompt and complete solution of this extremely difficult problem" (44). There is no evidence that James ever replied to this letter. In any event he was now terminally ill and would die on December 10, 1928. Hopkins had no doubts, however. He believed to the very end that he had discovered illinium (45).

Following James's death in 1928 his widow sold his collection of rare earth element samples to the National Bureau of Standards. Recently these samples have been returned to the University of New Hampshire, together with their inventory cards. Several of these cards indicate that samples were sent to Clement Rodden of the Atomic Energy Commission (a student of James) for examination. Cards for two samarium samples have a notation, "No trace of #61 with X-ray" (46).

What were the compositions of James and Hopkins's samples from which Cork and Yntema, respectively, obtained the published X-ray data? They could not have contained Element 61, for it has been found in nature only in trace amounts in uranium ores, as a product of uranium fission. If the spectral lines were not due to the presence of Element 61, where is the logical fault in either their reasoning or experimental procedures? There can be no definitive answers to these questions because Cork used James's entire sample for determination of the X-ray spectrum (47), and Hopkins's samples were apparently lost by Argonne National Laboratory (48).

Unfortunately, James's laboratory notebooks and correspondence have apparently been lost, the only examples being those saved by his wife and included in her scrapbook, now in the archives of the University of New Hampshire. No further information about the relationship between James and Hopkins was found in Hopkins's papers deposited in the Albion College Library Archives. However, an article in the *Chicago Tribune* reports that Hopkins sought "an unscheduled place on the program of the 112th national meeting of the American Chemical Society" to defend his claim to be the discoverer of Illinium (49). This was the meeting in 1947 at which Coryell, Marinsky, and Glendenin claimed discovery of element 61 and proposed the name promethium.

Conclusion

James and Hopkins were cordial rivals whose careers intertwined in unlikely ways. Both were considered for the same position in 1916; they were fellow investigators in the chemistry of the rare earths; they carried on a long standing scientific correspondence; were Alpha Chi Sigma fraternity brothers; and were rival claimants for discovery of element 61.

The Northeastern Section of the American Chemical Society, in which James was long active, published a memorial pamphlet, *The Life and Work of Charles James - 1880-1928*. B. Smith Hopkins wrote the essay "Charles James, the Chemist" for the pamphlet. The concluding paragraph probably best sums up James's contributions and Hopkins's evaluation of James (50):

Professor James was a prolific worker whose contributions to chemistry are both numerous and valuable. But no doubt the greatest professional contribution of his life was his quiet and kindly influence over the lives of his students. A list of his publications reveals the fact that he has been instrumental in the training of many chemists whose names stand high in chemical

circles. To train such men is to make a contribution whose influence is eternal.

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- B. Smith Hopkins (1873-1952), born Owosso, MI. A. B. Albion College, 1896, Ph.D. Johns Hopkins, 1906 (Morse). Professor, Nebraska Wesleyan (1906-1909), Carroll College (1909-1912). University of Illinois from 1912, Professor of Chemistry (1923-1941), Professor emeritus (1941-52). He studied the chemistry and separations of the lanthanides, beryllium, selenium, and tellurium.
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August 19-23, **2007**—Boston, MA
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August 17-22, **2008**—Philadelphia, PA
March 22-26, **2009**—Salt Lake City, UT
August 16-21, **2009**—Washington, DC
March 21-26, **2010**—San Francisco, CA
August 22-27, **2010**—Boston, MA
March 27-31, **2011**—Anaheim, CA
August 28-September 1, **2011**—Chicago, IL
March 25-29, **2012**—San Diego, CA
August 19-23, **2012**—New York, NY

CHEMICAL SOCIETIES AND THEIR JOURNALS: WHAT CAN BE LEARNED ABOUT THE STATE OF CHEMISTRY FROM AN ANALYSIS OF THE FIRST VOLUMES OF THESE JOURNALS

Martin D. Saltzman, Providence College

Introduction

What can we learn from the founding of chemical societies in the nineteenth century and an analysis of the first volumes of their journals as to the state of chemistry? Are these specific national phenomena or are there certain factors in common? Did chemical societies form because of a perceived need at the time of their founding or was it perhaps that a certain critical mass had been reached that led to the founding of the societies? What were the purposes set out by these societies for their journals? What influence did these journals have on the development of chemistry in their own country and for the chemical communities as a whole? These are a few of the questions that will be addressed in his paper. The societies that were analyzed are *The Chemical Society of London (1841)*, *The German Chemical Society in Berlin (1867)*, and *the American Chemical Society (1876)*.

The Chemical Society of London

In England the Royal Society had existed since 1660 (1), but because of the increasing specialization taking place in the natural sciences, specialist societies began to develop in the late eighteenth century. The first was the Linnaean Society of London (1778), and early in the nineteenth century in a relatively short time frame were founded the Geological Society of London (1807),

Zoological Society of London (1826), Royal Astronomical Society (1831), and the Chemical Society of London (1841). The Chemical Society of London is the oldest continuous chemical society in the world. In 1972 it merged with the Royal Institute of Chemistry, The Faraday Society, and the Society for Analytical Chemistry to form the Royal Society of Chemistry.

The most important influences on the development of British chemistry in the early nineteenth century were mainly external. British chemistry was influenced by the development occurring in Germany where new techniques of analysis and training were being developed by Justus Liebig at Giessen and Friedrich Wöhler at Göttingen. Of the two there is little doubt that the more important figure in shaping British chemistry was Justus Liebig (2). Bud and Roberts have remarked that (3):

He was a persuasive propagandist too, arguing both in Germany and abroad for the multiple utilities of chemistry. The aggressive and charismatic Liebig came to symbolize the powerful chemist to generations of chemists.

Liebig's emphasis on learning by doing in the laboratory served as an incentive for students from Britain as well as from other countries to come to Giessen. Among the contributors to the first volume of the *Quarterly Journal* here were seven who had Giessen connections. The most notable were the government chemist Lyon Playfair and the alkali manufacturer Sheridan Muspratt. By 1841

in Britain there existed a community of academic and professional chemists who saw the need for a society to serve all types of chemists. Thus the founding of The Chemical Society was a very practical one, whereas later societies, as we will show, had much loftier and idealistic aims.

Robert Warrington (1807-1867) was the guiding force in the call for the formation of a chemical society based in London. A chemist (pharmacist) by training, he had held positions as a brewery chemist, chemical operator for the Society of Apothecaries, and author of the first edition of the British Pharmacopoeia. He served as secretary of The Chemical Society from its founding until 1851. On the occasion of the jubilee of The Chemical Society Warrington's son recalled the following (4):

There are two circumstances which helped to determine the formation of the Chemical Society in 1841. The preceding year had seen the commencement of the penny postage, and this fact undoubtedly gave an impetus to all attempts at organization requiring much correspondence. The year 1841 was also a short period of leisure in the life of my father. Between 1839 and 1842 he held no official position, and was at liberty to turn his energies in any direction which he might desire.

Warrington shrewdly enlisted the support of a cross-section of the leading academics, chemical manufacturers, and consulting chemists in London. These included the academics Thomas Graham and William Braude, the manufacturer Warren de la Rue, and the consulting chemist Lyon Playfair among others (5). The organizational meeting took place on February 23, 1841, and twenty-five were in attendance. Thomas Graham of University College was elected the first president of the society.

The first scientific meeting was held on April 13, 1841 and, quite appropriately, the first paper read was a translation of Liebig's concerning "The Yellow Prussiate Potash." By the end of its first year in existence the Chemical Society had a membership of 77; this doubled by 1844 and tripled by 1848. By its jubilee in 1891 membership had risen to 1,754 members.

The program proposed by Warrington for the Chemical Society at its organization was (6):

The reading of notes and papers on chemical science ... and the discussion of the same. The formation of a laboratory, in which might be carried out the more abstruse and disputed points connected with the science. The establishment of a collection of standard chemical preparations, of as varied a nature as possible, for reference and comparison, and thus to supply a very

great desideratum in a metropolis; the formation of a library, to include particularly the works and publications of Continental authors.

Only the first part of this program was ever to be realized.

As the society grew in the 1840s, the London academics became the dominant force in the Chemical Society. The charter stated that the goal of the Society was the advancement of chemistry as a way of assuring the prosperity of the manufacturing sector, a most laudable goal. This goal was quickly abandoned as the basic science became much more of a paramount interest than the practice of chemistry. This tension between science and practice would lead to the founding of the Institute of Chemists in 1877 and the Society of Chemical Industry in 1881 to represent the interests of the industry. These moves transformed the Chemical Society into an organization whose main goal was the advancement of the science.

The founding of the Chemical Society was a response to internal factors operating in Britain. There were no external influences such as other chemical societies pushing for the founding; if anything the reverse would be true as the Chemical Society would become the model for most of the chemical societies of the world.

Initially papers read at the Chemical Society meetings were published in the *Memoirs and Transactions of the Chemical Society*, which appeared at sporadic intervals. In March of 1848 William Thomas Brande, in his Presidential Address, made the following statement (7):

At the last Anniversary Meeting, your Council was requested to consider any and what means could be devised for the purpose of ensuring a more regular and efficient publication of the Society's Memoirs, and it has accordingly been determined, in order to promote the more speedy and regular circulation of the communications made to the Society, amongst its Members, to publish the Memoirs and Proceedings...in the form of a *Quarterly Journal*

All issues of the *Journal* were also to include abstracts of important foreign papers ensuring that the publication would be of greater value to its readers. Thus subscribers would be able to keep up with what was happening on the continent if they did not have access to a library or could not afford the costs of subscribing to journals like Liebig's *Annalen der Chemie und Pharmacie*, for example. The January number for each year was to include an alphabetical list of all the domestic and foreign papers that appeared during the year. A publications commit-

tee decided what would be in the journal and picked the editor as well. The first editor was Edmund Ronalds, a Geissen-trained (D. Phil. 1842) chemist working in London. Ronalds left his position as editor after two years and was succeeded by Henry Watts (8), who held the position from 1849 until his death in 1884. This was a salaried position and involved the day-to-day activities of the *Journal*. The real power lay in the publications committee, and thus Watts' name as editor is not included along with the committee members' names that appear in each issue.

The first volume of the *Quarterly Journal* consisted of a mix of translated abstracts of papers from foreign journals as well as original contributions from Society members. The foreign abstracts represented some of the most notable names in continental chemistry, Wöhler, Gay-Lussac, Gerhard, Laurent, Gmelin, and Liebig, among others. The first volume also contained 29 papers in many different subject areas by a diverse group of British authors. The analysis of this volume will be discussed latter.

One of the most significant figures in British chemistry at the time was the German organic chemist, August Hofmann. Hofmann, a student of Liebig, had come to London in 1845 to head the newly founded Royal College of Chemistry (9). His investigations concerning the composition of coal tar were instrumental in the development of the synthetic organic chemical industry. William Henry Perkin, a student at the Royal College of Chemistry, discovered the first synthetic dye mauve in 1856, which led to the preeminent position of the British organic chemical industry over the next several decades. Many of Hofmann's students worked in the dye industry, and German-trained chemists, with their superb training, came to work in British chemical industry because there were few opportunities at home. Strong chemical ties developed between Britain and Germany that were to last until 1914. One of the consequences of the British experience for German expatriates, when they returned home, was the founding of the Deutsche Chemische Gesellschaft zu Berlin in 1868.

Die Deutsche Chemische Gesellschaft zu Berlin (10)

At the jubilee celebration of the Chemical Society of London on February 24-25, 1891, Edward Frankland, in his toast to the "Delegates of Foreign Chemical Societies," made the following remarks (11):

The Chemical Society of London whilst justly proud of the position as *alma mater* to all Chemical Societies of the world, can hardly claim to have exercised much parental care even during the infancy of her offspring. They did not require it.

In his remarks concerning the German chemical society, Frankland pointed out the pivotal role played by August Hofmann in its founding (11):

I am not sure whether this exceedingly vigorous child was smuggled into Germany by our friend Hofmann, whose absence we so much deplore. At all events the circumstances are very suspicious. You know that Professor Hofmann is a past President of the parent Society. You know he left this country in the year 1865; that he was one of the most active of our Society during the twenty years he spent in London; and we first hear of the German Chemical Society in the following year, and he was the first President. Were he here now, we should make him confess.

Through Hofmann the idea of a national chemical society came to Germany. The founding of the German Chemical Society in Berlin in 1868 can be attributed to a confluence of events, perhaps most important of which was the return of Hofmann to Germany. Germany was becoming a unified country under Prussian leadership, rather than a patchwork of many competing states. The transformation from an agrarian to an industrial society was well under way by 1868. The synthetic organic chemical industry was still in its infancy, yet in a few decades Germany would dominate this field of manufacturing. This was in part due to the expertise of many expatriate German chemists, who had worked in England and later returned home (12). One important reason for their return was the modernization occurring in the German universities with respect to the natural sciences. Sufficient funds now became available to build teaching laboratories that had been lacking at many of the universities. Previously there were only a few private laboratories, and a small number of universities existed where basic research was being performed. Of the major universities in Prussia only Breslau, Greifswald, Königsberg, and Halle had such facilities in 1863. As early as 1859 the Prussian government approved the building of a chemical institute in the capital of Berlin, but construction did not begin until May, 1865. In 1863 Hofmann was offered the chair at this new institute in Berlin, but he was reluctant to leave England. The position included the opportunity to design the chemical institute to his liking, and this proved to be an offer he could not refuse.

Hofmann brought his enthusiasm for research and also the realization that pure and applied chemistry were

intertwined. He had been instrumental in the development of the synthetic organic chemical industry in Britain and had acted as a consultant to many manufacturers. As an active member of the Chemical Society from his arrival in London in 1845, he was familiar with the organization as well as the benefits of establishing a similar society in Berlin. This new Berlin society was founded with the idea of eventually becoming a national chemical society (13). In the fall of 1867 Carl Martius (a student of Hofmann, who had followed him to Berlin) and Hermann Wichelhaus used the London model to formulate a set of statutes for the society. Adolf Baeyer and Carl Scheibler approved these and asked Hofmann to call an organizational meeting to be held on November 11, 1867. Hofmann wisely concluded that it would be better to have this invitation come from long established Berlin chemists since he was a relative newcomer in Berlin. The invitation to join this new society was signed by ten prominent Berlin chemists, and approximately 100 chemists attended the organizational meeting at the Commercial Museum. Adolf Baeyer chaired the meeting and pointed to the new chemical institute with its marvelous facilities for instruction as a good reason to found the society. The new institute, he believed, would be a magnet to draw the chemical community into a union "which would produce the richest fruits for the scientific as well as technical areas of chemistry"(13). Hofmann, asked to assume the provisional presidency for the organizational meeting, stated that (13, 14):

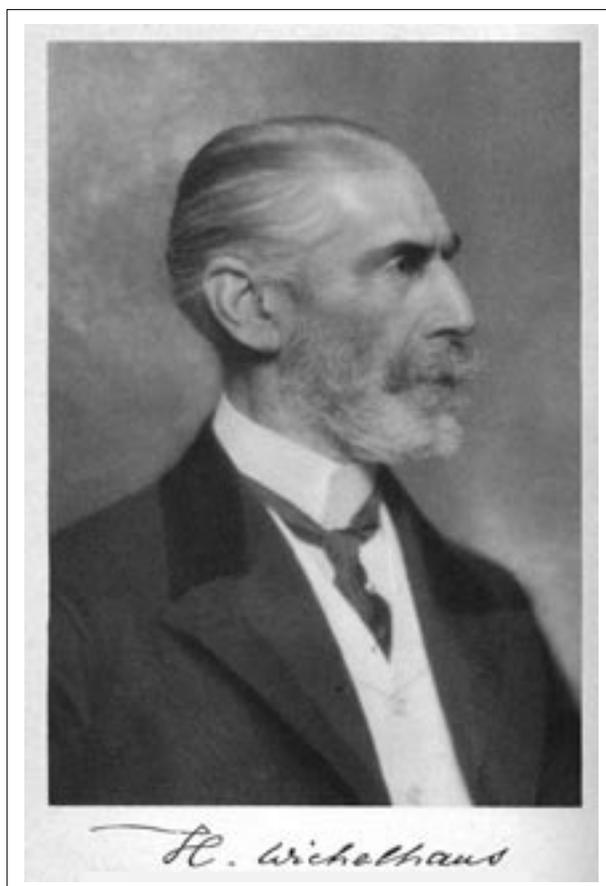
...he believed he could assure numerous assembled colleagues that at some future date they would look back with satisfaction on this day of establishment of a chemical society at Berlin...he in no way doubted that just as glorious a future lay ahead for the society, for whose establishment a great majority of Berlin chemists had assembled this evening.

The formal establishment of the society took place on January 15, 1868 when a revised set of statutes was adopted and

Hofmann was elected President—a position he held for 25 years. The Vice-Presidents were Adolph Baeyer, Carl Rammelsberg, Gustav Magnus, and Otto Barwald. The society had 95 members living in Berlin, 8 outside, and 3 honorary members (Bunsen, Liebig, and Wohler).

The founding of the DCG is an example of a combination of both the external factor of having the English society as a model and the internal factor of the rapid growth of the chemical industry and university facilities. Would the DCG have been founded much later than 1867 if Hofmann had not come to Berlin in 1865? Ruske in his history of the DCG touches on the question of Hofmann's motives by reference to opinions of Hofmann's contemporaries. Ferdinand Tiemann and Wilhelm Will were certain that it was patriotism and his love of the *Vaterland* that motivated Hofmann. In moving to Berlin he had assumed a lesser position than he had in Britain, and it was only his belief that he could do some good that motivated him. Gustav Magnus believed his motivation was the experience he had had with the English system of education and its emphasis on a practical approach. He was afraid that the evolving German

educational system would emphasize the practice of chemistry to the detriment of theory. Magnus noted "the fire in the belly of a teacher of science" that motivated Hofmann. A professional chemical society that was German would elevate the more lofty goals of what he believed to be the goal of the chemist. Hofmann, having only recently come to Berlin, used his colleagues who were better known to begin the process of the formation of the society. Had it not been Hofmann there is little doubt that others would have instigated the creation of a German chemical society, given the rapid growth occurring in Germany, especially after unification in 1871.



First Editor of *Berichte*

The American Chemical Society (15)

The development of a national chemical society in the nineteenth century in the United States presented a unique set of challenges. Whereas London and Berlin were major centers of chemical activity, there was no similar comparable venue in the United States. Given the size of the country and the scarcity of significant opportunities for doing chemistry beyond the elementary level and the lack of opportunities for chemists, a national chemical society seemed a distant goal until well into the late 1870s. The only truly national body for chemists prior to the founding of the American Chemical Society was the American Association for the Advancement of Science (founded in 1848) and its Section A, which dealt with mathematics, physics, and chemistry. Section A and Section B dealing with natural history were the two original divisions from the founding of the AAAS. Within Section A there developed a sub-section of Chemistry and Mineralogy. By 1874 a Section C that dealt exclusively with chemistry had been authorized. The original sub-section on chemistry and mineralogy within Section A had provided a very successful venue in terms of the presentation of papers from at least 1860 at the annual AAAS meetings. Most of the prominent American chemists of this era were members of the section and thus there seemed to be no pressing need for a national chemical society (16). Chemists had opportunities to publish their work in journals such as the *Proceedings of the American Association for the Advancement of Science* and the *American Journal of Science*, as well as in the journals of foreign chemical societies.

In 1874 at the Priestley Centennial Meeting in Northumberland, Pennsylvania, a discussion was held among the 77 chemists present concerning the advisability of establishing a truly national chemical society. The consensus was that the time was not yet right and that the best way to proceed was to strengthen the AAAS chemical section. However, a group of chemists from the New York metropolitan area, under the leadership of C.F. Chandler of Columbia College, decided in January

1876 to organize a chemical society initially restricted to the New York area. A preliminary mailing of a prospectus to 100 chemists in the metropolitan New York area produced such a favorable response that it was decided by the organizers in March, 1876 to bypass the local society model and form a national society. Chandler's and his associates' belief that the response of the New York group represented a pent up demand for a national society proved to be wrong. The organizational meeting for the American Chemical Society was then held on April 6, 1876 and the Constitution and By-laws were read and approved (15). The society was incorporated under the laws of the State of New York in 1877, and all 13 directors had therefore to be residents of New York. That the national society was really a local society in disguise led to almost immediate problems in terms of its membership and finances. As C. A. Browne has written (15):

Chemists outside of New York therefore looked upon the Society as a purely local organization

and were unable to see that conditions for them were any better than before the Society's foundation...The non-resident membership...reached its maximum by the end of the first year.

Articles published in the *Journal* were reports that had been delivered initially at meetings of the American Chemical Society in New York. Consequently, the first issue of the *Journal of the American Chemical Society* in 1879 is not truly representative of the current state of American chemistry since many of the most important chemists in America were not members of the society, and therefore their work would have not been included. The founding of the American Chemical Society was the result of a set of internal circumstances which included a local critical mass of chemists in New York and the feeling that it was time for chemistry to identify itself as a unique scientific endeavor with its own voice. Perhaps the centennial of the founding of the United States in 1876 played some role in this. However, it was an external model, The Society of Chemical Industry of Great Britain, that rescued the American Chemical Society and made it into a truly national chemical society.



Hermann Endemann
Editor, JACS, 1879, 1881

Those chemists dissatisfied with the New York-centered nature of the ACS urged the adoption of the English model in 1890. This led to the founding of local sections and national meetings that occurred in different venues so that more members would be able to attend them. This led to a rejuvenation of the society and its future success.

A more complete picture of American chemistry can be ascertained by including also the *American Chemical Journal* edited by Ira Remsen. The first volume also appeared in 1879. Remsen (17) was a member of the ACS from 1878 until 1881. His journal was founded as an outlet for the growing volume of research being produced by himself and co-workers at Johns Hopkins. Most of Remsen's previous work had been published in the *American Journal of Science*, edited by James Dwight Dana



Ira Remsen
Editor, ACJ

difference was that in JACS all the papers had been read at the monthly meetings of the society, whereas those in the ACJ were sent to Remsen in his capacity as editor.

Analysis of the First Volumes

In Table 1 are listed by discipline a comparison of the number of papers in the first issues of the British, German, and American journals under consideration.

In the first volume of the *Quarterly Transactions* all fields of chemistry are represented in almost equal numbers. Of the 11 organic papers all but two have a German connection. Hofmann and his students and assistants at the Royal College were responsible for six, three by Hofmann and one each from E.C. Nicholson, H. Medlock, and C. B. Mansfield. If we add to these contributions by

Table 1: Analysis of papers in Volume I by Subject Content—Number and Percent.

	Quarterly Journal (1849)		Berichte (1868)		JACS 1879)		ACJ (1879)	
Analytical	6	21%	5	6%	7	23%	9	21%
Inorganic	5	17%	19	22%	9	30%	5	12%
Organic	11	38%	54	62%	11	37%	26	61%
Physical	7	24%	9	10%	3	10%	3	6%

of Yale University. Dana had suggested that Remsen found his own journal as Remsen's work was becoming too specialized for the journal (18).

Both journals initially had many similarities in that a considerable part of each issue was devoted to summaries of important work appearing in foreign journals. Reviews of the progress being made in various fields such as analytical chemistry were also part of many individual issues. Volume I of JACS contained 32 pages of proceedings, 235 pages of original papers, and 324 pages of reviews, notes, and abstracts from foreign journals, as well as domestic and foreign patents. A major

persons with a connection to Giessen (S. Muspratt, J. H. Gladstone) then the number rises to 8 out of 11. The ninth paper was a collaboration of Kolbe (D. Phil., Göttingen) and Frankland, D. Phil., Marburg [Bunsen]). The importance of the German connection in organic chemistry and the Royal College of Chemistry (9) cannot be underestimated.

The first volume of the *Berichte der Deutschen Chemischen Gesellschaft zu Berlin*, appearing in 1868, consisted of a total of 87 items, these being divided in two types: 67 full papers and 20 communications. The first volume of the *Berichte* as well as subsequent volumes would contain only original papers. One cannot say with

any confidence whether this was a reflection of the sheer volume of research coming from the universities and research institutes or of a certain degree

of nationalism. All of the published material had been presented at the monthly meetings of the society by the author(s) or by a member of the society. The number of papers involving organic chemistry comes as no surprise, given the interest in the subject in Germany. The bulk of the contributions are from Hofmann and Baeyer and their circle of collaborators. The large number of inorganic papers is mainly due to the work of Carl Rammelsberg, the leading mineralogist in Germany at this time. Rammelsberg would contribute 20 papers in 1870, the peak of his scientific productivity.

In both the JACS and the ACJ the subjects most frequently dealt with were analytical, inorganic, and organic chemistry. In JACS there are 90% in these three areas versus 94% in the ACJ. The preponderance of organic papers in the ACJ is obvious as this was Remsen's journal. Remsen, with the largest research group, was the most productive organic chemist in America. The interest in analytical chemistry in the US is to be expected given the wealth and diversity of raw materials and finished goods that America was producing during this era of industrial expansion.

In Table 2 are shown the educational backgrounds of the authors by their highest degree.

When the Chemical Society of London was formed, the emphasis on formal credentials to call oneself a chemist or even to teach the subject was not as important as it would become in succeeding decades. This was still the era when talented amateur gentlemen of science could make significant contributions. It is also evident that medicine in Britain was one of the major opportunities for studying chemistry and doing chemical research. Those seeking to enhance their knowledge of chemistry and obtain a formal qualification went to the various German universities. There they learned of the latest discoveries and techniques and perhaps earned the D. Phil. degree. Of those who had the degree in the first volume only three were British, the other two being German-born and educated (Hofmann and Kolbe).

Table 2: Educational Background for Authors in Journals

Highest Degree	Quarterly Journal		Berichte		JACS,
Ph. D	5	21%	20	53%	12
M.D.	4	17%	3	8%	2
MA, MSc, BA,BS	1	4%			11
No formal degree	14	48%	15	39%	

By the time of the founding of the DCG a well developed system for the training of chemists was in place. Of the 95 Berlin members, 57 held the D.

Phil. degree (60%), 23 were listed as chemical factory owners or directors (24%), 4 were apothecaries (4%), and 7 were listed as chemists without the D. Phil. (7.5%). As can be seen from the data 61% of the 38 authors of papers in the first volume held the D. Phil. or M.D. degree. Nowhere else in the western world was there such a system for educating chemists that would produce the next generation of academic and industrial chemists. German methods would have an especially important impact in the direction of British and American chemistry in future decades. Curiously, in Volume 1 of *Berichte* there are no American authors and only two contributions from the British chemists Warren De la Rue, Hugo Müller (London), and Peter Griess (Burton-on-Trent, England). Müller and Griess were expatriates and De la Rue had been awarded an honorary doctorate from Geissen by Liebig. The development of chemistry in America on the graduate level was influenced in large part by the assimilation of the German system (18, 19). Of the 42 authors of papers in Volume I of AJC and JACS we have been able to obtain the educational background of 25. Although incomplete it still represents a sample that can provide an overall feel for the American educational experience. Some American chemists after obtaining an American bachelors' degree then went on to study in various laboratories in Germany. Many stayed the required two years and obtained their D. Phil. degree. Others did not but, having acquired a superior knowledge of chemistry, were still offered academic positions on their return. Some chose to become manufacturers or consultants.

Table 3 shows an analysis of the contributors of three or more papers to the journals we have analyzed. The number three was picked to denote significant contributors and to provide a good cross section for analysis.

It is quite understandable that Hofmann is the only person who contributed three papers to the first issue of the *Quarterly Journal*, as he was the only chemist in Britain that had any ongoing, concerted research program in operation. By the time of the founding of the DCG and

Table 3: Authors of Three or More Papers in Volume I of Quarterly Journal (1849), Berichte (1868), JACS (1879), and ACJ (1879).

Name	No. of Papers	Institutional Affiliation	Education
A. W. Hofmann	13 3(QJ)*	U. of Berlin Royal College of Chemistry	Dr. : Geissen
C. Rammelsberg	8	U. of Berlin	Dr. : Berlin
H. Wichelhaus	6	Docent, Berlin	Dr. : Bonn
C. Graebe & C. Liebermann	4	Gewerbeakademie Berlin	Dr. : Berlin(both)
C. Scheibler	4	Zentrallaboratorium der deutschen Zuckerindustrie	Dr. : Königsberg
R. Schmitt	3	Gewerbeschule, Cassel	Dr. : Marburg
A. Ladenburg	3	U. of Heidelberg	Dr. : Heidelberg
A. Oppenheim	3	Docent, Berlin	Dr. : Göttingen
A. Remele	3	Docent, Berlin	Dr. : Berlin
Leeds, A.R.!	12**	Stevens Institute	Dr. (hon): U. of New Jersey
Remsen, I !!	7	Johns Hopkins	Dr. :Göttingen
Michael, A !!	7	Private laboratory, Buffalo, New York.	Studied with Bunsen, Hofmann, Wurtz
Mallet, C !!	5	Univ. of Virginia	Dr. :Göttingen
Goessmann, C. A.!	5	Mass. Agricultural College	Dr. :Göttingen
Endemann, H.!	3	Columbia School of Mines	Dr. :Marburg
Smith, E.F !!	3	Univ. of Pennsylvania	Dr. :Göttingen
Casmajar, P. !!	3	Havermeyer & Elder Sugar	Studied at Harvard, École Centrale, Paris
Gooch, F.A. !!	3	US Government	Ph.D. : Harvard

* Hofmann was the only author of three papers in volume I of Quarterly Journal.

** Many of the papers in JACS are very short, i.e. one page but are still numbered as individual entries in the index.

! denotes JACS, !! denotes ACJ

the ACS in 1867 and 1876, respectively, chemistry had advanced to the point where formal and highly structured training was required. The era of the talented amateur with little formal training making any significant contributions to the science had long passed. With few exceptions all the major authors reported in Table 3 had obtained a D. Phil. at a German university (20); or if they had not taken the degree, they had spent some time in study there. Increasingly, research as measured by papers published in journals became the province of the academic chemist whether at a university or a research institute supported by the public purse. This did not mean that industrial chemists were not doing important work, but their contributions were being overshadowed by the academics, especially in Germany. Of the 19 chemists in Table 3 only two were connected with non-academic organizations.

The Chemical Society of London produced the model for organizing and maintaining a successful chemi-

cal society. German universities provided the model for the education of a new generation of chemists. The cooperation between pure and applied chemistry was also a hallmark of the DCG, whereas in Britain this caused a major problem and led to the formation of two new societies catering to the applied aspects of chemistry. Americans learned from their German counterparts how to organize higher education and from the British how to structure a chemical society which could accommodate the interests of those involved in pure or applied chemistry as well as the establishment of a decentralized society. The publication of journals by the societies provided a way for chemists to become aware of the latest developments in a rapidly changing science, even if they were unable to attend meetings. Chemical societies and the journals they published serve as an indicator of the growth and development of the chemical sciences in the latter part of the 19th century.

ACKNOWLEDGMENT

I would like to thank Providence College through its Committee to Aid Faculty Research Grant for funding this project.

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THE CIRCUMSTANCES OF KEKULÉ'S “MOLECULAR DREAM” IN LONDON IN 1854

Anthony D. Dayan

Among the more famous ‘dreams’ or reveries in the history of science are two instances attributed to August Kekulé (later Kekulé von Stradonitz) in the second half of the 19th century. He developed the ideas that single carbon atoms could combine with each other to form chains and subsequently that chains of carbon atoms could become rings by combination of their ends, resulting in attribution of the special 6-membered ring structure to the benzene molecule (1, 2, 3, 4, 5). This was a fundamentally important step in our understanding of organic chemistry and molecular structure.

Kekulé himself did not mention the occasions in public until 1890, neither the first and perhaps more novel event in 1854 nor the second in 1861-2, when he gave limited descriptions of them during a special congress in his honor in Berlin, the ‘Benzolfest’ (6); the German text and various translations are reproduced in Ref. 1, 4, 6 and 7. There has been controversy about the reality

and nature of Kekulé’s dreams on historical and chemical grounds (2, 3, 4, 5). Many psychological and psychoanalytical interpretations have been offered, despite the limited information available about these events (3, 6), and in turn they have generated controversy (2, 6).

No independent account of the physical circumstances at the time of his dreams has been reported, which might aid understanding of the *post-factum* accounts of the events. I have recently been able to explore the setting in which his first purported dream occurred in 1854.



Knifeboard omnibus (C.H. Moore, Omnibuses and Cabs, Chapman Hall, LD., London, 1902, p 75.)

Circumstances of Kekulé’s Dream in 1854

Kekulé (7; see diverse translations in 2, 4, 6 and 8) describes how in 1854 he was living in south London, in the Clapham Road near Clapham Common. On a fine summer evening he visited Hugo Müller, his close friend and fellow chemist from Germany,

in Islington, and then caught the last omnibus home. He sat outside, on the top of the omnibus and fell into a 'dream' or 'reverie' ["Traumerei"] in which he 'saw' in his mind atoms in a whirling dance that formed strings that grew by adding smaller strings at their ends (7).

In the same report he recalled how one evening in 1861, while living in Ghent, he again had a vision of atoms (6, 7):

... in motion, winding and turning like snakes. One of the snakes grabbed its own tail (translation from Ref. 6 and 8).

These dreams are the basis of his claim to priority in proposing the ring structure of the benzene molecule.

What was the nature of his bus ride in 1854 when he first realized how carbon atoms could combine into organic chemical molecules? There was no regular public transport from Islington to Clapham, but there was a horse-drawn omnibus that ran from Islington via Goswell Road, Aldersgate Street, the GPO, and London Bridge to the Kennington Turnpike, which is the start of the Kennington Road (9, 10). The end of the journey at Kennington Road is the start of Clapham Road, which still passes along the north side of Clapham Common. The service ran every 8-9 minutes until 10:30 – 11:00 p.m. The ride was timed to take about 1 hour at that time of night, over a distance of about 13 km.

Where Kekulé lived in London has not previously been known except that he was helped to come there by his wealthy step-brother, Karl, who is described as a 'Merchant living and working in London' (1). Inspection of public records shows that there was a Charles Kekulé living in No. 3, Dudley Villa, Clapham Common in 1854 (11). The entry reads:

Clapham Road 'Gentry', Kekulé Chas. Esq. 3, Dudley Villas, The Road.

The British National Census record for 1861 (12) shows a Charles Kekulé, of German birth, living at 3, Dudley Villa, on the north side of Clapham Common. He is described as a 'Merchant,' with a household of three women comprising a housekeeper, a servant, and a telegraph office clerk. Charles Kekulé also appears in the Post Office London directory for 1854 (13):

Charles Kekulé, Merchant, 60 Mark Lane in the Corn Exchange Tavern, London

On that evening in 1854, therefore, it is a reasonable assumption that Kekulé would have taken the omnibus

and then walked along the Clapham Road to stay in his brother Charles' house at 3, Dudley Villas, Clapham Common.

Horse-drawn omnibus travel in the 1850s was accepted as uncomfortable and physically demanding. The vehicles were made of wood, they had wooden wheels, shod with iron tires, there were only two weak leaf springs and they were pulled by three horses. They were called 'knifeboard omnibuses' because the solid wooden seats on each side of the open top, which were not protected by a roof or side walls, were separated by a long plank of lightly padded wood, like a knife board. Passengers sat on the right and left sides both downstairs and on top. The left was favored because the tilt produced by the camber of the road at least kept the passenger in his seat. Contemporaneous accounts of riding in such omnibuses described the vehicles as swaying, reeling and crawling in a crab-like fashion, and passengers, especially those on top, as experiencing "abrupt jerking motion, violent in proportion to the velocity of movement, and with rapid and sustained concussions" as the omnibus wheels rattled over roads made of rough granite slabs, macadam with potholes, and irregular muddy patches (14). There would have been a lot of noise and vibration from the iron shod wheels on the road, the horses, the bell and shouts by the conductor and passengers to tell the driver to stop, and the general hubbub of a busy city, even at that time of night.

A Reverie?

Given the nature of Kekulé's journey in 1854, even late in the evening, it is intriguing that he was able to experience such a dream as he described, albeit 36 years later (1, 7). Unless physically very fatigued, many people would experience difficulty in falling into such a state of external unawareness and deep internal concentration in so noisy and uncomfortable a setting.

The physical circumstances of Kekulé's molecular dream in 1854 may appear unpromising in terms of high level thought and a productive mental state, but they can be pictured now in perspective as he eventually reconstructed the legendary event.

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LETTER TO THE EDITOR

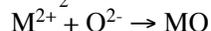
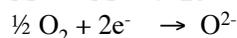
PHLOGISTON AND MODERN CHEMISTRY

In a recent issue of *Bulletin for the History of Chemistry* Woodcock made a clever attempt to compare phlogiston with Gibbs free energy (L. V. Woodcock, "Phlogiston Theory and Chemical Revolutions," *Bull. Hist. Chem.*, **2005**, *30*, 63-69).

While this is quite interesting, it is however rather complicated. A more simple analogy would be to compare phlogiston with the electron. Thus, the classic equation:

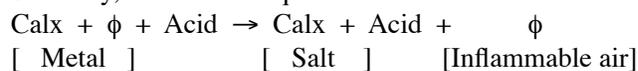


would be represented by:

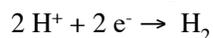
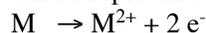


where M is a divalent metal and MO will be the calx.

Similarly, the familiar equation:



can be represented in modern terms as follows:



Fathi Habashi, Laval University, Canada

AUTHOR'S RESPONSE

I think Habashi's short communication provides a clearer background to phlogiston than my article, and the idea of identifying phlogiston with the electron is, I believe, both novel and fascinating. Indeed, had the phlogiston theory survived longer, as it might have done, I can imagine that the unit of energy that developed from electrochemistry, namely the "electron-volt," might have become known as the "phlogiston."

In my article, I identified phlogiston with the Gibbs chemical potential of a material with respect to its oxide. That is a phenomenological interpretation consistent with 18th century phenomenology, and needs make no reference to either the molecular or electronic level of interpretation of chemical reaction phenomena. These levels of interpretation were unknown at the time.

The connection between phlogiston and the electrons transferred is: phlogiston = number of electrons x Faraday constant x EMF (voltage). It is clear that all chemical reactions involving, for instance, the transfer of one electron have widely varying amounts of phlogiston, depending on the EMF, which is proportional to the phlogiston content.

This highlights the limitation of the phlogiston = electron transferred idea. For example, hydrogen would have only one quarter the amount of phlogiston as carbon, when they react with oxygen; but in fact, hydrogen has about 10 times the phlogiston (i.e., Gibbs free energy) of carbon.

Leslie V. Woodcock

Message from Jeffrey I. Seeman, Chair of HIST

Greetings from Richmond, Virginia!

So much is happening in HIST these days, it is necessary to share the top line items and to let you know the best way to stay informed of HIST's activities.

HIST's website is the place! We are at <http://www.scs.uiuc.edu/~mainzv/HIST/> You can also get to us by searching "HIST ACS" on your favorite search engine.

Last year, HIST's Executive Committee decided unanimously to fully direct our budget to projects that add value to the membership and support HIST programs. The cost of printing and mailing HIST's newsletter and the ACS National meeting abstracts had risen so dramatically that it consumed a major portion of HIST's budget. These are now being posted promptly and, for us, easily on our website. We understand that a few of our members do not have computer access. To be sensitive to all our members' needs, we are continuing to mail materials to members who request this special consideration. Please write to our Secretary-Treasurer, Vera Mainz, whose contact information is found elsewhere in this *Bulletin*. And you are also encouraged to contact me directly for any need.

The Bulletin for the History of Chemistry will continue to be mailed directly to our members and our subscribers. We are proud of *The Bulletin* and urge you to consider submitting an article for publication. Kudos to Paul Jones and his Associate Editors!

HIST's National and Regional meeting programming has expanded greatly over the past few years. HIST has sponsored and organized a number of ACS Presidential symposia and continues to do so. The symposia have been designed to respond to the specific agenda proposed by each new ACS President. Mary Virginia Orna is organizing a new series of ACS Presidential symposia, *Research and Development over the Past 25 Years*.

HIST's awards programming is strong, diverse and growing! HIST is honored to announce the selection of Peter Morris (Science Museum, London) for the 2006 Sidney M. Edelstein award. 2006 marks the 50th anniversary of the Dexter/Edelstein awards. The San Francisco ACS National meeting will feature a special celebration of these 50 years plus a wonderful symposium to honor Peter. Tony Travis is the lead organizer for the 50th anniversary celebrations. And to look ahead, HIST is planning an 85th Birthday celebration for HIST in 2007 to be led by Jim Bohning!

HIST's 2nd Annual Concert will also take place at the San Francisco meeting. Together with the Chemical Heritage Foundation, our fine partner in many events, HIST initiated and organized a piano concert at the Fall 2005 Washington, D.C. National meeting. Eminent pianist-chemist Victoria Bragin performed a number of pieces including two of chemist-composer Alexander Borodin's works to a huge and appreciative audience. We thank ACS staff for their special facilities support.

2006 will also see the inauguration of a new HIST award program, *Citation for Chemical Breakthroughs*. This program will recognize and honor seminal publications, patents, and books. Plaques, to be placed in the hallways outside the office or laboratory where the breakthroughs were achieved, will be presented annually to the institutions at which these breakthroughs occurred.

There is much more to tell. Please visit expanded HIST's website for topical news as well as historical information. Have you seen HIST's new logo? And check out HIST's new mission statement.

Welcome to HIST! And enjoy this issue of *The Bulletin*.

Jeff



BOOK REVIEWS

Promethean Ambitions: Alchemy and the Quest to Perfect Nature. William R. Newman, University of Chicago Press, Chicago, IL, 2004, 352 pp, 8 color plates, 20 halftones, Cloth, ISBN: 0-226-57712-0, \$30.

Our understanding of alchemy and its influence continues to grow. In this significant book, Newman does not directly examine the theoretical content of alchemy but the attitudes towards claims made by alchemists that they were able to perfect and even outdo nature by artificial means. From the beginning of alchemical practice, critics of alchemy regarded such claims of transmutation as either impossible or morally objectionable. Alchemists, on the other hand, defended their art by claiming to work with the genuine substances of nature, and not, like the other arts, by only superficially manipulating the external characteristics of materials. In short, alchemists raised two questions: are we capable of duplicating or even outdoing nature, and *should* we try to outdo nature? In the light of current questions about genetic engineering and cloning, these two questions seem remarkably modern. But as Newman shows, these are old questions, dating in part to ancient Greek ideas about the distinction between the artificial and natural.

The idea that man could compete with and possibly outdo nature arose in ancient Greece and was most closely associated with the decorative arts. In the Greek classical ideal, the purpose of statuary, for example, could have two goals. The first was mimicry, the creation of as realistic a representation of a natural object as possible. The second was “perfective,” an attempt to create something not actually found in nature, such as a

composite sculpture of Helen of Troy more beautiful than any existing woman. As a human art, ancient alchemy followed closely this distinction between the real and artificial, and often alchemists claimed their craft to be a perfective art that went far beyond other human arts in its power. A frequent claim of early alchemical texts, including those recorded in the Leiden and Stockholm papyri, touts the ability of man to create products “no different from their natural exemplars” (p 25). But critics of alchemical claims appeared at the same time: they maintained the Leiden and Stockholm papyri also clearly described recipes for making substances that only “mimic” natural things, and that many of the claims contained in alchemical works were fraudulent.

Newman’s second chapter is a reworking and enlargement of his classic 1986 *Isis* article on the alchemical debate in the Middle Ages. The central claim of alchemists was that their art was different from the other arts—alchemists suggested they had the power to do genuine conversions, accelerating nature’s own processes to outdo nature. Such claims were met with skepticism from the outset. In the influential thirteenth-century manuscript titled *Sciant artifices*, the Arabic philosopher Avicenna gave a powerful two-fold argument against transmutation. Avicenna claimed that art, always weaker than nature, could never duplicate it, and that the genuine underlying causal properties of substances were unknowable and therefore immune to manipulation. For this reason, according to Avicenna, synthetic gold crafted in the laboratory would never actually be real gold. Given authority by a mistaken attribution to Aristotle, the *Sciant Artifices* provided the fundamental argument against the power of artificial production of

natural substances and the primary point of attack for the defenders of alchemy. Some commentators, including Albertus Magnus and Thomas Aquinas, used Avicenna's argument to demonstrate that demons and witches were incapable of transmutatory acts. Another common criticism of alchemical claims was theological: humans could never imitate, and certainly could not exceed God's own creative power. A number of writers came to the defense of alchemy, among them Paul of Taranto, who made a distinction between the "perfective" arts (alchemy or medicine) that get at the essences of substances, and the "mimetic arts" (sculpture, for example) that manipulate only surface features.

This well developed dichotomy between the natural and the artificial had further ramifications in the visual arts, the subject of Chapter 3. Examining the attitudes of Renaissance artists Leonardo Da Vinci, Vanoccio Biringuccio, and Bernard Palissy towards alchemy, Newman notes that all three acknowledged the value of alchemical recipes for making pigments and materials useful to their crafts, yet all three rejected the central claim of transmutation. Da Vinci used the common argument that true transmutatory powers would usurp the power of God, while Biringuccio thought the claims of alchemists absurd because, were they true, alchemists would have acquired untold wealth. While denying alchemical claims to transmutation, Palissy appropriated those same alchemical motives to the process of petrification and the creation of his extraordinarily life-like ceramic pottery.

Perhaps the most bizarre subject (to modern readers at least) of *Promethean Ambitions* concerns medieval and renaissance notions on spontaneous generation and the creation of artificial life. In Chapter 4, Newman recounts the history of the idea of artificial generation from the (possibly) Greek legend of Salaman, the artificial child of King Harmanus who, according to legend, was "cooked" in a sealed vessel outside the womb. Later works in this tradition offer numerous explicit recipes for creating artificial life. *The Book of the Cow*, for example, describes the growth of a "rational animal" in the womb of a mutilated cow, and Jewish magical texts offer numerous recipes for creating a golem. But the most influential figure in the story of the homunculus was Paracelsus, who fully incorporated the major issues of the art/nature debate into the artificial production of life. Paracelsian conceptions of the homunculus took on eugenic overtones, as recipes for the homunculi commonly involved allowing male semen to mature without the female component. The growth process outside of the female womb would result in a more perfect being. The production of a homunculus

was also important for theological reasons, in particular for explaining the incarnation of Christ in Mary's womb. Reaction to these Paracelsian claims followed the same pattern as in alchemy itself, as it was argued that man could never mimic, and certainly not improve on God's creative acts.

As becomes clear in Chapter 5, Newman has a larger historical point to make than simply recounting odd and bizarre medieval and renaissance ideas about transmutation and spontaneous generation. The rich and detailed medieval debate about the powers of alchemical arts and technology in general strongly influenced the attitudes of natural philosophers in the seventeenth century, in particular concerning the new approach to experiment. By a careful analysis of important works of Daniel Sennert, Francis Bacon, and Robert Boyle, Newman shows that formerly cryptic statements in their most famous works make more sense in light of the alchemical debates of previous centuries.

In the final chapter, Newman outlines the continuing influence of alchemical discussions about the powers of alchemy and chemistry as art. In Goethe's *Faust*, Part II, for example, the main character Wagner is occupied in making a homunculus to render sexual reproduction obsolete. One source of Darwin's ideas on transmutation of species, the eighteenth-century naturalist Joseph Kölreuter, used the explicit imagery of "transmutation" and the sulfur-mercury theory of alchemy to describe the results of his plant hybridization experiments. Mary Shelley's *Frankenstein* must be understood as being written within a long tradition of discussions about creating artificial life. In chemistry, the debate over whether Friedrich Wöhler made "natural" or "synthetic" urea in 1828 was a logical continuation of the artificial/natural distinction first laid out by the alchemists.

Newman draws numerous parallels between the medieval debates to our own current discussions about genetically modified foods, genetic therapies for disease, cloning, and stem cell research, pointing out that these debates are only the latest manifestation of long-standing ethical issues surrounding the proper use of technology. *Promethean Ambitions* is extraordinarily rich in detail and for that reason in places is not an easy read, as Newman draws on a large number of well-known and obscure texts. The chapters also seem long (Chapter 2 is 80 pages, for example), but *Promethean Ambitions* is an important and ambitious book that will reward the careful reader. *Peter J. Ramberg, Division of Science, Truman State University, Kirksville, MO 63501*

The Chemical Industry at the Millennium: Maturity, Restructuring, and Globalization. Peter Spitz, Ed., Chemical Heritage Press, Philadelphia, PA, 2003; 387+xii pp, ISBN 0-941-90134-3, \$45.

In the decades following World War II the U.S. chemical industry was especially significant both nationally and internationally. By contrast, the past few decades seem to have been especially uncertain times for the industry. The competitive landscape has been reshaped by both abrupt increases in feedstock prices and globalization. Pressure from the financial community has forced mergers and acquisitions that have eliminated famous company names, like Union Carbide, Allied Chemical, and American Cyanamid. A traditionally strong positive contribution to the national balance of trade has slipped into negative values. Some companies have changed their main focus to the life sciences and agriculture, hoping that a shift to specialty chemicals would insulate them from large swings of the business cycle, but these efforts do not seem to have been very successful. It is important to understand these developments, both for chemists as well as for the general public, and Peter H. Spitz, the principal author of this book, is well qualified to discuss these changes.

For some time, Spitz has been the managing director of a consulting company that specializes in the chemical industry, and his previous book, *Petrochemicals: The Rise of an Industry*, is a classic that reviewed developments in the industry from 1930 to about 1980. Spitz identifies three factors that were the dominant influences on the chemical industry over the past two decades: globalization and foreign competition, the increasing influence of the financial community on industry strategy, and increased governmental regulation. Spitz and an impressive roster of coauthors discuss the role that each of these factors has played.

International trade has always been important for most major chemical companies, but now the U.S. chemical industry is faced with a truly global market. Spitz notes that four national characteristics have traditionally made the U.S. Gulf Coast a major petrochemical exporting region: (1) availability of infrastructure, skilled labor force, and raw materials; (2) high demand for the products in the home country; (3) the presence of related supplier industries; and (4) a well managed and competitive local market. By the 1990s, oil and natural gas production had peaked in the Gulf Coast, and production increasingly shifted overseas. Many countries developed

local chemical suppliers, either in combination with existing multinational corporations or as independent operations. Even though the United States continued to be an important market, more and more industrial production is shifting to Asia, Latin America, and the Middle East, attracted by cheaper feedstock and labor costs.

Spitz and his coauthors argue that stock prices and a greater focus on short-term performance rather than long-term objectives play an increasingly dominant role in the development of industrial policy. The pressure by Wall Street for quick profits has forced a focus on mergers and acquisitions at the expense of long-term research and development. Many companies have attempted to produce higher profit margins and greater growth by switching from commodity to specialty chemicals. This emphasis on short-term profits was accompanied by efforts to develop simplified matrices to predict future business performance. Despite suggestions that this was not appropriate for a highly integrated industry like chemicals, these models were used to justify financial pressure to restructure and merge companies and to favor specialty chemicals over commodity chemicals. It was predicted that these changes would produce higher profitability and greater growth, but in most cases these expectations have not been met. Other initiatives, such as cost cutting, layoffs, and early retirement, also appear to have been generally inadequate to improve the bottom line. Ironically, one of the authors points out that the recent period of lagging profits may simply have resulted from the underlying fact that commercial innovation occurs on a fifty-year cycle, and the most recent period of major product releases was 1940-1970.

Finally, Spitz argues that "...issues related to the environment and toxicity have in many respects had a greater effect on the industry than almost all the other traumatic events it had to face in the declining years of the twentieth century" (p 208). A succession of widely publicized chemical incidents has given the public a perception of the chemical industry that Spitz labels as dismal. In some cases, such as the Bhopal disaster or the contamination of the James River by Kepone, there has been some factual basis for this negative view; in other cases, such as the pesticide Alar, the justification for public outcry has been more arguable. During this period, a combination of governmental encouragement and changing industrial attitudes has produced more information sharing with local communities, massive investments in environmental controls, and elimination of many hazardous products from production processes.

The chemical industry has made intense efforts to improve how it is perceived by the public. In the 1980s, the chemical industry developed the Community Awareness and Emergency Response (CAER) and the Responsible Care initiatives to enhance health, safety, and environmental performance. These efforts have produced concrete improvements, which the industry can point to with pride, but these successes do not seem to have alleviated the public's apprehensions. Unfortunately, a single negative incident has more influence on public attitudes than a consistent history of improvements. Government regulations are surely burdensome to the chemical industry, but they may also provide the best way to prevent a few rogue companies from disrupting the efforts made by most of the industry to be responsible citizens.

Spitz closes with a cautiously optimistic evaluation of the future of the chemical industry. He predicts that many of the trends described above will continue to be important. Despite the recent period of relative inactivity, the industry will continue to be reshaped by restructuring and consolidation. Industrial efforts to re-establish public confidence will continue, with sustainable development and "Green Chemistry" as major themes in the process. One of the biggest challenges for the chemical industry will be to find ways to enhance economic growth rates now that it has become a mature industry; that is, one that grows no faster than the GNP. Spitz suggests that there may be another price spike for energy and feedstocks. He summarizes by saying that despite all the problems, we live in a society where the products of the chemical industry are essential. Thus, his conclusion might be described as guardedly optimistic.

Events since the publication of this book suggest that the three factors identified by Spitz et al. may sometimes be closely related to a fourth topic, namely, the weather. Hurricanes Katrina and Rita had a devastating impact on both the drilling platforms and the production facilities along the Gulf Coast. These hurricanes have affected the U.S. chemical industry in several ways. Natural gas is the preferred feedstock for much of the chemical production in this country, and prices of this critical raw material were already beginning to soar before the two hurricanes struck. It is not yet clear how much damage these storms did to either the drilling platforms or the refineries, [but it seems likely when the price of natural gas stabilizes, it will be at unusually high levels.] This may tend to further weaken the ability of U.S. chemical producers to compete in the global marketplace.

The hurricanes have also forced Congress to become aware of how much the petrochemical industry, as well as the drilling platforms that it depends upon, are concentrated in a relatively small and vulnerable geographic region. Congress is discussing the possibility of expanding the allowed offshore drilling areas and/or diminishing environmental regulations, but at this point, it is still unclear whether these efforts at deregulation will be successful. Regardless of what happens in this country, chemical exports to the European Union are threatened by a growing tendency in Europe to challenge the idea that the United States will define the limits of environmental regulation. Examples of more aggressive environmental policies already passed in Europe include new recycling requirements and RoHS (which restricts the use of toxic substances, like lead, within electrical and electronic equipment). In addition, a broad new regulatory framework, called REACH, Registration, Evaluation, and Authorization of Chemicals, is currently under discussion. REACH proposes to require toxicity testing for 30,000 widely used chemicals that were accepted without significant testing when the U.S. passed the Toxic Substances Control Act. Adjusting to these regulations may be a special challenge for American companies, which are already facing a more competitive global marketplace.

By the end of the century, despite the introduction of various new strategic theories, the chemical industry is still failing to deliver the sustained growth rates desired by the financial community. Unless some new family of widely used products goes into production in the near future, it seems unlikely that the financial community will change this attitude. Development of nanomaterials could help boost chemical profits and make the sector more attractive, and large amounts of research time and money are being invested to bring this possibility to fruition. Aside from the long-recognized danger of putting all the eggs in one basket, there is a real danger that the enthusiasm to move as rapidly as possible may not allow enough time to examine the potential environmental problems associated with this development. As the resistance to genetically modified foods suggests, public suspicion that problems may exist can be almost as detrimental to broad acceptance of a new product as an actual environmental disaster.

This book is recommended for a broad range of readers, including historians of science, investors, and industrial chemists. It may, however, be most useful to academic chemists. Most students who major in chemistry will ultimately be employed by the chemi-

cal industry, and so planning the future direction for chemistry departments requires an understanding of what is happening in the industry as well as what is likely to happen in the future. These authors have done us all a service by providing a compact and accessible

evaluation of the chemical industry at a crucial time in its development. *Harry E. Pence, Dept. of Chemistry and Biochemistry, SUNY Oneonta, Oneonta, NY, 13820, pencehe@oneonta.edu.*

François Blanchet, Tome I: L'Étudiant et le Savant. Stéphane Castonguay and Camille Limoges. VLB ÉDITEUR, Montréal, Québec, 2004, 396 pp, paper, ISBN 2-89005-884-0, \$29.95.

François Blanchet (1776-1830) was the first person born in Canada to write a scientific work, and the present book is the first part of a two-volume biography in French. Blanchet's claim to scientific fame was a short book that he published in 1800 entitled *Recherches sur la médecine, ou l'application de la chimie à la médecine*. This biographical volume covers the first 25 years of Blanchet's life and includes the complete text of his short book; the last third of the volume is devoted to extensive notes and references. While the subject might not appear to hold much of chemical historical interest, Blanchet's early life ties in with the history of medicine in North America, with the close connection between medicine and chemistry at that time, and with how quickly the new chemistry of Lavoisier was introduced into North America.

Blanchet was one of six children of a farmer near Montmagny, Québec. His early schooling is unknown, but from 1790-1794 he was enrolled in the Séminaire du Québec. After leaving the Séminaire a year early, he took the unusual step of deciding to pursue a medical career and apprenticed himself by contract to Dr. James Fisher (British) in Québec City during the period 1795-1799. While with Fisher, he learned English and became interested in science. He must have read widely at this time, including Lavoisier's *Traité Élémentaire de Chimie* (publ. 1789). Fisher and a colleague, Dr. John Nooth, thought enough of Blanchet to urge him to attend the School of Medicine at Columbia College in New York, the oldest in America. Blanchet was financially

able to do this only because he had sold his share of his deceased father's estate to a brother. He spent two years at Columbia, where he came under the influence of Drs. Samuel Mitchill and David Hosack. Mitchill played the principal role in the early introduction of Lavoisier's chemical system into the United States, and Blanchet was an eager disciple. [By this time phlogiston was dead in North America even though Priestley was living in Pennsylvania.] In fact, although this twenty-three year-old medical student did not do any experimental chemistry himself, he had the youthful audacity to write and self-publish a book (246 small pages) whose purpose was to apply the new chemistry to suggest a new system of medicine based on oxygen and Lavoisier's caloric. The book was written in French, apparently because Blanchet was intent on making a name for himself in Québec, and most copies sold in Québec.

After his sojourn in New York, Blanchet returned to Québec in 1801 where he practiced medicine and entered politics. The second volume of the biography will be concerned with this later part of his life.

Blanchet's book strikes the present-day reader as a curiosity. The word *recherches* in the title really refers to Blanchet's thoughts and observations rather than to experiments. In places the book reads like a manifesto with many assertions, some plausible at that time, some not. Blanchet rejected all traces of vitalism; the body was a machine and everything in it was chemical: "To give life, I only ask for an organized machine, some oxygen and some caloric." Such a view separated him from most of his medical contemporaries. At a time when the nature of bacteria, viruses, and biochemistry was as yet unknown and organic chemistry was barely in its infancy, Blanchet explained all medical conditions and problems (*e.g.* sleep, fevers, inflammations, diar-

rhea, plague, etc.) as the result of imbalances between oxygen and caloric (regarded as a material substance). In his mind there is not much room for doubt. Oxygen is responsible for all decomposition/decrepitude in the body. Sleep results from an accumulation of caloric in the body during the day. Gout is caused by an excess of oxygen. In times of famine, plague is caused by oxygen and caloric attacking the muscles for lack of fatty material on which to act. Blanchet also included his thoughts on some nonmedical natural phenomena such as the nature of light. He rejected Newton's views and insisted that light is contained in and comes directly from the viewed object. Comet tails were the result of the combination of oxygen with combustible substances from the comet. There is even a rather poetic view of

death as a chemical process involving oxygen combining first with the carbon and hydrogen of the body and then with the body's nitrogen and muscles: "Death is the tribute which we render to the universal reservoir of the elements of nature, toward which our existence has indebted us." All in all, the speculations in Blanchet's opus provide a colorful picture of an early but premature attempt to understand biological processes in terms of Lavoisier's chemistry.

The present volume could be useful to those interested in the early development of chemistry and medicine in North America. The extensive notes and references in the last third of the book are particularly valuable. An English translation of the volume seems unlikely. *Edgar W. Warnhoff, University of Western Ontario.*

Lebenslinien: Eine Selbstbiographie. W. Ostwald, K. Hansel, Ed., Verlag der Sächsischen Akademie der Wissenschaften zu Leipzig and S. Hirzel Verlag GmbH, Stuttgart, 2003, xii + 626 pp, ISBN 3-7776-1276-6, € 140.

Given the well-known tendency among American and British historians of science to emphasize the social, political, and religious consequences of science rather than its internal conceptual and experimental content, it comes as something of a surprise that the Latvian-German physical chemist, Wilhelm Ostwald (1853-1932), has attracted so little attention from this audience. Certainly few other late 19th-century and early 20th-century chemists so closely approximate the ideal of a Renaissance Man than did Ostwald, for in addition to his Nobel-Prize winning work in physical chemistry, he was a tireless leader and organizer of scientific journals and societies, an active atheist and proponent of Haeckel's monism, a promoter of the universal language movement, a relatively decent philosopher and historian of science, one of the leaders of the energetics movement in Germany and one who very early recognized the role of energy and pollution in human history, the originator of one of the major systems of color classification, and an author of books on the theory of oil painting, as well as a relatively talented painter himself. Yet the only

full book-length biography of Ostwald I am aware of is the 1969 Russian biography by Rodnyj and Solowjew, which, though available in German translation since 1977 (N. I. Rodnyj, J. I. Solowjew, *Wilhelm Ostwald*, Teubner, Leipzig, 1977), has never been translated into English.

Central to the writing of any future biography of Ostwald is his own three-volume autobiography, *Lebenslinien*, originally published by Klasing & Co. GmbH of Berlin between 1926 and 1927, and now made available once more in a single-volume, corrected reprint sponsored by the Saxony Academy of Science of Leipzig. In contrast to the original edition, with its blue matte covers and 5.5 x 8.5-inch format, the reprint is in an 8 x 12-inch double-column journal format and is officially volume 61 of the Academy's transactions. It comes with a new forward and introduction, explanatory footnotes by the editor, Karl Hansel, and a photographic appendix which contains many items not found in the original edition.

It goes without saying that the editor and Academy of Science are to be congratulated in making this important document once more accessible to historians of chemistry. It is an indispensable source of information on the life, thought, and times of this important chemist, and will hopefully plant the seeds that will eventually lead to a full book-length, English-language, biography of this fascinating man. *William B. Jensen, Department*

Dyes Made in America 1915-1980: The Calco Chemical Company, American Cyanamid and the Raritan River. Anthony S. Travis, Sidney M. Edelstein Center, Hexagon Press, Jerusalem, 2004, xiv + 582 pp; ISBN 965-555-149-0, £ 60. Distributed by Jeremy Mills Publishing Limited.

“The plant is so vast, it uses 20 million gallons of river water daily, burns 700 tons of coal a day.... Its tools range in size and ease of dexterity from a small delicate laboratory scale to a crane with a 100-foot long boom.... Its employees produce one patentable idea a week, a profusion of products as diverse as a resin to protect silk and a pharmaceutical to aid ulcer victims.... This CHEMICAL CITY has ... a power house supplying enough electricity to serve a community of 30,000 families ... [and] was one of two locations in the State at one time to have the new electron microscope. ... It produces the most beta naphthol of any source in the U.S. It operates one of the largest biological waste treatment plants in the world.” (pp 29-30).

In quoting this passage from a 1965 publication of American Cyanamid's Organic Chemicals Division, Tony Travis has laid down the scale and scope of history of the Calco Chemical Company. This is the history of the production site at Bound Brook, New Jersey, of an early dye-making enterprise that became a major and diversified corporate division, and of management strategies and their impacts on R&D. At the same time, it is a history of the rise and fall of the American dyestuffs industry, and an exemplary study in environmental history. To my knowledge, it is the first work dealing with the history of chemical industry that combines these several strands in a single volume. With this book, a well-researched and documented study becomes available that draws on and explains the interplay of R&D, production, management, and pollution and regulation, all presented within the history of a single chemical plant.

The roots of Calco were in a burlap manufacturing firm, its brand name Cott-A-Lap providing the first three letters of the company's name. In 1915, when World War I made imports of German dyes difficult (and soon impossible), the board decided to take on production of dyestuff intermediates at a site in northern New Jersey, some 20 miles from the Atlantic coast line and adjacent to the Raritan River. The new enterprise, the Calco Chemical Company, followed, and sometimes set, trends in the American organic chemical industry. After adding numerous dyestuffs and pigments to its

portfolio, Calco in 1927 opened one of the first U.S. industrial research departments. In 1929, American Cyanamid acquired Calco, which became effectively the organic chemicals division of this industrial giant. The division, America's largest producer of sulfa “wonder” drugs, took up manufacture of amino plastics, which first brought colored plastic items into U.S. households. At the same time, its personnel led the introduction of physical methods into chemical analysis and set standards for instrumental color matching. During World War II, the Calco division contributed substantially to the efforts of the Allied forces, with intermediates for explosives and the processing of rubber, as well as melamine laminates for protection of maps, and drugs to heal the wounded. After 1945 the division maintained an innovative pace in dyestuffs, polymers, and agrochemicals. The conversion of discontinuous, “batch” manufacturing into continuous production processes received considerable emphasis. In addition, what from the early 1950s was formally known as the Organic Chemicals Division collaborated closely with American Cyanamid's biological unit, Lederle Laboratories. From the late 1960s, however, increasing competition, management failures, and lack of innovation contributed to a crisis, which in 1980-82 led to the end of most chemical production at the Bound Brook site. This happened as American Cyanamid increasingly moved into the life sciences, which had been made possible by the tremendous research experience accumulated at its now near-defunct Organic Chemicals Division.

As early as the beginning of the 1930s, Calco made efforts to deal with its pollution of both the Raritan River and of the atmosphere. This followed constant public pressure and, increasingly, state involvement that was considerably intensified in the 1960s and 1970s. While American Cyanamid took measures to avoid heavy capital investment in waste treatment and became engaged in litigation with state agencies, it did, when forced to do so, apply state-of-the-art technologies for environmental protection, and contributed substantially with R&D to their evolution. The third, activated carbon, stage of its wastewater treatment facilities opened in 1977, only a few years before the production site closed shop.

Travis tells these stories in two parts, the first one dealing with the organizational and R&D history, the second with environmental issues. Nevertheless, they are shown to be closely interrelated and connected by two very useful summary chapters at the end of each part. Also in many other ways, *Dyes Made in America* is two books in one. Travis gives documentation of sources high priority, sometimes quoting extensively from reports,

letters, and interviews. This adds a feeling of immediacy to a story that does not lack detail in presenting facts. Furthermore, the wide choice of sources (and historical actors) leads to a balanced view. In other words, the author gives the reader not only the opportunity to arrive at his or her own reasoned judgment, but even exerts a subtle push in this direction. However, Travis does not hesitate to present his own analysis on, among other issues, the reasons for decline, the changes in character of chemical production, and, especially, environmental aspects.

The author notes that this book is the third in a trilogy. With his *Rainbow Makers*, Travis tracked the origins of the synthetic dyestuffs industry in Western Europe in terms of a macro-history. The second book, written in col-

laboration with this reviewer, was a biographical study of Heinrich Caro, research director at BASF, the largest German chemical enterprise at the end of the 19th century. Now, with Calco, Travis presents the micro-history of one company's production site. His work on the rise and decline of the dyestuffs industry—once the pinnacle of high technology, a provider of life-style goods and life-saving treatments, prior to becoming an industry in economic crisis, and a major threat to the environment—has come full circle. As he does in his other works, in *Dyes Made in America* Travis weaves knowledgeable explanations of chemical processes together with insightful analyses of structural history into a form that makes complex history understandable and accessible. Carsten Reinhardt, *University of Regensburg*.

The Elements of Murder. John Emsley, Oxford University Press, Oxford & New York, 2005; xiii + 421 pp, ISBN 0-19-280599-1, \$30.

The Elements of Murder describes uses and abuses—including homicidal abuses—of five toxic elements. Its protagonists are mercury, arsenic, antimony, lead, and thallium. The book is not really about the history of chemistry; however, it contains much interesting material about chemistry in history, and for that reason it may well capture the interest of readers of the *Bulletin*.

John Emsley, longtime Science Writer in Residence at Cambridge University, is a prolific and popular writer about chemistry in everyday life. *Molecules at an Exhibition* (1998) and his most recent previous book, *Vanity, Vitality, and Virility: The Science behind the Products You Love to Buy* (2004), are fine examples of the genre. He has also written in a more explicitly historical vein in *The 13th Element: The Sordid Tale of Murder, Fire, and Phosphorus* (2000), published in the UK as *The Shocking History of Phosphorus: A Biography of the Devil's Element*. And he has demonstrated a facility for marshalling information about the elements both in the form of a data compendium (*The Elements*) and in a more discursive combination of data and interesting facts (*Nature's Building Blocks: An A-Z Guide to the Elements*, 2001).

Echoes of all of these forms and of these earlier books can be seen in *The Elements of Murder*. Writing about the various hazards of phosphorus led Emsley to investigate other dangerous elements, and *The Elements of Murder* was the result. What he tells us about these elements includes basic information that might be found in a compendium—abundances, typical sources, and common uses. These everyday uses are what put the elements into contact with people, where they could cause harm. Emsley also highlights the uncommon use of the elements mentioned in the title: as means to commit murder.

After an introductory chapter on toxic elements in alchemy, each of the five protagonist elements appears for several successive chapters. Each element is introduced through background information such as environmental abundances and sources, typical concentrations in humans, harmful concentrations, sites in the body where the element tends to accumulate, and the like. The narrative then turns to ways in which human beings might encounter these elements or their compounds, whether in medicines or pigments or through occupational exposure. Each element's section concludes with rather detailed descriptions of murders committed by means of that element. The book's final chapter treats 12 other poisonous elements in a summary fashion. More than 30 pages of supplementary material round out the book, including a glossary that defines some technical terms

and acronyms, a bibliography that points to sources of additional information, and an index.

The book is well organized, as the previous paragraph suggests, but it is not arranged historically. It is, however, easy to find material of historical interest in two broad categories. One such area is the history of chemical technologies: an overview of how the protagonist elements and their compounds were used. The other area comprises specific cases of how the elements harmed or killed particular people in history.

Several of the featured elements were employed in medicines, perhaps surprisingly, in view of their toxicity. For instance, a great many medicines contained mercury in forms ranging from the inorganic mercury compounds used by Paracelsus to treat syphilis to the ethyl mercury compound thimerosal used as a vaccine preservative. Pigments were another common application of several of these elements. The arsenic-based compound orpiment (As_2S_3) was popular among artists as a golden-yellow pigment until it was displaced by the lead-based chrome yellow (lead (II) chromate, PbCrO_4). Another lead compound, tetraethyllead, was used as an anti-knock additive in gasoline recently enough so that gasoline pumps still read “unleaded.” Lead (II) acetate was used much longer ago as quite a different kind of additive; it was the “sugar of lead” used by ancient Greeks and Romans to sweeten wine. *The Elements of Murder* is filled with information of this sort on the great variety of uses of its featured elements, past and present.

In my opinion, the exploits of notorious poisoners are the least interesting part of the book. The many 19th-century cases of rogues poisoning a series of spouses and lovers ran together in my mind. The fault is not with

the author, whose writing here is clear and meticulous as throughout the book. The fault, rather, is with the repetitive monotony of the subject, perhaps a reflection of the banality of evil.

The poisonings I found much more interesting were ones that involved famous victims rather than infamous perpetrators. British Kings Charles II and George III, composers Beethoven and Mozart, Isaac Newton, and Napoleon Bonaparte are among the celebrated figures who may have been harmed by encounters with the book's featured elements. Poison and murder have long been suspected by some in Napoleon's death. It is difficult to say whether poison was the cause of his death; however, Emsley carefully reviews the evidence as well as possible mechanisms for exposure to more than one toxic element. When Napoleon became ill in the spring of 1821, his doctors treated him with tartar emetic (potassium antimony tartrate) and later with calomel (Hg_2Cl_2). Posthumous analysis of his hair showed high levels of arsenic, which may have been inhaled in the form of trimethylarsine. Emsley describes how many humbler 19th-century Europeans were poisoned in this way by their wallpaper: microorganisms that grew in damp rooms converted arsenic from the pigment Scheele's green (CuHAsO_3) to the volatile trimethylarsine.

Emsley's account of arsenic poisoning by wallpaper-damaging microbes represents the best of *The Elements of Murder*, by which I mean most of the nonmurderous parts. It is an exposition of one of the myriad fascinating and complex interactions between people and chemicals that makes chemistry so interesting—whether from the past or the present. *Carmen Giunta, Le Moyne College, Syracuse, NY 13214-1399.*

Elegant Solutions: Ten Beautiful Experiments in Chemistry. Philip Ball, Royal Society of Chemistry, London, 2005, viii + 212pp, ISBN 0-374-22979-1, £19.95.

What are the Ten Most Beautiful Chemical Experiments? This is a question that can excite both chemists and historians. In his latest book, Philip Ball readily admits that lists such as this are bound to be contentious. Even the definitions of ‘beautiful’ and ‘experiment’ are fraught with controversy. Ball invites, even revels, in the

heated debate that such endeavors generate; and his book provides an excellent starting point for such discussions. For Ball, both art and science are blended in the traditions of chemistry, and this blend leads to his notions of what constitutes both ‘beauty’ and ‘experiment.’ All the beautiful experiments selected for this book were “shaped by human attributions: invention, elegance, perseverance, imagination, ingenuity.” The historical trip through all ten beautiful experiments covers several centuries and makes for an engrossing book.

Ball begins *Elegant Solutions* with Francis Bacon and experimentation. To be beautiful, experiments should be more than “perfectly and elegantly designed to yield an insight about the way the world works.” For Ball and Bacon, experimentation should also be about art or *techne*; it should be about the craft of making things, both the objects of nature and novel things conceived by people. Chemistry, its values, and its ability to create and characterize new things do indeed need more emphasis in popular and scholarly writing on science and technology. Ball likens chemistry to engineering: “beauty need not lie in the conception or execution, but in the product.” Though ‘elegant’ is the first word of his title, Ball wants beauty in experiment to be about more than elegance or simplicity. Elegance and simplicity are just two of the elements of Ball’s conception of beauty. His ten chapters are each subtitled so as to pair ‘beauty’ with one of these elements: quantification, detail, patience, elegance, smallness, simplicity, imagination, simplemindedness, economy, and design. These may not be every reader’s choices, but Ball’s reasoning is sound and his aesthetic examples do cover a profound range of what makes experiments beautiful. His focus on the aesthetics of chemistry is refreshing.

Ball presents not an in-depth exegesis for each of his chosen experiments, but a clear explanation of its scientific content and import, situated in a broad historical and cultural context. In the end, his book is as much about people—mostly chemists—as it is about experiments, and rightly so. His experimental protagonists range from the seventeenth-century Flemish physician, Jan Baptista van Helmont, to the Curies, to Pasteur, to a number of twentieth-century chemists, including Stanley Miller, Neil Bartlett, and Leo Paquette. Experiments for Ball are not necessarily singular processes, but can comprise a series of investigations conducted over time, such as the twelve-year collaborative synthesis of Vitamin B₁₂ by Woodward and Eschenmoser or the Curies’ “ravishing” “years-long experiment” to isolate radium. While most of the book deals with the twentieth-century, an early chapter on Henry Cavendish and the constitution of water is a fine example of how Ball treats experiment. In this fourteen-page essay, Ball brings to life a major episode in pneumatic chemistry and the emergence of modern

elements from the realm of phlogiston. He does this gracefully—complete with the politics, patronage, and personalities of science in the late eighteenth and early nineteenth centuries—telling how water came to be not an element but a compound constituted from inflammable air (hydrogen) and dephlogisticated air (oxygen). Not all Ball’s experiments have withstood the tests of time with regard to being right, but they all illustrate his aesthetic of experiment and the transcendence of beauty.

Ball is the author of other popularizations of chemical science, such as *Designing the Molecular World* and *Stories of the Invisible: A Guided Tour of Molecules*. Commendably, Ball continues to write about chemistry for a general, educated audience, something that is too seldom done today. Ball has always tried to ensure that his books can be understood without scientific training and laments two failings in much of recent history and philosophy of science: the neglect of chemical history and the failure of contemporary authors to treat the aesthetics of chemistry. I highly recommend Ball’s book, which makes much of chemistry—including such concepts as bonding, chirality, and fission—accessible to the nonchemist. Though many readers of the *Bulletin* will be familiar with its contents, it is well written and thought provoking. We can always learn more about what constitutes an experiment—the essential unit of chemical knowledge—and what role experiments played in the history and development of chemical science.

In spite of the book’s inherently episodic structure, Ball ties his tales together with grace, yielding a coherent narrative. I have just two quibbles with the book. First are its two “Divertissements.” While they are very brief and somewhat interesting, these interrupt the flow of the chapters and contribute little to the book’s major themes. Second and more troubling is the lack of scholarly apparatus for references and sources. Although the book contains a modest bibliography, this is not explicitly linked to the text through notes, a bibliographic essay, or any other mechanism. Even direct quotations lack footnotes or page numbers to identify sources. Ball has clearly read widely in chemistry and history, and one wishes for more of a bibliographic apparatus to guide the interested reader to the relevant literature. *Leo B. Slater, Stetten Fellow, Office of NIH History.*

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