

BOOK NOTES

A History of the International Chemical Industry. Fred Aftalion (Translated by Otto Theodor Benfey), University of Pennsylvania Press, Philadelphia, PA, 1991. 435 pp. Cloth (Typeset), \$39.95. Paper, \$14.95.

This volume, the latest in the Chemical Sciences in Society Series of the Beckman Center, is a welcome attempt to fill a serious void. Few books have been written on the history of the chemical industry, and the majority of these are now out of print. For an enterprise which has played such an important role in the development of modern society, this lack of books is surprising.

Spitz, in his 1988 book, *Petrochemicals*, dealt with the complexity of this topic by providing an overview, then covering only a few representative episodes in depth. Aftalion not only provides a broad analysis of the industry but also reviews hundreds of specific companies and products. Despite the diversity and number of topics covered, this is a very readable book, which is an indication of the quality of both the original text and the translation. The narrative is presented in historical sequence, interspersed with numerous short sections on the main products, new processes, and company reorganizations during each period.

The first industrial chemicals were mainly inorganic substances used in the production of soap, glass, textiles, or fertilizers. Soon, better trained chemists and more systematic methods resulted in improved production processes and synthetic substitutes for some of these basic materials. The author points out that by the beginning of World War I, Germany had significant advantages in chemistry over both England and France. In Germany, the strong demand for organic chemicals to make synthetic dyes had encouraged industrial expansion; there was a good working relationship between universities and chemical companies, and adequate funding was available for research. Thus, the chemical industry was important for the war effort of all of these countries but was especially so for Germany.

After World War I, cartels and pricing agreements discouraged international competition. In Germany, the United States, and England, consolidation produced giant corporations, such as I. G. Farben, Du Pont, and ICI, but the lack of such mergers in France left its chemical companies small and fragmented. Here, as throughout the book, Aftalion describes not only the activity in these major industrial nations, but also in countries such as Belgium, Japan, Italy, and the Soviet Union, whose contributions are sometimes neglected.

The international chemical industry played a key role in World War II. Synthetic rubber, catalytic cracking to obtain aviation fuel, synthetic polymers, and large-scale production of penicillin are only a few of the vital contributions that Aftalion discusses. This was also the period during which the

U.S. solidified its position as the world leader in petrochemicals. By the war's end, this country had established a lead in the chemical industry which lasted over 25 years.

The 1973 oil embargo and the ensuing higher cost for petroleum feedstocks were a severe blow to a business that already suffered from over-capacity for many major products. Aftalion tells about this period of intensive reassessment and reorganization from the perspective of someone who has been personally involved with the French chemical industry from 1951 to the present. He evaluates many of the developments during this time, ranging from the effects of leveraged buyouts in the U.S. to the problems that large multinationals encountered in trying to emphasize specialty chemicals. His analysis of current issues is a particular strength of the book.

Unfortunately, this book lacks the structure that historians of chemistry would normally expect. No references are provided, nor is there a general bibliography. Two helpful indices include names of individuals and companies, but the lack of a general index will be an inconvenience to readers who wish to review events related to a specific country or product. These omissions are especially regrettable in an otherwise good book.

This is a well-written, comprehensive work in a field where too few resources are available. It should be valuable to chemists and others who wish to learn more about the history of the chemical industry. Historians of science will be particularly sensitive to its shortcomings but may still find it to be useful. *Harry E. Pence, Chemistry Department, SUNY-Oneonta, Oneonta, NY 18320.*

Chemistry. Hélène Metzger (Translated by Colette V. Michael), Locust Hill Press, West Cornwall, CT, 1991. xv +151 pp. Cloth (Typeset), \$25.00.

This book is an English translation of a short history of chemistry written by the French historian Hélène Metzger in 1926 and first published in 1930 as part of the French historical series *Histoire du monde*. The translator has added additional explanatory notes and an index and has supplemented Metzger's original bibliography and biographical appendix (as well as dispersing the latter throughout the text in the form of footnotes). Aaron Ihde of the University of Wisconsin has also provided a new foreward.

While I am delighted to have this work available in English, there are, in my opinion, a number of problems connected with both the mechanics and motivation for this translation. The first and most serious of these is the decision of the translator, Colette Michael, to opt for "a literal translation in order to keep the style conveyed by the author". Despite her claim that changes were made whenever "the rendering in English became so awkward that it detracted from the meaning", the resulting English syntax does exactly that at every turn. Elabo-

rate run-on sentences separated by endless semicolons, unusual verb tenses, uncommon word choices, and curious inversions of word order abound on every page, not to mention incomplete and incorrect translations.

As an example of the incomplete category, one encounters on page 129 the sentence:

With these speculations, and these and similar experiments, chemists were thus invited to orient themselves on the road indicated by Berthollet's static but which nobody had thought to travel.

Whatever the original French, there is no way to render this into clear English with out expanding the word "static" into "static approach to chemical equilibrium". Likewise, on page 60 we read, concerning the use of the pneumatic trough, that:

To prevent these gases from escaping into the atmosphere, the experimenter collected them in a vessel he plugged with care as soon as it was blown ...

Presumably this refers to the inflation of gas bladders but instead suggests the quaint image of a glass blower standing at the collection end of a pneumatic trough and providing freshly blown bottles which are then sealed prior to collecting the gas.

Examples of the mistranslation category are even more numerous: on page 8 we are informed that in the 17th century reagents "were of all shapes" (rather than the containers in which they were stored); on page 10 we are told that 17th century chemists were concerned with the alkalis "sodium and potassium" (rather than soda and potash), whereas on page 92 we are told that the symbol K stands for potash (rather than potassium); on page 16 alchemy is "swept out of the science of mechanical philosophy" (rather than swept out of science by mechanical philosophy); on page 46 affinity tables allow chemists to "predict what would happen if certain reagents were mixed for the first time", but apparently not for the second or third time (rather than allowing chemists to predict, for the first time, what would happen when reagents were mixed); on page 69 Lavoisier explains combustibility by "invoking the chemical structure of the combustible substances themselves" (rather than their composition); on page 111 the discovery of scandium, gallium and germanium is "prophetic of Mendeleev's table" (rather than the table being prophetic of the elements); on page 121 the unitary theory becomes the advocate of the idea that all compounds are the result of addition reactions (rather than substitutions); on page 130 the Scottish chemist Thomas Thomson becomes William Prout's "collaborator" (rather than corroborating Prout's hypothesis); and on page 136 J. W. Gibbs become the author of several "phase rules" (rather than "the" phase law).

The sensation that one is reading the instructions to a Japanese VCR, rather than an important work on the history of chemistry, is further reinforced by a variety of spelling and

typographic errors. For example, Thomsen is spelled Tomson on page 130, Kolbe is spelled Kalbe on page 123, benzene is spelled benzine on page 124, and the number 0 is used as symbol for oxygen on pages 107 and 108. Indeed, Tomson is dutifully reproduced in the index, whereas both Kalbe and Kolbe are missing, as is the term "electrochemical dualism", despite the fact that it is the subject of an entire section of the text. In short, Metzger would have been better served by a freer translation and some decent editing. A popular history is, after all, not a work of literature, and clarity rather than style must surely be the prime objective.

Even more curious, however, is the motivation for this translation. Metzger wrote any number of works that virtually all historians of chemistry would agree deserve to be translated. Most notable are her *Les doctrines chimique en France du debut du XVIIe siecle à fin du XVIIIe siecle* (1923) and her *Newton, Stahl, Boerhaave et la doctrine chimique* (1930). Even her doctoral thesis, *La genèse de la science des cristaux* (1918), and her volume on the philosophy of science, *Les concepts scientifiques* (1926), are not without current interest. Given all this, why was this particular book, which is surely her weakest and least important work, chosen instead? The answer perhaps lies in the observation that this translation forms Volume 1 of a projected series entitled "Women in the Sciences" and that the choice had more to do with length and the interests of an audience totally unrelated to historians of chemistry - but in these halcyon days of political correctness one belabors the obvious only at great risk.

Suffice to say that as a history of chemistry this work is of questionable importance. Originally intended for a lay audience, the treatment is extremely brief and impressionistic, and most of the quotations are not properly referenced. The most satisfactory portion of the book is Part I, which is largely a summary of Metzger's more detailed works on 17th and 18th century chemistry. Here, despite the brevity and awkward English, the brilliance of Metzger's interpretive genius manages to shine through, though I question whether this would be apparent to a layman without any previous background in the history of chemistry. If anything, these flashes of brilliance merely underscore the desirability of having these more substantive works properly translated. Part II of the book, which deals with 19th century chemistry and which terminates around 1870, is much less satisfactory. It quickly degenerates into a list of names, most of which are French (indeed, the entire book is decidedly biased toward the contributions of French chemists) and into vague generalized statements about theoretical trends. The treatment of 20th century chemistry is, of course, nonexistent.

Having said all of this, I must now confess to a great sense of guilt. Translations of works in the history of chemistry are so rare these days that one hates to discourage any attempt, however ill-conceived. This is especially true of the work of French historians, where not only Metzger's important books

still await the pen of the translator, but also several by Pierre Duhem, including his 1902 study of chemical composition, *Le mixte et la combinaison chimique*, and his 1893 history of chemical thermodynamics, *Introduction a la mécanique chimique*. William B. Jensen, Department of Chemistry, University of Cincinnati, Cincinnati, OH 45221.

The Historical Development of Chemical Concepts. Roman Mierzecki, Kluwer Academic Publishers, Dordrecht, and Polish Scientific Publishers, Warsaw, 1991. xi + 281 pp. Cloth (Typeset), \$129.00.

This book is an English translation of a Polish history of chemistry first published in 1985 and now appearing as Volume 12 of Kluwer's history of chemistry series, "Chemists and Chemistry". The text is organized around the history of selected topics, rather than being a continuous chronological account, and is supplemented with five tables summarizing significant dates and events.

The book is divided into five chapters, the titles of which somewhat imperfectly reflect their contents: 1. "Division of the History of Science" (i.e., a survey of significant historical periods); 2. "The Element" (i.e., the evolution of the concept of a chemical element); 3. "The Elementary Particle of Matter" (i.e., the evolution of the atomic-molecular theory); 4. "The Structure of Chemical Compounds"; and 5. "Capacity of a Substance for Transformation" (i.e., the history of chemical kinetics and thermodynamics). Despite the awkward chapter headings, the quality of the English translation is generally good, though the quality of both the typography and illustrations leaves something to be desired. As implied by the chapter headings, the book is directed primarily at chemists, rather than historians of science, and tends to focus on the internal evolution of chemical concepts, rather than on external sociological and cultural factors.

Unlike many recent histories of chemistry, the book makes use of extensive quotations from the primary literature, much of which the author actually appears to have read. However, I must confess that I do not always agree with his interpretations and in some cases rather curious errors have crept in. Thus, on page 179, we are told that Charles Gerhardt was an advocate of the idea that a chemical formula should represent the actual physical structure of a molecule, when in fact he was the leading advocate of exactly the opposite view.

However, the most serious problem with this otherwise interesting book is that it has been parochialized relative to the history of Polish chemistry. No less than 20 Polish chemists and alchemists appear in the index and one repeatedly comes upon statements that will raise the eyebrows of most chemists and historians outside of Eastern Europe and Russia, such as the claim that:

Wladyslaw Natanson, a professor at the Jagiellonian University in Cracow, is considered to be one of the founders of the modern thermodynamics of irreversible processes.

Likewise, 15 index entries and five continuous pages of text are devoted to the writings of the 18th-century Polish chemist, Jędrzej Sbuadeck, while Avogadro merits only four index entries and one page of text, and Irving Langmuir rates only two index entries and one sentence of text. Had this material been collected into a single chapter (or appendix) on the evolution of chemistry in Poland, it would have been a valuable contribution, but by dispersing it among the various chapters and substituting quotations from obscure Polish chemists and textbooks for the more standard and historically more significant references, the author produces a distortion which would largely preclude the use (outside of Poland, at least) of this book as a text in a standard history of chemistry course, had this fate not already been guaranteed by the outrageous price. William B. Jensen, Department of Chemistry, University of Cincinnati, Cincinnati, OH 45221.

Nineteenth-Century Attitudes: Men of Science. Sydney Ross, Kluwer Academic Publishers, Dordrecht, 1991. xi + 235. Cloth (Typeset), \$69.00.

This book is a collection of seven essays (all but one of which are based on previously published articles) dealing with various topics related to the history of natural philosophy and chemistry in the first half of the 19th century, and forms Volume 13 of Kluwer's history of chemistry series, "Chemists and Chemistry". In assembling the essays for republication, Ross has incorporated numerous revisions and has added an epilogue and index. In addition, all of the essays have been reset in uniform type and are well illustrated with portraits and diagrams. Indeed, both the typography and the quality of the illustrations are above average.

The titles of the seven essays are "Scientist: The Story of a Word"; "The Story of the Volta Potential"; "The Search for Electromagnetic Induction: 1820-31"; "Faraday Consults the Scholars: The Origin of the Terms of Electrochemistry"; "Herschel and Hypo"; "Herschel on Faraday and Science"; and "Herschel's Marginal Notes on Mill's *Liberty*". The essays are both well-written and scholarly, if somewhat antiquarian in flavor, and the choice of subject matter appears in many cases to have been dictated by original documents, memorabilia, and books personally collected by the author over the years. Those who love old books, and especially books on science, are sure to find at least one, if not several, essays that will be of interest. William B. Jensen, Department of Chemistry, University of Cincinnati, Cincinnati, OH 45221.

A History of Lactic Acid Making: A Chapter in the History of Biotechnology. H. Benninga, Kluwer Academic Publishers, Dordrecht, 1990. xxi + 478 pp. Cloth (Typeset), \$79.00.

This book, which forms Volume 11 of Kluwer's history of chemistry series, "Chemists and Chemistry", is a detailed history of the lactic acid industry from the 18th century to the present. In order to provide an adequate technical and historical context, a sizable portion of the text is also devoted to the chemistry and bacteriology of lactic acid and to the history of chemistry in general. At times the book has a tendency to lapse into the format of a textbook/review article, and both Benninga's technical emphasis and his method of presentation will undoubtedly appeal more to chemists than to those historians who are primarily interested in the managerial, marketing and political aspects of the chemical industry.

Previous case studies of industrial chemistry have tended to focus on either heavy inorganic chemicals (e.g. soda, sulfuric acid, etc.) or on petrochemicals and dyes. Consequently Benninga's case study of lactic acid as representative of the fermentation industry is a welcome addition to the literature on the history of industrial chemistry. Indeed, the most fascinating portion of the book deals with the challenge presented by the petrochemical industry to traditional fermentation methods in the period between 1945 and 1960.

The weakest aspect of the book is the background sections dealing with the general history of chemistry. Many of these are of poor quality and questionable relevance, and much of this material could have been eliminated without damaging the book as a whole. An even more serious problem is the English, which is marginal in many places and badly in need of a good editor. On the other hand, both the typography and illustrations in the book are excellent and, from the technical point of view, at least, Kluwer is to be congratulated on having produced a quality product.

Contrasts in Scientific Style: Research Groups in the Chemical and Biochemical Sciences. Joseph S. Fruton, American Philosophical Society, Philadelphia, PA, 1990. ix + 473 pp. Cloth (Typeset), \$40.00.

Readers interested in the history of biochemistry will need no introduction to the work of Joseph Fruton. Beginning with his 1972 book, *Molecules and Life: Historical Essays on the Interplay of Chemistry and Biology*, and proceeding through the editions and supplements of his *Bio-bibliography for the History of the Biochemical Sciences since 1800*, and, most recently, his book of essays, *The Skeptical Biochemist*, Fruton has established himself as one of that most rare of breeds - a practicing scientist who is also an extremely well-read and sophisticated practitioner of the history and philosophy of science.

In the book under review, Fruton turns his attention to the question of the definition of research groups and styles in science, drawing his examples from 19th century chemistry and biochemistry. To what extent do the students and postdoctoral assistants of a famous scientist truly constitute a research group? What percentage of the students continue on in the same area of research as their mentor? To what extent do they study the same problems? To what extent do they diversify? To what extent do these percentages reflect the managerial style of the mentor, his ability to obtain funding, his laboratory skills, his abilities as an educator, etc.? These are but some of the questions raised by Fruton and addressed in his study of the research groups of Justus Liebig, Felix Hoppe-Seyler, Willy Kühne, Adolf von Baeyer, Emil Fischer, and Franz Hofmeister. In addition to an extensive bibliography and name index, Fruton has also included seven appendices (covering over 145 pages) giving brief biographical accounts of the students and assistants for each research group.

This is an innovative and very interesting approach to the question of the role of intellectual mentorship in science and should be of interest not only to sociologists and historians of science, but to young scientists on the verge of establishing their own research groups, or even to those all-too-numerous chemists whose historical interests run no deeper than the establishment of their own personal chemical genealogies.

Atti del III° convegno nazionale di storia e fondamenti della chimica. Ferdinando Abbri and Franco Crispini, eds., Brenner, Cosenza, 1991. 402 pp. Paper (Typeset), L. 35.000.

This volume is a collection of 32 papers presented at the 3rd National Congress on the History and Foundations of Chemistry held at the University of Calabria in 1990. All but three of the papers are in Italian and about half deal with the history of Italian chemistry. Compared to the somewhat feeble attempts to preserve the proceedings of many symposia on the history of chemistry held in the United States and elsewhere, this is an impressive publication that others might well take as a model.

Some Recollections of Gap Jumping, Sir Derek H. R. Barton. American Chemical Society, Washington, D.C., 1991, xxiv + 143 pp. Cloth (Typeset), \$24.95.

Sir Derek Barton's contribution to the J. I. Seeman series "Profiles, Pathways, and Dreams" provides a fascinating kaleidoscope of the life experiences, research interests, intellectual accomplishments, and personal philosophy of one of the key players in the "Golden Age" of organic chemistry. A primary goal of these books is to provide the reader with a clear understanding of each author's evolution as a researcher. The emphasis on evolution is particularly significant because so

much of the lastingly important science produced by the various contributors to this series is obviously more than simply the result of intelligence (or luck) in having picked the right scientific problem to investigate. Just as important is the scientist's advancing sophistication in knowledge and understanding as the research project matures. Ultimately, a collection of key insights required for a breakthrough solution must come within the investigator's grasp. From the perspective of the historian of science, as well as the working bench scientist, the three crucial elements in a major scientific advance are as follows: (1) defining exactly what is the key question that should be asked, (2) deciding how the problem could best be first approached and (3) being flexibly pragmatic in order to develop fresh, new pathways of attack if the first assault fails. Because aspects of these processes are often intense personal reflections of an individual's intellectual style, a carefully crafted discussion - high in candor, precise in its analysis, and factually accurate - can provide a powerful lesson in inspired creativity as exhibited by that particular scientist. Sir Derek's efforts to show us how he has made his own breakthroughs have left us an extremely valuable legacy.

As with many books in this series, the title of the volume is quite illuminating. By "gap jumping", Barton refers to the ability of a researcher to bring together two disparate points of view and then to bridge the gap. This juxtaposition and fusion has some of the elements of a "grand synthesis", as the phrase is used by Kuhn to describe one crucial stage in scientific revolutions. While "gap jumping" is perhaps best illustrated by Barton's melding together aspects of chemical physics and steroid reactivity in order to develop conformational analysis, the contribution that led to the 1969 Nobel Prize in Chemistry, he stresses the use of gap jumping over and over again in all of his major studies.

Barton takes great care to point out how important to a young scientist is the achievement of intellectual acceptance by the scientific elite. Each generation of scientists must pay its dues. Brilliant minds are able to compete with one another in an exciting duel of the intellect but only if they can simultaneously maintain grace, wit, honesty, and good manners. Science may be deeply competitive but there is no place for real viciousness (although an occasional umbrella-bashing may be in order). As a man obviously blessed with both extraordinary ability and tremendous self-confidence, Barton repeatedly emphasizes the importance of being able to carve out his own research niche and yet, at the same time, having to gain the respect of acknowledged leaders on the basis of his continued ability to produce work of the highest quality.

Barton appears to have pursued a relatively small number of interests but to have done so in such a way that every time he returned to an earlier topic, his creativity was able to add an innovative twist as a significant new embellishment. In addition to his lifelong interest in free-radical chemistry, he has also been keenly involved in elucidation of natural product

structures using degradative and chiroptical techniques, in natural product total synthesis, in the discovery of novel photochemical and thermal reactions, in uncovering biosynthetic pathways, and, of course, in stereochemistry.

Barton's intellectual curiosity has been constantly stimulated by the obvious one-to-one correlation of molecular properties with molecular structure. On a modern level, in exploring a reaction that is known to succeed with a simple prototype, the first stage of such an investigation would involve a systematic examination of what influence structural modifications might have on the facility of that reaction. Typically, as an assumed first approximation, one can examine separately both the electronic as well as the steric effects induced by these systematic variations. Resonance and inductive features, when taken together, often provide useful predictive information regarding electronic influences on chemical reactivity. For instance, practical organic chemistry of aromatic materials - such as the logically planned, and financially lucrative, synthesis of dyestuffs - involves application of these electronic considerations. While classical stereochemistry was one of the earlier intellectual triumphs of organic chemistry (as the work of giants such as Pasteur, LeBel, and van't Hoff attests), certain kinds of steric effects were not nearly so easily understood. In spite of the well-accepted tetrahedral carbon atom, when Barton first became an active researcher most practicing chemists were more or less satisfied by visualizing cyclohexane rings as essentially planar. While in some situations this approximation works reasonably well, to explain the detailed chemistry of the steroids, such a model was obviously unsatisfactory.

When functionalized steroid reactivities first began to be explored in the 1940s, there occurred a unique historical phase characterized by the exciting concurrence of several key elements: (1) the isolation of important, biologically active molecules, (2) the uncovering of useful, but somewhat inexplicable, chemical behavior especially in regards to product distributions, (3) the availability of new physico-chemical techniques, and (4) the presence of several unique individuals. Thus the medicinal promise of the steroids (e.g. cortisone) was of such transcendent importance that it had attracted the efforts of hundreds of organic chemists who now possessed powerful new tools (e.g., efficient column chromatography and, soon thereafter, infrared spectroscopy) to help them understand the progress of functional transformations and to purify previously intractable mixtures. Data was produced at an astounding pace. Finally, there were present on the scene several enormously talented individuals who each contributed to the unique, almost revolutionary, development of this field.

A psychohistorian could engage in an intriguing analysis of the roles played by Fieser, Woodward, and Barton. Fieser, an eminent steroid chemist with an encyclopedic grasp of factual organic chemistry, was an acquaintance of Barton's. Fieser influenced Barton's work both by devising a semi-workable

explanation for some of the observed steroid steric effects and by helping to secure for Barton a temporary post at Harvard as a sabbatical replacement for R. B. Woodward. Fieser was a traditional organic chemist whose forte was not mechanistic subtlety. On the other hand, Fieser knew a great deal of chemistry and thereby helped direct Barton's attention to many "anomalies" (e.g., gaps in understanding). Woodward, of course, was the genius who acted as a catalyst in unraveling the basis of conformational analysis. Woodward's mechanistic sophistication and clarity of vision were crucial in helping Barton to crystallize his thinking in the now classical *Experientia* paper which was the basis for his Nobel Prize. Repeatedly, Barton pays homage to the significance of Woodward in helping to gain acceptance by the chemical community of Barton's abilities. Barton even goes so far as to describe the exact process by which Woodward eventually communicated to Barton that Barton had now qualified as a trusted member of the inner circle of organic chemists. How tragic that Woodward, whose impact appears over and over in this autobiographical series, died so prematurely. Despite the fact that he is beyond contributing his own volume, he has, in effect, become a ghostly spirit hovering over the "Golden Age".

Barton's research evolution has a spectacular way of fitting together. His first major independent academic study, a systematic compilation of molecular rotation differences, gave him an opportunity to correct previous errors in the literature as well as to assign empirically new structures for a wide range of natural products. Besides indulging and challenging his lifelong curiosity about natural products, the very nature of this investigation would inevitably have sensitized Barton to the three dimensional features found in complex natural products. If, as Pasteur asserted, "Chance favors the prepared mind", then from the very beginning of his independent career Barton has been making the necessary preparations by continuously meditating about structure and stereochemistry. The intellectual challenge of trying to understand just why a particular organic structure could elicit so specific a biological response had to be endlessly appealing. Later, after Barton had developed conformational analysis, his natural product investigations moved towards those materials that might have been formed by radical coupling reactions. Here was a marvelous exercise that allowed the marriage of free-radical chemistry (the field that had formed the basis of his Ph.D. research) with structure elucidation of complex materials. Barton's biosynthetic insights, beginning with his correction of the structure of Pummerer's ketone and extending to his theory regarding the origin of the morphine alkaloids, were all logical consequences of careful, but straightforward, consideration of possible free-radical pathways. Such a feat no doubt was, in an aesthetic sense, highly satisfying to Barton. His style of combining projects that involved structure elucidation, stereochemistry, and synthetically useful free-radical chemistry is nowhere better illustrated than in his creation of a brand new approach

that made the key steroid metabolite aldosterone accessible from simple starting materials. Only an individual intimately involved in stereochemical thinking would see, in the aldosterone precursor utilized in his successful synthetic route, the possibility of a putative cyclic transition state involving an interaction through empty space of components separated by several atoms.

Much of Barton's later work has involved the direct invention of new organic reactions. Why would Barton want to "invent" synthetic transformations? Just as there is one type of knowledge that comes from the human mind's ability to participate in pattern recognition, so too, there is another kind of understanding that emerges from being able to predict reactivity.

If we can imagine a unique interaction of various reagents and intermediates that are able to accomplish a useful and non-obvious transformation, then we certainly must know something about the underlying chemistry. The collection of established organic chemical reactions did not spring into being all at once. Instead, a rich history has taken place wherein serendipity and personality played major roles. This is best illustrated by the fact that there are literally several thousand known reactions that are immortalized by the name of the individual(s) who first uncovered their novel chemistry. Before the advent of modern spectroscopic and diffraction techniques, structure elucidation by wet-chemical techniques was both an art and a science. In the application of relatively simple reagents, such as concentrated aqueous sodium hydroxide or molecular bromine, to a complex natural product, much could be learned by systematic examination of the resulting molecular debris. In large measure, the simplicity of the reagents used in the distant past often helped make the chemical behavior more clearcut since one was more likely to generate low to moderate concentrations of a single reactive intermediate that could then react with the molecule's remaining functionality. In the post-World War II period, these classic structure elucidations became much less popular. As a consequence, a major path by which totally unprecedented chemistry might be observed was in danger of extinction.

As Barton's chemical abilities increasingly matured during the 1960s and the 1970s, his attention turned more and more towards crafting new reactions by the logical analysis of intermediate generation and predictable chemical behavior. While his aldosterone synthesis is arguably the best example of such an invention, his neat approach to highly hindered alkenes via selenoketones and his long series of valuable new synthetic methodologies that employ free radicals have also had a very significant impact on organic chemistry. The rebirth of free-radical chemistry during the 1970s and 1980s is in no small measure a direct outcome of Barton's contributions. To have moved from having been a major contributor of stereochemical insights to becoming a "Thomas Edison" of new synthetic tools is merely one more indication of the breadth and dedica-

tion of this unique scientist. *John Belletire, Department of Chemistry, University of Cincinnati, Cincinnati, OH 45221.*

LETTERS

The Faraday Issue

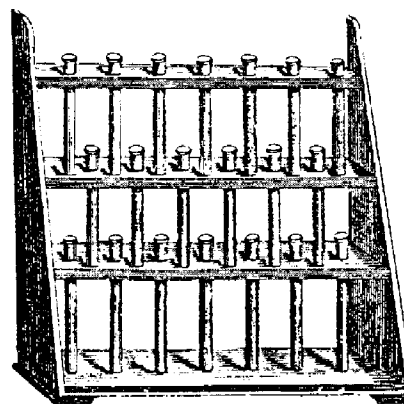
* My hearty congratulations on the Faraday number of the *Bulletin*. It's splendidly put together, a very good read and will be very useful. *William H. Brock, University of Leicester*

* In my article "Michael Faraday and the Art and Science of Chemical Manipulation" (No. 11, Winter 1991) I noted that Faraday's description of his home-made test tubes and test tube rack in his book *Chemical Manipulation* strongly suggested that "neither was commercially available at the time" and that one was "witnessing the incipient stages of their introduction" into the chemist's laboratory. Though I also added the caveat that, on the basis of this evidence, one could not "definitely assert that Faraday was the inventor of the test tube and test tube rack." Since then, Dr. Derek Davenport has donated a copy of volume 2 of the 1814 American edition of Frederic Accum's *System of Theoretical and Practical Chemistry* (Kimber and Conrad, Philadelphia) to the Oesper Collection. Figure 6 of Plate II of this volume clearly shows a test tube rack and test tubes, which Accum describes as:

... a test rack or wooden stand, containing glass tubes, for examining small portions of fluids, by the action of reagents or tests, or for dissolving small quantities of earths, or metals, by means of heat over a candle or lamp.

This plate may date back to the first London edition of 1803 or to the second London edition of 1807, and the rack and tubes were almost certainly offered for sale by Accum as part of his business as an apparatus dealer, though I am unfortunately unable to verify either of these surmises as I do not have access to earlier editions of Accum's text nor to his apparatus catalogs. Even if these earlier leads should prove dead ends, the 1814 reference cited above predates Faraday's book by over a decade. *William B. Jensen, University of Cincinnati*

* With every issue I'm increasingly impressed with the vitality, quality, and visual attractiveness of the *Bulletin*. The editorial staff is doing an outstanding job and in the process are making membership in the History of Chemistry Division a pearl of great price. You've put extraordinary value in our dues assessment. Our composition has always tended toward senior age chemists and small college academics. Neither of these groups can physically or financially turn out in great numbers at national meetings, so it has long devolved on the newsletters and bulletins to be the glue that holds us together.



Frederic Accum's 1814 test tube rack

In your hands the *Bulletin* has become a very sticky adhesive. Congratulations! And from this long-time member ... many thanks! *Ned D. Heindel, 1993 ACS President-Elect*

Robert Hare's Lecture Hall

* I was delighted to receive Issues 10 and 11 of the *Bulletin* with the articles on Faraday and the superb historical and pictorial celebration of the art of the lecture demonstration. It's so sad to see what we once had and no longer have. An opportunity to use facilities like Robert Hare's laboratory and lecture hall at the University of Pennsylvania Medical School around 1830 (figure 10) would attract me back to teaching in an instant ... The article - especially the last paragraph - should be required reading for anyone who is involved with education in science in any way - teachers, students, parents, architects, administrators at all levels, perhaps not least of all "education presidents." *Henry A. Bent, Pittsburgh, PA*

* I enjoyed the paper on chemical lecture halls which appeared in Issue 10 of the *Bulletin* (Fall 1991). I suppose you will have recognized that figure 10 of Robert Hare's lecture hall is printed backwards. The figure used is from the second edition of Hare's *Compendium*. An earlier version, published in the *American Journal of Science*, 1830, 19, 26, did not have the electrical apparatus fastened to the front of the balcony. This earlier engraving was also used in some 1828 copies of Hare's *Compendium* (actually published or bound in 1830 as they contain a publisher's catalog dated October 1830). Both of these sources also printed a "Description of the Laboratory and Lecture Room." It is interesting to take the picture and the description and try to reconstruct the original floor plan [see figures on page 46]. The second edition of the engraving appeared in the 1836 edition of the *Compendium*. A closeup of the details of the electrical machine is given in Hare's *A Brief Exposition of the Science of Mechanical Electricity ...* (1835 and 1840). *William D. Williams, Harding University*