

BOOK NOTES

Chemical Sciences in the Modern World, Seymour H. Mauskopf, editor, University of Pennsylvania Press, Philadelphia, 1993, XXII + 417 pp. Bibliography (11 pp), Index (9 pp), 15 x 23 cm., hardbound, dust jacket, acid free paper, excellent printing and workmanship, \$39.95, ISBN D-8122-31 56-2.

Chemical Sciences in the Modern World (CSMW) is a collection of 18 essays by as many authors, varying in length from three to 58 pages (av. 21), which are based on talks given at a conference held in May, 1990, under the auspices of the Beckman Center for History of Chemistry. CSMW is the third book in a series, *The Chemical Sciences in Society*, published by the Chemical Heritage Foundation.

The 16 contributors for whom vitae are given include a museum administrator, an archivist/librarian, a corporate historian, a professor of the history of chemistry and chemical education, and 12 academic science historians.

In the first page or so of a 22-page introduction, the editor cites a few instances of how chemical products have transformed our lives and surroundings "for better or worse." Of the many problems, the one being addressed by these essays is the "invisibility of chemistry to the gaze of the modern historian." Specific objectives include 1) to make visible the best and most sophisticated scholarship in this field to the wider community of historians of science, technology, and medicine and to their students, 2) to serve the needs of historically interested chemical scientists and chemistry teachers, 3) to be useful to those concerned with promoting public understanding of science, 4) to be a valuable resource for those engaged in science policy studies, and 5) to stimulate greater interaction and communication with general historians, social scientists, and philosophers of science.

The 18 essays are grouped into four sections: Practice (5), Production (3), Public Interface (7), and Prospects (3). The first three are mostly analyses of comple-

mentary aspects of the chemical sciences and process industries. The fourth considers the future.

The subject matter is well researched and documented and, individually, each essay adds to the stock of historical knowledge and perspective. The wording of a few of the essays is so ponderous that I had to re-read many paragraphs several times before I thought I understood them. One excellent feature is that authors were able to modify their texts before publication, as a result of new insights gleaned from others at the conference. Since the book is relatively inexpensive, it should be in the hands of every serious student of the history of the chemical sciences.

As a collection, however, the essays were disappointing to me because they failed to represent clearly the status of the chemical sciences in the modern world. Perhaps I was led to expect something different. The illustration on the dust jacket is a reproduction of the frontispiece from A. Cressy Morrison's (b. 1864) 1937 book, *Man in A Chemical World*, which is entitled "Chemical Industry, Upheld by Pure Science, Sustains the Production of Man's Necessities." Therefore, from the dust jacket I inferred that CSMW would continue Morrison's theme but update it by half a century to correct for Morrison's myopia and one-sidedness. Writing as he did at the depth of the great depression and at the height of what historian Williams Haynes called "The Chemical Age", in Morrison's eyes chemistry and its industries could do no wrong, but beckoned all to share in a cornucopia of inexhaustible blessings flowing out of the nation's research laboratories.

Therefore, I expected CSMW not only to tell of the profession's and the industry's achievements during the past fifty years, but to point out what went wrong and to suggest ways in which mistakes can be corrected so that a tarnished public image may regain its luster. Although the chemical industry and the chemical sciences are widely depicted as a public enemy, the phrase is often heard, "There is good chemistry between us." I wanted the essayists to deal with this public ambivalence and to

consider how history can bring economic, political, and social perspective to corporate downsizing, reductions in fundamental research, pollution, superfunds, government red tape, globalization, maturation of glamour industries into makers of commodity products, and the ill preparation of some college graduates to enter the workplace (assuming jobs are available).

By contrast to these essayists, most of whom are far removed from the lab bench, reaction vessel, and business world, Morrison was a captain of industry. Associated with Union Carbide Corporation from 1906 until his retirement in 1930, he had held leadership roles in countless associations, councils, societies, and government agencies. His book, sponsored by the American Chemical Society, was to celebrate 300 years of chemical achievement in America. It was to be written in simple language so as to give the reader "a better understanding of the part that applied chemical science has had in raising the plane of living to a higher level than that enjoyed by any previous generation."

In CSMW, the section on Public Interface came nearest to meeting my expectations. Here, Helen Samuels and Joan Warner-Blewett show the increasing complexity of preserving the record of our chemical past. W. B. Jensen forcefully deals with the barriers to communication between chemist-historians and professional historians. Robert Bud discusses the problems museums

face in capturing the attention of the viewing public long enough to enhance its understanding of science. Christopher Hamlin shows how the study of past environmental disputes makes it quite difficult to see contemporary disputes in terms of "good guys and bad guys." Suzanne White traces the intricacies of regulating chemicals in food in a rapidly changing society since World War II and the resulting conflicts between large and small processed food manufacturers, regulatory bodies, and public advocates for nature's way. Finally, as a capstone, E. W. Brandt uses case histories to show how history can help industry communicate with the public, particularly in times of crisis.

I believe that most people in the chemical professions are very much aware of the widening gulf that lies between a vast and often hostile public on one side and relatively small numbers of chemical scientists and engineers, historians of science and technology, and industrialists on the other. Since bridging that gulf is going to be primarily the responsibility of those of us on the chemical side, an imperative first step will be to find better ways to communicate with each other and to meld our individual strengths in science, history, engineering, education, communication, and business into a workable, cooperative whole. *Herbert T. Pratt, 23 Colesbery Dr., Newark, DE 19720-3201*

Ideas in Chemistry. A History of the Science. David Knight, Rutgers University Press, New Brunswick, NJ, 1992. vi + 213 pp. Paper (Typeset), \$18.00.

Don't judge a book by its cover or its title. The adrenalin-stimulating cover blurb says: "In this unconventional history of chemistry, David Knight takes the refreshing view that the science has 'its glorious future behind it.' Today chemistry is primarily a service science." Refreshing? A cold beer on a hot day is refreshing: this provocative view of contemporary chemistry is anything but refreshing to a chemist and chemistry teacher. We are eternally young and vigorous, and we refuse to listen to anyone who says we are middle-aged. We must put aside our defensive attitude and listen to David Knight, who has many interesting things to tell us about history and chemistry.

As for the title, Derek Davenport, in his review in *Chemical and Engineering News* (May 24, 1993, p. 32),

says that "it takes considerable chutzpah to title a book of 200 or so pages 'Ideas in Chemistry' and even more to subtitle it 'A History of the Science'." Knight discusses the aim of the book in the first chapter and contrasts this book with other histories:

All these (other) writings will nevertheless give us a history of chemical ideas, whereas this book is about ideas in chemistry, where they are realized not merely in scientific books and papers, but also in apparatus, in laboratories and institutions, and in dyes. This is not a blow by blow account of the progress of chemistry, which it would be foolish to attempt in one small volume, but rather an attempt to pick instructive episodes in a more or less chronological order to see what roles chemistry has played over its long history.

The difference between "chemical ideas" and "ideas in chemistry" is, unfortunately, extremely subtle; and a better title would be "A Biography of Chemistry", which

is in fact the title of the introductory chapter. History as biography is not novel; Spengler said in *The Decline of the West*, "For everything organic the notions of birth, death, youth, age, lifetime are fundamentals." But Knight has concluded that chemistry has aged to the point where the biography is pretty much complete, with little anticipated for its future. This point is certain to be the one that attracts the most interest (better, scorn), but there is a fundamental chronological problem in treating the history of chemistry as a biography. A biography of Washington, for example, might be divided according to the various roles he played, with chapters entitled, "The Surveyor," "The Farmer," "The Soldier," "The President," arranged in chronological order. Knight's biography of chemistry is divided into chapters named for particular roles that chemistry has played, as noted in the passage above, for example, "A Useful Science" or "A Deductive Science." Knight connects these roles to certain historical periods, and one might mistakenly conclude that the period when chemistry was a deductive science was different from the time when it was an experimental science. As Knight says in the first chapter, "Naturally at no time was chemistry simply inductive or deductive, and it has always been experimental." In the following list I have given Knight's choice of chapter titles followed by a short description in parentheses of the content and/or chemists that are discussed. As can be seen, some chapters really involve unique historical periods and others do not. Thus, for example, one could write as well about the experimental science in the 18th or 20th centuries as the early 19th, as Knight has done:

1. Introduction
2. An Occult Science (alchemy)
3. A Mechanical Science (Boyle; Newton)
4. An Independent Science (Priestley, Black, Lavoisier)
5. The Fundamental Science (vitalism, electricity and chemistry)
6. A Revolutionary or an Inductive Science? (Dalton, Davy)
7. The Experimental Science (Davy, Wollaston, Faraday, Berzelius)
8. A Useful Science (Davy, Liebig, Playfair, Perkin)
9. A Deductive Science (chemical structure: Dalton, Kekulé, van't Hoff)
10. A Descriptive, Classifying Science (Davy, Avogadro, Mendeleev, Rayleigh, Ramsay)
11. A Teachable Science (chemical education)

12. A Reduced Science (Ostwald, Crookes, the Braggs, Moseley)
13. A Service Science (late 20th century)

If we only criticize the title or the way that Knight characterizes historical periods, emphasizing some aspect of the period and ignoring others, then we overlook the worth of this stimulating and informative book. I believe that the way to appreciate the book is to read each chapter as a separate essay on an aspect of chemistry and to ignore chronology. Each chapter begins with a short discussion of the topic at hand—science education, or deduction in science, or the nature of a revolution in science—and then proceeds to illustrate the topic with examples from the history of chemistry. Now, more examples may be cited from other periods, but that does not vitiate the value of Knight's examples. The chapters (essays) are well written, informative and interesting. Along with scientific aspects of chemistry are discussions of religion, philosophy, sociology, political history, as well as other sciences. The changing nature of the scientific profession, the development of scientific societies, the publication of scientific journals, the availability of specialized apparatus are discussed along with the major theoretical chemical developments: atoms, structural formulas, periodic law, etc.

Chapters 2, 5 and 6 are particularly rewarding. I have noticed a peculiar quantitative effect when I try to understand alchemy. If I read too little I feel ignorant; too much and I feel lost. Knight seems to have a good sense of proportion and gives us insight into the "chemical philosophy," as well as the practical discoveries of the alchemists. His discussion of chemistry as the fundamental science (Chapter 5) involves a period in which topics such as heat and electricity, as well as some biological theories, were considered to be part of chemistry, and I found this essay to be especially enlightening. The discussion of the chemical revolution in Chapter 6, adapted from a conference paper of 1988, raises several interesting points about continuity and discontinuity in chemical ideas and suggests that there is much more to explore besides the end of phlogiston and the new nomenclature.

The essay on chemistry as a useful science does not fit well with the time period which Knight assigns to it. He begins with a discussion of pure and applied science, and how gentlemen in England avoided things connected with "trade;" but he overlooks the experience in other nations in earlier times (practical investigations by Glauber, Boerhaave, Lavoisier) and the flourishing of industrial chemistry in later times. Knight tells us more than I care to know about Davy's investigations

of leather tanning but omits the important story of LeBlanc's process for soda production and its effect on the developing textile industry in Britain. Indeed, this may be the first commercial synthesis of a natural product, and the entire topic of synthesis of useful materials is virtually absent from the book. The agricultural research of Liebig and others mentioned in Chapter 8 ultimately led to the work of Haber in the laboratory and Bosch in the factory and the industrial synthesis of ammonia, which is ignored. Similarly, the lack of a synthetic dye industry in Britain after Perkin is discussed briefly, but the triumphs of synthetic organic chemists in the laboratory and the factory, leading to dyes, medicines, and plastics are not recorded.

The last two chapters concern the present century and reflect Knight's opinion that we are on the descending side of chemistry's trajectory. In the early days of the century, chemistry lost its position of importance in science to physics, as the physicists' explanations of chemical phenomena were adopted. Thus chemistry is "a reduced science" to Knight, meaning not quite as fundamental as before. Modern biology is based on organic chemistry and physical chemistry. In many instances, research could be classified as biochemistry or molecular biology. (See the recent discussion by P. G. Abir-Am, "The Politics of Macromolecules: Molecular Biologists, Biochemists, and Rhetoric," *Osiris*, 1992, 7, 164-191, on the power struggles between these disciplines.) Following Knight, should biology be considered a "reduced science?" Or should chemistry and biology be called "enhanced sciences" because they have been strengthened by contributions from other disciplines?

Further, Knight believes that chemistry has become "a service science" because other scientists have to know some chemistry, but the other sciences (e.g., biology, astronomy) are producing brilliant new discoveries while chemistry has become "not a senile science but a middle-aged one perhaps." I believe that one could look at the same evidence and call chemistry, as the American Chemical Society sometimes does, "the central science," sounding important, vigorous, fundamental, challenging. Chemistry now encompasses a vastly wider area of investigation and application. A colleague in my chemistry department publishes his research results in physics journals, and another publishes in ecology journals; they both call themselves chemists.

The importance of the question, "Is chemistry a service science or the central science?" depends on your professional outlook. The American Chemical Society is not likely to publish literature which urges students to become service scientists, nor to urge Congress to

fund more service research. Every scientist must be on the cutting edge, pushing back frontiers, creating potential technological employment. A social scientist who can look at the question "objectively" should conclude, I think, that modern chemistry is a complex enterprise involving many people and interests, and that any short label must be an inadequate and misleading descriptor.

Knight recognizes that "any book is personal, and its structure may seem implausible or misleading," and invites us to do our own research and form our own history of chemistry. In his short epilogue he urges us to learn what historians have said recently, as well as to read the original scientific literature. He sees the history of science as a fresh and open field of study, comparable to early 19th century science, and much more exciting than his view of modern chemistry.

Knight's writing is lively and interesting, though occasional ultra-SAT words like "inosculated" and "rebarbative" appear. Indeed, the spelling is often "rebarbative," with numerous omitted ("eighteenth"), inserted ("Lavoilsier") and changed ("chanded") letters, as well as missing and added words. The notes are extensive and useful, the index short and idiosyncratic. For example, there are index entries for "Failure", "Fashion" and "Fraud", but it would be difficult to find the discussion about the discovery of the "noble gases" on page 139, because there are no entries for argon, helium, inert gas, nitrogen, noble gas, or Rayleigh.

I also noticed a few factual errors. The claim that coal gas was a valuable by-product of the making of coke (p.104) might be modified; in the U.S., coal gas and coal tar were burned as they formed, and water gas was used for lighting. G. N. Lewis's definition of an acid (p.169) is an electron-pair acceptor, not a proton donor.

Publishers invariably overstate the audience for a book, and the tradition is upheld here. The recommendation of the book to "a general reader" is unjustified because of the necessary scientific jargon ("Pauling's theory of resonance giving way to the study of molecular orbitals") which makes much of the book inaccessible; "a student" might do better if she or he knew some chemistry, but the book is not really a useful textbook for the history of chemistry. The readers who will derive the most from the book are the "scientist" and "historian of science," who will bring their own knowledge of chemistry and history to interact with Knight's novel presentation of the subject—a presentation which, as the cover blurb says, will "engage the attention of anyone interested in the interplay of science and ideas." *Martin R. Feldman, Department of Chemistry, Howard University, Washington, DC 20059.*

My 32 Semesters of Chemistry Studies. Vladimir Prelog, American Chemical Society, Washington, D.C., 1991. xxiv + 120 pp. Cloth (Typeset), \$ 24.95.

The intriguing autobiography by Vladimir Prelog serves as an excellent companion to Barton's *Some Recollections of Gap Jumping* and Havinga's *Enjoying Organic Chemistry, 1927-1987* in that all three of these highly creative scientists were repeatedly able to weave a thread of stereochemistry throughout their research efforts. Indeed, the recognition of novel stereochemical principles was a primary reason why both Prelog and Barton were awarded Nobel Prizes.

The title of Prelog's book conveys useful background information. As a committed scientist who was highly motivated to remain at the frontier of chemical truth, Prelog was in a serious quandary as to how he might deal with retirement. With the ending of his formal career on the faculty in 1976, the most workable ploy by which Prelog might remain an active member of the ETH community was to accept the position of "postdoctoral researcher." Thereby, Prelog may have succeeded in becoming the only postdoc in the history of science who had previously won a Nobel Prize!

Like many chemists of his generation, Prelog conducted an extensive series of studies involving natural products. As a testament to his courage, intelligence, and versatility, his research in this area, especially in the 1940's, was of enormous breadth encompassing steroids, indole alkaloids, and quinine. By a careful reanalysis of earlier publications and a small number of inspired experiments, he was able to show that the published structure of strychnine was in error and then to propose a more accurate, alternative structure. In his classical investigation of the macrolide narbomycin, he isolated a simpler degradation product that since has become known as the Prelog-Djerassi lactone, a molecule of such importance that it has been prepared synthetically numerous times since 1975. As Prelog notes with some sense of irony, obtention and identification of this relatively simple by-product probably led to more fame than any of his other research projects at ETH. Yet many of his natural product studies led to real insight into major compound classes. This is exemplified by his work on nonactin, with its marvelous stereochemical peculiarities, and by his extensive investigations of the iron-containing ferrioxamines.

Relatively early in his career, Prelog was intrigued by conformational questions involving medium- and large-ring alicycles. Under the maxim that "necessity is the mother of invention," he independently developed the utility of the acyloin condensation as an entry into the heretofore rare medium-ring systems. From this "purely" synthetic research emerged marvelous forays into transannular chemical phenomena, an interest in large-ring compounds that was maintained with his now classical macrolide antibiotic studies, and an ever-expanding desire to raise sophisticated questions involving apparently arcane stereochemical issues. Yet from the latter came a re-exploration of the foundations of asymmetric synthesis that ultimately led to a much deeper understanding of the nature of stereoselectivity in enzymatic processes. As Prelog considered even such "simple" issues as which face of a molecule is prone to attack by an asymmetric reagent, questions of specific nomenclature arose as a natural consequence. The asymmetric component of enzyme catalysis is slowly losing its mystery as an affectionate marriage occurs between formal mechanistic electron flow arrow-pushing descriptions and fundamental stereochemical concepts. How intriguing that the "secret of life" still might actually be held by the distinguishable shape of our left and right hands, a perspective of reverse anthropomorphism probably first grasped by Pasteur! From the coming together of several key individuals to work out the specifics of such questions, we now have in place the extremely important Cahn-Ingold-Prelog (CIP) specification that has had a major impact on communication between organic chemists. Furthermore, adoption of this system and consideration of its implications have inevitably forced considerably greater sophistication in stereochemical thinking among virtually all contemporary organic chemists and biochemists.

The natural development of Prelog's research interests has often led him into very unusual stereochemical areas. Noting how the modern explosion of supramolecular organic chemistry emerged from the seemingly esoteric work of Cram, Lehn, and others, Prelog, through his current emphasis on geometric enantiomerism, vespirenes, enantioselective ion-specific electrodes, and oligomeric crown ethers, may be giving us an intriguing glimpse into the future of this discipline. What appeals to the curiosity of a very experienced scientist such as Prelog could well constitute "bread and butter" chemical studies for the next generation.

One of the most stimulating contributions made by Prelog was his rational synthesis of adamantane, a compound whose highly symmetrical structure is so aesthetically satisfying. Relating a fascinating anecdote, Prelog describes one of those incredibly rare intellectual leaps where intuition outperforms intellect. The Czech chemist Landa had isolated a hydrocarbon with molecular weight 136 and the unpredictably high melting point of 266 C! While Landa was repeating an elemental analysis, he noticed that this compound readily sublimed to afford tetrahedral crystals. When Landa showed these to Prelog's coworker Lukes, the latter walked to the blackboard and, within a few seconds, wrote the structure of "tetracarba-hexamethylenetetramine." To have this insight (and to be right) has to qualify as a rare instance in science where magic crossed the fog of ignorance—all before any modern instrumentation might assist in solving the structure. With a touch of sadness, Prelog confides that, because of wartime problems with his synthetic publications, many libraries are missing the critical papers and that this has given rise to the myth that he was only able to prepare traces of adamantane whereas in fact he made multi-gram quantities. The sharing of such sagas is an essential part of this series. The heroic players from the "golden age" of organic chemistry will soon pass on. This is the last time not only for them to share these wonderful adventures but also to clarify the facts behind some of the most intriguing intellectual advances of this century.

Besides containing an articulate, carefully crafted discussion of his many intellectual accomplishments, Prelog's autobiography abounds in wise observations regarding the "craft" of organic research. He notes, how, after a particularly long struggle to elucidate the correct structure for the antibiotic rifamycin, his final answer was at variance with a photograph that appeared in the publication documenting rifamycin's first successful X-ray structure. His anguish at making such a serious "blunder" was short-lived when he soon discovered that the model used for the published photograph had accidentally come apart during transport to the photographer and had been incorrectly reconstructed. From this incident comes his advice that chemists should be willing to have faith in their own wet chemical results when they have carefully taken pains to achieve internal consistency and should not be so willing automatically to capitulate in the face of the various modern structure-elucidation techniques. A second, much more tragic anecdote concerns the treatment of a graduate student by an advisor who became convinced that the particular student had somehow "cheated" on an important experi-

ment. In spite of the student's vehement protestations of innocence, the advisor publically condemned that student out of science. Even though later evidence demonstrated that the student's observations were probably correct, he had been denied a career in science. How sad that anyone else in science might also be able to commit such an injustice under conditions in which he or she strongly believed that they were "right." With all the current attention being paid to scientific fraud, perhaps we should be somewhat more willing also to give the benefit of doubt in cases where it is not absolutely certain that an unethical misdeed has transpired!

In reading these American Chemical Society autobiographies, one cannot help noticing that something very special sets these chemists apart from others. While these individuals are obviously gifted with extraordinary intellect, they also appear to have two special additional characteristics: a remarkable flair for choosing critical (and solvable) problems in organic chemistry and a driving force of will that allowed each of them to make a real difference in the development of their discipline. Whereas most of us in the daily practice of our profession consider ourselves fortunate if we uncover even a few publishable nuggets of new chemistry, these individuals shook the very fabric of their field and folded it into an entirely new form. There is evidence in each of their careers for a personal style that is unique. It may have been that, because they were so acutely original, they stood out from all the rest and thereby were able to attract the elite of their generation who, in turn, even further expanded an adventure newly begun. The special spark of creativity exhibited by individuals such as Prelog often kindles unique excitement in those with the intelligence and curiosity to perceive its presence and who also might wish to share in its warmth and illumination.

A key feature of Prelog's book shared in common with Barton's autobiography is the stark realization of just how important one or two key individuals can be in shaping the chemistry of their era and their continent. Chemists like Robert Robinson and R.B. Woodward appear over and over as lightning rods for the development of the careers of their junior colleagues. For example, the intellectual and spiritual debt of Barton to Woodward is made crystal clear in Barton's autobiography. For Prelog, the comparable individual was Ruzcika. The affection and esteem Prelog feels for Ruzcika is at once both endearing and educational. In a similar fashion to Barton, Prelog makes a great effort to point out how much his mentor influenced his career development. The leadership and stimulation of Ruzicka ranged from

the most simple task of securing entry visas for Prelog and his wife to more intangible aspects such as encouraging Prelog always to work up to his potential. Prelog makes the interesting and not generally known point that Ruzicka was an important backer of Woodward early in his career when many of his American contemporaries had not yet recognized Woodward's genius. While it might currently be fashionable to denigrate the possibly patronizing, career-shaping aspects of the old-fashioned scientific establishment, it is just as questionable whether a competitive "community" of vicious scientists is preferable. Perhaps all of us can learn a lesson in the importance of true encouragement and collegiality among pro-

fessionals. In my opinion, Prelog, in a subtle but effective fashion, is trying to communicate to the reader that the common enemy in the progress of science is our ignorance and should not be each other; and, furthermore, that positive, helpful individuals (such as Ruzicka), by rising to the occasion, can have an enormously constructive impact on their colleagues and institutions. One of the truly outstanding aspects of this series of books is the inclusion of these little homilies. Perhaps a cynic might find such comments by Prelog and others trite and inappropriate; but they might also just as easily be perceived as real food for thought. In the "Golden Age," the giants had class. *John Belletire, Ricerca Inc., Box 1000, Painesville, OH 44077-1000.*

The Quiet Revolution: Hermann Kolbe and the Science of Organic Chemistry. Alan J. Rocke, University of California Press, Berkeley and Los Angeles; 1993, xiii + 501 pp. Cloth \$50.

Alan Rocke has masterfully combined in one volume the definitive study of the development of the structural theory of organic chemistry during the first six decades of the nineteenth century and the first full-length biography of Hermann Kolbe (1818-1884). Those who wish to obtain a more in-depth discussion as well as the latest scholarship concerning the development of structural theory will find Rocke's book a valuable companion to O. T. Benfey's classic *From Vital Force to Structural Formulas*, recently reprinted by the Chemical Heritage Foundation.

Herman Kolbe's many valuable contributions to the development of organic chemistry have been largely overlooked in this century. This is probably due to his self-destructive behavior in his latter years. Several of the newer introductory texts in organic chemistry reprint Kolbe's diatribe concerning van't Hoff's publication in 1874 of "The Arrangement of Atoms in Space." For most chemists today, knowledge of Kolbe extends to the reaction that bears his name for the synthesis of salicylic acid (1873), his electrochemical method of decarboxylation (1846), and his total synthesis from inorganic reagents of acetic acid (1844). However, Kolbe in his time was considered

to be one of the leading lights of German chemistry, being on the same plane as Liebig, Wöhler, Bunsen, Kekulé, and Hofmann. Among Kolbe's Ph.D. students were Griess, Clauss, Crum-Brown, Volhard, Graebe, Zaitsev, Menshutken, Markovnikov, Armstrong, E. von Meyer, Curtius, Schmidt, and Beckmann. Kolbe produced 156 solo papers and 20 co-authored publications; the students he supervised added another 287 over a period of 43 years. Students from Russia, Britain, and the United States came to Kolbe's laboratory in Marburg (1851-1865) and Leipzig (1865-1884) in large numbers and thus his influence extended well beyond the borders of Germany.

Kolbe's career spanned that most exciting period in the nineteenth century when the question of the structure of organic compounds was still in flux. Kolbe was the son of a Lutheran pastor and grew up in rustic simplicity in the villages of Elliehausen and Stockheim in the then Kingdom of Hannover in central Germany. He was very much influenced by his rural upbringing and his mentors Wöhler and Bunsen. Rocke summarizes the influence as follows:

Both chemists were enormously prolific, and moreover, extraordinarily skilled, inventive, and precise in laboratory operations...Wöhler and Bunsen were also alike in their brilliant teaching abilities, their predilection for experimental investigations, and their habitual avoidance of theory...But, significantly, both scientists left the discipline of organic

chemistry just when it began to explode theoretically in the early 1840s.

Kolbe never felt comfortable with the ideas of Dumas, Laurent, and Gerhardt and preferred to try to stem this rising tide of reform with inventive and polemics. However, by late 1855 Kolbe had realized that he was fighting a losing battle, especially after Wurtz's synthesis of both symmetrical and unsymmetrical hydrocarbons. These results showed that complete substitution of hydrogen by many elements was possible. Kolbe's most productive period now followed, as he used his own version of type theory. Rocke presents us with a detailed description of Kolbe's life and work at Marburg which was the most scientifically creative period in his life. Although poorly paid, his institute chronically underfunded and ill-equipped, he nevertheless managed to attract many students because of his brilliance.

In 1865 Kolbe was called to Leipzig, There he received an excellent salary while the best chemical institute in all of Germany was constructed for him. Just as he had accommodated himself to type theory, structural formulas were being almost universally accepted by German chemists. Rocke does a superb job of showing how Kekule developed his structural ideas based upon the reform of atomic weights and the concept of valence. Kekulé is portrayed as the diametrical opposite of Kolbé. Whereas Kolbe came from a humble background, Kekulé came from a prosperous family in Darmstadt, Hesse. Whereas Kolbe's education was very basic, Kekulé received a classical as well as scientific education at the Darmstadt Gymnasium. He is described by Rocke as "handsome, tall, strong, and athletic, an enthusiastic gymnast and dancer."

Kolbe could not and would not accept the idea of a carbon chain because structural theory excluded any electrical basis as the reason for bonding. Kolbe was an

early adherent of Berzelian dualism and never really felt comfortable with other views. From the pages of the *Journal für praktische Chemie*, of which Kolbe was editor from 1870, there appeared the most venomous ridicule of structural theory. Increasingly, Kolbe became isolated and alienated from the German chemical community, something which did not seem to bother Kolbe at all. Kolbe manifested the worst excesses of xenophobia, particularly anything French, and a virulent, almost pathological anti-Semitism. As Rocke states in his book:

Kolbe was nothing if not conservative in his theoretical preferences, and he began to view novel developments in chemistry as just another aspect of modernism. Somehow he began to associate structural formulas with sensualism and materialism, possibly even with irreligion. His whole life was devoted to the science of organic chemistry, and he saw that science almost in the personification of a pure virgin being seduced and destroyed by meretricious villains, by liberals, social democrats, traitors, atheists, Catholics, and Jews. In the 1850s and 1860s, he suffered periods of paranoia and severe depression, and after 1870 he appears to have had delusions of grandeur.

This is the Kolbe that most remember, rather than the man of many major scientific achievements. Rocke presents a balanced view of Kolbe with a level of scholarship, thoroughness, and documentation (83 pages of notes) that will please both the chemist and historian alike.

The Quiet Revolution will serve as an invaluable reference work on the development of organic chemistry in the nineteenth century and belongs on the chemist's bookshelf and in the library of every college and university with a chemistry program. *Martin D. Saltzman, Providence College, Providence, RI 02918.*

Issue 15/16, page 10: Credit for the portrait of Benjamin Silliman, Jr. to the Smithsonian.

Erratum

Issue 15/16, page 38: The structure of benzoic acid, the product in the last equation, should be:

