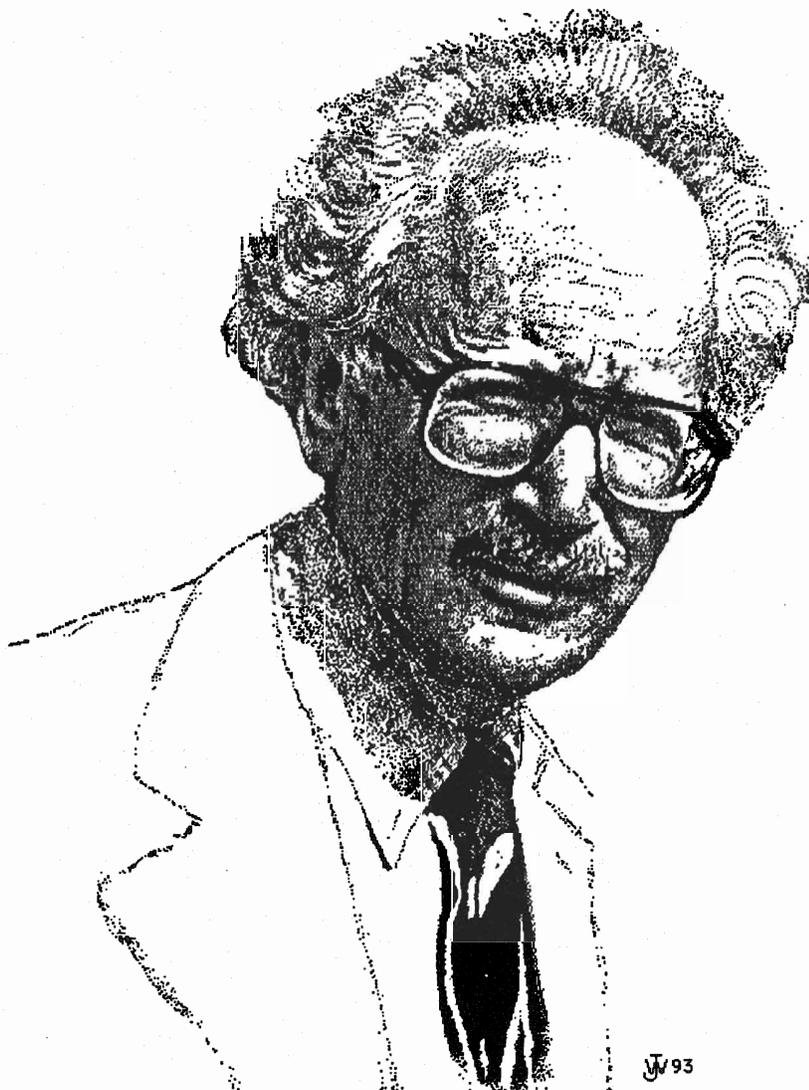


BULLETIN FOR THE HISTORY OF CHEMISTRY

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A Festschrift in Honor of Otto Theodor Benfey

**BULLETIN FOR THE HISTORY
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The Cover...

... shows a sketch of Otto Theodor Benfey by William B. Jensen. This special double-issue of the *Bulletin* is dedicated to Dr. Benfey as a Festschrift in honor of his retirement from Guilford College in 1989.

All illustrations, unless otherwise indicated, are from the Oesper Collection in the History of Chemistry of the University of Cincinnati.

CONTENTS

Festschrift Introduction	3
The Eclectic Life of Otto Theodor Benfey <i>by James J. Bohning</i> A biographical portrait of the life and work of Ted Benfey	4
By Way of Explanation <i>by Otto Theodor Benfey</i> On eclecticism and the pursuit of chemistry	16
Corpuscular Alchemy <i>by William Newman</i> A case study of the mechanization of 17th-century alchemy as exemplified by the transmutational theory of Eirenaeus Philalethes	19
Robert Mayer and the Conservation of Matter <i>by Kenneth L. Caneva</i> Did conservation of energy precede conservation of mass?	27
Pride and Prejudice in Chemistry <i>by Alan J. Roche</i> A look at chauvinism and the pursuit of science among 19th-century European chemists	29
The Chemists' War <i>by David J. Rhees</i> An analysis of the impact of World War I on the American chemical profession	40
The Historical Development of the van Arkel Bond-Type Triangle <i>by William B. Jensen</i> Even textbook diagrams have a history	47
Perspectives Lecture <i>by Otto Theodor Benfey</i> On Pythagoreanism and the origins of the periodic table	60
Book Notes	66
Letters	72
Divisional News	
Awards	73
Events of Interest	75
Future Meetings	76
Officers Directory	76

FESTSCHRIFT INTRODUCTION

The following Festschrift is based on a two-day symposium held in honor of Dr. Otto Theodor Benfey at the 41st Southeastern Regional ACS Meeting in Winston-Salem, North Carolina, on 11-12 October 1989. Organized by Dr. Lawrence Gains of the Lorillard Research Center in Greensboro, NC and Dr. David MacInnes of the Department of Chemistry, Guilford College, Greensboro, NC to commemorate Dr. Benfey's retirement from Guilford, both the title of the original symposium - *The Context of Chemistry: Conceptual, Historical and Social* - and the list of participants mirrored the rich diversity of Dr. Benfey's career and the breadth of his personal and professional interests.

Dr. Benfey's work as a chemical educator was reflected in the presentations by Bassam Shkhashiri of the University of Wisconsin ("Science Literacy in the 1990s"), the late Frank Halliwell of the University of East Anglia ("The Educational Impact of the Limiting Characteristics of Science"), Laurence E. Strong of Earlham College ("Levels of Explanation in the Teaching of Chemistry"), and Edward Arnett of Duke University ("Chemistry for Non-Scientists at Duke following the 1988 Westheimer Report").

His interests in stereochemistry and the conceptual foundations of organic chemistry were reflected in the presentations by Eugene Garfield of the Institute for Scientific Information ("Prospective Uses of Citation Data for Identifying New Research Specialties in Chemistry"), Stephen Weininger of Worcester Polytechnic Institute ("Representation and Reality: Chemistry Considered as Language"), William Jensen of the University of Cincinnati ("Mers, Morphs and Tropes: Unraveling the Tangled Vocabulary of Chemical Composition and Structure"), Ernest Eliel of the University of North Carolina at Chapel Hill ("Conformational Analysis, Past, Present and Future: A Personal Account"), and Jeffrey Seeman of the Philip Morris Research Center ("The Context of Organic Chemistry: Conceptual, Historical, Social").

His historical interests were reflected in the presentations of David Rhees of the Bakken Library and Museum ("The Chemists' War and the American Chemical Profession"), William Newman of Harvard University ("Eirenaeus Philalthes and George Starkey"), Kenneth Caneva of the University of North Carolina at Greensboro ("Robert Mayer and the Conservation of Matter"), Alan Rocke of Case Western Reserve University ("Pride and Prejudice in Chemistry: Kolbe, Hofmann, and German Antisemitism"), Derek Davenport of Purdue University ("On the Comparative Unimportance of the Invective Effect in Physical Organic Chemistry"), and O. Bertrand Ramsay of Eastern Michigan University ("The Use of Molecular Models by Chemists: Some Solved and Unsolved Problems in the Historical Development of Stereochemistry").

And last, but not least, Charles Price's address on "The Environment and War: Are There Technological Fixes?" re-



Dr. Otto Theodor Benfey

flected Dr. Benfey's continuing concern with the social responsibilities of scientists and with the social and cultural impact of science and technology in general.

Even this variety failed to do justice to the complete range of Dr. Benfey's interests (notably absent were presentations dealing with both Oriental science and with the Platonic solids), though it did unhappily preclude any reasonable hope of finding a single journal willing to open its pages to such a wide spectrum of topics. In the end, it was felt that the *Bulletin* could at least provide a home for those papers dealing specifically with the history of chemistry, and on this basis Dr. Gains and Dr. MacInnes undertook to collect manuscripts from the appropriate authors. Two of the original historical papers (Davenport and Ramsay) had been previously published and have not been reprinted in this collection. Likewise, the original paper by Dr. Rocke was already committed to publication elsewhere. However, he has substituted a new paper on a closely related topic ("Chauvinism and the Pursuit of Science") written especially for this Festschrift. Lastly, I have substituted a new paper, reflecting more recent work, in place of my original presentation, which will probably be published in the *Bulletin* at a later date.

In addition to the historical papers derived from the original 1989 symposium, Dr. James J. Bohning of the Chemical Heritage Foundation has provided a biographical sketch of Dr. Benfey ("From Stereochemistry to Social Responsibility") based on several hours of oral history interviews conducted by the Foundation, and Dr. Benfey has kindly consented to allow us to publish both his introductory comments at the symposium dinner ("By Way of Explanation") and a full-length Perspectives Lecture ("Precursors and Cocusors of the Mendeleev Table") originally prepared in honor of another well-known chemical educator, Dr. Ronald J. Gillespie of McMaster University. *William B. Jensen, University of Cincinnati*

FROM STEREOCHEMISTRY TO SOCIAL RESPONSIBILITY

The Eclectic Life of Otto Theodor Benfey

James J. Bohning, *Chemical Heritage Foundation*

It was an "idyllic" period for a young boy born on 31 October 1925 into a Berlin family of culture and accomplishment. In the summer there were long hikes to the snow-covered areas of the Austrian or Swiss Alps, followed by a refreshing drink of naturally cool apple cider and perhaps a snowball fight with his friends. During the school year in Berlin, he was very happy with his elementary school teacher, who stayed with his class during the first four years of schooling. He even was caught up in the excitement accompanying the massive parades of Adolf Hitler.

But the innocence of youth and the protection of his parents could not shield Otto Theodor Benfey from being frightened when he watched some boys jeering another boy their age as they squeezed him into a wire trash basket and rolled him down a hill. When his family decided to send him to live with friends in England in 1936, he experienced a similar taunting from his Berlin peers which included statements about his Jewish background. Yet for the ten-year-old Benfey, the first two years in "exile" were still idyllic. He was staying in a "lovely place" with the Mendls, a family he had known in Berlin. He was attending the Watford Grammar School with their son, Wolfgang, who was Benfey's age. And for the first two summers, he went back to Berlin on transatlantic luxury liners, once seeing Max Schmeling returning after his defeat by Joe Louis. What he did not know was that his mother was hiding jewelry in his suitcases for safe-keeping with the Mendls when he returned to England.

As the political situation in Germany deteriorated, Benfey's family made plans to leave the country. His father, Eduard, had been the Chief Justice of the Supreme Court of Economic Arbitration during the Weimar Republic and had earned a doctor of law degree at Göttingen. At first he believed that Hitler "was a passing phenomenon who wouldn't be a serious problem," but after the *Kristallnacht* in November 1938, he also sent Benfey's younger brother and older sister to the Mendls in England. Benfey's mother, Lotte, refused to leave without her husband, but did apply for an American visa because her sister and brother-in-law, Anni and Josef Albers, who had been on the faculty at the Bauhaus, were then teaching at Black Mountain College in North Carolina and could vouch for her. Fortunately, the American consulate later put Eduard on his wife's application number rather than assigning him to the end of the list. Otherwise, Benfey is convinced, his father would never have gotten out.

Benfey's siblings stayed only a few months in England, and then joined their parents as the family immigrated to the United



As a Boy Scout in England, circa 1937

States. When the Mendls suggested that young Theodor should stay in England until the family was settled in their new home, he didn't object. "I was doing well at school and I was very happy there," Benfey recalls. Little did he realize that while his family would be starting out as typical immigrants in an "awful hole in New York and slowly working their way out of it," he would seven years later be "sailing in with a Ph.D. and a traveling fellowship to Columbia."

In spite of his Jewish family background, Benfey was baptized as an infant in the Lutheran church because his parents had converted during a time when many of the Jews were trying to forget their Jewish background and become assimilated into the German culture. When the bombing started in London, it became impossible for Ted and the Mendls to travel from Watford in the northwest suburbs to the only Lutheran church in the center of the city. Both Ted and Wolf Mendl were subsequently confirmed in the Church of England, but its "nationalistic attitude" and prayers for "destroying the Huns" disturbed Wolf's mother, who "had great sympathy for the German people and felt the suffering of the Germans under the

Nazis." She and Wolf found the Quakers a satisfactory religious community, while Benfey "went through the whole gamut of nonconformist churches before joining the Friends." It was a decision that would have far-reaching consequences for the rest of his life.

Even though his mother and father spoke English, Benfey had "minimal English" when he was first placed in the Watford Grammar School. But he quickly learned the language, and was urged to go into language teaching because of his German background. Considering that an insult because it was not something he had mastered but something he "just knew," Benfey found math and science more appealing, "in part because it required less facility in the language." Since the English education system specializes rather early, Benfey selected a science track that was university directed. He contemplated specializing in math, but wanted to bring it "down to earth, to do something useful and practical with it." In the British system science subjects were not concentrated

in a single year but administered in smaller doses. From age eleven on, he was exposed to some chemistry every year, with the other subjects going "forward in a similar manner." His chemistry teacher, "Inky" [R.W.] Knight, was not very exciting, but sufficiently "good and solid" to maintain interest through a prosaic exposure to substances and their properties.

It wasn't until he reached the sixth form (pre-university year) and was introduced to organic chemistry that he really became excited. Benfey found stereochemistry particularly inspiring because he had "always loved geometry" and had already worked through many of Euclid's proofs. It was then that Benfey decided to major in chemistry, and the geometric

aspects of chemistry have been important to him ever since.

With Knight's assistance, Benfey applied to universities, and was subsequently notified that University College London (UCL) had a place for him. While Eduard Benfey was still in Germany, he had been able to give some of his money to a Jewish fund that could still ship out money. Some of it went to Woburn House, a London organization that dispensed money for refugees who were separated from their parents or were otherwise in need. The crystallographer Rosalind Franklin did volunteer work there, and Benfey speculates that she may have processed some of this "Wundergeld" that helped with expenses as he started his studies in 1942, living in a

cooperative student hostel in the seaside resort of Aberystwyth in Wales, where most of the UCL chemistry department had been relocated. The only time the war got close to him there was during a physics lecture, "when the professor was talking about something having an impact, and at that moment a mine exploded at the end of the pier!"

Physically, the 30 stu-

dents from London were part of the University of Wales, and Benfey found the Welsh singing ability to be very "memorable." He was also impressed with the scholarships given to Welsh students who were willing to teach in the Welsh school system for four years, a concept that is slowly being adopted by some states in the U.S. some 50 years later. Living in the hostel brought contact with a diversity of other students, and a close friendship developed with Stephen Awokoya, who would later become a leader in science education in his native country of Nigeria and then head of science education for UNESCO in Paris.

The UCL students and faculty were partially integrated into



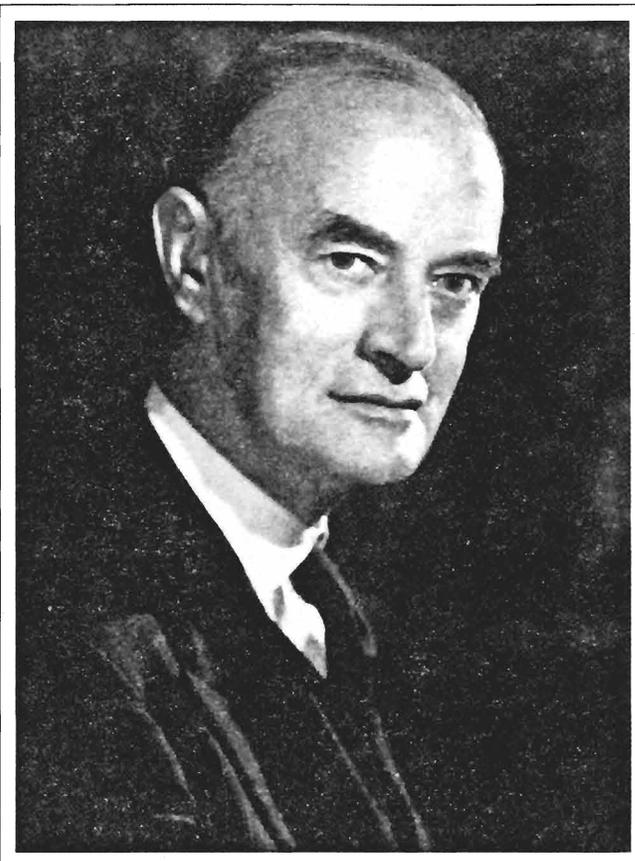
Benfey (left front) and fellow members of "The Lodge" hostel during his student days in Aberystwyth, 1944

the Welsh program, and the rigorous schedule called for an hour of lecture at 9:00 a.m. and 4:00 p.m., with labs in between, five days a week. Classes were not "quantized;" instead, one professor would lecture "until his subject ran out," and then another would begin a different topic. In addition to the traditional topics, some covered prosaically and some superbly, there was even Samuel Sugden talking about his parachor (a measure of surface tension used to deduce structure). Benfey found the organic lab, supervised by C. A. Bunton, to be most rewarding. He admits to a "tremendous esthetic delight in preparing colored, beautiful crystals," an enjoyment that in subsequent years he would pass on to his own students. Only a few years ago did he discover that the author of the lab text, Julius B. Cohen, was his father's first cousin (1).

During the two years Benfey spent in Wales, Christopher Ingold was a "benign presence." There were rumors that he spent three hours preparing each lecture. He lectured only on those areas he had worked on himself, except for "one strange series of six or eight lectures called "history of chemistry," which Benfey found "deadly dull" and which he ironically confesses turned him off "almost permanently from ever conceiving of teaching a course concentrating only on chemical history." These lectures, Benfey vaguely recalls, covered early ideas on stoichiometry, an area which had been impressively surveyed by Ida Freund in 1904 (2). Yet within the last few years Benfey has recognized that his interest in presenting topics historically, the sense "that you can't understand anything in the sciences unless you see it from its intellectual and historical context," stems directly from Ingold's organic lectures where topics such as aromatic substitution and the nitronium ion were developed in an historical context, as can be seen in Ingold's classic text (3).

In 1944 Benfey experienced the final examination endurance test, maneuvering through a marathon of six consecutive three-hour exams in a three-day period, followed by six-hour lab tests on qualitative and quantitative analysis, organic preparations and qualitative organic analysis. His physics minor was tested immediately afterward. Even though it was a "frightening" experience, he received the B.Sc. degree with first class honors. Sensing that after the Normandy invasion on 6 June the war might end shortly, the UCL contingent returned to London and Benfey was asked to continue with Ingold for the Ph.D. By that time Benfey was attending Quaker meetings regularly, and considered himself a pacifist. If he didn't stay at UCL, he would be subject to the draft and would have to file a conscientious objector request as a Quaker.

Acknowledging that for most of his life he didn't wonder where he would be going, but that it "just happened," Benfey agreed to remain, but informed Ingold that he could not work on a problem that was related to weapons and destruction, such as aromatic nitration, which had direct relevance to TNT. Ingold put him to work on aliphatic substitution and evidence for the carbonium ion (now called the carbocation). Benfey



Christopher K. Ingold

proceeded, "blissfully confident" that his topic could have no destructive consequences. Only later did he realize that mustard gas action involves the sulfonium ion, a carbonium ion analog. But nitrogen mustards also have medical applications, forcing Benfey to realize that "the purer the science, the more it is capable of constructive and destructive applications."

Although two wings of the main structure at University College had been destroyed by bombs, the chemistry building remained intact. The graduate students were lined up in a large basement laboratory, and Ron Gillespie, now at McMaster University in Canada, worked on the opposite side of Benfey's bench. Gillespie was preparing nitronium perchlorate, and had triplex glass panels between himself and his experiments for protection in case of an explosion. But there was only a wooden partition separating those same experiments from Benfey's bench, and it "wasn't even hardwood!"

With Patton's armies caught in the Ardennes Forest, the war continued through the winter, and the V-2's started striking London. Living back at Watford and commuting daily, Benfey remembers the difference between the V-1 used earlier in the war and the newer V-2. The V-1 was a pilotless drone, and when the sound stopped, "you knew it was about to descend and you ran for cover." The V-2 was a rocket that moved faster

than sound, giving no advance warning. "If you heard the explosion, you were safe, because it had landed elsewhere." Fortunately, they never got closer than about a mile from UCL.

Ingold had been using the effects of salts on the rates of alkyl halide hydrolysis reactions for evidence of the carbocation and Louis Hammett had proposed that there could be specific solvent effects of the salts affecting the dielectric constant of the medium. Benfey's assignment was to test the salt effects and demonstrate that they were only explainable by the carbocation mechanism. The kinetics became very complex, but he was able to show that two salts could have inverse effects depending on the leaving group of the alkyl halide, exactly the opposite of what Hammett had predicted (4). He is particularly proud of learning to determine chloride ion in the presence of bromide ion electrochemically, because he did that through his own literature search. At the time, Benfey didn't realize that his Welsh connection was following him experimentally, for it was K. J. P. Orton in Wales who had taught kinetics to Edward Hughes, who in turn "brought the kinetic emphasis to Ingold."

It was expected that the Ph.D. would be completed in two years, and Benfey remembers being very discouraged during his first year because he couldn't get repeatable data. He saw Hughes and Ingold daily, and was particularly encouraged when Ingold told him, "You'll do one-eighth of your work during the first year, and seven-eighths the second." The dichotomy of Ingold's personality is reflected in Benfey's description that in "personal relationships he was the most warm and gentlemanly individual; when you put a pen in his hand he could be absolutely vicious." Consequently, Americans were genuinely surprised by his charm when they first met him in this country.

Hammett "graciously" accepted Benfey's results, indicating that he wasn't fighting the carbocation but just offering an alternative explanation that needed to be explored. Based on Benfey's written research proposal, a London University postdoctoral traveling fellowship was awarded, paying for all his expenses to work with Hammett at Columbia for one year. It was assumed that Benfey would return to a suitable teaching position in England, and he indicates that not only was he "totally convinced that he was coming back," but that he had been "brainwashed" into believing that "no persons in their right mind would immigrate to America unless they had to as refugees. Culture was in Europe, and Americans were too materialistic."

When he arrived in New York at the age of 21, Benfey experienced "one of the most embarrassing moments" in his life. As the first postwar Ingold student to arrive in the States, he was visited by C. Gardner Swain, E. S. [Ted] Lewis, and George S. Hammond, who quizzed him on the real meaning of several of Ingold's papers. Although he had read Hammett's text (5), Benfey admits that he had never heard of them or their work, and was unable to discuss results in the wider context of physical organic chemistry. He found it "shocking" that UCL

did not encourage students to read the literature, and felt that when he did so it was something "that was not quite approved of," as if he were checking on his teachers. "Even though we were given a historical background in Ingold's lectures," Benfey concedes, "it was not to see that science keeps on changing. It was to show how science was being completed by UCL. We were taught chemistry as if it were the word of God - the answer."

Except for once-a-week conferences with Hammett, Benfey was all alone and had one year to produce results. His "strange" problem was on mercury-catalyzed solvolysis and olefin formation, which he claims he "sort of solved," but "never felt very confident" that he did "something significant." Hammett's name does not appear on the publication, which Benfey attributes to Hammett's desire to give him "stature" by indicating that Benfey was the only contributor (6). Benfey was impressed with the vigor and scientific turmoil at Columbia in 1946 and 1947, but in spite of the superb faculty talent there was little coherence between the professorial research groups. He admits that he was living "in his own little world" where his mental intensity was focused on his research problem and did not allow for much exploration and interaction.

Benfey's interest in the history and philosophy of science can be traced to his graduate school years at UCL. Although he was head of the Student Christian Movement at UCL, many of his peers were quite left-wing oriented, and some of the graduate students and faculty were members of the Communist party. Benfey's religious convictions led him to disagree with their philosophy that science was just the tool of capitalism and that after the war communism would take over, planning and organizing science to serve the people. Realizing that as "an amateur" it would be futile to sustain an argument with the communist proponents because they were so well informed and could "demolish you instantly," Benfey embarked on a reading program that began with Arthur Eddington and James Jeans. Eddington, who was a Quaker, made Benfey feel "it was safe to be influenced by him." During a confining illness, which he suspected was caused by a fellow student working with cyanide, Benfey's religious reading ranged from Albert Schweitzer's autobiography to the French Catholic writers and the English mystics (7). These religious interests continued at Columbia, where he attended 15-minute worship services at Union Theological Seminary in his morning transition between International House, where he was staying, and his own work at Columbia. Here he heard such luminaries as Reinhold Niebuhr, Paul Tillich and Arnold Toynbee.

Benfey's deep concern for the human condition is reflected in his reaction to the 1945 bombing of Hiroshima. Already sensitized by the increasing lack of feeling about killing humans as the Allies resorted to blanket bombing of German cities rather than pinpointing targets, he "walked in a daze through the streets, wondering whether humans had gone totally mad." He "seriously questioned what place there was

for someone with any kind of sensitivity and concern for doing constructive work to stay in science, if this is what can happen to scientific knowledge." It also forced him to be more aware of the "left-wing claims that this kind of activity had to be controlled." Influenced by Albert Schweitzer's example, he "toyed with the idea of switching to medicine" and even attended some biology lectures at Columbia, where he gave the idea more serious thought. He finally talked himself into staying in chemistry, working "as far as possible towards directing it in humanitarian directions." To justify that position, he rationalized that he could always get out later if his goal was unattainable.

After arriving in New York, Benfey had quickly changed his opinion about America, marveling at its technical proficiency and overwhelmed by its beauty. As a Quaker, he had heard of Haverford College and Rufus Jones, a leading theological figure who taught there. He had thought that if he were ever "stuck in America," Haverford would be the place where he wanted to teach chemistry. Nevertheless, he was amazed when "out of the blue" came a letter from the Haverford chemistry department saying, "Dr. Henry Cadbury of Harvard University Divinity School informs us that you might be interested in a teaching position." (Unknown to Benfey, his sister was in contact with Cadbury, a former Haverford professor, through the American Friends Service Committee in Cambridge, and told him about her brother.) Coming to the conclusion that he really wanted to stay in America, Benfey felt it was only fair to the British who were supporting him that he finish the calendar year at Columbia, and arranged to start at Haverford in January of 1948.

At the age of 22, Benfey became the fourth member of the Haverford chemistry department, which was graduating four to eight chemistry majors a year from a student body of about 500. William Buell Meldrum, the department head, had written a basic text in which the early chapters were organized historically and philosophically (8). Because of that and the Ingold influence, Benfey began his teaching career using that same historical emphasis, believing that "was the way to teach." Only later did he discover that it was very unusual. He was also prepared to introduce physical organic chemistry and reaction mechanisms into the first year organic course. But Meldrum told Benfey that he had contacted such stalwarts as Roger Adams and Henry Gilman, who agreed that approach was not acceptable. Thus Benfey "taught analytical and physical and all sorts of things," but never the first year organic course. He was allowed to present his "new" ideas as theory in a later course.

Feeling that it was "obvious" to have students involved in research, Benfey started with a "kind of gap-filling" project on molecular addition compounds between dinitrobenzoates and α -naphthylamine that reflected his own "enjoyment of qualitative organic analysis." The collected results from several summers of work supported by the Research Corporation re-

sulted in two papers (9). He also found that he could grow whiskers of the addition compound from vapor deposition of the amine, though this was never formally published (10). But it was not laboratory research that would become Benfey's forte. His brief career at Haverford was the launching pad for activities in chemical education and the history of science that remain life-long commitments, and have earned him much professional and personal respect.

In 1949 Benfey attended the founding meeting at Haverford of the Society for Social Responsibility in Science (SSRS), the brainchild of Victor Paschkis, a Viennese immigrant and engineer at Columbia. Most of the group were Quakers or like-minded people who were "energized by the Hiroshima bomb" and who:

... felt the need for scientists to see if there would be some way of influencing society to move, as far as possible, to the prevention of science being used for greater and greater destructiveness. They also wanted to see if they could help with problems where science could be used for good purposes such as in third-world countries.

The idea spread rapidly, with similar groups forming in other countries and among other disciplines. Benfey "threw himself into that organization," and became its second president in 1951.

Also in 1949, Benfey attended James B. Conant's renowned summer school on case histories in experimental science. Here he heard Leonard Nash and Thomas Kuhn among others, and the "excitement of getting at the original writings of such chemical pioneers as Boyle and Dalton, seeing their struggles within the total context of their work," gave him "the real impetus to explore the history of chemistry." His fascination with Prout's hypothesis, "because it involved a numbers pattern," led him to a more detailed study and ultimately a publication (11). But the Prout paper was another learning experience, for Benfey confesses that he knew "nothing about literature searches" and thought he was "terribly original." After being told his draft contained little that was new, he "slowly learned what was new and original, or at least what was somewhat novel." He acknowledges that it was Claude Deischer, Curator of the Edgar Fahs Smith Collection of the University of Pennsylvania, who helped him successfully rework the manuscript.

The developing intensity of Benfey's interest in the history of science is reflected in his early Haverford days. He spent the summer of 1950 "just reading the classic works in history and philosophy of science," and embarked on what would become an almost annual offering of a course on some aspect of the history and philosophy of science. Knowing that he could be demanding with the Haverford students, Benfey began by taking his first course through Kant's *Critique of Pure Reason* (12). Although "glad" he "plowed through it," Benfey concedes that he was uncertain how much of it he understood and

never dared to repeat this *tour de force* after Haverford.

When Yale's Henry Margenau came to Haverford as a visiting lecturer in physics and the philosophy of science, he started Benfey on yet another project. Yale was publishing a series of Ernst Cassirer's works in English and had already rejected the efforts of two translators for *Determinismus und Indeterminismus in der modernen Physik* (13). Margenau decided that Benfey was the person "to make something of it," and the result was Benfey's first book publication. Finding that he "enjoyed working over other people's translations and turning them into something useful," Benfey has continued this activity "off and on;" his most recent translation is of Fred Aftalion's *History of the International Chemical Industry* (14).

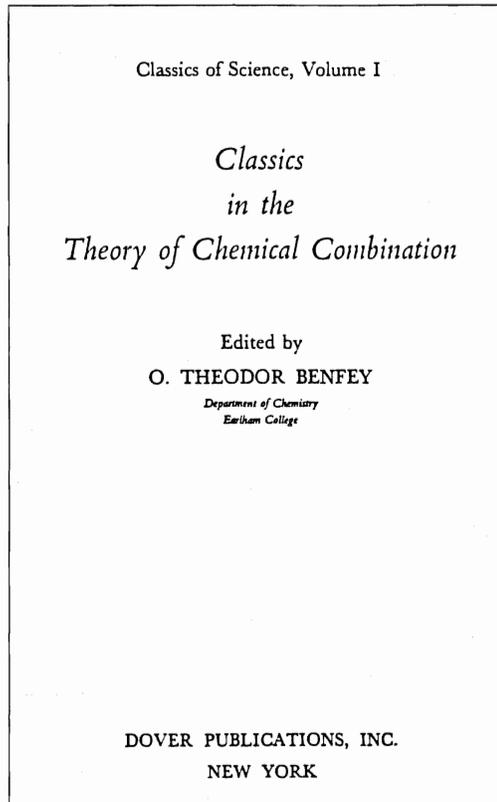
As the time approached for Benfey to take his first sabbatical leave, changing conditions in the chemistry department gave him cause for concern. Meldrum was retiring as chairman, and his successor was suspicious of Benfey, raised questions about his overall competence, and disagreed with his aims. Benfey credits his wife Rachel, whom he met while she was teaching at the Haverford Friends School and whom he married in 1949, with giving him the courage to leave Haverford, convincing him that he shouldn't work under someone who didn't want him (15). Although he had announced his resignation "with a great flourish to all the faculty," Benfey was granted tenure and his sabbatical half-salary by Haverford president Gilbert White, who said he deserved it "based on previous work." A financial gift from retired Haverford professor Albert Wilson provided additional help for his sabbatical year beginning in September of 1955.

For his sabbatical, Benfey chose Cambridge to be near his mother and grandfather, near Harvard University because of its prestige, and near Frank Westheimer because he was a physical organic chemist:

I had known about Westheimer because one chemical problem that I got fascinated with at University College was the racemization of optically active biphenyls whose optical activity was due to steric hindrance. I tried to figure out, from Ingold's teachings, what factors might have led to the observed order of racemization rates - size of substituent groups, inductive, resonance effects. That was the time I really did some literature searching and developed some ideas. I showed them to Ingold, and he rather liked them. Nothing came of that, but then I discovered that Westheimer had done calculations on the biphenyl racemizations. That intrigued me about him. I also had discovered he had worked on the nitration mechanism. So he seemed the right person.

With Westheimer, Benfey went back into research of a significant kind, working on a problem involving bipyridyl which was closely related to his earlier fascination with the biphenyl problem (16):

Westheimer was really delighted with this work, because it completed



something that he had published a number of papers on. It separates out the hydrogen bonding of an added proton from steric repulsion, and the resonance tendency to planarity as against nonplanarity.

Benfey also planned to work with Leonard K. Nash, trying to turn the development of organic structural theory into a case history for Conant's series:

I never went to Conant to talk to him about it because by then he was [High Commissioner to Germany]. I never saw him that year. I just went ahead and drafted the whole thing. I sent it to Harvard University Press, and they showed it to Conant. It turned out that Conant had attempted to do the same thing, and decided it couldn't be done, so he wasn't about to admit that I had done it. Luckily, that was the era when all the publishers were looking for supplementary texts to enrich the freshman curriculum. My book [*From Vital Force to Structural Formulas*] became number one in the series published by Houghton Mifflin, because the manuscript was all there, and it was exactly the kind of thing they wanted, quoting from the original papers and then commenting on them (17). That was exciting.

At Harvard, Gerald Holton was planning an English language equivalent of Wilhelm Ostwald's *Klassiker der exakten Wissenschaften* (18). He asked Benfey to prepare *Classics in the Theory of Chemical Combination*, which appeared as the first volume of the "Classics of Science" series published by Dover in 1963 (19). It contained complete papers by Couper,

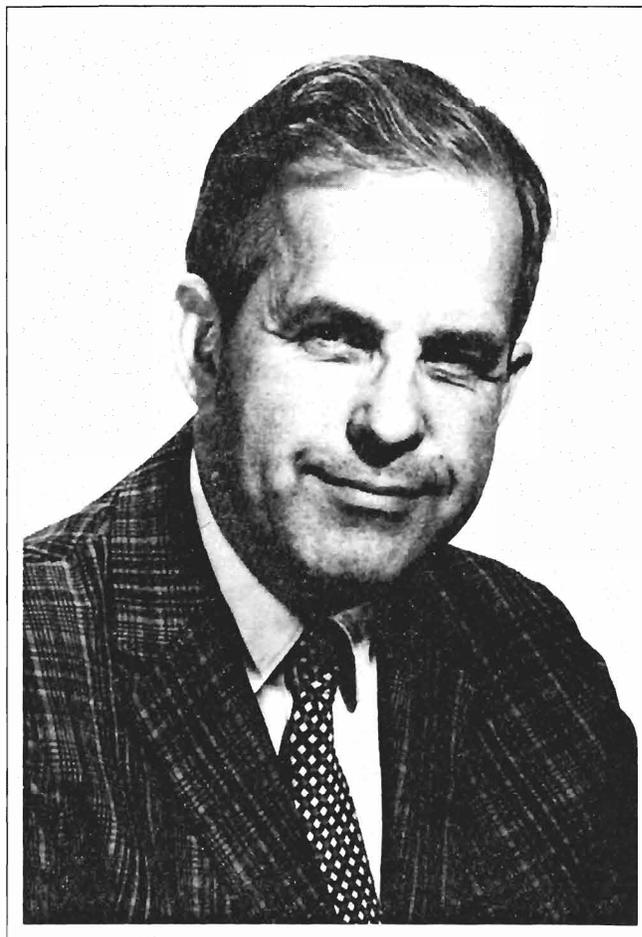
Kekulé, Laurent, van't Hoff, Le Bel, and others. The 1858 paper by Kekulé on structure theory had not been previously translated in full.

When Benfey left for Harvard in 1955, he faced an uncertain future, having burned the bridge to Haverford without having a new one to cross. He was scared and nervous, especially since his third child was just seven months old. He had interviews with Boston University and with a representative from Claremont Men's College, but it wasn't until an unexpected letter arrived from Larry Strong at Earlham College in the fall of 1955 that his future would again be secure. At first, Benfey was skeptical of Strong's invitation to join the chemistry department, remembering that people at Haverford told him: "Whatever you do, don't go to Earlham." Haverford had "always felt it was way above Earlham, and in earlier years they had taken people with Earlham degrees and given them an extra year to prepare them for graduate school."

But by 1955 people were moving in the opposite direction. Wayne C. Booth, whose book *The Rhetoric of Fiction* "inaugurated a whole new direction in literary criticism," had already been lured from Haverford by Earlham (20). By the spring of 1956 Benfey had agreed to join the westward migration to Richmond, Indiana, a small town that he describes as then being "reactionary in politics" with "violently anti-United Nations and anti-British" attitudes. The Earlham faculty "were a very educated, intelligent group in a community that had very little awareness of them." In one period of high town-and-gown tension, the Earlham president had paid the faculty in dollar coins so that when they spent the money in town "the town people would realize just how important their local college was to the community."

When Benfey arrived at Earlham, the college stood on the brink of rapid change that would see it transformed into one of the leading liberal arts colleges in the country, due in no small part to the efforts of a new president, Landrum Bolling, a political scientist with "tremendous energy and vitality." He found the Earlham students "a delight because for many of them the intellectual atmosphere of the college was very exciting" and "markedly different from their own environment." While Haverford was a men's college at the time, with at least half of the students the sons of professionals, Earlham was coed; many of the students came from rural backgrounds or small towns. "One could see them blossoming and thriving under the stimulus of the new ideas. There were fields that were opened before them that they didn't know existed as possible for their careers." At Earlham, Benfey continued his Harvard experimental work with James W. Mills, now a professor at Fort Lewis College in Colorado. They looked at the next, more complicated case where two pyridyls are tied together to try and keep them flat. The results appeared in his last experimental paper, for by then Benfey was totally committed to chemical education and the history of chemistry (21).

Benfey was the third member of the chemistry department



Laurence Strong

at Earlham, joining Strong, a physical chemist, and Wilmer Stratton, an inorganic chemist. Benfey found Strong to be a "mature scientist who, at the same time, was very eager to do something new and interesting in chemical education":

We very soon started discussing what was wrong with the general chemistry course, and decided the problem was that there were too many subjects, going all over the map. We began to think that we ought to develop a curriculum where each course would deal with a much more concentrated group of subjects. It began with a course called "Particles of Chemistry," and then "The Covalent Bond," which was a freshman exposure to aliphatic organic chemistry and other non-ionic compounds in the context of bonding. That was followed by a course on ions, which included an introduction to inorganic qual and quant. There was a course on chemical energy, and then back to organic in the junior year with a course called "Resonance and Aromaticity." Advanced organic chemistry became "Kinetics and Mechanism."

In developing the new curriculum, Benfey and Strong focused on concepts rather than on "the classic divisions inherited from

the 19th century." In 1957, Strong was invited to a conference on precollege chemistry at Reed College. Organized by Harry Lewis of the Institute of Paper Chemistry, the group consisted of "disgruntled people who were worrying about high school chemistry." Strong proposed that the high school course should have "coherence and structure," not just a series of topics that colleges insisted should be covered. This was related to Jerome Bruner's concept "that the only way one can really understand a subject is by tying facts and details into a broader conceptual structure." The group encouraged Strong to seek funding for the development of the course he was proposing.

In the years immediately following the 1957 launching of Sputnik by the Soviet Union, the National Science Foundation became interested in curricular revisions as a means of lifting "American chemistry to keep ahead of any Soviet challenge." While Strong was looking for support for a new high school course, the new Earlham curriculum was published in the 1958 report of the newly formed and NSF-supported Advisory Council on College Chemistry.

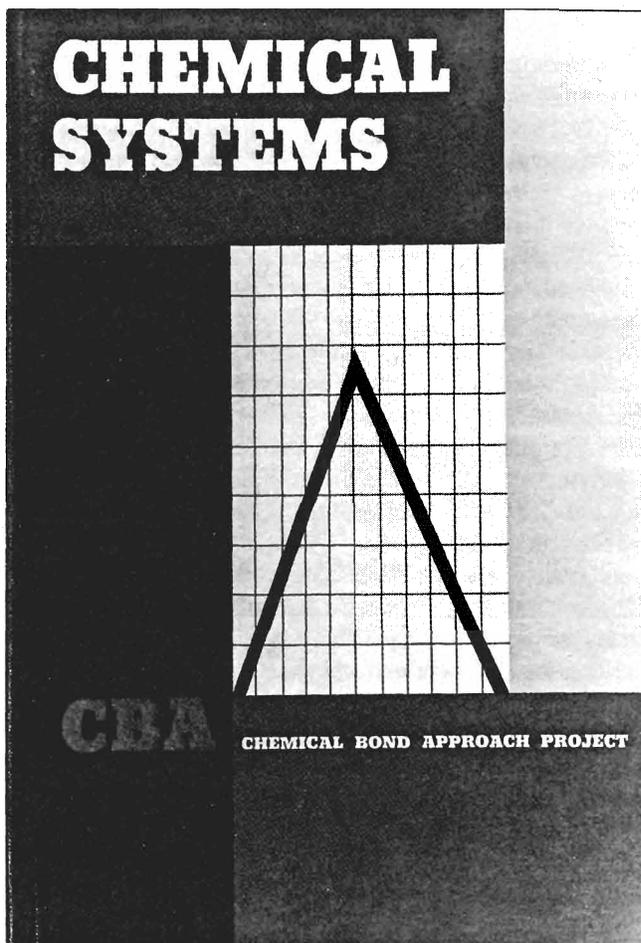
The major changes proposed by Strong and Benfey were not easily implemented because they were counter to "accepted practice." After adding a fourth member to the department, Earlham requested program approval by the American Chemical Society's Committee on Professional Training (CPT). Although the CPT was satisfied "on every count" of its usual criteria, they were skeptical about the "massive transformation" in the curriculum and refused to put Earlham on its approved list until the first group of graduates was produced under the new arrangement. With great satisfaction, Benfey recalls that there were three National Science Foundation Fellowship awardees in that first group. Faced with this evidence, the CPT not only placed Earlham on its list of approved chemistry programs, but "almost immediately" started using the Earlham model "as proof that they believed in curricular experimentation."

Another barrier to curriculum innovation was the lack of suitable textbooks. Benfey tried to teach the way he had learned in England, without texts, but found that "extremely hard." Laboratory innovation was easier, and many new experiments were developed. For the energy course, Strong and Stratton developed a small paperback entitled *Chemical Energy*, while other publications in that Prentice Hall series were used for supplementary and overview material (22). In retrospect, Benfey realizes that they should have created their own texts. "Even though we had the new names and the new concepts," he concedes, "we tended to slide back into being textbook courses." More importantly, they did feel freedom to move far and wide, and insisted that the students move in those directions as well. Although widely recognized and "warmly received," the Earlham program essentially had no imitators. Benfey doesn't know of anyone who switched to the conceptual approach in their program. "The general feeling was that

you just can't move that far from the textbook and the accepted practices."

But the Earlham group's impact on stimulating the development of new ways of teaching chemistry was not limited to the college level. NSF support was secured following the Reed College meeting on high school chemistry, and in 1959 a conference of high school and college chemistry teachers was held to begin writing a new high school text that would be known as *The Chemical Bond Approach (CBA)*. A preliminary version was already available in 1961 when Benfey was in Ireland, talking about chemical education and the CBA project to European high school teachers at a meeting sponsored by the Organization for European Economic Cooperation (23):

Before that we were training teachers. It was an absolute conviction that you just can't hand out materials. What we wanted the students and teachers to do was to think about the concepts and how they related to data, rather than to memorize. Both students and teachers always tried to find the easy ways of coping with material. We had trial schools all over the country. We had regional conferences. There were six-week-long summer institutes.



Cover of the final version of the CBA text, *Chemical Systems*

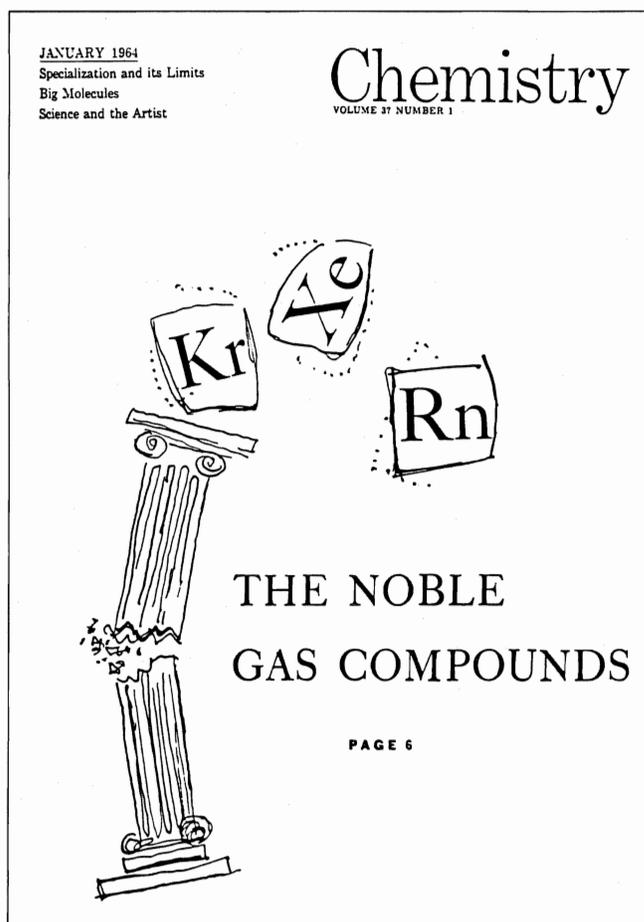
The CBA attracted interest all over the world, and by 1963 translations were in progress in Brazil, Japan, and Spain. Benfey even spent two weeks in Brazil in 1963, directing a teacher training program which was held at the aeronautical college at San José dos Campos. Many of the group could only follow written English, and found spoken English very difficult. For the teachers from the large German community in southern Brazil, Benfey found that if he used German words after English key words, "their faces would light up and they would catch on to whatever" he was talking about.

CBA had originally hoped to get ACS support, but the ACS felt CBA was "too radical" or "too set" in their ways for the ACS to have any input. Instead, ACS formed a national advisory board headed by Glenn Seaborg and George Pimentel, and launched the Chem Study project in 1959. According to Benfey, "they were much less innovative in repackaging chemical material. But it had a lot of prestige and a lot of money, and was much closer to accepted practice and more easily accepted and used by people trained in standard university programs." As a result, Chem Study received greater adoption and was translated far more than CBA. While the Chem Study text still appears in new editions, a second edition of the CBA text was never published.

In the midst of all this curriculum activity at CBA and Earlham, Benfey received an invitation in 1963 "that came completely out of the blue." The ACS had purchased *Chemistry* magazine from Science Service in order to acquire the name. Benfey was asked to become the editor, and was given the mission of developing a product that would serve the top 40% of high school chemistry students - those who had been stimulated by these new high school experiences. For the next 15 years Benfey managed to publish a highly-acclaimed journal, raising the subscriptions from the originally acquired 6000 to a high at one point of 30,000. Significantly, it served as the model for subsequent similar publications in Germany (*Chemie in unserer Zeit*), Canada (*Canadian Chemical Education*), South America (*Revista Iberoamericana de Educacion Quimica*), France (*Le jeune Scientifique*), South Africa (*Spectrum*), and Japan (*Gendai Kagaku*).

In his first year as editor, Benfey took a sabbatical leave at ACS in Washington. When he returned to Earlham, ACS agreed to pay one-third of his salary and thus reduce his teaching load. It was an arrangement no other ACS editor enjoyed. But *Chemistry* was not just printing articles submitted by others. In addition to Benfey there was a managing editor, an editorial assistant, and a secretary in Washington, and a lot of new copy was being generated in the *Chemistry* offices. Benfey's editorials were particularly incisive, and he credits ACS artist Joe Jacobs with the creation of the design and layout (24).

Feeling that chemistry courses dealt with the "internal" material of chemistry, Benfey devoted all of his concern as editor to fascinating students "by showing all the interconnec-



The cover of the first issue of *Chemistry* magazine (January 1964) to appear under Benfey's editorship

tions." He envisioned that the chemical community needed something like *Scientific American* but focused on chemistry. In fact, the readership did expand beyond the targeted high school student, and the magazine was read by high school teachers, university professors, and other professionals who found it a "useful way of keeping up with the whole of chemistry."

The initial mission, however, became part of the magazine's downfall during a growing attack on elitism. "Anything being produced had to serve everybody." It was also the era of accountability, and each unit had to be self-sufficient. Because *Chemistry* was under the Publications Division and not under dues-supported services, it had to pay for its proportion of the ACS building and other peripherals in proportion to its subscriptions. To save money, several monthly issues were combined, reducing the yearly output to ten issues. Finally, an ACS evaluation committee "decided to rethink" the whole purpose of the magazine, and determined that it could be done in-house. They suggested conditions under which Benfey could remain as editor, but he rejected them. When Benfey left

in 1978, the name, purpose, and format were changed. He was thankful that *Chemistry* under ACS was always under his editorship, and "that it didn't go on with a different editor and purpose to confuse readers."

In addition to his teaching, activities in chemical education, and magazine editorship, Benfey's scholarship in the history and philosophy of science continued to flourish during his years at Earlham. His papers often show an amazing combination of depth, breadth, and insight, with linkages spreading out in different directions to touch several intellectual regimens. He wrote about two "underdogs - Archibald Scott Couper (25), whose work on structural theory was overshadowed by Kekulé; and Lothar Meyer (26), whose work on periodicity was overshadowed by Mendeleev. His interest in

the creative process in science led to a translation of van't Hoff's inaugural speech after he had launched the tetrahedral carbon theory (27). Using the title, "The Role of the Imagination in Science," van't Hoff described the creative interests of many scientists, including their activities as

poets, musicians, and dramatists. Benfey also analyzed the two papers that Alexander William Williamson wrote about his ether synthesis, suggesting that the two versions "probably represented the period of transition to the modern way of reporting research," changing from personal pronouns to the impersonal passive style (28). He speculates that scientists did so because it is related to the feeling that "science is universally true. Once data are discovered, they have nothing to do with the particular context of the discovery or the scientists involved, and the publication should reflect that."

While Benfey was at Earlham there was considerable national interest in programmed instruction, a question and answer procedure for teaching with immediate feedback, and a method soon taken over by computers. When a number of

Earlham faculty agreed to develop and test such materials, Benfey decided that organic nomenclature and the writing of Lewis and resonance structures would lend themselves to this type of training. His *Names and Structures of Organic Molecules* was published by Wiley in 1966 (29). It was also at this time that Charles C. Price, Head of the NSF-sponsored Advisory Council on College Chemistry, asked Benfey to write *Introduction to Organic Reaction Mechanisms*, which became the first of the Council's "Interface Books," published by McGraw-Hill in 1970 (30). Appropriately, Benfey dedicated the book to Ingold, Hammett, and Westheimer.

During the Cold War period of the 1960s the American government became concerned that their overseas diplomats and representatives didn't know - and had great difficulty in

learning - foreign languages, forcing them to work through interpreters. The Soviet Union, however, would bring a complete staff who were fluent in the language. The U.S. government established centers for training area specialists, and Earlham became the Japan center for the Great Lakes Colleges Association.



Editing *Chemistry* magazine, Earlham College, circa 1964

Each year "somebody from Earlham was sent to Japan on a Fulbright-Hays Research Study Fellowship to broaden faculty horizons to include non-Western aspects," which Benfey describes as a "deprovincialization" process.

Benfey, his wife Rachel, and two of their three sons left for Japan in 1970 to spend a year at Kwansai Gakuin University in Nishinomiya. Even though he had spent the previous year studying the language with Japanese nationals at Earlham, he "still knew very little." Benfey regards the lack of a language skill as a benefit, for otherwise he would have spent "endless hours in libraries." Instead, he went looking for signs of geometric patterns, visiting antique shops, museums, local stores, and craft and pottery centers. At the same time, he continued to study the language by trying to translate a small

book by Kyoshi Yabuuchi on the history of Chinese science (31).

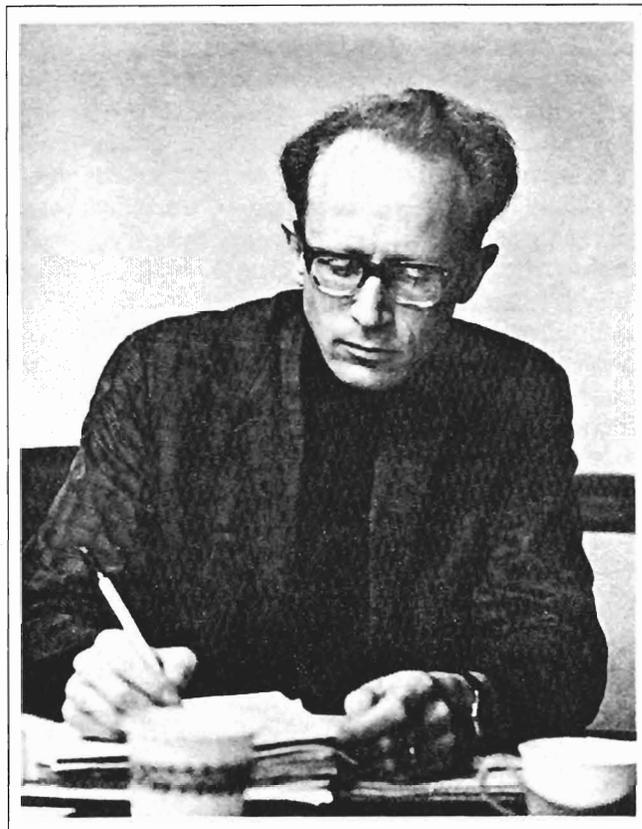
To Benfey, "structural theory is almost synonymous with chemical geometry" (32):

When I became interested in the Orient, I noticed Joseph Needham saying that one finds no interest in the regular solids in China or even much interest in geometry. I discovered that was just plain wrong. What Needham should have said was that there was no interest in ancient China in deductive Euclidean-style geometry. Endlessly in Japan and China one is aware of their love of geometric pattern. Chinese window lattices and Japanese wrapping papers attest to that. I think many of Euclid's conclusions just seemed too obvious to the Chinese to require elaborate demonstration.

Benfey was the first to point out the regular solid geometry in an 8th-century bronze spherical incense burner at the Imperial Treasure House in Nara, Japan, on which twelve pentagons can be seen (33). He has also speculated about how the Chinese might have come across the dodecahedron "because the usual assumption is that Euclid didn't get to China until 1600." To Benfey, "it's pretty clear that the dodecahedron was discovered by using vines in basketry." Hexagons were used to make flat surfaces, and to turn a corner one strand is omitted, leaving a pentagon at the corner. When only pentagons were used, the result was the wicker ball, 12 pentagons created by intertwining six equators, still used in ball games all over southeast Asia. While in Japan, Benfey also discovered a physical chemist who used origami techniques to train students in the construction of the regular solids. Benfey still delights audiences with his explanations of the relationship of geometry, chemical structure, and origami techniques which he continues to explore and develop.

When Grimsley Hobbs became president of Guilford College in North Carolina, he tried luring Benfey from Earlham. He and Benfey had known each other from their Haverford days, when Hobbs was a graduate student in philosophy. Later they shared in teaching a course in the philosophy of science at Earlham. Rachel Benfey was a Guilford graduate, but she and Ted were quite annoyed with the school for many years because it was so slow to integrate. By 1967 that had changed, primarily because the Friends World Conference would not accept the Guilford offer to host the meeting unless it was an integrated institution. In part this was because there was a large Quaker community in Kenya, where some African chiefs and their regions had become Quakers.

The Benfey family rejected Hobbs at first, but when he repeated the offer in the fall of 1971 after their return from Japan, they reconsidered. In Richmond, Indiana, they felt "the lack of cultural diversity and the distance from the major eastern seaboard centers." There was also the appeal of being able to create and head a chemistry department. Feeling they should stay at Earlham another full year, they agreed to accept the



At Guilford College, circa 1975

invitation, but not until 1973.

Benfey found a totally "blank" department when he arrived, for the one previous full-time member, Harvey Ljung, who had agreed to stay on for a one-year overlap period, decided to leave early. At a Quaker conference, Benfey met a former Earlham student, David MacInnes, who had a Ph.D. from Princeton and was teaching at a Friends school near Philadelphia. He subsequently became Benfey's long-term colleague at Guilford. Almost immediately they began establishing an unusual evening program for technicians at the newly arrived Ciba-Geigy agricultural, dye and chemical divisions and research groups at Greensboro. Rotating the basic chemistry courses through evening sessions, it was possible for a Ciba-Geigy employee who already had freshman chemistry to complete the degree requirements in three years. But it was a grueling pace for the faculty, who had to teach three-hour sessions of lecture and laboratory two nights a week.

There was a traditional opinion that of the Quaker schools, Haverford, Bryn Mawr, and Swarthmore were the leading intellectual group, followed by Earlham, with Guilford ranked slightly behind Earlham:

But all of that is changing now. In terms of intellectual stimulation, the delightful thing about Guilford was to be part of its transformation

from a very traditional campus to a nationally recognized one. I was involved again in an exciting transforming period both during the time Grimsley Hobbs and then later William Rogers were president. Rogers, a former colleague at Earlham, gave up a named professorship at Harvard to become president of Guilford in order to work out a vision he had of a supportive community, not just intellectual, but of a total community. It's been very exciting working on all three campuses because in each one, three new presidents were transforming campuses in directions I was very much in sympathy with.

Benfey's history of science course at both Earlham and Guilford was very popular because it was one of four courses that would satisfy the science requirement for non-science majors. Drawing up to 50 humanities students at a time, Benfey did not use a chronological approach but instead chose three themes - cosmology, atomism, and evolution. Out of that course came Frances Moore Lappé (who later wrote *Diet for a Small Planet* (34)), David Rhees (now director of the Bakken Library and Museum in Minneapolis), and William Newman (now on the Harvard history of science faculty).

Benfey's range of thought and interest is reflected in the joint courses taught with other Guilford faculty. One was on Oriental science, shared with William Beidler, a chemist who had moved into Indian philosophy. In a science and religion course he and Melvin Keiser discussed Michael Polanyi and Loren Eiseley. With Rex Adelberger from physics he taught a course in the history of technology.

It is a Quaker tradition "to retire early from worldly pursuits in order to devote time and energy to the needs of the Quaker community and other social concerns." Having found chemistry teaching and "endless involvements in a small college community ever more demanding," Benfey decided to take early retirement once his three sons were sufficiently independent. Since he and Rachel had met at Haverford and they liked the Philadelphia area, they searched for part-time possibilities there; eventually Benfey accepted a position as editor for the Beckman Center for the History of Chemistry (now the Chemical Heritage Foundation).

In an attempt to be "footloose and travel," the Benfey family sold their house and car and moved into a one-bedroom apartment in the Society Hill Towers of Philadelphia. When they discovered that wasn't their style, they eventually were accepted into the Bryn Gweled community in Southampton, Pennsylvania. Founded in 1940 by a group of Quakers and others who were "influenced in part by some theories of Henry George regarding land values, income taxes, and the evil of land speculation," Bryn Gweled now has 70 families who own their homes but have a 99-year lease on the two or sometimes up to four acres surrounding it. All 243 acres are communally held, and it is "designed for inter-racial housing, away from zoning laws." Benfey had known about Bryn Gweled from his SSRS days at Haverford, since many of the SSRS committee heads lived there. Wherever the Benfey family lived, they had always looked for

such a community, but never found one. Thus Ted Benfey has returned to some of his early roots in the United States, living in the supportive environment at Bryn Gweled while pursuing a myriad of intellectual concepts. His life continues to have a profound effect on others, including students and colleagues. His passionate concern for the human condition, which has permeated his personal philosophy, remains as a brilliant model for others to emulate (35):

Out of our self-examinations may come a new fusion of the means we have now mastered with the hopes and dreams of the human spirit to provide a fit habitat for all people. We must turn all that we possess into the channel of universal love.

References and Notes

Author's Note: All quotations, unless otherwise indicated, are taken from an oral history interview conducted by the author on 24 May and 5 June 1991. (See The Beckman Center for the History of Chemistry, Transcript #0094.) It is a distinct pleasure to work with Ted Benfey as a colleague at the Chemical Heritage Foundation. His meticulous, constructive, and supportive editing enhances manuscripts beyond any author's expectations.

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35. Foreword to reference 24. The last sentence in the quotation is from John Woolman, the 18th-century Quaker who "lovingly and persuasively campaigned against slavery."

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BY WAY OF EXPLANATION

Otto Theodor Benfey, Chemical Heritage Foundation

When I began contemplating early retirement from Guilford, I expected to fade quietly into the background from the world of activity and achievement. I never expected the recognition given me, including the plans for this symposium, Arnold Thackray's invitation to join him at the Beckman Center for the History of Chemistry, and with it a most satisfying faculty linkage with the University of Pennsylvania's Department of the History and Sociology of Science. Now I am again savoring the delights of being an editor - periodically seeing a mass of jumbled notes, manuscripts and pictures being miraculously transformed by my excellent production staff, led by Frances Kohler, into a pleasing product.

Not often does a group of academic and industrial research chemists and educators, historians and government officials participate in the same gathering. Yet such a group was

gathered in Winston-Salem at the ACS Southeast Regional Meeting. Its diversity is a reflection of the motley jumble in my brain. Until a few weeks ago I could find little to justify my various enthusiasms, there seemed little to link them together. What could possibly unite my interest in Plato's *Timaeus*, the Newton-Leibniz particle-monad controversy (in which my sympathies were all on Leibniz's side), structural organic chemistry, chemical education, the Orient, the Society for Social Responsibility in Science, my fascination with the five regular solids - the tetrahedron, octahedron and so on - and searching for scientifically meaningful and sound definitions of common yet not clearly understood chemical concepts?

Three weeks ago in a most unexpected place I found the link - rereading after several decades Nicholas Berdyaev, the Russian religious philosopher (1):

The natural world, according to the Aristotelian and Thomist conception, is not penetrated by divine forces; it lives according to its own laws and is only subject to the organized action of external grace.

He contrasts this with the Platonic view in which (1):

The natural is rooted and grounded in the supernatural; the divine energy comes into the world and makes it divine. The empirical world is rooted in the world of ideas and the world of ideas rests upon God.

I have discovered that I am hopelessly anti-hierarchical, objecting strongly to the notion that the material is fundamentally different from and inferior to the spiritual. I am, it seems, a neo-Platonist, seeing, and if not seeing then passionately searching for divine perfection, that of God, as Quakers would say, in my fellow human beings and in the world of nature. I am fascinated by harmony, mathematical pattern and geometric form in chemistry and long to share my fascination with students and with a wider public via the printed word.

The unity of all creation, that pervasive interconnectedness, is stressed in Oriental thought. It characterized Leibniz's monads which seek to harmonize with each other, in contrast to Newton's isolated lonely billiard-ball atoms that needed superior guidance to dragoon them into acceptable behavior.

There was an occasion some years ago when I made an attempt to make a virtue of my dilettantism. In speaking to a group of high school teachers at a Dreyfus-Woodrow Wilson summer institute held at Princeton University, I called on them - as a way of preserving their intellectual sanity and their belief in their mission as teachers - to find some topic that interested them and to pursue it whenever an opportunity presented itself and wherever it led.

My personal topic of fascination had been the regular geometric (Platonic) solids, the tetrahedron, octahedron, icosahedron, dodecahedron, and cube, from which Plato in his *Timaeus* constructs the world. I learned of Plato's *Timaeus* in the early 1950s from the German nuclear astrophysicist-

philosopher Carl Friedrich von Weizsäcker in a conversation whose focus was the social responsibility of the scientist. Strange that chemistry texts continue to equate ancient science with Aristotle - probably to show how superior we are. If freshman chemistry began with the *Timaeus*, how different it would be.

I have pursued the regular Platonic solids ever since that conversation. Geometry is the clue to understanding organic chemistry, maybe to all of chemistry, and its organizing power had lured me into chemistry. The Platonic solids led me from a tinker-toy-type construction set I had given my children, via an 8th-century spherical incense burner with dodecahedral design in Japan's imperial treasure house, to T'ang era China, to the pottery of Iran, to Malayan basket makers and Euclid's *Elements of Geometry*, via Kepler's spacing of the planets and the speculations of Couper, Kekulé, Le Bel, van't Hoff, and Alfred Werner, back to my field of structural organic chemistry and beyond it to the icosahedral geometry of the boron hydrides, intermetallic crystals and of tobacco mosaic and other viruses.

I suggested to my teacher audience that there is today a great need for lateral as well as longitudinal research - something like the distinction between external and internal history of science. In addition to the usual prescription for making one's mark in science by concentrating and specializing, learning, as some say, more and more about less and less, there is a need for the networkers too, those who show how all the specialized nuggets of expertise are interconnected. That, it seems to me, is the peculiar task of educators, to show the next generation the beauty and grandeur of the edifice of knowledge, in order to convince young people to participate in its construction and elaboration. Mendeleev acted in this manner when he was facing the task of organizing his new textbook. He sought for a rational basis for discussing the ever-growing list of elements - and found it in the periodic recurrence of chemical properties.

And similarly Kekulé's insight into the cyclic structure of benzene, with all that followed from it in the clarifying and rationalizing of aromatic chemistry. The rise and astonishing power of the organic-chemical and dye industries, arose, so Kekulé informs us, from a moment when his textbook writing did not progress and he dozed and daydreamed and saw the dancing atoms link up in a ring. It doesn't matter if, as some now contend, the atoms never danced and the dream never occurred. Be that as it may, no one so far has questioned Kekulé's claim that he was trying to write a coherent textbook and that "the work did not progress." He had this passion to connect fragments that he, maybe more than others, knew were fragments, that they belonged together if only he could find the key.

Here are two textbook writers, pedagogues, concerned, when engaged in that task, not so much to enlarge the frontiers of knowledge as to fashion a map that showed how the newly explored territories fitted with each other and with the old

world. And in carrying out what seemed not at all a research task, they in fact immeasurably advanced the tempo and success-rate of research, not to mention the massive contribution to a country's gross national product.

I'd like to come back to my list of fascinations and comment on the last area, the clarification of chemical concepts. When I was a student at University College London, my professor, Christopher K. Ingold, gave a lecture on the optical activity and stereochemistry of certain substituted biphenyls. Their stability, their rates of racemization, depended markedly on the nature of groups in positions *ortho* to the interannular bond. Size did not seem a sufficient explanation. I went to the library and read some papers, something we had never been encouraged to do - reading the literature was not part of our training! I think I was even then aware that here was a form of stereoisomerism, of chirality, quite different from that enunciated by van't Hoff. For his type of isomerism, it was enough to examine the structural formula in terms of number of bonds and to what atoms they were attached. If four different groups radiated from a carbon, optical activity was expected and the number of isomers could be predicted with confidence.

In the case of the biphenyls there were no chiral centers - there were no carbons with four separate bonds to other atoms, and whether the molecule would be chiral or not could not be read from the formula. Biphenyl with two *ortho* carboxyl groups and two *ortho* nitro groups was chiral. If nitro groups were replaced by fluorines the molecule was not. To predict isomer number suddenly was no longer a task of simple arithmetic but required detailed physico-chemical knowledge of group sizes, kinetics and electronic effects on the interannular bond. There is a profound difference conceptually between van't Hoff's stereochemistry and that of the biphenyls, but in my 40 years of chemistry I have never seen mention of it in conceptual terms. My own writings on it have elicited no comments (2-4).

Thus my interest in chemical concepts surfaced in my student days in Europe, but they were powerfully reinforced by experiences in this country. I spent a postdoctoral year with Louis P. Hammett at Columbia in 1947 and was intrigued by Hammett and his mode of thinking. He had been influenced by Percy W. Bridgman's emphasis on operational definitions of every concept used in science. Hammett approached his own field of physical organic chemistry from that viewpoint. What, he would ask, operationally in terms of measurements performed, corresponded to resonance, to acid strength, to pH and so on. It was a field ripe for such analysis and Hammett did much to move the discussion of reaction mechanisms from speculation to an intellectually respectable area of research.

During that year I discovered George Willard Wheland's *Advanced Organic Chemistry*, a treasure trove of conceptual analysis, spending pages and pages on analyzing what we mean by isomer, while we "enlightened" teachers of the 1990s expect our students to understand isomerism as an obvious

concept comprehensible after a brief description (5).

Where today do we find discussions of these and other basic concepts? They have largely vanished from our textbooks; it is in these grapplings that important new research problems will surface. However, some of the participants in this symposium are engaged in tasks of this kind. It is my hope that publication of this symposium will rekindle interest in such discussions, and will inspire some students to continue such lines of inquiry.

I want to return to my student days. The war years were years of intellectual as well as physical, material and political turmoil. Around me, among students and faculty, were intense discussions about the nature of science, its role and function in society, its future organization and direction. Some of the graduate students - even some among the faculty and staff - were members of the Communist Party. They were highly articulate and confident in their diagnosis of the ills of science and the way it had better be operated in the future. They exuded a remarkable confidence that they would be in charge after the war. It was extremely hard to argue with them successfully because they were consummate debaters and thoroughly prepared with facts and arguments. Yet some of us knew, deep down, that they were wrong. Their analyses of the ills of society and of the misuse of science were incisive and to a large degree valid. Their solutions for the future, however, were suspect. They were based on assumptions about the nature of human beings and the reasons for their behavior that I and others sensed to be plain wrong. They blamed all evil on the dominant exploiting class. They were utopian; with the right organization of society, humans would be good. They ignored the reality of sin, of self-centeredness and selfishness, and vastly overrated the ability of planners to know and implement what was good for society.

Their presence made some of us read intensively and grapple with the causative factors of science's historical development and with the responsibility of the scientist. And in the midst of this intellectual and other turmoil, on 6 August 1945, the atomic bomb was dropped on Hiroshima. I walked in a daze through the streets of London. A force of nature, whose power for destruction had been hinted at in our lectures, had been unleashed by a society I still believed was animated by ethical norms, against an unsuspecting country of a different race. All the criticisms of science and society were brought into focus and I had to subject my counter arguments to intensive scrutiny. Here were the highest intellectual capacities cooperating with government leaders to develop and utilize the high achievements of science for mass destruction and massive human suffering. Where now was Pasteur's confidence that science would in balance be used for human good?

I decided to drop science, to become a doctor maybe like Albert Schweitzer. But then I reconsidered and chose to continue my Ph.D. - but with a resolve to do what I could to help science serve the common good.

Around 1950 I was part of the early years of the Society for Social Responsibility in Science. I was delighted to read recently that both the head of DuPont and the editor of *The Scientist*, Eugene Garfield, one of our symposium speakers, were calling on scientists and on the chemical industry to pledge themselves to an ethic of social responsibility and environmental sensitivity. The way I put it is that, just as biologists are the guardians of the biosphere, so we of the chemical community must become the guardians of the lithosphere, the guardians and protectors of the material world.

My interests in the concepts and history of science and the role of science and scientists in society have remained intense ever since. Hence the title of this symposium: "The Context of Chemistry: Conceptual, Historical, Social".

References and Notes

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CORPUSCULAR ALCHEMY

The Transmutational Theory of Eirenaeus Philalethes

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Among the most influential works of 17th-century alchemy, the treatises attributed to "Eirenaeus Philalethes Cosmopolita" surely deserve a prominent place. As I have recently shown, several works attributed to this Philalethes were actually written by an American alchemist educated at Harvard, George Starkey (1). Starkey was born in 1628 in Bermuda, then considered part of "America." He entered Harvard College in 1643 and graduated with an A.B. in 1646. In 1650 Starkey immigrated to London, where he became a member of the scientific circle centered around Samuel Hartlib. In the early 1650s he performed a series of experiments with Robert Boyle, who was also a member of the Hartlib group. During this same period, Starkey wrote two works of major importance under the pseudonym of Eirenaeus Philalethes - the *Introitus apertus ad oclusum regis palatium* and the *Tractatus de metallorum metamorphosi*; both texts were published after Starkey's death during the great London plague of 1665.

The well-known Danish savant Olaus Borrichius reported

posthumously in 1696 that Philalethes' *Introitus* was considered "by the whole family of chemists" to belong among "their classics" (2). Similar accolades had been uttered by Daniel George Morhof in his *Epistola ad Langelottum* of 1673 (3) and, to judge by the translations of the *Introitus* into English, German, French, and Spanish, and its numerous printings between 1667, when it first appeared in Amsterdam as the printing of Johann Lange, and 1779, it would seem that Philalethes' popularity was great indeed (4). Three further works by Philalethes, collectively named the *Tres tractatus*, were printed by Martin Birrius of Amsterdam in 1668 (5). In the following year the *Introitus* was translated into English and published as *Secrets Revealed* by William Cooper of London (6). Cooper became one of Philalethes' greatest promoters, publishing other *opuscula* by the alchemist whom he referred to in his *Philosophical Epitaph* as the "English phoenix." Cooper even advertised in the hope of discovering lost Philalethan manuscripts, promising to print whatever he could find (7).

Despite the almost frenzied interest in Philalethes during the Scientific Revolution, historians of science have been happy to ignore this alchemist until quite recently. Before the mid-1970s, virtually all the scholarship devoted to Philalethes had focused on the question of his identity, and most of this had been written by scholars in fields other than the history of science. Philalethes' alchemical writings have recently come to occupy an important place in the historiography of early modern science, however, thanks to the current interest in Isaac Newton's alchemy.

It is well known, of course, that Newton transcribed and composed a massive amount of alchemical literature, according to Richard Westfall's estimate over a million words (8). Those hardy few who have tried to ascertain the sources of Newton's alchemy, such as Westfall, Betty Jo Teeter Dobbs, and Karin Figala, agree in assigning an important role therein to Eirenaeus Philalethes (9). As a result of this discovery, virtually all serious analysis of the Philalethan *corpus* has been done by Newton scholars. Anyone who presently wishes to know what Philalethes thought will have to view his ideas through a Newtonian prism, which exercises its own peculiar refraction on our image of the American alchemist. It is my intention here to reconstruct the theory that lies behind the alchemy of Philalethes. In the course of this I shall make occasional reference to the recent work on Newton's alchemy, especially that of Figala. A judicious examination of Newton's debt to Philalethes will therefore serve both to illuminate some trends in Philalethan alchemy and to determine whether or not Newton's interpretation of it was in reality faithful.

De metallorum metamorphosi

Although the most famous of the Philalethan works is the *Introitus*, this work has more the character of an extended

riddle than that of an alchemical *theorica*. For the latter we are much better off looking at the *Tractatus de metallorum metamorphosi*, first published as part of the *Tres tractatus* appearing in 1668. *De metallorum* is in fact the most sustained treatment of alchemical theory that I have found in the Philalethan *corpus* and thus will form the primary focus of this paper. Philalethes begins his theoretical treatment of alchemy there by saying that the metals do not differ essentially but accidentally. The base metals are really immature gold, and they contain its substance *in potentia* along with a supervenient humidity which, due to their incomplete cooking in the bowels of the earth, has not been expunged from them. It is this immature humidity that is responsible for the defects of the base metals, defects such as friability, corrodibility, and low melting point. Evidence for this is found in mines, where lead, for example, usually coexists with silver: obviously the lead is merely a less mature form of the noble metal (10).

After giving further evidence that the metals are all composed of a substantially identical material that differs only in maturity and purity, Philalethes says that what is needed for transmutation is a "homogeneous agent excelling in digestive power" (11). This agent, furthermore, is simply gold "digested to the highest possible degree" (12). Such digested gold can penetrate metals radically, tinting them and fixing them so that they lose their volatility and low melting point. Even natural gold, if one ounce be used to gild six pounds of silver, will unite with the smallest particles of the exterior silver to the degree that it can be drawn out to a hair's breadth without any silver being exposed. But gold that has been alchemically digested will become much more subtle than natural gold, and so will be able to penetrate the very depths of a base metal and color it from the inside out. In fact, Philalethes continues, such digested gold will be fiery, due to what he calls the "law of the disproportion in subtlety between the four elements" (13). As we shall see, Philalethes is the exponent of a naive corpuscularism. The import of this "law of disproportion" is that the so-called four elements merely represent different sizes of constituent corpuscles - *minimae partes* or simply *minima*. What traditional philosophers call "fire" is made up of the smallest particles, so if gold is going to be digested, that is, broken down to the smallest possible particles, it will therefore become fiery. Only then, Philalethes says, will it be able to be mixed *per minima intrinsice* with the base metals (14).

Does this mean that Philalethes believes all mixture among the four elements to result from agglomerated particles of different sizes? Perhaps surprisingly, it does not. Rather, he says, the great "disproportion" in size between particles of different elements prohibits "the mixture of things suitable to generation, or even the possibility thereof" (15). Why? To use his words, because (16):

... natural generation comes about by means of a general union of ingredients. Union, moreover, is the ingress of the things to be united

per minima. Yet if the minimum of one be ten times or a hundred times smaller than that of another, these minima (not having been made equal to one another) cannot combine, since it is necessary to bring together *per minima* what we wish to unite *per minima*.

Water mixed with wine, Philalethes says, can be separated precisely because this *mixtio per minima* has not taken place. Nor can it take place, because the particles of water are too big to conjoin with those of the subtle spirit in wine. The same is true of mixtures involving phlegm and spirit in wine, as well as earth and water. Let us now return to Philalethes' words (17):

If anyone should say that in order to bring about [true] mixture, one [element] acquires the subtlety of another, and thus they are united immediately, I reply that if that (which was thick) becomes subtle to the degree that it can enter the liquid (by uniting with it), it is necessary that it be brought to the same nature, and what then I ask is the earth but water ... and thus, how fatuous must this be considered, that earth must be converted into water in order that it (be mixed) with water (to) bring forth the generation of a concrete body ...

Philalethes' argument hinges on the fatuity of earth retaining its earthiness after its minima have been reduced to the size of aqueous minima. Clearly he is assuming that the qualities traditionally associated with the four elements depend primarily on particle size. Indeed, when he continues to discuss water and its relationship to air, this becomes quite clear (18):

... if water should have the same subtlety as air, it is held to have the same primary qualities as air, and the same must be said of the earth that was made equal in rarity to water.

In other words, particles of earth reduced to the size of water particles will in fact be water particles, as they will share the same primary qualities. But if this is so, no *mixture* will have taken place, since there will be no more earth present to mix with the water. To drive the point home further, Philalethes asks rhetorically (19):

I wish to know (the following:) if one *primum* takes on the primary (qualities) of another *primum*, will not the first really become that *primum* whose qualities it assumed? To argue otherwise is not philosophical.

Having thus proven to his satisfaction that natural things do not come about from a mixture of four elements, Philalethes concludes in truncated fashion that all the so-called elements really derive from one origin, which, echoing Van Helmont, he says to be water. In other words, there are not really four elements in the sense of original constituent bodies, but one, water, and its particles really do undergo the subtiliations described above, which result in material change. The reader might then ask how the *minima pars* of water, if it is a true

minimum, can be reduced in size to produce air, for example. But Philalethes has already pre-empted this. The particles of water *per se* are not true minima. Water particles contain yet smaller particles or *semina*: these act on grosser matter, operating by means of a fermentative force, to produce products of varying subtlety. The fermentative force is itself supplied by "a certain ineffable particle of light" found within the *semen* (20). This "particle of light" is therefore the true *minima pars*, and it appears that all grosser matter is capable of division down to that terminus.

From this account we know that matter is corpuscular in composition, and that the root of all matter is water, which is acted upon by *semina* contained within itself, thus producing other substances. Philalethes then proceeds to detail a theory of artificial transmutation based on the above. Returning to his concept that metals vary only in their degree of purity and digestion, he remarks that the alchemist must therefore find an agent which both digests the metallic substance and expunges its impurities. In his words (21):

Our *Arcanum* (because it is a spiritual, homogeneous substance) enters into imperfect metals of this sort *per minima*, and what it finds like itself, it seizes and defends from the violence of the burning fire by means of its own powerful force, and it preserves it with its own more than perfect fixity, while Vulcan destroys the combustible with its burning flame. And once the combustible is consumed by the fire, there remains pure gold or silver.

To understand this the reader must recall that Philalethes earlier said that the alchemical elixir was simply gold digested to the

highest degree, and that this was a homogeneous, spiritual substance. This meant that the particles of gold had been reduced to a smallness like that of fire particles, and because all impurity had been removed, all these minute particles were of the same size, that is, homogeneous. It is because of this uniformly minute character of the elixir's particles that it can penetrate into base metals *per minima*, that is, between the smallest particles of the base metals. Once the particles of elixir have entered into the internal structure of the base metals, their affinity with the pure metallic substance within the base metal allows them to mix with it. They are after all

materially identical with this pure substance, and they are particles of the same size.

After the elixir's particles have mixed with those of the pure metallic substance in the base metal they form a protective barrier between the latter and the destroying fire. The fire then burns up whatever impurities are found in the base metal, and the substance that remains will be composed of minute, homogeneous particles: in Philalethes's words, a "Chrysopoetic transmutation" will have taken place, and gold will have been produced (22). It is possible, however, to produce silver rather than gold, depending, Philalethes says, "on the quality of the medicine." But what determines the quality of the medicine? How should the alchemist go about the production of this elixir?

As we now know, the elixir is itself highly digested gold. Gold contains in each of its minimal parts the *semina* responsible for transmutation, but in natural gold as it is dug from the mines, the *semina*, are sealed up and hidden "under very

In Sudore Vultus tui Vescitor Cibo



In't Sweet uwes Aanschyns sult ghy broot eeten.

An idealized portrait of George Starkey at his furnace with reagents pictured above him. The Biblical caption reads "You must earn your bread by the sweat of your brow." From *Pyrotechnia ofte Vuur-stook-Kunde* (Amsterdam, 1687), a Dutch translation of several works by Starkey.

dense coverings" (23). Therefore Philalethes says the following (24):

Let the sons of art know, that in order to arrive at our *arcantum* it is necessary to manifest the most occult *semen* of gold which may not happen without the full and total volatization of the fixed, and therefore the corruption of its form.

In other words, the *semina* hidden deep within the substance of the gold and thus "occult," must be revealed, made "manifest" by a breaking down of the metal's gross substance. In corpuscular terms this means that the grosser particles of the metal must be made to disintegrate, thus freeing the smaller particles or *semina* contained therein. As Philalethes says, "properly and exactly speaking, the *semen* is the *minima pars* of the metal" (25). It is thus possible to convert the entire substance of gold into *sperma* by a simple breaking down of its metallic corpuscles into still smaller corpuscles, that is, into *semina*. As Philalethes also tells us, when the *semina* have been released, the metal will liquefy at room temperature. In other words, metals owe their solidity to what are, relatively speaking, gross particles. When the gross particles are eroded to become more subtle, the internal rigidity of the metallic substance is lost. Liquidity, therefore is a macroscopic property of extremely small particles making up the microscopic structure of a metal. As I have shown elsewhere, the origin of this theory lies in medieval alchemy (26).

The Epistle to King Edward Unfolded

The terminology that Philalethes uses in *De metallorum* suggests that he had a definite idea about the corpuscular structure of metals on the micro-level. He repeatedly speaks of the *semina* as existing within the larger corpuscles or *partes* of gold, for example. The *semina* are found in *profunditate* or in *occulto*, or *sub involucris densissimis*. What exactly does he have in mind here? At this stage it will be useful to turn to another Philalethan work, *The Epistle to King Edward Unfolded*, which has already been analyzed by Karin Figala in her work on the alchemy of Newton (27). Here Philalethes lays out a theory that Figala calls the "shell-theory" of matter, employed by Newton in his alchemical studies. In *The Epistle* Philalethes adopts the well-worn sulfur/mercury theory of the metals, according to which metals are composed of these two substances. To use his words (28):

... all metall, & severall mineralls have [mercury] for their next matter, to wch for the most part (nay allways in imperfect metall) there adheres, & is concoagulated an externall [sulfur].

In what may be called the traditional form of the sulfur/mercury theory, mercury is in effect a passive material that is acted upon by sulfur to produce the different metals. This is in

the back of his mind when Philalethes says that an external sulfur is "concoagulated" to the mercurial substance of the metals. But Philalethes has far more than this in mind. He maintains that metals in general are composed of three different types of sulfur in conjunction with mercury. Although the three types of sulfur may be removed to some degree from their mercury, it is impossible to isolate mercury from all its sulfur: indeed sulfur itself is merely an active, mature form of mercury (29).

The base metals have first an "externall [sulfur], wch is not metalline, but distinguishable from the internall kernell of the mercurie" (30). This external sulfur acts as the principle of corrosion in imperfect metals, and must be removed if they are to be perfected. The second type of sulfur lies within the first, and is called the "metalline sulphur" (31). This metalline sulfur is found in all metals, and is responsible for the coagulation of their mercurial substance into a solid form. In gold and silver, however, the metalline sulfur is pure, while in other metals it is less pure. But Philalethes tells us that even this metalline sulfur is "externall to, because separable from the Secret Nature of [mercury] ... in form of tinted sweet oyle..." (32). Once the metalline sulfur has been removed, Philalethes continues (33):

The remaining [mercury] then is voyd of all [sulphur], Save that wch may be called its centrall incoagulable [sulphur], on which no corrosive can then worke ...

As Figala has shown, the import of this theory is well represented by three concentric circles depicting the layers or "shells" of sulfur. The outermost shell is the "external" or mineral sulfur which, acting on the metallic mercury, only causes corruption and corrosion in the base metals. Within this is the layer of "metallic sulfur" responsible for the mercury's solidification in metals. Finally, at the center of the circles we encounter the "centrall, incoagulable" sulfur which can never be separated from its mercury (34).

Figala's use of the term "shell-theory" is indeed appropriate for Philalethes' concept of three sulfurs. By comparing *The Epistle* to the passages in *De metallorum* where Philalethes describes the structure of gold, we can further see that when he speaks of external and internal sulfurs, Philalethes has in mind the different layers of a complex corpuscle. The external sulfur of *The Epistle* is identical to the gross, superfluous impurities of *De metallorum* that had to be removed from base metals in order to effect their transmutation. This external shell is absent in gold, thus accounting for its resistance to corrosion.

The *minima* of gold *per se*, that is the smallest parts of natural gold, correspond to the second type of sulfur - the "metalline sulfur" that in base metals is covered by the outward, unclean shell of the mineral sulfur. This metalline sulfur, as Philalethes told us, is responsible for coagulating the mercury of gold, which exists within it. In other words, particles

of gold are composed of an outward metalline sulfur surrounding a central core of incoagulable sulfur and mercury. But since the central, incoagulable sulfur cannot be separated from its mercury, the two can be conflated and referred to simply as "mercury." As Philalethes says in *The Epistle* (35):

... one [sulfur] is the most pure red Sulphur of gold, which is Sulphur in *manifesto* and Mercurius in *oculto* ...

Particles of this sort make up the homogeneous solid, gold, and thus may be called the *minimae partes* of the metal. But as Philalethes already told us, more properly speaking, the *minimae partes* within the metal are the *semina* contained within the corpuscles of gold, existing *sub involucri densissimis*. These *semina*, I propose, correspond to the "incoagulable," "central," "fiery," sulfur that Philalethes tells us exists at the kernel of the metal. In *De metallorum* Philalethes told us that the *semina* are freed when the gold is disintegrated and made liquid in the course of its digestion. What he has in mind clearly is the removal of the metalline sulfur, the agent responsible for metallic coagulation: when this has been deleted, the remaining substance will thus be incoagulable. Its lack of solidity will be due to the extreme fineness of its particles: as we stated before, Philalethes makes use here of a medieval theory relating solidity to particle size. Similarly it will be "fiery," again because its corpuscles will be extremely small, like those of fire. Finally it will be "central" in the sense that it composed the central "nucleus" of the complex corpuscle whose outer shells have now been removed (36).

In *De metallorum metamorphosi* Philalethes clearly describes the concept of a complex corpuscle, where the *minima pars* of gold, for example, is composed of yet smaller particles, down to the "ineffable particle of light" that forms the smallest of all corpuscles. As we have shown, the complex corpuscle was tied up in Philalethes' mind with the notion of different shells of sulfur, which are described in *The Epistle*. At the center of the complex particle there is a "nucleus" composed of extremely fine "sub-particles." The very subtlety of these corpuscles prevents their "coagulation" into a solid mass: indeed, Philalethes speaks of them as being "spiritual." But when tightly packed into the center of the complex *sub involucri densissimis*, their concentration yields tremendous weight. Philalethes' alchemical sources explicitly link the subtlety and close-packing of ordinary gold's particles to its ponderosity and great malleability.

But Philalethes has altered their corpuscular ruminations by adding on his shell theory of matter. Surrounding the central kernel of tiny, densely packed corpuscles, there is a shell composed of larger particles, which are responsible for compacting the tiny particles in the center into their concentrated mass. This compaction results in the solidification of metals: hence Philalethes calls it the "metallic sulfur," as we earlier discussed. Finally, in impure metals, there is yet another shell,



Isaac Newton

the layer of "external sulfur" which can easily be removed. Philalethes told us that this external sulfur was responsible for the corrodibility of base metals. If we now envision this shell of external sulfur as being composed of particles that are still larger than those of the metallic sulfur or incoagulable sulfur, the reason for its inability to withstand corrosion will be clear. Just as the density of gold and mercury is due to the fact that they are made up of small particles which can be closely packed, so the presence of large particles in a substance will result in loose packing. The external sulfur shell will be made up of precisely such loosely packed large particles, separated by large pores. The presence of such large pores in a metal allows the corpuscles of a corrosive agent to enter into its structure and attack it, resulting in the breakdown of its metallic integrity (37). The absence of such pores in gold leads to the opposite effect - hence it is far more difficult to corrode gold than base metals. Similarly, the presence of large particles and pores will result in a loss of density, and so the base metals will be of lighter specific weight than gold.

Philalethes and Newton

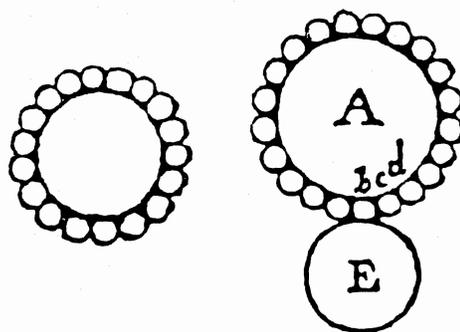
In order to see where a corpuscular theory of this sort can lead, let us now turn to the detailed exposition of Newton's transmutational theory given by Figala. In the *Opticks* and in his opusculum *On the Nature of Acids*, Newton develops a highly un-Cartesian corpuscular theory. It was named the "nutshell theory of matter" by Joseph Priestley, because it allowed that all the solid matter in the universe could be fit into a walnut

shell. Now in his *Opticks*, Newton argues for the existence of "solid, massy, hard, impenetrable" particles or atoms, out of which all gross matter is made (38). These totally solid particles are, to use Arnold Thackray's phrase, inertially homogeneous; they are composed of a uniformly dense matter separated by great expanses of void. In order to account for the differentiation of matter on the macro-level, Newton says these solid particles combine with an equal quantity of void, to form what Newton calls the "first stage of composition." Such particles of the first stage of composition in turn combine with an equal quantity of void to produce particles of the second stage of composition. Higher stages of composition are produced in the same way (39). As none of the metals known to Newton belong to a composition stage lower than the first, they therefore contain vast quantities of void. As the quantity of their void diminishes, their specific gravity increases. Now Newton argues that the reactions of "vulgar chemistry" - what we would call simply chemical reactions - take place between the particles of higher composition. If one wants to transmute metals, which is not merely a process of "vulgar Chymistry" but of "Hermetick" philosophy, he must break down the particles of higher composition to arrive at the simpler, denser ones, then recombine them with the proper amount of void requisite to the specific gravity of the desired metal (40).

It is important to note that Newton's hierarchical schema, while giving the proportion of void to matter in different types of particles, does not necessarily describe the physical structure of the particles themselves. In Query 31 of the *Opticks*, Newton pictures the micro-structure of a salt particle in the following manner (41):

As Gravity makes the Sea flow round the denser and weightier Parts of the Globe of the Earth, so the Attraction may make the watry Acid flow round the denser and compacter Particles of Earth for composing the Particles of Salt ... Now, as in the great Globe of the Earth and Sea, the densest Bodies by their Gravity sink down in Water, and always endeavour to go towards the Center of the Globe; so in Particles of Salt, the densest Matter may always endeavour to approach the Center of the Particle: So that a Particle of Salt may be compared to a Chaos; being dense, hard, dry, and earthy in the Center; and rare, soft, moist, and watry in the Circumference.

So in the case of a salt particle, at least, the denser, simpler, sub-particles migrate towards the center of the corpuscle, to find themselves surrounded by their rarer counterparts. Newton calls the denser particles "earth" and the rarer ones "acid." Hence the salt particle is composed of a kernel-like center surrounded by a shell-like circumference (42). It is fascinating that Newton, in various draft additions to *On the Nature of Acids* argues that "what is said by chemists, that everything is made from sulphur and mercury, is true, because by sulphur they mean acid, and by mercury they mean earth" (43). Hence he meant his theory that salt is composed of earth and acid to



Figures from Newton's letter to Robert Boyle of 1678/9. The sphere surrounded by smaller particles illustrates Newton's concept of saline particles "encompassing the metallick ones as a coat or shell does a kernell ..." The same terminology appears in the corpus of Eirenaeus Philalethes, with which Newton was intimately acquainted. From the 1744 edition of Boyle's *Works*.

apply also to metals. As a result, their internal structure must also in his eyes have resembled a dense kernel surrounded by a lighter shell.

To anyone familiar with Philalethes, Newton's reference to a mercurial core surrounded by a layer or layers of sulfur will bring to mind at once the alchemist's shell-theory of matter (44). Once we make this terminological substitution, it is easy to see how Philalethes' sulfurous shells, progressing inward towards ever greater perfection, correspond to the complex Newtonian particles, whose density - and hence perfection - increases as we approach the center. The transmutational import of this can be better appraised if we now consult the so-called *Clavis* previously thought by various scholars to be by Newton. In reality it is but part of a letter written by Starkey to Boyle, probably in 1651 (45).

The *Clavis* or *Key* teaches the production of an amalgam composed of mercury, antimony, silver, and ultimately gold. Starting with the specifically light antimony sulfide, the alchemist strips off its "external sulfur" to produce metallic antimony (46). The antimony is then fused with the denser silver, which Starkey says will act as a mediator between the antimony and mercury. When the silver/antimony alloy is added to the still-denser mercury, blackness is given off, and a "great stink." This reveals that the second sulfurous shell has been removed from the previously solid silver/antimony alloy. The product, a mercury/silver/antimony amalgam, called "actuated mercury," is then added to gold. The idea is that the "actuated mercury" will then penetrate into the central kernel of the gold, free it, and by a process of "fermentation," lead to the philosophers' stone (47).

If one now views this shell theory in the light of Newton's hierarchical arrangement of particles, especially as described in Query 31 of the *Opticks*, it is easy to see how Newton's corpuscular theory may have been influenced by Philalethes. The particle of salt described there did in fact consist of a dense

earthy kernel surrounded by a rarer acid shell. And we know from Newton's additions to *On the Nature of Acids* that he meant this earth and acid to be coextensive with mercury and sulfur. There he maintained that "what is said by chemists, that everything is made from sulfur and mercury, is true, because by sulfur they mean acid, and by mercury they mean earth." Thus Newton's alchemical practice, described in his manuscripts, consisted of penetrating to the "innermost core" of a gold particle by employing a special mercury whose particles were small and dense enough to work their way through the interstices in the outer shell. Once this had been achieved, Newton seems to have believed that the Philaethan "actuated mercury" would produce the philosophers' stone. Newton refers to this reduction of gold to its "first matter," not only in his unpublished alchemical manuscripts, but also in his work *On the Nature of Acids* (48).

The introduction of Newton into an analysis of Philaethes raises certain questions which must now be answered. Despite the clear affinities between Newton's transmutational theory and that of Philaethes, there are obvious differences as well. First, there is no evidence that Philaethes associated mercury and sulfur with void and matter, as Figala argues that Newton did. Indeed, there is little reason to think that Philaethes was committed to the existence of absolute void, a concept that in Newton's work had profound resonances with his theology and physics. The corpuscularism of Philaethes is not a Democritean or Epicurean atomism. There is no indication that he was attracted to the philosophical atomism of antiquity, with its insistence on the existence of void. The direct sources of Philaethes' corpuscular theory, as we shall show elsewhere, are alchemical, not philosophical, and their ultimate origins are peripatetic and academic, not atomistic. Second, the abstractly quantitative aspect of Newton's work is totally absent in Philaethes. Newton committed himself to a determination of the relative proportion of particles and pores in all sorts of matter. He even derived a sort of formula for relating the different "stages of composition" to the relative proportion of void to matter in a given substance (49). This work was significant not only to Newton's alchemy, but had importance for his optical theory, where he attempted to relate color to particle size, and by extension to the relative proportion of matter to void (50). Again, this is an aspect that is totally lacking in Philaethan alchemy.

Despite these differences, the similarities between Philaethes' "shell-theory" and that of Newton are still striking. One of the most suggestive traces of Philaethes' influence lies in the already quoted Newtonian description of a particle of salt, where the physicist compared the dense core of the particle to the globe of the earth, saying that just as gravity makes the sea flow round the globe, so chemical attraction causes the acid in salt to encase the central core of earth. In each case, the denser matter exists at the core, and is surrounded by a rarer counterpart. Newton goes on to compare the salt particle to a "chaos,"

saying that it is "dense, hard, dry, and earthy in the Center; and rare, soft, moist, and watry in the Circumference." At first sight, this analogy between a salt corpuscle and a "chaos" seems inexplicable. In antiquity, "chaos" of course meant an indistinct mixture of elements, or a prime matter which existed before the elements *per se*. In the early 17th century, the term had taken on a new sense with Van Helmont's creation of the word "gas," which he claimed to derive from "chaos." But neither sense seems to apply here - Newton is thinking neither of an indistinct mass nor of a vapor. What he has in mind is a complex particle composed of two layers. The outer shell is in a sense the opposite of the inner core, being "rare, soft, moist, and watry," while the center is "dense, hard, dry, and earthy."

Where does Newton get this peculiar usage of the term "chaos," and what is he trying to impart to his reader? Let us consult the *Index chemicus*, an alchemical dictionary gathered together by Newton in the 1680s (51). After giving the conventional definition of chaos as a "confusion of elements" or "materia prima," Newton goes on to paraphrase the *Introitus apertus* of Philaethes (52):

This chaos is earth on account of its coagulation, and the mother of minerals on account of the minerals hidden in it, and yet it is a volatile air, and it contains a [sulfurous] heaven, in which the stars revolve in its center, which center is astral and which illuminates the earth up to its surface.

Philaethes' description of "chaos," loosely quoted by Newton, refers obliquely to antimony and to its role in the amalgamation process of the *Key*. Antimony is indeed a metallic substance, and thus an earth "on account of its coagulation." Yet it is also volatile upon heating, and so an "air." In its center it contains a "heaven" - in other words, the volatility of antimony is a function of the subtle matter forming the nucleus of its particles. Being small, these sub-particles at the center of an antimony particle are fluid and volatile, as in Philaethes' description of the incoagulable sulfur at the center of a metal. The solidity of the antimony, on the other hand, is literally a property of its "surface" - it is due to the coagulative power of the metallic sulfur surrounding the otherwise fluid core.

We see in Philaethes' description of "chaos" a reference to the shell-theory of matter complete with its terminology of "center" and "surface" describing the respective extremes of the particle. In the passage quoted from the *Opticks*, however, Newton seems at first to have reversed the order of the kernel and shell. While Philaethes calls the kernel "volatile" and links the coagulation of antimony to its shell, Newton calls the center "dense, hard, dry, and earthy," while the circumference is "rare, soft, moist, and watry." Why this reversal? The answer is easy to locate if we remember that when Philaethes describes the central kernel of a particle as being "fiery," "incoagulable," and "volatile," in *De metallorum metamor-*

phosi and *The Epistle to King Edward Unfolded*, he is not thinking of the pent-up sub-particles as they naturally exist, for example in gold, imprisoned *sub involucris densissimis*. Rather Philalethes has in mind the constituent sub-particles of the kernel as they would exist in their free state, if unrestrained by the exterior shells surrounding the core. Otherwise, he would be committing himself to the absurd conclusion that natural, undigested gold is under normal conditions (what we would nowadays call room temperature and normal atmospheric pressure) volatile and liquid. Newton, on the other hand, is thinking primarily of the central core and surrounding shell of the corpuscle as they exist in their natural state, before an alchemist has tried to liberate them. Within the close-packing of a salt-particle, the central corpuscles do indeed make up a "dense, hard, dry, and earthy" nucleus, being pressed into a rigid structure. The external shell, on the other hand, is "rare, soft, moist, and watry," since it is composed of loosely packed particles, less tightly bonded to a rigid structure. Newton is surely thinking in terms of his different "stages of composition," according to which each ascending particle contains the previous stage of particles plus additional void. This is precisely what he attempted to quantify by means of his formula linking different stages to their proportion of void and matter. This element is of course absent in the naive corpuscularism of Philalethes, since he eschews any overt reference to a vacuum, Philalethes could hardly have determined its proportion to the absolute matter in a particle. In his peculiar use of the term "chaos," then, we see Newton apparently borrowing from Philalethes for his use of that term to mean a complex corpuscular structure composed of kernel and shell. And yet we also see Newton adding to Philalethes that most Newtonian of characteristics - the urge to order his chaos by means of quantification.

References and Notes

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10. *Philalethae tractatus de metallorum metamorphosi*, in Manget, reference 2, Vol. II, p. 677.
11. *Ibid.*, p. 678, "... Agens Homogeneum digestiva vi pollens ..."
12. *Ibid.*, "... Aurum in supremum gradum ... digestum ..."
13. *Ibid.*, "... ex lege disproportionis in subtilitate inter Elementa quatuor ..."
14. *Ibid.*
15. *Ibid.*, p. 68; "Disproportio siquidem miscendorum mixturam generationi idoneum tollit, ejusve possibilitatem."
16. *Ibid.*, "Nam Physica generatio fit per generationem [*sic* Manget. Royal Society, Boyle Papers XLIV, 9r *legit* generalem] ingredientium unionem. Unio porro est per minima rerum unendarum ingressio, sin autem minimum unius minimo alterius decuplo vel centuplo subtilius, non possunt haec minima adaequata [*sic* Manget. R. S., B. P. XLIV, 9r *leg.* (minime adaequata)] coire, siquidem per minima convenire oportet, quae per minima unire quaerimus."
17. *Ibid.*, "Si quis dixerit: ad mixturam hanc faciendam unum subito alterius subtilitatem, atque ita deinceps uniuntur; Insto, quod si aequae subtile fiat [*spissum prius quod fuit*] ut liquidum possit [*uniendo sese*] ingredi, oportet ut ad eandem naturam prorsus deducatur, et quid tum quae terra quam aqua ... & sic, quam fatuum hoc imaginari, terram in Aquam esse convertendam, ut cum aqua concreti generationem promoveat ..."
18. *Ibid.*, "... siquidem ut aqua eandem habeat cum aere subtilitatem, eadem cum illa qualitates primas habere tenetur, idem & de terra iudicandum est, ut adaequetur raritati aquae."
19. *Ibid.*, "... scire cupio, utrum, si unum primum alterius primi primas induat, non fiat realiter illud primum, cujus sic induit primas. Contrarium asseverare non est Philosophicum."
20. *Ibid.*, p. 681, "... lucis quaedam ineffabilis particula ..."
21. *Ibid.*, p. 682, "Arcanum proin nostrum (quia Spiritualis Substantia homogenea) istiusmodi metalla imperfecta per minima intrat, & quod simile invenerit, apprehendit, & praepollenti sua vi ignis flagrantis violentia defendit, & fixitate sua plusquam perfecta retinet, interea Vulcanus ardens combustibile quoque flamma sua depascitur, quo per ignem consumpto, purum remanet Aurum, Argentumve."
22. *Ibid.*
23. *Ibid.*, p. 684, "... sub involucris densissimis ..."
24. *Ibid.*, "Sciunt itaque omnes Artis filii, quod ad arcanum nos-

trum consequendum opus sit Auri semen occultissimum manifestare, quod non fit nisi per plenariam omnimodam fixi volatisationem, ac proinde formae istius corruptionem.”

25. *Ibid.*, p. 683: “... proprie & exacte loquendo minima pars metalli est semen ...”

26. W. Newman, *The “Summa perfectionis” of pseudo-Geber*, Leiden, 1991, pp. 143-192.

27. Figala, “Newton as Alchemist,” reference 9, pp. 123-124.

28. [Eirenaeus Philalethes,] *Sir George Ripley’s Epistle to King Edward Unfolded*, MS. Glasgow University, Ferguson 85, pp. 1-80, p. 11.

29. MS. Ferguson 85, p. 13. Cf. also Philalethes, *Introitus apertus*, in *Bibliotheca chemica curiosa*, in Manget, reference 2, Vol. II, p. 664.

30. *Ibid.*, MS. Ferguson 85, pp. 11-12.

31. *Ibid.*

32. *Ibid.*, pp. 15-16.

33. *Ibid.*

34. Figala, “Newton as Alchemist,” reference 9, p. 120.

35. [Eirenaeus Philalethes,] *Sir George Ripley’s Epistle to King Edward Unfolded*, in *Chymical, Medicinal, and Chyrurgical AD-DRESSES: Made to Samuel Hartlib, Esquire* (London, 1655), p. 22.

36. The word “nucleus” is the Latin term for “kernel” or “nut.” Philalethes uses it in his unfinished manuscript of the *Vade mecum Philosophicum* (British Library, MS. Sloane 633, 107v).

37. This explanation of corrodibility as a product of porosity in the base metals was a commonplace in the alchemical tradition utilized by Philalethes. Cf. Newman, reference 26, p. 158, again quoting the *Summa*: “Propter enim eorum [i.e. copper and iron] multam terreitatis quantitatem et sulphureitatis adjustive et fugientis mensuram, defacili hoc modo adducuntur in calcem. Et illud ideo, quoniam ex multa terreitate argenti vivi substantie intermixta turbatur argenti vivi continuatio, et ideo porositas in eis creatur, per quam et sulphureitas transiens evolare potest. Et ignis ex causa illa ad eam accedens comburere et elevare potest illam. Per hoc igitur derelinguatur et partes rariores fieri et in cinerem per discontinuitatem raritatis converti.”

38. I. Newton, *Opticks*, Dover, New York, NY, 1952, p. 400.

39. A. Thackray, *Atoms and Powers*, Harvard University Press, Cambridge, MA, 1970, pp. 25 and 64.

40. For Newton’s distinction between “vulgar Chymistry” and “Hermetick” philosophy, see P. M. Rattansi, “Newton’s Alchemical Studies,” in A. G. Debus, ed., *Science, Medicine, and Society in the Renaissance*, Neale Watson Publications, New York, NY, 1972, pp. 176-177, and 170.

41. Reference 38, p. 386.

42. I. B. Cohen, *Isaac Newton’s Papers & Letters on Natural Philosophy*, Harvard University Press, Cambridge, MA, 1958, p. 252. Also Dobbs, *Foundations*, reference 9, p. 219.

43. Dobbs, *Foundations*, reference 9, p. 220.

44. Figala argues, furthermore, that Newton associated the alchemists’ mercury with the presence of “pores” (and hence void) in matter, echoing the Paracelsian view that mercury was the principle of volatility and hence attenuation. Similarly she thinks that Newton

viewed sulfur, the traditional principle of coagulation or solidification, as representing “matter.” See “Die exakte Alchemie”, reference 9, pp. 163-167, 183-186.

45. W. Newman, “Newton’s *Clavis* as Starkey’s *Key*,” *Isis*, 1987, 78, 564-574.

46. Philalethes, *Introitus apertus*, BCC, II, 663, “... sulphur externum ...”

47. Reference 45, pp. 572-574.

48. Dobbs, *Foundations*, reference 9, p. 218.

49. Figala, “Die exakte Alchemie”, reference 9, p. 164.

50. Dobbs, *Foundations*, reference 9, pp. 222-225.

51. Westfall, *Never at Rest*, reference 8, p. 358.

52. Here it will be useful to give both Newton’s paraphrase (Cambridge University Library, Keynes MS. 30, f. 22^r) and the original (Philalethes, *Introitus apertus*, in *Bibliotheca chemica curiosa*, Vol. II, p. 663):

Newton: Hoc chaos est terra propter coagulationem suam, et mineralium matrix propter mineralia quae in ipsa occultantur, et tamen aer volatilis, et caelum [sulphureum] in quo astra revolvuntur continet in centro suo, quod centrum astrale est & terram ad usque superficiem illuminat. Introit. apert p. 10.

Philalethes: Chaos etenim Nostrum est quasi Mineralis Terra, coagulationis suae respectu, & tamen aer volatilis, intra quod est Coelum Philosophorum in Centro suo, quod Centrum est revera Astrale, irradians Terram ad usque superficiem suo jubare.

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ROBERT MAYER AND THE CONSERVATION OF MATTER

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Robert Mayer (1814-1878) is well known as one of the discoverers of the principle of the conservation of energy. A physician from the kingdom of Württemberg in southwestern Germany, Mayer sailed to the Dutch East Indies in 1840, where he was startled by the brighter-than-expected color of the blood he let from European sailors recently arrived in the tropics. Reflecting on the implications of Lavoisier’s combustion theory of animal heat, and of his own failed childhood attempt to construct a perpetual motion machine, Mayer employed a widely invoked analogy between organisms and machines to conclude that there must be a constant numerical relationship

between heat and "motion" (1). Two years later he published his first paper, "Remarks on the Forces of Inorganic Nature", in Liebig's *Annalen der Chemie und Pharmacie*. That paper contained his calculation of the mechanical equivalent of heat; as he put it, the fall of a given weight through a distance of 365 meters is equivalent to a rise in temperature of one degree Celsius of an equal weight of water.

Although this essay cannot develop the full context of Mayer's reflections, it is important to recognize that the line of reasoning sketched above yielded Mayer a number, not a general concept of energy (he said "Kraft", or force), let alone of its conservation. Of central importance in the exposition of his theory of force was an analogy between force and matter - the fundamental concepts, he argued, of (respectively) physics and chemistry. Both force and matter are quantitatively invariable and qualitatively variable. Just as matter is, in the normal course of nature, neither creatable nor destructible, so, too, is force neither creatable nor destructible, at least as far as the processes of inorganic nature are concerned. It is thus tempting to suspect that Mayer transferred to the new conceptual entity "force" the well-known chemical principle of the conservation of matter.

The only thing wrong with this explanation is that there was no such principle in the chemistry and physics texts of the first half of the 19th century, at least not in Germany. The substance of the principle was, to be sure, tacitly assumed as a working principle by chemists, but it had no visibility as a fundamental principle, let alone the fundamental principle upon which the science of chemistry is based. In a real sense Mayer had to "discover" for himself the principle we know of as the conservation of matter. Nor was it a simple business for him to settle upon the analogy between force and matter. He first had to define for himself the meaning of force and to decide what its fundamental characteristics were. It was only gradually and with great conceptual difficulty that Mayer concluded that force, too, cannot be created or destroyed under any circumstances. He never doubted the indestructibility and uncreatability of matter, but whether those characteristics apply also to force was precisely the difficult question to answer. A confused application of the parallelogram of forces to central-force motion, coupled with residual uncertainty as to the status of the vital force and attachment to a widely invoked image of the solar system as an "organism," led him to conclude until around 1844 that force is continuously produced in the sun via the neutralization of a portion of the planets' centrifugal force. In other words "organisms," unlike machines, are truly capable of creating force effectively out of nothing.

The nonexistence of an explicit principle of the conservation of matter is as unambiguous as it is startling. The overwhelming majority of German chemistry and physics texts and handbooks of the period contain no explicit mention of anything like the conservation, indestructibility, or uncreatability of matter, mass, or substance. One needn't quibble



Robert Mayer

over terms. I've looked at about 30 works by 17 authors from the 1820s till the early 1840s, including the well-known texts of Baumgartner, Berzelius, Biot, Leopold Gmelin, Kastner, Mitscherlich, Pouillet, and Wöhler. Most list as the general properties of matter things like extension, impenetrability, divisibility, porosity, elasticity, compressibility, inertia, and motility, rarely also weight or gravity ("Schwere"). Some don't even mention mass as an important concept, let alone its conservation.

One of the standard reference works of the day was *Gehler's Physical Dictionary*, published in 11 multipart volumes between 1825 and 1845. Neither the short article on "Mass" nor the long one on "Matter" specifically mentioned its conservation or indestructibility (2). To be sure, passing mention of such a principle did occur here and there in the course of particular discussions, quite as Lavoisier's oft-quoted enunciation of it was tucked away in the section of his *Traité* dealing with vinous fermentation. Thus Lamé referred to "the indestructibility of matter and the constancy of the quantity of vis viva" in his discussion of the constancy of the quantity of heat in certain reactions, but he otherwise passed in complete silence over the conservation of matter (3). Interestingly enough, the two other incidental references I've encountered to something like the conservation of matter have to do with

reactions involving oxygen - perhaps a faint resonance of the historical association of that principle with Lavoisier.

Such silence is perhaps doubly surprising since not only had Lavoisier enunciated the principle of the conservation of matter in 1789, but in Germany Immanuel Kant had laid down a similar principle in his influential *Metaphysical Foundations of Natural Science* of 1786. For Kant, the first principle of mechanics was that "in all changes in the corporeal world the quantity of matter remains on the whole the same, unincreased and undiminished." Yet Kant also assigned to matter primitive attractive and repulsive forces, and the "dynamical" philosophies of nature which were popular in early 19th-century Germany tended to eliminate matter entirely in favor of its construction out of ontologically more primitive forces; hence there was no matter, let alone mass, to be conserved.

The very notion of the conservation of matter was problematic because of the widespread lack of precision concerning the conception of matter as a distinct entity, especially as it related to the nature of the so-called imponderables, i.e. heat, light, electricity, and magnetism, regarded as weightless fluids. (Recall here that even Lavoisier listed caloric and light among the elements.) One prominent writer, Jacob Friedrich Fries, interpreted ponderable matter and the so-called imponderables as merely different states of aggregation of the same underlying substance (4). This was a notion which held out the implicit possibility of the effective disappearance of ponderable matter and hence cut the ground from under the utility of a principle of the conservation of matter. Heidelberg professor of physics Georg Wilhelm Muncke insisted that the alleged weightlessness of the imponderables had not been proven empirically, and thus he held open the possibility that they were only tenuous states of matter, again blurring the concept of ponderable matter and rendering its conservation less than obvious (5). As he observed, the imponderables would only have to be as light with respect to hydrogen as hydrogen is with respect to platinum in order to escape detection by our most sensitive balances. One of Mayer's professors of medicine at Tübingen, the then-prominent Johann Heinrich Ferdinand Autenrieth, concluded his discussion of phenomena of electricity, galvanism, magnetism, chemical reactions, heat, and light with the judgment that "imponderable substances" differ only in degree from "ordinary heavy bodies: They do not constitute a class of entities wholly different from the other material substances, and between the magnetic fluid and rigid flint there is an almost continuous transition" (6). In other words, it would be hard to insist on the conservation of matter as a principle if one's conception of ponderable matter was such that there was nothing in principle to conserve. It took the clarification of the concept of energy and the abandonment of the time-honored but vague notion of the imponderables before the concept of matter was distinct enough to make its conservation a meaningful principle of science.

At issue is not chemists' routine acceptance (after La-

voisier?) of the fact that the weight of the chemical reagents before and after a reaction must be unchanged, but rather the explicit enunciation of a particular principle and the kinds of assumptions which finally made that enunciation reasonable in ways it hadn't been before. The parallel and explicit formulation of both conservation principles as fundamental principles of the sciences of chemistry and physics was in the first instance the work of Robert Mayer. Lavoisier notwithstanding, it appears to me that, for the larger scientific community, the general recognition of the principle of the conservation of matter went hand in hand with, and was only made possible by the general acceptance of the principle of the conservation of energy during the second half of the 19th century.

References and Notes

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PRIDE AND PREJUDICE IN CHEMISTRY

Chauvinism and the Pursuit of Science

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Imbued as they are with the ideal of scientific objectivity, scientists and their historians can forget or neglect an important truism: scientists are just as susceptible as their fellow human beings to chauvinism, bigotry, greed, ambition, and all the other faults to which humanity is prey. Two news articles published in *Science* in 1989 are relevant to the first sin in my

list. One article describes the concern of members of the chemistry section of the Nobel Prize Committee over the nearly invariable tendency of American chemists to nominate other Americans - in fact, "in the great majority of cases," members of their own departments. The 1988 winners, three Germans, together received nominations from ten countries, but not one from an American chemist (1).

The other article concerns the outcry among French Canadians over a decision to change the name of the Parisian journal *Annales de l'Institut Pasteur: Virology* to *Research in Virology*. The director of the Institut Pasteur defended the decision by pointing out that while about half of their submissions in 1988 were from Francophone countries, nearly all were written in English. One Canadian is quoted as saying that "It is an Anglo-Saxon point of view to say that science is universal and that the language of scientific communication should be English because of that." Another critic argues that the real tragedy is the loss of Pasteur's name from the title, for "Pasteur belongs to the world" (2). The introduction of Pasteur's name into the debate is ironic, for Pasteur himself, an ardent patriot, was involved in a nationalist contretemps. Awarded an honorary doctorate from the University of Bonn, he returned it in anger during the Franco-Prussian War (1870-71), saying that he no longer believed "that science has no country" (3).

He who seeks examples of chauvinist fervor among 19th-century scientists finds a true embarrassment of riches (4). It is my intention here to examine this subject, with the goal of forming a judgment as to the extent to which such an assuredly widespread phenomenon may have been harmful to the successful international pursuit of science. My focus here is not so much on specific international institutions, conventions, congresses, scientific societies, and formal or informal social networks, but rather on the question of how the social-psychological phenomenon of shared prejudices in national groups interacts with the cognitive phenomenon of the growth and transformation of scientific ideas. My case study centers on German views of French chemistry in the 19th century, and especially during the Franco-Prussian War. I will examine the opinions of August Kekulé, who has been accused of Prussian chauvinism (5); more space will be devoted to the views of Kekulé's teacher, Justus Liebig, and his chief tormentor and rival, Hermann Kolbe. I will argue that the prevalence and intensity of chauvinist fervor does not necessarily interfere with the rational and successful pursuit of science.

Kekulé, Hofmann, and Kolbe

August Kekulé, August Wilhelm Hofmann, and Hermann Kolbe, the three premier German chemists in the generation after Liebig, form interesting contrasts, in their personal lives as in their science. Kekulé was cosmopolitan and patrician in style, and was much inclined toward internationalism. After his initial education at Liebig's hands, he enjoyed a four and a



August Kekulé

half year "Wanderjahre," divided among the countries of France, Switzerland, and England, followed by a brief period in Heidelberg - all of this being but a prelude to nine years as a professor in French-speaking Belgium. By the time he was called to Bonn he had spent 13 of the previous 16 years abroad; he could speak English and French almost without accent, and fluent Italian as well. He was also principal organizer of the first international chemical congress. Hofmann, for his part, was likewise a product of Liebig's Giessen laboratory; he then spent 20 happy and productive years in England before returning to Germany by accepting a sumptuous position at the University of Berlin. A suave sophisticate like Kekulé, Hofmann's oral and written English was so masterly that he did not hesitate to correct the language of his English students. Henry Armstrong's thumbnail sketches were apt (6):

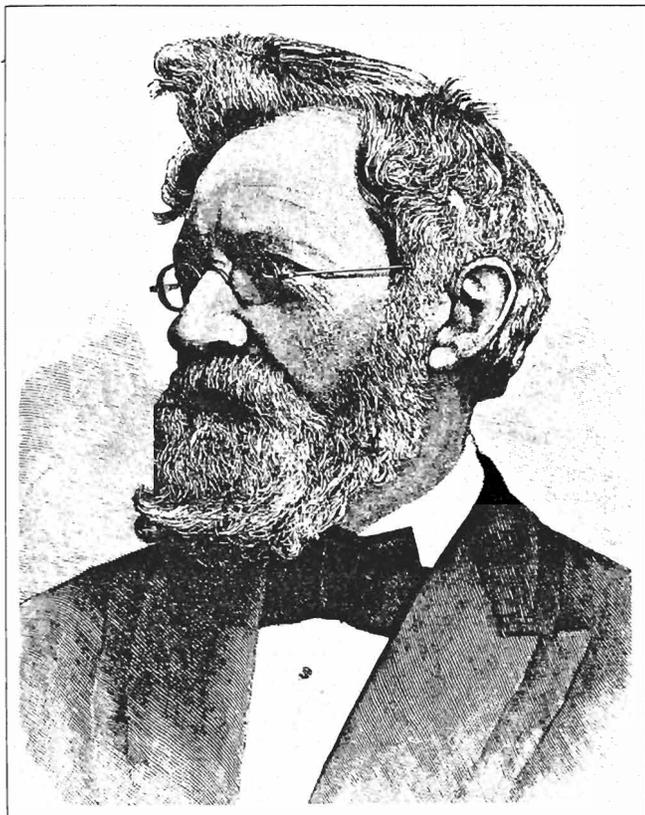
Kekulé was a born aristocrat in manner. An intellectual of a high order, many-sided in his interests, he was too critical and cynical to be a leader of men in the way that Hofmann was, though even superior to him as an orator; he attracted through his clear-cut talent, his gift of precise speech and his great command of knowledge ... Kolbe was equally simple [as Frankland], never a man of the world, a good lecturer and a far better writer but not an orator: the best chemist of

them all. Hofmann and Kekulé were cosmopolitans; ... Kolbe - just the dear old German, academic pedagogue of the highest class: there is no other way of describing him.

Indeed, Kolbe's was a very different character. With the single exception of Jacob Berzelius, whom he considered an honorary countryman, all of Kolbe's models were German - above all, the heroes of the classical period of the rise of German chemistry: Liebig, Friedrich Wöhler and Robert Bunsen. Educated at the hands of the theoretically conservative experimental masters Wöhler and Bunsen, Kolbe served at the Universities of Marburg and Leipzig after a postdoctoral stint in England. He spent his entire career trying to develop and to preserve the radical theory and its electrochemical basis, in the face of the ultimately successful attacks by a French-English reform movement founded by Charles Gerhardt and Auguste Laurent and promoted by Alexander Williamson and Kekulé. Linguistically as well, Kolbe forms a contrast; although he learned a reasonable amount of English in the 18 months he spent in London, he soon forgot most of it, at least as far as oral communication is concerned (7). There is no evidence he ever mastered or even seriously studied any other foreign language. Apparently he could read French, although certainly he avoided doing so as much as possible. As for foreign travel, aside from his one postdoctoral period and a



Hermann Kolbe



August Wilhelm Hofmann

brief laboratory tour to England, a fishing vacation in Norway with his friend Eduard Vieweg, and his semiannual "cures" taken often in Swiss resorts, he never left the German Confederation or Empire. He particularly avoided the Catholic countries of Austria and France.

Kolbe's first recorded derogation of the French dates from the period in 1848 just after the February revolution in Paris and the "March days" in Germany. Certain French chemists, he wrote in an encyclopedia article, had been "irresponsible" in proposing "imagined laws" based on "vague hypotheses" that purported to overturn the radical theory. Similar slurs are found in Kolbe's long paper on radicals published in the fall of 1850 (8). But his language became much sharper when it appeared that the reformers might really carry the day. His concern and anger can be discerned in the first fascicle of his textbook, published in 1854. French chemists, he wrote, were only playing games with formulas, with "unbelievable self-deception." They opposed the (predominantly German) radical theory out of chauvinist spite, since "it had not developed on French soil." There was much more here in a similar vein (9).

Liebig and Dumas

But Berzelius was not Kolbe's only model for ferocious critiques; he also followed the pattern established by his other

great hero, Liebig. Liebig's views of foreign chemistry are best exemplified by examining his relationship with his greatest rival, J. B. Dumas. During the 1830s Liebig and Dumas were contemporaries pursuing the same field of research in different countries, and had much in common - so it was perhaps inevitable that they would become rivals. Both men were demon workers with extraordinarily creative minds, cultivating a field that had too many mysteries and too few facts. Both men had occasion to accuse the other, sometimes justly, of experimental work that was fast but sloppy and both had occasion to accuse the other, probably also sometimes with some truth, of poaching results. As violent as their disputes at times became, by 1840 they found themselves not very far apart - though neither man was then willing to admit this to the other.

In his worst moments Liebig thought of Dumas as a true charlatan or "Schwindler," who was not above using questionable tactics or sleight of hand to achieve renown, and whose greatest concern was pursuit of effect, flourish, and the rhetorical turn of phrase, all for the sake of personal ambition. For his part, Dumas often viewed Liebig as a heavy-handed and hotheaded chemical empire-builder. After a brief alliance at the end of 1837 and the beginning of 1838, Liebig became dissatisfied with the pact he had made with Dumas, and in 1840 made a "total break" from the Frenchman, the quarrel resulting from substitution theories, and based upon some real issues along with some pure misunderstandings. Dumas was a "tightrope dancer," a "Jesuit," a "highwayman," and a "thief," like "nearly all Frenchmen" (10). To Berzelius he complained (11):

These Frenchmen truly have no feeling of true honor, no sense of justice and fairness, they have for many years been occupying themselves with theoretical speculations that are useless for science, and solely to satisfy their own vanity and arrogance; they have discovered that the word Radical must be banned and must be substituted by the word Type. This is the greatest of their discoveries. Unfortunately when I step forward there is in Germany only envy and weakness, so I stand completely alone, no one who has enough power to stand up to them supports me. In short, it is a bad time and I am very unhappy, and have turned from these miserable matters to applications of chemistry to physiology, which now interests me tremendously.

Unfortunately here too Liebig collided with Dumas, when Liebig became convinced in 1842 that Dumas had stolen his original ideas on plant and animal nutrition, and the heat of discord only became more intense (12).

Even in the midst of some of these disputes, however, Liebig was able to recognize Dumas' merits and to concede when he had been in the wrong, and when the violence of his replies sometimes had done nothing but damage (13). On 23 April 1850 Liebig wrote his friend C. F. Kuhlmann in Lille, whom



Justus von Liebig

he was about to visit to help dedicate his new factory; he was very much looking forward to the expectation of seeing Dumas there, as he was anxious to renew their old friendship (14):

... since I have always very highly esteemed Herr Dumas as one of the most outstanding and ingenious men among the chemists and scientists of our day. Perhaps more than any other chemist in Europe I find myself in the position of judging and prizing the value of his work, since we very frequently have encountered each other in our investigations, and have cultivated the same field.

Liebig's hopes for the encounter were realized, as he wrote to Wöhler (15):

We all arrived at the same time, embraced each other, and everything was fine. Dumas was extremely cordial, and looked so young that I hardly recognized him. His wife and daughter were with him, to serve as witnesses to the plans for revenge that he had brewed. On Whitsunday the celebration was splendid and merry, the next evening a banquet, to which the civil and military leaders of Lille were invited. At the end of the banquet Dumas stood up, gave a long speech, flattered me with various puffery, and finally took a decoration for the legion d'honneur from his pocket, and handed it to me along with the brevet in the name of the President of the French Republic. I was unprepared and thought I would faint; but I managed a speech and received an accolade. Thus he revenged himself on me. Despite all he has a magnificent nature.

The following year Liebig dedicated a new edition of his *Chemische Briefe* to Dumas, and the two exchanged a number of warm letters until Liebig's death in 1873.

Kolbe certainly absorbed an extremely negative view of Dumas from Liebig, who was one of his idols and models, and whose diatribes were quite open and often published in the scientific literature. Berzelius, and his former student Wöhler, also had opinions of Dumas and other French chemists which were not much more positive than Liebig's. But Dumas had retreated around 1840 from a leading theoretical role, replaced in the theoretical dialectic by such chemists as Laurent and Gerhardt, and in the 1850s by Adolphe Wurtz as well. Kolbe's relationships with Gerhardt and Wurtz paralleled Liebig's relations with Dumas, except for the lack of a final reconciliation. It was with Gerhardt and Wurtz that Kolbe felt the strongest sense of rivalry, enmity, hatred - and once in a while even affinity, if not regard.

Chemistry: A French or German Science?

Kolbe's prejudices against foreigners, especially the French, were not necessarily tied to conservative political sentiments. Kolbe's general political orientation during his 30s was quite typical of his class and time period, namely center to center-right liberalism. During his years at Marburg he had nothing but contempt for the reactionary regime governing the state of Electoral Hesse. He vaguely distrusted Prussia but despised Austria; feared republicans, extreme democrats, and socialists; and he hoped for German unification, presumably under Prussian leadership but with constitutional guarantees. He looked with deep suspicion on Bismarck's and King Wilhelm's struggles of the 1860s with the Prussian "Landtag." When in the spring of 1866 war with Austria threatened, Kolbe (with most fellow Germans) feared a catastrophe, for it was by no means clear that the Prussian army was sufficient to the task, and the Austrian yoke promised to be infinitely more onerous than that of Prussia. "Lieber Bismarckisch (so schlimm das auch ist)," commented Kolbe to Edward Frankland about the alternative outcomes of the approaching war, "als österreichisch-Jesuitisch!" Moreover, Saxony (where Kolbe had moved as a result of his call to the University of Leipzig) was ominously sandwiched between Prussia and Austria, and everyone expected the battle zone to be close to Leipzig (16).

In the event, the decisive battle occurred at Sadowa (Königgrätz), 200 miles southeast of Leipzig, and was handily won by the Prussian army. Kolbe's sentiments, again like those of most of his countrymen, were profoundly altered by this military success and by the prospect of a unified German nation. "Say what you like against Bismarck," Kolbe wrote Frankland, "one cannot deny that he is a decisive, quietly reflective man, *the premier statesman of Europe*" (17):

The situation is perhaps the following. Had Austria won the upper hand and destroyed Prussia, Germany would be lost and we would have Austrian conditions: lies, Jesuitism, concordat, systematic corruption, general moral disintegration, destruction of material prosper-

ity, abolition of free scientific research, etc. With the battle of Königgrätz a new star rose over Germany; from this day Germany is a unified nation. Further, our political, material, moral and scientific development will receive a new impetus.

In short, Kolbe was convinced that "Prussia's victory signifies freedom and free development in every direction."

Kolbe's long-simmering hatreds burst into the public domain at the time of the Franco-Prussian War. The decline of his influence in theoretical chemistry, along with his general isolation in the collegial community, must have increased Kolbe's ill temper, and after January 1870 he had his own journal to express his unexpurgated opinions. The war, along with the uproar over Wurtz' opening of his recently published history of chemistry, proclaiming that "chemistry is a French science," provided the occasion for his outbursts (18). In a polemical article "On the State of Chemistry in France" published simultaneously with the French declaration of war (and obviously modeled on Liebig's identically-titled essays on Prussian and Austrian chemistry), Kolbe lambasted the French for their dissolute ways and their feeble scientific establishment. There is *no* French university, he declared, that can compare with *any* German university for chemical education (19).

As the war proceeded Kolbe was even further radicalized. He was delighted by the Prussian victories at Sedan and Metz, but impatiently abided the long siege of Paris; he did not



Jean-Baptiste Dumas

understand why Moltke held off on the bombardment for so long (20). To Franz Varrentrapp he wrote (21):

The French are truly a nation of half children, half madmen. I have had deep hatred and contempt for the French, but I had never considered them so uncivilized, barbarous and base as we now see them to be. I believe France is now in a rapid decline, and will never recover ... The whole nation puts no value at all on honor, only on gloire [sic].

The sharpest contrast in this respect could be drawn between the French and the Germans, Kolbe thought, as he wrote to Frankland (22):

The Germans, who seek their gloire [sic] in the arts of peace, and go to war only as a last resort, would never sacrifice their sons to the whim of anyone, even if a narrow-minded, fanatical, bellicose German emperor should one day accede to the throne. In our country the only kind of war that will be popular and possible is one that defends the fatherland.

Frankland ought therefore have no fear of future German aggression. Furthermore, Kolbe bristled at Frankland's sentiments in favor of a republic, for the example of the United States illustrates that a republic is no more than "a playground for swindlers and adventurers, on which the insolent mediocrity bring their influence to bear, a language in whose dictionary the word 'gentleman' does not appear ... My dear friend, for heaven's sake no republic." The Germans, like the English, Kolbe concluded, would rather have a German king than an emperor, and not one from Prussia; "aber die Nothwendigkeit hat eiseme Arme," and he and his compatriots were delighted with their new situation (22).

When the French Academy of Sciences neglected to remove from the wrapper of their *Comptes rendus* mention of the Alsatian cities of Strasbourg and Mulhouse, and Metz in Lorraine, after their transfer to Germany, Kolbe was enraged (23). He wrote Liebig (24):

My contempt for the whole contemporary French chemical world is beginning more and more to turn into pity. Even the Parisian Academy appears to have no idea how ridiculous it appears to the scholarly world by this miserable bickering, for which Herr Pasteur constituted the ferment. Forgive my exhortation. The behavior of this lost and lying nation sometimes makes me a little passionate.

But Liebig's was a sympathetic ear. The French, Liebig complained, were displaying "insane arrogance," demonstrating that they were a "dissolute race;" the "megalomania of this unfortunate nation is certainly capable of anything" (25). "How terrible it must be for this vain and arrogant nation to have achieved *not a single* advantage in battle" (26). Bismarck's adroit behind-the-scenes manipulations maneuvering

both countries toward crisis had been essentially invisible to the German public, and the war propaganda was skillful. Even Kekulé was induced to denounce the "nation of scoundrels" they were fighting (27).

Emotions began to cool, at least on the German side, after peace was concluded, but Kolbe kept up the heat, continuing his Francophobic polemics for more than two years. Having been elected, with Liebig, Wöhler, and Bunsen, a charter honorary member of the German Chemical Society, Kolbe resigned in 1871 out of anger that the Society had not defended his critique of Wurtz's dictum when that critique had met public foreign opposition. Meanwhile Kekulé, together with Jacob Volhard and Emil Erlenmeyer, successfully persuaded the Society to become less provincial; among other reforms suggested by this group, after 1872 the Society only named foreigners as Honorary Members. But to Kolbe the Society had already been far too internationally oriented (28).

Hofmann, who very much wished to soothe the raw feelings between the two countries, picked up the cue at this point, proposing Auguste Cahours as the first Frenchman to receive such an honorary membership after the war ended. This was the last straw for Kolbe, who protested loudly, both publicly and privately (but without effect, partly because he had now resigned). In his journal he asserted that there were "dozens" of more deserving Germans. "What a disgrace," he wrote Varrentrapp, "again with Cahours; what is the purpose of this international coquetting with France? Hofmann unfortunately lost the fatherland in England" (29).

Kolbe's tone became even harsher in his final years, after the death of his wife, when he became truly irrationally preoccupied with his various crusades. Ironically, the French were far less oriented toward structure theory than the Germans; Kolbe noticed this fact with alarm, for to him it indicated a surprising source of French strength that was dangerous for the future health of the German chemical community. "I know full well," he wrote Volhard (30):

... that if Prussia continues to ruin chemistry ... the time will soon return when, as in the second decade of this century, German chemists will go to Paris to educate themselves in chemistry. As at that time, when everyone in Germany was crazy about the Naturphilosophie of Hegel and Schelling, this swindle made no headway in France, and for that very reason France was far superior to us in science, so today, with the single exception of Wurtz, French chemists keep away from the Naturphilosophische swindle of the modern structural and bonding chemistry, and therefore they will gain a head start on us once more.

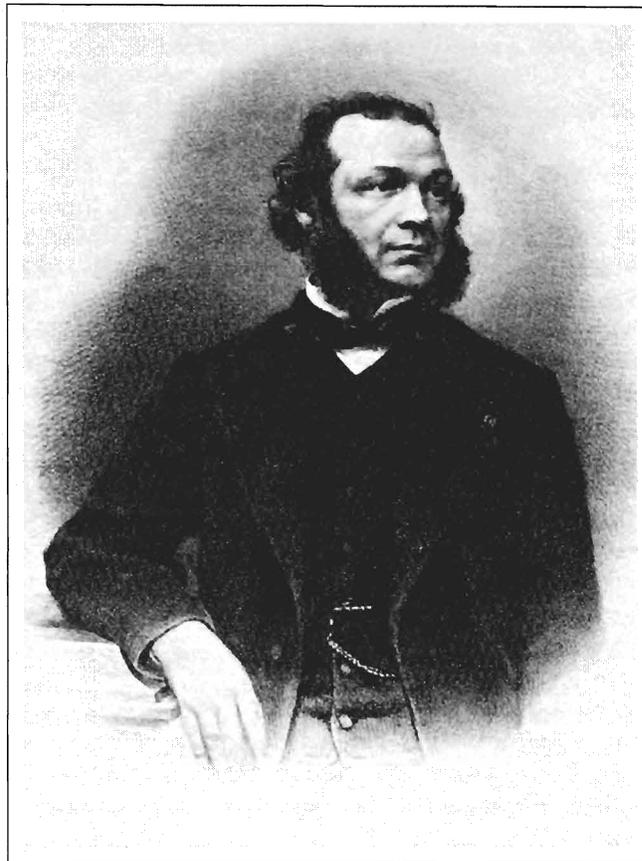
The irony was, as Kolbe well knew and loved to point out, that this same unscientific structural chemistry was a direct product of French chemistry - namely an outgrowth of the type theories of Dumas, Laurent, Gerhardt, and Wurtz. Kolbe thought this was where Kekulé had gone wrong; he had followed not only the bankrupt theories of the French, but also their larcenous

behavior. The more highly Kekulé's textbook was valued, the more Kolbe railed against the "tendentious forgeries" committed by its author (31).

Despite Kolbe's quirkiness, he saw a number of points quite clearly. Kekulé was indeed an internationalist at heart, and he had been decisively influenced by the French chemists Dumas, Laurent, Gerhardt, and Wurtz. He and other (predominantly German and German-influenced) chemists - such as Erlenmeyer, Crum Brown, Frankland, Ladenburg, Butlerov, Baeyer, Fischer, Victor Meyer, Graebe, and Wislicenus - had developed structural chemistry from that essentially French background. Kolbe was also correct in viewing Kekulé and Wurtz as flawed historians, for the latter did have hidden agendas in mind, and neglected the very real contributions of those they disagreed with - especially Kolbe, Frankland, and Couper. Finally, Kolbe was right to see Wurtz as one of the few prominent representatives of structural chemistry in France.

Indeed, Wurtz' isolation in France was sort of a mirror image of Kolbe's in Germany, placing the contretemps over his chauvinist historical comment in even sharper relief. Read with attention to the thematic orientation of the entire work, and placed in context with Wurtz' other interpretive, historical and polemical writings of the 1860s, the apparently gratuitous chauvinism of his opening motto is subject to a different, or at least additional interpretation. Wurtz had accepted essential parts of the Gerhardtian reform in 1853; by 1858 he was a full and enthusiastic convert. But continued opposition among his colleagues led him, rather isolated in France, to initiate a concerted campaign for the new chemistry, including structural ideas. He started a new journal (*Répertoire de chimie pure*) and a new society (Société Chimique de Paris), became a leader, with Kekulé, of the Karlsruhe Congress organizers, wrote a heavily subtitled éloge for Gerhardt and Laurent, presented invited historical lectures to the Société Chimique, the Collège de France and the Chemical Society of London, wrote a textbook, and finally published a full, formal history prefacing a multi-volume dictionary. All were designed to propagate the new chemistry in a country still dominated by older ideas. None was notably successful (32).

I want to suggest, in short, that Wurtz' "chemistry is a French science" has a thematic load heavier than mere chauvinism. It was not so much Lavoisier and the first chemical revolution that Wurtz wanted to promote, as Lavoisier's countrymen Laurent and Gerhardt (not to mention of course Wurtz himself, aided by foreign Francophiles such as Williamson and Kekulé) who were the authors of the still incompletely consummated "second" revolution. The work was directed inward rather than outward, its intended audience was Wurtz's fellow Frenchmen. What better way to persuade them to join the new movement than to appeal to their patriotism by arguing for the continued dominance of French chemistry in the international arena? If I am right, we have here an example of nationalism put to rhetorical purposes, but for a cognitive goal - and not for



Adolphe Wurtz

mere chauvinist puffery. But it was difficult for foreigners to get past that first fearsome line.

Kekulé practiced the same technique. His 1859 history of chemical theory, prefacing his textbook, had a number of significant omissions. As was the case with Wurtz, these were partly due to selfish priority interests, and chauvinism may have also played a role; but there was also a rational didactic or rhetorical intent promoted by the distortions. He had a new theory to push, and needed to tell the history behind it in such a way as to make the theory appear rational, even inevitable. The work of Kolbe and Frankland in particular failed to fit into the neat story Kekulé wanted to tell. This historical-didactic technique was of course very old and well-attested (33). It had been practiced with particular skill by Lavoisier himself. Somewhat devious and covert (or perhaps self-deluding) such a procedure may be - but chauvinism was only at best a secondary motive.

The historical work of Hermann Kopp, a close friend to Kolbe, Hofmann, and Liebig, forms a sharp contrast to Kekulé's and Wurtz' partisan histories. Despite having been commissioned to write a history of chemistry "in Germany," moreover just at the time of the Franco-Prussian War and in the immediate aftermath of Wurtz' apparent chauvinism, Kopp's *Ent-*

wicklung der Chemie in der neueren Zeit was aggressively and explicitly international in orientation. The case of Kopp is sufficient to show that chauvinist currents were by no means all-pervading, even during the most jingoistic of times (34).

Concluding Observations

The somewhat optimistic interpretation to which such considerations lead - that chauvinism in science is perhaps less damaging than has hitherto been thought - can be further supported by looking again at some of the characters we have already met. Liebig, for instance, exhibited prominent elements of Francophilia as well as Francophobia, and not only because his first rigorous scientific education took place in Paris. His biographers emphasized his international outlook, which was often in evidence (35). As the war with France progressed, Liebig expressed compassion and concern for his French colleagues, some of whom were good friends. In September 1870 Liebig wrote his brother-in-law, the army physician Karl Thiersch, then with the Prussians in Versailles, requesting (36):

... that he might seek out Regnault and offer him his help. I wonder how our friends in Paris, Dumas, Peligot, Boussingault, etc. are doing? If only it were possible to do something for them, but they will not be allowed out of Paris. The lovely city, what suffering she faces!

Through Thiersch, Liebig succeeded in getting a letter to Deville in Paris from his wife, a refugee in Geneva. He sent 500 francs to L. C. Barreswil's wife in Boulogne, under the presumption that she needed it; he considered the same charity for Madame Deville (37).

In the first meeting of the Bavarian Academy of Sciences after cessation of hostilities, Liebig delivered a speech assessing the causes of Prussian victory and French defeat. He suggested that German superiority was an indirect but very real consequence of wise governmental policies that, inter alia, gave sufficient support to academic research, which led in the long term to efficacious "scientific" rather than mere rote applications. He concluded with some comments that he intended as conciliatory (38):

This is perhaps the place openly to acknowledge, on the part of our Academy, that racial hatred between the Germanic peoples and the Romanic countries does not exist ... It is characteristic in the nature of the German, with his knowledge of languages, his understanding of foreign nationalities, and his cultural-historical standpoint, to be just to other peoples, often to the point of being unjust to himself; and so we recognize what we owe to the great philosophers, mathematicians, and scientists of France, who have been our mentors and models in so many fields. Forty-eight years ago I came to Paris to study chemistry; ... my entire career was thereby determined.

Indeed, Liebig, like Kekulé, had begun his career as a Francophile, showing nothing but contempt for his previous German teachers. He always revered his French mentors Arago, Dulong, Thenard, and above all Gay-Lussac. He subsequently formed an exceedingly close relation with J. T. Pelouze and others, spoke and wrote French fluently, and until his death kept in close contact with the leading figures of the Parisian establishment. In 1845 he wrote Wöhler: "Indeed, Frenchmen have something exceptionally appealing and amiable that is generally missing from the Germans" (39). As we have seen, he successfully reconciled with Dumas. Even Gerhardt, whom he accused publicly of being an assassin and a highwayman, eventually managed to elicit kind and generous comments from his former teacher, and became fully reconciled before his death in 1856.

Liebig concluded his speech (38):

A warm sympathy for all that is noble and great and an unselfish hospitality are among the finest traits of the French character; these features will be rekindled and reactivated on the neutral ground of science, on which the best minds of the two nations must meet in their endeavors toward the high goal common to both; thus will the ineradicable feeling of brotherhood gradually contribute in the field of science to soothe the bitterness that the deeply wounded French national pride feels toward Germany, as a result of the war which they forced upon us.

Partisan emotion was clearly showing through here, but we must grant that Liebig's heart was in the right place, and at a difficult time for German as well as French hearts.

It may be noted parenthetically that Liebig's relations with English chemists were also very close. Despite disparaging comments on English dilettantism and their lack of attention to pure science, and a public attack on the idol of English experimentalism, Francis Bacon, Liebig's high regard for English chemists and his continuous collegial contact with them has prompted one prominent English Liebig scholar to refer to Liebig quite justly as "very much an honorary Englishman" (40).

In sum, there is no evidence that Liebig was prey to the sort of pathological national prejudice that might have chronically interfered with his appreciation of foreigners' work, and thus with his pursuit of science. None of this is to deny a certain hot-headed and instinctual chauvinism at the heart of Liebig's character, but the judgment of one historian that "Liebig was the undisputed champion of this growing and squalid German nationalism in scientific affairs" is quite unjust (41).

Many would want to award such championship honors to Hermann Kolbe, and in truth it would be hard to find a better candidate. And yet, close examination of Kolbe's career reveals an interesting irony. No one had more contempt for the French or their theories in the late 1840s and early 1850s than Kolbe. However, the striking new reactions and brilliant argu-

ments by Gerhardt, Williamson, Wurtz, and Frankland during the early 1850s that convinced most of Kolbe's German colleagues to accept the French-English theories were by no means lost on Kolbe either. By 1857 he had developed a theory of his own that was strikingly similar to the Williamson-Gerhardt newer type theory, namely that all common organic compounds could be regarded as substitution products of carbonic acid. He retained this theory almost without modification for the rest of his life.

Colleagues, friends and rivals all pointed out, from the late 1850s until Kolbe's death, both publicly and privately, that Kolbe had become a de facto convert to Gerhardt's system. Kolbe denied it with all the energy at his command. There were indeed some substantive distinctions between what Kolbe called his own "real types" and the purely "formal types" of Gerhardt's theory, and between his hierarchical radical formulas based on tetravalent carbon and the structural formulas of the Kekulé school. But the similarities were striking, both to Kolbe's contemporaries and to modern observers. In 1868, two years before the war broke out, Kolbe even converted to modern atomic weight formulas, the last highly visible difference between him and the structuralists - a step that most French chemists did not take for another quarter century.

To put the matter a bit simplistically, Kolbe's pathological chauvinism had failed to prevent him from understanding and being persuaded by the hated French ideas; it had only operated to prevent him from believing he had adopted them. Using his faux types during his most productive years in the 1860s, Kolbe practiced substantively and very successfully the same sort of theoretical chemistry being pursued simultaneously by the structuralists. In short, to the extent that he was an exceptionally good scientist - and there can be little doubt that he was - he was also an internationalist in spite of himself. It would be wrong to suggest that Kolbe's bigotry did not damage the quality of his science, for I believe it is clear that it did, especially after 1870. But what is striking is that a man of such violent and ineradicable prejudices against the very direction that we have come to know as modern chemistry was able essentially to become a modern chemist in spite of himself.

I would not want to push my argument too far, for there are well known instances where national feelings seriously damaged the free interplay of scientific ideas. Just as French chauvinism played a role in delaying for half a century the reception of Newtonian mechanics in a Parisian context dominated by Cartesian ideas, so English chauvinism influenced the retention in Cambridge and Oxford for more than a century of the Newtonian calculus, in preference to the more powerful Leibnizian version. Governmentally enforced pseudoscientific orthodoxies in the Soviet Union and in Nazi Germany, based partly on chauvinist emotion, threatened virtually to destroy certain branches of science deemed foreign and hence maleficent. And yet even here it is significant that the most powerful totalitarian states have great difficulty in

enforcing such dicta from above. Stalin's suppression of genetics required wholesale murder; Hitler could drive many of the finest physicists from Germany, but even so his campaign to purge German science of the "Jewish" theories of relativity and quantum mechanics was largely unsuccessful (42).

Returning to the field of chemistry, the reception of French antiphlogistic chemistry in the homeland of Stahl provides another interesting case. Karl Hufbauer has shown that during this episode German chemists were strongly conditioned by the cultural nationalism then being promoted so ardently by German romantic writers. Still, Hufbauer demonstrates that Lavoisier's new chemistry conquered Germany nearly completely, despite strenuous (and openly chauvinistic) initial opposition, during the course of only four years, 1789-1793 - moreover, just a few years after French chemists themselves were converted (43). More recently, H. G. Schneider has examined the same events; his emphasis on the outspoken nationalism of the principals only underlines (to my mind) the irony of their relatively fast capitulation to the hated French chemistry. A similar course of events took place when the chemical atomism of Dalton and Berzelius encountered German soil. Despite a romantic antimaterialist culture flirting with Naturphilosophie and other dynamical idealist notions, which one would think ought to have provided an inimical climate of opinion, chemical atoms flourished in Germany as they had elsewhere (44).

Similarly, and despite all of the examples of expressions of virulent German chauvinism given here, in the event the Germans accepted the French-English chemical reforms of the 1850s astonishingly rapidly. In fact, it is a striking irony that these essentially French reforms were pursued much more aggressively and enthusiastically in Germany than in France: by the 1860s structure theory had become a quintessentially German field, while Wurtz felt his to be a voice in the French wilderness.

In sum, the prevalence of nationalist fervor provides much less predictive guidance in explaining the growth, development, and differential national reception of scientific theories than one might have expected. Chauvinism is powerful and pervasive, but so is the strength of ideas and evidence as pursued by conscientious (though very human) scientists.

References and Notes

Author's Note and Acknowledgments: A companion piece to this essay is "Pride and Prejudice in Chemistry: Kolbe, Hofmann, and German Antisemitism," to be published in Y. Rabkin and I. Robinson, eds., *The Intersection of Jewish and Scientific Cultures*. The latter was actually presented at the Benfey Festschrift session in 1989, and both essays are dedicated to O. T. Benfey. Portions of this essay have also appeared in my book, *The Quiet Revolution: Hermann Kolbe and the Science of Organic Chemistry*, University of California Press,

1993. For their kind assistance and permission to quote from documents in their possession, I wish to thank the staffs of the Archives of the Académie des Sciences, Paris, the Royal Society of Chemistry, London, the Bayerische Staatsbibliothek and the Library of the Deutsches Museum, Munich; also Mr. and Mrs. Raven Frankland, owners of the Frankland letters cited, and Professor Colin Russell, whose microfilm copies I used; and finally, the professional staff of the Vieweg Verlag archive, Wiesbaden. This research was generously supported by the National Endowment for the Humanities, grant RH-20801-88.

1. C. Holden, "Chauvinism in Nobel Nominations," *Science*, **1989**, 243, 471.
2. D. Dickson, "L'Affair Pasteur' Prompts Canadian Outcry," *Science*, **1989**, 244, 280-281.
3. For discussions of this event see G. Geison, "Louis Pasteur," in C. Gillispie, ed., *Dictionary of Scientific Biography*, Vol. 10, Scribner's, New York, 1974, pp. 350-416 (on p. 354), and J. Wotiz and S. Rudofsky, "Louis Pasteur, August Kekulé, and the Franco-Prussian War," *J. Chem. Educ.*, **1988**, 6, 34-36.
4. A convenient discussion and guide to the existing secondary literature on nationalism and internationalism in science is provided in two articles by B. Schroeder-Gudehus: "Science, Technology and Foreign Policy," in I. Spiegel-Rösing and D. Price, eds., *Science, Technology and Society: A Cross-Disciplinary Perspective*, Sage, London, 1977, pp. 473-506; and "Nationalism and Internationalism," in R. C. Olby, G. N. Cantor, J. R. R. Christie, and M. J. S. Hodge, eds., *Companion to the History of Modern Science*, Routledge, London, 1990, pp. 909-19. Not mentioned by Schroeder-Gudehus is E. Crawford, *Nationalism and Internationalism in Science, 1880-1939*, Cambridge, 1992, Chapt. 2; and on the topic of chemistry in particular, see also C. Meinel, "Nationalismus und Internationalismus in der Chemie des 19. Jahrhunderts," in P. Dilg, ed., *Perspektiven der Pharmaziegeschichte*, Akademische Druck- und Verlagsanstalt, Graz, 1983, pp. 225-43. Meinel's and my treatments of this material are essentially complementary; Schroeder-Gudehus is more strongly influenced by the viewpoint of the social construction of scientific knowledge than are either Meinel or myself.
5. J. Wotiz and S. Rudofsky, "The Unknown Kekulé," in J. G. Traynham, ed., *Essays on the History of Organic Chemistry*, Louisiana State University, Baton Rouge, 1987, pp. 21-34 (on 30-31).
6. H. E. Armstrong, "The Doctrine of Atomic Valency," *Nature*, **1930**, 125, 807-10, on 808-9.
7. Edward Frankland began to give Kolbe English in return for German lessons soon after their arrival in London, and Kolbe could "soon speak with facility" (Frankland to Hermann Ost, 20 December 1884, Sondersammlungen des Deutschen Museums, Munich [hereafter abbreviated "SSDM"], document number 3576). However, by 1864 Kolbe reported to Frankland that he had forgotten so much that he needed to be allowed to speak German in presenting a lecture to the Chemical Society (Kolbe to Frankland, 4 December 1864, microfilm frame number 01.04.59, Frankland archive, Milton Keynes, England). The lecture was never given.
8. H. Kolbe, "Formeln, chemische," in *Handwörterbuch der reinen und angewandten Chemie*, Vol. 3, Vieweg, Braunschweig, 1848, pp. 174-78, on 176; idem, "On the Chemical Constitution and Nature of Organic Radicals," *J. Chem. Soc.*, **1850**, 3, 369-405, and **1851**, 4, 41-79, on 76.
9. H. Kolbe, *Ausführliches Lehrbuch der Organischen Chemie*, Vol. 1, Vieweg, Braunschweig, 1854, pp. 35, 40-41.
10. Liebig to Berzelius, 8 May 1831, 2 July 1832, and 17 May 1841, in J. Carrière, ed., *Berzelius und Liebig: Ihre Briefe von 1831-1845*, Lehmann, Munich, 1898, pp. 11, 34, 230.
11. Liebig to Berzelius, 17 April 1841, *ibid.*, p. 223.
12. For a summary of this dispute, see F. L. Holmes, "Justus Liebig," in C. Gillispie, ed., *Dictionary of Scientific Biography*, Vol. 8, Scribner's, New York, 1973, pp. 333-44, and idem, *Claude Bernard and Animal Chemistry*, Harvard University Press, Cambridge, 1974, pp. 34-47.
13. Liebig to Berzelius, 30 May 1833, 14 September 1833 and 31 December 1834, in reference 10, pp. 62, 71, 99. "The most maddening thing is," he wrote in the latter letter, "somewhat upset by the oxamide business, in my paper on the constitution of ether I permitted myself some expressions of a personal nature against Dumas, which I should not have done ... The devil take these accursed affairs." Even earlier (28 December 1831, *ibid.*, p. 25) Liebig expressed great contrition over a published critique of some work of O. B. Kühn: "I will write no more critiques as long as I live," he vowed.
14. Liebig to [C. F. Kuhlmann], 23 April 1850, Archives of the Académie des Sciences, Paris (Dossier Liebig in the Fonds Dumas).
15. *Ibid.* (additional letters from Liebig to Dumas); reference 10, pp. 276-78; Liebig to Wöhler, 1 June 1850, in A. W. Hofmann, ed., *Aus Justus Liebig's und Friedrich Wöhler's Briefwechsel in den Jahren 1829-1873*, 2 Vols., Vieweg, Braunschweig, 1888, in Vol. 1, pp. 352-53.
16. Kolbe to Vieweg, 21 July and 31 December 1862, 19 October 1863, 31 December 1864, and 18 March, 5 and 16 May 1866, Vieweg Verlag archive, Wiesbaden, Kolbe file (hereafter abbreviated "VA"), letters numbered 184, 187, 196, 213, 242, 243, and 245. The passage quoted is from Kolbe to Frankland, 27 May 1866, frame 01.02.1558, Frankland archive.
17. Kolbe to Frankland, 23 July 1866, frame 01.02.1505, Frankland archive; Kolbe expressed similar sentiments in his letters to Vieweg, 9 and 22 July 1866, VA 246 and 247.
18. A. Wurtz, "Histoire des doctrines chimiques depuis Lavoisier," in A. Wurtz, ed., *Dictionnaire de chimie pure et appliquée*, 3 Vols. in 5, Hachette, Paris, 1868-78, volume 1, pp. i-xciv, on p. i (republished monographically in 1869).
19. H. Kolbe, "Über den Zustand der Chemie in Frankreich," *J. prakt. Chem.*, **1870**, 110, 173-83.
20. Kolbe to Liebig, 12 November 1870 and 2 December 1870, Bayerische Staatsbibliothek, Munich, Liebigiana IIB, Kolbe letters nos. 30 and 31.
21. "Die Franzosen sind doch wirklich eine Nation von halb Kindern, halb Wahnsinnigern. Ich habe in mir tiefen Hass und Verachtung gegen die Franzosen gehabt, aber für so entcivilisirt roh und gemein, wie man sie jetzt kennen gelernt hat, hatte ich sie doch

nicht gehalten. Ich glaube, Frankreich ist stark im Untergange begriffen, und kommt nie wieder auf einen grünen Zweig ... Ehre hat überhaupt bei der ganzen Nation keinen Werth, nur gloire." Kolbe to Franz Varrentrapp, 26 February 1871, VA 267.

22. Kolbe to Frankland, 26 December 1870, frame 01.04.645, Frankland archive. Frankland had regretted to see that "there is far too much Gottes Gnaden in [Wilhelm's] nature," and predicted that "in form & constitution the German Despotism will be worse than the French;" still, he thought that in the results of the war "the rest of the world will be greatly benefitted ... unless indeed (which is not likely) the German people, excited by victory, turn to be a warlike, instead of a peaceful people" (Frankland to Kolbe, 23 December 1870, SSDM 3564).

23. H. Kolbe, "Haltung der Pariser Akademie der Wissenschaften," *J. prak. Chem.*, 1872, 113, 225-26; idem, "Chemischer Rückblick auf das Jahr 1872," *ibid.*, 1873, 114, 461-70. See also Kolbe's letters to Frankland of 18 March 1872 (Frankland Archive, frame 01.02.948), and to Liebig of 4 April 1872 (see next note).

24. "Meine Verachtung gegen die ganze heutige französische Chemikerwelt fängt mehr und mehr an, sich in Mitleiden zu verwandeln. Auch die Pariser Akademie [sic] scheint gar keine Ahnung davon zu haben, wie lächerlich sie sich von der Gelehrtenwelt durch diese erbärmlichen Zankereien macht, wozu Herr Pasteur das Ferment bildete. Verzeihen Sie meine Expectoration. Das Gebahren dieser verlogenen und verlorenen Nation macht mich zuweilen etwas leidenschaftlich." Kolbe to Liebig, 4 April 1872, Bayerische Staatsbibliothek, Liebigiana IIB, Kolbe letter no. 35. For the allusion to Pasteur, see above, note 3.

25. Liebig to Emma Muspratt, 27 September 1870, H. E. Roscoe Collection, Royal Society of Chemistry, London ("Die Franzosen bei ihrer wahnsinnigen Eitelkeit ..." will surely lose the war); Liebig to Wöhler, 25 September 1870, in Hofmann, reference 15, vol. 2, p. 299 ("Die Franzosen haben sich als ein so verrottetes Geschlecht gezeigt ..."); Liebig to Kolbe, 2 October 1870, SSDM 3614 ("Der Grössenwahn Sinn dieser unglücklichen Nation ist freilich zu allem fähig").

26. Liebig to Wöhler, August 1870, in Hofmann, reference 15, vol. 2, p. 295.

27. "Hundevolk, diese Franzosen," Kekulé to Hans Hübner, 15 July 1870, August-Kekulé-Sammlung, Institut für Organische Chemie, Technische Hochschule, Darmstadt; cited in Wotiz and Rudofsky, reference 5, n. 6, p. 31. Wotiz and Rudofsky use the phrase "sons of bitches" to translate the German "Hundevolk." Here they have committed the common error of preferring a more literal to a connotatively more accurate translation. In fact, "Hundevolk" is undocumented in the German language. "Hunde-" is simply a negatively intensifying prefix, and has none of the connotations of profanity that the English expression "son of a bitch" has.

28. H. Kolbe, "Chemischer Rückblick," reference 24, pp. 465-66.

29. "Welche Blamage wieder mit Cahours, was soll nur das internationale Cokettiren mit Frankreich? Hofmann ist leider in England das Vaterland abhanden gekommen." Kolbe to Varrentrapp, 22 January 1873, VA 304.

30. "... ich verhehle mir nicht, dass wenn Preussen so fortfährt, die

Chemie zu ruinieren ... so wird bald die Zeit wiederkehren, wo, wie im zweiten Jahrzehnt dieses Jahrhunderts, die deutschen Chemiker, um sich in der Chemie auszubilden, nach Paris gehen. Wie damals, wo in Deutschland alles sich für Hegel-Schellingsche Naturphilosophie begeisterte, dieser Schwindel in Frankreich gar keinen Boden fand und Frankreich eben dadurch in der Wissenschaft uns weit überragte, so halten sich auch jetzt die französischen Chemiker, mit alleiniger Ausnahme von Wurtz, von dem naturphilosophischen Schwindel der modernen Struktur- und Bindungsschemie frei, und sie werden dadurch einen Vorsprung vor uns gewinnen." Kolbe to Volhard, 9 June 1876, SSDM 3681.

31. "... die tendenziösen Fälschungen des Sachverhaltes, welche Kekulé in seinem unverdient hochangesehenen Lehrbuch, das in Wirklichkeit ein liederliches Machwerk voller Unwahrheiten und bewusster Entstellungen ist ..." Kolbe to H. Vieweg, 6 November 1882, VA 482.

32. A. Wurtz, "Eloge de Laurent et de Gerhardt," *Moniteur scientifique*, 1862, 4, 482-513 (also an offprint separate); "Histoire générale des glycols," in Société Chimique de Paris, *Leçons de chimie professées en 1860*, Hachette, Paris, 1861, pp. 101-39; "On Oxide of Ethylene, Considered as a Link between Organic and Mineral Chemistry," *J. Chem. Soc.*, 1862, 15, 387-406; *Leçons de chimie professées en 1863*, Hachette, Paris, 1864; identical to *Leçons de philosophie chimique*, same publ. and date); *Cours de philosophie chimique*, privately printed, Paris, 1864; *Leçons élémentaire de chimie moderne*, Masson, Paris, 1867-68; *Dictionnaire de chimie pure et appliquée*.

33. See L. Graham, W. Lepenies, and P. Weingart, eds., *The Functions and Uses of Disciplinary Histories*, Reidel, Dordrecht, 1983. For the discipline of chemistry, see also J. Weyer, *Chemiegeschichte von Wiegleb (1790) bis Partington (1970)*, Gerstenberg, Hildesheim, 1974; and C. A. Russell, "'Rude and Disgraceful Beginnings': A View of History of Chemistry from the Nineteenth Century," *Brit. J. Hist. Sci.*, 1988, 21, 273-94, on pp. 288-94, who has some additional apposite examples.

34. H. Kopp, *Entwicklung der Chemie in der neueren Zeit*, Oldenbourg, Munich, 1873. The work was published in three parts, beginning in 1871. A discussion of this work in a Kolbean context is my "'Between Two Stools': Kopp, Kolbe, and the History of Chemistry," *Bull. Hist. Chem.*, 1990, 7, 19-24.

35. See, for example, J. Volhard, *Justus von Liebig*, 2 Vols., Barth, Leipzig, 1909, vol. 2, pp. 418-22.

36. Liebig to Wöhler, 30 September 1870, in Hofmann, *Briefwechsel*, reference 15, Vol. 2, p. 300.

37. Liebig to Wöhler, 7 December 1870, in *ibid.*, p. 304.

38. Liebig, "Eröffnungsworte ... nach dem Friedensschluss," 28 March 1871, in *Reden und Abhandlungen*, Winter, Leipzig and Heidelberg, 1874, pp. 331-33; excerpted by Volhard, reference 36, 420-22.

39. Liebig to Wöhler, 24 May 1845, in Hofmann, *Briefwechsel*, reference 15, Vol. 1, p. 257.

40. W. H. Brock, "Liebig, Wöhler, Hofmann: An English Perspective," in W. Lewicki, ed., *Wöhler und Liebig: Briefe von 1829-1873*,

Cromm, Göttingen, 1982, pp. xvi-xviii (this is a photographic one-volume republication, with new front matter, of Hofmann's edition of the Liebig-Wöhler correspondence).

41. S. Kapoor, "Jean-Baptiste Dumas," in C. Gillispie, ed., *Dictionary of Scientific Biography*, Vol. 4, Scribner's, New York, 1971, 242-48 (on p. 243).

42. On which see L. R. Graham, *Science and Philosophy in the Soviet Union*, Knopf, New York, 1972; and A. D. Beyerchen, *Scientists Under Hitler: Politics and the Physics Community in the Third Reich*, Yale University Press, New Haven, 1977.

43. K. Hufbauer, *The Formation of the German Chemical Community (1720-1795)*, University of California Press, Berkeley, 1982, pp. 96-144.

44. H. G. Schneider, "The 'Fatherland of Chemistry': Early Nationalistic Currents in Late Eighteenth Century German Chemistry," *Ambix*, 1989, 36, 14-21; A. J. Rocke, "The Reception of Chemical Atomism in Germany," *Isis*, 1979, 70, 519-36.

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THE CHEMISTS' WAR

The Impact of World War I on the American Chemical Profession

David J. Rhees, The Bakken Library and Museum

World War I was one of those momentous and horrifying events in American history that permanently reoriented, even revolutionized, American society. Indeed, it is difficult for us today to imagine the profound shock experienced by Americans in general and chemists in particular upon the outbreak of the war with Germany - that most scientific of all nations - in August 1914. Various known as the European War, the Kaiser's War, the Great War, the Great Crusade, and, of course, the Chemists' War, it was a major turning point in Western civilization, marking the actual, if not the chronological divide between the Victorian world of the 19th century and the modern world of the 20th - a divide, a fault line, that was simultaneously social, political, economic, cultural, and moral.

In the standard accounts of the history of American science, however, World War I is usually overshadowed by its even more destructive successor. Understandably, the development of radar, the synthetic rubber project, and the Manhattan

Project have captured the lion's share of historians' attention. I certainly would not dispute the importance of the Second World War in giving rise to Big Science, characterized by large-scale team research, close relations with industry, and heavy reliance upon government (especially military) funding. Nevertheless, I would like to suggest that insufficient attention has been paid to the importance of the First World War in terms of its impact upon the scientific profession, particularly the chemical profession. After all, chemistry played an extremely important role in the production of high explosives, poison gas, optical glass, synthetic coal-tar dyes and pharmaceuticals, and other chemical products of direct or indirect military value.

Although historians of science and technology are more or less familiar with how chemistry changed the war, relatively little is known about how the war changed chemistry (or, more precisely, the chemical profession), and it is the latter which constitutes the subject of this paper. Even though the United States was involved in the Great War for only 18 months (from April 1917 to November 1918), I wish to argue that it affected the American chemical community in five important ways:

1. *Industrialization*: The war greatly accelerated the growth of the American chemical industry, thus enhancing the financial and ideological importance of industry to the chemical profession.

2. *Militarization*: The war resulted in the development of strong ties between the chemical profession and the military establishment.

3. *Politicization*: The war jolted chemists out of their ivory-tower, laissez-faire mentality and led them to engage in aggressive political lobbying for the first time.

4. *Nationalization*: The war stimulated a surge of patriotism in the chemical community which helped build morale and pride in the achievements of American chemistry, but which at times degenerated into strident nationalism and nativism.

5. *Popularization*: The war engendered a new self-consciousness among chemists and a new awareness of their public image which led to a vigorous campaign to popularize chemistry.

Before I proceed to discuss these five trends, a few qualifications are in order. First, this analysis can only suggest the broad lines of change and is intended to be suggestive rather than comprehensive. This is particularly true of my necessarily brief discussion of the role of chemistry in the war, which, of course, is fundamental to any understanding of the impact of the war on chemistry. Secondly, I do not wish to overemphasize the importance of the war, for nearly all of the five trends I have identified had their origins in the prewar era. My point is simply that the war dramatically and decisively accelerated the pace of these trends. And third, I do not wish to imply that other scientific disciplines played trivial roles in the war. The important work of American physicists on submarine detection devices and of psychologists on Army "intelligence" tests, to cite but two examples, are well known. Nonetheless, of all

the sciences involved in World War I, chemistry played the dominant role and this was publicly recognized almost from the beginning of the conflict by the fact that it became known as a "chemical war" or "chemists' war."

Industrialization

What were the industrial contributions of chemistry during the war and how did the industrialization of chemistry affect the chemical profession? Very briefly, the war triggered a major boom in the American chemical industry and in the industrial demand for chemists even before Americans joined the conflict in April 1917. It did this in two principal ways. First, it increased demand for munitions and other chemicals needed in the war effort. The manufacture of TNT, for example, the most important explosive of the war, rose from 3.4 million pounds in 1913 to a rate of 16 million pounds per month in 1916. Similar growth was reported in chlorine, potash, and coal-tar dyes and pharmaceuticals. Secondly, the British Navy placed an embargo on trade with Germany, which had supplied many chemicals to America, thus opening a golden window of opportunity for domestic manufacturers. In the case of coal-tar dyes, for instance, the U.S. was importing about 90% from Germany when the war broke out. When the supply of German dyes was cut by the naval blockade, a "dye famine" resulted.

In response to the embargo of German imports and the booming demand for munitions and other chemicals, many American companies rapidly expanded the manufacture of existing products and initiated production of new chemicals, notably dyes. Before the war, American dye companies simply assembled finished dyes from intermediates supplied by Germany. In 1917, however, DuPont began construction of a complete coal-tar dye plant at Deepwater, New Jersey. As one observer put it, the war "touched off the wildest explosion of chemical activity this country had ever seen" (1).

As chemical production boomed, the need for chemists boomed as well, especially as manufacturers diversified into "high tech" areas such as synthetic organic dyes, drugs, and plastics. From 1914 to 1920, DuPont increased its staff of chemists from 40 to about 300. The boom in chemical research spread throughout other industries as well: from 1916 to 1920 more than 200 industrial research laboratories were founded, and chemists played the dominant role in the new research organizations.

As more and more chemists with advanced degrees moved into industry, efforts to strengthen the relations between industry and the chemical profession were accelerated. Industrial fellowships modeled on those given by Pittsburgh's Mellon Institute of Industrial Research (founded in 1913) were established at half a dozen universities even during the disruptions of the war, and companies such as DuPont established their own fellowship programs as well. At the American Chemical Society, a committee on university-industry relations was

established in 1916 which sponsored symposia on ways to improve the application of chemistry to industrial needs. The pages of the Society's *Journal of Industrial and Engineering Chemistry* were filled with exhortations for greater cooperation between pure and applied chemists, and in 1918 the ACS elected as its president William H. Nichols, president of the General Chemical Company. Although both of Nichols' predecessors were academics - Charles H. Herty of the University of North Carolina (1915-16) and Julius Stieglitz of the University of Chicago (1917) - they, too, were quite sympathetic to industry. Even chemists who had reputations for sneering at industrial chemistry, such as Ira Remsen of Johns Hopkins University, felt obliged during the war to make public declarations supporting the application of chemistry to industry.

The Great War stimulated chemical production, expanded the industrial demand for chemists, and strengthened the links between academic chemists and industry. It also served to legitimize industrial chemistry and to raise the status of the industrial chemist both professionally and publicly. The Chemists' War thus helped make chemistry's role in industrial progress the dominant theme of the professional ideology and public image of the chemical profession.

Militarization

When we speak of World War I as the Chemists' War, the image that usually comes to mind is the famous battle near the Belgian town of Ypres (sometimes referred to as the "Battle of Wipers"), where on 22 April 1915 the German army released a greenish-yellow cloud of chlorine gas on Allied troops. This was the first use of chemical warfare on the Western Front, and though the battle lasted only 15 minutes, it produced over 7,000 casualties and 5,000 deaths. At this point in time, of course, there were no American troops in Europe, it being another two years before the U.S. would enter the conflict. Indeed, it was not until the spring of 1917 that the U.S. began to organize its chemical warfare research program under the initial direction of the Bureau of Mines. That program, which was eventually folded into the Army's Chemical Warfare Service, established in June 1918, was a massive project. As historian of science Daniel Jones has argued, the U.S. gas research program was "the largest of the government sponsored research organizations of the war" (2). In spirit, if not in scale, it presaged the Manhattan Project of World War II.

The gas research program was centered at the so-called "Experimental Station" located at American University in suburban Washington, DC. By the end of the war, 60 buildings had sprouted on the American University campus, and about 1,000 technical personnel (mostly chemists) were employed there. Many of these chemists were actually inducted into the Army after the Army took over the program from the Bureau of Mines. By the end of the war in November 1918, 5,400

chemists were serving in the Armed Forces, and one-third of all American chemists were serving, or had served, in some agency of the federal government.

What was the impact of the crash program in chemical warfare research and development? Initially, many chemists were concerned about the impact of military control on their research, worrying that bureaucratic red tape and a scientifically naive military brass would impede scientific progress. They objected strenuously to the Army's campaign to take control of the gas research program from the Bureau of Mines, and even the *New York Times* implored President Woodrow Wilson not to burden chemists with "the military harness, which they could not help finding uncongenial and embarrassing" (3). Indeed, Roger Adams of the University of Illinois

found it quite irritating that he was required to participate in military drills every afternoon.

However, these kinds of problems were gradually worked out and the chemists' worst fears about military control proved unfounded. Their participation in the crash research program, with

its patriotic sense of life-or-death urgency, soon brought them much closer to their military colleagues. The ACS, for example, quickly established a Committee to Cooperate with the Chemical Warfare Service, and the *Journal of Industrial and Engineering Chemistry* established a special section on "Contributions from the Chemical Warfare Service." During the demobilization period after the war, the ACS would lobby successfully to prevent the Chemical Warfare Service from being dismantled.

The gas research effort also had the effect of bringing academic and industrial chemists closer together. For instance, chemists such as Roger Adams, working at the Experimental Station at American University, received their first hands-on experience in problem-oriented team research and the technical challenges of large-scale manufacturing. This

helped break down the "class barriers" between the pure and applied chemists, thus reinforcing the industrialization trend noted above. Roger Adams also observed that the gas research program resulted in the formation of a network of friendships between chemists from geographically diverse areas. This encouraged an unprecedented degree of unity and cooperation in the American chemical profession which continued into the postwar era, helping create a new sense of disciplinary identity and solidarity.

And finally, the war helped lay the basis for the mobilization of science during World War II. Roger Adams, for instance, went on to play a leading role in organizing the scientific community during the next war, as did James Bryant Conant, who supervised the production of mustard gas at the Edgewood

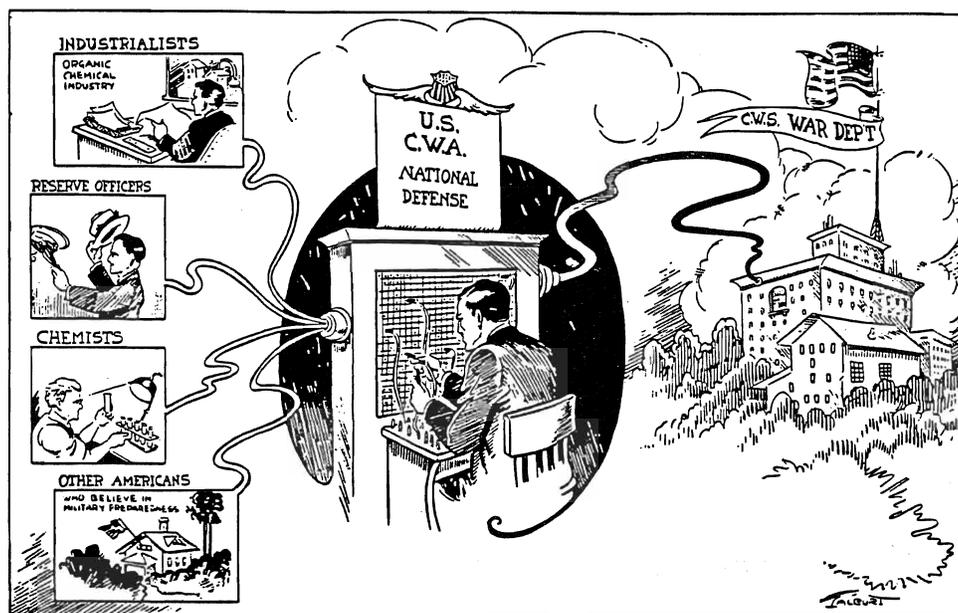
Arsenal in Maryland. Conant, who became president of Harvard University in 1933, headed the National Defense Research Committee during the Second World War.

Just as the war opened a new era of cooperation between chemists and industry, it also established closer ties between the

chemical profession and the military, gave national defense an important place in the professional ideology and public image of chemistry, enhanced the respect of academic chemists for industrial problems, helped unify the profession, and served as a dress rehearsal for World War II (4).

Politicization

And just as the war brought chemists closer to industry and to the military, it also brought them into a new relationship with politics and politicians. Before the war, the ACS had rarely involved itself in matters of national policy. Science, after all, was supposed to be above politics. The war, however, forced chemists to come down from the ivory tower and plunge into the hurly-burly world of lobbying and log-rolling.



This illustration shows the network of groups who lobbied successfully against Senate ratification of the Geneva Protocol, an international treaty that called for a ban on chemical warfare. From *U.S. Chemical Warfare Association Bulletin*, 1925, No. 14 (13 May).

Why did the chemists shed the traditional apolitical ideology of science? First of all, it was not because they were eager for federal funds, which is one of the principal reasons why scientists today become involved in national politics. (The ACS did briefly consider the notion of federal funding of research, and even formed a committee to investigate this subject in 1918, but enthusiasm for the idea seemed to evaporate with the return to normalcy.) Rather, chemists became involved in politics primarily because of their desire to promote and protect the American chemical industry. This desire, in turn, rested on two motives: their belief that establishing an independent American chemical industry was of vital importance to national welfare, and their awareness that industry provided jobs and research funds. Based on these motives,

whose importance was greatly accentuated by the war, chemists became involved in a number of major legislative campaigns both during and after the war.

Perhaps the best example of the new political activism of the chemical profession was the campaign for tariffs to protect the infant American dye industry against the

expected postwar resumption of German chemical imports. That campaign actually began soon after the guns of August began firing in 1914, when a committee of the New York Section of the American Chemical Society issued a report calling for a considerable increase in the tariff on synthetic dye imports. This report became the basis of a tariff bill introduced to the House of Representatives in December 1915. A number of ACS leaders went to Capitol Hill to lobby for this and a succession of other dye tariff bills which the Congress considered between 1914 and 1922. The Society issued a flurry of resolutions urging protection for the chemical industry and it waged an extensive campaign of popular education, hoping to

bring public pressure to bear on Congress. In 1922 these efforts culminated in the passage of the Fordney-McCumber Tariff Act, which placed high duties on imports of German dyes and medicinals.

Aiding the ACS and the chemical companies in the dye tariff battle was a little known but quite important organization named the Chemical Foundation. The Chemical Foundation was very much a "war baby," being founded in March 1919 to hold the German chemical patents sequestered by the U.S. Alien Property Custodian, Francis P. Garvan, during the war. Garvan became president of this quasi-public corporation, which licensed the German patents to U.S. chemical companies and used the proceeds to fund chemical research. The Foundation gave about three-quarters of a million dollars to the



"David" in this cartoon is Francis P. Garvan, president of the Chemical Foundation, who helped to lead efforts to defend the American chemical industry against German competitors after World War I. The "hyphenated American" refers to German-Americans who were thought to be aiding the "enemy." From *U. S. Chemical Warfare Association Bulletin*, 1925, No. 21 (1 July).

development of chemistry and the chemical industry during the postwar period.

The ACS and the Chemical Foundation collaborated on several other political campaigns after the war, most notably the campaign to establish a "chemo-medical institute." This proposal went through a number of changes during the 1920s and led to the founding of the National Institute of Health in 1930. The ACS, the Chemical Foundation, and a lobbying group called the U.S. Chemical Warfare Association also waged a successful battle to defeat Senate ratification of the Geneva Protocol in the mid-1920s, an international treaty that called for a ban on chemical weapons. (In 1973 the Society

ACS, for instance, in support of its publications program. However, the Foundation also spent millions of dollars in lobbying for dye tariff legislation and for popular literature promoting chemistry and the chemical industry. Through its research, lobbying, and promotional efforts, the Chemical Foundation played a key role in the

reversed its position and the Geneva Protocol was subsequently ratified.)

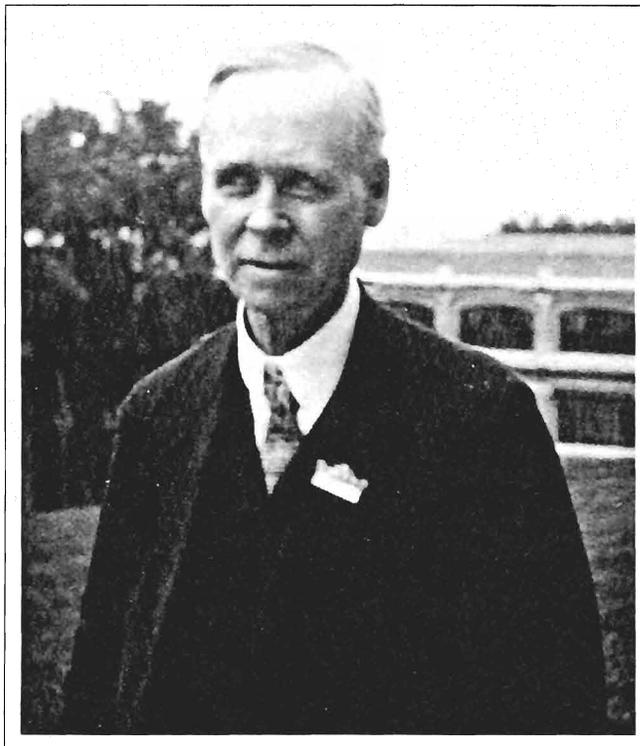
Perhaps the most telling indicator of the politicization of the chemical profession was the gradual transfer of ACS offices to Washington, DC. The move began somewhat by accident in 1912, when the Society's secretary, Charles L. Parsons, moved to Washington to take a position with the Bureau of Mines. In 1921 the offices of the *Journal of Industrial and Engineering Chemistry* (the predecessor of *Chemical & Engineering News*) were moved from New York to the nation's capital, though again, not specifically for political reasons but because the new editor, Harrison E. Howe, was then working for the National Research Council. With the Society's increasing involvement during the Depression in public policy issues such as Prohibition and the revision of food and drug laws, the ACS successfully applied to the Congress in 1937 for a federal charter, which helped strengthen the Society's stature as a national scientific advisory body. The culmination of this trend came in 1941 when the Society moved into its own headquarters building on 16th Street, just a few blocks from the White House (5). Although the war did not by itself bring about the politicization of the chemical profession, it certainly accelerated the process.

Nationalization

Every war generates its own domestic pathologies, and during World War I the forces of nationalism and nativism ran particularly strong. Though one might have hoped that scientists would have resisted such prejudices, chemists proved as susceptible to these forces as any other group. To be sure, there were positive aspects to this trend, for the war enhanced the chemical profession's sense of national identity, encouraged pride in American contributions to chemistry, and inspired efforts to make American chemistry stronger and independent of Germany. It also gave impetus to the study of the history of American chemistry, prompting a search for American patron saints to replace the foreign, especially German, "fathers" of chemical science. Hence the publication of such books as *Chemistry in America* (1919) and *Priestley in America* (1920) by the University of Pennsylvania's Edgar Fahs Smith (ACS president, 1895, 1921-22), and the founding of the American Chemical Society's Division of the History of Chemistry under Smith's and Charles A. Browne's guidance in 1921. Smith's underlying aim was stated in a letter he sent to Charles Herty in 1923 (6):

Be assured, my dear boy, that there is a growing regard for our science in the hearts of many, many people, and we want to put the stamp of Americanism on it so that it can't be effaced.

But nationalism had an uglier aspect as well, degenerating at times into a virulent "100% Americanism," anti-Germanism,



Organic chemist William A. Noyes (1857-1941), a lone voice of moderation during and after World War I.

and anti-Communism. The Chemists' Club in New York, for example, banned the use of the German language and purged its membership of suspected alien sympathizers. In 1918 the ACS revoked the honorary memberships of three prominent German chemists, Emil Fischer, Wilhelm Ostwald, and Walther Nernst. (Cooler heads prevailed after the war, and their memberships were restored in 1926-27) (7). In 1921 the ACS Council also expelled from the Society a chemist named Charles Bramson of Joliet, Illinois, who had distributed propaganda of the United Communist Party (8).

There was, however, at least one voice for peace and reason during this unfortunate phase of the war - that of the organic chemist William A. Noyes. Noyes (1857-1941) edited the *Journal of the American Chemical Society* from 1902 to 1917 and was one of the founders of *Chemical Abstracts*. He built up the chemistry program at the University of Illinois into one of the leading departments in the country, and he served as president of the ACS in 1920. A deeply religious man, a Congregationalist who was raised on an Iowa farm, Noyes had many friends in Germany and was deeply distressed over the bitterness that divided the scientific community during the war. Although he was not a "dyed-in-the-wool pacifist," according to his son, the chemist W. A. Noyes, Jr., he opposed the war and worked diligently to promote international peace, disarmament, and good will, publishing a number of pamphlets on these subjects. The son wrote of his father that "he

did what he could after World War I to minimize nationalism and hatred among scientists" (9).

In 1922, for example, Noyes sent a lengthy letter to the editor of the *Journal of Industrial and Engineering Chemistry* which deplored the rise of nationalistic and capitalistic tendencies after the war, particularly the excessively punitive war reparations which the Allies were imposing on Germany. He even went so far as to argue that "'America First' has become so exactly like 'Deutschland über Alles' that it is hard to distinguish the spirit of the two slogans." This provocative statement earned him a strong rebuke from the well-known food chemist Harvey W. Wiley in a subsequent issue (10). Neither the chemical profession nor the country at large were in a mood to listen to Noyes's call for tolerance.

Popularization

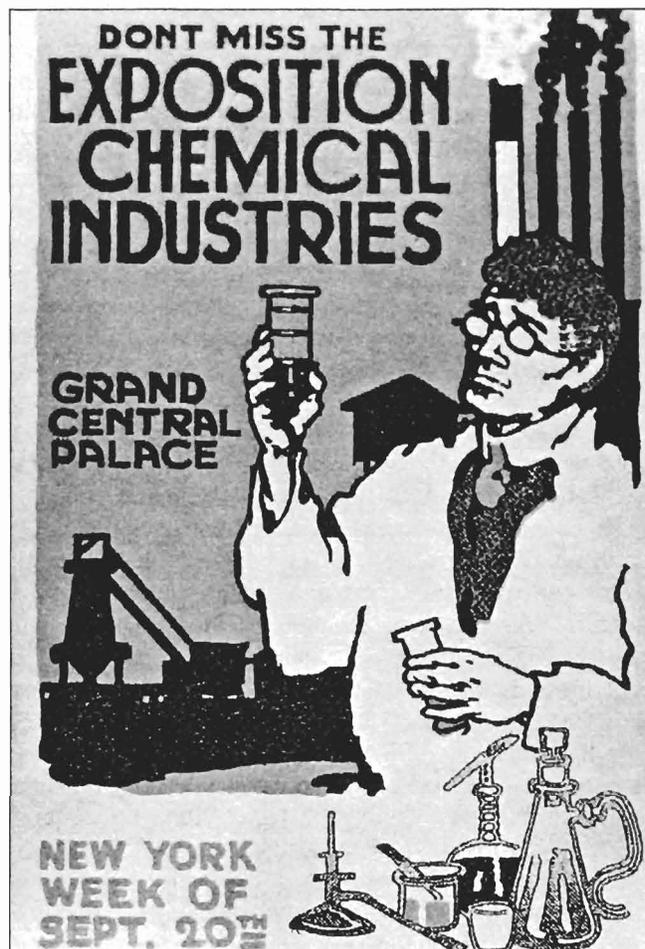
The war changed not only the relations of chemistry with industry, the military, and the Congress, but also with the media and the public in general. Before the war, it is safe to say, the average citizen was scarcely aware that this country possessed a chemical industry until the dye famine demonstrated its deficiencies. Insofar as the chemist had a public image at all, he (rarely she) was generally confused with the druggist, or with that distant ancestor, the alchemist. The chemist was perceived either as an insignificant pill-pusher or a disreputable crank.

The war helped change that image practically overnight due to the publicity generated by the use of high explosives, the dye famine, and chemical warfare. As one chemist rejoiced in April 1915, the public had "discovered" chemistry (11):

Hundreds of newspapers and periodicals are devoting editorial space to the discussion of the chemists and chemical engineers and their relations to the coal-tar industries.

Along with public attention came public misunderstanding. Neither the press nor the public knew much about chemistry, resulting in both uninformed criticisms and wildly unrealistic expectations. To correct these misunderstandings, and to advance some of the political goals mentioned above, leading chemists in the ACS, together with the Chemical Foundation and some of the larger chemical companies, organized a massive crusade to popularize chemistry. Of the many educational activities launched during this campaign, only four can be briefly mentioned here: the National Exposition of Chemical Industries, the ACS News Service, the Chemical Foundation's mass distribution of popular literature, and the ACS Prize Essay Contests.

The first National Exposition of Chemical Industries was held in 1915 in New York City's Grand Central Palace. Though the "Chemical Show," as it was called, was essentially a trade exhibition, it was opened to the general public during



Stamp issued for the first National Exposition of Chemical Industries in 1915. (Courtesy of the Woodruff Library, Emory University)

the war years as a way to win public appreciation for the industrial achievements of chemistry and to show how chemistry was helping win the war. Attendance had reached a high of 128,000 in 1922 when the Exposition's organizers decided to exclude the general public. During the crucial war years, however, the Chemical Show played an important role in making chemistry and chemical industry visible to a wider audience (12).

The American Chemical Society News Service proved to be a more lasting player in the chemical publicity business. Formally established in January 1919, the origins of the News Service date back to the Society's Press and Publicity Committee, appointed in April 1916. It was the first permanent publicity service for the newspapers founded by an American scientific society, and to this day it has been busily engaged in issuing news bulletins about ACS meetings, new discoveries published in ACS journals, and ACS positions on political issues.

Only a few months after the News Service was born, the

Chemical Foundation began operations, as noted above, and in its quest to build public support for dye tariff legislation it began distributing massive quantities of popular literature on chemistry. Francis Garvan chose as the centerpiece for this campaign a book independently authored by Edwin E. Slosson titled *Creative Chemistry*, published in 1919. The Chemical Foundation distributed over 73,000 free copies of Slosson's book to Congressmen, editors, industrialists, women's clubs, and other groups during the final stages of the tariff campaign in 1921. By 1937, when most of its patents had expired and its source of income ran out, the Foundation had published or disseminated a total of eleven million pieces of educational literature.

One of the most successful popular educational projects of the interwar period was a joint endeavor of the Chemical Foundation and the ACS - the Prize Essay Contests. Funded by the Foundation and personal contributions from Francis P. Garvan and his wife, Mabel Brady Garvan, these contests offered cash prizes and scholarships to high school and college students for the best essays on the relations of chemistry to industry, national defense, agriculture, the home, medicine, etc. From 1923 to 1931 more than five million students participated in the contests, which were administered and judged by volunteer ACS members. Although the ACS Prize Essay Contests did lure a few students into pursuing chemical careers, the primary objective was not recruitment but improving the public's opinion of chemistry.

The war thus instilled a missionary mentality in the chemical community which resulted in an ambitious popular crusade. This crusade, in turn, was generally quite successful in enhancing the public image of chemists and the chemical industry and in winning support for dye tariffs, the Chemical Warfare Service, and other public policy issues related to chemistry.

Summary

In this paper I have attempted to suggest a few of the ways in which the Chemists' War deeply affected the chemical profession in the United States. Industrialization, militarization, politicization, nationalization, and popularization - these five trends were decisively accelerated by World War I and introduced a new era in the social and cultural history of American chemistry. By the end of the war, the chemical community was transformed: industry and the military took their place as the profession's most powerful patrons; the ACS took its place in Washington as one of a growing number of professional interest groups; nationalism both energized the profession and caused some severe lapses in judgment; and thousands of chemists were converted into evangelists who enthusiastically spread the chemical gospel to the masses.

No study of the impact of the Chemists' War would be complete, however, without pausing to reflect on its terrible

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Poster for the ACS Prize Essay Contest, circa 1924. (Courtesy of the American Heritage Center, University of Wyoming)

human toll - its "frightfulness," to use the contemporary term for describing the war's horrors. I would not consider myself a true student of Theodor Benfey, whom we honor in this symposium, if I failed to remind myself and my audience that World War I was a global war, an industrial war, a total war, and it was a cruel and brutal affair. While as historians we may find the occasional silver lining even in this depressing conflict, we should not forget that it caused the deaths of nine million people, not a few chemists among them.

The Chemists' War is one of the many burdens of history that we must bear as a nation and as members of the community of scholars and scientists. Although the cause may have been a just one, and though life-saving drugs and other beneficial spin-offs may have resulted, we cannot avoid the fact that chemistry, too, added to the death and destruction. It is well to periodically remind ourselves that we carry such burdens, for only by facing them is there hope that we may someday transcend history and break out of the seemingly endless cycle of war and devastation. By remembering the Chemists' War in all its frightfulness, perhaps we will learn to listen more carefully in the future to those who call, as William A. Noyes called, for peace, reason, and tolerance.

References and Notes

Acknowledgment: The principal documentation and citations for this paper may be found in the author's 1987 University of Pennsylvania doctoral dissertation, "The Chemists' Crusade: The Rise of an Industrial Science in Modern America, 1907-1922." Only direct quotations and new material are referenced below.

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THE HISTORICAL DEVELOPMENT OF THE VAN ARKEL BOND-TYPE TRIANGLE

William B. Jensen, University of Cincinnati

As the biographical sketch by James Bohning in this issue of the *Bulletin* reveals, one of the key events in Ted Benfey's career was his association with Larry Strong at Earlham College and their mutual involvement in the development of the Chemical Bond Approach (CBA) course in the late 1950s and early 1960s (1). CBA was undoubtedly the most innovative of the many attempts at curriculum reform in chemistry which appeared during this period in the United States and elsewhere, and was constructed, as its name implied, around the development of self-consistent models of the chemical bond, starting from a fundamental knowledge of the laws of electrostatics (2). By the end of Chapter 13, the CBA textbook, *Chemical Systems*, had led students through a presentation of the three basic models used to describe the bonding in covalent, metallic, and ionic materials, and had paused for a reflective overview of what had been accomplished up to that point. The finale of this bonding retrospective was a brief discussion of the possibility of intermediate bond-types using the simple triangular diagram shown in figure 1 (3):

Covalent, metallic, and ionic bonds prove to be a useful way of regarding the structures of many substances. These three types of bonds symbolize three different arrangements of atoms to give structures characteristic of particular substances. The underlying principles for the three types of bonds, however, are based on electrostatics in each type. Each substance represents a system of low energy consistent with the limitations imposed by the Pauli exclusion principle and geometrical relations of the electrons and nuclei which are more fundamental units of structure than are atoms.

With the same underlying principles common to all structures, it is not surprising that not all substances can be neatly classified into one of three possible types. The situation can be symbolized by a trigonal diagram [see figure]. The vertices of the triangle represent bond types characteristic of the three extreme bond types. Along each edge of the triangle are represented bond types characteristic of the many substances which do not have extreme bond types.

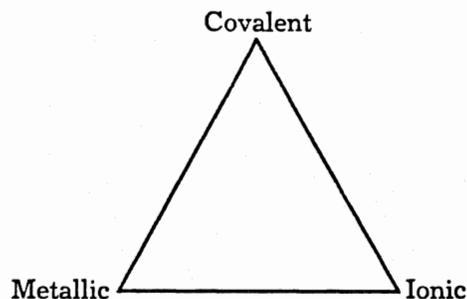


Figure 1. The CBA Bond-Type Triangle

The use of simple, incisive diagrams, like figure 1, lies at the core of effective teaching. Yet, with the exception of the periodic table, most diagrams of this sort appear without acknowledgment in the average chemistry textbook. Their effectiveness rapidly converts them into community property and, like the inventors of controlled fire, the wheel, and metallurgy, their originators appear to be condemned to perpetual anonymity. Given Ted Benfey's interests in both chemical education and the history of chemistry, I thought it might not be improper to honor him by rescuing figure 1 from its "ahistorical" fate, both by tracing its early history and by reviewing some recent extensions of the diagram which have been made since its appearance in the pages of the CBA textbook nearly three decades ago.

The Three-Fold Way

A necessary prerequisite to the development of any diagram purporting to represent the gradual transition between the three idealized limiting-cases of ionic, covalent, and metallic bonding is, of course, an explicit recognition of the existence of the three limiting-case bond types themselves. The first to receive this recognition was the ionic bond, whose essentials were imperfectly anticipated by the German physicist, Hermann von Helmholtz, in his famous Faraday Lecture of 1881 (4). Arguing that Faraday's laws of electrolysis implied that electricity itself was particulate in nature, Helmholtz opted for the two-fluid theory of electricity in which particles of matter could combine with mobile particles of both positive and negative electricity. Neutral atoms contained equal numbers of negative and positive electrical particles, whereas positive and negative ions contained an excess of the corresponding electrical particle. Helmholtz further identified the number of excess electrical particles with the valence of the resulting ion, thus, in effect, postulating that all chemical combination was the result of the electrostatic attraction of oppositely charged ions, and showed that this model was capable of accounting for the magnitude of the energy release observed in typical chemical reactions.

With the exception of the British physicist, Sir Oliver Lodge, few physicists, and even fewer chemists, paid attention to Helmholtz's suggestions until they were revived by J. J. Thomson in conjunction with his ill-fated plum-pudding model of the atom in the period between 1904 and 1907 and reinterpreted in terms of a one-fluid model of electricity in which ionic charge was due to an excess or deficiency of a single mobile negative electrical particle or electron embedded in a nonmobile sphere of positive electrification. Whereas Helmholtz had grafted his electrical particles onto an underlying substratum of classical Newtonian matter, Thomson had reduced matter itself to electricity. In sharp contrast to the low-key reception accorded Helmholtz, Thomson's version of the polar or "electron transfer" model of bonding excited wide-

spread enthusiasm and predictions of an impending chemical revolution (5). In the United States, it led to the development of a polar theory of organic reactivity in the hands of such chemists as Harry Shipley Fry (1908), K. George Falk and John M. Nelson (1909), William A. Noyes (1909), and Julius Stieglitz. In Germany, Richard Abegg (1904) successfully connected it with the periodic table, and in 1916 both the American chemist, Gilbert Newton Lewis, and the German physicist, Walther Kossel, reinterpreted it in terms of Rutherford's 1911 nuclear atom model (6).

Quantitative calculations of heats of reaction using the model were made as early 1894 by Richarz and again in 1895 by Hermann Ebert (7). It was successfully applied to the calculation of crystal lattice energies by Max Born and Alfred Landé in the years 1918-1919 (8) and to the calculation of the energies of coordination complexes by Kossel, A. Magnus, Gustav F. Hüttig, F. J. Garrick and others in the late teens and 1920s (9). Further refinements and applications were made by Kasimir Fajans, Hans Georg Grimm, and Victor Moritz Goldschmidt in the 1920s, culminating in the publication in 1929 of the monograph *Chemische Bindung als Electrostatisch Verschijnself* (*Chemical Bonding as an Electrostatic Phenomenon*) by the Dutch chemists, Anton Eduard van Arkel and Jan Hendrick de Boer (10).

Nonpolar or electron-sharing models of the chemical bond date back to the first decade of the 20th century and the proposals of the German physicist, Johannes Stark (1908), and the German chemist, Hugo Kauffmann (1908). Related models were also suggested by J. J. Thomson (1907), William Ramsay (1908), Niels Bohr (1913), Alfred Parsons (1915) and others (6). However, the overwhelming success of the ionic model and its rapid quantification tended to eclipse these electron-sharing models to such an extent that in 1913 G. N. Lewis felt compelled to write a paper arguing that not only were the physical properties of typical organic compounds incompatible with the ionic model, they also strongly suggested the necessity of a second "nonpolar" bonding mechanism (11). A successful candidate for this nonpolar mechanism was finally provided by Lewis himself in his famous 1916 paper on the electron-pair bond (12). This received widespread attention as a result of its extension and popularization by Irving Langmuir in the period 1919-1921 (Langmuir also introduced the term "covalent bond" in place of Lewis' more cumbersome electron-pair bond) and with the publication in 1923 of Lewis' classic monograph, *Valence and the Structure of Atoms and Molecules* (13).

Beginning in the mid-1920s, qualitative extensions and applications of the covalent bond were made in the field of organic chemistry by the British chemists, Arthur Lapworth, Robert Robinson, Thomas Lowry, and Christopher K. Ingold, and in the field of coordination chemistry by the American chemist, Maurice Huggins, and the British chemist, Nevil Sidgwick (14). Quantification of the model began two years

before the publication of the van Arkel - de Boer book on the ionic model with the advent of wave mechanics and the classic 1927 paper on the H_2 molecule by the German physicists, Walther Heitler and Fritz London (15). However, despite this early start, intensive efforts at quantification of the covalent model really did not get underway until the 1930s and 1940s, via the work of, among others, Linus Pauling, John Slater, Robert Mulliken, Friedrich Hund, and Erich Hückel - or fully a decade after the process was completed for the ionic model. Indeed, these efforts are still a part of the ongoing program in theoretical chemistry.

The initial attraction of both the ionic and covalent models lay in their ability to qualitatively correlate the known compositions and structures of compounds with the number of valence electrons present in the component atoms. These "electron-count correlations" appealed strongly to chemists and are still the basis of much current chemical thought, as witnessed by the more recent development of valence-shell electron-pair repulsion theory (VSEPR) and the current rash of electron-counting rules for cluster species (16). Only after these bonding models had proved capable of qualitatively correlating electron counts with composition and structure for significant classes of compounds did chemists exhibit a further interest in their quantification and in their ability to predict cohesive energies and other properties.

The importance of this observation for our survey lies in its implications for the history of the last of our three limiting-case bond types - the metallic bond - since, to this day, chemists have been unable to uncover a significant pattern governing the composition and structure of intermetallic compounds and alloys (many of which are inherently nonstoichiometric), let alone establish simple electron-count correlations for them (17). The resulting failure to attract the attention of chemists has meant that the development of the metallic bonding model has been left largely to solid-state physicists, who, in turn, have tended to stress the explication of thermal, electrical and optical properties, rather than cohesive energies or patterns of composition and structure. In addition, the models which they have developed for this purpose have tended to have a very different conceptual basis than those employed in the chemical literature and it is fair to say that, even today, such concepts as Brillouin zones and pseudopotentials are not part of the everyday working vocabulary of the average chemist. Given this scenario, it also goes without saying that most historical accounts of the development of the chemical bond have little or nothing to say about the evolution of the metallic bond.

Luckily, however, the question of identifying when chemists first recognized the necessity of a separate metallic bonding model is largely independent of the question of whether they did or did not play a significant role in its historical development. Here, as with so much in the history of the chemical bond, we again encounter G. N. Lewis (figure 2), as the earliest explicit recognition in the chemical literature of the

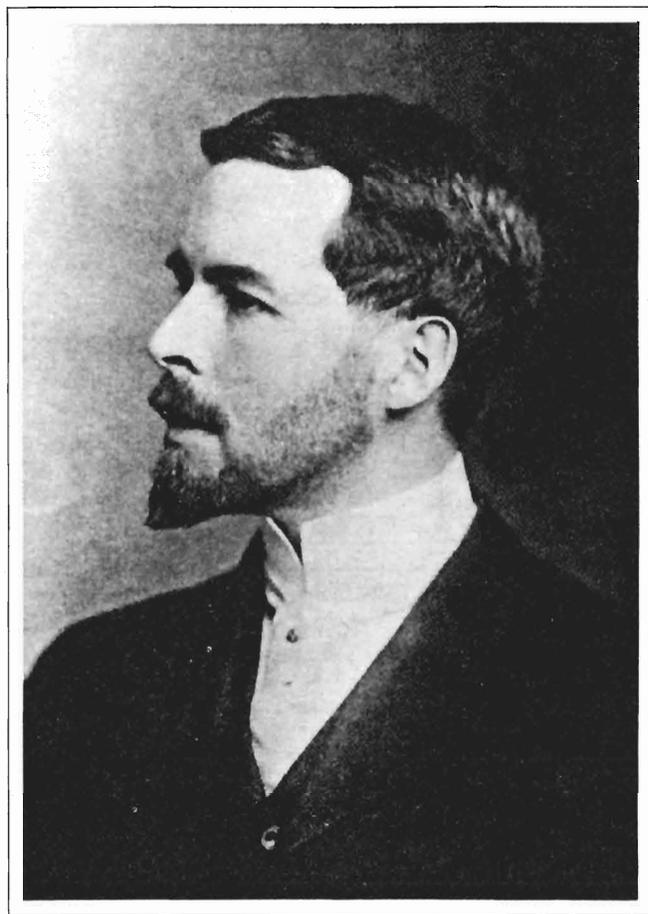


Figure 2. Gilbert Newton Lewis (1875-1946)

necessity of a separate metallic bonding model that I have been able to locate occurs in the same 1913 paper in which Lewis so forcibly argued for the separate existence of the nonpolar or covalent bond. In the final section of this paper, entitled "A Third Type of Chemical Bond," Lewis noted that (11):

To the polar and non-polar types of chemical compound we may add a third, the metallic. In the first type the electrons occupy fixed positions within the atom. In the second type the electrons move freely from atom to atom within the molecule. In the third or metallic type the electron is free to move even outside the molecule ... All known chemical compounds may be grouped in the three classes: non-polar, polar and metallic; except in so far as the same compound may in part or at times fall under two of these groups.

The first attempt to visualize all three bond types (figure 3) appeared two years after Lewis' paper in part three of Johannes Stark's monograph *Prinzipien der Atomdynamik: Die Elektrizität im chemischen Atom* (18). This appears to have been an independent development, since Stark (figure 4) does not cite Lewis. Also recall that, though Lewis argued for the

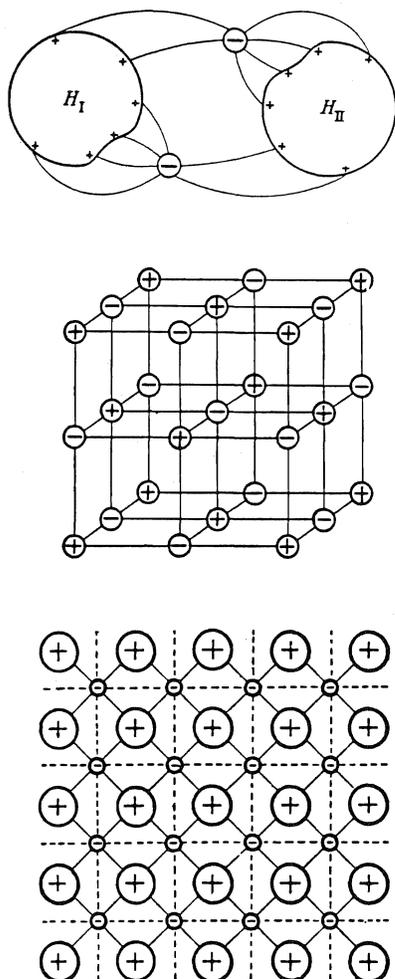


Figure 3. Stark's 1915 representation (from top to bottom) of the shared-electron bond in dihydrogen, the structure of sodium chloride as a lattice of positive and negative ions, and the structure of a metal as a lattice of positive ions and free electrons.

necessity of a nonpolar bond in his 1913 paper, he did not propose a specific model for that bond until 1916, a year after the appearance of Stark's monograph. As already noted, Stark and the German organic chemist, Hugo Kauffmann, had both argued for an electron-sharing model of the nonpolar chemical bond as early as 1908 and, in the case of simple, single-bonded diatomics, had correctly inferred that this bond must correspond to a pair of shared electrons. However, both Kauffmann - who was seduced by the special problems surrounding the bonding in benzene and the theories of partial valence proposed by the German chemist, F. K. Johannes Thiele - and Stark - who elected to follow only the qualitative dictates of classical electrostatics - failed to extend this conclusion in a useful way to more complicated molecules, and opted instead for a wide range of multicentered one-, two- and three-electron

bonds. The final result was far too flexible to allow for meaningful electron-count correlations and it remained for Lewis to take the results of classical valence theory seriously and to successfully develop the consequences of the conclusion that the chemical bond of the 19th-century chemist was "at all times and in all molecules merely a pair of electrons held jointly by two atoms" (13).

A second attempt to visualize Lewis' three bond types, as well as weaker intermolecular attractions, using Bohr's dynamic atom model, was made eight years later by Carl Angelo Knorr in one of the first German papers to describe Lewis' electron-pair bond (figure 5). Like Lewis, Knorr recognized the possibility of transitional bond types and was able to further correlate the various limiting-case models with the growing body of solid-state structural data that had been obtained from X-ray crystallography since the publication of Lewis' original paper (19):

These four extremely different bond types, between which there exist countless transitions and which can be schematically illustrated in the following manner [see figure], also correspond to four different kinds of crystal lattice, namely the ionic lattice (cesium fluoride), the atom

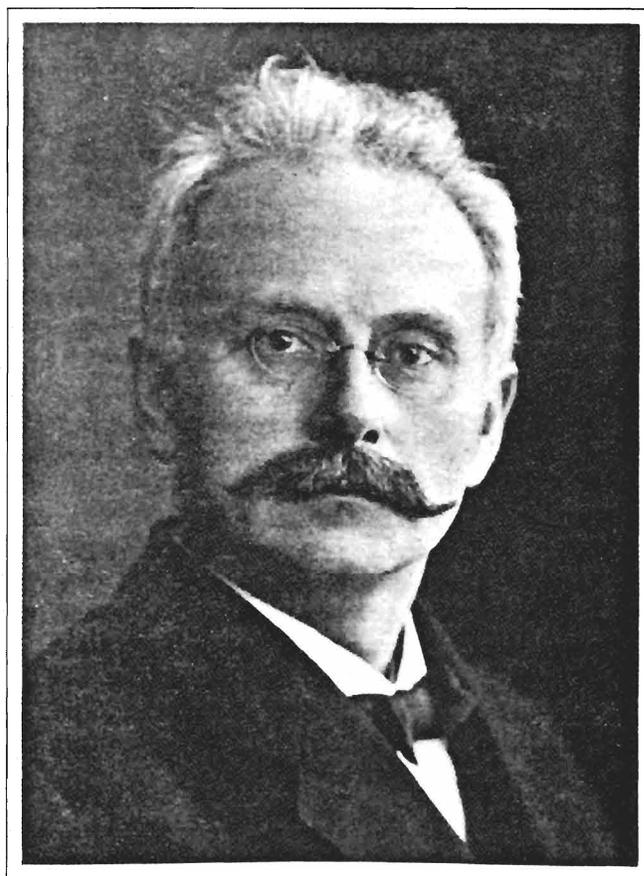


Figure 4. Johannes Stark (1874-1957)

lattice (diamond), the molecular lattice (ice), and the metallic lattice (sodium).

The free-electron model for the metallic bond suggested by Lewis, Stark, and Knorr had, in fact, been around for more than a decade by the time Lewis wrote his paper, having been in-

troduced by the physicists, Paul Drude (1900) and Hendrick Antoon Lorentz (1909), in order to account for the electrical and optical properties of metals (20). Such a model is immediately suggested by the high electrical conductivity of metals and is still invoked in the crude form used by Lewis, Stark, and Knorr in the modern freshman chemistry text, where it is usually coupled with a description of the crystal structures of typical metals. However, the examples used are always simple substances and all mention of the eccentricities of intermetallic compounds and alloys is carefully avoided. Indeed, it is fair to say that in English-speaking countries this topic has never formed a major part of the mainstream chemical literature, having instead been largely consigned to the metallurgical literature. The same, however, does not appear to be true of the German chemical literature, where a concerted effort to establish electron-counting correlations for intermetallic species has remained a part of the province of the inorganic chemist, as exemplified by the significant contributions made by such chemists as Eduard Zintl and Ulrich Dehlinger throughout the 1930s (21). In keeping with this assertion, it is also of interest to note that, despite Lewis' prescience in his 1913 paper, no mention of the metallic bond can be found in either his 1916 paper or in his famous monograph of 1923.

The Grimm-Stillwell Bond-Type Diagram

The first attempt to construct a triangular diagram linking the three limiting-case bond types appears to be that of the German chemist, Hans August Georg Grimm (figure 6), who has already been mentioned in connection with his work on the development of the ionic model (22). Beginning in 1928, Grimm published a series of six articles dealing with the systematization and classification of binary compounds (23-28). In order to trace out the pattern of ionic, covalent, and metallic bonding throughout the periodic table, Grimm constructed both intra-

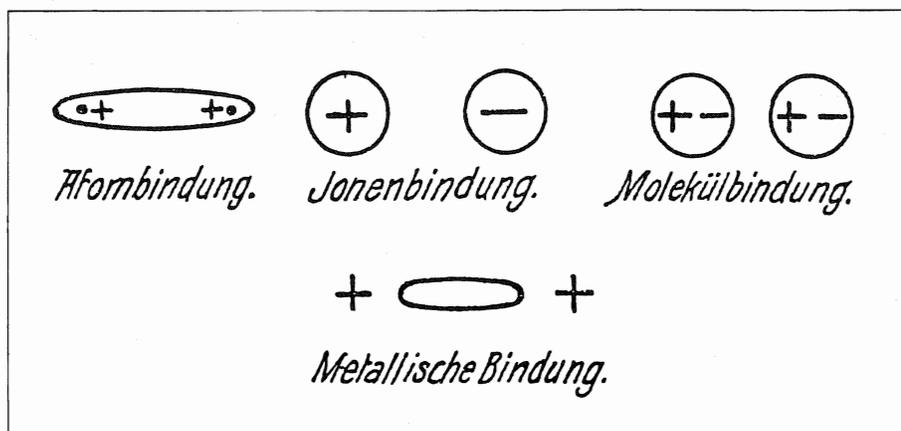


Figure 5. Carl Angelo Knorr's 1923 representation of the three limiting bond types and weak intermolecular attractions (Molekülbindung) in terms of Bohr electron orbits.

and inter-row binary combination matrices for the elements, with the elements placed in order of increasing group number on the *x*-axis and decreasing group number on the *y*-axis (figure 7). Each square of the resulting triangular matrix represented a real or potential

binary compound, whose predominant bonding character was indicated by means of a characteristic cross-hatch pattern. Complete coverage of the entire periodic table required the

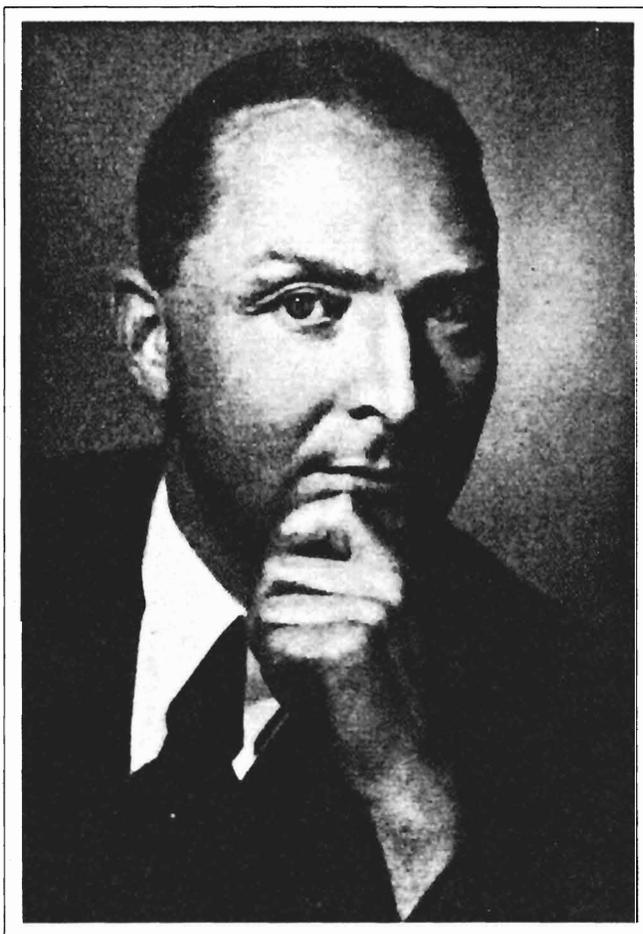


Figure 6. Hans August Georg Grimm (1887-1958)

construction of a separate matrix for each possible intra- and inter-row combination, and Grimm attempted to assemble these diagrams into a master diagram or so-called periodic table of binary compounds (26). Moreover, since all of the matrices gave similar results, he also summarized this pattern in the form of a generalized "Dreieckschema" which linked the three limiting-case bond types to one another via a characteristic pattern of intermediate bond types (figure 8).

Of particular interest is Grimm's use of plus and minus signs along the diagonal of his "Dreieckschema" in order to indicate the predominant electrochemical character of the component elements in the resulting binary compounds. Thus metal-metal or electropositive-electropositive combinations leading to metallic bonding were indicated by the symbol $+/+$, metal-nonmetal or electropositive-electronegative combinations leading to ionic bonding were indicated by the symbol $+/-$, and nonmetal-nonmetal or electronegative-electronegative combinations leading to covalent bonding were indicated by the symbol $-/-$. Like all chemists since Berzelius, Grimm was aware that the electronegativity of the elements increased as they became increasingly nonmetallic. He was further aware that electronegativity always increased on moving across a period of the periodic table (Indeed, in recognition of this fact, the German chemist, Lothar Meyer (29), had suggested the term "electrochemical period" in 1888 as a way of characterizing the conventional choice of periods in the periodic table) so, in

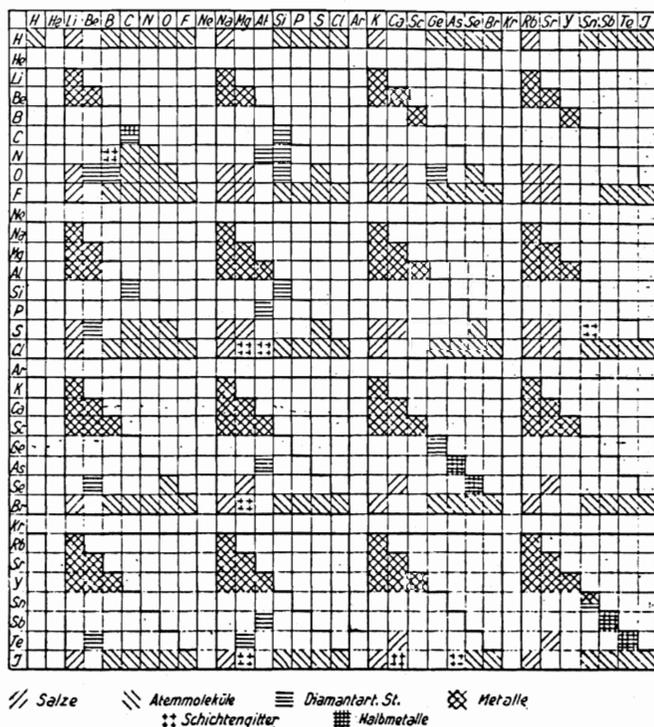
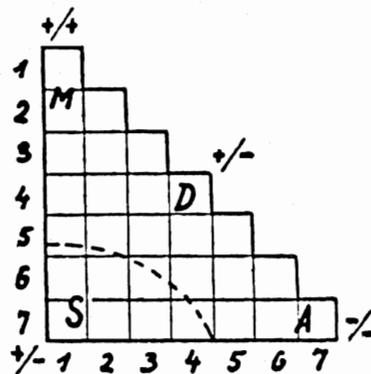


Figure 7. Examples of Grimm's triangular binary combination matrices.



A = Aemoleküle D = Diamantartige Stoffe
M = Metalle S = Salze

Figure 8. Grimm's generalized triangular binary combination matrix or "Dreieckschema."

effect, each of Grimm's diagrams represented a qualitative plot of the electronegativity of element A versus the electronegativity of element B in the resulting binary compounds $A_x B_y$. As long as he restricted each axis to a single period of the periodic table, Grimm could be confident that the elements were placed in order of either increasing (x -axis) or decreasing electronegativity (y -axis). However, in the absence of a quantitative electronegativity scale, he was unable to intermix elements from different periods of the periodic table on the same axis, and thus collapse all of his diagrams into a single quantitative master diagram.

An attempt at the latter step was taken by the American chemist, Charles Stillwell, in 1936 (30). He constructed a gigantic triangular master matrix by placing all of the elements along both the x - and y -axes in the order of their decreasing "metallicity" (figure 9). Though Stillwell did not explicitly spell out how he determined his metallicity order, we can infer his reasoning from an examination of his axes. These listed the elements by group from left to right across the periodic table, beginning with all of the alkali metals and ending with all of the halogens. Within each group, the nonmetals were generally listed from the bottom to the top of the group (save for N, B and Al, which were interdispersed), whereas the metals were listed from the top to the bottom of each group. With the exception of the ordering of the metals within each group and the listing of hydrogen as the least metallic element, this order roughly corresponds to the qualitative order given by Lothar Meyer a half-century earlier for the variation of electronegativity across the periodic table (29).

Like Grimm, Stillwell also attempted to characterize the binary combinations corresponding to each square of his matrix as metallic, ionic, or covalent, though his notation was much more complicated and intermixed both structural and bond-type criteria. However, despite the imperfections of his

metallicity order, he was able to sort the binary compounds in his matrix into regions characteristic of each bond and/or structural type.

The Yeh Bond-Type Triangle

The first quantitative electronegativity scale - Pauling's thermochemical scale - did not appear until 1932 (31). Though this preceded the publication of Stillwell's diagram and most of Grimm's publications, the scale would have been of little use to them in constructing their bond-type diagrams as the original paper reported electronegativity values for only ten elements. Despite the fact that this number had climbed to 33 by the time the first edition of the *Nature of the Chemical Bond* appeared in 1939, it was still far too small to quantify the kind of massive overview envisioned by these authors (32).

The first attempt to construct a bond-type diagram based on a quantified electronegativity scale was made by the Chinese chemist, Ping-Yuan Yeh, in a short note published in the *Journal of Chemical Education* in 1956 (33). Using the electronegativity values reported in Pauling's introductory text, *General Chemistry*, which Yeh was using in his freshman course, Yeh produced his bond-type diagram by plotting the electronegativity of element A versus that of element B for both binary compounds, A_2B_n , and for simple substances (figure 10). Though Yeh was apparently unaware of the earlier work of Grimm and Stillwell, his bond-type diagram was in

fact a partial quantification of Stillwell's triangle - partial because Pauling's text was still reporting electronegativity values for only 33 of the elements - indeed, the same values as had appeared 17 years earlier in the first edition of *The Nature of the Chemical Bond*. The apparent difference in the orientation of Stillwell's diagram was, of course, due to the fact that

his binary combination matrix was redundant with each binary compound appearing twice, once above and once below the 45° diagonal, and Stillwell had arbitrarily chosen to eliminate the bottom rather than the top half.

Yeh's presentation of his diagram also reflected some of the biases of American chemical education mentioned earlier. Thus he divided his diagram into three sharp regions - in response to the ever-present demands of students that, in the interests of examsmanship, they be given a black and white answer to the question of when a material is or is not ionic, cova-

lent, or metallic - even though he was fully aware that in reality there were "no sharp transitions from one type to another." Even more revealing was the fact that the region of the diagram labeled "metallic compounds" contained no specific examples other than simple substances, again reflecting the absence of any substantive discussion of these compounds in most introductory textbooks (34).

Despite its simplicity and attractiveness, the Yeh diagram appears to have been an educational dead end, as I have never encountered an example of its use in a textbook. This oversight is almost certainly traceable to the cause just mentioned - after all, why would a textbook be interested in using a diagram

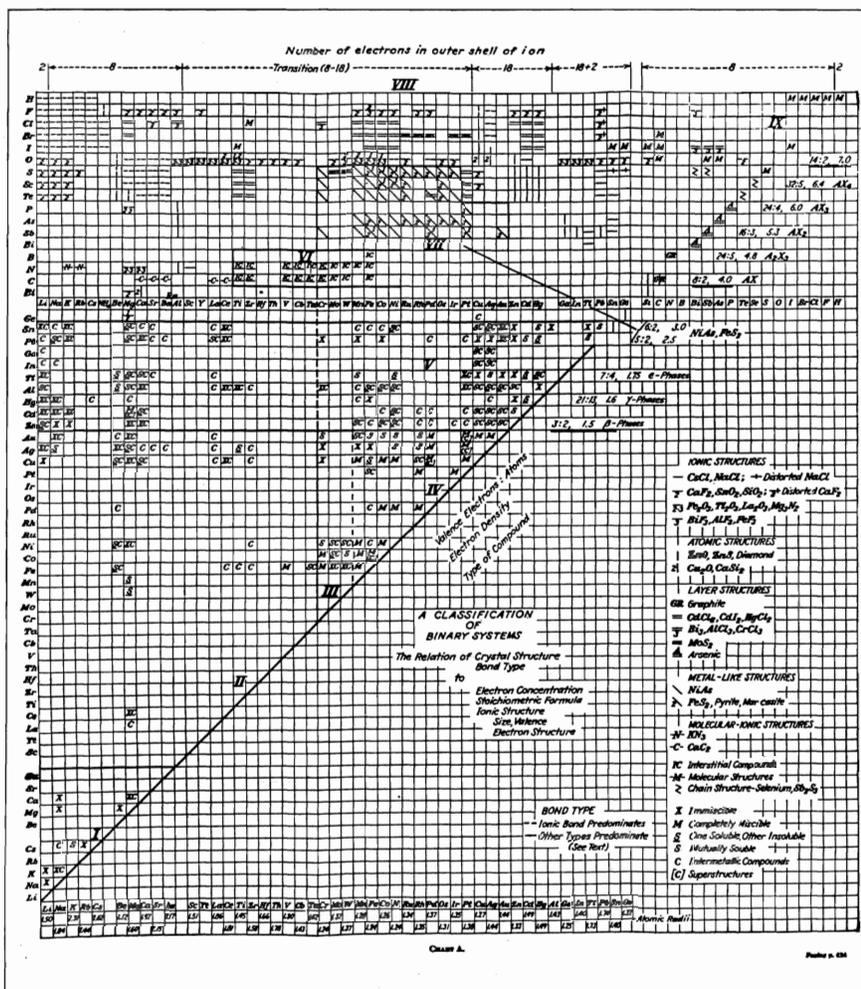


Figure 9. Charles Stillwell's 1936 bond-type matrix for binary compounds.

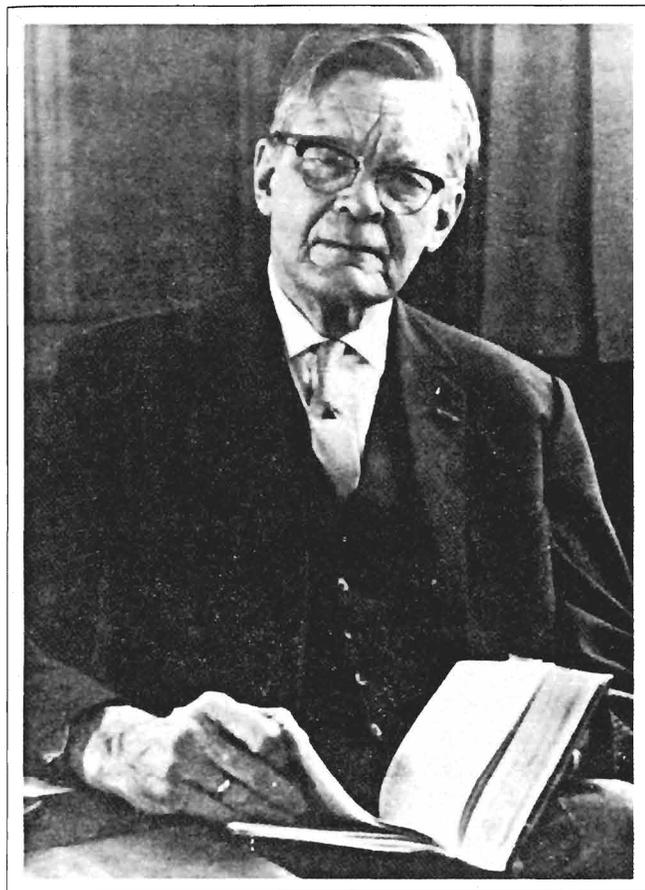


Figure 11. Anton Eduard van Arkel

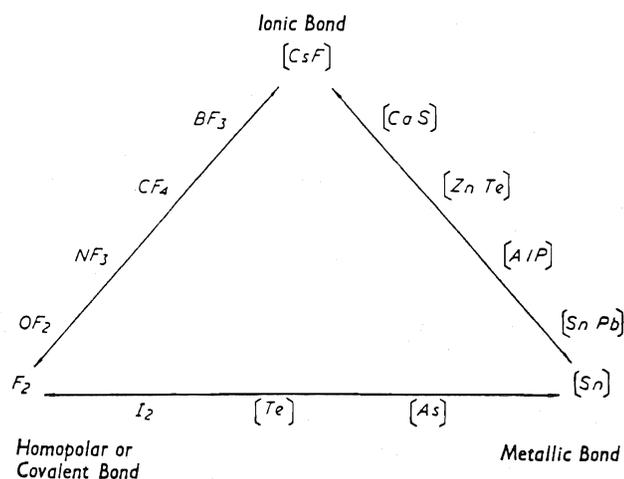


Figure 12. Van Arkel's 1941 bond-type triangle.

$$\tau = \Delta EN = (EN_B - EN_A) \quad (1)$$

This parameter will have a large value in the case of the low EN_A -high EN_B combinations characteristic of ionic compounds and a small value for the high EN_A -high EN_B and low EN_A -low EN_B combinations characteristic of covalent and metallic compounds respectively.

Likewise, the covalency parameter, \mathcal{C} , is defined as the average of the electronegativities (EN_{av}) of the two elements, A and B, in a binary compound, $A_a B_b$, irrespective of stoichiometry:

$$\mathcal{C} = EN_{av} = (EN_A + EN_B)/2 \quad (2)$$

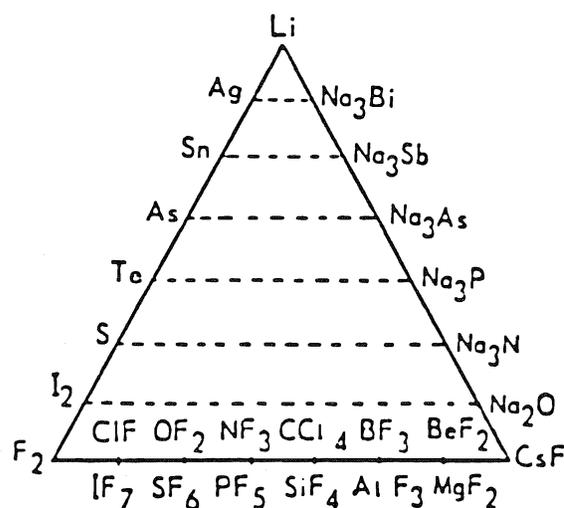


Figure 13. Ketelaar's 1947 version of the van Arkel bond-type triangle.

This parameter will have a large value in the case of the high EN_A -high EN_B combinations characteristic of covalent compounds and a small value in the case of the low EN_A -low EN_B combinations characteristic of metallic compounds. It will have an intermediate value for the low EN_A -high EN_B combinations characteristic of ionic compounds. Just as τ can be associated with the asymmetry of the bond, so \mathcal{C} can be associated with its localization. As \mathcal{C} decreases, the bonding will become less directional and more diffuse - in short, more metallic.

A plot of these two parameters for a variety of binary compounds and alloys is shown in figure 14. As can be seen, the compounds all lie within an equilateral triangle, with the ionic, covalent, and metallic extremes at each vertex. Just as in the case of the Yeh diagram, compounds of intermediate character, representing the transition between one extreme and another, lie along the edges and within the body of the triangle. For completeness, I have also included simple substances in

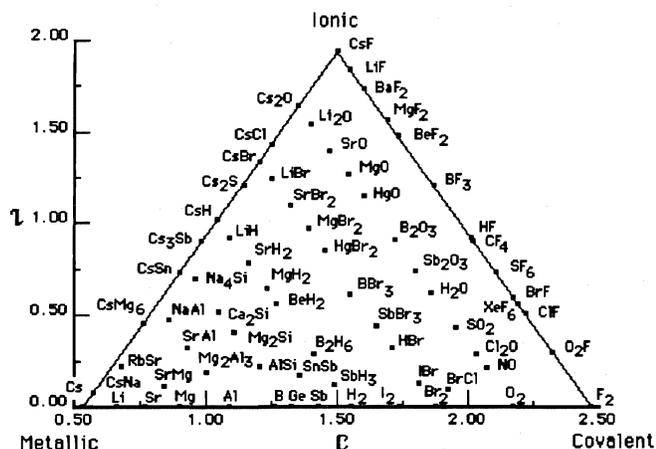


Figure 14. A quantified van Arkel diagram based on a plot of ionicity versus covalency for a variety of binary compounds, alloys, and simple substances.

the plot in order to have a transition along the edge joining the covalent and metallic extremes. These can be artificially viewed as a special type of compound in which both of the elements have the same EN. Equation 1 automatically assigns them an ionicity of zero and their covalency, as defined by equation 2, is identical to their electronegativity. Since the noble gases do not undergo self-linkage, they cannot be thought of as being compounds even in this artificial sense and hence are excluded from the diagram. However, their binary compounds with other elements (e.g., XeO_4 , KrF_2 , etc) are included. Because of the intense radioactivity of the element francium and the resulting nonavailability of its compounds for display and demonstration purposes, I have taken cesium as the archetypical metallic species and cesium fluoride as the archetypical ionic species. Since, as already mentioned, neon does not undergo homocatenation, difluorine (F_2) serves as the archetypical covalent species.

Closer examination of the figure shows that, in sharp contrast to the horizontal lines of Ketelaar's diagram, the compounds of each element lie on two diagonal lines which meet at the location of the corresponding simple substance on the x-axis, the left branch of which contains those compounds in which the element in question is the more electronegative component and the right branch those compounds in which it is the more electropositive component. The only exceptions are the compounds of fluorine, for which the electropositive branch is missing, and the compounds of cesium, for which the electronegative branch is missing, their remaining branches forming the two ascending sides of the triangle.

In making the plot in figure 14 and those which follow in figures 15-17, I have used the absolute values of a slightly modified version of the electronegativity scale introduced by the Russian chemists, Martynov and Batsanov, in 1980, based on an averaging of the successive ionization energies for an

element's valence electrons (45). The more familiar Allred-Rochow scale works just as well at the level of correlation used in freshman chemistry, provided that it is supplemented by published estimates for the electronegativities of the noble gases (46).

The definitions of the I and C parameters given in equations 1 and 2 also reveal that the van Arkel and Yeh diagrams are related via a simple series of coordinate transformations. Aside from the greater aesthetic appeal of the resulting equilateral triangle, the major advantage of using the more complex I/C coordinates versus the simpler EN_A/EN_B coordinates of the Yeh diagram, lies in the fact that the corresponding ΔEN and EN_{av} combinations can be loosely correlated with energy terms used in approximate quantum mechanical treatments of the bonding in binary solids, such as the well-known charge-transfer (C) and homopolar (E_h) parameters of Phillips (47).

Figures 15-17 illustrate some additional uses of the diagram obtained by plotting limited groups of compounds subject to additional external constraints. Thus figure 15 shows a plot of a series of compounds that are both isostoichiometric (1:1 or AB) and isoelectronic (total of eight valence electrons). As can be seen, the compounds are nicely sorted into regions corresponding to their crystal structures. Because structure depends on stoichiometry and valence electron counts, as well as bond character, it is necessary to fix two of these parameters before varying the third. This is an important limitation on the use of the van Arkel triangle and one which most introductory treatments of chemical bonding unhappily ignore. Thus it is not uncommon to find freshman textbooks implying that a one to one correlation exists between bond type and the physical properties of binary solids, such as melting point and conductivity, irrespective of their stoichiometry and valence-electron counts, though in actual fact, the first of these properties depends much more strongly on structure type than bond type (48).

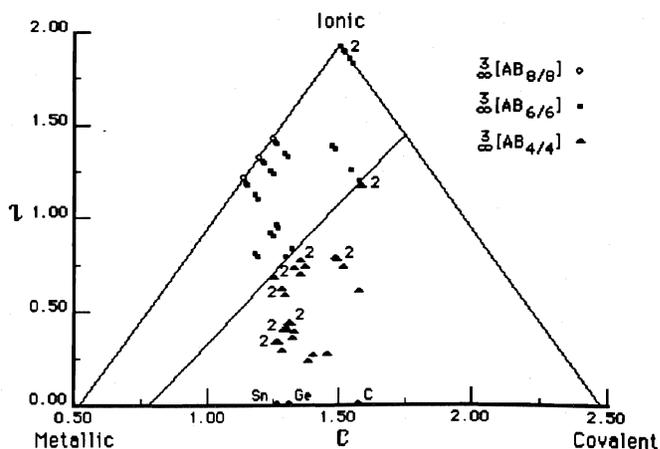


Figure 15. A structure-sorting map for 1:1 AB compounds composed of main-block elements and having eight valence electrons.

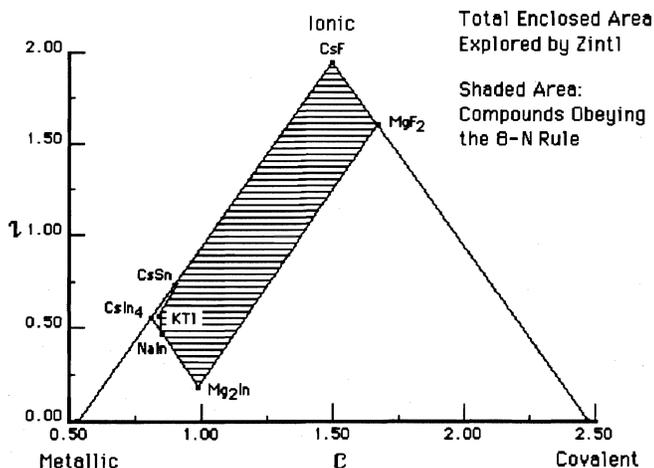


Figure 16. The van Arkel characterization of over 516 "Zintl" phases representing the transition between ionic and metallic bonding.

Similar structure-sorting maps can be obtained for other stoichiometric classes of compounds (AB_2 , AB_3 , etc.). Again, the ΔEN and EN_{av} combinations can be loosely correlated with the various combinations of pseudopotential radii that have been widely used as structure-sorting parameters by solid-state physicists (49).

Figure 16 gives an example of how I use the diagram in my inorganic course to locate characteristic groups of compounds before discussing the details of their descriptive chemistry. The shaded area on the triangle represents the location of over 516 "Zintl phases," first investigated by the German chemist, Eduard Zintl, in the 1930s and, more recently, by the late Herbert Schäfer of the Technische Hochschule in Darmstadt, as part of a systematic study of the transition between ionic and metallic bonding in binary compounds. All of the compounds within this region have structures which can be rationalized via an electron-count correlation known as the generalized 8-N rule, which is based, in turn, on our traditional ionic and covalent bonding models (50). Attempts to move further down the diagram toward the metallic vertex result in the formation of typical alloy phases whose structures no longer obey the 8-N rule.

Finally, figure 17 gives an example of how I use the diagram in my freshman chemistry course. In this case samples of the materials in question are shown to the students and a quick and dirty test of their conductivity is made (or simply provided, in the case of gases) with a probe-buzzer-battery tester. A plot of the compounds and simple substances on the triangle shows that those with detectable conductivities are located near the metallic vertex, that metallic appearance does not necessarily correlate with conductivity (i.e., solid I_2), and that both the I and C parameters are needed in order to accurately sort the compounds and simple substances into conductors and non-conductors (i.e., solid NaCl doesn't conduct despite having a lower EN_{av} than solid SiC).

Conclusion

It was Henry Bent, I think, who sagely observed that all chemical demonstrations automatically illustrate all of the principles of chemistry, since every principle is involved, to a greater or lesser degree, in our understanding of the phenomenon in question. Our use of a demonstration to illustrate a single principle is an artifice produced by intentionally focusing the students' attention on only one aspect of the phenomenon. The same is true to a lesser degree of the diagrams and illustrations that we use in our textbooks and in our classrooms. As we have seen in the case of the CBA bond-type triangle, when restored to their historical context, such diagrams can serve as microexamples of the evolution of chemistry itself. And, in our particular case, this history also serves as elegant testimony to the creativity and originality of Larry Strong, Ted Benfey and the many other teachers who played a role in the development the CBA program and its accompanying text.

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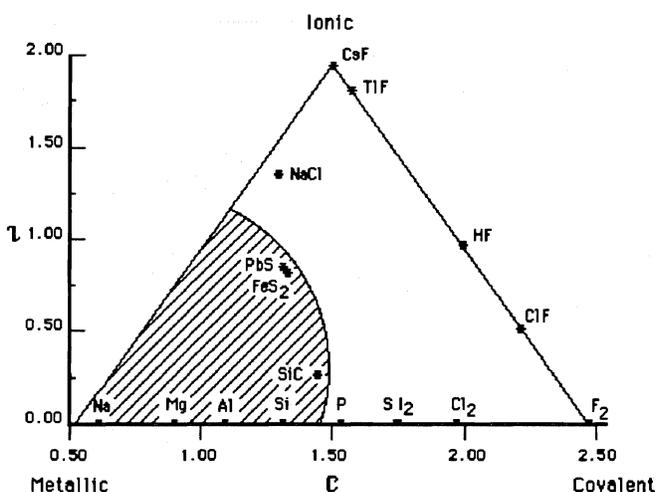


Figure 17. A plot of a selection of compounds and simple substances used as part of a demonstration in freshman chemistry to illustrate the development of incipient metallic character in binary compounds. Substances in the shaded area show detectable conductivity with a crude probe-buzzer-battery conductivity detector, whereas those outside the shaded region do not.

4. H. von Helmholtz, "The Modern Development of Faraday's Conception of Electricity," *J. Chem. Soc.*, **1881**, 38, 277-304. In actual fact, Helmholtz's suggestions merely replaced one puzzle with another. In the course of providing an electrostatic explanation of why atoms attracted one another to form molecules, he implicitly introduced the necessity of having to postulate a new force in order to explain the attraction between the underlying particles of matter and the particles of both positive and the negative electricity.
5. For a typical example of this somewhat premature enthusiasm, see R. K. Duncan, *The New Knowledge: A Simple Exposition of the New Physics and the New Chemistry in their Relation to the New Theory of Matter*, Barnes, New York, NY, 1908.
6. These proposals and extensions are discussed in great detail in A. N. Stranges, *Electrons and Valence: Development of the Theory, 1900-1925*, Texas A&M Press, College Station, TX, 1982. This is a well-documented history of the electronic theory of bonding up to 1925 with an excellent bibliography. The only point on which it is weak is in its coverage of the ionic model during the period 1916-1923, largely because of its overemphasis on the emergence of Lewis' electron-pair bond.
7. These early calculations are summarized in S. Arrhenius, *Theories of Chemistry*, Longmans, London, 1907, pp. 61-64.
8. Summaries of the work of Born and Landé can be found in reference 10, chapter 3, and in M. Born, *Atomtheorie des festen Zustandes*, Teubner, Leipzig, 1923. A more personal account is given in M. Born, *My Life: Recollections of a Nobel Laureate*, Scribner, NY, 1975, pp. 181-183 and 188-190.
9. Summaries of the development of the electrostatic theory of coordination compounds can be found in reference 10, chapter 8, and in R. W. Parry and R. N. Keller, "Modern Developments: The Electrostatic Theory of Coordination Compounds," in J. C. Bailar, ed., *The Chemistry of Coordination Compounds*, Reinhold, New York, NY, 1956, Chapter 3.
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33. P-Y Yeh, "A Chart of Chemical Compounds Based on Electronegativities," *J. Chem. Educ.*, 1956, 33, 134.

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39. M. B. Ormerod, *The Architecture and Properties of Matter: An Approach Through Models*, Arnold, London, 1970, p. 103.

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Note Added in Proof: I have recently discovered that an equilateral bond-type triangle similar to that of van Arkel was given as early as 1935 in W. C. Fernelius and R. F. Robey, "The Nature of the Metallic State," *J. Chem. Educ.*, 1935, 12, 53-68. Since this article was reprinted in R. K. Fitzgerald and W. F. Kieffer, *Supplementary Readings for the Chemical Bond Approach*, Journal of Chemical Education, Easton, PA, 1960, it, rather than van Arkel's book, is the most likely origin of the diagram which appeared in the CBA text, though the van Arkel book is better known.

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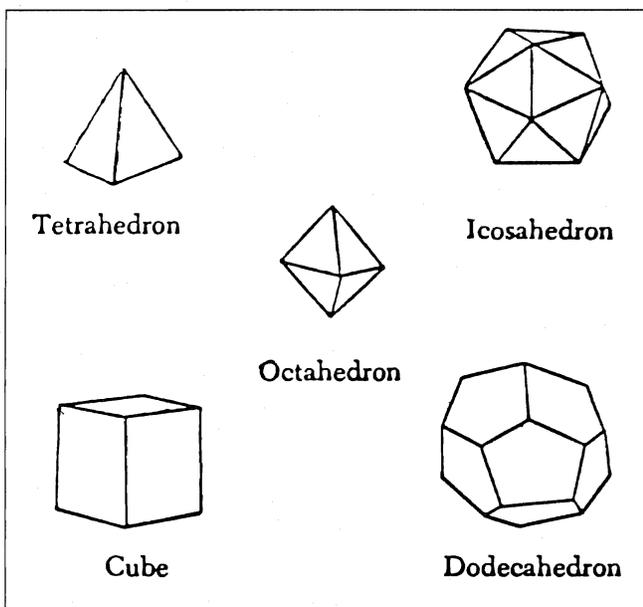
PERSPECTIVES LECTURE

Precursors and Cocursors of the Mendeleev Table: The Pythagorean Spirit in Element Classification

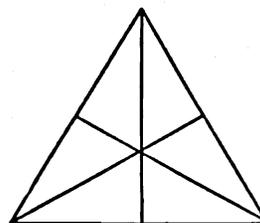
O. Theodor Benfey, Chemical Heritage Foundation

The ancient Pythagorean faith in simple numerical patterns as guides to the structure of the natural world constantly reappears in science. The Pythagoreans worshipped numbers and by numbers they meant whole numbers - integers. Since two points can define a line, three a plane and four a three-dimensional body, they believed all reality could be subsumed by whole numbers and geometry. The Cubist movement in art hints at the Pythagorean vision. That vision, that religious movement, was shattered by the discovery of irrationals, unreasonable quantities that were incapable of being expressed as ratios or other combinations of whole numbers. The square root of 2, the reciprocal of 7 and the ratio of the circumference to the diameter of a circle are examples.

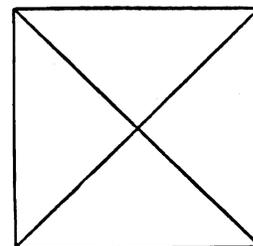
The simplest Pythagorean principle is the search for identity. The identity of the velocity of light and of electromagnetic waves united optics and electromagnetic theory. The next simplest is the identity in the number of members of two sets. The four elements of Empedocles - earth, air, fire and water - found a counterpart in the discovery of four regular solids - the tetrahedron, octahedron, icosahedron and cube. The belief grew that there must be a connection between them. When the fifth (and last) regular solid, the dodecahedron, was discovered, a fifth element was postulated that corresponded to it. It became the quintessence, the fifth essence, the ether of antiquity. Since terrestrial events were sufficiently described by the



The five regular polyhedra or Platonic solids.



Plato's Triangle



Plato's Square

Plato's building blocks for the equilateral triangle and the square.

four elements, the fifth element was relegated to the heavens. It was a logical assignment since the behavior of celestial bodies differed from that of objects on earth. Since the natural motions of earth, air, fire and water when displaced from their normal abode were rectilinear, returning to their "proper" place, celestial motions, which were circular rather than linear, must be due to a different kind of stuff, of which the heavens were made. Behavior was seen as integral to an object, not something imposed upon it.

Plato in the *Timaeus* spells out the identification of regular solids with the elements of antiquity. The atoms of fire are the sharpest, hence tetrahedra; the next sharpest are octahedra which are assigned to air since it too can slip through very small interstices. Earth is the most stable, hence corresponds to the cube. The dodecahedron is the closest to the sphere, thus fittingly belonging to the heavens, leaving the icosahedron as the form of the atom of water.

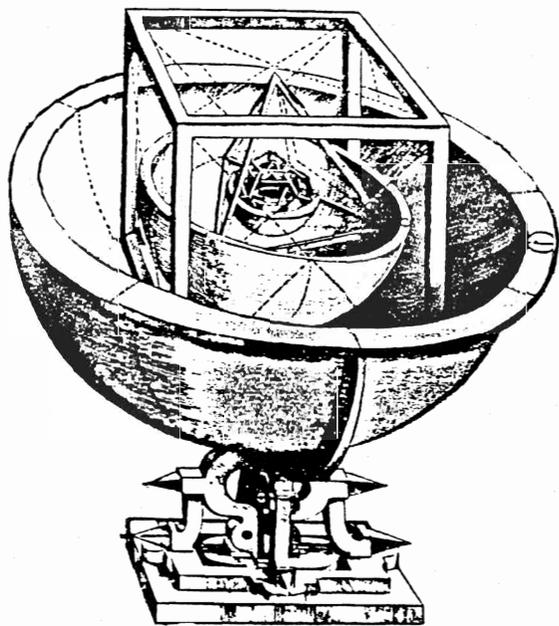
We need to recognize the genius of Plato's view. First it accounted for transmutation. Since tetrahedra, octahedra and icosahedra were all made from equilateral triangles, they could be taken apart into their constituent triangles and reassembled into new forms. These transformations were quantitative. The eight faces of two tetrahedra can be taken apart and reassembled into those of one octahedron. One water particle (20-sided icosahedron) can be changed into two air particles and one fire particle. We have here a universe made up of a small number of particles which, by rearrangement, make up the phenomena we observe. It is the form of the descriptive pattern we now use in describing nuclear transformations. The more commonly accepted precursor of modern atomic theory, the atomism of Democritus and Leucippus, had neither of these two key characteristics. Those thinkers postulated an unlimited number of different atoms and had no quantitative predictive theory for explaining change.

The second of Plato's contributions to element theory is more speculative. Karl Popper has suggested that Plato's choice of atoms was designed to overcome the Pythagorean scandal. Plato's atoms were actually the half-equilateral triangle (for tetrahedron, octahedron and icosahedron) and the half-square for the cube. These two triangles have sides in the

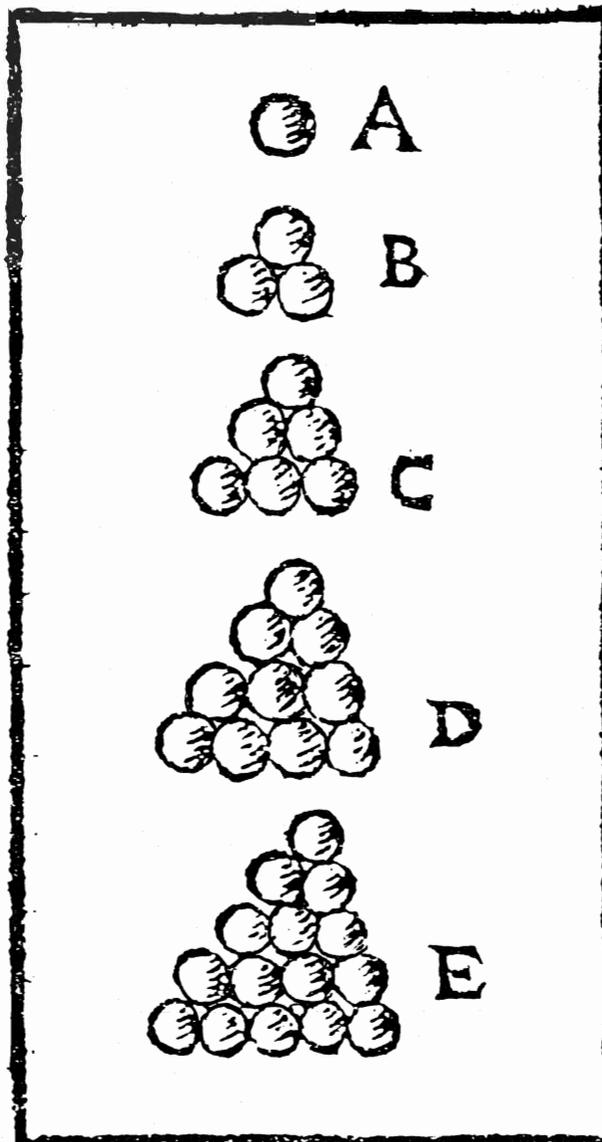
ratios of 1, 2, $\sqrt{3}$ and 1, 1, $\sqrt{2}$ respectively, thus incorporating two of the Pythagorean irrationals into the fundamental building blocks of nature. Popper further suggests that the hope existed that all irrationals could be derived from $\sqrt{2}$ and $\sqrt{3}$. Their sum, for instance, to four significant figures is 3.146, a figure within the limits of accuracy of the calculation of π of that era (1). Thus irrationals, though not exactly banned from the universe, were at least tamed. They were incorporated into the building blocks of nature.

The dream of finding a numerical pattern to the phenomena of nature lay in abeyance for centuries. Although the *Timaeus* was the only dialogue of Plato translated into Latin during the ancient period, the sections of the *Timaeus* dealing with the regular solids were not included in the early Latin versions (2). The prevailing description of terrestrial nature throughout the medieval period was the qualitative Aristotelian world-view. Mathematical perfection could only be found in the heavens. Only as we enter the period of the Renaissance do we again find significant attempts to correlate what we would call chemical events with numbers.

The most famous Pythagorean or Neoplatonist of the Renaissance was no doubt Johannes Kepler (1571-1630). His most celebrated attempt to apply the Pythagorean vision was his rationalization of the sizes of the planetary orbits by inscribing and circumscribing the orbits with the Platonic solids. But Kepler did not confine his interest to the heavens. In his work, *The Six-Cornered Snowflake*, he proposed an essentially modern explanation for the snowflake's shape in terms of the packing of spheres. L. L. Whyte, in the preface to the new edition of Kepler's booklet, describes it as "the first recorded step to-



Planetary orbits and the regular solids.
(from Kepler's *Mysterium cosmographicum* of 1596).



Kepler's use of sphere packing to explain crystal forms.

wards a mathematical theory of the genesis of inorganic or organic forms (3)."

The iatrochemist William Davison (1593-1669) was equally convinced that number and geometry were the key to understanding nature. One of two engraved plates in his *Les élémens de la philosophie de l'art du feu ou chimie* of 1651 shows the Platonic solids followed by 15 other geometric forms. The opposite page shows natural forms - crystals, flowers, leaves and the beehive hexagon, to illustrate the Biblical phrase written in Latin across the center of the page: "all is disposed in measure, number and weight."

The Pythagorean fascination with integers makes a permanent reentry into chemistry with John Dalton's (1766-1844) chemical atomic theory. Antoine Lavoisier's (1743-1794) emphasis on weight relations led logically to the determination

of percentage compositions. But what assurance of being on the right path of theory is contained in the fact that the lower oxide of carbon contains 57.1% oxygen while carbonic acid gas contains 72.7% oxygen? Dalton's atomic theory gave a visualizable model for the Pythagorean law of multiple proportions: For two compounds of elements A and B, the weights of A combined with a fixed weight of B are in the ratio of small whole numbers. Thus, recalculating the composition data, 1 g carbon combines either with 1.33 grams of oxygen or 2.66 grams of oxygen, an integral ratio of 1 to 2.

Another Pythagorean pattern was proposed within six years of the completion of Dalton's book *New System of Chemical Philosophy*. In 1816, the physician William Prout (1785-1850), whose quantitative analysis of natural urea was used by Friedrich Wöhler (1800-1882) to compare it with the urea he had accidentally made synthetically, pointed to the remarkable fact that most atomic weights used at that time were close to integral multiples of the atomic weight of hydrogen. That particular Pythagorean venture has, of course, had a checkered history, being espoused by some, such as Thomas Thomson (1773-1852), as true, by others, such as Jean-Servais Stas (1813-1891), as pure illusion, while Jean-Charles Marignac (1817-1894) accepted it as an ideal law analogous to the ideal gas laws and Dmitri Mendeleev (1834-1907) hinted at a mass-energy interconversion to account for the deviations. There can be little doubt that Prout's proposal acted as a most powerful stimulus to accurate atomic weight studies and encouraged others to look for additional numerical patterns in the atomic weights slowly being accumulated.

Even if Prout's simple proposal had been right, if all atoms were in fact aggregates of hydrogen atoms, such a conclusion would have done little to illuminate the richness and diversity of chemical behavior. It certainly was not the final clue, because the elements then would only differ quantitatively and progressively as their atomic masses increased. Johann Wolfgang Döbereiner (1780-1849) from 1816 to 1829 searched for numerical relations between similar elements in the same way that the Pythagoreans sought number patterns relating the lengths of strings producing harmonious chords. Döbereiner's triads not only demonstrated such arithmetic relations but thereby suggested unit building blocks converting atoms of lithium to those of sodium and hence to potassium, or calcium to strontium and then to barium. Döbereiner was influenced by the Romantic movement and *Naturphilosophie* which had been flourishing in Jena around 1800, a decade before Döbereiner's arrival. "His predilection" according to Alan Rocke, "was toward a Pythagorean synthesis, the mathematization of nature" (4).

The idea of unit building blocks gained support from Max Pettenkofer (1818-1901) who pointed to the analogy between an atomic weight series of similar elements and the pattern of molecular weights in organic homologous series (5). Thus $\text{CH}_4 = 16$, $\text{C}_2\text{H}_6 = 30$, $\text{C}_3\text{H}_8 = 44$, etc. The common increment

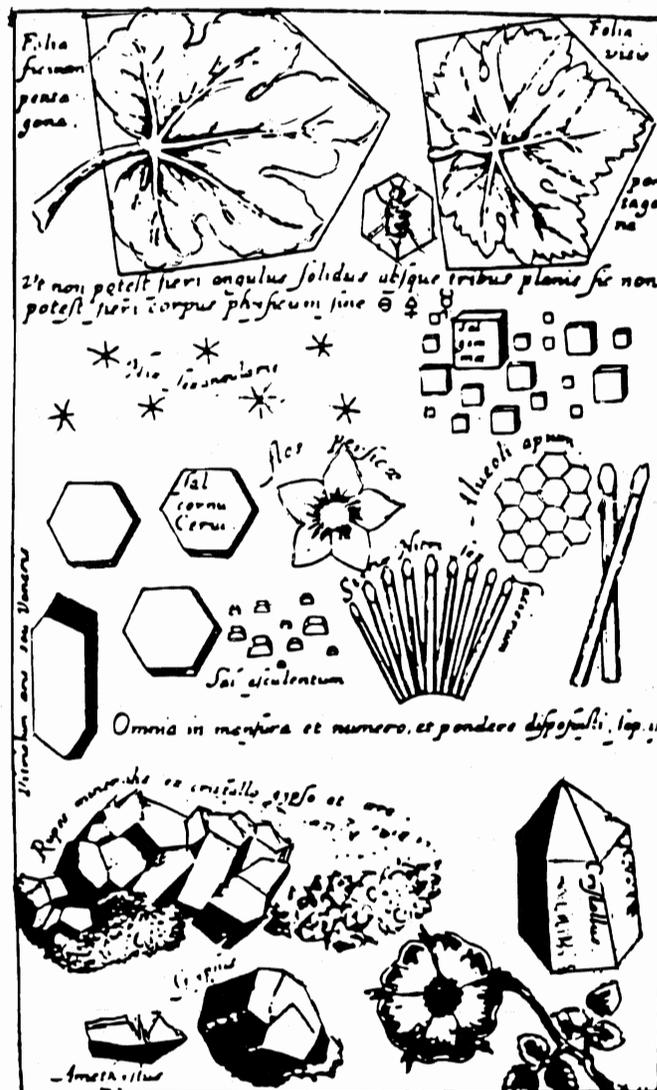


Plate from William Davison's *Les éléments de la philosophie de l'art du feu ou chimie* showing natural forms with shapes similar to those of regular solids.

(of 14) in these weights suggested that perhaps organic radicals held the clue to the internal structure of inorganic atoms. Jean-Baptiste André Dumas (1800-1884) and Justus Liebig (1803-1873) had already proposed in 1837 that organic radicals play in organic chemistry the role played by atoms in mineral (or inorganic) chemistry (6):

In mineral chemistry the radicals are simple; in organic chemistry the radicals are compound; that is all the difference. The laws of combination and of reaction are otherwise the same in these two branches of chemistry.

What Pettenkofer was now proposing was that the radicals or

atoms of mineral chemistry were not simple - they were as compound as were organic radicals because the same mathematical patterns occurred in the unit masses of both.

Dumas developed these ideas about the composite nature of atoms independently in 1851 in a speech before the British Association for the Advancement of Science but only published them six years later (7). His parallel tabulations of organic radicals and families of elements (Table 1) show that he had freed himself from Döbereiner's preoccupation with sets of three elements, and that he was developing the families of elements as they would later be incorporated in the periodic table.

John Alexander Reina Newlands (1837-1898) broke away from searching for numerical patterns in the atomic weights of similar elements and proclaimed instead a pattern for *all* elements, similar or dissimilar. He did not need the actual values of atomic weights; all that was needed for his law of octaves was a rank order, the ranking of elements in order of increasing atomic weight. In some of his earlier tables Newlands left gaps for missing elements - Mendeleev certainly was not the first to do this - but by 1864 he had found an arrangement not requiring gaps and obeying his now famous law of octaves, though he did not give it this name until 1865 (8). On presenting it at a meeting of the London Chemical Society, the British chemist George Carey Foster posed the immortal question of whether a similar pattern might exist if elements are arranged alphabetically. Carey Foster clearly did not comprehend the essence of the Pythagorean discovery - that numerical order lies in the essence of things while alphabetical order is man-made and arbitrary. What Newlands discovered was an orchestration of the elements, a periodicity, a repetition combined with novelty, the essence of all musical composition. It would have delighted Pythagoras (9).

And yet something was missing. Except for the atomic weight order, why do the alkaline earths follow the alkali metals rather than the halogen family? Was there any intrinsic pattern that linked elements of *different* families, that would show an essential order to the relation among families? The year 1864, the same year as Newlands' law of octaves, saw the publication of the first edition of Julius Lothar Meyer's (1830-1895) *Die Modernen Theorien der Chemie* (10). Meyer was fascinated by Prout's hypothesis, by Döbereiner's triads and by Pettenkofer and Dumas' analogies between chemical element families and organic homologous series. He had attended the Karlsruhe Congress in 1860 as had Mendeleev. Both had been deeply influenced by Stanislaw Cannizzaro's

Table 1. Dumas' analogy between families of chemical elements and homologous series of organic radicals.

H = 1 + (0 x 14)	O = 8	= 8
CH ₃ = 1 + (1 x 14)	S = 8 + 8	= 16
C ₂ H ₅ = 1 + (2 x 14)	Se = 8 + (4 x 8)	= 40
C ₃ H ₇ = 1 + (3 x 14)	Te = 8 + (7 x 8)	= 64
C ₄ H ₉ = 1 + (4 x 14)		
	N = 14	= 14
	P = 14 + 17	= 31
	As = 14 + 17 + 44	= 75
	Sb = 14 + 17 + (2 x 44)	= 119

(1826-1910) speech and pamphlet. Meyer later wrote how, on reading the booklet, "the scales fell from my eyes and my doubts disappeared and were replaced by a feeling of quiet certainty." His *Moderne Theorien* was a direct outcome of that experience. Calculating all atomic weights according to Cannizzaro's principles, he arranged them by increasing atomic weight and in families, and calculated the increments of weight from each atom to the next similar one. So far nothing was new. But when we look at the table he published of 27 elements arranged in this way, three remarkable facts stand out:

1. He leaves a gap between silicon and tin and estimates the atomic weight of the missing element to be $28.5 + 44.55 = 73.05$. Germanium's atomic weight was later found to be 72.59. Newlands had also done this in 1864.

2. He places tellurium *before* iodine in spite of the fact that its atomic weight, 128.3 is greater than that of iodine (126.8). Newlands also did this.

3. Most impressive of all, he places at the heads of the families the notations 4-werthig, 3-werthig, 2-werthig, 1-werthig, 1-werthig, 2-werthig, that is successive valences of 4, 3, 2, 1, 1, and 2. These are the valences toward hydrogen, the number of hydrogen atoms that attach themselves to an atom of the element.

The fact that water was H₂O and not HO, and that oxygen therefore was divalent, was not universally accepted until after the Karlsruhe Congress. Within organic chemistry, Avogadro's

hypothesis (based on that other Pythagorean law - Gay-Lussac's law of combining volumes) had been widely accepted particularly by Auguste Laurent (1807-1853) and Charles-Frédéric Gerhardt (1816-1856) in their thorough reexamina-

No.	No.	No.	No.	No.	No.	No.	No.	No.	No.
H 1	F 8	Cl 15	Co & Ni 22	Br 29	Pd 36	I 42	Pt & Ir 50		
Li 2	Na 9	K 16	Cu 23	Rb 30	Ag 37	Cs 44	Os 51		
G 3	Mg 10	Ca 17	Zn 24	Sr 31	Cd 38	Ba & V 45	Hg 52		
Bo 4	Al 11	Cr 19	Y 25	Ce & La 33	U 40	Ta 46	Tl 53		
C 5	Si 12	Ti 18	In 26	Zr 32	Sn 39	W 47	Pb 54		
N 6	P 13	Mn 20	As 27	Di & Mo 34	Sb 41	Nb 48	Bi 55		
O 7	S 14	Fe 21	Se 28	Ro & Ru 35	Te 43	Au 49	Th 56		

Newland's Table of Octaves of 1865.

tion and reorganization of organic theory. Alexander Williamson's (1824-1904) studies of ethers had established the water type and divalent oxygen, August Wilhelm Hofmann's (1818-1892) amine work did the same for the ammonia type and the trivalence of nitrogen. Friedrich August Kekulé (1829-1896) in 1857 rescued organic radicals from being looked at merely as good substituents in inorganic type formulas, and established methane as the parent type of all carbon compounds and hence gave carbon a valence of 4. Laurent and Gerhardt accepted and universalized Avogadro's conclusion beyond the

ready applicability of Avogadro's hypothesis. They tended to assume that all elements, not only the common gaseous ones, were composed of diatomic molecules. With that assumption formulas of numerous inorganic compounds looked most unlike the formulas of today. Cannizzaro's reform introduced two other major criteria for atomic weight

determinations, particularly useful for elements that do not readily form gaseous compounds. Besides Avogadro's hypothesis, he used as guides the law of Pierre-Louis Dulong (1785-1838) and Alexis-Thérèse Petit (1791-1820) and Eilhard Mitscherlich's (1794-1863) law of isomorphism.

Meyer's book in its first edition was in large measure a detailed exposition of the application of these three methods. With them formulas could now be confidently established, and when they were examined a new pattern emerged. The elements from carbon to magnesium change by one valence unit, decreasing first to unity and then increasing again. The same pattern recurs from silicon to calcium, from arsenic to strontium, from tin to barium. No element is expected to be found between any pair of successive elements, for valence can only change by integral steps. It was a true Pythagorean

property. The only places where sets of new elements could be located would be at the beginnings and ends of each horizontal series, or at the beginning of the whole list or beyond the heaviest element. Meyer organized 21 other elements in seven further families which can be appended to the earlier table but do not show the stepwise change in valence. They are all transition metals.

No one, it seems, suspected that from a purely numerical point of view there was in fact one other place for new elements - between the two univalent families, a family of valence zero.

The absence of any expectation of such a family reminds us of the centuries that it took before the zero symbol was introduced into the Hindu-Arabic numeral notation - before zero was recognized as a number.

Mendeleev's periodic table of 1869 was characterized by his successfully arranging *all* the elements into one table and in demonstrating that

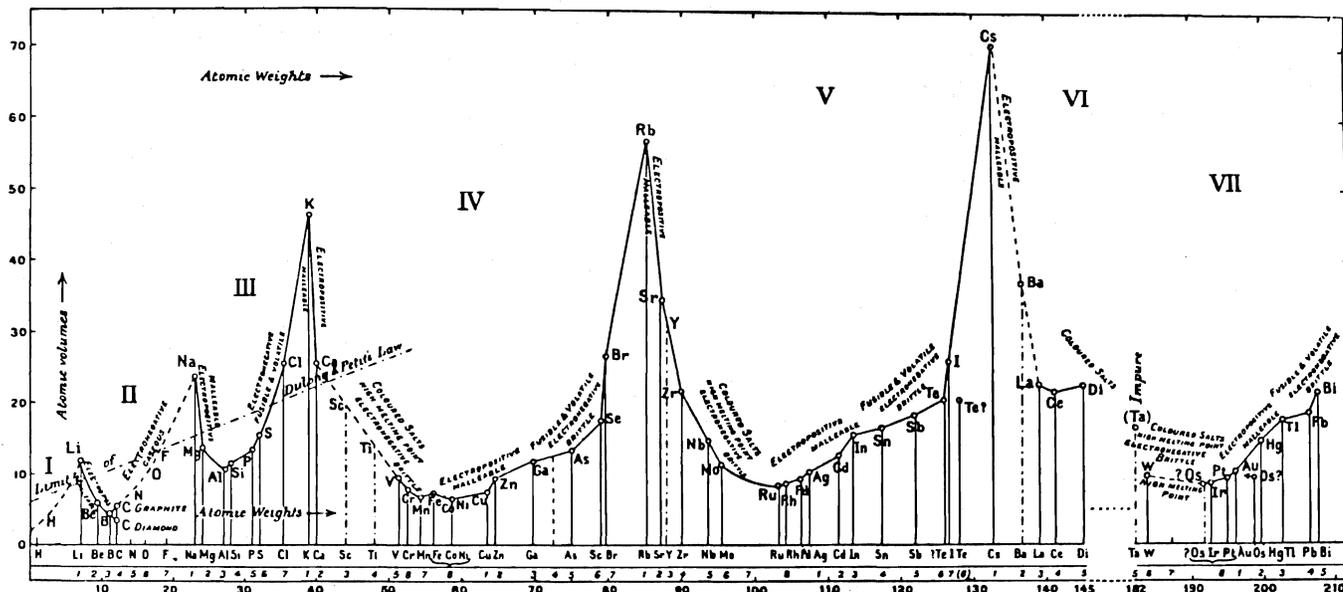
	4 val.	3 val.	2 val.	1 val.	1 val.	2 val.
	Li 7.03	(Be 9.3)
Diff.	16.02	(14.7)
	C 12.0	N 14.4	O 16.00	F 19.0	Na 23.5	Mg 24.0
Diff.	16.5	16.96	16.07	16.46	16.08	16.0
	Si 28.5	P 31.0	S 32.0	Cl 35.46	K 39.13	Ca 40.0
Diff.	$\frac{89}{2} \cdot 1$ 44.45	44.0	46.7	44.51	46.3	47.0
	As 75.0	Se 78.8	Br 79.97	Rb 85.4	Sr 87.0
Diff.	$\frac{89}{2} \cdot 1$ 44.55	45.6	49.5	46.8	47.6	49.0
	Sn 117.6	Sb 120.6	Te 128.3	I 126.8	Cs 133.0	
Diff.	$\frac{89}{2} \cdot 4$ 44.7	$\frac{87}{2} \cdot 4$ 43.7	35.5	
	Pb 207.0	Bi 208.0	(Tl 204.0?)	Ba 137.1
	4 val.	4 val.	4 val.	2 val.	1 val.	
	{ Mn 55.1	Ni 58.7	Co 58.7	Zn 65.0	Cu 63.5	
	{ Fe 56.0					
Diff.	{ 49.2					
	{ 48.3	45.6	47.3	46.9	44.4	
	Ru 104.3	Rh 104.3	Pd 106.0	Cd 111.9	Ag 107.94	
Diff.	$\frac{92}{2} \cdot 8$ 46.0	$\frac{92}{2} \cdot 8$ 46.4	$\frac{92}{2} \cdot 3$ 46.5	$\frac{88}{2} \cdot 3$ 44.5	$\frac{88}{2} \cdot 2$ 44.4	
	Pt 197.1	Ir 197.1	Os 199.0	Hg 200.2	Au 196.7	

Lothar Meyer's table of 1864.

periodicity holds throughout. His 1871 table clearly indicates that valence periodicity, by integer-unit steps, applies to all elements.

It appears that Mendeleev was extremely skeptical of any Pythagorean or Proutian implications of his work, considering them mere utopias. Yet he did much to suggest their significance (11).

There seems to be no question now that Meyer and Mendeleev independently discovered the periodic law. For a number of years a somewhat bitter debate raged between Mendeleev and Meyer regarding the original contributions of each to the chemical literature. Meyer's paper was submitted in December 1869 and published in March 1870 (12) and refers to the brief German notice of Mendeleev's longer paper in Russian (13). Meyer, in that paper, published the atomic volume



Lothar Meyer's atomic volume versus atomic weight curve as redrawn by Thomas Bailey for the *Philosophical Magazine* in 1882.

curve for which he is most generally remembered. However, we need to recognize that Mendeleev as well as Meyer in their classic papers discuss both atomic volume and other properties which vary periodically as atomic weight rises.

Meyer begins his paper with the assertion that it is most improbable that the chemical elements are absolutely undecomposable and mentions Prout, Pettenkofer and Dumas as precursors of this idea.

In 1893, two years before Meyer's death, his successor, Adolf Remelé, at the School of Forestry in Neustadt-Eberswalde, showed him a handwritten draft of a periodic table which Meyer had given to him in July 1868 and which was intended for a new edition of the *Moderne Theorien*. Meyer had totally forgotten the existence of this draft since, after Mendeleev's 1869 paper, it had to be redone. It demonstrated clearly Meyer's independent arrival at a table contain-

ing almost all known elements, arranged by increasing atomic weights and in periods, with the A and B subgroups separated, and with a gap left between silicon and tin suggesting the future discovery of germanium. Meyer's table places lead correctly below tin (column 8) while Mendeleev had put it with calcium, strontium and barium. On the other hand, hydrogen, boron and indium are not on the table, presumably because Meyer did not know where to locate them. The 1868 table was published posthumously by Karl Seubert, Meyer's successor in Tübingen (14). Mendeleev and Meyer were recognized as independent developers of the periodic table of the elements by the Royal Society of London when they were

both awarded the Davy medal in 1882.

The Periodic Table was not by any means the final triumph of the Pythagorean dream in element classification. Prout's hypothesis was shown to be essentially

1	2	3	4	5	6	7	8
Cr=52.6	Mn=55.1 49.2 Ru=104.3 92.8=2.46.4 Pt=197.1	Al=27.3 #1=14.8 Fe=56.0 48.9 Rh=103.4 92.8=2.46.4 Ir=197.1	Al=27.3 Co=58.7 47.8 Pd=106.0 93=2.465 Os=199.	Ni=58.7	Cu=63.5 44.4 Ag=107.9 88.8=2.44.4 Au=196.7	Zn=65.0 Cd=111.9 88.3=2.44.5 Hg=200.2	C=12.00 16.5 Si=28.5 #2=1=44.5 #3=1=44.5 Sn=117.6 89.4=2.41.7 Pb=207.0
9	10	11	12	13	14	15	
N=14.4 16.96 P=31.0 44.0 As=75.0 45.6 Sb=120.6 87.4=2.43.7 Bi=208.0	O=16.00 16.07 S=32.07 46.7 Se=78.8 49.5 Te=128.3	F=19.0 16.46 Cl=35.46 44.5 Br=79.9 46.8 I=126.8	Li=7.03 16.02 Na=23.05 16.08 K=39.13 46.3 Rb=85.4 47.6 Cs=133.0 71=2.35.5 Tc=204.0	Be=9.3 14.7 Mg=24.0 16.0 Ca=40.0 46.3 Sr=87.6 49.5 Ba=137.1	Ti=48 42.0 Zr=90.0 47.6 Ta=137.6	Mo=92.0 45.0 Vd=137.0 47.0 W=184.0	

Lothar Meyer's unpublished table of 1868.
(from F. P. Venable, *The Development of the Periodic Law*)

correct as far as the weights of individual nucleides were concerned. Non-integral atomic weights are mainly due to the presence of isotopic mixtures in the usual samples of elements. The ordinal number of Newlands' "rank order" of elements became identified in 1913 by Henry Gwyn-Jeffries Moseley (1887-1915) with the number of increments that the square root of the frequency of X-rays must be shifted to predict the correct X-ray frequency for a given element (15). The ordinal number became the atomic number, the integral positive charge and number of protons of an atom's nucleus and the number of electrons surrounding it. But these Pythagorean identifications once again did not account for the diversity of chemical properties. That was achieved by arranging the electrons in superbly simple Pythagorean patterns, by recognizing that similar chemical properties imply similar arrangements of electrons. We are the true inheritors of an idea 2500 years old - that the properties of the elements are the properties of numbers (16).

References and Notes

- Acknowledgments:* An early version of this paper was presented at the Mendeleev Symposium, December 1969, Annual Meeting of the American Association for the Advancement of Science. It was originally prepared for publication to honor the chemist and chemical educator Ronald J. Gillespie on his 65th birthday, but was considered too historical for the special issue (November 1989) of the *Canadian Journal of Chemistry* published in his honor.
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 16. Since writing the original draft of this lecture several additional references have come to my attention. W. B. Jensen, "Classification, Symmetry and the Periodic Table," *Comp. & Maths. with Appls.*, **1986**, *12B(1/2)* 487-510, has examined the structure and development of the periodic table in the light of a wide range of contemporary mathematical concepts. These observations have been further expanded within an historical context in his *From Triads to Aufbau: Twelve Lectures on the Nature and History of the Periodic Law*, based on lectures given at the 1991 Beckman Center Workshop on the History of Chemistry and at the 1992 Woodrow Wilson Institute at Princeton and due to be published next year. Also of great interest is the paper by E. Ströker, "Die Ordnung der Elemente: Entstehung und Bedeutung des Periodensystems," in W. Sawodny, R. Opferkuch, and A. Schunk, eds., *Chemie im Spiegel der Jahrhunderte*, Universitätsverlag, Ulm, 1992, pp. 67-79.

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BOOK NOTES

Curiosity Perfectly Satisfied: Faraday's Travels in Europe 1813-1815, Edited by Brian Bowers and Lenore Symons, Peregrinus, London, 1991. xvi + 168 pp. Cloth (Typeset), \$33.00. Michael Faraday's 'Chemical Notes, Hints, Sugges-

tions and Objects of Pursuit' of 1822, Edited by Ryan D. Tweney and David Gooding, Peregrinus, London, 1991. xvii + 152 pp. Cloth (Typeset), \$55.00. *The Correspondence of Michael Faraday, Volume 1, 1811-1831*, Edited by Frank A. J. L. James, Institution of Electrical Engineers, London, 1991. xlix + 673 pp. Cloth (Typeset), \$104.00.

Yet more fallout from the Faraday bicentennial. These three very different books all treat of Faraday's formative years when, somewhat self-consciously, he was furthering his delayed education and pursuing his scientific ambitions. In *The Life and Letters of Faraday*, Bence Jones devotes 128 pages to "extracts from his journal and letters whilst abroad with Sir Humphry Davy" but he is forced to admit that "the journal ... is remarkable for the minuteness of the description of all he saw, and its cautious silence regarding those he was with." Bence Jones seemingly had access (the perfunctory preface is vague on this) to Faraday's original manuscript, now lost, as well as to the "fair copy" which survives and from which the transcript given in *Curiosity Perfectly Satisfied* is derived. As in Bence Jones, the Journal proper is fleshed out with letters Faraday wrote to friends and relatives at home. While it is good to have the complete text in print, it cannot be said that the new material does much to satisfy our curiosity about how the constant company of Davy contributed to Faraday's maturation as a scientist. His accounts of Davy's experiments and observations on iodine, hot springs, rocks, the combustibility of diamond, and so on, are strangely dispassionate. How one wishes entries such as "T. 22. The day principally employed in the laboratory. W. 23. The same as yesterday." were expanded and the often tedious baedekering reduced:

Proceeding straight from this gate into the city the eye is caught by the ruins of a dome or vault in a vineyard on the right hand. They are the remains of a temple erected to Minerva Medici or the goddess of health. It is of brick and of a diagonal form. Farther on were the ruins of the Chateau de l'eau Juilie commonly called the trophies of Marcus because ...

It is hard to decide whether such passages reflect mere boredom, unconscious plagiarism or self-mockery. Certainly Faraday was no Byron, but then Byron was no Faraday. Most of the nuggets have been previously mined by Bence Jones and L. Pearce Williams, but there are many incidental small pleasures: Faraday's enjoyment of carnivals ("went in a domino to the mask ball this morning and was much amused"); his paean to French bread ("it has a positive excellence that one would not wish it surpassed, beyond what is would be undue luxury"); and this backhanded tribute to the climate of Montpellier ("that climate which when united to the medical abilities of the faculty no disease can withstand"). Above all there is his obvious joy in minute observation of the works of nature in sharp contrast to his dutiful observation of the works of man.

For Faraday, the GLOWWORM!!! (his capitals and his exclamation points) was far more fascinating than all the architectural grandeur that was Rome.

The second of these books, the handsomely produced *Michael Faraday's 'Chemical Notes, Hints, Suggestions and Objects of Pursuit' of 1822*, is a curious piece of work. It is in three parts: a somewhat tendentious introduction (much of the substance of which has already appeared in this *Bulletin* (1991, 11, 51-55), a complete photographic facsimile of the manuscript with facing-page transcription, and an idiosyncratically interesting glossary. The editors open boldly:

The notebook that we are publishing here is a remarkable document. Kept by one of the major scientists of the nineteenth century, at a time when he had made his first important discoveries and was preparing for those major findings that would propel him into the very first ranks, it is a window into the thoughts of a scientific genius and, at the same time, a revealing portrait of the culture and community of a new century in the history of science.

and quote Faraday's own estimation approvingly:

I already owe much to these notes and think such a collection worth the making by every scientific man. I am sure none would think the trouble lost after a year's experience.

What is there to justify such a high assessment? As can be seen from the splendid photographic plates, there is little problem with Faraday's exemplary handwriting. After all, this was one reason why Davy had hired him as a temporary amanuensis. The motivation, purpose, and utility of what he wrote, however, remain obscure in spite of the editors' efforts. They have transcribed but not, or at least not completely, deciphered. Superficially we have a book of lists. At times the lists seem almost random: "Sinking of ice" comes between "Colour of eyes" and "Triple tartrate of ammonia and antimony"; "Shadows of thumb from several windows?" between "Crystallisation in Cods headbones - boil long time", and "Sol of soluble Prussian blue on yellow ferro-prussiate?" At other times the lists are tightly categorized under familiar heads - "Sulphur", "Electricity", "Organic Chemistry" - though even here Faraday's restless imagination keeps breaking the bounds. Some of the entries refer to work already in the literature (though they are rarely referenced) while others are objects to pursue. Many of the latter Faraday (and others) subsequently did pursue, but Tweney and Gooding do not attempt any kind of concordance apart from a brief "Table of Correspondences" drawn solely from Faraday's relatively rare annotations. cursory reference to *Experimental Researches on Chemistry and Physics* would reveal, for example, that "Passage of gases through tubes" had already been addressed in 1818 and "For Julin's (not Julens) chloride carbon" in 1821, while a detailed examination of "Light through liquid with precipitated gold" had to wait until

near the end of Faraday's life. Even a partial concordance with Thomas Martin's magisterial edition of the *Diary* and, through him, with Faraday's published work might reveal more of what he was about than the vague speculations on "cognitive processes" found in the introduction. What we have here is an intriguing historical/scientific mystery story and Tweney and Gooding are to be thanked for making the "documents in the case" available and for providing an interesting and novel contemporary glossary that will help the interested reader in search of clues.

With the last of these three books, there can be no quarrel. Many of Faraday's letters are readily available in print, particularly in Bence Jones and in L. Pearce Williams' *The Selected Correspondence of Michael Faraday*. For the years 1811-1831, Frank James' new compilation of all the surviving letters more than doubles the number of entries in Williams' work. While some of the additions would scarcely have been missed (e.g., letters 6, 22, 29, etc.), there is no arguing with completeness and others (e.g., letters 30, 56, 58, 105, etc.) are valuable additions to the published canon. Thirty of the letters are from the peripatetic Davy. Though couched in polite terms, these peremptory notes show that Sir Humphry often considered Faraday something of a personal lackey long after they had returned from Europe. With one exception, this one-sided exchange ended at the time of the Wollaston affair. Were there no more such notes or have they merely not survived? That Faraday treasured the first of the series is evident from letter 419. The scholarly apparatus is extensive and impeccable, though one questions the necessity of including "Newton, Isaac" sandwiched between "Newman, John" and "Nicholl, Whitlock" in the Biographical Register. Perhaps as a sign of the scholarly times, all letters in French and Italian have been admirably translated into English by the editor's wife. The book reproduces many of Faraday's spidery drawings and is illustrated by 13 black and white plates of variable quality.

This last quibble brings up my only real complaint. For a book listing at \$104, this is a mean and crabbed production, particularly when compared with Williams' splendid two-volume set and the other two books reviewed here. The dedicatee, whose own binding graces the dust-jacket of *Chemical Notes*, wrote in his travel *Journal*:

Went into the workshop of bookbinder and saw there the upper part of a fine Corinthian pillar of white marble which he had transformed into a beating stone of great beauty. Found my former profession carried on here with very little skill neither strength nor elegance being attained.

He would not be any better pleased by the present production. However, we must give thanks for present if expensive mercies and look forward expectantly to the appearance of James the Second. Derek A. Davenport, Department of Chemistry, Purdue University, West Lafayette, IN 47907.

The Green Flame: Surviving Government Secrecy, Andrew Dequasie, American Chemical Society, Washington, DC., 1991. xii + 220 pp. Cloth (Typeset), \$22.95.

This is a truly outstanding book, written by one of the participants in the U.S. government-sponsored program to develop boron-based jet fuels at the height of the Cold War in the 1950s. It has a light-hearted chatty style, which demonstrates that chemistry (and chemists) need not be boring, but it is also a serious book, packed with new information and a penetrating analysis of a major defense-related research program. *The Green Flame* is a richly textured book, with several different levels. Fundamentally, it is an account of one young chemist's personal development in the 1950s. It is also a hard-headed analysis of a government-sponsored defense-related program that was excessively dangerous and expensive, and which threatened to spiral out of control. Dequasie stresses the role of government secrecy which permeated and hindered the research program. But it is also a fine account of chemical innovation, of how a dedicated team of chemists developed a potentially viable boron-based fuel in the face of numerous technical problems and personal danger. On a more technical level, Dequasie provides many interesting facts about boron hydride chemistry. However, the main charm of this splendid gem of a book lies in its evocation of 1950s America, an era of innocence during which the goodness of the American way was unquestioned, patriotism was the supreme virtue, and researchers assumed their government knew what was best. Peter Morris, *The National Museum of Science and Industry, London, SW7-2DD, England*.

The Chemical Revolution: A Contribution to Social Technology. Archibald Clow and Nan L. Clow, Gordon and Breach, Philadelphia, PA., 1992. xx + 680 pp. Paper (Typeset), \$33.00.

This quality reprint of the classic 1952 monograph by the husband-wife team of Archibald and Nan Clow forms volume 8 of Gordon and Breach's reprint series "Classics in the History and Philosophy of Science." Readers familiar with the literature dealing with the history of chemical technology will need no introduction to this book, which was and remains the definitive study of the rise of the heavy chemical industry in Great Britain (with special emphasis on Scotland) in the period 1750-1830. As indicated by their provocative title, the Clows consider this event to be a true "industrial" chemical revolution, which paralleled the well-known academic chemical revolution born of Lavoisier and his collaborators. The quality of this reprint, including that of the numerous illustrations, is excellent, the price is reasonable, and a new introduction has been added by Frank Greenaway, formerly of the London Science Museum. On all these counts Gordon and Breach are

to be congratulated, but most of all for the simple act of making this wonderful volume available again to a new generation of chemical historians. *William B. Jensen, University of Cincinnati, Cincinnati, OH 45221.*

The French Paracelsians: The Chemical Challenge to Medical and Scientific Tradition in Early Modern France. Allen G. Debus, Cambridge University Press, Cambridge, 1991. xvi + 247 pp. Cloth (Typeset), \$64.95.

This volume represents another installment in Allen Debus's life-long commitment to the study of iatrochemistry. Beginning with his 1964 monograph, *The English Paracelsians*, and continuing through his two-volume classic, *The Chemical Philosophy: Paracelsian Science and Medicine in the Sixteenth and Seventeenth Centuries* (1977), and his later monographs, *Man and Nature in the Renaissance* (1978) and *Chemistry, Alchemy and the New Philosophy, 1550-1700* (1987), as well as in numerous articles and lectures, Debus has pursued his study of this key transitional period in the history of both medicine and chemistry with single-minded purpose and dedication. This latest volume is up to his usual high standards of scholarship and provides us with yet another key to understanding this enormously complex movement which, while challenging the rationalism of new mechanical philosophy, managed at the same time to precipitate indirectly the transition of chemistry from the very mysticism which it advocated into the modern science we know today. As with his previous studies, Debus's portrayal of the debates, which racked the French medical community from the middle of the 16th century through the early 18th century, clearly shows the complexity of this event and how unintentionally misleading are the versions which appear in the average history of chemistry text. By divorcing the purely chemical aspects of this movement from the broader medical and philosophical issues, which gave this chemical content its meaning, the resulting textbook accounts of iatrochemical discoveries and theories often make the chemistry appear disjointed, haphazard and idiosyncratic.

One interesting aspect of this new volume is the use of a large number of high-quality line drawings and portraits which have been integrated into the text of the book. Hopefully this is a positive sign that the example set by the *Bulletin for the History of Chemistry* has made historians more sensitive to the use of such pictorial materials than has been the case previously, though perhaps we are immodestly taking more credit than we deserve. In any case, this is an excellent book which belongs on the shelves of all serious students of the history of alchemy, chemistry, and medicine. *William B. Jensen, University of Cincinnati, Cincinnati, OH 45221.*

Enlightenment Science in the Romantic Era: The Chemistry of Berzelius and Its Cultural Setting. Edited by Evan M. Melhado and Tore Frängsmyr, Cambridge University Press, Cambridge, 1992. xiv + 246 pp. Cloth (Typeset), \$49.95.

As is all too frequent with academic books these days, and especially with those in the humanities, the true content of this volume is revealed by its subtitle rather than its title and consists of a series of essays, by a distinguished team of chemical historians, dealing with various aspects of the life and science of Jöns Jakob Berzelius (1779 -1848), who was, without doubt, one of most influential European chemists of the first half of the 19th century. In addition to a brief introduction by the editors, the volume contains nine essays: "Berzelius and His Time" by Sten Lindroth; "'Truth, the Angel of Light': Berzelius, Agardh, and Hwasser" by Sven-Eric Liedman; "Berzelius and the Atomic Theory: The Intellectual Background" by Gunnar Eriksson; "Berzelius, Dalton, and the Chemical Atom" by Anders Lundgren; "Berzelius's Animal Chemistry: From Physiology to Organic Chemistry (1805-1814)" by Alan Rocke; "Novelty and Tradition in the Chemistry of Berzelius (1803-1819)" by Evan Melhado; "Berzelius as Godfather of Isomorphism" by Hans-Werner Schütt; "Berzelius, the Dualistic Hypothesis, and the Rise of Organic Chemistry" by John Hedley Brooke; and "Berzelius as a European Traveler" by Carl Gustaf Bernhard.

In keeping with current trends in the history of science, most of the essays tend to stress the details of the social, cultural, and philosophical context of Berzelius's life rather than the experimental and conceptual details of his science. Also surprising, at first glance, is the absence of a separate essay dealing with his electrochemical theory, though aspects of it (particularly its taxonomic consequences) are touched on in several of the essays, most notably those by Melhado and Brooke. However, this omission becomes understandable when one realizes that this topic was covered in great detail in Evan Melhado's 1981 monograph, *Jacob Berzelius: The Emergence of his Chemical System*, and that it is the intent of the present volume to fill in the details of Berzelius's life and work and, in particular, to examine its larger cultural context, rather than to present a comprehensive overview.

Though all of the essays are well done and highly informative, especially for a reader already familiar with the basic outline of Berzelius's life, and the technical production of the book is excellent, it is nevertheless difficult for the English-speaking reader, in the absence of an English translation of H. G. Söderbaum's monumental three-volume biography of Berzelius, to accurately gauge the novelty of those essays which focus on the biographical details of his life. Unhappily, the translation of foreign monographs, however important, does not form a part of the Weltanschauung of the present generation of historians of science, so that it is highly probable and highly regrettable that historians and chemists who cannot read

Swedish will continue, for the foreseeable future, to be denied access to what is certainly the single most important source of information on Berzelius. In the meantime, this small volume will do much to fill the void for the English-speaking reader. *William B. Jensen, Department of Chemistry, University of Cincinnati, Cincinnati, OH 45221.*

Eilhard Mitscherlich: Baumeister am Fundament der Chemie. Hans-Werner Schütt, Deutsches Museum in Kommission bei Oldenbourg Verlag, Munich, 1992. 192 pp. Cloth (Typeset), NP.

Schütt has here provided us with one of the very few full modern biographies of a 19th-century German chemist. Although there is much on his subject's well-known "difficult" personality, Schütt maintains his principal focus on career matters - isomorphism remained Mitscherlich's greatest discovery, but numerous discoveries in organic, inorganic, geological, and biological chemistry flowed regularly out of his Berlin laboratory. As one of the most significant chemists of the century, Mitscherlich deserves such attention, and Schütt has spared no pains to give us a detailed and accurate vision of the man and his work. *Alan J. Roche, Department of History, Case Western Reserve University, Cleveland OH 44106.*

Enjoying Organic Chemistry, 1927-1987, Egbert Havinga, American Chemical Society, Washington, DC, 1991. xvii + 122 pp. Cloth (Typeset), \$24.95.

Sadly, Egbert Havinga's death in 1987 cut short a brilliant career and caused this book to be the first posthumous publication in this special series published by the American Chemical Society. Far more than simply a hard-working scientist, Havinga was widely admired for his humane and gentle nature, for his wide knowledge of arts and letters, for his nearly inexhaustible storehouse of chemical information, and for his deep humility that made him constantly praise the efforts of his many students while always down-playing his own achievements. The long exciting tradition of Dutch scholarship and intellectual curiosity was never better served than by Havinga.

As with so many individuals involved in this series, a very significant portion of Havinga's career dealt with stereochemical themes. In keeping with such a focus, Havinga's first staff position was in the Veterinary Faculty of the University of Utrecht, in precisely the same building where "van't Hoff had found his Pegasus," to paraphrase the now ironic sarcasm once leveled by Kolbe against van't Hoff. As Havinga's research program progressed, a style of work emerged that cleverly combined both physical studies and classical organic chemistry. Beginning with his own doctoral research that explored the stereochemical implications of chemistry in monolayers (still

an intriguing and far from well-understood area with important biophysical implications) and followed by bold forays into the realm of spontaneous resolution of racemic mixtures, the maturing independent stereochemical work of Havinga followed a distinctly personal pathway. Long before it became fashionable, Havinga embarked on a research program in which each doctoral student not only had to prepare a series of target substrates exhibiting systematic variation in structure, but then had to subject this collection of compounds to rigorous scrutiny by the most sophisticated physico-chemical instrumentation currently available. The result was a treasure trove of data showing how properties such as density, dipole moment, infrared absorption, and electron diffraction patterns, were a function of a particular compound's three-dimensional structure.

Many aspects of alicycles that are now taken so completely for granted as worthy of mention only at the most elementary level in our standard undergraduate textbooks were first explored and established by the work of Havinga and his school. Given the somewhat reclusive character of Havinga's contemporary, the Norwegian chemist, O. Hassel, the mainstream popularity of Havinga's efforts helped secure a much more appreciative audience for alicyclic stereo-physical properties. In addition, such studies helped lay the foundations of conformational analysis as later developed by D.H.R. Barton. Havinga's systematic investigations of five-, six-, seven-, and eight-membered rings uncovered many surprising phenomena, such as the still intriguing "anomeric effect" that is of such importance in carbohydrate chemistry. While there was an initial desire to examine alicyclic compounds that were directly related to common natural product examples, the investigations gradually achieved their own momentum. Even today, force-field calculations and NMR studies of alicycles continue to excite the curiosity of investigators. Extremely weak intermolecular interactions, such as the "benzene effect" (p. 16), still defy complete understanding.

In following Havinga's discussion of how his research interests evolved, one cannot escape the sophisticated and fascinating marriage that he achieved between precise measurement of apparently obscure physical characteristics and phenomena, coupled with his being able to come up with valid and generally applicable rationalizations as to what was being observed. A common criticism of organic chemistry in the past was that the discipline resembled a colossal slag heap of unrelated facts. Individuals such as Havinga have helped enormously to make such notions less tenable and thereby have helped to transform organic chemistry into a vibrant, mature discipline with its own internal logic and excitement.

One of the greatest intellectual advances in chemistry, the conservation of orbital symmetry, enunciated so powerfully by Woodward, Hoffmann, Fukui, and others, had a number of critical antecedents in the now classic studies of Havinga on Vitamin D. Havinga's series of superbly detailed experimental

publications on the thermal and photochemical transformations of Vitamin D analogs provides an excellent complement to the work of Woodward and Eschenmoser on the synthesis of Vitamin B-12. Together, both projects served as a "prompt" to focus attention on electrocyclic reactions. A powerful lesson has been provided to the scientific community demonstrating how general interest in a particular compound (usually due to some valuable biological activity) can draw researchers into a very detailed examination of esoteric and unusual behavior. From such studies, there emerge deeper insights able to propel all of chemistry forward - in a sense, a kind of cosmic reward for a seemingly micro-reductionist focus.

At a time when photochemistry was still an emerging discipline, it must have taken real courage for Havinga to have made such a commitment to the Vitamin D problem. Nevertheless, once he began, Havinga prevailed. Observation followed observation until, with the able assistance of his collaborator, the brilliant theoretician Oosterhoff, in 1961 the importance of orbital symmetry gained recognition. The attention to detail that was so characteristic of Havinga is what really made the difference. If one wanted to press an analogy, Havinga's focus on product analysis of seemingly nearly identical materials and a desire to explain their rational formation reminds one of Pasteur's efforts to understand the nature of racemic tartaric acid. For this system, Pasteur was only able to make progress when microscopic examination of the crystalline racemic tartrate revealed the previously overlooked presence of a random jumble of enantiomeric crystals.

As is so common with many "giants" of organic chemistry, Havinga had a pronounced tendency to return again and again to the same problem - a kind of intellectual willingness to mine the "Mother Lode." Careful analysis of the Vitamin D by-products had revealed the presence of a large number of intriguing materials. Obviously tedious isolation of by-products present only in ultra-trace quantities presented a serious challenge even in regards to collection of sufficient material (i.e. a few milligrams) for spectroscopic characterization. This greatly complicated the difficulty in arriving at unambiguous structures for these materials. Why are the obscure by-products present? A lesser chemist might have ignored this loose end but Havinga's attention to detail encouraged his own group and others to press forward. Ultimately, knowledge of the structures of such Vitamin D "molecular mutants" as the toxisterols and the suprasterols has given chemists valuable mechanistic insights into what is actually a very complex photochemical system. Recognition of the inherent complexity of much chemical behavior may be a good sign that, after hundreds of years, chemistry may finally be reaching maturity and can indeed explain the "real world."

Coming into organic photochemistry at its infancy, Havinga's group made another major discovery by their serendipitous observation of nucleophilic aromatic photosubstitution. From their first observations in 1954 and throughout the late

1960s, these Dutch chemists carried out an extensive exploration of the scope and mechanism of this reaction class. The persistent attention to detail that Havinga had originally given to his Vitamin D work was profitably transferred to this classic investigation. Studies on aromatic photosubstitution allowed Havinga to probe fundamental aspects of photochemistry, such as the importance of triplet and singlet species and the ability to create reactive transient intermediates via high intensity nanosecond flashes. All of this was done within the context of reactions that were similar to the familiar ground state chemistry known to every organic chemist. As always, Havinga's photosubstitution work exhibits his own special style, an initial spartan simplicity that made possible seminal insights only to be followed by additional detailed studies that cleared up any remaining confusion. Always, his experiments were carefully crafted with great elegance and relevance.

Even in what appears to be a "minor" investigation - the study of nitroso dimerization - Havinga displays his characteristic ability of drawing the reader into a shared interest in substrate-dependent reactivity and mechanistic subtlety. His work always piques our curiosity. Forever present is an intriguing virtue, perhaps best described as the quality of being a "natural scientist," that is found only in a very small percentage of practicing academic and industrial scientists. These "naturals" are the men and women who have a remarkable knack for extracting something very interesting from even the most seemingly mundane scientific study. To reverse-paraphrase, "they look locally and perceive globally." Havinga serves as an archetypical example of just such an individual.

In his last major research effort, Havinga brought together his interest in all aspects of physical organic phenomena to probe the mechanistic features of the structurally well-characterized enzyme ribonuclease. As one of the first enzymes to have its structure elucidated, ribonuclease has long fascinated chemists. How can one make the leap from a hodge-podge of atoms somewhat resembling pizza dough onward to proposing the mechanism of action of a highly efficient catalyst? Without any convenient way to capture the enzyme-substrate activated complex, chemists have been forced to rely upon completely indirect evidence. The synthesis of numerous analogs and the systematic investigation of trends in their catalytic behavior was the key employed by Havinga as well as many other early participants in this field. As a mature artist and craftsman of science, it would have been out of character for Havinga to have taken any short-cuts. Instead, by adopting a courageously bold frontal assault, he prepared the requisite substrates and was indeed able to propose a credible mechanism. Much work has led to much understanding. The message: no pain, no gain.

Personal characteristics of Havinga that are showcased within this book include the humility, gentleness, humane sensitivity, and enormous decency that, from all indications, were essential features of this man's behavior. Even to the extent of almost always citing his graduate students' efforts in

lieu of his own contributions, Havinga exhibited a rare degree of intellectual generosity. His discussions of the various cultural and non-scientific activities reveal a widely read, highly philosophical mindset. From his youth on into his mature years, this man maintained and cultivated an enormously wide circle of loyal friends who greatly appreciated his intellect and strength of character. These were friendships that persisted for decades. As might have been expected out of respect for such a noble individual, one of Havinga's colleagues, Harry Jacobs, in what was basically a labor of love, helped to guide the extant manuscript into an effective posthumous document. In a touching, but very characteristic fashion, Havinga pays tender tribute at the end of his book to his partner for life, his beloved wife, Louise D. Oversluys, by stating that he would follow exactly the same career path all over again but only if he could once more enjoy her companionship, moral support, and spirit of adventure. *John Belletire, Department of Chemistry, University of Cincinnati, Cincinnati, OH 45221.*

LETTERS

The Stereochemistry of Benzene

I should like to say how much I appreciated Dr. Paoloni's nonpolemical article on "Stereochemical Models of Benzene, 1869-1875" (*Bulletin*, 1992, 12, 10-24). I was pleased to see this area of research pursued since these questions were the ones that got me started in my research in the history of stereochemistry while an NSF Science Faculty Fellow at the University of Wisconsin working with Aaron Ihde in 1968-69. How chemists attempted to reconcile the planar structure of benzene with van't Hoff's tetrahedral carbon atom becomes even more interesting when you examine the numerous "space filling" models of benzene proposed in the latter part of the 19th and early 20th century. The problem is also entangled with the early development of conformational analysis.

I would also like to offer my congratulations on this excellent publication. I can remember the frustration many of us HIST members experienced in the 1980s as we struggled to find ways of raising the \$10,000-20,000 required to start and publish a journal to replace *Chymia*. We never found the money. Desktop publishing, Bill Jensen, and a newly "professional" group of HIST members arrived to revitalize what had been a somewhat moribund activity of the American Chemical Society. *Bert Ramsay, Eastern Michigan University*

Chemical Slide Rules

I am sure William Williams' article on "Some Early Chemical Slide Rules" (*Bulletin*, 1992, 12, 24-29) will elicit many comments and contributions of additional "chemical" slide rules. An early 20th century German-made and designed slide rule

"nach Dr. Tisza" distributed in the USA by the Scientific Materials Company of Pittsburgh, PA was introduced as part of a promotional brochure (for my "chemical" calculator) at the August 1993 meeting of the American Chemical Society. The slide rule in question contained a number of element symbols and chemical formulas to be used to calculate chemical "Equivalencies." The major limitation of the traditional "chemical" slide rule was that there simply was not enough space to place all of the formulas for which one might have wished to calculate a formula mass. This limitation, as well as a number of other shortcomings of slide rules in general, has been overcome with my patent (pending?) chemical calculator. The chemical formula, as well as chemical equations, can be easily "written" from the periodic table keypad which then carries out the required series of operations to calculate the formula mass. [Editor: Further information about Dr. Ramsay's calculator can be obtained by writing to him at his company: Chemical Concepts Corporation, 912 North Main Street, Ann Arbor, MI 48104] *Bert Ramsay, Eastern Michigan University*

* Since the publication of my article on "Some Early Chemical Slide Rules" (*Bulletin*, 1992, 12, 24-29) I have uncovered several additional references and have also received several letters from readers containing further information on the locations of existing examples, which I have summarized below:

A. John Johnston's *Manual of Chemistry* (Thomas Cowperthwait, Philadelphia, 1846, p. 133) mentions the Beck-Henry scale.

B. Professor B. P. Huddle, of Roanoke College, Salem, VA, sent photocopies of an "Ashley" rule in his possession. The photocopy of his rule shows what was unclear on the illustration used in the *Bulletin*; the manufacturer was Keuffel & Esser Company and it carried patent dates of 5 June 1900 and 22 December 1908. A good magnifying glass on the "Ashley" illustration in the 1914 E. H. Sargent catalog revealed that it was indeed made by K & E. Professor Huddle's rule presented formulas for "ACID, BASE, SALT," on one side, while the other side listed "OXIDE, ELEMENT."

C. David J. Bryden, of the Royal Museum of Scotland in Edinburgh, sent several pertinent items about different chemical slide rules:

i) In 1834, Carpenter's Chemical Warehouse, 301 Market Street, Philadelphia, published a "Catalogue of Chemical and Philosophical Apparatus, Utensils and Materials, manufactured by a distinguished artist of this city." (G. Carpenter, *Essays on some of the most important articles of the Materia Medica ...*, G. W. Carpenter Chemical Warehouse, Philadelphia, 1834, p. 285). Among the items in that catalog was a "Sliding Scales of Chemical Equivalents, in which oxygen is called eight, as taught in the schools of America."

ii) There is a chemical equivalents rule produced by Newman, the instrument maker at the Royal Institution, in the Whipple

Museum of the History of Science at Cambridge, England.
iii) The Playfair Collection in the Royal Museum of Scotland once held four different models of the Wollaston scale. Sadly, they were listed as missing in 1978 when R. G. W. Anderson published *The Playfair Collection and the Teaching of Chemistry at the University of Edinburgh 1713-1858* (The Royal Scottish Museum, Edinburgh, 1978, p. 153).

D. In the above publication Anderson included the following comments and references concerning the scales:

Thomas Charles Hope demonstrated a variety of scales (2) to his classes. His manuscript "List of Specimens" (used by his assistant for preparing apparatus for demonstration at Hope's lectures) includes four: "Dr. Wollaston Table Cheml Equivalents - Dr. Dewars - Dr. Reids - Prideaux"(3). These four may be those which once formed part of the Playfair Collection.

Scales bases on the diagram in Wollaston's original paper seem to have been relatively common, (4). On the other hand, no examples of Dewar's, Reid's or Prideaux's tables appear to have survived ... John Prideaux's scale of 1830 was complex, including symbols for about 500 substances. It was based on oxygen = 1 and was doubled, opening on hinges like a book (6). Dr. Dewar's is not easily traceable. The reference may be to a scale (unpublished) by Henry Dewar, M.D. (7).

2. Various forms of instrument are discussed by D. C. Goodman, "Wollaston and the Atomic Theory of Dalton," *Historical Studies in the Physical Sciences*, 1, 37 (1969). Michael Faraday, *Chemical Manipulation* (London, 1827, 551) warned on practical grounds that the instrument was not dependable: "It is almost impossible that the scales should be accurate, because of the extension and contraction of the paper when it is damped, and again dried, and the facility with which it yields to mechanical impressions."

3. Edinburgh University Library MS Gen 270, "List of Specimens," under heading "Chemical Action."

4. There are three examples at the Science Museum, London (inv no 1932-578, see A. Barclay, *Handbook of the Collections Illustrating Pure Chemistry* (London 1937), 13), two examples at Harvard (in my *Bulletin* paper), and one example at the Museum of the History of Science, Oxford (see C. R. Hill, *Museum of the History of Science Catalogue 1* (Oxford 1871) 42, item 292).

6. J. Prideaux, "Continuation of the Table of Atomic Weights, and Notice of a New Scale of Equivalents," *The Philosophical Magazine*, 8, 423 (1830).

7. H. Dewar, "The Influence of Chemical Laws on the Phenomena of Physiology," *Edinburgh Medical and Surgical Journal*, 17, 479 (1821).

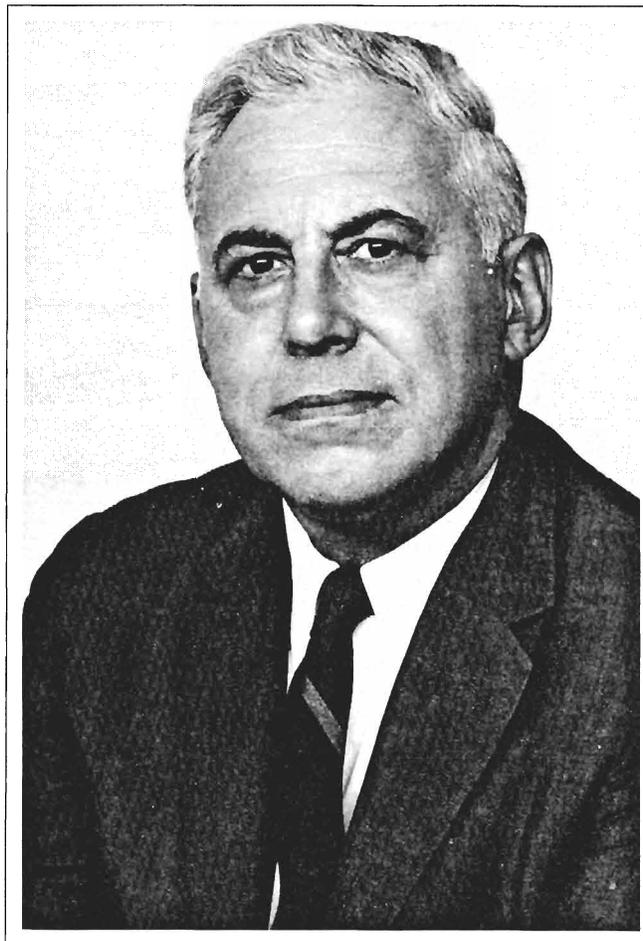
Readers with further information concerning chemical slide rules should contact either me or Bill Jensen. *William D. Williams, Harding University*

AWARDS

The Dexter Award

The 1993 Dexter Award for outstanding accomplishment in the history of chemistry has been awarded to Dr. Joseph S. Fruton of Yale University for his work on the history of biochemistry. The award, which consists of a cash prize of \$2000 and an engraved plaque, was presented at the Fall National ACS Meeting in Chicago, IL in August of 1993.

Born in Czestochowa, Poland, on 14 May 1912, Dr. Fruton became a naturalized U.S. citizen in 1929. He received his B.A. in chemistry with honors from Columbia College in 1931, and his Ph.D. in Biological Chemistry from Columbia University in 1934. From 1934-1945 he was associated with the Rockefeller Institute for Medical Research. In 1945 he became Associate Professor of Physiological Chemistry at Yale University, followed by promotion to Professor of Biochemistry in 1950, and appointment as Eugene Higgins Professor of Biochemistry in 1957. In 1980, Dr. Fruton was also appointed Professor of the History of Medicine at Yale, and since 1982 he has



Dr. Joseph S. Fruton

served as Professor Emeritus.

A distinguished biochemist who has authored over 300 papers dealing with the chemistry of proteins, peptides, amino acids, and the specificity and mechanism of proteolytic enzymes, as well as co-author, with his wife, Sofia Simmons, of a well-known textbook of biochemistry, Dr. Fruton has received many honors and awards, including election to the National Academy of Sciences in 1952, election to the American Academy of Arts and Sciences in 1953, and an honorary doctorate from Rockefeller University in 1976.

Dr. Fruton's interest in the history of chemistry and biochemistry dates back to his early years at Yale. One of his first publications in the field was a 1950 review of volumes 1 and 2 of the Division's first historical journal, *Chymia*, written for the *Yale Journal of Biological Medicine*. Since then he has regularly contributed book reviews and biographical articles to various journals, dictionaries, and yearbooks, and especially to the splendid *Dictionary of Scientific Biography*, edited by Charles Gillispie, and published by Scribners in the 1970s.

In 1972 Dr. Fruton's first book on the history of biochemistry, *Molecules and Life: Historical Essays on the Interplay of Chemistry and Biology*, was published by Wiley-Interscience. In 1974 he published a *Selected Bibliography of Biographical Data for the History of Biochemistry since 1800*. This appeared as an expanded second edition in 1977 and was expanded yet

further in 1982 under the title *A Biobibliography for the History of the Biochemical Sciences Since 1800*. A supplement was also published in 1985. In 1990 Dr. Fruton's third book, *Contrasts in Scientific Style: Research Groups in the Chemical and Biochemical Sciences*, was published, followed by his most recent book, *A Skeptical Biochemist*, published by Harvard University Press in 1992.

The Division would at this time also like to solicit nominations for the 1995 Dexter award. Nominations should include a complete vita for the nominee, consisting of biographical data, educational background, awards and honors, publications, presentations and other services to the profession; a nominating letter summarizing the nominee's achievements in the field of the history of chemistry and citing unique contributions which merit a major award; and at least two seconding letters. Copies of no more than three publications may also be included, if available. All nominations should be sent in triplicate to Dr. Alan J. Rocke, Chairman of the Dexter Award Committee, Program for the History of Science and Technology, Case Western Reserve University, Cleveland OH, 44106 by 10 January 1995. It should be emphasized that the award is international in scope and that nominations are welcomed from all quarters. Previous winners have included historians and chemists from Germany, France, Holland, Hungary, and Great Britain.

The Outstanding Paper Award

The 1993 Outstanding Paper Award has been given to Dr. June Z. Fullmer of the Department of History of Ohio State University and to Dr. Melvyn C. Usselman of the Department of Chemistry of the University of Western Ontario for their joint paper, "Faraday's Election to the Royal Society: A Reputation in Jeopardy," which appeared in the Winter 1991 (No. 12, 17-28) issue of the *Bulletin*. The award, which consists of \$100, a plaque, and \$150 worth of books from University of Pennsylvania Press, was presented in absentia to the authors at the Fall National ACS Meeting in Chicago, IL in August of 1993.

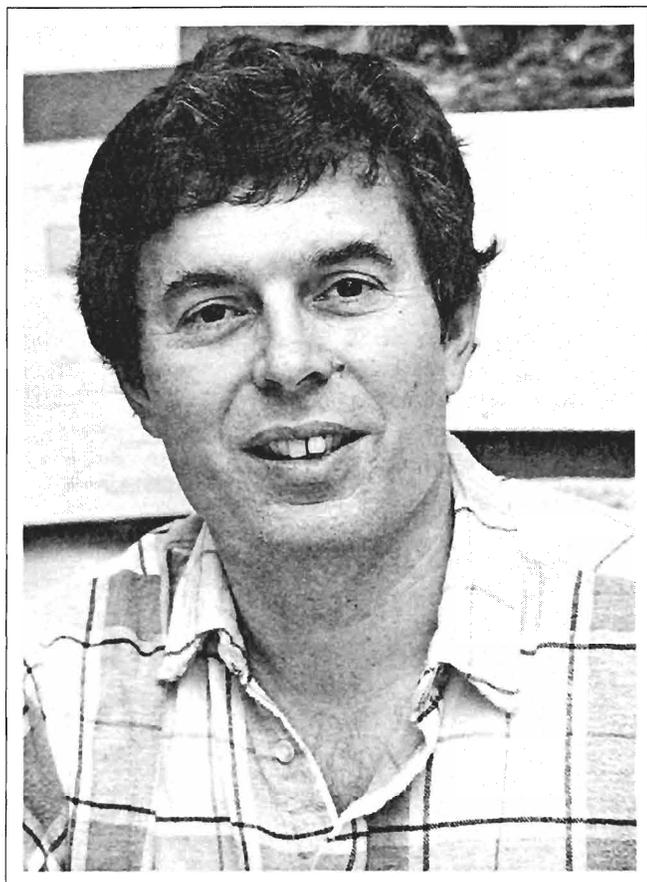
An internationally-known authority on the life and times of Sir Humphry Davy, Dr. Fullmer is the author of nearly 100 papers and reviews and of the monograph, *Sir Humphry Davy's Published Works* (1970). She is also currently working on a biography of Davy as a young man and is editing the collected letters of Davy and his wife Jane. Dr. Usselman is best known for his work in replicating the early experimental work of William Hyde Wollaston, Thomas Thomson, and John Dalton. He is currently working on a biography of Wollaston.

The Edelstein International Fellowship and Studentship

The 1993-1994 Edelstein International Fellowship in the History of the Chemical Sciences and Technologies has been awarded to Dr. Owen Hannaway of Johns Hopkins University. Because



Dr. June Z. Fullmer



Dr. Melvyn C. Usselman

of illness, Dr. Hannaway was unable to accept the Fellowship, which he had intended to use in continuing his work on Georgius Agricola and on the history of early chemical laboratories. The 1993-1994 Edelstein International Studentship has been awarded to James Altena of the University of Chicago, who has used it to pursue work related to his doctoral dissertation on the "Energism of Wilhelm Ostwald: Science, Philosophy and Social Reform in Imperial Germany." Dr. Altena's year was divided between the Beckman Center for the History of Chemistry in Philadelphia and the Edelstein Center for History and Philosophy of Science, Technology, and Medicine at the Hebrew University in Jerusalem, Israel.

EVENTS OF INTEREST

* A symposium on the "History of Chemistry in the Pacific Northwest" will be held at the 49th Northwest Regional Meeting (NORM 94) of the American Chemical Society in Anchorage, Alaska on 17-19 June 1994. For further information, contact Dr. Richard Rice, Department of Chemistry, The University of Montana, Missoula, MT 59812, (406) 243-4022, FAX (406) 243-4227.

* Wilhelm Lewicki, a direct descendant of Justus von Liebig, has recently established a *Liebig-Wöhler-Freundschafts Preis* for outstanding research in the history of chemistry relating to the careers of either Liebig or of Wöhler. The sum of DM 2000 will be administered by the Göttinger Chemische Gesellschaft Museum der Chemie. For further information, contact Dr. Herbert W. Roesky, Tammannstrasse 4, D-37077, Göttingen, Germany.

* The Fachgruppe Geschichte der Chemie of the Gesellschaft Deutscher Chemiker (GDCh) has announced that it will receive applications for the 1995 *Bettina Haupt Förderpreis für Geschichte der Chemie*. The prize will awarded be at the 18 March 1995 meeting in Bonn and applications are due by 1 October 1994. For details please contact Professor Christoph Meinel, Lehrstuhl für Wissenschaftsgeschichte, Universität Regensburg, 93040 Regensburg, Germany.

* Gordon and Breach have recently reprinted Robert Multhaupt's 1966 classic *The Origins of Chemistry* as Volume 13 of their series "Classics in the History and Philosophy of Science."

* The Royal Society of Chemistry has recently issued four wall charts dealing with the history of chemistry: "The Origins of Organic Chemistry 1800-1900," "Chemical Atomic and Molecular Theory 1800-1900," "Industrial Chemistry 1800-1900," and "Analytical Chemistry 1800-1900." Each chart is 640 x 900 mm in size and is printed in full color. For further information, contact The Royal Society of Chemistry, Turpin Distribution Service Limited, Blackhorse Road, Letchworth, Herts SG6 1HN, United Kingdom. An advertisement in the March 1993 issue of the *Journal of Chemical Education* (p. A59) also indicates that Mallincrodt has reissued its 1969 "History of Chemistry Chart" for \$24.95.

* John Park has called our attention to a new electronic journal in the history of science called HOST (for History of Science and Technology). The journal is published twice a year and is free. Just send an e-mail request to Julian Smith of the Institute for the History and Philosophy of Science at the University of Toronto (jsmith@epas.utoronto.ca). Be sure to include your e-mail address.

* A special exhibit commemorating the 500th anniversary of the birth of Paracelsus will be held at the Washington University School of Medicine Library from March-July 1994 in connection with the Robert E. Schlueter Paracelsus Collection, which is on deposit with the library's Archives and Rare Books Division from the St. Louis Metropolitan Medical Society. Similar exhibits were held last year at the National Library of Medicine in Bethesda MD and at the library of Hahnemann University in Philadelphia. A booklet entitled *Paracelsus and the Medical Revolution of the Renaissance: A 500th Anniversary Celebration* has also been prepared by Dr. Allen Debus of the Morris Fishbein Center of the University of Chicago.

* Four historical displays developed by the Chemical Heritage Foundation are now available for use by academic insti-

tutions, museums, science centers and corporations for the cost of shipping the display to and from the borrowing institution. The currently available titles include "Scaling Up," "Structures of Life," "Polymers and People," and "R. B. Woodward and the Art of Organic Synthesis." For further information contact "Exhibits," CHF, 3401 Walnut Street, Suite 460B, Philadelphia, PA 19104-6228, (215) 898-0081.

* Travel grants are available from the Chemical Heritage Foundation to enable interested individuals to visit Philadelphia to make use of the Othmer Library of Chemical History, the Edgar Fahs Smith Collection, and other associated facilities. The grants, which may be used for travel, subsistence, and copying costs, will not normally exceed \$500. Applications should include a vita, a one-paragraph statement on the research proposed, a budget, and the addresses and telephone numbers of two references. Deadlines are 1 February for grants covering the period April-June, 1 May for July-September, 1 August for the period October-December, and 1 November for the period January-March. Send applications to "Travel Grants," Pamela Sanders, The Chemical Heritage Foundation, 3401 Walnut Street, Suite 460B, Philadelphia, PA 19104-6228, (215) 898-0081.

* The Oesper Collection in the History of Chemistry of the University of Cincinnati is looking for donations of old chemistry texts, photographs, prints, molecular models, and chemical apparatus to add to its collections. Interested parties should contact Dr. William B. Jensen, The Oesper Collection in the History of Chemistry, Department of Chemistry, ML 172, University of Cincinnati, Cincinnati, OH 45221.

FUTURE MEETINGS

Washington DC ... 21-25 August 1994

* *General Papers.* Contact M. D. Saltzman, Department of Chemistry, Providence College, Providence, RI, 02918, (401) 865-2298.

* *The 75th Anniversary of IUPAC,* Contact M. D. Saltzman (see address above).

Anaheim ... 2-7 April 1995

Four copies of 150-word abstract on ACS Abstract Form by 1 January 1995. Title of paper by 1 December 1994.

* *General Papers.* Contact M. D. Saltzman (see address above).

* *Archaeological Chemistry.* Contact M. V. Orna, Depart-

ment of Chemistry, College of New Rochelle, New Rochelle, NY, 10801, (914) 654-5302.

Chicago ... 20-25 August 1995

Four copies of 150-word abstract on ACS Abstract Form by 1 May 1995. Title of paper by 1 April 1995.

* *General Papers.* Contact M. D. Saltzman (see address above).

* *75th Anniversary of the Division of the History of Chemistry.* Contact J. J. Bohning, Chemical Heritage Foundation, 3401 Walnut Street, Philadelphia, PA, 19104, (215) 898-1302.

* *Centenary of the ACS Chicago Section.* Contact J. J. Doheny, 3625 McCormick Ave., Brookfield, IL, 60513.

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Pierre Laszlo
- * "A Letter from Franklin Bache to Robert Hare"
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- * "The Chemist as Consultant in Gilded Age America"
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Mary R. S. Creese and Thomas M. Creese
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Martin D. Saltzman
- * The 1993 Dexter Address: "Thomas Burr Osborne and Protein Chemistry"
Joseph S. Fruton
- * "Fritz Feigl (1881-1971): The Centennial of a Researcher"
Alda Espinola, Mário Abrantes da Silva Pinto, and Claudio Costa Neto
- * "Aleksandr Mikhailovich Zaitsev (1841-1910): Markovnikov's Conservative Contemporary"
David E. Lewis
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