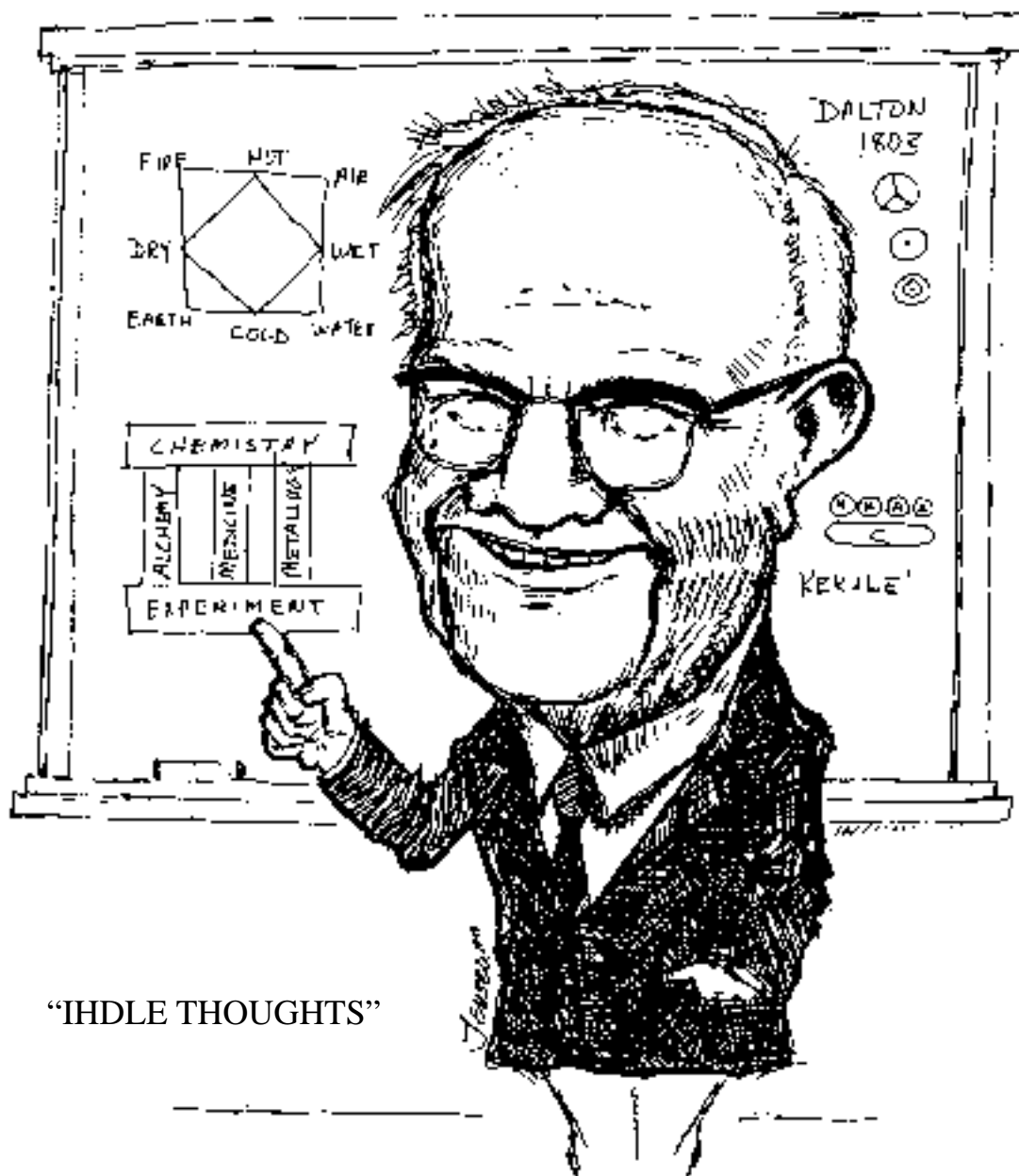


BULLETIN FOR THE HISTORY OF CHEMISTRY

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"IDLE THOUGHTS"

BULLETIN FOR THE HISTORY OF CHEMISTRY

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AARON JOHN IHDE 1909 – 2000

INTRODUCTION

Aaron John Ihde, one of the major 20th-century figures in the history of chemistry, passed away on February 23, 2000 in Sarasota, Florida at the age of 90. Born on December 31, 1909, the son of second-generation immigrants, Aaron was raised on a farm near Neenah, Wisconsin. Following an elementary education in a typical one-room, "K through 8," rural school house and graduation from high school, he entered the University of Wisconsin as an undergraduate in 1927, from which he received his B.S. in chemistry in 1931.

Though his original intention was to be a teacher, the economic realities of the depression caused him to accept a job with the Blue Valley Creamery Company of Chicago after graduation. It was while working in Chicago that Aaron married Olive Tipler, a former high school classmate and a teacher of Latin and history. The marriage produced two children: a daughter (Gretchen) and a son (John). However, the intellectual limitations of industrial employment at a one-product company with no real research and development program soon became apparent, and in 1938 Aaron returned to the University of Wisconsin to pursue graduate work in chemistry. In 1941 he received a doctoral degree for research done in the field of food chemistry under the direction of Professor Henry Schuette, with a minor in biochemistry done under the direction of Professor Harry Steenbock.

After teaching at Butler University for a year, Aaron returned to the University of Wisconsin once again, this time as an instructor in freshman chemistry with a one-year, renewable contract. He proved to be an excellent teacher. Not only was his contract renewed twice, in

1945 he was moved to a tenure-track position within the Chemistry Department. Here he would remain until his retirement as Professor Emeritus in 1980.

Aaron's progressive involvement in the history of chemistry occurred in three stages. In 1946 he was granted permission to revive a long defunct history of chemistry course within the chemistry department. He not only revived it, he eventually expanded it to two semesters. Many of his most important books and papers evolved out of his teaching commitment to this course. Stage 2 occurred in 1948 when he was asked to participate in the newly founded Integrated Liberal Studies (ILS) Program, where he initiated an historically based approach to the teaching of physical science called "The Physical Universe." Further inspiration for the ILS program came in 1951-1952 when he was appointed as a Carnegie Intern at Harvard University. Here he was able to study first-hand the "Historical Case Studies Approach" to integrating the teaching of science and history initiated by James B. Conant, Leonard Nash, and Thomas Kuhn, and was also able to interact with such historians as George Sarton, I. B. Cohen, and Gerald Holton. The third and final stage came in 1957 when he officially received a joint appointment in the History of Science Department. Though Aaron had begun directing doctoral theses with a heavy history of chemistry component as early as 1952, the connection with the History of Science Department legitimized his involvement at the graduate level. Eventually he would direct the graduate studies of 21 students and would also interact with several postdoctoral fellows and visiting pro-

fessors seeking to extend their knowledge of the history of chemistry. This development, more than any other, provided him with the opportunity to pursue history of chemistry as a full-time professional commitment, rather than as a part-time hobby typical of so many chemists with historical interests.

A long-time member of HIST, Aaron served as divisional chair from 1962-1964. In 1968 he received the Division's Dexter Award for his work in the history of chemistry. Other honors included the University of Wisconsin Chancellor's Award for Distinguished Teaching in 1978, election to fellowship in the American Association for the Advancement of Science, and service as President of the Wisconsin Academy of Science, Arts, and Letters.

This special issue of the *Bulletin for the History of Chemistry* is dedicated to the memory of Aaron J. Ihde. The lead article by Jim Bohning, based on interviews

conducted in 1983, summarizes in Aaron's own words many of his early experiences at the University of Wisconsin and especially those related to his involvement in the ILS program. The article by Alan Rocke deals with Aaron as a teacher, not only of undergraduates but of doctoral students, and delineates his influence on a generation of chemical historians. The article by Bill Jensen summarizes Aaron's books and papers in the history of chemistry, while that by Robert Siegfried, Aaron's first doctoral student and fellow colleague in the History of Science Department at the University of Wisconsin, provides a more personal tribute. The special section on Ihde closes with a posthumous article by Aaron himself on the subject of chemical genealogies based on a paper that he contributed to a 1992 ACS symposium in San Francisco.

William B. Jensen, Guest Editor
Paul R. Jones, Editor

NOTE FROM THE EDITOR

The first portion of this issue is dedicated to the memory of Aaron J. Ihde (1909-2000), widely recognized historian of chemistry, recipient of the HIST Dexter Award, and devoted teacher. I want to express my gratitude to those who have contributed articles and, in particular, to William B. Jensen, Guest Editor and Founding Editor of the *Bulletin*, who coordinated the project. All photographs are courtesy of the Oesper Collection, University of Cincinnati.

AARON IHDE: A LIFE FROM BASCOM'S HILL

James J. Bohning, Lehigh University

This article is based on taped interviews with Aaron Ihde, conducted by Laura Smail on May 3, July 22, and August 1, 1983, in Madison, Wisconsin (1). All quotations, unless otherwise indicated, are taken from the interviews.

The Social Lessons of Undergraduate Education

By his own admission, Aaron Ihde was a "raw" freshman when he came to the University of Wisconsin in 1927, with little understanding of "university social life and social organizations." He lived at Tripp Hall for the first two years, but during his sophomore year he was invited by a fellow student in the quantitative analysis laboratory to visit a campus fraternity house. After Ihde went through the usual "hocus pocus" and consulted with his parents about finances, he decided to go ahead and join the fraternity. Ihde later regretted this decision, because of the "subterfuge and artificiality of such organizations." He moved to the fraternity house in his junior year, where he found living conditions to be worse than those he had just left in the dormitory. He wasn't concerned about the food or the sanitary conditions, which were about equal to those in dormitory life. Instead, it was the relationship with his fraternity brothers that was bothersome.

The relationships proved to be very artificial. There was an attitude that one owed everything to his fellow brothers, even if these fellow brothers didn't pay their fraternity dues and bills, and were a drag on the rest of us who did. At that time, 1930 and 1931, hard times were clearly evident, and there were many in the fraternity who had hard going. Most of us tried to keep our bills current, but there were some who ran up bills in the hundreds of dollars. Many

times they ended up graduating while never meeting those bills, which were carried on the organization's roles. In fact, that organization went defunct about 1933 because of the unpaid bills and the failure of the organization to keep up (2).

For Ihde, it was a case of "getting blood out of turnips," which was especially difficult when some of the turnips were "very resistant to being squeezed." He also found the social arrangements superficial, especially when the fraternity teamed up with some sororities and expected the members to date only women from those specific houses. Coming from a family that "looked askance at alcohol use," Ihde rebelled against the "inordinate amount of drinking" and the parties that often began and usually ended with "much drunkenness." Although Ihde finally realized this was not his kind of organization, he had several good friends in the fraternity and his graduation was approaching, so he remained through his last two years.

Although Ihde was not a Lutheran, he had become a member of a fraternity that was founded by Lutherans and mostly restricted its membership to Lutherans. He was appalled at the prejudice he found directed towards other religious groups. There was also bias of another sort. A student with only one arm was rushed by the fraternity. He was a "personable fellow" and many of the brothers supported his membership. But the physical handicap was sufficient for at least one member to blackball the individual's candidacy, because the fraternity house was on a street with heavy student traffic and some members did not want to have an image of everyone in the house not "being physically fit." Many years later one of Ihde's students asked him to come to a fraternity house for dinner and give a short talk to the membership. Ihde was glad to accept, and made the evening

“very interesting” by talking about “Why Fraternities Should be Abolished.”

While he managed to continue his precarious fraternity membership, Ihde also looked disapprovingly at The Experimental College, an organization whose short-lived existence paralleled his own undergraduate years on campus (3). This radical program was established by Alexander Meiklejohn in 1926 as an experiment in liberal arts education.

There was a certain amount of looking down noses at the Experimental College students because they didn't go to classes regularly, and therefore there was some question of whether they had to do any studying. The set-up of the E-College was that they did a lot of reading and met with tutors. Formal classes were almost non-existent, and those of us who had laboratories six hours a week resented their never getting up on the Hill. The E-college students were not very popular on campus and Glen Frank [University of Wisconsin President] called them guinea pigs at the Freshman Convocation. Other students picked up that term and continued to call E-College students “guinea pigs, even to their faces.” There were cases where the E-College students were thrown into Lake Mendota by other students, and sometimes the reverse occurred. But then, “there was a good deal of throwing of unpopular people into the lake in those days.” E-College students were often suspect of being “dirty radicals.”

They might have been communists, and certainly they were socialists or worse. They were tarnished with a broad sweep of a brush. There was a disproportionate number from out of state, including New York. New Yorkers were suspect generally at that time. Even Jewish students were suspect and not well accepted by the student body, suspect as to whether they really belonged on the campus, especially if they came from Chicago or New York.

There might have been a “certain element of envy involved, but more importantly, there was a real lack of understanding of what Meiklejohn and his faculty wanted to do.” Years later, Ihde revised his original estimation of the E-College, because he found that many alumni had “a broad and deep outlook on life and many of them” were very successful in their careers.

Ihde pursued athletics as a respite from these distractions. Because he had played some football in high school, Ihde was a walk-on for the freshman football team. He lasted all of two weeks, and then “was demoted to an intramural program which supplied players to scrimmage against the varsity.” Quickly learning that

he had no future in the football program, Ihde “wisely turned in” his suit and sought out other means of satisfying the freshman athletic requirement. He talked to Dad Vail (4), who told him that, at six feet, he was rather small for the rowing program. He finished that first year by taking swimming classes.

Vail died the next summer, and when Ihde began his sophomore year, he showed up at the crew house. Without a permanent coach, the team was relying on the captain from the previous year to take over the coaching duties, and Ihde “slipped into the squad” despite his supposedly small size. When George “Mike” Murphy later became the permanent crew coach, Ihde's size was never mentioned. Murphy, who was smaller than Ihde, had been the stroke on the University of Washington team that won the national regatta in Poughkeepsie. Ihde remained on the crew for three years, but the Depression was taking its toll. His 1931 team was the last to make any trip for some time as funds for new boats and off-campus trips evaporated. In fact, the 1931 team had no preliminary competition and only raced at the Poughkeepsie nationals (5).

An Industrial Interlude

Ihde had always felt he wanted to be a teacher. After receiving his chemistry degree in 1931 (6), Ihde wanted to attend graduate school, but in the depths of the Depression there were no graduate assistantships available and his parents were having a hard time keeping the farm going. When a job in Chicago became available, Ihde “grabbed it,” but in doing so postponed his desire to spend the rest of his life in the classroom. He was one of only three chemistry graduates that year who had a job on commencement day.

I went to Chicago, to the Blue Valley Creamery Company, a modest-sized food company headquartered in Chicago with 24 branches scattered around the Midwest. I was their chemist. They had a laboratory associated with the office and plant in Chicago, a fairly well-equipped laboratory with one bacteriologist and one chemist. The Director of Research spent most of his time as chief of the manufacturing department.... The sign on the door said “Research Laboratory,” but I would characterize our operation as “control and development” rather than research. We were trouble-shooting. When a consumer complained about the quality of the butter, we found out why the butter was causing the complaint.

As a single-product company, the Blue Valley Creamery was floundering during the Depression. Ihde devel-

oped a line of salad dressings that supplemented the butter sales. But he became bored with the lack of real research and soon realized that he didn't want to spend the rest of his life doing that sort of work. As the Depression problems exacerbated the company's financial position, Ihde became disillusioned with some of the company's tactics and its tendency to "operate on the margin of proper ethics," something he also saw as characteristic of other companies who were competitors.

We were asked to do things I didn't exactly approve of, such as adding chemical flavorings to butter when it was being churned rather than ripening the cream the old-fashioned way and producing a well-balanced flavor. By adding one or two chemicals you got what I thought ... was a harsh flavor, but it was cheaper to do it that way.

Returning to Graduate School

These experiences caused Ihde to rekindle his interest in the academic profession, but he also realized that it was necessary to have a Ph.D. to operate at the university level. After corresponding with Professor Henry A. Schuette and having many discussions with his wife Olive, Ihde "burned his bridges" and returned to Madison in 1938, just six months after his daughter was born, to begin work on his Ph.D. Still unable to get an assistantship, Ihde began with the several thousand dollars he and his wife had saved during his tenure in Chicago, and some assistance from his parents. He did not get any Wisconsin Alumni Research Fund money because he "wasn't in that superlative class of graduate students that attracted WARF money." In fact, Ihde admits that his high school and undergraduate records were not "particularly brilliant," which he blames on his tendency to examine in depth those things that interested him, while not completing all of his assigned work.

Initially, Ihde came back to Wisconsin to work with Schuette, who was a food chemist, because he had done some undergraduate work with him and he "liked his field." But during Ihde's time in Chicago, vitamins had become an important research topic, and Schuette was not doing any vitamin work. This caused Ihde to pursue a minor in biochemistry, which was then in the agricultural school. Harry Steenbock was one of a team who taught the first course Ihde took, and eventually Ihde "hooked up with him" because of his work in Vitamins A and D. Ihde found that the biochemistry department "was not blessed with many great classroom teachers," describing one as a "bumbler," another as a "neurotic," and others as marginal.

He was not impressed with Steenbock's teaching either. "Straight-laced and moralistic," Steenbock tended to "find fault with very nearly everyone. In his laboratory, he expected the student to be there early and be there late." If a student disappeared for a few hours, Steenbock would be waiting for an explanation when the student returned. He could be kind and understanding at one time and "mean" at another time. Steenbock's temperamental mood swings caused much unhappiness for some students in the last stages of their work.

Ihde ran into Steenbock's inflexibility when he took the introductory biochemistry course but did not sign up for the laboratory. Steenbock sent a message to Ihde, demanding an explanation. Ihde responded that he had been doing food analyses for seven years in Chicago and would rather take something new and more advanced, rather than repeating work he was already familiar with. Steenbock brought out a list of experiments and said that he doubted Ihde had done everything on the list. Ihde confessed that there were some procedures he was not familiar with and offered to do them. But Ihde "didn't see any point in taking the whole course



Aaron Ihde at age 34 (c. 1943), shortly after he returned to the University of Wisconsin as an Instructor.

because a lot of it was redundant." Steenbock was unwavering, insisting that the course was a requirement and no one was going to get a biochemistry minor without it. Without any recourse to an appeal, Ihde took the entire course. Ironically, when he took the course, the two teaching assistants came to Ihde to get some of the problems resolved, "because they didn't know what the answers were."

Ihde, however, managed to get along well with Steenbock, although he wasn't sure why.

I got away with some things that I don't think other people generally did. For example, I was doing some work on the effectiveness of unsaturated fatty acids on rats, in connection with Vitamin E deficiency. I had over 100 rats for a couple of weeks, and these had to be attended to every day. On Saturdays and Sundays I often brought my wife and little girl along. Bringing a non-biochemist woman into the animal laboratory was probably not done. Steenbock came in one time and found my wife and daughter there. I introduced them and he was very gentlemanly. He came to be very fond of my little girl because she could fill the feed receptacles in the rat cages and enjoy it. She did it very professionally and he thought sure she was going to become a biochemist. He treated my wife and daughter very well, despite the fact that he wouldn't have put up with another student bringing his girl friend in.

While Ihde's biochemistry minor work with Steenbock eventually resulted in a publication (7), his work with Schuette in chemistry continued. There was a striking contrast between Schuette and Steenbock.

There was a fine esprit de corps in the group, and we had a tendency to help one another a great deal. There was a lack of competitiveness and a friendly cooperativeness in the group. [Schuette] was a low-key person who didn't look over a grad student's back at eight o'clock every morning ... to find out what you had accomplished last night. On the other hand, if you were in trouble, you were always welcome to step into his office ... and discuss the problem. My feeling about research in the two departments was that I enjoyed the chemistry department more than I enjoyed the minor problem that I did in biochemistry.

Ihde introduced new chromatographic techniques into his food research, under Schuette's watchful eye. He was encouraged by Schuette to explore these new possibilities, but at the same time Schuette would make sure he didn't stray too much into difficult waters. Four publications resulted from his work for the Ph.D., which he received in 1941 (8).

From Freshman Chemistry to the History of Chemistry

When Ihde began teaching at Butler University later that year, he found that many times a student would understand a rather complicated concept best if he could see how it unfolded, "how the best minds in the field who first caught this concept came about catching it." As a

result, he began using historical anecdotal material very early in his teaching career. Ihde also didn't hesitate to teach students things that were erroneous, if it "could help them see the thing unfold properly."

I think there was a latent interest in history that goes far back, even to my youth. I have always had a tendency to wonder how we got from here to here, and that involves history. By the time I was out of college, I was deeply interested in the history of food legislation. In fact, Dr.

Schuette had planted some of that interest as an undergraduate. I even started to write a history of food legislation at that time, a history that has never been published and never really completed in proper form (9).

By the time World War II ended, Ihde was "deeply interested in the history of chemistry." He was teaching freshman chemistry, which he had been brought back to Wisconsin to do in 1942. The little bit of research he was doing "wasn't going very well" and he had feelings of indifference towards the experimental research. Early in 1946 he went to the department chairman, Howard Matthews, and asked if the course in the history of chemistry was ever going to be taught by Norris F. Hall, the professor whose name was listed with the course in the catalog. Matthews assured him that Hall would not be doing anything with it, so Ihde took over the course. Convinced that the history fit in very well with his teaching of freshman chemistry and would make him a more effective chemistry teacher, Ihde was determined to look much more into the history of chemistry.



Olive (left), John (center) and Aaron (right) eating lunch during a field trip sponsored by the Wisconsin Academy of Sciences, c. 1953. Aaron is wearing his beret, which was to become an integral part of his campus persona.

Lord knows, teaching freshman chemistry can be a losing game if you don't find an approach that interests the students. So I revived that defunct history of chemistry course in the summer of 1946 and taught it practically every year since then, until my retirement. In fact, I developed a very broad-based history of chemistry program at the university. Once I was into it, I was hooked!

Initially, Ihde did not have much interaction with the History Department. The History of Science Department at Wisconsin had been started as an independent department by Dean George C. Sellery, who brought Henry Guerlac to the campus in 1941. Sellery was adamant that Guerlac should not be in the History Department, because "History would not treat science sympathetically." Guerlac was at MIT during the war, and when he then went to Cornell, Dean Mark Ingraham hired Marshall Clagett and Robert Stauffer to replace him. Shortly thereafter, Erwin Ackerknecht began the history of medicine program, and George Urdang took over the history of pharmacy. This four-man department included Ihde in their activities as the fifth man, and eventually Ihde received a joint appointment to the department.

The chemistry department tolerated Ihde's activity in history as long as he didn't give "too much visibility to it." When Ihde decided in the 1950s that this indeed was the direction he wanted to move in, Farrington Daniels, the chemistry department chair, "started digging in his heels" and told Ihde bluntly that he would never get anywhere.

Farrington was not inclined to look with great favor on somebody who wasn't doing experimental research. Daniels was of the opinion that history was something you could do on evenings and weekends, and a chemist ought to be working in the laboratory. He told me that very frankly. Yet, when I had a job opportunity to go into the New York area [at Consumer's Union, Mount Vernon, N.Y.], he suddenly found that he loved me very much and that the department loved me very much and I shouldn't leave. I was investigated for a job with Consumer's Union in which I would have been in charge of their laboratory and their publications. They ran a quite sizeable laboratory, including a fair amount of chemistry and a lot of physics and some biology. My position would have been director of that activity, so my scientific background would have fit in there very well. It was a very tempting offer because my take-home would have more than doubled. When I got this letter inquiring if I was interested, I took it to Daniels and laid it on his desk. From that point on he worked very hard to get me promoted in a department that

didn't want to promote people who were not hot-shot experimentalists.

In spite of this support, Ihde's salary was kept low until H. Edwin Young became Dean of Letters and Science. At that time, Ihde was offered the deanship at Northern Illinois University and Young persuaded him to turn it down. For the next few years after that, Ihde's salary "began responding."

Joining the Integrated Liberal Studies Program

In 1939, a faculty committee issued a report that dealt with the place of science in the general curriculum in the Letters and Science. This report had some influence in creating the History of Science Department, but more importantly, it started the faculty thinking "about how general science courses in the university ought to be approached." Earlier attempts to create survey courses in each of the sciences were soon abandoned because "they were counterproductive." In part, this was because "they were taught by someone who wasn't particularly interested and taken by students who were anxious to work off the science requirement as inexpensively as possible."

Ultimately, another committee chaired by Robert Pooley of the English department recommended to the faculty a new department that was called the Department of Integrated Liberal Studies (ILS). Established as a two-year program in general studies, the ILS avoided the isolationism of the defunct Experimental College. Unlike similar programs in other universities, it did not recruit a separate faculty to staff this program. Instead, it "borrowed its faculty from existing departments," thus using faculty already established in their academic discipline.

The sciences were always the trouble area in programs of this type, and "some schools didn't even try to include the sciences. Where they did, the science courses were generally the weak spot." Two committee members, Richard Hartshorne of the Geography Department and Homer Adkins of the Chemistry Department, were at odds over how the sciences should be handled. On one hand, Hartshorne "wanted to create a completely designed program." Adkins, however, argued that the course should not be written by the committee. "Instead," Adkins said, "Let us simply designate what the course should deal with, and then look for a good faculty member to teach it and give him a free hand to teach

it as he sees fit." Fortunately for Ihde, his chemistry colleague prevailed.

As presented to the faculty, the science program called for a "Physical Universe" course which would draw its material from chemistry, physics, and astronomy. That was followed by an earth science course that was mostly geology, but could include meteorology and some geography. A one-year biology course then completed the sequence. All of this was implemented in the fall of 1947, when Pooley was appointed chairman of the department that was to administer the ILS program. Pooley immediately began to recruit "what turned out to be an excellent faculty, with two or three exceptions."

In his faculty search, Pooley shrewdly approached the department chair first and asked for permission to speak to a particular faculty member that he was interested in, rather than approach a faculty member directly.

I was picked early to teach the Physical Universe course. Pooley came to my chairman, Matthews, and sought permission to talk to me. I was interested. I had been interested even before. I had talked to Adkins at least once or twice about this program while it was still in the process of being created. I think that Adkins may have given Pooley my name and Pooley followed it up. I think my general chemistry teaching had become known around the campus to the point that I might have been looked at anyway ... Effective undergraduate teaching was one criterion Pooley was looking for. He was also looking for people who had the vision to meld together material from several different disciplines, as I put together material from astronomy and physics as well as chemistry.

Building on the reputation he had established in the freshman chemistry program, Ihde continued to have a "good rapport" with the students in the ILS program and his course was very well received by the students. Not all of the new ILS faculty, however, enjoyed the same success. Courses in anthropology and the social sciences did not receive high student ratings. In some cases, faculty were asked to leave ILS and return to their department. Later, when he was ILS department chair (1963-1970), Ihde tried to work with several faculty members to get them "to see their shortcomings and do something about them."

It is my own feeling about history that a student coming into the subject fresh must not be bogged down by all sorts of endless detail. Instead, the student should begin to appreciate the flow of ideas, the flow of experiments in history of science, the significance

of certain experiments, to the exclusion of all sorts of detail.

Ihde tangled with one faculty member "rather viciously" because he could not see Ihde's approach. Ihde sought to help him rather than "boot him," but without much success. When this particular course became a disaster and student activism increased, the faculty member offered to withdraw, and Ihde "immediately accepted his resignation." In spite of these isolated incidents, the ILS faculty "was much better than a student would run into by accident in the usual selection of courses. In fact, they were not only good teachers, they were so good in their own research that many were frequently on leave or away from the campus, necessitating temporary replacements that were often inadequate."

When Ihde began teaching the physical universe course in 1948, he found it "challenging" and "one of the most exciting periods" in his life. The astronomy component of the Physical Universe course was a subject with which Ihde was totally unfamiliar. (Ironically, Ihde later felt that he did his best teaching in astronomy.) Feeling on "unsound ground" in astronomy, he sought out Joel Stebbins, the observatory director.

We had a very interesting talk. I said to him that I had reservations about my ability to handle the astronomy in the course. He said to me, "Well, Ihde, you have a Ph.D. in chemistry, don't you?" "Yes." "Therefore, you must know some chemistry. I understand that chemists also study some physics, don't they?" "Yes." "So you ought to be able to handle the elementary physics." Then he looked me in the eye and said, "Now, Ihde, if you can't learn enough astronomy to handle the elementary aspects of astronomy for a course like this, you should never have been given a Ph.D. in science."

Having Stebbins' blessing, Ihde began to prepare himself. He felt that in order for his students to appreciate his case history on the transition from an earth-centered to sun-centered universe, they should go out at night and learn to identify constellations and planets. Of course, Ihde needed to learn in advance what he expected his students to know. He spent a lot of nights "star gazing" and systematically exploring the constellations with published star guides. His wife warned the neighbors not to be alarmed if they saw a man "prowling around" their backyard "in the dark of the night." By the time he started teaching the course, he felt he had a "solid background" in sky lore. For many years, Ihde and his assistants would meet the students after dark on Observatory Hill and study the evening September sky. For many

years these personal observations were supplemented with a field trip to the Adler Planetarium in Chicago.

Ihde found that astronomy was the perfect way to introduce the course, because of the large variation in science background of the freshmen. Some had four years of high school science, while others had no back-



Aaron at age 47 (c. 1956).

ground at all. But none of the students had any experience in astronomy, and that subject became a great leveler to start out with.

I learned in this experience that students many times understand difficult material best if it appears before them in the same way that this appeared before important scientists. For example, if a student sees a body

of facts develop in the way that scientific leaders who uncovered those facts see them develop, a student has a better understanding of them. Many college science teachers teach science from the present state of the art, with no consideration of where the ideas came from.

There were no laboratory sessions in the physical universe course. Ihde compensated by giving a "significant amount of exercise work" and performing many demonstrations during his lectures.

A Year at Harvard

Ihde himself was away at Harvard in 1951 - 1952, and his course was turned over to four other people. "That pretty much proved to be a disaster," because "it was a case of everybody's responsibility and nobody took the responsibility." But his experience at Harvard was invaluable.

My approach and the approach of some of the others was that we won't attempt to cover these fields. We will look at a few of the important developments. It was a kind of case history approach. James B. Conant of Harvard had just published his book *On Understanding Science* (10) and started his case history science course in the Harvard general education program. I was immediately influenced by that, and my

leave of absence ... was to go out to Harvard for that year and work as a teacher in that program. I was intimately associated with Leonard Nash and Thomas Kuhn, who were Conant's right hands ... I was already using some of the ideas but I developed them further in my own course.

Ihde found that much of the content of the Harvard course was similar to his own, but there were differences. Harvard had cases on electricity and fermentation, which were new to Ihde. He started his course with the Ptolemaic view of the heavens, the earth-centered view, and then followed the transition to the Copernican view of the sun-centered heavens and the reasons for it.

It was a beautiful case to use for students at the freshman level, some of whom had virtually no science, and a few had a fair amount of science. But this was not something out of high school science, and it was a beautiful example that could be used to show how scientists think, how they cling to outmoded ideas in spite of better evidence. I always thought that my course in the physical universe was a course in how scientists work and think rather than a course in astronomy, physics, and chemistry.

Ihde's year at Harvard and his interactions there with George Sarton and I. B. Cohen served as the catalyst that forever diverted his scholarly field from laboratory research into research in the history of chemistry.

Expanding the ILS Program

The ILS program was designed to be an integrated program, and initially the ILS faculty met often to discuss how their courses actually meshed with each other. One instructor could use another's material "to amplify" their own teaching. But meeting with each other wasn't enough to guarantee relevance and relationships. Ihde, and some others, actually attended all the lectures of their colleagues in the program. It was a time-consuming process "beyond the line of duty," but it was a sign of dedication to the program and its ideals. As new people came into the faculty, however, this kind of activity diminished.

Ihde attributed the program's success, particularly in the early days, to the ILS students.

It was presumably a general group of students in letters and science, but it turned out to be a self-selective group that was a bit better than average. There was no requirement that the students had to come with a particular high school achievement. They selected the program on their own.

In the 1950s, the Education Department was establishing an elementary education program and wanted to require the ILS curriculum of its students in the first two years. Ihde and his colleagues resisted that, saying that students would be welcome to elect the program, but should not enter because it was required. A significant number of these majors did elect ILS courses, but "they had difficulty with the program and they tended to drop off after a few years."

Another group that was involved early in the early ILS program was the Ford Scholars. The Ford Foundation sponsored this program, which took very bright high school sophomores and juniors and "sent them off to college."

The people setting up the Ford Scholar program wanted to put everybody in the ILS. We objected as a faculty to that, because we foresaw certain problems and we thought it would be well if some were in and some were not in. For one thing, this was a very sizeable group of students, between thirty and fifty, and in a program of not more than 200 students, it would have skewed the student population rather badly. It turned out that these kids were very bright, but they were also socially immature. They were a problem almost from the very beginning.

ILS was a "tough-grading program," but the bright Ford Scholars were picking up the highest grades, leaving the regular students further down in the grade distribution. Many were loners, but they were also arrogant about their intellectual ability, which "turned off" the other students in the class. Ihde felt that the worst problem, however, was their attitude that the ILS courses were an interlude that was preventing them from getting on with what was important and reaching their professional goal. What was important to them was the science and mathematics that would allow them to "become great research scientists right away, or go into medicine at an early age." The ILS faculty did not try very hard to keep the Ford Scholars in the program, and their numbers quickly dwindled and the program itself was eventually phased out.

The ILS program was never well-advertised, and attracting high school students to it was always a problem. In fact, the early enrollments never reached the maximum of 300, which was "disappointing" to Ihde and his colleagues. "The best recruiters proved to be the students themselves," as they would return to their high schools and talk about their experiences and report "it was a good program and they ought to try it." Eventually a group of high school teachers developed who had learned about ILS and recommended it to their stu-

dents. On the other hand, the freshman advisors at Wisconsin were "often antagonistic towards ILS and either deliberately withheld information" or otherwise dissuaded students from the program. "ILS was never popular with the faculty." Ironically, a large number of faculty children were in the program. Some programs, especially interdisciplinary ones, welcomed ILS students because they tended to do very well. But many departmental programs were "not terribly enthusiastic."

In spite of these attitudes, Ihde shared the enthusiasms of others teaching in the program, and "thought that everyone ought to be attracted to it." The program grew slowly. It became quite popular in the early 1960s, and the enrollment finally reached the 300 peak. When ILS went to the faculty and asked for a 400 peak, it was "surprisingly" approved. But breaking the 300 barrier didn't last long. While students could only enroll in the program when they entered the university as a freshman, they could leave at the end of any semester. Once the numbers approached 400, the attrition rates began to rise. Ihde attributes this to marginal students who weren't satisfied with their grades and soon left. After 1965 the student numbers never broke 300 again. "The rest of the university was always looking for ILS disasters," and this decrease in students was viewed as a "portent of failure."

Ihde's lecture in the "Physical Universe" course would include the entire freshman ILS class, but the discussion sections were held to twenty and were handled by teaching assistants. Recruiting these assistants was a problem, however, because "most graduate students did not have the breadth of disciplines that some of the ILS courses demanded." Ihde solved this problem by looking for students who had an interest and ability in teaching and had a background in one of the physical sciences, usually chemistry. In weekly meetings, Ihde would go through the forthcoming material and help them anticipate problems and questions. This was a successful carry-over from Ihde's own experience as a teaching assistant in the chemistry department, where this was a common practice.

Long after they had left the classroom, many students would seek Ihde out on a campus visit, and remark that his asides and digressions were things that they had never forgotten. Many remembered his remark before the first examination in the Physical Universe course. Ihde would explain that it wasn't necessary to get an "A" in the course to be a success in life, but "hopefully they would develop some appreciation of science that would stand them in good stead in the future." The

students claimed this was “one of the best things he ever did,” because it caused them to have a more “level-headed” attitude towards examinations and grades.

I was always ... a tough grader, and I felt that only very outstanding students deserved A's. I was probably a soft touch for a B, but A's had to be earned and very definitely represented a lot of understanding of the subject matter.

Ihde continued to teach his freshman chemistry course while he was active in the ILS program. This meant that he taught an overload “compared to most chemistry professors,” but he never felt “overburdened” because he enjoyed teaching freshman chemistry. He gave that course up only when he became chairman of the ILS department in 1963.

Surprisingly, [Chemistry Department] chairman Matthews was unenthusiastic about my taking on this [ILS] job, but if I were to continue teaching freshman chemistry, he would tolerate the deviance. I have a feeling that the course clicked very well in the first few years, and Matty was actually rather proud that I was involved in ILS. By the time he retired, he was telling people what a nice job I was doing.

Ihde taught in the ILS program until his retirement in 1980. He saw ILS going downhill during the period he was chairman, citing three reasons for that development.

Mr. Pooley and I got badly bloodied when we attempted to create a second ILS program called Basic Studies around 1962. It would vary from the original ILS program with a smaller number of required courses based to some extent on required elementary courses in the professional field. Students going into engineering, agriculture, medicine, we thought could benefit from the program, which had many of the objectives of ILS but was planned in a tighter way.

The faculty soundly rejected this idea, especially the humanists and some of the scientists. Secondly, the ILS also suffered from curriculum changes in other areas that made the program unattractive to many students. Thirdly, Ihde saw the student discontent contribute to the slippage of the ILS program, which peaked in 1964 (11).

Surviving Student Unrest

As ILS moved into the 1960s, it faced a new challenge—this time not from skeptical faculty and administrators but rather from the students themselves.

In the fall of 1948, we brought together 200 students who didn't know what they were getting into, and we didn't know what we were getting into. Pooley proved to be an exceedingly able administrator who

not only had good control of his faculty but immediately developed a rapport with the students. Things worked out very harmoniously. The students developed an esprit de corps that was very much a part of the program. Over the years, that esprit de corps held pretty well until we got into the age of dissent, when the capability of getting together easily and often caused them to think of ways of rejecting the ILS program.

Ihde attributed that early student attitude to a device that Pooley initiated at the beginning. By having the stu-



Aaron at age 61 (c. 1970) lecturing at a session of the Wisconsin Academy of Sciences, Arts, and Literature.

dents attend the same discussion section number in all four courses, Pooley kept the same students together in a small group, helping them to get acquainted quickly with each other and feel comfortable in a recitation situation. “In the age of dissent, the students took advantage of the situation to mount complaints” about

being in class with the same students all the time, and the arrangement was abandoned. Pooley also had an ILS student dinner early in the first semester as another device to having students and faculty get to know each other. These innovations were important to the esprit de corps, which led the students to initiate their own newspaper, the *ILS Pioneer*. Containing everything from poetry to faculty profiles, it was a great success from the start, “and it was a popular part of the student activity up until the 1960s.” It fell by the wayside in the age of dissent, when students started preparing competitive newsletters “with vile language” that took the program to task.

As chairman of the ILS program from 1963 to 1970, Ihde went through the very worst part of the student unrest.

In 1970, I decided I couldn't take it any longer and I told the faculty about six months earlier that they

shouldn't elect me again. At the time I retired from the chairmanship, I was a very unpopular person with the students. Within a year, the students were eating out of my hands. I was now an ordinary faculty member, a teacher, and they liked my course. Suddenly, I had the kind of popularity that I had had back in the 1950s and early 1960s. But while I was an administrator, I was their enemy. They were not even sure of me in the lecture room and as director of one of their courses.

From the beginning, the ILS program attracted "a disproportionate number of rather liberal, even radical-minded students" that added "some attractive color" to the program. By the 1960s the radical student movement at Berkeley had spread to Wisconsin. Ihde and his colleagues were "rather complacent" and thought that it couldn't happen in Madison, because Wisconsin had "always listened to students and given them a hearing." It came as a surprise to many when they found out just how out of touch they were with the radical element. The ILS student body was "ready-made to be a leader in the dissent movement."

I remember one morning when I went into my lecture room where most of the students were assembled for my Physical Universe lecture. I saw ... my demonstrator talking to a very young person at the back of the room. This chap came to me and said that he requested an opportunity to speak to the students on relevance in academic affairs. I recognized that this was trouble, but I told him I would give him five minutes to state his case. When the bell rang, I moved back several rows and sat down among some of the students, after telling the students that this young man wished to make a statement. He started in by condemning courses like this as having no relevance to things that were important these days. Professors were teaching them reactionary points of view, giving them no opportunity for input into course material. His case was actually poorly presented. I found out later that he was a boy from Brooklyn who was moving across the country, attempting to create student disturbances.

The outsider's rambling presentation continued until the students started chanting, "We want Ihde!"

His message was crushed through the student chant, which picked up intensity. Finally he said, "I am requesting the students in this class leave trivialities of this sort and go out and start the revolution." He started to walk up the stairs to the back of the room, apparently expecting students to follow him. He had a few henchmen planted in the back of the room and they followed him out. The rest of the students re-

turned to their places. I used the rest of that period, not to discuss science, but to discuss the problems that had been raised by this intrusion.

In the ensuing discussion, the relevance of the history of science was never addressed. Instead, the discussion centered around whether the United States was an imperialist nation, as charged by the intruder. All parts of the political spectrum were presented, and Ihde thought the ex-

perience was "healthy" and "fruitful." In the next few years following this incident, there was "a great deal of attack on the content of the ILS courses." Ihde believed that "down deep in their minds, students had the attitude that it [ILS] was as good a program as was available in the university, but ... they weren't going to admit that anything was good."

What bothered Ihde most about the dissent movement was that students "assumed whatever they wished." This included a lot of "very false history of the university," including the charge that the ILS program was created in 1948 and had never been changed. Ihde denied that this was true and vigorously asserted that his own course changed "very drastically" over the years, and he continued making changes almost to the point of his retirement.

One of the problems that students have in criticizing a university is that they are transients. They are around for only four years. As a result, they do not see programs in the same way that faculties see them. ... I think that faculty ought to philosophize more than they do, about the subject, about the university, about the world in general. I have sometimes felt



Son, John (left) and Aaron (right) in 1990.

that I did my best teaching on a one to one basis when a student came into my office, where there were no straight and narrow paths that the discussion must follow. I sometimes did some of this in the classroom. I had a tendency to get off the subject in the classroom ... I felt that even departing from the subject matter in favor of philosophical or historical points of view was perhaps one's best teaching.

In 1968, Ihde was scheduled for a trip to the State University of New York Albany, where he was a consultant to the History of Science Department. He was scheduled to leave early in the afternoon, but student dissent was at a very high level, particularly in the ILS. When he went home for lunch, he discussed it with his wife, who ventured that his place was in Madison. Ihde agreed, and arranged an evening meeting at the Episcopal House for all ILS students, teaching assistants, and faculty.

We had quite a knock-down, drag out evening, lasting from 7 until 10:30. Some of the students wanted to pass a resolution that the university take action to condemn the U.S. policies in Viet Nam. The faculty was successful in getting that watered down a great deal. Several resolutions were passed at that meeting expressing a lack of sympathy with the Viet Nam situation and other things dealing with ILS policies.

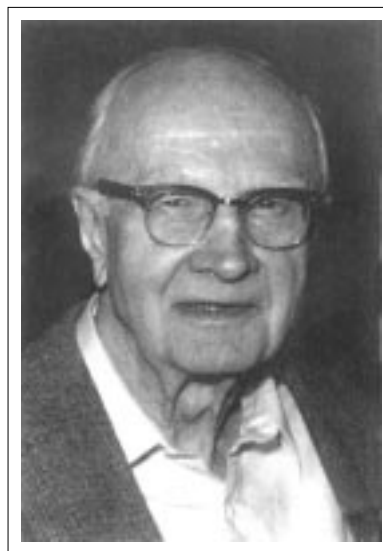
This was the first of several evening meetings where there was a great deal of "heady give and take" between ILS faculty and students which addressed the relevance issue of the ILS course content. About twenty students comprised the activist core, both freshmen and sophomores. Some of the ILS teaching assistants were actually "egging on" the students, meeting with them privately and giving them ideas about the course of action they should take to criticize the program. These were the same people who were heavily involved in the 1970 strike of the new Teaching Assistant Association (TAA), an action in which ILS "suffered quite a bit." Although none of Ihde's assistants participated in the strike, many of the nonscience assistants did strike, leaving the program "in a shambles." Many ILS students were very supportive of the strike, and the TAA took advantage of them and persuaded the student leaders to picket lectures. Those students who were not supportive did not have the courage to cross the picket lines. Ihde was "never supportive of the strike" and thought that "many of the issues could have been resolved short of a strike."

It's my feeling that TA's represent a rather unusual group. They are not full-time students, nor are they full-time faculty. They are really faculty assistants who are helping to get a teaching job done, and by doing that they can support themselves to a signifi-

cant degree in their graduate studies. They will not be permanent members of the faculty, and therefore I fail to see why they should have a substantial voice in determining academic policy.

Although Ihde was in "total disagreement" with the TAA's demand for input into course content, he was sympathetic with their class-size argument. He felt an effective section could not be run with 25 or 30 students, because it is difficult to get all students involved in a discussion with that many students. For Ihde, 20 students was a maximum size, although he preferred numbers even smaller than that.

As for the Vietnam war, my stand drifted from sadly supportive of the early action in Viet Nam to very strongly opposed to the Vietnam action in the later years of the decade. I had some feeling in the beginning as [John F.] Kennedy and [Lyndon] Johnson were beginning to escalate this thing that it was the most unfortunate thing that the country could become involved in, because we were beginning to make progress on racial problems, and here comes this Vietnam war, tearing the country to pieces ... I think my early impression was that ... probably we have to do what we are doing in Southeast Asia, but probably we ought to do it around the conference table rather than on the battlefield. I became more firmly of the position that we were doing the wrong thing as that issue escalated ... I was in great sympathy with the anti-war movement, but I was not in sympathy with the way they were getting their point across. Basically, I am a simple person, and I prefer to see issues resolved by discussion rather than by violence.



Aaron at age 83 (c 1992).

But, despite the ups and downs, Ihde felt that, in the final analysis, the ILS program had been a success.

Many of the ILS students have done very well in their professional careers, and the program at least didn't hurt them. I like to think that part of their success is in some ways attributable to ILS.

Those That Count

Aaron Ihde was the consummate juggler of his deep interests in many academic and intellectual activities. He was a master teacher of undergraduates in the ILS and general chemistry programs at Wisconsin. He established himself as a preeminent historian of chemistry with a record of publications and students that will remain his legacy for future generations. Throughout all of this, there was one person who remained foremost in his thoughts, and that was his wife Olive. She often accompanied him to professional meetings, student seminars and outings, even in later years when advanced arthritis confined her to a wheel chair. She often took an intellectual interest in the topics under discussion and provided her husband with sage advice on more than one occasion.

My wife has been a gem! She has been fully supportive of my entire career. In fact, she many times gave me sound advice which I probably would have overlooked if I had to come up with it myself.

REFERENCES AND NOTES

- University of Wisconsin Archives, Oral History Project Interview #263.
- The fraternity Ihde joined was Delta Pi Epsilon. While Ihde was a member, they sold the house and went to another one, and then joined Theta Kappa Nu, a national organization.
- The Experimental College was a two-year program which had a single course on the nature of society and social order in which a single grade was given at the end of two years. The grade was meant "to establish not what the student had done but what the student was capable of doing." In order to establish a feeling of community, shared dormitory living (men and women) became part of the experiment. See <http://www.sit.wisc.edu/~psohandbook/beyond/exp.htm>.
- Harry Emerson [Dad] Vail was a professional sculler who became a legendary coach at Wisconsin. The Dad Vail Regatta began in 1934 and is now the largest collegiate regatta in the United States.
- The financial struggles of the Wisconsin crew team during the Depression are detailed in an oral history interview with Murphy, University of Wisconsin Archives, Oral History Project Interview #217.
- In this interview, Ihde did not discuss his undergraduate experience in the chemistry department (1927-1931) because he was then writing *Chemistry as Viewed From Bascom's Hill: A History of the Chemistry Department in Madison*, published in 1990.
- D. S. Anthony, F. W. Quackenbush, A. J. Ihde, and H. Steenbock, "Antiacrodynamic Potency of Seed Oils," *J. Nutrition*, **1943**, 26, 303-307.
- A. J. Ihde and H. A. Schuette, "Honey and the Lead Contamination Problem," *Food Research*, **1939**, 4, 555-562; A. J. Ihde and H. A. Schuette, "The Chemistry of the Rye Germ," *J. Am. Chem. Soc.*, **1941**, 63, 2486-2487; A. J. Ihde and H. A. Schuette, "Thiamine, Nicotinic Acid, Riboflavin, and Pantothenic Acid in Rye and its Milled Products," *J. Nutrition*, **1941**, 22, 527-533.
- A. J. Ihde, *And Still There is Death in the Pot: A History of the Pure Food Movement*, University of Wisconsin, Department of Special Collections, Memorial Library, Manuscripts, Call Number MS 378. This was probably composed between about 1938 and 1941.
- J. B. Conant, *On Understanding Science*, Yale University Press, New Haven, CT, 1947.
- At the time of this interview, the ILS program was in serious trouble. It has since been revitalized and is quite successful once again. For more on the current ILS program, see <http://www.wisc.edu/ils/>.

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AARON IHDE AND HIS STUDENTS ⁽¹⁾

Alan J. Rocke, Case Western Reserve University

The Making of a Teacher

Aaron Ihde was a born teacher. He once recalled (2) that he always thought that he would become a teacher: in grade school he wanted to be a grade school teacher, in high school he assumed that he would eventually teach high school, and in college he wanted to become a professor. When he received his B.S. in 1931, he was lucky to find any job at all, and went to work as a food chemist in Chicago. But it was not long before his early ambitions caught up with him. In February of 1938 he and his wife packed up their belongings and their six-month-old daughter, and returned to Madison, on the basis of meager savings and no initial teaching assistantship. Three years and four months later, Ihde was a newly minted Ph.D., eligible for a college teaching career (3). Unfortunately, this was one of the worst of times to look for university work. Although the Depression had broken, the war in Europe (and its likely implications for the United States) meant that university enrollments were declining all over the country. Ihde found an instructorship at Butler University in Indianapolis, but the



Aaron J. Ihde, circa 1985.

work was difficult and not particularly rewarding. He taught four courses per semester, each a separate preparation. Moreover, this food chemist found himself teaching the full spectrum of courses, particularly physical chemistry. Ihde wrote his former adviser Henry Schuette

about his troubles, and Schuette brought the matter to the attention of the Chemistry Department. The department was having its own troubles, for defections of faculty members to Manhattan Project work exacerbated the normal rate of faculty attrition. In the spring of 1942 Ihde was brought back to Madison for one year to plug a gap in staffing for the general chemistry program.

Ihde immediately established a reputation, both among students and colleagues, for conscientious and attractive teaching, and he was clever enough (and motivated enough) to figure out how to make himself indispensable. Conditions at the university during the war years were chaotic, and Ihde was willing and capable of doing anything, on the shortest notice—from the most challenging teaching assignment to the sort of routine section work normally done by teaching assistants. And everything he did brought rave reviews. His one-year terminal contract was renewed twice more. In 1945, with returning servicemen now

pushing enrollments through the roof, Ihde was offered a tenure-track contract (4). He was promoted to associate professor with tenure in 1948, and to full professor in 1958.

Ihde loved teaching general chemistry, and his students loved him. He continued teaching freshman chemistry until he was made chair of the Integrated Liberal Studies department in 1963 (about which more below). Early on he began to incorporate historical material into his chemistry teaching, purely for pedagogical reasons, as he had witnessed Schuette do. Gradually he came to understand the importance of history of science for its own sake. In summer semester 1946 he revived a course in the history of chemistry that had lain dormant the previous few years. He continued to teach history of chemistry (breaking it into a two-semester survey sequence in 1963) until his retirement in 1980.

The Department of Integrated Liberal Studies (ILS) was founded at the University of Wisconsin in 1948 to provide an innovative two-year undergraduate curriculum in interdisciplinary liberal arts, for selected students who wished to have a broader general education than that offered by the conventional "breadth" requirements. Influenced by the Great Books curriculum, as well as by the "Experimental College" pioneered a generation earlier by Alexander Meiklejohn, ILS was an immediate success, partly on the strength of its instructors. The founders of ILS were clever enough to select professors who were not only committed to interdisciplinary approaches, but were gifted teachers, as well. (All faculty retain their primary disciplinary departmental affiliations when they join the ILS Department.) Naturally enough, Aaron Ihde was invited to participate, and he was delighted to do so. He created the introductory science course in this new college: ILS 131, entitled "The Physical Universe." Intended for nonscience majors, Ihde

structured his course around the leading themes of energy, motion, and matter, and developed these ideas in astronomy, physics, and chemistry, using a case-historical approach. Students responded well to this, as they did to all of his courses.

So Ihde began teaching historical material in ILS in 1948, just two years after starting to teach history of chemistry in the Chemistry Department. Simultaneously, a potential third historical opportunity arose, for a new Department of History of Science was created at UW-Madison

in 1947, with the arrival of Marshall Clagett and Robert Stauffer (5). These men, and later additions to the department, warmly welcomed Ihde as an unofficial colleague. Ihde's Carnegie year at Harvard (1951-52) fully professionalized him into the emerging discipline: he got to know James Conant, Thomas Kuhn, Leonard Nash, I. B. Cohen, and, not least, George Sarton. In 1957 Ihde formally



Aaron (left) consulting with Odell Taliaferro, the chemistry department demonstrator, just before the beginning of one of Aaron's ILS lectures. December 1949.

added affiliation to the History of Science Department to his professorial title. Gradually Ihde taught more courses for the department, including Science, Technology, and Society, and the Evolution of Food and Drug Controls. He also conducted an advanced seminar in history of chemistry for his graduate students.

By the time of his retirement in 1980, he had taught for thirty-eight years at the University of Wisconsin. Other than seven years in an industrial position in Chicago, a year in Indianapolis, and a year in Cambridge, Ihde had the good fortune to have spent his entire life in the state of Wisconsin.

Personal Recollections

I arrived in Madison as a graduate student in chemistry in the fall of 1969. In one respect I may not have been typical: as the product of a Great Books undergraduate curriculum, I had long been attracted to interdiscipli-

nary and historical approaches to knowledge, and I thoroughly enjoyed reading history of science. Two years later, seduced by the first course I took with Aaron in the spring of 1970, I abandoned the Chemistry Department for the Department of History of Science. Aaron hired me as a teaching assistant for Physical Universe (PU), beginning in the fall of 1971.

Aaron decided to revamp completely this course (giving up the chairmanship of ILS in 1970 gave him the leisure to accomplish this). He had taught PU for 23 years essentially the same way, and it continued to be a great success with students, about two hundred of whom took the course every year; colleagues were astonished that he would meddle with success and deeply impressed by Aaron's commitment to productive innovation. In brief, Aaron was not unhappy with his course, but he wanted to see whether he could exercise his interests and competencies in science and public affairs, in a way that would make the course seem more exciting and relevant. The new PU sought to "look at certain scientific developments, examine their impact on practical affairs, and examine the good and the bad which has resulted." Topics included energy production and use, natural resources, water quality, food and nutrition, pesticides, and nuclear weapons. The debut of this course was my first teaching in ILS.

The course was a smashing success. American undergraduates were, in general, anxious for "relevance," and this course was relevant with a vengeance. Vietnam protests were hot, the first Earth Day had recently occurred, the environment was becoming the watchword it remains today, and battles over clean water, clean air, and pesticide use were raging. Through it all Aaron provided a thread, while teaching nonscience students a goodly amount of real science in the process.

This course required the organization of massive amounts of information. What amazed his TAs was that he accomplished this, with virtuoso skill, and with no apparent sense of effort. One could speak with him right before class, in the midst of organizing his lecture and ever-present slides, and he had as relaxed a manner as if we were taking tea on Sunday afternoon. Organized he certainly was, but paradoxically he was not obsessively neat, for which his desk may serve as a case in point. At the start of every semester he began with a large, perfectly clean desk, which gradually began to accumulate memos, correspondence, and other scholarly and administrative detritus. As the volume increased, he had a tendency to push the remaining mass

toward the wall. This gradually created a sloped surface, well designed for writing. By the end of the semester the front edge of his desk would have perhaps a half-inch cover, while the back edge might have upwards of a foot of paper. Somehow, though, he always knew how to find the piece of paper he needed at the moment. After the semester ended he would do the necessary archeology, in order to prepare for the semester to come.

In lecture as in conversation, Ihde spoke with admirable deliberateness, clarity, and precision. Modest and self-effacing, he was not the least flashy. He was also, despite what I have just written, something of a ham. In one of his lectures during that first semester of the "new PU," he wanted to describe the physiological effects of DDT on animal life, and had the sudden inspiration to act it out. The lecture hall, being in the chemistry building, had a large demonstration table in front. Ihde said, "Let us suppose that I represent a fly that has just been subjected to a lethal dose of DDT." He then crawled onto the table on all fours and began to narrate (while graphically illustrating) the next events in the life (or rather death) of that fly. Suffice it to say that the demonstration ended with Professor Ihde flat on his back, limbs stiffly raised, dead as a doornail. The room erupted in applause. Ihde's "dying fly" immediately became legendary at the University of Wisconsin, and he was obliged by popular demand to repeat his performance every year thereafter.

Aaron's student evaluations were superb. Part of his secret of success was simply his extraordinary personality: he exuded an utterly irresistible combination of erudition and kindness, which was apparent to all of the students. The course asked for complexity, thoughtfulness, and wisdom, and he gave them that. The undergraduates often told me that they idolized him, and viewed him as a wise grandfatherly figure (and he was that). They flocked to his classes. In 1978 Aaron was given the Chancellor's Award for distinguished teaching at the University of Wisconsin, a highly select honor. In a letter supporting Aaron's nomination, his colleague Victor Hiltz (who regularly co-taught "Science, Technology, and Society" with him) wrote that Ihde was much more than simply a superb classroom performer (6):

There is an integrity about Ihde as a person, as a scholar, and as a teacher that comes through to even the most inexperienced undergraduate. Ihde never gives the impression that he is in the classroom just to do his job or to impart a certain quantity of information. Long before most others were concerned

about the interrelationships of science and society, Ihde was deeply concerned with these things. This concern comes through in his lectures, and this is what he teaches above all else. Students leave the lectures with the feeling that they have heard something important, and that they want to know more—indeed, that they *must* know more. Perhaps this should be the criterion of distinguished teaching.

Hilts concluded that "... if one could somehow define the secrets of Ihde's success as a teacher, one would have a very nearly perfect model of what a university teacher ought to be."

Aaron had a similar following for his graduate courses as for his undergraduate offerings, and for the same reasons. His ability to summarize complex developments in brief compass was uncanny, and when queried on this or that detail he was virtually never at a loss. He had an astonishingly precise and retentive memory. Aaron conducted his graduate seminar in the evenings in his beautiful home in the UW Arboretum. We would gather after supper, spend a pleasurable

Aaron was not excessively directive or intrusive in his supervision of doctoral work. He held to the philosophy that an important part of the exercise of writing a dissertation was to work through puzzles and problems oneself, and thereby acquire the self-confidence and resourcefulness necessary to succeed as a scholar. (Of course, like a father at poolside, he always stood ready to rescue you, if it became necessary.) He always said that he viewed the Ph.D. degree not as a certificate of perfect competence in the field, but as a scholarly "hunting license," one should—that is, one *must*—continue to learn and to improve throughout one's life.

My Ph.D. was granted in 1975, but jobs had become very scarce after about 1971, and I did not immediately find a tenure-track position. The silver lining in this disappointing cloud was that I was privileged to continue my association with Aaron, first as his research associate and then with a lectureship to assist him with his ILS duties. I left Madison in 1978 when Robert Schofield brought me to Case Western Reserve Univer-



Aaron and Olive Ihde with many of Aaron's former students and postdoctoral fellows. Midwest History of Science Junto Meeting, Madison, WI, 11 April 1980.

couple of hours talking about history of chemistry, then Aaron would prepare and serve a small treat for dessert. Aaron's wife Olive, who was confined to a wheelchair during the time I was privileged to know her, participated fully (as an informed layperson) in all of the seminars.

sity, to replace fellow Ihdean Reese Jenkins, who had just been hired as the Director of the Thomas Edison Papers Project at Rutgers University. Schofield told me at the time that he felt very comfortable with hiring me, not least because Reese had done so very well in his eleven years at CWRU; the fact that I was another Ihde product, he thought, augured well (7).

I could not have asked for a better dissertation director. Aaron became not only a trusted adviser, but a close fatherly friend. I regard him as one of the finest human beings I have ever met—a model of probity, wisdom, erudition, and benevolence.

The Next Generation

Aaron Ihde directed a total of 21 Ph.D. dissertations in history of science (see Appendix I). In addition, Ihde supervised three doctoral dissertations in chemistry (8) and 10 master's theses (five each in chemistry and in history of science) (9). He also directed postdoctoral research for Owen Hannaway, O. Bertrand Ramsay, and seven other historians of chemistry (10). Hannaway, whose articles and book on the history of early modern chemistry have been extraordinarily influential, has always emphasized the significance for his intellectual development of his postdoctoral study with Ihde.

I began to tabulate all the refereed articles and book chapters written by Ihde's students but eventually gave up the task when the numbers started to get out of hand (Siegfried, Hamlin, Whorton, and Parascandola were the most flagrant culprits in this regard). Suffice it to say that they number easily in the hundreds. As far as books are concerned, Appendix 2 lists 19 monographs written by Ihde students (this excludes edited works, which would be a large category in itself).

Ihde's first doctoral student in history of science in 1953 was Robert Siegfried, who was also the first Ph.D. produced by the still young Department of History of Science. Siegfried was hired to teach in the general science program at the University of Arkansas in Fayetteville, but he also offered a specialized course in the history of chemistry. The first time he taught this course, an undergraduate by the name of Betty Jo Dobbs was among his students. Dobbs went on to become a distinguished scholar of the history of alchemy and chemistry, and Siegfried and Dobbs published an important article together in 1968 (11). Siegfried was a prolific researcher—an "article man," like Ihde's great contemporary Henry Guerlac—specializing particularly in the life and work of Humphry Davy. By 1963 he was sufficiently prominent that Ihde was able to bring him back to the University of Wisconsin as a colleague. During his 24 active-duty years in Madison, Siegfried directed six Ph.D. dissertations: David Dyck (1967), Virginia Schelar (1969), Homer LeGrand (1971), Bernard Langer (1971), Jean-Claude Guédon (1974), and Anthony Stranges (1977).

Some of Ihde's first few Ph.D. students—Siegfried, Costa, and Baker—chose research topics that illuminated the development of the science of chemistry in Europe during the nineteenth century; later on, Fisher, DeKosky, and Rocke did the same. Early in their careers, Costa, Baker, Fisher, and DeKosky published landmark books and articles spun off from their dissertation research, then moved to different concerns. A new direction in the "Ihde group" was indicated by the work of Reese Jenkins (1966), who moved toward American topics and technology. Jenkins' book on the U.S. photographic industry, *Images and Enterprise* (1975), based on his dissertation work, won the 1978 Dexter Prize (the outstanding book award by the Society for the History of Technology) (12). Jenkins has had a distinguished career at Case Western Reserve and then Rutgers University in New Brunswick. He has directed one M.S. thesis (Michael Sokal), and three Ph.D.s (Michael Swords, David Channell, and Paul Israel), all of whom are distinguished scholars today (13).

The American direction was reinforced by the dissertations of Becker, Parascandola, Jones, Whorton, Kopperl, Johnson, Paradowski, Trottman, Hochheiser, and Meyer. The work of Becker and Parascandola was directed toward the history of biomedicine; that of Kopperl and Paradowski was more oriented to the development of basic science; and that of Jones, Whorton, Hochheiser, and Meyer related more directly to science, technology, and society.

John Parascandola's research has been enormously prolific and wide-ranging. Concentrating at first on the history of biochemistry and the work of the Harvard scientist Lawrence Henderson, he then turned to the history of pharmacology, chemotherapy, and pharmaceutical chemistry. His publications on the history of chemical pharmacology shed light on a subject that had received almost no attention, and earned him the Edward Kremers Award of the American Institute of the History of Pharmacy. His work on Paul Ehrlich and the origins of chemotherapy is of special interest to historians of chemistry, pharmacy, and medicine; and for his seminal book *The Development of American Pharmacology* he was awarded the George Urdang Medal of the AIHP. He directed the Ph.D. work of John Swann (1985).

James Whorton has also had a distinguished career. After his highly important first book (based on his dissertation), *Before Silent Spring*, Whorton has concentrated on investigating the history of health culture in America and Great Britain. His second and third

books—along with a raft of important articles—provided groundbreaking explorations of the evolution of scientific and medical thinking about diet, exercise, and other health behaviors, and the intersection of expert advice and popular or commercial programs. He supervised the Ph.D. work of Barron Lerner (1996).

Sheldon Hochheiser was one of Ihde's last two students, finishing after Aaron retired. His work has focused on the practice of science and technology in corporate America—at first centering on food processing and applied chemistry, then on telecommunications. Since he became corporate historian for AT&T, his professional efforts have been devoted to activities other than scholarly publication.

Christopher Hamlin's work, like that of Whorton, emerged out of Ihde's interest in the social history of applied chemistry. This has led him in several directions—disease theory, public health, environmental history, the history of public administration, and the cultural history of chemical concepts. His work has pertained both to Great Britain and the United States and has been oriented toward public policy and social justice, as well as to more classic themes in the history of science and technology. Hamlin's several books and numerous articles have had a major impact on the fields he studies. He has directed three Ph.D. students: Craig Stillwell, Vladimir Jankovic, and Barbara Wall; another two are still in progress.

My own work has been directed towards investigating the development of the science of chemistry in Europe, especially Germany and France, during the nineteenth century. I directed the Ph.D. work of R. Richard Hamerla (2000).

Few historians of chemistry—indeed, few historians of science—have had the kind of productive teaching career that Aaron Ihde enjoyed. His students remember him with fondness and gratitude, and the "Ihde legacy" is passed on through them.

REFERENCES AND NOTES

1. I would like to thank James Bohning, Christopher Hamlin, Sheldon Hochheiser, Reese Jenkins, William Jensen, John Ihde, John Parascandola, Gretchen Serrie, Robert Siegfried, and James Whorton for their assistance and advice.
2. In an oral history taped in 1983 (five one-hour tapes) and preserved in the University of Wisconsin Archives.
3. The biographical material in this paper mostly comes from sources in the University of Wisconsin Archives and from additional material provided by the family. An informative biography was published in the Madison *Capital Times* at the time of Ihde's retirement in 1980. There is a short biography in the Festschrift edited by John Parascandola and James Whorton, *Chemistry and Modern Society: Historical Essays in Honor of Aaron J. Ihde* (American Chemical Society, Washington, DC, 1983). In addition, there is much of interest in Ihde's own *Chemistry as Viewed from Bascom's Hill: A History of the Chemistry Department at the University of Wisconsin in Madison* (University of Wisconsin Chemistry Department, Madison, WI, 1990).
4. Ihde, writing in the third person, allows that he had been "versatile and effective" in his teaching in the war years, hence his elevation to tenure track (*Chemistry as Viewed from Bascom's Hill*, p 605). In light of Ihde's habitual modesty, one can only imagine how positive his impact actually was.
5. Actually, Henry Guerlac had taught history of science briefly at the University of Wisconsin as early as 1941, but he left shortly thereafter to serve as the historian of the MIT radar project, and there is no evidence that he and Ihde became acquainted at that time.
6. Hilts, nomination letter, March 10, 1977.
7. Ihde and Schofield held deep respect for each other, despite their very different styles of history. They had been closely associated in the founding of the Midwest Junto for the History of Science.
8. Don Kromer (1950), Robert J. Kline (1953), and Irving Domskey (1959).
9. Hans Breuer (1948), Charlene Steinberg (1948), George Hood (1949), Mary Glassner (1949), William Kinsman (1952), Virginia Schelar (1953), Rosemary Ehl (1953), Shirley Pomeroy (1958), David Biggins (1973), and Grady Frenchik (1978).
10. David Biggins, Dale Warren, E. Frank Korman, Jerry Janssen, Vincent Cash, Ernest Spittler, and A. Truman Schwartz.
11. Siegfried and Dobbs, "Composition: A Neglected Aspect of the Chemical Revolution," *Annals Sci.*, **1968**, *24*, 275-293.
12. Not to be confused with the Dexter Prize for outstanding work in the history of chemistry, awarded annually by the History Division of the American Chemical Society.
13. For his outstanding book *Edison: Life of Invention*, Paul Israel received the SHOT Dexter Prize for 2000—the first time in the history of this prize that a student of a winner received the award.

ABOUT THE AUTHOR

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earned his doctorate at the University of Wisconsin under the tutelage of Aaron Ihde. Dr. Rocke was the recipient of the 2000 Dexter Award from the History of Chemistry Division.

Appendix 1.

Ph.D. Dissertations in History of Science Directed by Aaron J. Ihde (in chronological order)

Robert Siegfried (1921-), "A Study of Chemical Research Publications from the United States Before 1880" (1953). Siegfried taught at the Universities of Illinois and Arkansas before returning to the History of Science Department at the University of Wisconsin in 1963, where he remained until his retirement in 1987. He lives in Madison.

Albert B. Costa, "Michel Eugène Chevreul and the Chemistry of Fatty Oils" (1960). Costa taught in the History Department of Duquesne University; he is now retired in Pittsburgh.

Victor A. Triolo (1932-), "Systems of Renal Physiology before Malpighi" (1962). Triolo is currently associate professor in the School of Library Science and Instructional Technology at Southern Connecticut State University in New Haven.

A. Albert Baker, Jr. (1926-), "The Development of the Understanding of Unsaturation in Organic Chemistry in the Nineteenth Century" (1964). Baker was hired at Grand Valley State College (Allendale, Michigan), then was director of the Science Library at the University of Southern California for many years. He is now retired.

Suzanne Kathleen Boram Sabbagh (Nixon) (1940-), "A Preliminary Study of Fossils as Stratigraphic Indicators (1022-1820)" (1964). Sabbagh did not pursue an academic career; she resides in Madison.

Reese V. Jenkins (1938-), "Some Interrelations of Science, Technology, and the Photographic Industry in the Nineteenth Century" (1966). After teach-

ing in the Department of Interdisciplinary Studies at Case Western Reserve University, in 1978 Jenkins became Director of the Thomas Edison Papers Project; he is currently Professor of History at Rutgers University in New Brunswick.

Stanley L. Becker (1929-), "The Emergence of a Trace Nutrient Concept through Animal Feeding Studies" (1968). Becker teaches in the General Science program at Bethany College in Bethany, West Virginia.

John Parascandola (1941-), "Lawrence J. Henderson and the Concept of Organized Systems" (1968). After working his way up to full professor in the departments of history of pharmacy and of history of science at the University of Wisconsin, in 1983 Parascandola became Chief of the History of Medicine Division of the National Library of Medicine. In 1992 he was hired as Historian of the Public Health Service in Bethesda.

Daniel P. Jones (1941-), "The Role of Chemists in Research on War Gases in the United States during World War I" (1969). After a period teaching at the University of Illinois, Jones moved to the National Endowment for the Humanities in Washington; he is currently a Senior Program Officer in the Division of Research Programs.

James C. Whorton (1942-), "Insecticide Residues on Foods as a Public Health Problem: 1865-1938" (1969). In 1970 Whorton was hired in the Department of Biomedical History at the University of Washington School of Medicine; he is currently full professor in the Department of Medical History and Ethics.

Nicholas W. Fisher (1942-), "The Taxonomic Background to the Structural Theory of Organic Chemistry" (1970). Fisher was hired at the University of Aberdeen, where he teaches today in the Center for Cultural History.

Sheldon Kopperl (1943-), "The Scientific Work of Theodore William Richards" (1970). Kopperl teaches in the School of Biomedical and Health Sciences of Grand Valley State College.

Diane O'Neil Johnson (1929-), "Edwin Broun Fred: Scientist, Administrator, Gentleman" (1971) Johnson worked for the University of Wisconsin athletic department, and did not pursue an academic career.

Robert K. DeKosky (1945-), "The Scientific Work of Sir William Crookes" (1972). DeKosky taught first at the University of Wisconsin-Milwaukee, then moved to the University of Kansas, where he is associate professor.

Robert J. Paradowski (1940-), "The Structural Chemistry of Linus Pauling" (1972). Paradowski was hired at Eisenhower College, then went to Rochester Institute of Technology, where he is professor in the Program in Science, Technology, and Society.

Charles H. Trottman (1934-), "Edwin Bret Hart: Agricultural Chemist" (1972). Trottman taught at Jackson State University (Mississippi); I do not know his current location.

Alan J. Rocke (1948-), "Origins of the Structural Theory in Organic Chemistry" (1975). Rocke is Bourne Professor of History at Case Western Reserve University.

Stephen L. George (1946-), "The Origins and Discovery of the First Nitrated Organic Explosives" (1977). George taught in the Chemistry Department of Millikin University, Dekatur, Illinois; I do not know his current location.

Christopher S. Hamlin (1951-), "What Becomes of Pollution? Adversary Science and the Controversy on the Self-Purification of Rivers in Britain, 1850-1900" (1982). Hamlin is Professor of History, and currently department chair, at the University of Notre Dame.

Sheldon Hochheiser (1951-), "Synthetic Food Colors in the United States: A History Under Regulation" (1982). Hochheiser taught at Rensselaer Polytechnic Institute and the University of Minnesota; in 1988 he became Corporate Historian of AT&T.

Phyllis Anderson Meyer (1945-), "The Last *Per Se*: The Delaney Cancer Clause in United States Food Regulation" (1983). Meyer is currently associate professor of chemistry at St. Xavier University, Chicago.

Appendix 2.

Books Written by Students of Aaron Ihde (alphabetical by author, not including edited books)

A. Albert Baker, Jr., *Unsaturation in Organic Chemistry*, Houghton-Mifflin, Boston, MA, 1968.

Albert B. Costa, *Michel Eugène Chevreul, Pioneer of Organic Chemistry*, Wisconsin State Historical Society, Madison, WI, 1962.

Robert K. DeKosky, *Knowledge and Cosmos: Development and Decline of the Medieval Perspective*, University Press of America, Washington, DC, 1979.

Christopher Hamlin, *What Becomes of Pollution? Adversary Science and the Controversy of the Self-Purification of Rivers in Britain, 1850-1900*, Garland, New York, 1987.

Christopher Hamlin, *A Science of Impurity: Water Analysis in Nineteenth-Century Britain*, University of California Press, Berkeley, CA, 1990.

Christopher Hamlin and Philip Shepard, *Deep Disagreement in U.S. Agriculture: Making Sense of the Policy Conflict*, Westview, Boulder, CO, 1993.

Christopher Hamlin, *Public Health and Social Justice in the Age of Chadwick: Britain, 1800-54*, Cambridge University Press, Cambridge, 1998.

Sheldon Hochheiser, *Rohm and Haas: History of a Chemical Company*, University of Pennsylvania Press, Philadelphia, PA, 1986.

Bruce H. Bruemmer and Sheldon Hochheiser, *The High-Technology Company: A Historical Research and Archival Guide*, Charles Babbage Institute, Minneapolis, MN, 1989.

Reese V. Jenkins, *Images and Enterprise: Technology and the American Photographic Industry, 1839 to 1925*, Johns Hopkins University Press, Baltimore, MD, 1975.

Diane Johnson, *Edwin Broun Fred: Scientist, Administrator, Gentleman*, University of Wisconsin Press, Madison, WI, 1974.

Carl Buckner, Kenneth A. Connors, John Parascandola, Glenn Sonnedecker, and George Zografi, *The University of Wisconsin School of Pharmacy: Its First Century*, Office of University Publications, Madison, WI, 1997.

John Parascandola, *The Development of American Pharmacology: John J. Abel and the Shaping of a Discipline*, Johns Hopkins University Press, Baltimore, MD, 1992.

Alan J. Roche, *Chemical Atomism in the Nineteenth Century: From Dalton to Cannizzaro* Ohio State University Press, Columbus, OH, 1984.

Alan J. Roche, *The Quiet Revolution: Hermann Kolbe and the Science of Organic Chemistry*, University of California Press, Berkeley, CA, 1993.

Alan J. Roche, *Nationalizing Science: Adolphe Wurtz and the Battle for French Chemistry*, MIT Press, Cambridge, MA, 2000.

James Whorton, *Before Silent Spring: Pesticides and Public Health in Pre-DDT America* Princeton University Press, Princeton, NJ, 1974.

James Whorton, *Crusaders for Fitness: The History of American Health Reformers* Princeton University Press, Princeton, NJ, 1982.

James Whorton, *Inner Hygiene: Constipation and the Pursuit of Health in Modern Society*, Oxford University Press, Oxford, 2000.

FUTURE ACS MEETINGS

Spring 2001 — San Diego, CA

Fall 2001 — Chicago, IL

Spring, 2002—Orlando, FL

Fall, 2002—Boston, MA

Spring, 2003—New Orleans, LA

Fall, 2003—New York, NY

Spring, 2004—Anaheim, CA

Fall, 2004—Philadelphia, Pa

Spring, 2005—San Diego, CA

Fall, 2005—Washington, DC

Spring, 2006—Atlanta, GA

Fall, 2006—San Francisco, CA

Spring, 2007—Chicago, IL

Fall, 2007—Boston, MA

Spring, 2008—San Antonio, TX

Fall, 2008—Philadelphia, PA

Spring, 2009—Salt Lake City, UT

Fall, 2009—Washington, DC

Spring, 2010—San Francisco, CA

Fall, 2010—New York, NY

AARON IHDE'S CONTRIBUTIONS TO THE HISTORY OF CHEMISTRY

William B. Jensen, University of Cincinnati

Aaron Ihde's career at the University of Wisconsin spanned more than 60 years — first as a student, then as a faculty member, and, finally, as professor emeritus. The intellectual fruits of those six decades can be found in his collected papers, which occupy seven bound volumes in the stacks of the Memorial Library in Madison, Wisconsin. His complete bibliography lists more than 342 items, inclusive of the posthumous paper printed in this issue of the *Bulletin*. Of these, roughly 35 are actually the publications of his students and postdoctoral fellows; 64 deal with chemical research, education, and departmental matters, and 92 are book reviews. Roughly another 19 involve multiple editions of his books, reprintings of various papers, letters to newspaper editors, etc. The remaining 132 items represent his legacy to the history of the chemistry community and appear in the attached bibliography (1).

Textbooks

There is little doubt that Aaron's textbooks represent his most important contribution to the history of chemistry (2). The best known of these are, of course, his *The Development of Modern Chemistry*, first published by Harper and Row in 1964 and still available as a quality Dover paperback, and his volume of *Selected Readings in the History of Chemistry*, culled from the pages of the *Journal of Chemical Education* and coedited by the journal's editor, William Kieffer. Less well known outside the circle of his students and colleagues at the University of Wisconsin are his general science text, *The Physical Universe*, which was used for many years in the general science course that he taught for the Integrated Liberal Studies program, and his *The Dawn of Chemistry, A History of the Study of Matter Before*



Aaron Ihde, 1968

Dalton. This latter material was used in the form of photocopied handouts as the text for the first semester of his introductory history of chemistry course and was, in fact, the manuscript for a projected book designed to supplement *The Development of Modern Chemistry*, which, as its title implies, began its coverage of events starting in the late 18th century. Unfortunately, Aaron never got around to revising this material for final publication before his retirement. In 1992 I approached him on behalf of the new ACS history series with a proposal for its possible publication, but by then he felt that too much time had passed since he had done the original research for the manuscript and that he no longer had

either the time nor the interest to do the necessary revisions and updates.

A number of excellent translated German (Ladenberg, 1905; von Meyer, 1906) and British (Pattison Muir, 1906; Hilditch, 1922; Marsh, 1929; Thorpe, 1930; Holmyard, 1931) history of chemistry texts had appeared during the first three decades of the 20th century, but by the early 1950s most of them were badly outdated. The only serious candidates available for use in a general history of chemistry course intended for chemistry majors were *A Short History of Chemistry*, by the British chemist and historian, James Partington (first published in 1937), the later revised editions of *A History of Chemistry* by the American chemist F. J. Moore (3rd edition 1939), and the more recently published 1952 and 1956 texts, *The Evolution of Chemistry* and *The Historical Background of Chemistry*, by Aaron's contemporaries, Edward Farber and Henry Leicester. But again none of these books provided any substantive coverage of events after about 1923. Consequently, the appearance of Aaron's *The Development of Modern Chemistry* in 1964 filled a well defined need. Not only did the book extend its coverage of events through the early 1950s, it was far more thorough than any of its predecessors, was impeccably documented, and contained an unprecedented number of high-quality illustrations and portraits.

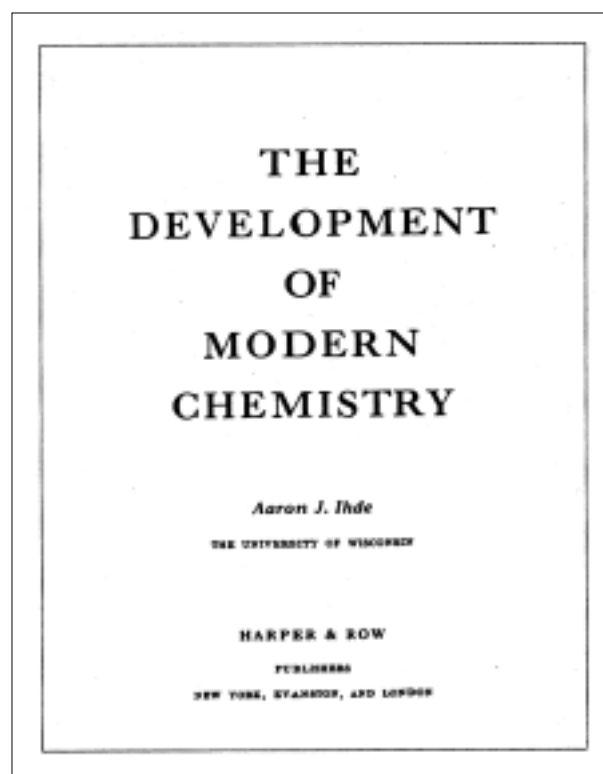
I can still recall being told as a junior in high school that the school library had just received a new history of chemistry written by none other than the father of our new high school chemistry teacher, John Ihde. I immediately checked it out during my first free period and spent the afternoon thumbing through its heavily illustrated pages, much to the irritation of my 5th-period physics teacher, who felt I should be taking class notes on Newton's laws of motion instead.

Though *The Development of Modern Chemistry* was well received when it was first published, Aaron later came in for his share of criticism by the newer generation of professional historians of science, who throughout the 1960s and 1970s indulged themselves in an immature orgy of repudiating the work of earlier historians, largely by the device of accusing them of the imaginary sin of historical whiggism (3). I once asked Aaron how he felt about this. Though he was obviously unhappy about this turn of events, he also felt that it was a fad that would eventually pass. "Their turn will come," he told me. Subsequent events have largely justified Aaron's equanimity. Roughly a dozen general histories of chemistry have been published since 1964, none of

which has come close to supplanting Aaron's text. Only Partington's massive, four-volume *A History of Chemistry*, which was intended as a reference work, rather than as a textbook, has surpassed its level of scholarship, and only William Brock's recently published *Norton History of Chemistry* has equaled it for clarity of writing and mature judgment, though still falling short of its breadth of coverage.

General Writings on the History of Chemistry

Most chemists who become active in the history of chemistry are initially attracted to the field through their admiration for the work of some famous scientist. An unfortunate consequence of this motivation is that their historical efforts are frequently marred by an uncritical sense of hero worship. Conversely, professional historians tend to become narrowly focused on one particular era, such as the Renaissance, the Chemical Revolution, the rise of organic chemistry, etc., often to the exclusion of other periods — a fact which largely accounts for their failure to produce any general histories of chemistry. Aaron managed to avoid both of these pitfalls. Indeed, his general writings on the history of chemistry are best described as eclectic and range from the prehistory of spectrum analysis, antecedents of Boyle's famous



definition of a chemical element, and the importance of Faraday's laws of electrolysis through the history of isomerism, early research on the isolation of free radicals, and Boyle's speculations on the degradation of gold (4). This breadth of interest was no doubt due to the fact that his work in history of chemistry was always firmly rooted in his primary commitment to the teaching of a general history of chemistry course for chemistry majors.

His attitude toward hero worship and the cult of the unique genius in science is most explicit in one of his earliest published papers, entitled "The Inevitability of Scientific Discovery," in which he argued that the interlocking nature or "consilience" — to use William Whewell's famous phrase — of scientific fact and theory makes the eventual discovery of new facts and theories almost certain (5). As scientific communication increases and more and more pieces of the overall picture are uncovered, the process of discovery becomes more and more inevitable, and examples of simultaneous discovery become more and more common. In other words, had there been no Linus Pauling, modern 20th-century chemistry would probably still include the concepts of bond hybridization, resonance, and the thermochemical electronegativity scale.

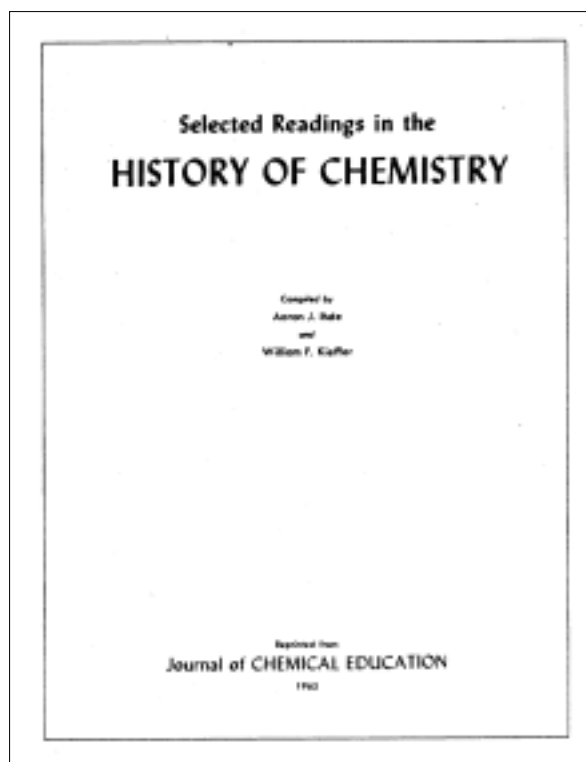
Roughly 29 of the items in Aaron's historical bibliography may be placed in this general history of chemistry category. Of these, one of the most important, in my opinion, is a 1969 paper done in collaboration with John Parascandola on the "History of the Pneumatic Trough," which is a model study of the important role of apparatus innovation in the history of chemistry — a topic sadly neglected by most textbook authors (6).

History of Chemistry and the Curriculum

Not only did Aaron do historical research, he was also a tireless advocate of the importance of teaching the history of science to science and nonscience majors alike. Roughly 16 of the items in his bibliography deal with this subject or with the description of the history of chemistry program and historical resources at the University of Wisconsin — the earliest appearing in 1951 and the most recent in 1990 (7). His vision of the role of history of chemistry in the training of professional chemists was well expressed in his 1971 paper, "Let's Teach History of Chemistry to Chemists!" in which he wrote (8):

There is no question that we can train a chemical technologist without teaching him any history of chem-

istry and he may be a very good technologist indeed. I would argue with equal vehemence that we cannot *educate* a chemist without history of chemistry. I am interested, and I believe most of us are, in the *education* rather than the training of chemists. The person who is merely trained to carry out analyses or syntheses can do his job quite satisfactorily without much chemical theory or any history of chemistry. On the other hand, the chemist who is in a position of responsibility for the planning of investigations needs to know something about the past history of chemical investigation and the development of chemical thought. Without such knowledge he is merely a technologist.



History of Nutrition and Biochemistry

Though Aaron's work in history of chemistry was wide ranging, he did have a strong specialized interest in the history of nutrition and food chemistry, reflecting his own graduate training in this field under the direction of Professor Henry A. Schuette in the 1930s and early 1940s. This interest eventually widened to include aspects of the history of biochemistry, pharmacology, and environmentalism as well. Roughly 20 items in Aaron's bibliography fall into this category, including his very first contribution to the field — an historical study of maple sugar done in collaboration with Dr. Schuette (9).

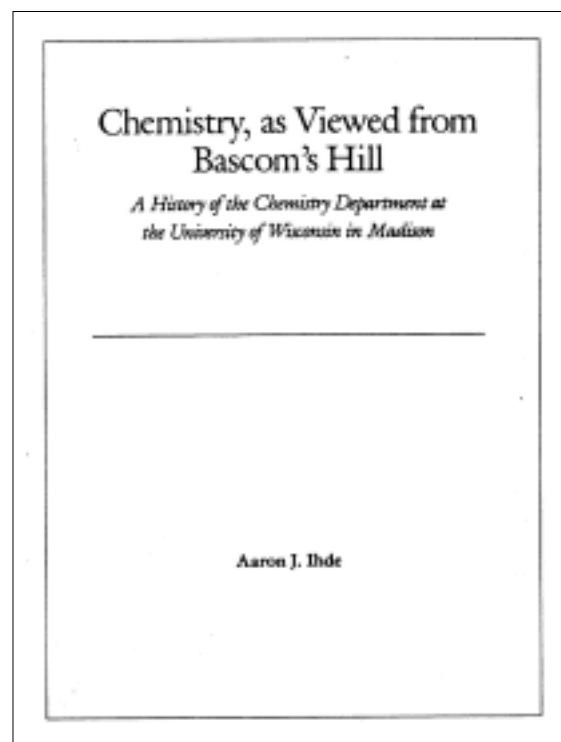
History of Chemistry at the University of Wisconsin

Aaron's entire academic career was spent at the University of Wisconsin, beginning with his undergraduate training in the late 1920s. Hence it is not surprising that about 11 items in his bibliography, excluding the biographical material discussed in the next section, deal either with the history of the chemistry department at Madison or with the history of chemical technology and education in Wisconsin and the Midwest in general (10).

Aaron's office at the university was a veritable museum of departmental history. Here one could study the research notebooks of Villiers Meloche, handle chemical samples prepared by Farrington Daniels or by Victor Lenher, or examine the rotor to the prototype of the ultracentrifuge developed by The Svedberg in 1923 during his year as a Visiting Professor at Wisconsin. There were large ring binders containing hundreds of photographs of famous chemists and former faculty, many obtained from the files of deceased colleagues and later used in Aaron's books. And, of course, there was Aaron's wonderful personal library. Again, this included many chemical classics that Aaron had inherited from retired and/or deceased faculty, as well as the entire personal library of Charles Joy (1823-1891), who had been professor of chemistry at Columbia in the 1860s and 1870s. Joy's daughter had donated the library to Beloit College sometime in the early 20th century; and Beloit, in turn, had dumped it into a library discard sale sometime in the 1950s. Aaron had been lucky enough to attend the sale and was able to purchase the entire collection at 25¢ a volume. Many of the items in the Joy collection dated back to his student days at Göttingen in the late 1840s and early 1850s.

When, as a graduate student, I needed a break from the drudgery of laboratory research, I would spend it in the back room of Aaron's office complex poring through his accumulated treasures. Typical of his generous nature was the fact that, if any of these items — however valuable or rare — caught one's attention, it could be borrowed, almost indefinitely, by the simple act of scribbling a short note on an index card and dropping it into the small metal box kept on the shelf near the door in the outer office.

Aaron's interest in departmental history culminated in 1990 with the publication of his book, *Chemistry as Viewed from Bascom's Hill, A History of the Chemistry Department at the University of Wisconsin in Madison*

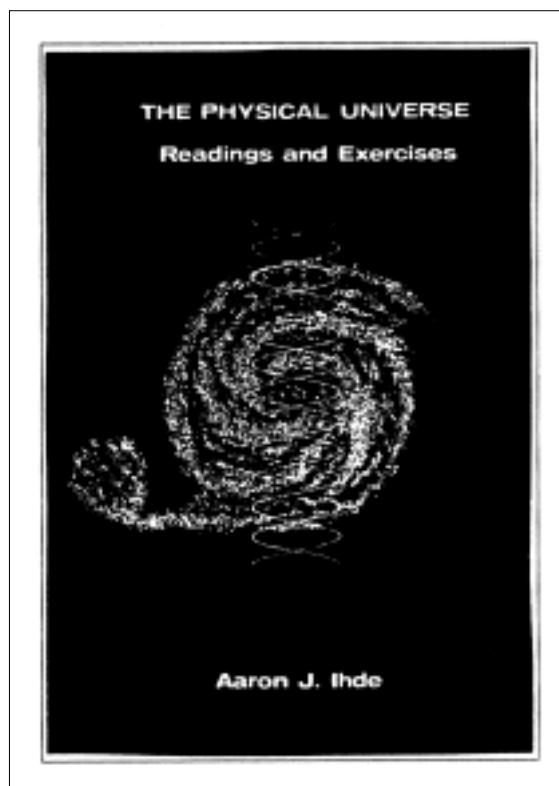


(11). Weighing in at 688 pages, it is probably the most thoroughly researched departmental history ever to appear in print.

Chemical Biography

Like his contemporary, Ralph Oesper of the University of Cincinnati, Aaron had a strong interest in chemical biography. Nearly 55 items in his bibliography fall into this category (12). These range from short entries in popular encyclopedias and dictionaries, such as *World Book Encyclopedia*, *Encyclopedia Britannica*, *Encyclopedia Americana*, *Harper's Encyclopedia of Science*, *Biographic Encyclopedia of Scientists and Inventors*, *Dictionary of Wisconsin Biography*, and the *Dictionary of American Biography*, to more scholarly entries in such works as Gillispie's famous *Dictionary of Scientific Biography*, Wyndham Miles' *American Chemists and Chemical Engineers*, and Edward Farber's book, *Great Chemists*. Many of these biographical entries involved chemists associated with the history of the department at Madison and so reflected Aaron's interest in departmental history, whereas others overlapped with his interest in the history of nutrition, food science, and biochemistry.

Aaron felt that contributing to dictionaries and encyclopedias was one of the social responsibilities of an



historian, not unlike a chemist contributing to published tables of chemical and physical data. Indeed, one of the tasks assigned to students in his advanced seminar on the history of chemistry was to learn the art of writing for dictionaries and encyclopedias by selecting the names of three chemists mentioned in *The Development of Modern Chemistry* and writing a concise biographical summary of not more than 750 words on each of them.

On more than one occasion, Aaron expressed his great admiration for Wyndham Miles' biographical dictionary of *American Chemists and Chemical Engineers*, for which Aaron wrote seven entries. This reflected his attitude that science was the accumulative result of the efforts of many scientists and not just the result of sporadic flashes of genius on the part of a privileged few. In keeping with this, he felt that the lives documented in Miles' compilation gave a far more accurate picture of a typical scientist and of the day to day practice of science than did the detailed biographies of the more famous.

Perspective

Aaron's legacy to history of chemistry is not only rich and diverse, it is also unique. Unlike most of his contemporaries in the Division of the History of Chemis-

try, who were able to pursue history only as a side line to their primary jobs, Aaron succeeded in molding his appointment at Wisconsin into a full-time position in the history of chemistry, complete with doctoral students and postdoctoral fellows (though not without some opposition, as he was fond of telling). In many ways his years at Wisconsin represent the apogee of constructive interaction between the humanities and science. They were a model for what the historical perspective could do for the training of professional chemists and what intimate day to day contact with practicing chemists could do for historians (13). Regrettably this particular confluence of events is unlikely to happen again. The chemistry department at Wisconsin has not seen fit to replace Aaron, and the rise of history of science as a separate profession has increasingly led to its isolation within history departments and to its progressive domination by the social sciences. To those of us who experienced the Ihde years at Wisconsin, these trends represent a great loss for chemistry and history alike.

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2. Items 41, 42, 47, and 53 in the bibliography.
3. See, for example, the condescending remarks in A. Thackray, "The Chemistry of History," *Hist. Sci.*, **1966**, 5, 124-134.
4. Items 2-3, 8, 11-12, 15-17, 19, 21-22, 30-31, 43-45, 48, 50, 52, 56, 60-63, 96, 99, 122-123, 129 in the bibliography.
5. Item 2 in the bibliography.
6. Item 63 in the bibliography.
7. Items 4, 6, 10, 13, 20, 37, 49, 54-55, 59, 73-74, 120, 125, 127, 130 in the bibliography.
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9. Items 1, 7, 35, 39-40, 51, 57-58, 61, 67, 69, 88-89, 91, 95, 97-98, 100, 113, 128 in the bibliography.
10. Items 5, 9, 14, 18, 46, 114-116, 126, 131-132 in the bibliography.
11. Item 131 in the bibliography
12. Items 7, 23-29, 32-34, 36, 38, 64-66, 68, 70-72, 75-87, 90, 92-94, 101-113, 117-119, 121, 124 in the bibliography.

13. It would be less than honest to claim that these interdisciplinary interactions always went smoothly. Aaron once told me that he was asked to write an inorganic cumulative examination for the second-year graduate students. It consisted of only one question, "Given an analytical balance, standard laboratory glassware and chemical reagents—but no mass spectrometer—outline how you would go about determining a table of self-consistent atomic weights." In light of the modern-day graduate student's well publicized ignorance of both descriptive and classical chemistry, it goes without saying that the results were a total disaster; not a single student passed the cume! "And you know," mused Aaron, "I was never asked to write another one."

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MEMORIES OF AARON IHDE

Robert Siegfried, University of Wisconsin

My very positive memories of Aaron Ihde are both professional and personal in nature. Indeed, these two aspects can hardly be distinguished, for nearly everything he did for me in my career he did in a very personal way. Yet his personal approach reached beyond only the professional. He and I were wonderful friends for more than fifty years.

My earliest clear memory of Aaron Ihde was a meeting after a Sunday service of the First Unitarian Society back in 1947, shortly after I had started attending their services regularly, then being held at the YWCA on State Street in Madison. I think he recognized me as a graduate student in the Department of Chemistry and, after our personal introduction, I identified his name as that of a chemistry professor. Only later did I become his teaching assistant and take one of his courses.

In the spring semester of 1948 I enrolled in his enjoyable course in the history of chemistry, where we



Aaron Ihde (left), Sidney Edelstein (center), and Robert Siegfried (right) caught in animated conversation at the 129th Annual ACS Meeting held in Dallas, Texas, in the Spring of 1956.

became much better acquainted. That was the time when the College of Letters and Science was organizing the general educational program known as Integrated Liberal Studies (ILS). Aaron was to be the teacher of the freshman course in general physical science. He asked me to be his teaching assistant, and I was pleased to accept the offer. That turned out to be one of the most rewarding experiences I ever had!

At about the same time Aaron invited me to accompany him to a conference on general education held on the University of Wisconsin campus. The session was devoted to a discussion of diverse ways for effectively presenting scientific material to the general student; and, as is usual with such discussions, no consensus was apparent. As we left, Aaron remarked, "It probably doesn't matter very much what material you include if you have the right teacher." I didn't know whether he thought of himself as the "right teacher," although I suspected that he did; and during the next four years as the teaching assistant for his ILS course, "The Physical Universe," I definitely learned that he was indeed just such a "right teacher." The course was marvelously taught, not only

for the freshman students, but also for me as the leader in their discussion classes. Back in those days, the freshmen often volunteered the so-called "UW skyrocket," a combination of cheers and applause for Aaron, sometimes in the middle of an especially spectacular lecture demonstration, sometimes at the end of a lecture of particular moral intensity, such as that about Galileo and freedom of inquiry—but always at the end of the semester. I often joined the students in their show of enthusiasm.

From Aaron's lectures I learned the value of history of science in basic science education. He offered historical examples of scientific ideas in ways that basically illustrated the virtue of the growing human understanding of the physical world in which we live. Since I was simultaneously enrolled in various courses in the history of science, this teaching experience became the fundamental theme of my own lifelong teaching philosophy: the nature of science as the search for functional truth, as well as the specifics of scientific knowledge, or, as has been said elsewhere, "Science without its history is like a man without a memory."

After I had become his first doctoral student in history of chemistry—rather than just chemistry—I remember having a talk with Aaron in which he contemplated confining all his graduate students to history of chemistry, a decision he later carried out. I carefully avoided giving my opinion on this decision, but I felt honored that he had sought my viewpoint.

Because my excellent chemistry professor at Marietta College had been an undergraduate at Ripon College in Wisconsin, I started some historical explorations of chemical education at that college. Aaron suggested many valuable sources for that kind of research and also stimulated me to explore the chemical history of Beloit College as well. He had already studied the chemical history of Lawrence College, and we presented these results jointly at a meeting of the Wisconsin Academy of Science, Arts, and Letters held at Beloit College in 1952. This was the first published paper with my name on it, as well as that of Aaron.

In the 1951-1952 academic year Aaron went with his family to Boston to gain information on Harvard's general education program, which was similar to that of Wisconsin's ILS program. He recommended to the administrators that I serve as his substitute in teaching the ILS freshman physical science course; but they would not allow a graduate student to take on that responsibility. As a result, other faculty members were selected

for the different subjects Aaron typically offered in the course: astronomy, physics, and chemistry. Because I was still one of the teaching assistants, however, Aaron assigned me the continuing task of attending the lectures and of adapting and "correcting" their different approaches, in order to retain the educational spirit of Aaron's tradition.

In the summer of 1952 Aaron hired me to be his teaching assistant for the freshman course in chemistry, where I met with the students in their laboratory work. I soon discovered that one of the students had been a member of the university rowing crew the previous spring. I informed him that his teacher, Dr. Ihde, had been a varsity crew member at Wisconsin back in 1931, and that I had been on the crew team at Marietta College when we had raced with Wisconsin in 1941. This stimulated the student to gather other varsity crewmen, so that Aaron and I could do some rowing again. Aaron chose to be the stroke oarsman, Number 8, while I was Number 7, so that we old folks were in a position to stop all rowing if the experience proved too much. But we had a good time!

I remember that, when I finished my doctorate degree at the end of the summer of 1952, Aaron and his wife invited my wife, our two young children, and me to have dinner with them out in Mineral Point, at the historically famous Pendarvis Building. It was a joyous family-like gathering with splendid food!

Many of these personal relationships continued during the next eleven years, when I was living elsewhere, for we exchanged frequent letters in which we shared our personal and professional experiences and intentions. An outstanding example of Aaron's confidence in me was his recommendation to Harper & Row that I evaluate a prepublication copy of his book, *The Development of Modern Chemistry*. Needless to say, I approved.

In the autumn of 1953 at the American Chemistry Society meeting in Chicago we shared time together and exchanged our responses to the history of chemistry presentations we heard there. This kind of thing happened frequently in the years before I returned to the University of Wisconsin. During that time I particularly remember that he invited me to come to Madison, then to share in the driving as Aaron and his wife proceeded to the American Chemical Society meeting in Minneapolis. The event that stands out as most charming, however, was our attendance at the ACS meeting in Dallas. At that time I was teaching at the University of Arkan-

sas, and I invited Aaron and his wife to stop in Fayetteville for a visit. As it turned out, they spent the weekend with us and then drove my wife and me to Dallas. We had a delightful time together. While at Arkansas, I became one of the three founders of the Midwest History of Science Junto, and I invited Aaron to attend its first meeting in Lawrence, Kansas in 1958 and to bring other members of Wisconsin's history of science faculty. Many did attend and became charter members.

Aaron had supported my applications for employment for the three positions I held after leaving Wisconsin in 1952; but most significantly, he was the initiator in the Wisconsin History of Science Department's decision to invite me to return to Wisconsin in 1963. Once I was back in Madison, I felt like a family member as we met socially with Aaron and his wife Olive, who was equally as gracious as her husband.

On the occasion of his being awarded the Dexter Prize for his contributions to the history of chemistry in 1968, Aaron invited me to give an introductory talk before the History of Chemistry Division at the American Chemical Society meeting in Atlantic City, NJ. It was an honor for me to be personally chosen by Aaron. In 1978 Aaron was nominated for a Distinguished Teaching Award at the University of Wisconsin. The supporting evidence for this well deserved honor was outstanding, contributed by the entire History of Science Department and by his colleagues in ILS.

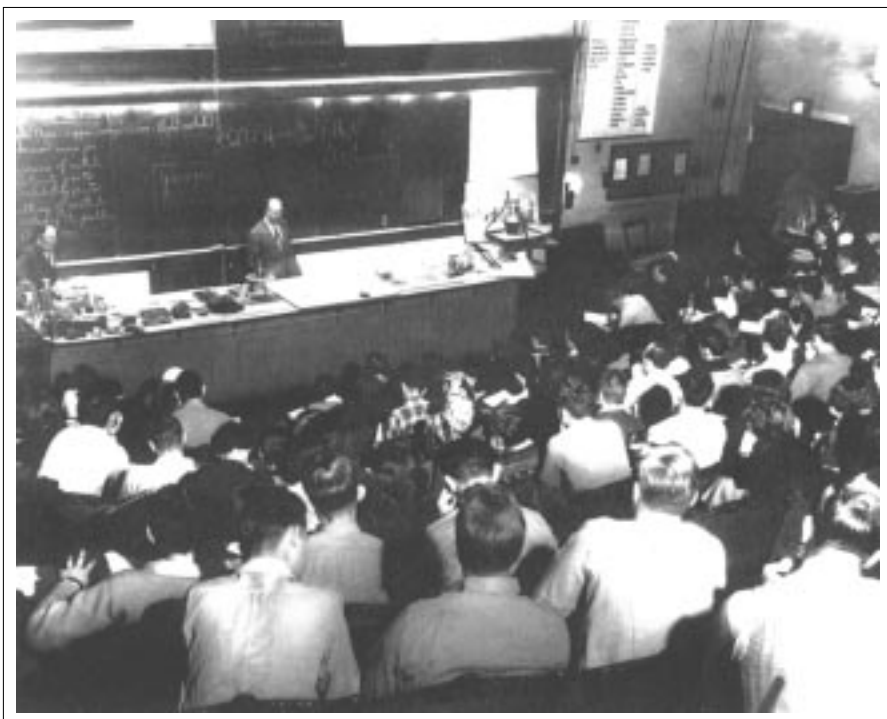
When he retired in 1980, the History of Science Department planned a celebration for Aaron to coincide with the Midwest History of Science Junto meeting in Madison. As one of the founders of that society, which Aaron had joined as a charter member in 1958, I organized that meeting by sending letters to all of Aaron's present and former graduate students, asking them to send him a personal letter of recollections, together with a scholarly paper written specially for the occasion. They were also invited to attend and to participate in the special activities to be held. The impressive result was that

nearly every one of the students sent both letters and papers, and most also came to Madison. There were 18 papers in all, one of my own, of course. Just as their response was personal, so Aaron later wrote a personal letter to me in thanks for organizing the occasion.

I have offered these memories in order to illustrate how personal Aaron was with me as my major professor. He was not just an excellent professor and teacher but, from the very beginning of our acquaintance, also a very good friend. In our relationship there was never any distinction between his personal friendship and professional communication. Aaron's professional sharing was always done in a friendly way.

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Aaron lecturing to his ILS class at the University of Wisconsin in January of 1949.

CRITERIA FOR GENEALOGICAL ROOTS⁽¹⁾

Aaron J. Ihde

It was almost five decades ago that I revived a then defunct course in history of chemistry at the University of Wisconsin. At that time I was aware of the genealogy prepared by Virginia Bartow (2), who dealt with the chemistry department at the University of Illinois. When I finished the semester I wrote lineages on the blackboard to show my students the genealogy of the Wisconsin department. The students became aware of how their professor fit into the chemical heritage we had been exploring during the semester. The device was a success, and I continued to finish the course with a period devoted to their own heritage. As new faculty joined the department, I inquired about their own graduate education and fitted them into the Wisconsin genealogy. I found that even when they had studied under someone not in the present Wisconsin genealogy, it was generally easy to trace them back in a step or two to a chemist already there.

In 1949 I presented the then current genealogy at the annual meeting of the Wisconsin Academy of Science, Arts, and Letters. Handouts I prepared included the professors of satellite chemistry departments in agriculture, engineering, medicine, and pharmacy. It raised a good deal of discussion and suggested that genealogies of other academic disciplines might be prepared. I am not aware that any such lineages were ever prepared at Wisconsin.

Updating the Wisconsin lineage became critical after World War II on two accounts: the death or retirement of several faculty leaders and the rapid expansion of enrollment in the post-war era. Sudden increases in faculty between 1946 and 1955 were common, and I then saw that incorporation of new men (there were no women) into the chart had best await their becoming

tenured. During this period the classification of "instructor" almost became obsolete because universities were competing for newly minted doctors of philosophy. It is my criterion, therefore, that nontenured faculty not be incorporated in a departmental lineage until the person becomes tenured, particularly if the genealogy is to be published.

Although I continued to use the Wisconsin lineage annually when the history course drew to a close, I became somewhat derelict in keeping the succession up to date as new faculty became part of the department or moved elsewhere. I continued to think of publication but that always raised questions about updating and resolution of uncertainties. Finally, in the mid 1970s, Alan Rocke had finished his doctorate but was without a job. Teaching positions were not readily available in the 1970s, particularly in the history of science. However, I was beginning to work on what ultimately became a history of the Wisconsin Chemistry Department (3) and was able to obtain a grant out of the bequest which Professor S. M. McElvain had willed the department a few years earlier to hire Alan to do some archival work for me.

Since I considered the faculty intellectual heritage a part of the departmental history, and since I had never found time to update the departmental lineage, I suggested that he become a co-author of the version we finally published in 1979 (4). Alan was largely responsible for the final layout of the genealogy and assembling the necessary background. I remained active in overseeing the project and am responsible for all final decisions, including those that turned out to be questionable. I remember that we were unsure of the Woodward lineage. Alan resolved this by calling Woodward

and was told that Avery A. Ashdown was the correct person, although he was out of the country when Woodward finished his degree. As a result his signature fails to appear on the principal documents connected with the degree awarded to Woodward at MIT in 1937.

I would argue that, if the person involved is still living, he or she is the most reliable individual regarding the major professor. That person is also the best one to ask if there is any suspicion that the designated major professor was a fraud and someone else was truly the major influence. I do believe also, that one should be skeptical of those who insist that their post-doctoral guide is the most important, particularly when the name of the post-doctoral professor is much better known (i.e., Adams, Woodward, Seaborg) than the Ph.D. sponsor. Very often the post-doctoral professor is merely a busy person who provides an income and a laboratory while he is away from the department attending conferences, giving lectures, and fulfilling consulting obligations.

When Alan and I had resolved our problems, completed the chart, and written the accompanying manuscript, we sent the finished product to William Lippincott, then editor of the *Journal of Chemical Education*. In due time he informed us that he had recently established a policy not to accept any more departmental genealogies for publication, but in this case he would break his policy. Then, never again! Despite the failure of the *Journal* to publish subsequent genealogies, I know that the pursuit of chemical heredity has not been abandoned. I have received frequent questions and have been challenged about some of our decisions as revealed in our published version in 1979. When visiting other chemistry departments, I have also been shown charts of their genealogies, some of them being displayed permanently on a wall of the chemistry building.

The Roger Adams niche has received the greatest number of challenges. Virginia Bartow traced him to Torrey and Richards jointly and connected Torrey to C. L. Jackson (2). I knew that Henry A. Torrey was a professor in the Harvard department and that Adams had begun graduate studies with him before he died in 1910. Although I was aware that Adams did not receive the doctorate until 1912, and that Adams was brilliant in organic chemistry, I persuaded Alan that Torrey deserved recognition and should not be lost to history. Very soon after publication I received letters from other chemists challenging my decision. While I agreed that C. Loring Jackson as the major professor certainly had merit because he helped guide Adams to the Ph.D. af-

ter Torrey's death, I failed to make a public effort to change the published version. After D. Stanley Tarbell and Ann Tarbell published their biography of Roger Adams in 1981 (5), I studied Adams' days at Harvard very carefully and felt somewhat content to leave Torrey as the principal influence on him. As a sophomore, Adams completed two half course in organic chemistry with Torrey (no laboratory), historical and elementary physical chemistry under C. R. Sanger and G. P. Baxter, respectively (D in historical, C in physical). In his junior year Adams took a year long course in organic chemistry with Torrey (grade A). By now he had completed enough courses to graduate, but he chose to combine the senior year with graduate studies and included a course in organic reactions given by Torrey (grade A) and a class with T. W. Richards in physical measurements. After Torrey's death in March, 1910 Adams was obviously moving ahead in studies of organic reactions. His work now came under the guidance of C. L. Jackson, as far as his studies on alkali-insoluble phenols were concerned. He received the A.M. degree that spring. The next year he completed his work on physical measurements with Richards, who agreed to let him take his examinations in analytical and inorganic chemistry in June, 1911. Richards is sometimes mentioned as being a joint major professor of Adams, but available information appears to suggest that he merely supervised a minor study. The Ph.D. dissertation, submitted in May, 1912, dealt with three separate investigations, all organic. In the light of the information in Tarbell's book, I am inclined to correct one error and change my position on Torrey as follows: 1) eliminate the solid line between Richards and Torrey since Torrey took no degree under Richards; 2) because Torrey received his Ph.D. in 1897 under Professor Henry B. Hill, an A.B. student of Josiah Parsons Cooke, insert the name of Hill under Torrey and run a solid line down from Torrey to Hill and another solid line from Hill over to Cooke; 3) insert the name of C. L. Jackson in the space under Richards and Hill, another solid line up to R. Adams, and another down to Cooke, since Jackson took his A.B. in 1867 and his A.M. in 1870 under Cooke. Jackson also went to Europe, where he heard Bunsen's lectures at Heidelberg and Hofmann's lectures at Berlin but took no degree under either (6).

Josiah Parsons Cooke, the mentor of Hill, took no degree beyond the Harvard A.B. in 1848 as a student of John W. Webster, the Erving Professor of Chemistry and Mineralogy, who was hanged in 1850 for the murder of Dr. George Parkman, from whom Webster, a high liver,

had borrowed money he could not repay. Webster does not appear in the Wisconsin genealogy simply because Cooke spent a year in Europe where he attended the lectures of J. B. A. Dumas in Paris. When Cooke returned to Harvard, he became a tutor in mathematics for a year and then was awarded the now vacant Erving Professorship of Chemistry, which he held until his death in 1894. If Cooke had not expanded his studies by going to Europe, we would have brought Webster into the genealogy and would have entered a new lineage into our chart. Webster earned his chemistry from Aaron Dexter, the first professor of chemistry at Harvard Medical School, from 1783 to 1815, and the chemist after whom the Dexter Award was named. Dexter is the first in the Harvard chemical lineage, followed by John Gorman and John Webster. Dexter learned a bit of chemistry from John Winthrop, professor of natural philosophy at Harvard, and studied it more extensively during medical training with Samuel Danforth, Jr., a Boston physician.

A few thoughts are worth considering in preparation of a chart of transmission. If one examines published genealogies, one finds that the early transmissions include Berthollet, Fourcroy, and Berzelius. The Berthollet transmission moves through Gay-Lussac to Liebig. In truth, Liebig obtained his doctorate, not from Gay-Lussac in Paris but from Wilhelm G. Kastner at Erlangen. Liebig joined Kastner at Bonn in 1820 and then followed him to Erlangen a year later. He completed his doctorate there in 1822 but felt his instruction from Kastner was very inadequate. The Grand Duke of Hesse granted him a fund for further study in Paris. The quality of instruction he received from Gay-Lussac started him on his brilliant career in Gießen, where he attracted many students, including some Americans. In 1852 he transferred to the University of Munich; he accepted no more students in his laboratory, but a number of Americans attended his lectures there.

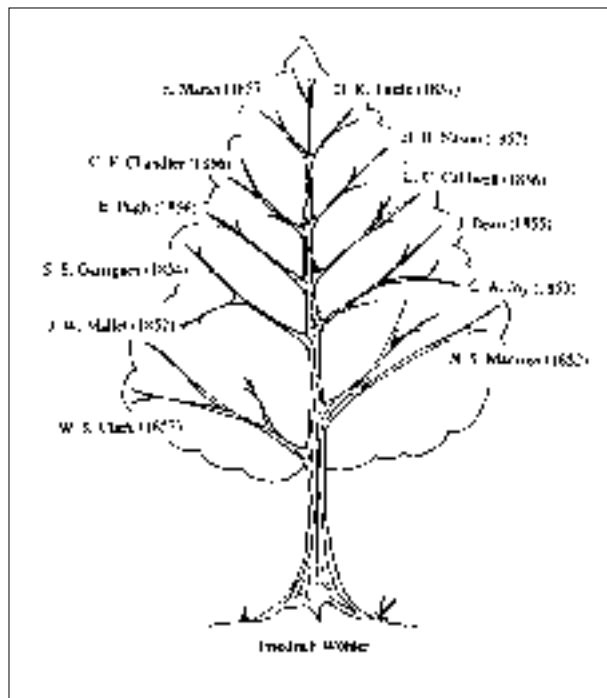
The Fourcroy lineage follows through Vauquelin, who trained Thenard and Stromeyer; and they in turn passed on their chemistry to Dumas and Bunsen, respectively. Both of the latter started their own groups of students, although neither attracted as many Americans as did Liebig and, especially, Wöhler. The latter spent a year with Berzelius after earning a medical degree at Heidelberg and then held minor teaching positions in Berlin and Kassel before setting up his famous program at Göttingen. Wöhler taught many students who became leaders in chemistry of the next generation.

Any attempt to extend the genealogy to earlier teachers has proven unproductive. Bartow introduced the name of Afzelius as the teacher of Berzelius (2); and Graham, in his McMaster genealogy (7), included Afzelius and T. O. Bergman, a man of varied scientific talents and vastly more competent in analytical chemistry than Afzelius, but hardly in a class with Berzelius. While it is

true that, when Berzelius studied medical sciences he attended chemistry classes of Afzelius, the latter offered little useful chemistry. Berzelius later acquired excellent knowledge of chemistry and physics through independent study and experimentation. In later years he added to his knowledge by travels in England, France, and Germany

There has been a temptation to introduce Lavoisier as the intellectual grandfather of our three lines of chemical development. This has been properly resisted, despite the fact that Berthollet and Fourcroy were contemporaries who knew Lavoisier through his contributions in the French Academy of Sciences. Although Berzelius never met Lavoisier, he was profoundly influenced by Lavoisier's chemical publications.

By the mid 1900s Americans began to appear for studies with the leaders in German laboratories, and this



Early American Doctoral Students of F. Wöhler, Göttingen

trend grew during the later decades. The trek to Germany began to decline by the beginning of the twentieth century, when chemistry professors with German degrees set up graduate programs in leading American universities.

Tracking of American lineages is seldom without problems. While university records are helpful, there is a lack of consistency between schools as to how graduate records are codified, how they are preserved, and who can gain access to them. Graduate schools vary greatly in the nature of requirements. As a result, investigators of chemical lineages report a variety of problems. One prestigious graduate school denies public access to dissertations for a period of years. Some libraries with space problems have actually destroyed certain kinds of theses.

Before I conclude, I would like to express discontent with the term "genealogy" as applied to chemistry students. I consider the term misleading in spite of having used it myself for a half century. I argue that we are not dealing with a genealogy at all but with a "Chart of Intellectual Succession." The term "genealogy" is related to the words generate, genus, genetics, and gene: that is, the areas of biology and biological succession, including the transmission of family characteristics. In the transmission of hereditary traits in the biological world, two parents of opposite sex are involved. In all of the "Chemical Genealogies" I have noted the almost total absence of the female gender. I would argue that the descendant is an intellectual descendant, not a biological one, because there is no transmission and pairing of genes. To be sure, there is transmission of ideas; but clearly lacking is a paired transmission of genes like that involved in family genealogy.

Perhaps I may be accused of raising a trivial distinction, particularly if we examine the reproduction of single-celled organisms that reproduce by cell division. Yet even here, there is evidence of exchange of genetic material under particular circumstances. In the botanical world, also, many plants can be propagated by slipping a part and placing it in nutrient solution or even in soil. In fruit-bearing trees the practice of grafting is widespread. However, in that part of the biological world we consider most common, propagation is by sexual activity. As an aside, I might call attention to the very highly developed genealogical records in the breeding of domesticated animals, not only in race horses, hunting and racing dogs, but in farm animals raised for production of milk, meat, eggs, or fur.

In what is termed chemical genealogy, the most important criterion is the transmission of chemical knowledge and skills. This suggests a serious partnership between master and apprentice during early maturity of the latter and influence with a major impact on the novice for the remainder of his career. In most cases the source of primary impact is the major professor who guides the work toward the Ph.D. degree. The choice of major professor may be made for a variety of reasons: an undergraduate's fascination with a talented teacher or a particular area of chemistry; interest aroused in the research pursued by a particular professor; opportunity for financial support in a particular department; or the recommendation by an undergraduate teacher. These are perhaps the principal, but not the only, reasons that an apprentice ends up being linked with a particular major professor. There is also the unforeseen instance where a candidate begins with a professor who dies within a year or two, moves to another university, or transfers to an industrial position. In cases where the professor moves, he may keep in touch by periodic visits, correspondence, telephone or FAX and return for the final examination. In other situations the university may have rules against research being directed *in absentia*, or there may be other reasons for selecting a new mentor in the home university. In such cases, the "genealogist" must make a wise decision as to who contributed most to the education of the candidate.

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THE HISTORY OF OZONE. THE SCHÖNBEIN PERIOD, 1839-1868

Mordecai B. Rubin, Technion-Israel Institute of Technology

Introduction

Ozone has been known as an accompaniment to electrical storms during all the history of mankind. Its first identification as a distinct chemical compound was due to Christian Friedrich Schönbein (1) (Fig. 1), Professor of Chemistry at the University of Basel from 1828. To a considerable extent he dominated the study of ozone chemistry until his death in 1868. The molecular formula of ozone was determined in 1865 by Soret and confirmed by him in 1867, shortly before Schönbein's death. The year 1999 marks the 200th anniversary of Schönbein's birth and is a fitting time for a presentation of the early history of ozone from the time of his first report through the rest of his lifetime. It is interesting to note that at least 13 citations of Schönbein's work on ozone have appeared in the chemical literature during the period 1988-98.



Christian Friedrich Schönbein, 1799-1868

Discovery

On March 13, 1839, Schönbein reported to the local Naturforschung Gesellschaft in Basel that the electroly-

sis of water produced an odor at the positive electrode which was the same as the odor produced by an arc between electrodes (2):

D. 13. Merz 1839. Herr Prof. Schönbein macht die Gesellschaft auf die merkwürdige und bisher noch nicht beobachtete Thatsache aufmerksam, dass bei der Electrolyse des Wassers an der positiven Electrode ein Geruch entwickelt wird, auffallend ähnlich demjenigen, den man beim Ausströmen gewöhnlicher Electricität aus Spitzen wahrnimmt.

This odor had, of course existed since the occurrence of lightning in the presence of an oxygen atmosphere on earth. Much later, when static electricity machines were developed, van Marum (3, 4) attributed the odor accompanying operation of the machine in air or oxygen to the electricity itself and it became known as the odor of electricity. His results were largely ignored except for the term "odor of electricity."

Schönbein had acquired a Grove cell, paid for by popular subscription in Basel, after attending a conference in Manchester during the preceding summer. This cell provided a much more powerful current than the equipment he had used previously in his studies of passivation of metals and van Marum's "odor of electricity" was very

pronounced in his poorly ventilated laboratory. The suggestion that the odor was due to a distinct chemical substance was formally proposed in 1840 in a lecture to the Bavarian Academy of Science and to a wider audience when a letter to Faraday was read before the Royal Society (5) and one to Arago (6) before the French Academy of Science. In this latter paper Schönbein proposed the name ozone (7) for the new substance. A detailed 1840 report to the British Association for the Advancement of Science which appeared in 1841 included the following points (8):

1. The peculiar smell makes its appearance as soon as the electrolysis of water begins and continues to be perceived for some time after stopping the flow of electricity.
2. The phosphorus smell (*sic*) is produced at the positive electrode only, and under no circumstances whatsoever at the negative one: when the gases resulting from electrolysis of water are collected in separate vessels, the smell is perceived only in that which contains oxygen.
3. The odorous principle can be preserved in well-closed vessels for a great length of time.
4. Formation of the odorous substance depends upon:
 - a. The nature of the positive electrode. Only well cleaned gold and platina give the odor.
 - b. The chemical constitution of the electrolytic fluid. The odor is obtained from water when mixed with sulfuric acid, phosphoric acid, nitric acid, potash and a series of oxo-salts. It is not obtained with solutions of halides, HCl, HBr, HI, HF, ferrous sulfate, nitrous acid, or stannous chloride. Dilute sulfuric acid is best.
 - c. The temperature. A strong odor develops at comparatively low temperatures, no odor when the electrolysis solution is near its boiling point.
5. Addition of powdered charcoal, iron, tin, zinc or lead filings, antimony, bismuth, arsenic, or mercury to the odorous gas results in almost instantaneous disappearance of the odor. Likewise small quantities of nitrous acid, and solutions of ferrous chloride, ferrous sulfate, and stannous chloride cause disappearance of the odor.
6. Clean gold or platinum plates exposed to the odorous principle become negatively polarized. The odor must be due to some gaseous substance disengaged (conjointly with oxygen) from the fluid due to the decomposing power of the current. But what is the nature of that substance? Is it elementary or compound? It has some resemblance to chlorine or bromine, maybe part of the family of halogenia. We can hardly help drawing from the facts the conclusion, that the odoriferous substance is a body very like chlorine or bromine. However it may be nothing but a secondary result of the electrolytic action.

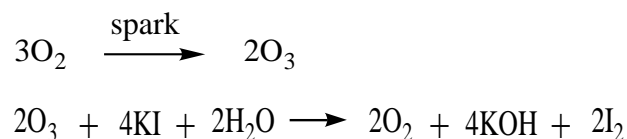
de la Rive (9), using Schönbein's term ozone, disputed the suggestion that the odor observed in electrolysis was due to a gaseous substance and suggested that it might be due to finely divided particles of oxidized electrode material (10). A lengthy reply by Schönbein (11) included the points that the odor should not persist for long periods of time if it were due to suspended particles, that it was also observed during lightning storms where no electrode was present, and that it was obtained upon arcing air using carbon electrodes where the electrode oxidation product would be odorless oxides of carbon. He agreed with de la Rive's remark that isolation of pure ozone would resolve many questions. Within a short time de la Rive (12) capitulated and accepted Schönbein's view that a distinct chemical substance was involved. Isolation of pure ozone was not achieved for many decades.

In his 1840 paper (6) Schönbein remarked that the odor of ozone is very similar to that of phosphorus when exposed to air. In 1844 (13) he added the reaction of white phosphorus with moist air to the list of ozone-forming reactions, a procedure confirmed (14) by Marignac (15) and by Rivier and Fellenberg (16). Schönbein allowed pieces of phosphorus to stand with air (or air and a small amount of added water) in a closed vessel at room temperature. When the luminescence had ceased, the gas was washed with water to remove phosphoric acid and found to have the characteristic odor of ozone. A variety of tests, particularly oxidations of metals and various dyes, showed the product to have properties identical with those of electrically produced ozone, not to mention the identity of odors. One of these reactions was the oxidation of potassium iodide to give elementary iodine. This led to the starch-iodide reaction as a test for ozone, although Schönbein continued to place strong reliance on odor as a diagnostic test for ozone. The formation of ozone was shown to parallel the luminescence of the phosphorus. Later it was shown (17) that the formation of ozone is limited to white phosphorus, another example of allotropic behavior.

A. Becquerel (18) visited Basel in 1850 and gave a detailed report of his observations to the French Academy (19). Later, an effort (20) by Fremy (21) and Becquerel to give ozone the name "electrified oxygen" was countered strenuously (22) by Schönbein, who pointed out that ozone produced by reaction of white phosphorus should then be called phosphorized oxygen and so on; the name ozone prevailed and is with us to the present day. Houzeau (23) apparently had problems with the term ozone and used the incorrect name "na-

scent oxygen" or "oxygène odorant" until about 1870 (24), long after ozone had achieved world-wide acceptance. He did confirm the earlier results on formation and reactions of ozone.

Fremy and Becquerel's 43-page paper (20) confirmed much of the work of Schönbein and of Marignac (see later). In addition, an important contribution was the demonstration that the very low concentrations of ozone formed by arcing oxygen must be due to the oxygen itself and not to impurities present. They repeatedly arced a sample of oxygen contained in a tube with electrodes at the closed end and immersed at its open end in potassium iodide solution. The volume of the gas decreased steadily as the arcing was continued until the volume was so small that the experiment had to be interrupted. Since the stoichiometry, as shown below, involves formation of two molecules of ozone from three of oxygen and the two molecules of ozone react with potassium iodide to form two molecules of oxygen, the volume of gas decreases steadily. This result was confirmed (25) by Andrews (26) and Tait (27).



Schönbein's conclusions did not remain unchallenged. N. W. Fischer (28) argued in 1845 (29) that the three methods gave three different substances: the odor from arcing air was the odor of electricity as van Marum had suggested, the odor from electrolysis was due to hydrogen peroxide, and the odor from reaction of phosphorus was simply phosphoric acid. A brief polemic between the two ensued (29, 30), Schönbein arguing that Fischer did not know how to perform the starch-iodide test properly (31). About 10 years later Andrews (32) addressed this question and showed that the products of arcing and of electrolysis were both decomposed very rapidly to oxygen by heating at 235-240° C or by boiling water.

Another dissent (33) came from A. W. Williamson (34), working in Liebig's laboratory in Giessen. He obtained the ozone odor from electrolysis of aqueous sulfuric acid solutions but failed to obtain anything similar from the reaction of moist air with phosphorus. This was later explained by others to be due to the fact that he used finely divided phosphorus so that the ozone formed was destroyed by reaction with phosphorus. Williamson's paper brought forth a testy reply from

Schönbein (35), who reiterated the identity of a long list of properties (eleven in all) of the electrolysis and phosphorus reaction products and went on to chide the young man for his lack of faith in his elders, "Does Herr W. not believe him and Marignac?"

Objections aside, ozone was quickly accepted by the chemical world of the mid-19th century. It presented a number of fascinating challenges: 1) determination of its composition, 2) its isolation as a pure substance, 3) the study of its chemistry, and 4) understanding the contrast between its behavior and that of ordinary oxygen. When the allotropic nature of ozone became established (see below), these questions became more acute. How could two such closely related substances as dioxygen and ozone be so different in their properties? In an 1847 letter to Schönbein, Berzelius (36) commented that Schönbein's discovery of ozone was one of the most important discoveries in chemistry. Likewise, Liebig, in a footnote to Schönbein's invited review in *Annalen* (37), commented in superlatives on the importance of his contribution. Schönbein continued to work on various aspects of ozone chemistry for the remainder of his life with about 200 papers on the subject, and many other chemists joined him. Reviews (*inter alia*: 11, 37, 38, 39, 40, 41, 42, 43) and books (44, 45) appeared with increasing frequency and by 1846 the topic had crossed the Atlantic (46). Ozone was off to a running start and has never slowed since. First it was a chemical curiosity of great interest, then a reagent for organic synthesis and an extremely useful tool for structure determination of natural products, and more recently a component of smog and a key ingredient of the upper atmosphere.

Analysis for Ozone

The first analytical instrument for ozone analysis was Schönbein's nose, and smell continued to be an important diagnostic for the presence of ozone, one of the most sensitive of all methods. A variety of other qualitative methods were developed (47), the most important being the starchiodide test although it was clear at an early stage that other substances could also give positive results. In an attempt to find a more specific test, Schönbein described a test paper based on manganous salts (48), usable as an invisible ink, as did Fremy and Becquerel (20). This turned brown with ozone, as did one with thallium oxide (49), which had the advantage of giving a negative test with nitrites. Unfortunately, this test was much less sensitive than starch-iodide paper.

Among other qualitative tests used were conversion of silver to its peroxide and a variety of color tests including the decoloration of colored substances such as indigo and litmus, the blue coloration of guaiacum and of pyrogallol, etc. (50).

Quantitative analysis for ozone was delayed until the determination of its molecular weight and the stoichiometry of its various reactions, discoveries that occurred at later stages of ozone research. A number of quantitative methods, useful for determining relative concentrations, were developed in spite of this limitation. Schönbein (51) used solutions containing known weights of indigo with the change to colorless serving as an end point. He concluded that a mixture from the reaction of phosphorus with moist air contained 1/1300 part ozone in air. After Bunsen developed a titrimetric method for iodine analysis, titration of the iodine liberated from potassium iodide solution became a standard method for ozone analysis. Houzeau (52) developed a variation on this procedure based on the fact that KI and ozone react to give elemental iodine and potassium hydroxide (see above). Acid-base titrimetry with tournesol as indicator was used after reaction of KI with ozone, but this method never gained wide acceptance. Here again, the stoichiometry of the KI-ozone reaction was not known. Another useful titrimetric method involved the oxidation of arsenious acid (53); this was used by Soret (54), although he later used the iodimetric method.

Preparation of Ozone

The three methods described by Schönbein, arcing air or oxygen, electrolyzing aqueous acid solutions, and exposing phosphorus to moist air, were all used by investigators in the early days of ozone research. The most convenient of these for many investigators was the phosphorus reaction. Marignac described (14) a simple apparatus in which air was passed through a long tube filled with pieces of white phosphorus. The resulting gas could be washed with water and dried before use. Erdmann (55) described (56) an even simpler arrangement in his work on the reaction of ozone with indigo. Two flasks were connected by glass tubing; one contained water to which were added pieces of white phosphorus and the second contained an aqueous suspension of indigo; additional phosphorus could be added as required.

The phosphorus reaction was the subject of considerable investigation. Schönbein showed (17) that only white phosphorus produced ozone and investigated various aspects of the reaction (57, 58, 59). Marchand (60)

also studied the reaction in detail (61) and concluded that many of Schönbein's observations were correct but limited to the conditions under which he performed his experiments. For example, Marchand obtained ozone from phosphorus and dry oxygen without the presence of water, another proof of the allotropic nature of ozone. Schönbein also reported a number of oxidations using the slow reaction of phosphorus in the presence of vapors of various compounds (see below). At best the phosphorus reaction produced ozone concentrations of much less than 1% and its use for preparation of ozone was gradually abandoned.

The electrochemical method could be improved by use of low temperatures (53). Much later it was shown that electrolysis provided much higher concentrations of ozone in oxygen than any other method by using specially constructed equipment and carefully controlled conditions.

The breakthrough in ozone preparation was achieved by Siemens (62), who exploited Rühmkorff's development of a high voltage transformer (63). In a long paper on electrostatic induction, Siemens described (64) in detail a silent discharge apparatus for preparing ozone from air or oxygen. It became routinely possible to achieve ozone concentrations in oxygen on the order of 5%, and commercial equipment for generating ozone utilizing Siemens' discovery eventually became available. A modification of this apparatus was described (65) by Babo (66), who also studied the effect of various experimental parameters on the yield of ozone.

A number of dubious methods for forming ozone by chemical reaction also appeared. Böttger (67) reported (68) that the reaction of sulfuric acid with permanganates formed ozone, and Weltzien (69) claimed (70) a similar result for reaction of dichromate with sulfuric acid. Both of these results were later (71) shown to be due to impurities in the oxidizing agents, purified potassium permanganate or potassium dichromate giving no oxidizing gas. Leeds (72) suggested that chloride impurities were responsible in both cases. Schönbein reported (73) that ozone was formed when barium peroxide was added in small amounts to a permanganate-sulfuric acid mixture. The reaction of barium peroxide with acids to produce hydrogen peroxide had been reported in 1818 by Thenard (74), so that Schönbein's system can be assumed to have contained this peroxide. He and Houzeau (24) claimed at various times that the action of sulfuric acid on barium peroxide (or other metal peroxides) produced ozone (via hy-

drogen peroxide?) but later confirmation of such chemistry is lacking. In fact, Schönbein used Thenard's method to prepare hydrogen peroxide for comparison with ozone.

Formation of ozone by passage of air over hot platinum wire was claimed by van der Willigen (75) and by Le Roux (76, 77), based on odor and starch-iodide tests; but St. Edme (78) attributed these results to nitrogen oxides.

Reactions of Ozone

1. Inorganic Reactions

In his very first papers on ozone, Schönbein (5, 6, 8) described its reactions with metals to give peroxides. The product of reaction with silver was shown to decompose thermally to give an 87% yield of silver metal and an odorless gas, which was oxygen. It was given the formula AgO_2 (Schönbein used 8 for the atomic weight of oxygen). Other metals that gave peroxides included lead, tin, iron, zinc, manganese, antimony, and bismuth; also (79) cobalt and nickel were transformed to oxides. Arsenic was converted to arsenic acid, phosphorus to phosphoric acid, nitrous acid to nitric acid, nitrites to nitrates, sulfurous acid to sulfuric, sulfides to sulfates, selenides to selenates, iodine to iodate, and so on (80). Schönbein sent a letter to Faraday using an invisible ink based on manganous sulfate that he had developed (81).

The high reactivity of ozone meant that it could only be used with a few materials such as glass, gold, and platinum. This made exact experiments very difficult.

Schönbein attempted to determine whether the oxidations proceeded in a stepwise manner via the lower oxides by exposing silver to a limited amount of ozone (82). In all cases he obtained only the peroxide, so that no firm conclusion could be drawn. He also performed competition reactions by exposing a number of metals simultaneously to an ozone atmosphere. Silver reacted most rapidly; zinc required a day for appreciable reaction. He was not able to evaluate the importance of surface condition in these reactions and the stoichiometry was not determined. In particular, the fact that reaction involved the formation of oxygen in addition to the metal peroxide was not appreciated and caused considerable difficulty in studies of the composition of ozone (see later).

Schönbein also reported in his first papers (5, 6, 8) that ozone was not formed in the presence of hydrogen halides and attributed this to the destruction of ozone by the halides. van den Broek (83) studied the reaction of ozone with hydrochloric acid in the gas phase at water aspirator pressure in the presence of metallic gold and concluded that chlorine gas (plus water) was formed as evidenced by the formation of gold chloride. The reaction of ozone with iodide to produce iodine has been noted above.

2. Organic Reactions

Progress in the investigation of reactions of ozone with organic compounds was much slower. Early work was of a purely qualitative nature. In spite of an avowed fear (84) of the complexity of organic chemistry, Schönbein reported (85) in 1845 that the ozone odor disappeared in the presence of straw, humus, humus-containing earth, sawdust, flour, potato starch, egg white, etc. One year later he added (79) wood alcohol, guaiacum, and ethylene gas to the list and later the reactions of mushrooms (84) and cyanine dye (86), and in 1868 additional natural materials (87). He noted (88) the important fact that organic substances were not converted to the highest oxidation state of carbon (CO_2) but instead to aldehydes, and carboxylic acids. In this paper he also commented that the product(s) of reaction of ethylene are similar to those obtained upon slow oxidation of diethyl ether in the presence of phosphorus (88, 89) without going into detail. These products were identified only much later (90) as formic and acetic acids and formaldehyde. A noteworthy sidelight of this work is the fact that he also observed a peroxidic material from the ethylene reaction.

Gorup-Besanez (91) explored a variety of organic reactions (92) and reported that uric acid is converted rapidly into allantoin, urea, and CO_2 ; amyl alcohol to valeraldehyde and valeric acid; tannic acid to oxalic acid and CO_2 ; potassium cyanide to potassium cyanate; albumin and casein undergo complex reactions, and tyrosine was rapidly converted to a red-brown product. He reported that urea, creatin, alloxan, allantoin, leucine, inositol, starch, fibrin, a number of acids (hippuric, acetic, butyric, palmitic, lactic, and tartaric), and glycerol failed to react. In all the above, ozone was generated by the phosphorus reaction, the gas being collected in glass vessels and washed with water before addition of the substrate.

The reaction with rubber was first reported by Soret in 1853 (53) and noted by a number of other workers.

Soret reported that when ozone was generated electrolytically at low temperature, the rubber connections of his apparatus were rapidly attacked but that this did not happen at room temperature. He correctly attributed the difference to the higher concentration of ozone formed at lower temperature. Much later this reaction became the basis of a simple method for determining when a solution bubbled with ozone had become saturated.

The only reaction of an organic compound with ozone which is clearly documented is the reaction with indigo. Schönbein described (93) the decoloration of indigo by ozone (from phosphorus) in 1851 and stated baldly that it is known that indigo is converted to isatin. However, Erdmann (56) reported full details of an experiment in which an aqueous suspension of indigo was treated with ozone from phosphorus and the product isatin was isolated and characterized by melting point.

Composition, Molecular Formula

What was this mysterious substance? The task of determining the composition of ozone was a formidable one in the 1840s, particularly because it was obtained as dilute solutions (<1% in the early work) in air or oxygen. Even today, the usual ozone generator produces a mixture of oxygen and ozone with concentrations on the order of 5%. This precluded the use of conventional methods, such as gas density, for determination of its relative molecular weight. A nontrivial technical problem was the extremely high reactivity of ozone towards rubber, most metals, etc., mentioned earlier. Much effort by Schönbein and others went into unsuccessful methods for producing pure ozone. As early as 1845 Schönbein (94) recognized that low-temperature methods might be successful. Later Andrews and Tait (25) tried to condense ozone at dry ice-ether temperature (-76°C) without success and many years passed before the necessary low-temperature techniques became available for obtaining pure ozone (and the hazards were realized). de la Rive (10) had proposed that the odor observed in electrolysis was due to particles of oxidized material disengaged from the positive electrode during electrolysis but abandoned this proposal (95) after Schönbein's work appeared. Schönbein considered at an early stage (96) that ozone was oxygen in an unspecified "nascent state."

The first question to be answered was whether ozone is composed of a single element or is a compound substance. Because the early methods for preparation

of ozone involved air and moisture, Schönbein considered combinations of hydrogen, oxygen, and nitrogen. His first proposal was that nitrogen was not an element but was formed by decomposition of ozone. This met with many objections and was quickly discarded when Marignac (14) showed that electrolysis of water under air-free conditions readily produced ozone. This result was confirmed by a number of workers including Houzeau, and Fremy and Becquerel. Schönbein's approach was mainly based on the preparation of substances that were possible candidates and comparison of their properties with those of ozone. He published papers reporting that ozone was not nitrous acid (97) and comparing ozone with chlorine (98) and was led to the conclusion that ozone was a compound of hydrogen and oxygen. Comparison with hydrogen peroxide, prepared by the Thenard procedure from reaction of barium peroxide with acid (74), showed that the two substances were different in spite of a number of similarities; his conclusion was that the ratio of oxygen to hydrogen in ozone differed from that in hydrogen peroxide. It was even suggested by Gentele (99) that ozone was CO₃.

The first breakthrough in the elucidation of the composition of ozone came from Geneva. After demonstrating that ozone was formed by electrolysis under nitrogen-free condition as mentioned above, Marignac (14), acting on a suggestion of his colleague de la Rive, reported in 1845 that ozone was formed by arcing pure, dry oxygen. Curiously, he did not include this result in a second paper published at about the same time in *Compt. Rend.*; but it was published by de la Rive (100) as a postscript to a paper on another subject. These results were reproduced by Marchand (101), who demonstrated them to Berzelius and Erdmann. Further confirmation was provided by Fremy and Becquerel (20), by Houzeau (24b), and by Babo (102). Ozone was an allotrope of oxygen! The repeated experimental confirmations indicate how important this allotropy was in the eyes of chemists of the time. How was it possible that two allotropes of the same element could differ as markedly in their properties as oxygen and ozone? In the 1847 letter to Schönbein referred to earlier, Berzelius wrote (36) that the question of the allotropy of ozone was settled. Pleased as he may have been by the compliment included in that letter, Schönbein had already objected to the conclusion (103), arguing that allotropy was reserved for solid substances and that Marignac's oxygen was not really dry. Since ozone was formed in very low yields, mere traces of moisture present would suffice for its formation. In the same paper he suggested

another experiment to settle the question, namely thermal decomposition of carefully dried ozone and measurement of the amount of water formed. Meanwhile he continued to maintain that ozone was a compound of hydrogen and oxygen until 1850 (see below).

The results of such thermal experiments were confusing. Williamson (104) and Baumert (105) reported independently that there was a significant gain in weight when thermally decomposed ozone was passed over a weighed amount of drying agent. Other workers, including Schönbein (42) himself in 1850, observed the opposite result. In one of the few quantitative experiments he performed, Schönbein prepared 300 liters of ozone by the phosphorus method, dried it by passing through sulfuric acid, and carried out its decomposition at 300°C. The gas was then passed over pumice coated with concentrated sulfuric acid. There was no gain in weight. This result was confirmed by Andrews (32), who obtained variable results using electrochemically generated ozone until he separated the electrodes in his apparatus by a diaphragm. Reproducible results were then obtained. The weight gain of the drying agent was less than 5% of the theoretical amount of water had ozone been a compound of hydrogen and oxygen. Andrews attributed the variability of results to the presence of traces of carbon dioxide in the ozone-oxygen mixture used, an argument which was disputed by Baumert (106). Soret (107) made the reasonable suggestion that the electrochemically generated ozone of Baumert contained traces of hydrogen. Using, like Andrews, a porous diaphragm to separate the electrodes in the electrolysis apparatus to ensure that no hydrogen was present in the oxygen-ozone mixture, he reproduced the Schönbein and Andrews results. The allotrope conception prevailed.

Speculation on the nature of ozone as an allotrope of oxygen was not lacking. As part of his contemplation of periodic relationships, Hunt (108) suggested (109) in 1848 that, by analogy with SO_2 , there should exist a compound of oxygen having the formula O_3 and that this was probably the formula for Schönbein's ozone. Odling (110) made a similar proposal in 1861; based on Andrews and Tait's evidence that ozone was denser than oxygen, he suggested O_3 as the simplest candidate. In 1858 Clausius (111), having learned that the correct formula for oxygen is O_2 , proposed (112) that ozone was atomic oxygen, O_1 . In order to explain the known fact that ozone was stable for long periods, he suggested that the oxygen atoms were polarized and repelled one another. Schönbein's response (113) to such speculation was to dissociate himself completely from

“the dogmas of present-day atomists.” This was strange behavior from the man who did not hesitate to propose a new substance on the basis of odor alone! Nonetheless, he read a paper (114) before the Naturforschung Gesellschaft in Basel on the “atomic weight” of ozone; but this dealt with the composition of the peroxides obtained with various metals and with a correction indicating that Osann's (115) supposed PbO_2 was, in fact, Pb_3O_4 .

The approach that eventually led to the correct molecular formula for ozone was due to Andrews and Tait (25), who reported their results in a detailed paper including literature citations, drawings of the apparatus used, and a detailed experimental section (unlike many of the papers appearing in those years). The thermal instability of ozone had been noted by Schönbein and others in the earliest stages of its investigation. Andrews and Tait performed precise measurements of the volumetric relationships involved in thermal decomposition. They used a pair of identical vessels, each filled with pure oxygen and fitted with sulfuric acid manometers. One vessel served as a reference while the other contained electrodes that allowed generation of ozone either by arcing or by the silent discharge method. They reported that formation of ozone by either method resulted in a decrease in volume. Heating at 237°C resulted in reversal of the process with regeneration of the entire original volume. Ozone was denser than oxygen! These results were reproduced by Babo and Claus (116) and by Soret (117). It should be noted that Meidinger (118) had observed earlier that the volume of oxygen formed in electrolysis was appreciably less than one-half the volume of hydrogen obtained at the other electrode. He concluded that ozone must be denser than oxygen but did not go further. Andrews and Tait also found that the volume decrease was larger with silent discharge than with arcing and that arcing the gas mixture from silent discharge resulted in an increase in the volume of the gas to the value obtained upon arcing oxygen directly.

Andrews and Tait realized that if a procedure could be found for completely converting the ozone formed into nonvolatile product(s), the ratio of the volume increase observed on thermal decomposition of an ozone-oxygen mixture to the volume decrease when ozone from a second sample of the same mixture reacted completely would provide the ratio of the density of ozone to that of oxygen. The problem was to find a suitable reaction for complete consumption of ozone. They attempted to use mercury or silver; the volume change was negli-

gible, leading to the conclusion that ozone consisted of many atoms of oxygen. The possibility that the reaction of mercury with ozone resulted in formation of an oxide of mercury together with a molecule of oxygen was rejected as being unlikely. This, the correct explanation, was suggested by Babo and Claus and by Soret. Reactions of ozone with metals afford one molecule of oxygen for each molecule of ozone consumed.

The solution to the problem was found by Soret, who first confirmed (107) Andrews and Tait's results on volumetric relationships. He then found (119) a method for the complete consumption of ozone in the reaction of oils of turpentine (térébenthine) and of cin-

sion that ozone is indeed O_3 is clear from the experiment. Later workers confirmed Soret's conclusion.

Additional confirmation for the formula O_3 was provided by Soret's studies, reported two years later (120) on the rate of diffusion of ozone through a small aperture (the use of porous materials for diffusion led to decomposition of ozone) using chlorine as a reference. The calculated value for the square root of the inverse ratio of diffusion rates is 0.82 for O_3 and 0.95 for O_4 , Soret found 0.84. Nearly 30 years had passed since Schönbein's initial suggestion that ozone is a distinct substance. His death in 1868 precluded comment by him on this result.

Table. SORET'S ANALYSIS OF OZONE CONSUMPTION

Corps absorbant.	Diminution de Volume par l'essence.	DILATATION PAR LA CHALEUR		
		Calculée	Observée	Différence
	cc	cc	cc	cc
Essence de térébenthine	6,8	3,40	3,77	+0,37
Essence de térébenthine	5,7	2,85	3,20	+0,35,
Essence de cannelle	5,8	2,90	3,14	+0,24
Essence de térébenthine	5,6	2,80	3,32	+0,32
Essence de térébenthine	6,7	3,35	3,30	-0,05
Essence de cannelle	6,9	3,45	3,45	0,00
Essence de cannelle	5,7	2,85	2,72	-0,13

namon (cannelle). Both of these naturally occurring materials contain considerable amounts of unsaturated compounds and react rapidly and quantitatively with ozone. An ozone-oxygen mixture was divided into two samples. One sample was heated and the volume increase was measured; the second was allowed to stand over one of the oils and the volume decrease measured. The results are summarized in Soret's table above. The calculated dilatation (volume increase) is the value of 2 calculated for O_3 from the ratio (volume decrease by reaction/volume increase by heating).

The value of the ratio calculated for O_4 is unity (instead of 2 for O_3) and becomes increasingly smaller as the number of oxygen atoms increases, so that the conclu-

Atmospheric Ozone

In his 1840 paper (6) Schönbein considered the possibility that ozone was found in the atmosphere and proposed testing for it by exposing platinum strips to air (121). This awareness derived in part from his experience (Ref. 11, p 251; Ref. 22, p 346) when lightning struck a church near his home in Basel, and the odor persisted long after the electrical storm was over; he had a similar experience earlier in life. He identified that odor with the one he later detected upon electrolysis of acidified water. The further knowledge that ozone had a variety of very unpleasant physiological effects (see

below) resulted in a concern with atmospheric ozone (122) which persisted to the very end of his work (49). Having shown that starch-iodide paper exposed to ordinary air developed the characteristic color due to iodine formed, he concluded that ozone was continuously present in the atmosphere. He also obtained a positive test for ozone in ground water after an electrical storm (123). The very low concentrations of ozone in the atmosphere were attributed to destruction of ozone because of its high reactivity. Schönbein developed a commercially available kit (124), based on starch-iodide paper, with a chromatic scale from 0-10 for ozone concentration. The dry paper strips, exposed to the atmosphere for a specified period of time, developed a brown color (iodine) and were then moistened to obtain a colored strip for comparison with the chromatic scale supplied. Berigny (125), who was largely responsible for introducing ozonometric measurements in France, later developed a similar system (with a chromatic scale of 0-21) as did Osann (126).

The only evidence, other than qualitative color tests and odor, that ozone was indeed present in ordinary air was provided by Andrews. Having established the conditions for thermal decomposition of ozone (25), he showed (127) that a sample of air which colored starch-iodide paper gave no such effect after passage through a tube heated to 237° C. This eliminated a variety of thermally stable possibilities such as halogens. There was wide acceptance of ozone as a constituent of the atmosphere. The availability of a simple and convenient procedure, using commercially available test kits, prompted many workers to undertake regular measurement of atmospheric ozone concentrations. A sampling from *Compt. Rend.* alone for the period 1854 to 1865 reveals such reports by Karlinsky (128), Berigny (129), Sylvestri (130), Pietra Santa (131), and Poey (132, 133). Long-term measurements were made by Boeckel (134). In the course of time, considerable criticism was leveled at the simple starch-iodide method (135). In addition to the known lack of selectivity of the starch-iodide test, results were found to depend on the quality of the paper and the reagents used, on exposure to light, on the relative humidity, etc. Defenders were not lacking (136). A partial solution to these problems was the design of special "ozone boxes" in which the test paper was placed (137). One of Schönbein's last papers (49) was concerned with developing a more selective test for ozone. Huizinga (47) reported use of Schönbein's thallium oxide paper (49), but this does not appear to have achieved much acceptance by other workers.

The period discussed in this article saw the beginnings of atmospheric ozone studies, with the pace accelerating rapidly during the latter part of the 19th century. Marengo *et al.* (138) have recently provided an overview of the early methods for atmospheric ozone analysis and a detailed evaluation of results obtained at a specific location. It would appear that systematic measurements at a given location over a long period of time do have merit and can be compared with current atmospheric ozone concentrations.

Numerous attempts were made to correlate atmospheric ozone concentration with public health (139, 140). Thus Schönbein (139), observing higher concentrations of ozone at a time of snowfall, attributed the increased frequency of catarrhal and other conditions in winter to the increased concentration of ozone.

Physiological Effects

A report by Schönbein (141) on the physiological effects of ozone appeared in 1851 and was summarized (37) in the 1854 review of ozone commissioned by Liebig. Ozone affected breathing and caused chest pains and irritation of the mucous membranes; these were reiterated (73) in 1863. Small animals, such as mice, died quickly in an ozone atmosphere. A fully-grown rabbit died after breathing ozonized air for one hour with symptoms similar to chlorine poisoning. As is well known, this subject acquired greater and greater interest with the passage of time.

False Trails

1. Antozone

In an 1858 letter to Faraday, Schönbein (142), an admitted lover of speculation, proposed the existence of another form of oxygen, which he called antozone. He commented that "I am far from believing that the above is correct but it is necessary to have a hypothesis on which to base further experiment." The two species, ozone and antozone, purportedly reacted together to give oxygen. On the assumption that both were formed under ozone-producing conditions, the low yield of ozone obtained was explained by its destruction by antozone. He gave ozone the symbol Q, implying that it is a negatively charged species, and its complement, antozone, the symbol ≈. Thus lead dioxide, a compound containing ozone according to Schönbein, was written PbOQ. Barium peroxide, on the other hand, supposedly contained antozone and was written BaO≈. The distinction

was based on the fact that, under similar reaction conditions, barium peroxide liberated chlorine from dilute hydrochloric acid while lead dioxide did not. This distinction was later shown to be a matter of proper choice of reaction conditions and to have no relevance to any fundamental difference between the two peroxides.

Antozone appeared frequently in subsequent papers of Schönbein (143) and consumed a considerable amount of his research effort. It was reportedly obtained by heating a certain fluorspar (144). This particular odorous material was eventually considered to have no relation to ozone. Antozone engaged the attention of a number of other workers, particularly Meissner (145), whose work appeared (146) as a 370-page book. He also coined the alternative term "atmizone." The importance attached to antozone is emphasized by sympathetic reviews that appeared in the United States (147).

Antozone was identified by its supporters as the white cloud formed when ozone was generated either by electrolysis or by the phosphorus reaction. Since this cloud passed through water without change, it was concluded that it could not be hydrogen peroxide. As work continued, more and more convoluted explanations had to be suggested to account for the observed results. Babo (148) and Weltzien (149) both suggested that antozone was simply hydrogen peroxide. The whole matter was laid to rest in 1870 (150) by Engler (151) and Nasse (152), who showed that the material in the cloud could be condensed in cool traps and that it consisted of aqueous hydrogen peroxide. They also generated a cloud having the same properties by application of a vacuum to a solution of hydrogen peroxide.

Leeds, summarizing the purported chemistry of antozone in 1879 (153) wrote, "By far the most important fact in the long and perplexing history of antozone, is the recent discovery that there is no antozone." The history of antozone has many of the attributes of pathological science as defined by Langmuir (154).

2. Photooxygenation Reactions.

Another confusion was introduced by the oxidizing properties resulting from exposure of various substrates to oxygen and sunlight. For example, Schönbein (155) exposed indigo to air and sunlight and inferred from the disappearance of color that ozone had been formed and reacted with indigo. In retrospect, these reactions undoubtedly are early examples of the reaction of alkenes with singlet oxygen, indigo acting as photosensitizer in this case. A number of reactions of

oil of turpentine with oxygen (156) also do not involve ozone.

3. Ozon-Wasserstoff.

Amid confusion as to whether ozone was a nascent state of oxygen, Osann (157) reported in 1855 (158) that electrochemically generated hydrogen could effect reductions of substances present in the neighborhood of the electrode which chemically generated hydrogen gas did not effect. Much of his work involved the reduction of silver oxide to silver. He proposed that there was a hydrogen analog of ozone which formed at the negative electrode during electrolysis and gave this the name ozone-hydrogen (Ozon-Wasserstoff) as distinguished from ozone-oxygen. A number of additional papers (159) on this subject followed. His results were disputed by other workers (160). An interesting summary of this and related work on the nascent state has been given by Jensen (161).

Conclusion

By the time of Schönbein's death in 1868 ozone was an accepted fact of chemistry, while his proposal of the existence of antozone had fallen by the wayside. The variety of early methods for preparation of ozone was largely superseded by Siemens' invention of the silent discharge apparatus. The isolation of pure ozone and determination of its properties remained for the future. At the end of this period, ozone had been identified as O_3 , an allotrope of ordinary oxygen, and provided a fascinating puzzle for chemical theory, which required the passage of many decades before an understanding of ozone's structure and reactivity was achieved.

Many oxidations of inorganic compounds to their highest oxidation states had been described, although the stoichiometry of these reactions was not known. Oxidations of a few organic compounds had also been reported without very much information on the products of reaction; Schönbein emphasized, however, that these oxidations did not proceed all the way to carbon dioxide and water. Systematic knowledge of ozone's behavior with organic compounds materialized at the beginning of the 20th century.

Interest in atmospheric ozone dated from the very beginning of Schönbein's discovery of ozone. Development of semiquantitative methods for analysis of atmospheric ozone prompted many workers to undertake regular studies of concentrations of atmospheric ozone

at ground level. Quantitative methods for determination of ozone were also developed.

In view of the state of chemistry at the end of Schönbein's lifetime, it is fair to say that a remarkable amount of progress was made between 1839 and 1868 in spite of a great deal of waste motion.

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REFERENCES AND NOTES

The journal cited here as *Annalen der Physik und Chemie*, now called *Annalen der Physik*, was commonly referred to during the period covered by this article by the name of its editor as *Poggendorff's Annalen*. Likewise, the *Journal für Praktische Chemie* was referred to as *Erdmann Journal*.

It was accepted practice in the mid-nineteenth century, in the absence of abstract journals and facile communication, to publish the same work, often in word-for-word translation, in journals of various countries. Some journals also included detailed summaries of significant work appearing in other journals. Where more than one report of specific work appears, we have chosen one for detailed citation and added the other references at the end of the citation.

- Christian Friedrich Schönbein, 1799-1868. Professor at the University of Basel from 1828. For biographical information see R. E. Oesper, *J. Chem. Educ.*, **1929**, 6, 432-40. This author has also given an overview of Schönbein's research work, *J. Chem. Educ.*, **1929**, 6, 677-85. A much more extensive early source is the book by E. Hagenbach, *Christian Friedrich Schönbein, Programm für die Rectoratsfeier der Universität*, Basel, 1868. A biography of Schönbein has appeared recently in honor of the 200th anniversary of his birth: P. Nolte, *Christian Friedrich Schönbein. Ein Leben für die Chemie 1799-1868*, Arbeitskreis Stadtgeschichte der Volkshochschule Metzingen-Ermstal e.V., 1999. ISBN 3-9802924-6-0. The Basler Magazine of the Basler Zeitung dated Saturday, October 16, 1999, also features a cover photograph of Schönbein and a long article with photographs on the man and his work.
- C. F. Schönbein, *Ber. Verh. Nat. Ges. Basel*, **1838-40**, 4, 58, from a lecture on March 13, 1839. Author's translation: Prof. S. calls the Society's attention to the noteworthy new observation that a smell develops at the positive electrode during electrolysis of water which is strikingly similar to that obtained by the flow of electricity across electrodes.
- Martin van Marum, 1750-1837. Dr. of Philosophy and Medicine, at first a practicing physician in Haarlem. In 1776 he began to give natural history lectures and in 1777 became Director of the Naturalienkabinets in Haarlem and, in addition, in 1784 of the physical and natural history Museums of the Teyler'schen Museum. He published a multi-volume work (French and German translations were published in 1775) on the researches he performed using the museum's static electricity generator.
- After sparking air and noting the odor formed, van Marum studied the behavior of oxygen as pure as could be obtained at that time. He observed diminution of the volume of oxygen upon sparking; the mercury was strongly calcined at its surface, and the glass was covered with a film of mercury, so that one could not see through the tube. "It is evident that the oxygen has united with mercury." Van Marum also investigated sparking of ammonia, nitrous acid, gaz olefiant, etc. In a long paper on his investigations of electrochemical phenomena, W. Cruickshank noted the odor accompanying electrolysis of aqueous acidic solutions (*Ann. Phys. [Gilberts Annalen]*, **1801**, 7, 107).
- C. F. Schönbein, "On the Odour Accompanying Electricity and on the Probability of its Dependence on the Presence of a New Substance," *Philos. Mag.* (III), **1840**, 17, 293-4. Also: *München Abhandl.*, **1837-43**, 587; also: "Beobachtungen über den bei der Elektrolyse des Wassers und dem Ausströmen der gewöhnlichen Elektrizität aus Spitzen sich entwickelnden Geruch," *Ann. Phys. Chem.*, **1840**, 50, 616-635.
- C. F. Schönbein, "Recherches sur la nature de l'odeur, qui se manifeste dans certaines actions chimiques," *C. R. Hebd. Seances Acad. Sci., Ser. C*, **1840**, 10, 706-10.
- From the Greek *ozein*, to smell, as suggested by his colleague, W. Vischer, Professor of Greek in Basel. Mohr has collected a number of early Greek references to the odor of ozone: "Aelteste Nachricht über Ozon und seine Benennung," *Ann. Phys. Chem.*, **1854**, 91, 625-627.
- C. F. Schönbein, "An Account of Researches in Electrochemistry," Report of British Association for the Advancement of Science for 1840, pub. 1841, Taylor, London, 209-15.
- Auguste Arthur de la Rive, 1801-1873. Professor of physics, Academy of Geneva. Member of the Paris

- Academy and the Royal Society, London. Author of "Traites l'Électricité theorique et appliquée," 3 vol., Paris, 1854-8.
10. A. de la Rive "Neue Untersuchungen über die Eigenschaften der discontinuirlichen elektrischen Ströme von abwechselnd entgegengesetzter Richtung," Parts 1 and 2. *Ann. Phys. Chem.*, **1840**, 54, 254, 378-410.
 11. C. F. Schönbein, "Ueber die Natur des eigenthümlichen Geruches, welcher sich sowohl am positiven Pole eine Säule während der Wasserelektrolyse, wie auch beim Ausströmen der gewöhnlichen Elektrizität aus Spitzen entwickelt," *Ann. Phys. Chem.*, **1843**, 59, 240-55. Also: *Arch. élec.(Supp. a la Bibliothèque Universelle de Genève)*, **1843**, 3, 295; *München Abhandl.*, **1837-43**, 587.
 12. A. de la Rive, "Quelques Observations sur le Memoire de M. Schoenbein, Relatif a la Production de l'Ozone par voie Chimique," *Arch. élec.(Supp. a la Bibliothèque Universelle de Genève)*, **1844**, 4, 454-6.
 13. C. F. Schönbein, "On the Production of Ozone by Chemical Means," *Philos. Mag.(III)* **1844**, 24, 466, 467. Also: *Proc. R. Soc., London*, **1844**, 5, 507, 508; *Ber. Verh. Nat. Ges. Basel*, **1844**, 6, 16; *Arch. élec.(Supp. a la Bibliothèque Universelle de Genève)*, **1844**, 4, 333. *Giornalè dell' I. R. Istituto Lombardo di Scienze*, **1844**, 27, 397 and 28, 201; *J. Prakt. Chem.*, **1845**, 36, 246-9; *J. Prakt. Chem.*, **1845**, 36, 379-81.
 14. J. C. Marignac, "Sur la nature et la production de l'ozone," *Arch. electr. (Supp. a la Bibliothèque Universelle de Genève)*, **1845**, 5, 5-11. Cf. J. C. Marignac, "Sur la production et la nature de l'ozone," *C. R. Hebd. Seances Acad. Sci., Ser. C*, **1845**, 20, 808. Also A. Houzeau, "Ueber den activen Sauerstoff," *J. Prakt. Chem.*, **1857**, 70, 340-4.
 15. Jean Charles Galissard de Marignac, 1817-1894. Professor of Chemistry at the Academy of Geneva from 1842. Discovered gadolinium and purified terbium. From 1866 correspondent of the Paris Academy, 1881 member of the Royal Society, London. Received the Davy Medal of the Royal Society in 1886.
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 17. C. F. Schönbein, "Ueber einen wesentlichen Unterschied zwischen gewöhnlichen und amorphen Phosphor," *J. Prakt. Chem.*, **1853**, 60, 154-8. Also *Ber. Verh. Nat. Basel*, **1854**, 1, 9.
 18. Antoine Cesar Becquerel, 1788-1878. Professor of Physics at Musee d'histoire naturelle, from 1829 member of Paris Academy. Received the Copley Medal of the Royal Society.
 19. A. Becquerel, "Communication de M. Becquerel relative aux expériences de M. Schoenbein sur l'ozone," *C. R. Hebd. Seances Acad. Sci., Ser. C*, **1850**, 30, 13-16.
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 21. Edmond Fremy, 1814-1894. In 1846 Professor at the École Polytechnique in Paris and, from 1850, at the Musée d'histoire naturelle; director of the museum in 1879. Member of the Paris Academy and numerous other societies.
 22. C. F. Schönbein, "Ueber die Natur und den Namen des Ozons," *J. Prakt. Chem.*, **1852**, 56, 343-53. Also: *Philos. Mag.* IV, **1852**, 4, 542-545; *Ber. Verh. Nat. Ges. Basel*, **1852**, 10, 82;
 23. Jean August Houzeau. Professor of Chemistry at École supérieure des science et des lettres à Rouen and at the École d'agriculture de Dept. de la Seine inferieure (both from 1858).
 24. Inter alia: a. A. Houzeau, "Researches on Oxygen in the Nascent State," *Quart. J. Chem. Soc. London*, **1854-6**, 7, 237-9; b. "Recherches sur l'oxygène à l'état naissant," *C. R. Hebd. Seances Acad. Sci., Ser. C*, **1856**, 43, 34-8; c. "Recherches sur l'oxygène à l'état naissant (oxygène odorant, ozone)," *Ann. Chim. Phys.* [3], **1861**, 62, 129-59 and subsequent papers until about 1870.
 25. T. Andrews and P.-G. Tait, "On the Volumetric Relations of Ozone, and the Action of the Electrical Discharge on Oxygen and other Gases," *Philos. Trans. R. Soc. London*, **1860**, 150, 113-131; also *Quart. J. Chem. Soc. London*, **1860**, 13, 344-367; *Ann. chim. phys.* [3], **1861**, 62, 101. For a preliminary report: "Note on the Density of Ozone," *Proc. R. Soc. London*, **1857**, 8, 498; *Chem. Gazz.*, **1857**, 319; *Ann.*, **1857**, 104, 128.
 26. Thomas Andrews, 1813-1885. Professor of Chemistry, Queen's College Belfast, Member of the Royal Society, London; discoverer of critical temperature of gases.
 27. Peter Guthrie Tait, 1831-1901. Professor of Mathematics at Queens University, Belfast. From 1860 Professor of Physics at the University of Edinburgh and secretary of the Edinburgh Royal Society.
 28. Nicolaus Wolfgang Fischer, M.D., 1782-1850. From 1814 Professor at the University of Breslau.
 29. N. W. Fischer, "Bemerkungen über das sogenannte Ozon," *Ann. Phys. Chem.*, **1845**, 66, 163-167; "Bemerkungen zu Hr. Schönbein's Beleuchtung meiner Meinung, betreffend das Ozon," *Ann. Phys. Chem.*, **1845**, 66, 168-173; "Ueber das Vermögen mehrerer gas- und dunstförmiger Körper, Metalle zu polarisieren und auf Jodkalium, Cyaneisenkalium etc. zersetzend einzuwirken," *J. Prakt. Chem.*, **1845**, 34, 186-191; "Ueber die Bemerkungen des Hr. Prof. Schönbein zu meiner Notiz, das Verhalten des Jodkaliums zu vershiedenen Gasarten etc. betreffend," *J. Prakt. Chem.*, **1845**, 35, 180-1; "Ueber das Leuchten des Phosphors," *J. Prakt. Chem.*, **1845**, 35, 342-356; "Bemerkungen zu dem Aufsatz des Herrn Schoenbein "Über die Erzeugung des Ozons durch Phosphor," *Ann. Phys. Chem.*, **1849**, 76, 158-160.
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- 65, 190-196; also *J. Prakt. Chem.*, **1845**, 34, 492; "Erwiderung auf Hr. Fischer's Replik," *Ann. Phys. Chem.*, **1845**, 66, 593-4; "Einige Notizen über das Jodkalium," *J. Prakt. Chem.*, **1845**, 34, 42-45; "Weitere Notizen über das Jodkalium," *J. Prakt. Chem.*, **1845**, 35, 181.
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 33. A. W. Williamson, "Einige Versuche über Ozon," *Ann. Chem. Pharm.*, **1845**, 54, 127-33.
 34. Alexander W. Williamson, 1824-1904. Studied in Heidelberg and Giessen (Dr. under Liebig, 1845). Professor of pure and practical chemistry at University College, London. Member Royal Society, London, corresponding member of the Academies of Paris and Berlin.
 35. C. F. Schönbein, "Einige Bemerkungen über die Versuche des Hr. Williamson, betreffend das Ozon," *Ann. Phys. Chem.*, **1845**, 66, 291-4; also *Philos. Mag.*, III, **1845**, 27, 197.
 36. J. J. Berzelius, Extract from a letter to Schönbein dated March 12, 1847. "Ihre Entdeckung von Ozon ist aus diesen Gesichtspunkte eine der schönsten die je gemacht worden sind."
 37. C. F. Schönbein, "Ueber verschiedene Zustände des Sauerstoffes," *Ann. Chem. Pharm.*, **1854**, 89, 257-300, footnote p 258.
 38. Anon., "Ozone," *J. Prakt. Chem.*, **1846**, 38, 59-61.
 39. J. J. Berzelius, "Ueber das Ozon," *J. Prakt. Chem.*, **1847**, 40, 242-246; taken from Vol. 26 (1847) of Berzelius' *Jahresberichte*.
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 54. Jacques Louis Soret, 1827-1890, Editor-in-chief of *Archives des sciences physiques et naturelles*. From 1876 professeur medicin physique, University of Geneva; a pioneer in spectroscopy.
 55. Otto Linné Erdmann, 1804-1869. Professor from 1830 at the University of Leipzig. Editor of the *Journal für Praktische Chemie* for many years.
 56. O. L. Erdmann, "Isatinbildung durch Ozon," *Ann. Phys. Chem.*, **1857**, 71, 209; also *J. Prakt. Chem.*, **1857**, 61, 209.
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 58. C. F. Schönbein, "Hat der Phosphor einen Geruch?" *Ann. Phys. Chem.*, **1848**, 75, 377-86.
 59. C. F. Schönbein, "Ueber den Ozongehalt der bei der langsamen Verbrennung des Phosphors in der atmosphärischen Luft sich bildenden Säuren," *Ann. Phys. Chem.*, **1856**, 99, 473-6.
 60. Richard Felix Marchand, 1813-1850. Professor of Chemistry, University of Halle from 1843. Editor, with Erdmann, of *J. Prakt. Chem.* until his untimely death.
 61. R. F. Marchand, "Ueber das Leuchten des Phosphors," *J. Prakt. Chem.*, **1850**, 50, 1-11. Light emission by phosphorus could be a subject for a separate historical study.
 62. Ernst Werner von Siemens, 1816-1892. Electrical genius, founder of Siemens & Halske, predecessor of the present Siemens AG.
 63. Heinrich Daniel Rühmkorff, 1803-1877. In 1840 he set up his own mechanical works in Paris; known for his induction apparatus. In 1864 he received a prize of 50,000 francs from the French government for this invention.
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 65. L. von Babo, "Beiträge zur Kenntniss des Ozon," *Ann. II Supp.*, **1863**; a. footnote, pp 266-8; b. pp 265-296.

66. Clemens Heinrich Lambert von Babo, 1818-1899, Professor in Freiburg im Bresgau.
67. Rudolph Böttger, 1806-1881, Professor of Physics in Frankfurt/Main.
68. R. Böttger, "Ueber die Bildung des Ozons auf chemischen Wege," *Ann. Phys. Chem.*, **1862**, 117, 188-90.
69. Carl Weltzien, M.D., 1813-1870, Professor of Chemistry in the Technical Institute of Karlsruhe; local chairman of the 1860 Karlsruhe Conference.
70. C. Weltzien, "Über die Bildung des Ozons," *Ann. Chem. Pharm.*, **1867**, 142, 107-110.
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72. Albert Ripley Leeds, 1843-1902. Professor of Chemistry at Haverford College from 1867 to 1871; from 1871 Professor at Stevens Institute of Technology, Hoboken, NJ/USA.
73. C. F. Schönbein, "On the Allotropic States of Oxygen; and on Nitrification," *Philos. Mag.* IV, **1862**, 23, 466-9; "Ueber die Darstellung des Ozons auf chemischen Wege," *Ber. Verh. Nat. Ges. Basel*, **1863**, 2, 305; also *J. Prakt. Chem.*, **1862**, 86, 70, 377; *Ann. Chim. Phys.* [3], **1863**, 68, 48.
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77. Francois Pierre Le Roux, 1832-1907. Professor at the Conservatoire des arts et metiers, then Professor of Physics in Ecole Superieure de pharmacie, Paris.
78. E. Saint-Edme, "Sur la faculté qu'a le platine rendu incandescent par un courant électrique de produire des combinaisons gazeuses," *C. R. Hebd. Seances Acad. Sci., Ser. C*, **1861**, 52, 408; also E. Saint-Edme, *C. R. Hebd. Seances Acad. Sci., Ser. C*, **1865**, 59, 291; *J. Prakt. Chem.*, **1865**, 94, 807.
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80. C. F. Schönbein, "Ueber die Oxidation des Silbers und anderer Metalle durch Ozon" *Ber. Verh. Nat. Ges. Basel*, **1851**, 9, 14.
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83. van den Broek, "Ueber die Zersetzung der Chlorwasserstoffsäure durch Ozon" *J. Prakt. Chem.*, **1862**, 86, 317-18.
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90. C. F. Schönbein, "Ueber die Erzeugnisse der langsamen Verbrennung des Aethers," *J. Prakt. Chem.*, **1868**, 105, 232-9.
91. Eugen Franz Cajetan von Gorup-Besanez, M.D., 1817-1878. Professor in Erlangen from 1855; author of two textbooks.
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108. Thomas Sperry Hunt, 1826-1882. From 1847-72 chemist and mineralogist in the Geological Survey of Canada, simultaneously from 1856-62 Professor of Chemistry at the University of Quebec and from 1862-67 at the University of Montreal. From 1872-78 Professor of Geology at the Technical Institute, Boston, then in 1878 back to Montreal. From 1859 member of the Royal Society, London.
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THE CONTRIBUTIONS OF PAYEN AND LABILLARDIÈRE TO THE DEVELOPMENT OF COLORIMETRY

Lluís-Garrigós Oltra, Carles-Millan Verdú, and Georgina-Blanes Nadal, Escola Politècnica Superior d'Alcoi

Two very important contributions to the development of colorimetry are identified with the 17th century. In 1729, the French mathematician and astronomer Pierre Bouguer (1698-1758) published the work *Essai sur la gradation de la lumière*. In the section where he studied transparency and opacity, he finished by concluding that when light crosses a transparent medium, it diminishes in geometric progression to the thickness of the medium crossed (1). Thirty years later, in 1760, the German Johann Heinrich Lambert (1728-1777) rediscovered Bouguer's ideas independently and published the work *Photometria sive de mensura et gradibus luminis, colorum, et umbrae* (2). It is advisable however to note the different positions held by each work: while Bouguer carried out a physical analysis, centered exclusively on the characteristics of an optical system, Lambert developed relative structural concepts about the nature of the matter, accepting a distribution of particles in a medium, and attributing the light absorption to the mentioned particles. Lambert's interpretation also had a greater mathematical emphasis, in which he proposed two basic theorems (sections 876 and 877 of his *Photometria*) (3).

The experiments of Bouguer and Lambert on the absorption of light were restricted to transparent solids. It was not until 1852 that the conclusions of Lambert about the absorption of light were extended to colored solutions. Two authors, Félix Bernard (4) and August Beer (5), independently and within a few months of each other, published doctoral dissertations in which they stated that concentration in colored solutions plays the

same role as opacity in transparent solid media. In both works the notion was introduced of the coefficient of absorption, being characteristic of the material studied.

These initial contributions serve as a foundation for the later contributions to the development of colorimetric methods Payen and Labillardière. Although some authors (6) place the origin of colorimetry in the 1830s, recently there has been an investigation into the existence of procedures and apparatus used prior to this (7-11).

The beginnings of volumetric analysis: Determination of chlorine dissolved in water

The use of chlorine to bleach cloth, according to the procedure of Berthollet, coincided with its discovery by Scheele in 1774. Technicians, interested in this process for its possible application to industry, began designing variations of the method very quickly. Before 1789 a French chemist, Henri Descroizilles, had also encountered the new bleaching procedure. Concluding that its effectiveness depended on the chlorine concentration in water, he designed a procedure to measure the chlorine content by making use of the property of hypochlorite to decolorize solutions of indigo in sulfuric acid (12). The resolution of this problem required the design of a glass instrument (Fig. 1), on which he marked a scale with hydrofluoric acid to quantify the results. In Fig. 1, three pieces are distinguished (sectors 1-3): the first represents the "buret," in the author's nomenclature, although it would currently be the "test tube" or "gradu-

ate cylinder." The second, given the name "mesure," is a pipet. This utensil is inserted into chlorine water until the liquid reaches mark A. The upper opening is then sealed with the finger, and later the volume of liquid is poured into the graduate cylinder until it reaches the zero level. The third piece represented is a suction pipet containing a fixed quantity of indigo, which is then poured little by little into the graduate cylinder. The different pieces are inserted in a receiving vessel that contains the solutions to be analyzed. A diagram of the instruments is shown in Fig.1 (sectors 4-7). This is probably the first well-known case of an apparatus designed for quantitative analysis. The first description of Descroizilles's method, however, was given by Berthollet in 1789 in an article in which he described his own procedure for bleaching cloth (13). According to Szabadvary (14, p 210) Berthollet and Lavoisier had already used the reaction between chlorine and indigo in 1788 to estimate the color intensity in indigo samples. Berthollet's description of Descroizilles' procedure, carried out in 1789, did not correspond to the author's description of the one carried out later in 1795. In this case, the indigo is poured into the chlorine water.

Other European authors independently described the same method, introducing variations on the initial model published in 1789. Berthollet himself (13, p 177) pointed out that Descroizilles' procedure, which he named "Berthollimètre," was used by Watt, who employed cochineal in place of indigo. Among all the technical innovations based on Descroizilles' work, it is important to highlight the proposals between 1815 and 1826 of other French chemists who improved substantially

the original procedure, to which was given the name "chlorometres." Thus, Welter (1817) (15), Gay-Lussac (1824) (16), and Henry and Plisson (1826) (17) all published works about "chlorometres." They all focused their objective on the quantification of the indigo decolorization process with sulfuric acid. However in 1824 another French chemist, Houtou de Labillardière, substituted starch-iodide for indigo in order to design an alternative procedure to estimate the chlorine content in commercial calcium hypochlorite. This procedure was based on the ability of the starch-iodide complex to turn blue in an acid medium (18). Houtou of Labillardière was the first to use the iodine-iodide system in volumetric analysis, thirteen years after the discovery of iodine by Bernard Courtois.

The means by which Labillardière's method was made public was very bizarre, and it caused a great deal of controversy between Labillardière and Payen. News of this procedure was reported in the March 1826 issue of the *Journal de Chimie Médicale, de Pharmacie et de Toxicologie* by Julia de Fontenelle (19), and also in the session of the Paris Société Philomatique, where Payen was present. Pierre Jean Robiquet, the Société Philomatique secretary, presented Labillardière's method, as is verified in the *Journal de Chimie Médicale, de Pharmacie et de Toxicologie* May issue (20). A concise description of this procedure appeared in the Fontenelle abstract. Later, in a meeting of the Société Philomatique, Anselm Payen criticized the method, while Robiquet defended Labillardière. This prompted Labillardière to publish a note in the May 1826 issue of the *Journal de Pharmacie et des Sciences Accessoires* (18), in which he criticized Descroizilles' method as being imprecise. He pointed out that he had built his apparatus two years previously, on April 2, 1824, at the Rouen Academy. The question of these dates is very important because in the same text he complained about the treatment accorded his own work, as well as the suggestion that Gay-Lussac had already designed a similar procedure. After detailing his procedure, he concluded with some derisive comments directed at Payen (18):

En effet, on a dû s'étonner comme moi que M. Payen, l'un des collaborateurs du Dictionnaire technologique et auteur d'un traité des réactifs, n'ait pas prévu l'utilité du sel marin dans la liqueur d'épreuve, et surtout n'ait pas fait la remarque que je faisais les essais, non sur le chlore, mais bien sur le chlorure de chaux. En

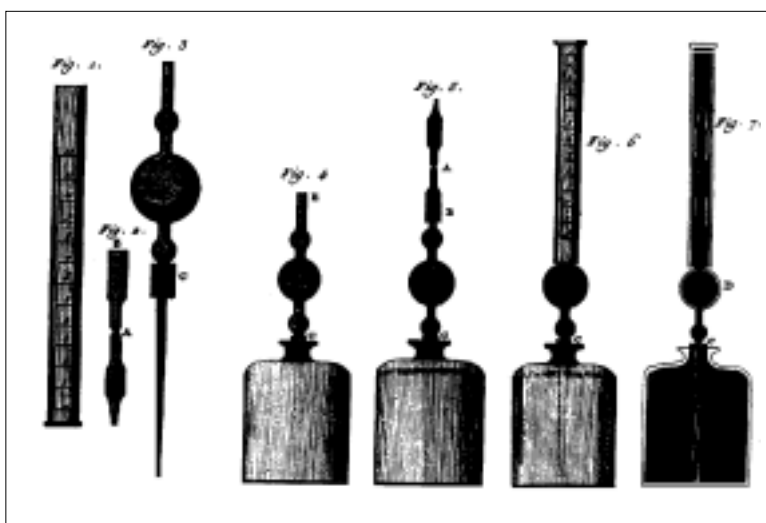


Figure 1. Descroizilles' glass instruments

consultant le *Traité de chimie élémentaire* de M. Thenard, 4^e édition, tome II, pag. 191, il pourra voir que le mélange de chlore et d'acide sulfureux dissous se transforme tout à coup en acide sulfurique et en acide hydrochlorique par la décomposition de l'eau.

Payen was not intimidated by this attack, immediately submitting a note to the Société de Pharmacie, which was published in the June issue of the *Journal de Pharmacie et des Sciences Accessoires* (21). The following text is an extract of this note:

Le fait que j'ai annoncé de la décoloration par l'acide sulfureux de l'amidon bleui est vrai, mais cet acide ne peut exister en contact avec le chlore que dans le cas où l'un et l'autre sont anhydres, ce qui n'était pas dans l'application que j'en ai faite par inadvertance.

Quant à l'utilité dont pourrait être la solution du sel marin, dans la liqueur d'épreuve, aucun des chimistes auxquels j'ai eu l'occasion d'en parler, et bien d'autres sans doute, ne s'en étaient pas douté avant l'explication donnée par M. Hotou-Labillardière.

This confrontation between Payen and Labillardière had a postscript in the following year when Henry presented Labillardière's colorimeter in the *Journal de Pharmacie et des Sciences Accessoires* and in a footnote, affirmed (22):

Le décolorimètre de M. Payen pour essayer la force des charbons était composé d'un appareil à peu près semblant.

Coal as a decolorizing substance: Bleaching of sugar

In 1785, the German-Russian chemist Johann Lowitz, while trying to obtain a crystalline form of tartaric acid, noticed that coal powder, which had inadvertently contaminated a solution of the acid, had very effectively eliminated the impurities in the solution. This accidental discovery led to a planned series of experiments on the decolorization of certain substances of vegetable or animal origin with vegetable and animal coal. As a result of all of these experiments, Lowitz concluded that coal decolorized some solutions and also eliminated suspended impurities and the inherent odor in the solutions. Consequently, he recommended the use of pow-

dered coal as a purifying agent in the production of vodka and sugar syrup and in the treatment of water for human consumption. Lowitz's work generated interest from many pharmacists and chemists. In 1811, Cadet de Gassicourt (23) and Pierre Boullay (24) published reviews, in which they described various studies on the decolorizing and bleaching properties of vegetable and animal coal. Boullay credited Figuier (25, 26) with the discovery in animal coal of a greater bleaching power for colored liquids.

In the second decade of the nineteenth century, the use of animal coal in industrial decolorizing and bleaching processes was increasing; as a result the production of coal was gaining greater economic importance. In

DÉSIGNATION de l'espèce de charbon employé.	Poids du charbon.	Quantité de liqueur d'essai d'indigo décolorée.	Quantité de liqueur d'essai de mélasse décolorée.	Expression numérique de la force décolorante du charbon sur l'indigo.	Expression numérique de la force décolorante du charbon sur la mélasse.
Sang calciné avec la potasse.	1 gramme.	1 litre 6	0, 18	50	30
Sang calciné avec la craie.	id.	0,57	0, 10	18	11
Sang calciné avec le phosphate de chaux.	id.	0,38	0, 09	13	10
Gélatine calcinée avec la potasse.	id.	1,15	0, 14	30	15, 5
Albumine calcinée avec la potasse.	id.	1,08	0, 14	34	15, 5
Fécule calcinée avec la potasse.	id.	0,24	0, 08	10, 6	8, 8
Charbon de Facécite de potasse.	id.	0,18	0, 04	5, 6	4, 4
Charbon obtenu de la décomposition du sous-carbon de soude par le plus haut.	id.	0,38	0, 08	12	8, 8
Noir de fumée calciné.	id.	0,123	0, 03	4	3, 3
Noir de fumée calciné avec de la potasse.	id.	0,55	0, 09	15, 2	10, 6
Charbon de os traité par l'acide muriatique et par la potasse.	id.	1,15	0, 18	45	30
Charbon de os traité par l'acide muriatique.	id.	0,06	0,015	1,87	1, 6
Boule végétale ou animale calcinée avec le phosphate de chaux.	id.	0,064	0,017	3	1, 9
Charbon de os brut.	id.	0,031	0,009	1	1

Figure 2. Bussy's Scale

keeping with this, Cadet de Gassicourt published an article in 1818 (27) detailing the different methods of obtaining coal, as well as some of its more important applications. Among these, he highlighted its use in the sugar beet industry, introduced by Monsieur Magnés and Charles Derosne (28). One question of economic importance remained unanswered: which type of coal had the greatest bleaching power?

Because of the uncertainty about the chemical behavior of coal in liquid solutions, the Société de Pharmacie of Paris decided in 1821 to offer a prize of six hundred francs for solutions to the following (30):

1. To determine the way in which coal operates in the decolorizing process.
2. To investigate the influence exerted by the other trace substances in the coal.
3. To investigate whether the physical state of the coal is important in the decolorizing process.

Seven entries were received for this competition, although one was submitted before the opening date. Antoine Bussy, Professor of the School of Pharmacy of Paris, was awarded first place, and second place went to Anselm Payen (31). Both reports, published in the 1822 May-June issue of the *Journal de Pharmacie et des Sciences Accessoires*, included a study of specific procedures for making comparisons in the bleaching powers of different coals.

In Bussy's work the system for evaluating the bleaching strength of coal was based upon its capacity to decolorize indigo in sulfuric acid. Indigo was chosen for its important properties: the quantity of indigo could be measured accurately; the bleaching point could be observed with certainty; and the indigo dye was unaffected by light or other factors. The author provided the following description (32):

C'est ainsi que j'ai reconnu que la dissolution qui m'a servi pour mes essais contenait un millièrme de son poids d'indigo. Pour essayer un charbon avec cette dissolution, j'en prends une certaine quantité que je mets dans une fiole, en contact avec une quantité connue de charbon; je chauffe légèrement, ce qui hâte un peu la décoloration, et j'ajoute de la liqueur d'essai jusqu'à ce que le charbon refuse de la décolorer.

Bussy carried out experiments with molasses solutions in order to determine whether the relative order of decolorizing power remained the same when the coal passed from one colored solution to another. Using 1

gram of each coal sample, he compared their bleaching power against that of bone black (*Charbon des os brut*), to which he arbitrarily assigned the value of 1. These results, expressed in table form, are reproduced in Fig 2.

Payen's work (33) was less systematic than the Bussy's. He carried out a series of experiments from which he tried to answer the three basic questions of the competition. He failed, however, to describe procedures for establishing the comparative bleaching power of various coals. In a footnote he observed (33):

Je me propose de publier un instrument (decolorimètre) à l'aide duquel j'obtiens la mesure exacte des actions décolorantes. Sa construction es fondée sur ce que l'intensité des couches colorés est en raison inverse de leur épaisseur; ainsi, en prenant pour unité une nuance quelconque, on obtiendra tous les multiples, tous les rapports possibles de cette nuance à tous les autres; il suffit pour cela de mesurer exactement la hauteur perpendiculaire entre deux plans diaphanes qui comprennent le liquide coloré ramené à l'unité de nunce au moyen de l'espace augmenté ou diminué entre les deux plans.

Payen's approach was illustrated in his *Traité Élémentaire des Réactifs* (34), where he gave a description of his apparatus, represented in Fig. 3. The analysis procedure was based on a visual comparison. In the small cylinder P, the standard solution is introduced along with 10 grams of a sugar solution and 1000 grams of water. The test solution is combined with 1 deciliter of standard solution and 2 grams of coal; the mixture is filtered and the filtrate introduced into the vertical tube DC. As the mobile piston BB is slowly withdrawn, the test solution falls into the fixed horizontal tube AA, and

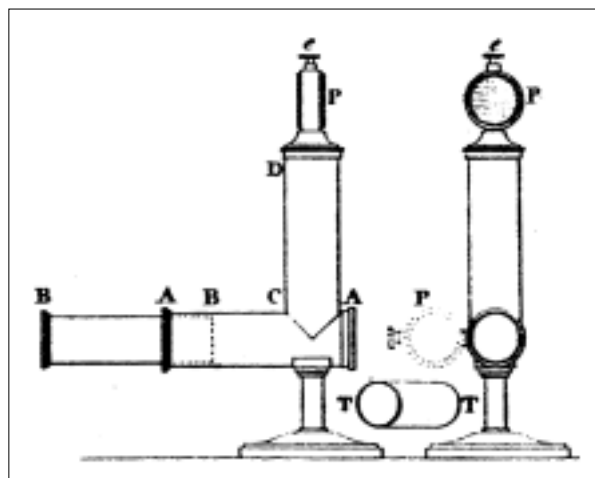


Figure 3. Payen's decolorimeter.

the optical path between the lenses in the right ends of the tubes BB and AA is increased. The piston BB is withdrawn until the color of the test solution is equivalent to that of the standard in small cylinder P. Since the mobile piston has a graded scale, it is possible to determine the distance between the lenses of tubes BB and AA. Each centimeter of the scale of tube BB is equal to the thickness of cylinder P. Thus, it is possible to make a relatively precise comparative table of the decolorizing power of different coals.

In 1827, Labillardière also used the absorption of light through a transparent colored solution to determine the bleaching power of commercial products. In spite of this, Snelders (6) says that Keates (1830) and Lampadius (1838) carried out the first colorimetric experiments, but it is evident that the credit for this should go to Labillardière, who designed his “colorimètre” for this purpose. Although the first report of his apparatus and procedure appeared in a paper published by the author in 1827 (35), an account written by Henry appeared in *Journal de Pharmacie* in the same year (22). (The authors have been unable to gain access to the former publication). A new description appeared one year later in a paper published in *Annales de l’Industrie française et étrangère*. This article was also reproduced in the work “Traité complet des propriétés, de la préparation l’emploi et des Matières Tinctoriales et des Couleurs” by Leuchs (36, 37). Labillardière justified the determination of the coloring capacity of various raw materials used in industrial dyes in order to evaluate their economic worth. He explained Descroizilles’s procedure, later modified by Welter and Gay-Lussac, which was based on the time required to decolorize raw materials with chlorine. He refused to use this imprecise method because chlorine can act upon the coloring substances and also other substances present in raw materials.

Labillardière’s central idea was to see whether he could transfer the successful results from the test tube to fabrics and other materials. In order to do so, he designed a black box through which he could compare two colored solutions, one of the dye and the other of the standard. He named this instrument “colorimètre.” His arrangement must be similar to the one depicted in Fig. 4, reproduced according to the author’s description. Later this was illustrated in some texts (38, 39, 40). We have found three different illustrations, two of which are shown in Figs. 5 and 6.

The method for using the apparatus is described in the instructions below (36):

Après avoir traité ou dissous comparativement dans l’eau, ou tout autre liquide convenable, des quantités égales de matières tinctoriales, on introduit de ces dissolutions dans les tubes colorimétriques jusqu’au zéro de l’échelle supérieure; on les place ensuite dans la boîte par les deux ouvertures pratiquées à cet effet, et, après avoir comparé leur nuance, si on trouve une différence, on ajoute de l’eau à la plus foncée, et l’on agite ensuite le tube après avoir bouché l’extrémité avec le doigt; si après cette addition d’eau on remarque encore une différence, on continue d’en ajouter jusqu’à ce que les tubes paraissent de la même nuance. On lit ensuite sur le tube dans lequel on a ajouté de l’eau le nombre de parties de liquide qu’il contient; ce nombre comparé au volume de la liqueur contenue dans l’autre tube (qui est égal à 100), indique le rapport entre le pouvoir colorant ou la qualité relative de deux matières tinctoriales; et si, par exemple, il faut ajouter à la liqueur la plus intense 25 parties d’eau pour l’amener à la même nuance que l’autre, le rapport au volume des liqueurs contenues dans les deux tubes sera comme 125:100; et la qualité relative des matières colorantes sera représentée par le même rapport, puisque la quantité de ces matières est proportionnelle à leur pouvoir colorante.

The author recommended several precautions to assure dependable results: exhaustive drying of the test tubes,

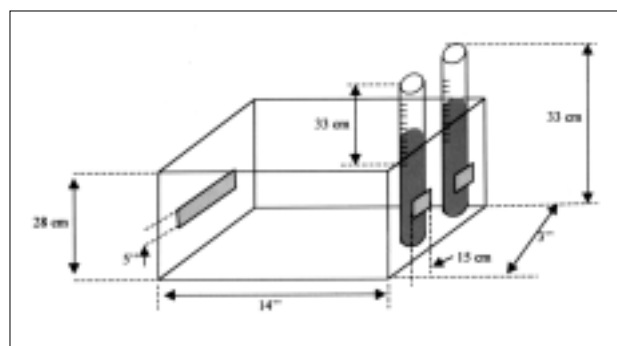


Figure 4. Labillardière’s colorimeter (from Labillardière’s description)

avoidance of contamination of the tubes by handling, blackening of the box, and the use of homogeneous light of appropriate wave length for various coloring substances. Finally, it is important to highlight Labillardière’s thought (36) that the method could be generally applied to different coloring materials but specifically to cochineal. It is not surprising that Girardin (41) put this generalisation into practice, for it was he (38) who described the apparatus and provided a graphical illustration (Fig. 6). He himself applied the procedure to the analysis of indigo, cochineal, and bija, the

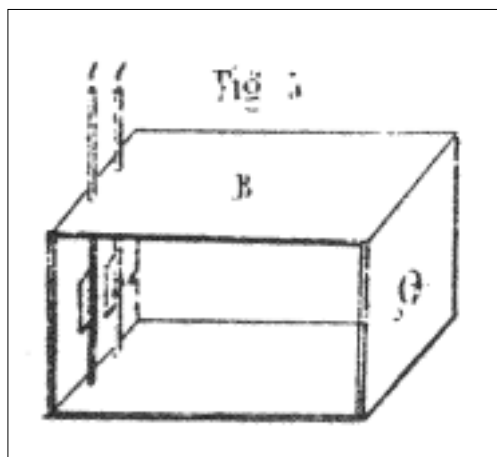


Figure 5 Labillardiere's colorimeter (from a paper by Chevallier)

common name given to the bixaceas species, better known as Achiote. In his text Girardin explicitly recognises Labillardière as the inventor of the apparatus.

A. Payen

Anselm Payen was born in Paris on January 17, 1795, son of the owner of an ammonium chloride factory. This product was made from animal waste from a slaughterhouse situated in Grenelle, a small village within the parish of Vaugirard. In this village, just as in the village of Javel on the outskirts of Paris, in which the Count d'Artois had established an acids and mineral salts factory, a chemical and food processing industry was emerging at the end of the 18th century. In 1824, as the importance of this industry was growing, a group of investors decided to establish an industrial town in this location; and so the village of Grenelle separated itself from the parish of Vaugirard in 1830 (50).

Payen's father forbade his son to attend school, insisting instead that he be educated at home. This meant that Payen was reared with a very strong scientific background and in an environment almost completely devoid of social contact with people of his own age. Possibly because of this he grew up to be a very ill natured man. He studied chemistry under the supervision of Vauquelin and Chevreul (49) and later, at the age of twenty, he went on to manage a sugar beet factory at Vaugirard (50). However, Payen considered this position as more a part of his family's industrial activity. In his prize-winning paper (33) of 1822, he chose to introduce himself as "fabricant de sel amoniac."

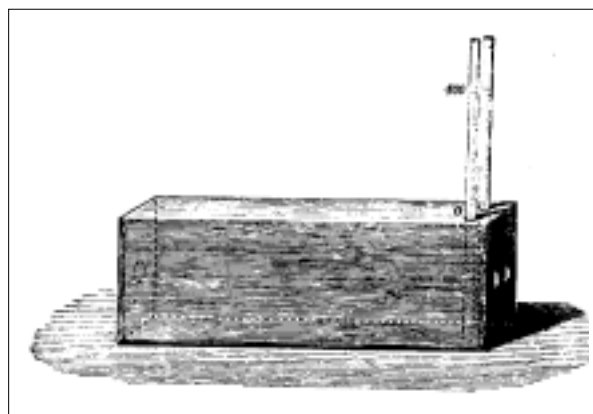


Figure 6. Labillardiere's colorimeter (from a paper by Girardin)

In the early 1820s he became interested in beer making in France. Payen and Chevalier published a paper describing the possibility of cultivating hops in Grenelle and in Vaugirard (29) for use in the making of beer. His first successful industrial venture was the manufacture of borax, which until then had been imported and about which he published various papers (51).

In 1829 Payen began to teach industrial chemistry at the École Centrale des Arts et Manufactures and ten years later he occupied a similar chair at the Conservatoire des Arts et Métiers. From 1842 Payen became a much more prominent figure in the academic world and was also made "Honour Legionkavalier," although he did not abandon his industrial interests. He wrote a large number of papers, most of which were concerned with the food industry. A complete bibliography of Payen's works is to be found in two reference sources (51, 52). His investigations revealed flaws in some industrial processes. For example, his studies on the analysis of wheat and other cereals forced a change in the method of bread making. His main contribution, in collaboration with Persoz, was work with carbohydrates between 1834 and 1835, involving hydrolysis of starch and its subsequent transformation into sugar by the action of a malt-containing substance they called diastase. These chemical processes were at the core of a controversy with Guerin-Varry, who claimed to have discovered them. Also interested in the study of plants, he discovered that the chemical composition of starch was independent of the plant from which it was obtained. He also characterised an isomeric starch obtained from woody plants, which he named cellulose, and achieved the separation from cellulose of another substance he named "lignina."

Until his death on May 13, 1871, Payen lived in a working-class suburb of Paris and was known for his dedication to teaching. In spite of ill health, he continued giving classes until one day when he collapsed in the classroom, two years before his death. He died during the "Comune" and was afforded a simple funeral.

François Joseph Houtou de Labillardière

François Joseph Houtou de Labillardière was born in Alençon (l'Orne Département, Normandy) on April 1, 1796 of a small, middle-class family. Although little is known about his parents, his grandfather, Michel Jacques Houtou, was the master of La Billardiere and his grandmother, Madeleine Jeanne Lepin, was a manufacturer and trader of handmade lace (42). His uncle was the naturalist Jacques Julien Houtou de Labillardière. Details about his childhood and education are sparse. In July 1817 he was "preparateur" (a post in the French universities) for Dulong's physics and chemistry classes in l'Ecole vétérinaire d'Alfort (43). Later he held the same post in the Collège de France in Paris with Thenard, with whom he worked from 1819 until 1821 (44). It is possible he was later forced to leave Paris and retire to his native Normandy for reasons of health (45). By 1825 he was in Rouen, where he had described his chlorometer to the academy on April 2, 1824 (18). On September 6, 1825 he was named a member of the "Académie de Médecine" in the Pharmacy section (46), but his dedication to continue chemistry teaching in Rouen is evident from his presentation of the colorimeter (22). His successor as Thénard's "preparateur, Lecanu, later wrote a memorial text to Labillardière upon his death in 1867.

As a chemist he worked on a great variety of problems, although he did not always successfully conclude his investigations. It is remarkable to note that Lecanu (45) once observed that Labillardière might have had the honor to codiscover quinine with Pelletier and Caventou, had he continued along a line of investigation carried out in the course of a lesson given in the Collège de France. He later continued his investigations into essential oils of petroleum and natural camphor and his search for the essence of turpentine (47), work that was at the center of a great deal of controversy. There was also controversy concerning his paper on malic acid, which he said was identical to sorbic acid (48).

Labillardière made important technical contributions in the field of dyes. He designed a piece of appa-

ratus we can consider to be the first colorimeter. From his work on dyes derived from rubber he devised a color he called "solitaire," which became important in the French fashion world and from which he realized some economic benefit (45).

Disillusioned over the constant controversies with other academic chemists, he found the industrial environment more attractive around 1827, when evidence of his activity as a scientific investigator seems to have disappeared. Toward the end of his life, he and his wife became interested in botany and entomology. He died in Alençon on January 12, 1867 (42).

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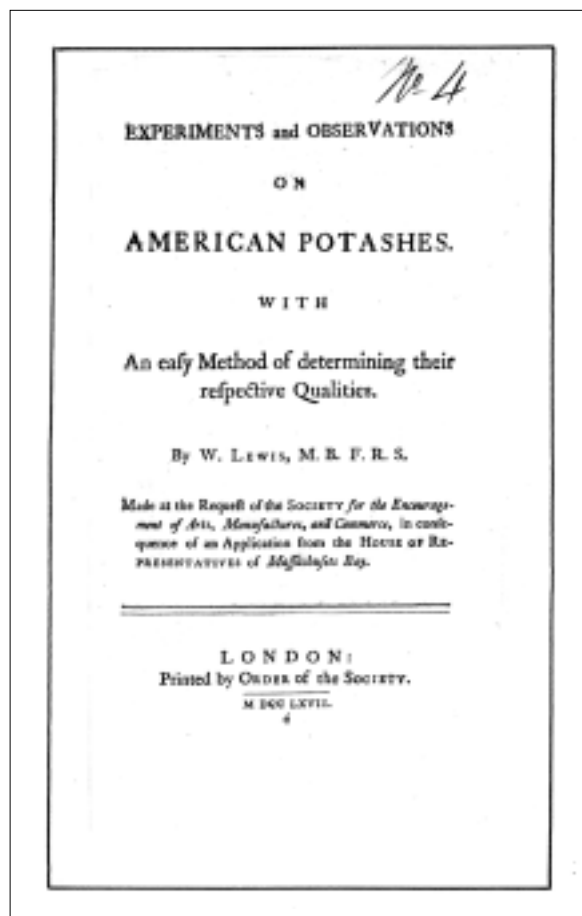
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THE BIRTH OF TITRIMETRY: WILLIAM LEWIS AND THE ANALYSIS OF AMERICAN POTASHES

Frederick G. Page, University of Leicester.

William Lewis (1708-1781), was a physician, author, and an experimental chemist. Sometime after 1730 he gave public lectures in London on chemistry and the improvement of pharmacy and manufacturing arts (1). With a growing reputation as a chemical experimentalist he was elected F.R.S., on October 31, 1745, and was then living in Dover Street, London. In 1747 he moved to Kingston-upon-Thames, where he set up a well-equipped laboratory and presumably continued in medical practice. From about 1750 until his death in 1781 Lewis employed Alexander Chisholm as his assistant in chemical and literary works (2). These improved the knowledge and practice of pharmacy, but as a practical consulting chemist Lewis has received little biographical recognition. He was awarded the Copley Medal by the Royal Society in 1754 for researches on platina (platinum), which he claimed was a distinct metal, and for devising methods of chemical identification. These results were published in *Philosophical Transactions*, 1754 and 1757 (3).



In order to place Lewis's work in the context of the early beginnings of the industrial revolution in Britain, Sivin (4) has used a chronological argument; he cites Ashton's suggestion (5) that 1782 was the beginning of the industrial revolution because in that year most statistics indicated a sharp increase in industrial production. But, as Sivin argues, it seems reasonable to assume that by the time such early statistics became available, those industries that had created salable products had already become established and were no longer in their early years of founding. On this basis the industrial revolution must have begun earlier. By the middle of the eighteenth century the social and economic demands of a greatly increased population more probably heralded this dramatic change, visible in the growing demands on the textile, metallurgical, and ceramic industries, all of which required chemicals. Clow relates this birthdate, 1760 to 1780, with the increased number of

registered patents as a barometer of the new industrial activity (6). This places Lewis's analytical developments in a key position relative to the early industrial revolution.

His considerable and yet little recognized contribution to quantitative chemical analysis is best seen in his work on American potashes in consultation with the Society for the Encouragement of Arts, Manufactures, and Commerce. The analytical methods he devised foreshadowed what later developed into titrimetric analysis, and for this work Lewis was awarded the Gold Medal of the Society in 1767.

The Society's archives confirm its part in promoting the manufacture of potash in America by offering premiums to British importers. An interesting illustration and one closely related to the work by Lewis can be seen in a letter written in 1766 from a member of the Society, Jno. [John] Mascarene of Cambridge, New England. This throws light on contemporary thinking regarding the status of potash manufacture, its supply, and the dearth of analytical knowledge. Mascarene had been making potash for about twelve years and in referring to exports of this material to England he mentioned concern about its quality (7):

But as all advantages are liable to abuse, and we have good reason to believe that a considerable quantity of Pot-Ash has been exported within this Year or two from thence to the English market, which was found not only bad in quality, but some Casks filled with an heterogeneous mixture.

Adulteration of the potash is obviously suspected, and he requests information regarding the lowest acceptable quality and the method of determining this. Lewis's use of a titrimetric method and color indicator to determine alkali content in American potashes shows a marked advance over earlier analytical methods. For example, in 1729 C. J. Geoffroy, in an essay presented to the French Academy and later published (8) in *Mémoires de l'Académie Royale des Sciences* (Paris, 1731), described an analytical method to determine the strength of vinegar by adding a controlled amount of powdered potassium carbonate to a known amount of vinegar until effervescence ceased. By this method Geoffroy formed a comparative idea of the vinegar's strength from the amount of potash used. Clearly this was an example of the quantitative use of an acid base reaction and employed aspects of what we now term titrimetry. However, the end point, or the point of "full saturation," can only have been within the observable accuracy given by cessation of effervescence.

This early record of an analytical process involving neutralization between an acid and a base occurred 27 years before the publication of Francis Home's *Experiments on Bleaching* (1756). Home's method of determining the strength of various alkaline salts such as pearl and blue ashes depended upon the use of a teaspoon as a volumetric measure (9):

In order to discover what effect acids would have on these ashes, and what quantity of the former the latter would destroy; from which I might be able to form some judgement of the quantity and strength of the salt they contained; I took a drachm of blue pearl ashes, and poured on it a mixture of one part spirit of nitre, and six parts water; which I shall always afterwards use, and call the *acid mixture*. An effervescence arose, and, before it was finished, 12 tea-spoonfuls of the mixture were required. This effervescence with each spoonful of the acid mixture was violent, but did not last long.

This was Home's method of measuring the strength of the alkali salt by neutralizing a weighed amount with a measured quantity of acid, the end point being the cessation of effervescence.

Lewis's original report on his work on American potashes is held by the Royal Society of Arts; the title page of the printed transcription reads (10):

Experiments and Observations on American Potashes with An easy Method of determining their respective Qualities. By W. Lewis, M. B. F.R.S. Made at the Request of the Society for the Encouragement of Arts, Manufactures, and Commerce, in consequence of an Application from the House of Representatives of Massachusetts Bay. Printed by order of the Society, 1767.

Eight potash samples were submitted to Lewis by the Society, and his report begins with a detailed comment on their physical appearance, taste, etc., and the wide variation of solubility in water. Using four-ounce samples, he determined the total dissolved solids in the clear filtrates by complete evaporation. After drying the crystals at "a moderate heat, below red hot," he showed by weighing that all eight samples contained over three ounces of soluble salt. Quantitative recrystallizations were carried out in an attempt to separate any salts present other than the alkali (potash). Lewis easily identified the initial nonalkali crystals, since in his experience true alkali would not crystallize out at the chosen dilution. Thus vitriolated tartar (potassium sulphate) and sea salt were isolated and a dried mixture of these, when tasted, indicated their presence together with some alkali; such was the application and apparent sensitivity

of tasting in this period. However, later in the report Lewis showed little enthusiasm for this method based on crystallization (11):

[this] was found so difficult and tedious, that the enquiry was dropt, and another way of examination tried.

That other way was to be:

...the quantity of true alcali in the salts might be discovered by their power of saturating acids, compared with that of an alcali of known purity; and this method succeeded so well, that it is hereafter proposed for the assaying of Potashes, and the manner of procedure described at large.

Tabulation of Lewis's results of the eight samples showed marked variation in alkali content. In this 34-page publication he described his analytical procedures in only the last four pages; and it is these that are now to be considered. In the opening paragraph he referred to other chemists who, using the methods now being considered, only achieved comparative results and not absolute values. In order to achieve accuracy he emphasized certain technical details; for example, he realized that the presence of 'earthy matter' in the potash samples would affect the amount of acid needed for complete saturation; this, and any other impurities must therefore be removed before the determination. As mentioned above, he dissolved the soluble or true alkali in water and removed any insoluble earthy material by filtration. He made no claim for the originality of this procedure which certainly became a standard technique (12):

The quantity of acid, necessary for the saturation of the lye, should be determined, not by drops or teaspoonfuls, but by weight [*a clear reference to Home's work*]; and the point of saturation, not by the ceasing of the effervescence, which it is extremely difficult, if not impracticable, to hit with tolerable exactness, but by some effect less ambiguous and more strongly marked, such as the change of colour produced in certain vegetable juices, or on paper stained with them.

In acknowledgment of Lewis's early use of a chemical indicator, it seems appropriate to quote his further instructions on this topic (13):

The finer sort of purplish blue paper used for wrapping sugar in, answers sufficiently well for this purpose; its colour being changed red by slight acids, and afterwards blue or purple again by slight alcalies. What I have chiefly made use of, and found very convenient, is a thick writing paper stained blue on one side with an infusion of lacmus or blue archil, and red on the other by a mixture of the same infusion with so much dilute spirit of salt as is sufficient

just to redden it. The paper is washed over with a brush dipt in the respective liquors, two or three times, being dried each time, till it has received a pretty full colour, and afterwards cut in slips a quarter of an inch or less in breadth; a bit of the end of one of the slips being dipt in the liquor to be tried, the red side turns blue while any of the alcali remains unsaturated, and the blue side turns red when the acid begins to prevail. If either the acid or alcali considerably prevails, the paper changes its colour immediately on touching the liquor: if they prevail but in a low degree, the change is less sudden. The part dipt is always to be cut off before a fresh trial.

Lewis chose spirit of salt (hydrochloric acid) rather than sulfuric acid in his belief that hydrochloric acid would not react with any sodium chloride that might be present in the soluble portion of the potashes under test. He gave very precise instructions on the method of preparing a conveniently diluted acid solution and of standardizing it by using a carefully weighed amount of perfectly dry potassium carbonate (Lewis assumed 100% purity). At no point did he know the actual amount of hydrochloric acid in his 'standard' dilute solution; this was not important for he was not calculating, as we would today, the results of a chemical reaction based on a chemical equation involving molecular weights as units in the calculation. What is significant in these details is that he standardized the acid by giving it a numerical value of strength in terms of equivalence to a known weight of what he believed was pure potash. Once this had been established, aliquots of the same acid could be used in subsequent determinations.

It would have been difficult to achieve greater accuracy in measuring the amount of acid used in the titration by any method other than that advocated by Lewis. In this simple gravimetric technique, a vial of dilute acid was merely counterpoised on a balance (there is no mention of its sensitivity). After he had poured off the amount needed for complete saturation of the potash, he again counterpoised the vial. The weight difference indicated the amount of acid used. Lewis extended this further by using a fixed amount of sample, whereby he could read directly the amount of potash from the marked balance weights.

It would be an exaggeration to claim that Lewis introduced the idea of what became known as "back titration" but he hints that if the end point, the change in color of the indicator, is accidentally exceeded, it is not necessary to repeat the entire experiment. This can only be taken to mean that more alkali might be added and the true end point determined more carefully. Then it

would follow that the net amount of base for neutralization be recalculated accordingly.

Regarding the possible presence of causticity (hydroxide), Lewis offered the following observation (14):

A person accustomed a little to this operation [*the titration*], will be able to determine by it, not only the quantity of pure alcali, but whether the alcali has any injurious causticity. Plain alcalies effervesce with the acid, from almost the first drop, till the saturation is completed: those which are fully caustic, make no effervescence at all; and those which are caustic in part, do not begin to effervesce, till a considerable quantity of the acid has been added, more or less according to the degree of causticity.

Within this original report Lewis included a section entitled "Hydrostatic assay of the strength of lyes, and of the quantity of saline matter contained in Potashes (15)." In order to determine accurately the density of lyes he devised an improved hydrometer, which gave a direct reading of the weight of potash in a pound of lye. He also recognized the importance of temperature in 'hydrostatic assays' but, more importantly, the limitations of the method (16):

To determine whether this salt be the pure alcali which it ought to be, recourse must be had to operations of a different kind, such as that described in the following article.

Here follows Lewis's titrimetric method.

Lewis's acid-base titration contains features and principles, for which there are apparently no precedents. His account of what now may seem to be a simple titration must stand as one of original invention marking a very important and well authenticated advance in early titrimetry. At no time did Lewis allow anything less than perfectly measured volume; also, he often resorted to measures of weight in order to reinforce perfectly acceptable volumetric measurement. However this aspect alone hardly stands as one of invention, and neither does his use of colored indicators; but taken together and with his rejection of the cessation of liberated carbon dioxide as a reliable end point, we see Lewis's work as an achievement of significance. The suggestion to use hydrochloric acid and not sulphuric is interesting inasmuch as Lewis anticipated a reaction between the latter and soluble marine salt (NaCl) in the potash solution; but the validity of this point is unimportant when compared with the meticulous procedure he used in preparing the acid solution and the potassium carbonate for the standardization process (17):

Take a quantity of spirit of salt [*hydrochloric acid*], and dilute it with ten or twelve times its measure of water; fill with this mixture a vial that will hold somewhat more than four ounces of water: the vial which I find most commodious is nearly of the shape of an egg, with a broad foot that it may stand sure, a funnel-shaped mouth for the convenience of pouring the liquor into it, and a kind of lip or channel at one side of the mouth, that the liquor may be poured or dropt out without danger of any drops running down on the outside. Hook the vial, by means of a piece of brass wire tied round its neck, to one of the scales of a balance; and counterpoise it, while filled with the acid liquor, by a weight in the opposite scale.

Although this does not describe a modern buret or measuring cylinder, the results would probably have been very accurate, provided the balance was sufficiently sensitive. It was this dilute acid solution which Lewis would titrate against one eighth of an ounce of prepared pure potash. This was made from thoroughly dried salt of tartar (potash, presumably from recrystallized material) followed by fusion, and then taken up in "an ounce or two of water." His description of this standardizing titration follows (18):

Pour gradually some of the acid from the vial into the solution of salt of tartar, so long as it continues to raise a strong effervescence; then pour or drop in the acid very cautiously, and after every small addition, stir the mixture well with a glass cane, and examine it with the stained papers. So long as it turns the red side of the paper blue, more acid is wanted: if it turns the blue side red, the acid has been overdosed. That there may be means of remedying any accident of this kind, without being obliged to repeat the whole preceding part of the experiment, it will be proper to reserve a little of the alkaline solution in another vial: this is always to be added towards the end, and washed out of the vial with a little water.

When the liquor appears completely saturated, making no change in the colour of the paper, hook the vial on the scale again, to see how much it wants of its first weight: this deficiency will be the quantity of the acid liquor consumed in saturating the two drams of alkaline salt. So much as this quantity wants of four ounces, so much, in proportion, of common water must be added to all the rest of the acid mixture. If for instance the quantity consumed in the saturation is three ounces, then, for every three ounces, or three pounds, or thirty pounds, of the acid liquor, must be added one ounce, or one pound, or ten pounds of water; the acid will thus be so adjusted, that four ounces of it will saturate two drams [*one eighth of an ounce, assuming Avoirdupois*] of alcali: it will be expedient to make another trial, to see whether it is exactly of this strength.

Put more simply, Lewis had shown that four ounces of his standard hydrochloric acid of unknown strength would always saturate one eighth of an ounce of pure potassium carbonate; in other words, he had standardized the acid against pure potassium carbonate. This was a unique feature and a significant practical achievement of that time, for in so doing, he demonstrated the way in which an absolute quantity could be determined. Madsen (19) has calculated the acid concentration expressed as HCl as 1.6 - 1.7% or 0.4 - 0.5 molar, but little useful historical value can be drawn from such a present day recalculation.

Speed and simplicity, characteristics of titrimetry, are illustrated in Lewis's developments. They may not have been essential requirements in his time, but no doubt became so, as the application of the procedure became useful in other industrial processes. In preparing and standardizing the acid within the range quoted above, Lewis arrived at a final calculation of marked simplicity; the weight of acid consumed, multiplied by four, indicated the quantity of pure alkaline salt contained in every pound of original sample. Within these six pages of reporting, Lewis had described all the essential practical features of what is now recognized as acidimetric titration. His report reflects an exceptional piece of analytical development. His rejection of approximate volume measurement in favor of weighing, the use of color to provide clear evidence of the completion of reaction, the creation of a standardized solution (albeit, not one based on molecular weight) all led to a method of determining absolute content, provided of course that his "pure" potassium carbonate was actually pure.

Lewis's titrimetric work was published in 1767 and, as already noted, it contained a definite, if indirect, reference to Francis Home's *Experiments on Bleaching* of 1756 (20). In determining the quality of American potashes by using a colored indicator, the standardization of the acid and the extreme accuracy in measuring and weighing, Lewis was significantly in advance of earlier methods particularly that suggested by Home in 1756. These improvements in titration taken together with his earlier analyses of Virginian Saltpetre (21), in which he emphasized the value of obtaining concordant analytical results and comparison against known standards, place Lewis's work of extreme importance in the development of early titrimetric analysis.

Oddly, in both pieces of work Lewis made no mention of moisture content in the original sample material, and there appeared to be some uncertainty about water

of crystallization. The latter was certainly not entirely understood at this time although Lewis had given some account of this in *Philosophical Commerce of Arts*, four years earlier. It is surprising therefore that occluded moisture in such commercial and impure products as saltpetre and potash had no consideration. Nevertheless, his awareness of what he believed to be absolute values of content must be noted as a major advance in the early stages of titrimetry.

His earlier work on Virginian Saltpetre involved the determination of the strength of nitric acid, but he saw this as merely balancing acid against alkali in terms of "saturation." However, his later experiments on potash show a distinct belief in true chemical content in absolute values; of course, we may now interpret this differently inasmuch as he was without the modern foundations of chemical formulae, equations, and molecular weights which we now see as essential in analysis.

It is surprising that there was so little recognition given to Lewis by several Scottish chemists in their later attempts to determine the alkali content in such materials as potash, ashes, kelp, and barilla. Fyfe, Jameson, and even Kirwan *et al.* were seemingly trying to reinvent the work already done by Lewis even though his innovative progress in titrimetry had been published by the Society of Arts. The explanation for this is not immediately obvious. Madsen commented (22):

It is strange that the analytical part of this treatise was not at all understood by Lewis's contemporaries, and that the treatise does not seem to have left any mark in the development of analysis.

The same enigma was described by Gibbs (23):

He [Lewis] was celebrated as a physician and occupied a secure place as the foremost British pharmaceutical writer of his day; his books were widely used, in particular by Cullen and Black at Edinburgh. Yet apart from a few scattered references to him in the literature of pharmacy, one can search the histories of the special sciences in vain for an indication of the extent of his work....Lewis was one of the best known and least known scientists of his period.

It is difficult to determine the audience reached by this early analytical work; Lewis's 1767 paper was published in London and copies were sent to the Colonies, but it is surprising that his analytical achievements were not better appreciated outside the interests of the Society of Arts (24).

This short account of Lewis's writings and analytical developments clearly shows his concern for the improvement of 'the arts.' His work on the analysis of

potashes and other researches on their manufacture were in the context of the promotion by the Society of Arts to import these materials from British Colonies rather than from uncertain European sources. In this work he did not aim solely at a theoretical understanding of chemical processes like that, for example, offered by Joseph Black (25). The latter saw chemical understanding of early bleaching of raw materials as a means of immediate help to bleachers, but in fact this was not borne out in practice for their rule-of-thumb empirical methods continued. Lewis attempted to show that chemical analysis could be used to improve 'the arts' by providing accurate means of determining quality and hence value and suitability to the user.

Lewis undoubtedly led the field in showing that practical chemistry through analysis could provide answers to industrial questions. His analytical work and hydrometry researches, alone, place him supreme for the period. His main texts, *Commercium Philosophico-Technicum* and *Chemical Works of Caspar Neumann* are full of answers to manufacturing problems and possibly set a pattern for future authors in this field. The researches on platinum were extensive and represented a program of work that proved the metallic status of platinum and its chemical detection as an adulterant of gold (26). As a quantitative chemical experimentalist his work on American potashes clearly exemplified the future alliance between science and industry.

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ABOUT THE AUTHOR

After retiring from the chemical industry in 1989, the author pursued an interest in history of science and gained an M.Sc., at the Oliver Lodge Laboratory of the University of Liverpool. His studies of early industrial analytical chemistry under the supervision of Professor W. H. Brock resulted in his earning a doctorate from the University of Leicester in 1999. The above article is a condensed and modified extract from his thesis.

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BOOK REVIEWS

The Jewish Alchemists: A History and Source Book. Raphael Patai Princeton University Press, Princeton, NJ, 1994, Paperback, 617 pp. \$24.95

In the standard history of chemistry textbooks authors usually write about the Chinese, Indian, Egyptian, Greek, Arabic, and European alchemies, though it is sometimes noted that alchemy had probably been present all over the world in some periods of history. So why should the Jews not have performed this mysterious practice? Alchemy seems to pertain to geographical areas or civilizations, less so to religions, as it was practiced by masters belonging to very different denominations. While Taoism, Hinduism, the Muslim church, and Christianity all occupied relatively well-defined geographical areas, Jews for the most part of their histories lived in small communities dispersed in territories dominated by other civilizations and religions. Perhaps this is the reason that the subject of Jewish alchemy has not been examined before. Another reason may be related to the Jewish tradition. Because alchemy in modern times has been considered not only unscientific but also suspicious, mostly performed by frauds in the hope to benefit from greedy and naive lords, it might have appeared better for the historians of Jews to overlook their being involved in such a sin. Hence, until now, historians of Jews regarded alchemy as an entirely insignificant aspect of their subject.

It is to Raphael Patai's credit that he showed how false these opinions are. Patai, a renowned scholar in the field of Judaic studies, arrived at his subject from Jewish studies, not from the history of chemistry. In this latter subject his brother, the late Saul Patai, organic chemist, editor of the book series, *The Chemistry*

of Functional Groups, probably was helpful; for Raphael dedicated his book to Saul. The Hungarian-born Raphael Patai divided his lifetime between Hungary, Israel, and the United States. His amazing knowledge of languages (Hungarian, Hebrew, English, German, French, Arabic, Persian, and Aramaic) made it possible for him to study the abundant, sometimes exotic sources he needed for doing this research.

The book consists of forty chapters divided into ten sections, arranged in chronological order. Each section covers a period, from the early biblical times up to the 19th century (when alchemy was still alive in North Africa), while the chapters are centered on some authors and/or sources. In this way, Patai leads the reader through all the existing sources possibly (but not always with certainty) related to Jews.

Indeed, what the reader can follow is not a continuous Jewish tradition, but rather the discontinuous line of some individual authors or some masters who were perhaps or surely Jewish. Patai identified them through the fragmented, printed and nonprinted sources hidden in archives and libraries in all relevant countries. Besides the well-known names, such as Maria the Jewess, he also found entirely forgotten Jewish alchemists that exerted important influence on Hellenist alchemy. He also ascertained that famous alchemists, such as Avicenna, had Jewish teachers and that some alchemists (*e.g.*, Artephius), though known otherwise, were in fact Jewish. Patai shed light on the shadowy origin of many Ancient and Middle Ages alchemist authors and also on the origins of such celebrities as the Comte de Saint-Germain in the 18th century. In these biographical sketches, the most fantastic stories are unraveled with the atmosphere of mystery and historic dimness when we learn, for instance, that the works attributed to the famous alchemist Raymond Lull were

actually written by several authors, among them some Jews. The identification of these authors might have been quite a complicated task.

The identification of texts written by Jewish authors required the same painstaking work of a philologist as that of the authors. Patai analyzed old manuscripts by linguistic and historical means to find texts, paragraphs, or just some words referring to Jewish connections. By this he established that, particularly in the Hellenistic period, the Hebrew words or just the Hebrew characters occurring in the non-Jewish alchemical texts gave the impression of authority to the readers of these texts, because at that time Jews were considered the most authentic experts in the field. Later, in gradually decreasing degree, the habit of using Hebrew expressions survived among the Arabic writers as well.

Often it was difficult to decide who the writer of a particular text was, and Patai explained the arguments for his stands with exciting details in an entertaining style. For instance, some of the texts were attributed to Maimonides, a most revered Jewish scholar in the 12th century, though he was an opponent of alchemy. Yet, some authors borrowed his authority to enhance their own credibility so much so that a pseudo-Maimonides emerged who authored alchemical texts in the name of Maimonides.

Besides the exciting, sometimes very amusing stories about the authors and texts, the greatest value of the book is that it contains translations of long and relevant parts from original manuscripts and rare books written in obscure languages. The texts contain detailed descriptions of apparatus, recipes, and materials just as alchemical texts usually do. In this way Patai significantly enriched our general knowledge about alchemy.

In addition, Patai attempted to follow the changes that occurred in the social roles of his heroes through the centuries from the point of almost invisible positions they occupied in the closed and insulated Jewish communities to recognized positions of informal membership in aristocratic circles. Unfortunately, this sociological line is not very well worked out. The author points out that the South European Sephardim community was the one that was in fact active in alchemy. When the East European Ashkenazim took over the dominant role in Jewry from the Sephardim in the 18th century, the interest of Jews in alchemy diminished. The connections with the non-Jewish community, the Jewish alchemists' social status, and the problem of secrecy, however, have not been thoroughly detailed.

A highly important question is whether the alchemy of the Jews differed cognitively from that of other groups. In this context Patai stressed that Jewish alchemy was basically practical, with less emphasis on theoretical interpretations. Indeed, with the help of Patai's book, in the future the experts of the history of alchemy may list the recipes, apparatus, and materials that were used specifically by Jews or subsequently taken over by non-Jews. Giving more stress to practical orientation, Patai proved that, in the Jewish tradition, alchemy meant a search for miraculous medications and cures, the "elixir of life," rather than endless attempts to transmute base metals into gold. Still, the texts he translated contain descriptions of many experiments intended to produce precious metals and stones from more common materials.

On the other hand, the theoretical, metaphysical background of Jewish alchemy appears quite similar to that of the Greeks, Arabs, and Europeans: all kinds of materials were supposed to be manifestations of the same ultimate, basic essence, which went through a long process of ripening inside the earth. Therefore, the alchemist should find either this basic essence, the philosopher's stone, which makes it possible to transmute any form into another form (in Aristotelian terms). Alternatively, relying on the basic identity of the matters, he should achieve the transmutation from one form to another without actually producing the basic essence. This latter method helps to ripen the materials more quickly than they would do solely by the forces of nature.

This theoretical approach was also absorbed by the kabbalah, through its most important book, the *Zohar* written by Moses de Leon in the 12th century, though the term alchemy was not mentioned in the *Zohar* nor in the Talmudic literature of that time. As the power of kabbalah rose in the 14th century, when alchemy had already reached a highly developed level, the Jewish authors took over its tenets and assimilated them into their thoughts. The kabbalah derived the possibility of transmutation from a spiritual not a material realm, but it also looked for an ultimate essence in the same way other alchemists did. In this tradition, the highly practical alchemy changed to a mysterious and mystic activity, a feature that became gradually more emphasized in the Middle ages. In the atmosphere of the Renaissance, the kabbalistic tradition and the Christian alchemists' reference to the Jewish literature contributed to the magical character of non-Jewish alchemy as well.

With the approach of the 17th century, the reader misses the comparison of Jewish alchemy with the gradually emerging chemistry. The strength of this book consists more in providing inventory-like descriptions of Jewish alchemists and their works rather than in analyzing them in terms of the history of philosophy, alchemy, and chemistry.

Patai did not intend to convince his readers of the existence of a long, continuous Jewish tradition with a series of great masters and many followers within the

history of alchemy. His book proves that some Jews can be found among alchemists and that alchemy, an important philosophy and practice in the history of mankind, had not been alien to Jewish religion and thinking. He found only Jewish alchemists and not Jewish alchemy as a tradition clearly distinguishable from the other traditions. *Dr. Gabor Pallo, The Institute for Philosophical Research of the Hungarian Academy of Sciences, Budapest V. Szemere u. 10, HUNGARY; e-mail: gabor.pallo@ella.hu*

The Business of Alchemy. Science and Culture in the Holy Roman Empire. Pamela H. Smith, Princeton University Press, Princeton, NJ, 1997, xii + 368 pp, paperback, ISBN 0-691-01599-6, \$ 29.95.

“The author to his good friend / which is very depressed by the distress and war. In order to do something against impoverishment and devastation ... and because I am sorry about your misery ... I have not any other means / but to tell you a good proposal and advice.” The long citation that introduces this review comes from a German book that appeared in 1684 as a translation of the French original. The title of this small tract is promising: *“Neue und sehr nutzbare Haushaltungs-Kunst ... Wie man mit 500 Gulden / ... jährlich 4500 Gulden / ... Mit Ehr und gutem Namen gewinnen kann...”*. This anonymous work documents the situation in Europe after the Thirty Years’ War, when the continent had been devastated by roving armies. As late as 1684 there was still a need to help improve the state of things. This German book appeared two years after the death of the protagonist in Smith’s book, his life having passed through this dramatic war and the subsequent decades. Johann Joachim Becher (1635 - 1682) became the child of a turbulent epoch that shattered the entire continent and brought about deep changes in society and its organization, in morals, economy, and also in science.

The book by Pamela Smith is impressive. It can be compared to a monumental canvas of European history in which the prominent figures are, however, not lost in the complicated background of the Holy Roman Empire split into many small states with different religious views, yet often joined by their poverty as well as by other links. Quite on the contrary, the reader is given a vivid picture of life at various royal courts, vicariously experiencing the feeling of sitting at the rich table of the West India Company enjoying exotic foods, and following in detail the struggle over the secret of a “cold fire,” phosphorus. There is less alchemy in this book than its title would suggest; it is rather an original portrait of an extraordinary man. Most readers with a background in chemistry know of Becher as a scientist whose “*terra pinguis*” paved the way to the phlogiston theory. This contribution of Becher is perhaps the only topic missing in the book. Yet, the absence of the roots of phlogiston theory does not change the fact that this book provides a thoughtful analysis of an important person and his world. Rich notes in the text, a long list of literature citations, and the list of Becher’s works make it an excellent contribution to the history of economics, education, and science; a book to which not much can be added. It ranks among the works by Dobbs on Newton, by Newmann on Starkey, or by Principe on Boyle.

Pamela Smith follows Becher’s life from childhood through its crucial stages at the noble courts in

Mainz, Munich, and Vienna. In other chapters she describes in detail his negotiations with the West India Company in an attempt to acquire a colony in the New World for his noble employers, and his plans of how to exploit this land. Eventually we also learn of Becher's alchemical undertakings. Individual stages of Becher's life are always presented within the broad framework of the political and economical situation. This approach makes it possible to understand more clearly the many activities of this undoubtedly extraordinary personality. Becher gained recognition in both positive, and if not negative, then at least less-positive respects. His broad interests ranged from attempts to invent perpetual motion and an artificial universal language, to problems of education and especially of the economy. At the same time he was "Hofmedicus" and a defender of alchemy. Becher clearly understood the deep social changes after the Thirty Years' War and tried to reflect this new state of things in his activities. Weakened traditional guild systems, the increasing role of towns, the shift of commerce from the country to ports as an effect of war: all of these and much more led Becher to the conviction that noble rulers must also be drawn into commercial society. This conclusion seemed all the more obvious to Becher after the Thirty Years' War with the appearance of a new aristocracy endowed with restricted rights as compared with the old traditional nobility, yet striving to achieve these rights and become influential.

It is interesting to follow Becher's economic views, according to which the mercantile cycle is a natural one similar to cyclic changes in nature. In his opinion money is the ultimate source of a ruler's authority, and therefore a ruler should become an entrepreneur if he wants to be successful, whereas wars and magnificent palaces would only result in loss and debts. According to Becher, only manufacturing and business can produce cash. He observed this evolution during his travel in Europe. In the second half of the 17th century, traditionally rich Spain was on the decline while Holland was prospering. As a result of his comparison of the poor royal court of Hanau and the wealthy merchants in Holland, Becher tried to graft the mercantile world onto the old aristocratic one.

This is the "positive" Becher, a man of a rather doubtful education who nevertheless saw many problems quite clearly. On the other hand, there was the Becher as a problematic personality, who fabricated various "stories" about himself to make a better impression, and who frequently came into conflict with his surroundings. As a result he moved, or was forced to

move, from one court to another. Some of these conflicts were the outcome of Becher's ambitious nature, yet some were unavoidable as conflicts between the old traditional world of guilds and landed nobility on the one hand and the new world of commerce on the other. This transition from agricultural to industrial society could hardly pass without controversy.

Although Becher devoted most of his life to economy, both theoretical in the form of books and tracts, and practical as a founder of manufacturing, his undertakings often failed. It is true that he saw clearly what was needed to help Europe improve its economy: the trend toward manufacturing, large-scale production, and innovation in all areas. There were, however, many Utopian ideas in his approach, as discussed in a recent publication [H. Breger, "Sozialutopische Tendenzen und (Al)chemie des 17. Jahrhunderts: Johann Joachim Becher und Johann Rudolph Glauber," *Aufklärung und Esoterik*, Hamburg, 1999]. This approach was part of what led to the repeated failure of Becher's projects.

On the other hand Becher reflected the changes occurring in society as well as at courts, and therefore his ambition was to function as an intermediary whether between craftsmen and his employers, or between alchemists and his noble sponsors. This activity stemmed from the weakening of guilds and from the changing position of alchemists. Especially in the 16th century, which witnessed the last flourish of European alchemy (as apparent from the number of printed books), alchemists constituted a typical group in royal courts. It was "fashionable" to employ court artists, musicians, astrologers, and alchemists. These groups mostly moved freely between various courts, with the exception of alchemists. Because they were expected to yield attractive results (gold being the most important one), the freedom of alchemists was often restricted; and because of the limitations of alchemy versus the hopes for untold riches, their life was often in danger. Sometimes they tried to reduce this danger by drawing up contracts with their noble employers [V. Karpenko, "Bohemian Nobility and Alchemy in the Second Half of the Sixteenth Century: Wilhelm of Rosenberg and Two Alchemists," *Cauda Pavonis*, 1996, 15 (2), 14]. Wealthy nobles in Central Europe often employed ten alchemists at once. As early as the 16th century, alchemists often produced something of value even when their attempts at transmutation of metals failed. These were products of practical importance such as paints, dyes, glues, but also various "aquam vitae," strong alcoholic beverages often prescribed for medical purposes. Less typical was a

book, “*The Art of Cooking*” written by Bohemian alchemist Bavor Rodovsky of Hustiran (? 1526 - ca. 1592/1600)].

Becher fully understood that to succeed he must regularly be in close contact with the nobility, to see and to be seen at court, and to cultivate personal connections (“Kundschaften”). One of the attractive activities he could propose was alchemy. Becher acted as a mediator between the alchemical world and the noble court, and for this he collected and published a large number of recipes in his famous “*Chymischer Glück-Hafen*” (1682). By that time, however, doubts concerning alchemical processes were gradually deepening, and the danger of extermination had not disappeared completely (as documented by the execution of Baron Krohnemann in 1686). Perhaps it was the increasing doubt about metallic transmutation, especially in the 17th century, that prompted production of coins and medals cast from the alleged alchemical noble metal. Coins and medals minted from supposedly transmuted metal represent something more than formless pieces of a precious metal. They could be looked upon as the first step to the further production of such metal, as expected by a noble Maecenas. At worst, if they represented only an isolated success, they could decorate the cabinets of curiosities popular at that time.

Becher seized upon this trend; as described in this book, he produced a medal from alchemical silver in July, 1675. His inspiration for this undertaking could perhaps be traced back to the Electors of Mainz, in whose service Becher began his career. Silver thalers dated 1630 struck by Anselm Casimir, Elector and Archbishop of Mainz, were said to be made from mercury. This conclusion was a result of misunderstanding, because the symbol ♀ on these coins is the sign of the mint master. As in more analogous cases with these signs, wish was here father to the thought. Seemingly more convincing Mainz ducats of 1658 from alleged alchemical gold [Becher mentioned them in his *Oedipus Chemicus*, Frankfurt, 1664, p 153] represent a complicated and confused situation. Smith writes (p 181 and footnote 2) that gold was made by transmutation before the Elector Johann Philipp von Schönborn (1605 - 1673, Elector and Archbishop from 1647). The same statement appears in other sources [K. Ch. Schmieder, *Geschichte der Alchemie*, Berlin, 1832, 402; H. Kopp, *Die Alchemie in älterer und neuerer Zeit*, Hildesheim, 1971, I, 141]. Yet other authors [P. J. Fabri, *Die hell-scheinende Sonne* ..., Nürnberg, 1705, unpaginated In-

troduction; *Die Edelgeborne Jungfer Alchymia*, Tübingen, 1730, 202; H. C. Bolton, *Am. J. Numismatics*, **1890**, 24, 83,] ascribe these coins to Georg Friedrich von Greiffenclau (1573 - 1629, Elector and Archbishop from 1626). The inscription GEORG FRID. on these ducats makes the second version more plausible. As in the preceding case, the symbol ♀ on these coins was explained as indicating that the gold from which they were minted was prepared alchemically. These coins are connected with the alchemist von Richthausen who performed a spectacular transmutation in the presence of the Habsburg emperor Ferdinand III in 1648. This story is mentioned by Smith, and there was yet another transmutation for this emperor by the alchemist J. P. Hofmann in Nürnberg one year earlier. There were also numerous coins of the Swedish king Gustavus Adolphus struck between 1632 and 1634 that were traditionally ascribed to alchemical activity. [The story of apothecary Strobelperger appeared in S. Reyher, *De nummis* ..., Kiel, 1692, 3 ff.]. Eventually Seyler's ducats struck allegedly by the emperor Leopold I appeared in the same year as Becher's medal. It should be noted that “Hoff Chymicus” Seyler was ennobled to von Reinburg, not von Reinberg as appears in Smith's book [details of his life in B. Koch, *Numismatische Zeitschrift*, **1990**, 101, 91 - 98]. Thus, there were ample examples to inspire Becher around this time. He could not but follow this trend and make himself visible in an analogous manner.

The production of coinage from alleged alchemical metal raised the crucial question as to whether one could become rich through alchemical transmutation. In this respect alchemy arrived at a crossroads that became especially apparent during Becher's life. We can distinguish three main positions as to what could be expected from alchemy at this time. The first one, represented by Becher, considered alchemy to be a “movable wealth:” knowledge of transmutation could be transferred from one place to another. Above all, Becher claimed that alchemy could provide the necessary means for the nobility to gain wealth. Robert Boyle, Becher's contemporary, although an ardent believer in alchemy, was more careful in answering this question. He represented the second viewpoint that metallic transmutation was possible, but not a practical means to wealth. [L. Principe, *The Aspiring Adept*, Princeton University Press, Princeton, NJ, 1998, 185]. There was also a third position represented in the anonymous tract cited at the beginning of this review. In its 47 pages we read detailed instructions of how to breed hens and become rich by

selling their eggs. The explanation for doing this is found on the last two pages, where it is stated: "... *and think, my dear friend, that never appeared better stone of alchemists than that from bellies of thine hens, and therefore, if you add pleasure to your work you can avoid danger brought by those alchemists who often put intentionally the inheritance of their fathers, and everything collected by their ancestors, into ovens and change it to smoke ...*". The anonymous author, as the title of his work suggests, laid stress on honorable activity; alchemists were discredited, not only by him, but by a growing number of his contemporaries.

There is a striking contrast between alchemist-dreamer Becher, on the one hand, and the unidentified pragmatic on the other. Yet they both shared the same intention: to improve the bad economy that plagued most of Europe after the Thirty Years' War. As the practical approach (not restricted to the production of eggs) gradually gained a firm basis, alchemy became an obscure activity. This process, which took another hundred years following Becher's death, is another story. Pamela Smith presents an excellent picture of a remarkable personality in the boiling pot of Europe in the second half of the 17th century, during the final stage of alchemy. V. Karpenko, Charles University, Prague, Czech Republic.

Communicating Chemistry: Textbooks and their Audiences, 1789-1939. A. Lundgren and B. Bensaude-Vincent, Ed., Science History Publications/USA, Watson Publishing International, Canton, MA, 2000. vii + 465 pp. \$56.

This assembly of chapters on chemistry texts is the outcome of a workshop held in Uppsala in 1996 and edited by two of the contributors, A. Lundgren and B. Bensaude-Vincent. In the 17 chapters chemistry is described as it evolved through texts of varying form and quality in 8 different countries, France being most prominently represented with six chapters. Germany, Sweden, and Britain each are allotted two chapters, while a single chapter each covers chemical texts in Spain, Hungary, Russia, and the United States. A unique chapter on the development of quantum chemistry involves texts written in the US and Britain. The title is somewhat misleading, inasmuch as the time span extends either to the mid-19th century or to the end of the 19th century, in all chapters except four. There is no apparent order of appearance of the chapters, neither according to nationality, chronology, nor alphabetically by author. An extensive introduction by J. H. Brooke is based on his con-

cluding remarks at the meeting. It is an invaluable summing up of many of the issues raised by individual authors. These include the definitions of texts, the motivation of textbook authors, the promulgation of pet theories and research accomplishments through texts, translations of texts and misunderstandings or misinformation that may result, and the various audiences to which texts are directed. Brooke observes that modern scientific textbooks had their beginning in about the mid-1800s; but a scholarly treatment of texts in their historical context has not been a popular branch of research.

It would be inappropriate to provide a detailed description of each of the contributor's analysis of textbook writing and its impact on chemists. The approaches vary considerably. I can hope only to cover some of the characteristics of these approaches by mentioning features in individual chapters (identified by the last name of the first author). The number of texts on which a chapter is based varies from over 200 (Lundgren, Orland) to a few (Nieto-Galan, Palló), to that of a single author (Blondel-Mégrelis, Brooks, Nye). Some authors have meticulously grouped and annotated the primary sources for easy reference (Bensaude-Vincent, Knight, Pigeard, Sanchez), whereas others have mixed the primary sources with secondary sources (Dolan). The texts

vary widely from being highly applied (Nieto-Galan, Dolan, Sanchez) to ones directed toward popular audiences, including women (Knight, Orland, Pigead). In several cases the direction of textbook writing for students was driven by the developing educational system (Belmar, Bensaude-Vincent, Knight, Lind, Sanchez). Single themes are the subject of some texts: dyeing (Nieto-Galan); water (Izquierdo); atomism (Kounelis); and quantum chemistry (Gavroglu). Some authors have interpreted the effect on textbook writing of such factors as politics (Palló) and a strong personality (Nye). Mendeleev's textbook writing facilitated his formulation of the periodic table (Knight), and Berzelius' strong influence on European chemistry far beyond his native Sweden came about through Wöhler's German translation and that subsequently into French, Dutch, Italian, and Spanish (Blondel-Mégrelis). His text was apparently never translated directly into French, although some attempts were undertaken. In the chapter by Lundgren, the reader is shown how Berzelius' era gradually gave way to second and third generations of Swed-

ish texts where the emphasis was on analytical and then organic chemistry. Finally, the separation of chemistry from physics is treated in two contrasting chapters. One by Lind covers this trend in Germany between 1780-1820; in the other (Gavroglu) the influence of early US and British texts on quantum chemistry was to render it as distinct from quantum mechanics.

The book is relatively free from awkward phrases and typographical errors, in spite of the fact that most of the chapters have been translated into English from the authors' native tongues. Missing words or phrases are nevertheless distracting (pp 17, 23, 171), as are obscure terms such as 'authorial' (p 276), 'legitimation' (p 280) or vague descriptions like "philosophical apparatus" (pp 145, 159) and "pedagogical marketplace" (p 146 ff). The reader will be deeply impressed with the huge number and wide variety of chemistry texts—defined in the broadest sense—belonging to our professional heritage. *Paul R. Jones, Department of Chemistry, University of Michigan.*

Four Centuries of Clinical Chemistry. Louis Rosenfeld, Gordon and Breach Science Publishers, Amsterdam, 1999. xvii + 562 pp, hardbound, ISBN 90-5699-645-2, \$149. (Available from AACC Press or directly from the publisher, PO Box 566, Williston, VT/USA 05495-0080).

This is a must for anyone remotely interested in clinical chemistry and well worth purchasing, even if costly. It is not merely the first book written on the history of clinical chemistry, but its contents are fascinating reading.

The book glows particularly in its coverage of the modern era. This is true to no small extent because of the work of several members of the AACC History Division, its predecessor committees and some of our re-

cently departed colleagues who wrote or stimulated the writing of many articles published in *Clinical Chemistry* since its beginnings in 1955. But the book contains much more of historical interest on such topics as the evolution of hypodermic syringes; origins of the chemical glassware industry in the US; commercial laboratories and World War I; and outstanding textbooks on clinical chemistry in the 19th century.

Clinical chemistry could become an entity only when medical and chemical sciences were allied. But four centuries ago this could only happen as medicine departed from its concepts of "humoral" pathology and philosophy and entered into laboratory post-mortem examination to learn the causes of disease, requiring study of "normal" human anatomy and physiology. Chemistry had to venture from alchemy's quest for the philosopher's stone to transmute metals into the testing

of biological materials while ignoring the “vitalists” who mixed in discouraging spiritual concepts about the creation of blood and issues.

In the first few chapters Rosenfeld reviews the fascinating evolution of clinical chemistry from its origins and development in the 17th and 18th centuries, and its solidification in the 19th century. He wisely does not separate the unfolding of the many scientific aspects of clinical chemistry into a matter of centuries, but deals with specific topics from their beginnings to their current status at the close of the 20th century. The last half of the book covers the actual birth and flowering of the profession: the specifics of laboratory transition from qualitative to quantitative chemical testing, the work of the giants in the field, the unfolding of new instrumentation and technology, and the growth of clinical chem-

istry via technical societies and publications. Nothing is mentioned without documentation cited with each chapter and in the bibliography at the book’s end.

This book pays homage to the science of clinical chemistry by a faithful, skillful, and lucid elaboration of its history. We readers emerge with a stronger backbone and a just cause of pride. It is a book addressed not only to clinical chemists but also to all those who are involved in medical technology, their teachers, and the few who love history whether of medicine or chemistry. *Samuel Meites, Department of Laboratory Medicine, Children’s Hospital, 700 Children’s Drive, Columbus, OH/USA 43205.* [NOTE FROM EDITOR: An earlier version of this review appeared in *History Newsletter*, AACC Division on the History of Clinical Chemistry, Volume 9, Number 1, 200.]

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