BULLETIN FOR THE HISTORY OF CHEMISTRY

Division of the History of Chemistry of the American Chemical Society

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VOLUME 32, CONTENTS

NUMBER 1

FIFTY YEARS OF THE DEXTER AND EDELSTEIN AWARDS *1*
WRITING THE HISTORY OF MODERN CHEMISTRY *2*
Peter J. T. Morris, Science Museum, London

A PERSONAL HISTORIOGRAPHY OF THE CHEMICAL INDUSTRY SINCE 1956 *10*
Peter J. T. Morris, Science Museum, London

SCIENTIFIC BIOGRAPHY IN THE HISTORY OF CHEMISTRY: THE ROLE OF DEXTER AND EDELSTEIN AWARD WINNERS IN THE LAST FIFTY YEARS *21*
Mary Jo Nye, Oregon State University 1

UNINTENDED TECHNOLOGY TRANSFER: ACETYLENE CHEMISTRY IN THE UNITED STATES *27*
Anthony S. Travis, Edelstein Center, Hebrew University

MAUVE AND ITS ANNIVERSARIES *35*
Anthony S. Travis, Edelstein Center, Hebrew University of Jerusalem/Leo Baeck Institute London

THE HISTORY OF OZONE. V. FORMATION OF OZONE FROM OXYGEN AT HIGH TEMPERATURES *45*
Mordecai B. Rubin, Technion - Israel Institute of Technology, Haifa, Israel

BOOK REVIEWS *57*

NUMBER 2

Welcome to this Issue of the Bulletin *65*
LOOKING BACK: EIGHTY-FIVE YEARS OF CHEMISTS AND THEIR HISTORY *66*
James J. Bohning, Lehigh University

HIST MISSION STATEMENT (2007) *81*
COMMUNICATING THE HISTORY OF CHEMISTRY *82*
Paul R. Jones, University of Michigan

INFLUENCES OF HIST AND THE HISTORY OF CHEMISTRY ON THE COURSE OF CHEMISTRY, EXAMPLES OF SYNERGY *87*
Jeffrey I. Seeman, SaddlePoint Frontiers

HIST 2007 STATE-OF-THE-UNION 97
LOOKING AHEAD: KEEPING HISTORY OF CHEMISTRY RELEVANT TO THE FUTURE OF CHEMISTRY *98*
Carmen J. Giunta, Le Moyne College

LETTER TO THE EDITOR *104*
“MOLECULAR” VERSUS “COLLOIDAL”: CONTROVERSIES IN BIOLOGY AND BIOCHEMISTRY, 1900–1940 *105*
Ute Deichmann, Leo Baeck Institute London, and Institute of Genetics, University of Cologne, Germany

THEODOR VON GROTTHUSS (1785-1822) – A TRAIL BLAZER *119*
Bruno Jaselskis, Carl E. Moore, and Alfred von Smolinsk, Loyola University of Chicago

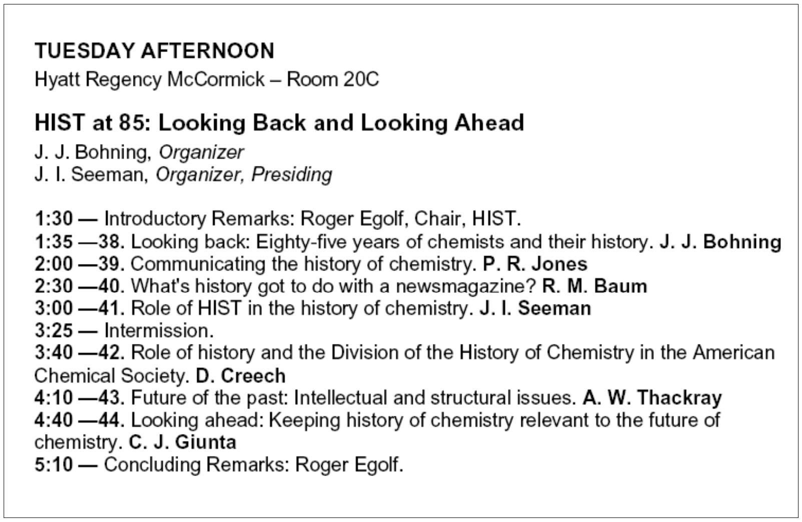
CLEAR WATERS AND A GREEN GAS: A HISTORY OF CHLORINE AS A SWIMMING POOL SANITIZER IN THE UNITED STATES *129*
Kevin Olsen, Montclair State University

BOOK REVIEWS *141*

The Cover…HIST Logo, designed by Vera V. Mainz

Welcome to this Issue of the Bulletin

In 2005, Jim Bohning proposed the idea that we celebrate HIST's 85th birthday with a symposium at one of the upcoming ACS national meetings. Jim, of course, had served as HIST Chair (1986) and has been HIST Archivist and Historian for many years. Jim's other idea was to ask me to join him in co-organizing the symposium. The symposium was held at the 223rd ACS National Meeting on Tuesday, March 27, 2007 in Chicago. The agenda is shown in the box below.



TUESDAY AFTERNOON
Hyatt Regency McCormick – Room 20C

HIST at 85: Looking Back and Looking Ahead
J. J. Bohning, *Organizer*
J. I. Seeman, *Organizer, Presiding*

1:30 — Introductory Remarks: Roger Egolf, Chair, HIST.
1:35 —38. Looking back: Eighty-five years of chemists and their history. **J. J. Bohning**
2:00 —39. Communicating the history of chemistry. **P. R. Jones**
2:30 —40. What's history got to do with a newsmagazine? **R. M. Baum**
3:00 —41. Role of HIST in the history of chemistry. **J. I. Seeman**
3:25 — Intermission.
3:40 —42. Role of history and the Division of the History of Chemistry in the American Chemical Society. **D. Creech**
4:10 —43. Future of the past: Intellectual and structural issues. **A. W. Thackray**
4:40 —44. Looking ahead: Keeping history of chemistry relevant to the future of chemistry. **C. J. Giunta**
5:10 — Concluding Remarks: Roger Egolf.

As Jim points out in his papers (1, 2), it is fitting that HIST's 85th birthday was celebrated in Chicago. Edgar Fahs Smith and Charles A. Browne attended the 60th ACS National Meeting in Chicago in 1920. Their discussions under a tree on the campus of Northwestern University led to the first official meeting of HIST as a section in Birmingham in 1922. Divisional status occurred in1927.

It was also our hope that there would be a permanent record of our symposium. In fact, an audio copy of the entire session has been placed in the HIST archives at the Chemical Heritage Foundation. However, we wished to have as much of the symposium made easily available, both to our current members and for archival purposes. All of the speakers were willing to provide manuscripts based on their presentation; four of them were able to do so.

Each of the papers followed the *Bulletin*'s standard peer review process. As Guest Editor, I chose the reviewers and facilitated the review of all the articles save my own which was handled by Paul Jones, the *Bulletin*'s Editor in Chief.

I shall not provide any summary of the articles, which speak for themselves. But I shall point out three matters in particular. First, I am extremely proud of HIST's mission statement that appears on page 81 and has been on HIST's website homepage for over a year. It is HIST leadership's vision for the future. Second, I am extraordinarily enthusiastic about Jim Bohning's contribution. For some time, Jim has been working on a series of papers describing the history of HIST. This is the first of that series. And third, five of the participants in the symposium were members of the HIST Executive Committee during the development and actualization of this symposium. It was my pleasure and honor to be one of them.

With all best wishes,

Jeff Seeman, Richmond, Virginia
August 2, 2007

1. J. J. Bohning, "Looking Back: Eighty-Five Years of Chemists and Their History " *Bull. Hist. Chem.,* **2007**, 32, 65-
2. J. J. Bohning, "The History of HIST. II. The Probationary Years," in preparation.

This issue of the *Bulletin* is dedicated to the founders of the Division of the History of Chemistry of the American Chemical Society, Edgar Fahs Smith and Charles A. Browne.

LOOKING BACK: EIGHTY-FIVE YEARS OF CHEMISTS AND THEIR HISTORY (1, 2)

James J. Bohning, Lehigh University

Introduction

It is duly and dually fitting that we are here in Chicago to celebrate the eighty-fifth birthday of the Division of the History of Chemistry (HIST) of the American Chemical Society (ACS). First of all, we are actually a year late in doing so, but then we are in good company because the great Chicago World's Fair designed to celebrate the first voyage of Columbus to the new world was also a year late, opening to the public for the year 1893 (3, 4). But unlike the Great White Way on the Chicago Lake front that was mostly destroyed by fires within a year of its closing, HIST has endured for 85 years, not without its ups and downs, but nevertheless a viable and venerable institution of chemists, by chemists, and for chemists. Secondly, HIST's origin is intimately connected to Northwestern University and an ACS meeting on its campus in the Chicago suburb of Evanston in 1920.

Chemists have had a long standing interest in their history. One of the earliest texts appeared roughly fifty years after Priestley and Lavoisier ushered in the beginnings of modern chemistry, when Thomas Thomson published his two-volume set in 1830 (5). As chemistry found its way as a serious discipline in the late nineteenth century, many teachers of introductory chemistry courses saw the history of chemistry as a way to humanize the course and sustain student interest with anecdotal stories that were great fun but often irrelevant if not erroneous.

When Forris Jewett Moore published his little book on chemical history in 1918, it was the second American text on the subject (6, 7) and it showed him to be "widely read, witty and lucid (8)." His introduction is an eloquent rationale for studying the history of chemistry that is worthy of reading even today. He concluded by saying (7):

> As we study how man's knowledge of nature has broadened and deepened with the years, we acquire a better understanding of the trend of thought in our own times, and of the exact bearing of each new discovery upon the old but ever-recurring problems of the science.

By the early 1920s formal courses in the history of chemistry were appearing in various curricula, either as an elective or a requirement (9). Later, many advocates felt, as Moore did, that a knowledge of the history of chemistry was in part what distinguished a *trained* chemist from an *educated* chemist (10) and that the history of chemistry could also be used to develop an appreciation of chemistry in the nonchemist as well (11). At the same time, the Belgian George Sarton, who had founded the journal *Isis*, was establishing the broader topic of the history of science as a formal field of study in the United States (12).

Northwestern University (1920)

Thoughts of a formal discipline and the educational value of the history of chemistry were the furthest from the minds of Edgar Fahs Smith and Charles A. Browne when they came to Chicago in 1920 for the 60th national meeting of the ACS. There were a number of enticements to attend this meeting, which began on Monday, September 6. While it was called a "Reconstruction Meeting" and labeled with the slogan "Increased Production Through Chemistry," the advanced publicity seemed to focus more on the entertainment committee and its "unique and delightful program."

Under the watchful eye of the honorary chairman, Julius Stieglitz, the organizing committee promised chemists that social features "punctuated the program everywhere." There was a "restful nook known as the [Chicago] Chemists' Club, where soft lights glow[ed] through alabaster lamps and deep-cushioned chairs invite[d] reactions of repose. At all times delegates [were] welcome to the portals of this retreat of the Knights of the Retort." Arrangements were also made for the women guests, as special buses would be available to transport them to Marshall Field & Company on Tuesday and Sears, Roebuck & Company on Wednesday. Not all women were there as guests, for the Chicago Section hosted a special dinner for the "professional women in attendance," something which had never been done before.

After opening ceremonies in the Gold Room of the Congress Hotel, special trains would be the "magic carpets on which members would be borne…to Northwestern University" in Evanston. The committee offered assurances that all speeches would be finished by 4:30 p.m. to "make way" for a "combined men and women's entertainment and reception, garden fete, and beach party." In the evening there would be "more recreation in the [Patten] gym in which various lighter qualities of chemistry would bubble to the top. All these diversions were offered as a substitute for that indoor sport traditionally known as a smoker. All the world of the outdoors is open to the devotees of Lady Nicotine as well as the companionship of the daughters of Eve (13)."

This was the setting for the first meeting of HIST. Charles A. Browne, age 50, the chief chemist in charge of the New York Sugar Trade Laboratory and Chairman of the ACS Division of Sugar Chemistry (14), was told by ACS Secretary Charles L. Parsons that Edgar Fahs Smith was anxious to meet him. It must have been interesting for Browne to receive such a message, for he had learned his first chemistry from a Smith textbook while a student at Williams College. During the Tuesday afternoon session held in the Patten Gymnasium, Smith and Browne sat listening to H. P. Talbot discuss "The Relation of Educational Institutions to the Industries," but both men were eager to share their common love—the history of chemistry. Browne contends that the afternoon was very hot, but the Patten Gymnasium was supposedly "wonderfully lighted and well-ventilated."

Nevertheless, somewhere after W. A. Patrick, the second speaker, started talking about "Some Uses of Silica Gels," Smith and Browne decided to abandon their colleagues in the gymnasium and retired to "a shady seat on the lake front," where they spent over an hour in conversation. Smith, at age 66, had just retired as Provost of the University of Pennsylvania and intended to devote his remaining years to continuing his historical research (15). He had heard of Browne's collection of autographed letters, prints, and books and wanted to find out more about this younger chemist and his preservation of historical memorabilia. Most of the time together, however, was focused on Joseph Priestley and included Priestley's bookplate, the Priestley house in Northumberland, Pennsylvania, and Priestley artifacts (16).

Edgar Fahs Smith

Before the two returned to the gymnasium, they discussed the possible formation of a Section of Historical Chemistry (17). Smith later said that he considered this the very first meeting of HIST, with two members sitting under a shade tree on the shores of Lake Michigan (18). Even after they returned to the gymnasium, Browne and Smith continued their discussions, reflecting on their student days at Göttingen among other matters, and an intense friendship had begun.

Discussions about "Cranks"

Three weeks after their Evanston meeting, Smith wrote his first letter to Browne on September 27 (19):

> I was happy in meeting you. I learned so much from you that it gave me a great deal of food for thought. I found on my desk a letter from a gentleman in

> Chicago who says there is a movement on foot to form a Section of the American Association for the Advancement of Science (AAAS) to be known as the Section on the History of Science. He told me to write to a certain gentlemen and lay before him the advantages I thought would come to those who would go into such a section.
>
> I really don't know what to do. It seemed to me, after conversing with you, that probably without forming a Section of the American Chemical Society that we, that is those interested in the history of chemistry, might make a point of meeting at some hour convenient to all whenever we attend the meetings of the Society. I wish you would let me know your thoughts on the matter.

Browne's response, dated the next day, reiterated some of the comments he shared with Smith in Illinois (19):

> In view of the interest in the History of Science, it seems to me that a Section of the AAAS to be known as the Section of the History of Science might have a promising future. Among the members of the American Chemical Society there are many who are interested in the historical side of chemistry, but who like myself, are at present so bound up with other sections, or divisions, that they are not in an immediate position to renounce their allegiance to these. In fact, historical chemistry is so directly related to all these sections and divisions, that it is not independent, but a part of these, so your plan of not having a separate section but a sort of informal gathering or symposium which will not conflict with other meetings has much to recommend it.

Smith's concern about a History of Science Section of the AAAS was tempered by his perception that a history of chemistry group should operate only on an informal basis. The first suggestion for the AAAS to form a history of science group came from Frederick E. Brasch in 1915 (20). By the time of the Chicago ACS meeting in September, 1920, *Isis* had resumed publication, the American Historical Society had conducted two history of science sessions, and an organizing committee headed by Brasch and including George Sarton was preparing for the first meeting of Section L in Chicago in December, 1920 (21). Thus both HIST and Section L were organized in the same city in the same year (22).

Section L grappled with the same problem plaguing Browne and Smith: namely, how to conduct sessions in the history of mathematics, for example, when there already was an AAAS Section of Mathematics. They resolved the issue by having the more technical history sessions in the parent group (*i.e.* mathematics) and the more general papers of broader interest in Section L (22). In the years to come HIST would act similarly, often holding joint sessions with other ACS technical divisions to reflect that specific technical interest.

Section L was not formally recognized by the AAAS Council until the Toronto meeting in December, 1921. Brasch, who was now secretary of the section, noted that the history of science movement was growing steadily in the United States and that many educators were now recognizing its place in science and engineering curricula (23):

> If we are to enter a new epoch of science teaching, and give more emphasis to the humanistic element in our sciences, it is evidently time now to consider the matter. Science, that which we love to call pure science, has been too long dominated by the ulterior motive of materialism.

It is doubtful that this attitude was part of the thinking of Browne and Smith, but they were generally on target with what was happening in the broader history of science community. During the fall of 1920, Smith and Browne corresponded a number of times, exchanging ideas about Priestley as well as the acquisition of eighteenth and nineteenth century chemistry books. Smith encouraged Browne to "write up the alchemical period in America" after Browne expressed surprise when his study of early records showed "the extent to which alchemy was pursued in early American colonies and even down to the middle of the last century (24)."

In December 1920, Smith was elected President of the American Chemical Society for 1921 (25), and his busy schedule prevented him from visiting Browne in New York to see the "treasures" which Browne admitted were "picked up in very random hap-hazard collecting (26)." Smith was also conducting research on the alkali tungstates and admitted to Browne "that I am at work from morning until night, and my relief comes when I turn to my old books and things of that kind (27)." He told Browne he thought it would be a "splendid thing" if [Browne] were to get out his book on [John] Winthrop, Jr. because "I know with what care you do this historical work…and it would be a great contribution (28)."

Browne and Smith met briefly at the Chemists' Club in New York on February 11 and March 17, 1921, but only had a few minutes to discuss historical matters. Smith was the principal speaker in February, and Browne found his description of a gradual "emancipation" from a narrow organic specialist of the 1870s to one with a broader view encompassing many branches of chemistry done "so pleasantly and with so much good humor and charm that everyone was pleased (16)." At the March meeting Browne recorded that the speaker, Irving Langmuir, was "handsome as an Adonis with his boyish charming manner [and] made a brilliant impression (29). Browne also shook hands with "old Dr. [Charles F.] Chandler, who in spite of his 84 years is always one of the boys (16)."

Early in April, 1921 Browne passed through Philadelphia and telephoned Smith from the train station, only to find that Smith had just left his office. Writing to Smith a few days later, Browne described the two volumes of *Bibliotheca Chemico Mathematica* that he had wanted to show Smith in Philadelphia (30). As a "descriptive account of some 13,000 books upon chemistry, physics and other exact sciences," Browne claimed "it was the most fascinating work which I have ever read," and promised to bring it with him to the Rochester meeting at the end of April (31).

On April 11 Smith acknowledged Browne's letter and noted that (32):

> As I write these lines it dawns on me that it is our bounden duty as 'cranks' to try and corral all the 'cranks' at some convenient hour and place when we are in Rochester and talk over our hobbies. I am having my Priestley bookplate copied, and shall bring some copies of it with me for distribution to any 'cranks' who may wish a copy. I have a couple of other little things which I could easily carry with me, and I think I shall do so. I beg of you to consider for a moment whether we can't get together. I imagine that perhaps a half a dozen men will be found who are interested in the history of our science and we might be able to have a pleasant symposium.

Browne replied that Smith's suggestion (33):

> ..to get together at the Rochester meeting is a good one, and if there is any gap in the program, we ought to make use of it. I will try to bring along a few curiosities in the way of autographed letters, etc….which will not take up much room.

Browne was also going to have copies made of the negative of the Priestley bookplate in his possession (34).

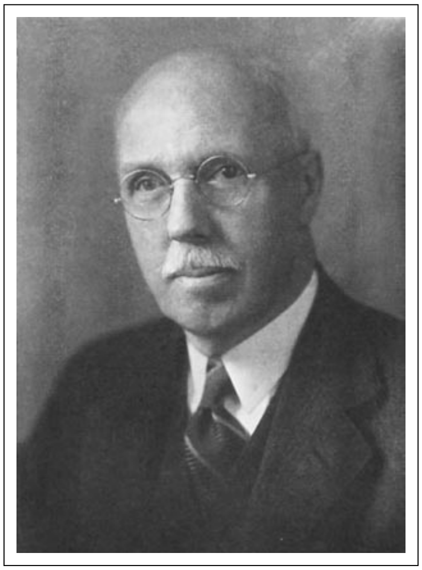
The Rochester Meeting (April 1921)

The 61st national meeting of the ACS was held in Rochester, New York, from April, 26 – 29, 1921. The social aspects so prevalent at the Chicago meeting were also in evidence at Rochester, although the Rochester planning committee "had been instructed to reduce entertainment features to a minimum" after the Chicago meeting (35). The organizing committee speculated that (36):

> The *piece de resistance* will be the banquet, free to members, to be held at Bausch & Lomb's, after which the company will furnish high class entertainment. At this banquet, it is hoped much of the formality will be dispensed with and the ladies will be in evidence.

[It was later noted that a "sorry lot of chemists" had to leave at 10:30 p.m. to catch the New York train, thus forcing them to miss some "of the best numbers," including the "Oriental dance number (37)"].

Attendance records for Rochester show 806 ACS members and 428 guests; 289 Divisional and Sectional papers were presented on Wednesday and Thursday at the Mechanics Institute at 55 South Plymouth Avenue (38). As President of the Society Smith presided at the ACS Council Meeting held on Monday, April 25. While he waited for ACS Secretary Charles L. Parsons to return with some papers he had forgotten, Smith told the 113 councilors that he was forming a section of the history of chemistry and invited them to attend a meeting on Thursday (39). He again used the term "cranks" to describe those chemists who had a historical interest. This meeting does not appear on any formal program for the meeting, and from the dates of the Smith/Browne correspondence there was apparently no advance announcement of this group's meeting. In addition to the Council meeting, the most obvious time for Smith to make a public announcement of this meeting would have been when Smith presided at the opening general session on Tuesday.


Charles A. Browne

Instead of the half-dozen people expected by Smith, the audience ranged from 20 to 50, depending on who was doing the reporting (40). Smith and Browne dominated the meeting with the historical items they had brought to Rochester. Smith showed a 1671 alchemical work in Latin which concluded with the words "All Honor and Glory to God, the Keeper of the Earth," and he recalled how Sir William Henry Perkin concluded a lecture to students at the University of Pennsylvania by lifting up his hands and exclaiming, "Praise be to God to whom belongs all Glory (41)." Smith also exhibited autographs and photographs, and he discussed early chemistry books by two Harvard professors, John Gorham and John White Webster, who had been hanged for murder.

But it was Browne who contributed the most to this meeting. Not surprisingly, he spent some time talking about John Winthrop and the alchemist George Starkey, who graduated from Harvard in 1646 (42). He also mentioned Amos Eaton (a pupil of Benjamin Silliman), Joel B. Nott (Union College) and Frederick Accum, "who did more than any other chemist of his time to popularize chemistry (41)." In addition to autographs and photographs from his collection, Browne had the most unusual item at this meeting—a lock of John Dalton's hair (43)!

Only one other person had something to exhibit. Charles L. Parsons, the venerable Society Secretary who probably had some advance notice of the meeting from Smith, had a copy of a rare early work by John Dalton that he shared at the meeting.

The others who are recorded as speaking did so spontaneously. Frank Dains of the University of Kansas described the library collection of chemical books at Transylvania University in Kentucky. Well known as an organic chemist, Dains had presented a paper on "Applied Chemistry in Prehistoric and Classical Times" at the AAAS Section L organizational meeting in December 1920 (22). F. O. Rice, then a young professor at New York University (NYU), who would later be known for his work on kinetics and photochemistry, talked about NYU professor John W. Draper and the first photographic portrait of a human face. An unidentified "younger member" called attention to the "forgotten life and work" of the Spanish chemist Andrès Manuel Del Rio who discovered vanadium. Edward Curtis Franklin of Stanford University described the recent book by his Stanford colleague John M. Stillman on Paracelsus (44). Forris Jewett Moore, who taught a well known course on the history of chemistry at MIT, commented on the value of studying the history of chemistry, and his views were supported by George Frankforter of the University of Minnesota.

Although this ended the "informal" presentations of what can be considered the second meeting of HIST, there was additional discussion about a more formal organization. Dains wondered whether the ACS might publish historical monographs, but Smith "feared the interest in historical chemistry in America was not sufficiently widespread to create a demand for such monographs (41)." C. E. Coates asked whether the section should be organized as other ACS sections and divisions. According to Browne (41):

> Smith thought it better not to force the movement, but to let things develop freely and spontaneously. Those who were interested could convene at each meeting for a friendly exchange of views; if any member had something of interest to show, let him bring it along; the program of each meeting would thus shape itself…and would be of more interest than a prearranged schedule of papers.

The Journal of Industrial and Engineering Chemistry reported that (45):

> The meeting was a great success, and it was agreed that without any formal organization, such a symposium should be planned as one of the features of future conventions of the Society. For the training of American chemists, one of the things needed is a proper appreciation of the historical and human side of our great science.

The New York Meeting (September 1921)

The next meeting of the ACS in the fall of 1921 was held in New York City (46). This time there was a formal but terse notice on the program (47):

> President Edgar Fahs Smith and kindred spirits will meet Friday afternoon, September 9, in Room 301, Mines, to discuss their hobbies.

It was shrewdly scheduled to follow the inaugural meeting of the Section of Chemical Education (CHED), which Smith had organized (48). After the CHED sessions ended, the "members adjourned to attend the "symposium" on the History of Chemistry (49). (It was a preview of the close relationship these two groups would have in the future, especially because of the role the history of chemistry would play both formally and informally in chemical education.)

Swelled by the CHED attendees, more than 100 people crowded into the room scheduled for this "symposium" (50). "This proved to be most interesting, on account of the many rare portraits, books, letters, and pamphlets which were shown (49)." In addition to Smith and Browne (who surprised many by speaking about alchemists in New England), other speakers included Ernest Cohen (University of Utrecht), who spoke on the teaching of the history of chemistry in Dutch universities, a Dr. Adolf (Shantung Christian College) who "gave an interesting account of chemistry among the Chinese," and Professor K. C. Pandyra who "discussed the Hindu's contributions to chemistry, both ancient and modern (49)."

It was obvious that this history of chemistry, even on an informal basis, was very popular; and the decision was made before adjournment to form a probationary Section of the History of Chemistry of the ACS (17, 50). A similar action had been taken in the CHED sessions when a motion was "made and seconded that the Section be formally organized (51)." Thus CHED and HIST were "officially" started as formal organizations on the same day, and Edgar Fahs Smith was the first chairman of each group.

The Legacy of Edgar Fahs Smith

Smith may have been enamored by Priestley, but his focus was on Priestley in America (52). Even as early as the Rochester meeting, Smith made it clear that the principal aim of the history of chemistry group should be to look at the history of chemistry in America. According to Browne (34):

> This, in his opinion, was to be the chief, although by no means the exclusive aim of the section, and he was always anxious to have a goodly number of papers upon some phase of chemistry in America upon our programs.

It was indirectly seconded in Rochester by the principal ACS founder Charles F. Chandler, who gave a public lecture on "Chemistry in America" at the Rochester meeting. "With a firm and steady voice, and youthful bearing," [Chandler was then 84] Chandler traced the history of the ACS and American chemistry. He received a lengthy standing ovation, after which Smith presented him with "a beautifully bound" copy of his Ph.D. thesis from Göttingen that he had written sixty years earlier (53).

Browne supported this idea, commenting years later that (34):

> There is nothing better which we can do in a constructive way toward promoting the work of the Division than in having always some papers upon the phases of the history of chemistry in America. The field of inquiry is so large, and so much of it unexplored, that it should attract a large band of our younger members.

In the years following the New York meeting, until his death in 1928, Smith's influence was permanently imprinted on the Division. Browne called attention to "the attraction of his wonderful personality, as it filled the meetings he attended (34)." Smith urged members to collect and preserve historical material, and hardly a meeting passed without Smith giving portraits, books, photographs, and letters to other members, enlarging and often starting collections with his generosity. L. C. Newell emphasized in 1932 that HIST (50):

> ..owes its inception, development, standards and permanency to the wisdom, generosity, intelligence and culture of our incomparably beloved patron.

But it was Charles Browne who predicted that "if we could only acquire something of his faith and spirit, the future of the Division would be secure (34)."

> What have we accomplished in the 85 years since Smith exhorted us to study chemistry in America? Do we have enough of Smith's faith and spirit to carry the division to its centennial in 2021 and beyond? To answer that question, we need to examine the achievements of the division, which are substantial considering that HIST has always been a Division with a very small membership.

HIST on Probation

Even though Smith was president of the ACS in 1921 and 1922, there was no favoritism shown his little group, which still had to pass through the standard probationary period before it could join the other divisions of the Society, which it did successfully in 1927, a year before Smith's death (54). There was the necessary schedule of papers at each national meeting, with 135 read during the 11 meetings of the probationary years. Not surprisingly, Smith and Browne dominated the program: giving papers, continuing to bring books and artifacts, and encouraging others do so as well. True to Smith's exhortation, much of the early programming focused on American chemistry, in particular the writing of the biographies of American chemists, many of which were published. Not only were rare books and artifacts part of the HIST sessions, but HIST arranged for special historical exhibits for the general public at almost every national ACS meeting (55).

Other activities beyond the national meeting program occupied that early HIST membership as well. One was the Priestley house in Northumberland, Pennsylvania. Browne and Smith encouraged the construction of the small museum at the house, and they willingly donated their extensive collection of Priestley materials for display. The museum was dedicated as part of the Golden Anniversary celebration of the ACS in 1926, with many HIST members in attendance (56).

More importantly, the Society, probably under Smith's influence, chose HIST to write its fifty-year history (57). Browne headed a committee of five that produced a separate volume of the *Journal of the American Chemical Society* that was light on the history of the society but heavy on the review of American contributions to chemistry in various branches of chemistry. Browne admitted the reviews were incomplete and hoped that HIST would continue to expand the areas covered (58). Over the years HIST has done that in different formats, especially through its joint programming with other technical divisions as anniversary years were celebrated.

As is often the case with small organizations run by volunteers, at any given time it is a small nucleus of people who manage its affairs and keep the ship afloat. Many early HIST officers were in place for many years, presumably because of a shortage of candidates. Nevertheless, they were dedicated to the concept of HIST and its programming. Smith called HIST members "cranks" and considered the collection of books and artifacts a hobby. When the History of Science Society (HSS) was formed in 1924 by Sarton and others to help preserve *Isis*, a serious scholarly pursuit of scientific history was being born. Actually, Smith was elected the fourth president of HSS in 1928, but he died in May of that year before completing his term of office (59). Generally HIST did not consider itself in that category, although some of its ardent early members, like Tenny L. Davis, Henry M. Leicester, Wyndham D. Miles, and Aaron J. Ihde did fit that mold. More often than not, HIST members were at the end of their careers and were content just to hear papers on chemical history without taking an active role in the organization or giving a paper.

While Smith's death brought others to the fore, gone were the wonderful items he brought to the meeting. Others picked up the slack somewhat and carried on the tradition for many years, although in a very spotty manner. Smith's collection remained sealed in his office for three years, when, in 1931, his widow bequeathed it to the University of Pennsylvania along with an endowment to preserve and maintain it (60). The collection quickly became the focal point of serious study in the history of chemistry. Many HIST members availed themselves of its resources and gave HIST papers based on their research in the collection. But with the passing of the cadre of charter members, HIST drifted through uncertain times, reaching a low point during World War II when Ralph Oesper was Secretary.

The Sidney Edelstein Era (61)

Beginning in 1948, HIST would experience a dramatic reversal when Sidney M. Edelstein became the secretary, a position he would occupy for almost 20 years. To say that Edelstein was a man of action would be an understatement at the very least, for Sidney was a person who liked to be at the forefront of what was happening in his spheres of activity. Characteristically, Sidney approached life with great enthusiasm and pursued his interests with some abandon once they became imbedded in his psyche. This included the history of chemistry, which began with a required history of chemistry course at MIT taught by Tenney L. Davis, an early HIST member. Edelstein began collecting books, and in the early 1940s he visited the Smith collection at the University of Pennsylvania, where he met the collection's indomitable curator, Eva Armstrong. He later used Armstrong as a sounding board for his purchases. When he told her that he had a chance to buy an original Boyle for $20, she advised him that Smith had only paid $2 for a similar copy and that the one Sidney was contemplating was "terribly overpriced."

Learning of the HIST division for the first time from a visitor to his New York office, Sidney attended a meeting in 1947 which he said "was simply a matter of a few people who didn't quite seem to know what they wanted (62)." They reelected the current chairman, Henry Leicester, and Sidney offered to be secretary. In typical Edelstein fashion, Sidney took it upon himself to change HIST. Sidney was a good promoter, and he did not slack in that regard when it came to HIST.

Henry Leiscester remained as HIST chair for four more years, until 1951 (63). Sidney reported (62):

> Between Henry Leicester and myself, we kept the Division going. About ten to fifteen people who had been working in the field soon came forward. Before I knew it, we had a small, active corps of people in the division really concerned with the history of chemistry. I would guess we had twenty-five or thirty, maybe up to fifty after a year or two.

Edelsetein ran HIST out of the New York Office of the Dexter Chemical Company, which he had founded. He arranged meetings, cajoled people into giving papers, and even paid for the luncheon held at each ACS meeting. Sidney's reign averaged more papers per meeting than Oesper's, but it was spotty. In fairness to Oesper, the war years certainly did not help, and it should be noted that there was no national ACS meeting in 1945 because the federal government refused to grant the ACS a permit to hold a meeting with more than 50 people. Perhaps what Sidney was most successful at was in establishing symposia as a HIST way of life.

Obviously, more symposia generated more papers, and the peak years for Sidney were 1957 through 1959. During that time there were eleven symposia and 176 total papers, including those in general session. Since five ACS divisions were celebrating their golden anniversaries in Chicago in the Fall of 1958, HIST played a major role and received much attention as it programmed symposia for the history of each division. For the first time there were joint symposia with divisions other than Chemical Education, the most notable in 1958 being O. T. Benfey's Kekulé-Couper Centennial on the "Development of Theoretical Organic Chemistry" (64).

Sidney's philosophy was pretty straightforward (62):

> The office was a powerful office because I could put together a program and push the president who would leave it up to me because nobody else was going to do the work. You cannot do everything by letting everybody decide everything, because nobody will decide anything. There always has to be a person who looks after and pushes and does things. I am proud of that. If I had not done that, we might not have a viable Division, with a number of people and a lot of activities.

It should also be noted that it was during Sidney's time that the first three sessions on Archaeological Chemistry were held, the first in 1950, which was chaired by Earle R. Caley of Ohio State (65). There is no evidence that Sidney was responsible for starting this, but he certainly was an ardent supporter. This group continues to operate as a subdivision of HIST, and to date six volumes have been published in the archaeological chemistry series based on HIST symposia which take place approximately every five years (66).

The Dexter Award

In 1956 Sidney established an award whose full title was "The Dexter Chemical Corporation Award in the History of Chemistry." He felt strongly that there should be something to recognize people who did good work in the history of chemistry, and there was nothing like it anywhere in the world. For Sidney, the recipient had to have advanced the history of chemistry in one of three ways—by publication of an important book or article, by the furtherance of the teaching of the history of chemistry, or by meritorious services over a long period of time which resulted in the advancement of the history of chemistry. For this, the recipient would receive $250 and the proverbial "suitable scroll (67)."

The first Dexter Award went to that long-time faithful HIST member, Ralph E. Oesper. Oesper received his honor for "meritorious service and continued interest in the field of the history of chemistry," which at least partially fits one of Sidney's three criteria. It may also have smoothed over any still ruffled feathers remaining from Sidney's replacing Oesper as HIST Secretary (68).

The third recipient was that longtime and faithful steward of the Edgar Fahs Smith Collection, Eva Armstrong (69). HIST celebrated the 50th anniversary of this award with a special symposium at the San Francisco ACS meeting in September, 2006 (70). Now named the Sidney M. Edelstein Award for Outstanding Achievement in the History of Chemistry in Sidney's memory, it is funded in part by Edelstein's daughter, Ruth Barish, with significant financial support from the Chemical Heritage Foundation (CHF). Since Oesper and Armstrong received their awards for service and a check for $500, the award has increased to $3,500. Most of the other recipients have been serious scholars of chemical history, and the award has gained recognition as the highest honor one can receive in the history of chemistry (71).

The Ihde Influence

In the early 1960s, HIST began to change again. Perhaps most notable was the presence of Aaron J. Ihde, an historian of chemistry who served as HIST chair from 1962 through 1964. It was at the end of this tenure that Aaron's text, *The Development of Modern Chemistry*, was published, the first new book of its kind in many years (72). It greatly enhanced Aaron's reputation as an historian of chemistry, enabling him to attract to HIST others for whom the history of chemistry was not just a hobby, as it had often been for many since the days of Edgar Fahs Smith and Charles A. Browne (73).

Historical Chemistry Journal

Shortly after the Division was founded, Smith asked Browne whether "we could prevail on Dr. [Charles H.] Herty to give us a page of his journal [*J. Ind. Eng. Chem.*] for the….history of science? (74)." This was followed by Smith's musing that "for some reason I can't free myself from the idea that we ought to have a Journal devoted to the interests of the history of chemistry (75)." When Browne accompanied Arthur B. Lamb (the editor of *The Journal of the American Chemical Society*) to the dedication of a new chemistry building at Dartmouth, he initiated talks about where history papers could be published and found Lamb sympathetic to his plight (76), but Smith doubted Lamb's associates would support history papers in the *Journal*, "yet it ought to appear someplace where it will meet the eyes of our entire membership (77)."

Eventually, Smith gave up on using an existing ACS journal and proposed to Browne that the Section could issue *The American Journal of Historical Chemistry* six times a year with 48 pages per issue, setting the price at $1.00 per annum to attract the high school and college teachers and perhaps even students (78). Browne was concerned about where the papers would come from for such a journal (79), but the ever optimistic Smith had "no misgivings on the subject matter for publication, but I would like to see right on the table $100,000, the income from which could be applied to bringing this pet scheme to light (80)." At the New Haven ACS meeting in April, 1923 Smith told Browne he had asked William H. Nichols for that amount (34). This is an astonishing amount of money for that time, roughly equivalent to $1 million in today's dollars (2006). Smith said that he considered such a journal one of the "greatest needs of American chemistry" and that it was one of his fondest hopes to have such a journal (34). There is no evidence, however, that Nichols gave Smith the money.

More importantly, Smith did not reckon with the young and energetic Neil Gordon, who was instrumental with Smith in founding CHED in 1921 (48). Gordon started the *Journal of Chemical Education* in 1924. He was committed to publishing papers in chemical history and even appointed Lyman Newell, who was then HIST Secretary, to be the associate editor in charge of the history papers. For the time being HIST had an outlet for its papers, and many appeared in the early volumes of the *Journal*. And even though chemical educators thought that the history of chemistry had a place in the education of chemistry students, the volume of their own papers slowly pushed the history papers aside.

In 1948 the first issue of *Chymia* appeared. Designed as an annual volume for scholarly papers in the history of chemistry, *Chymia* was not a HIST publication, but it was edited by a HIST member and other HIST members were contributors. *Chymia* was sponsored by the Edgar Fahs Smith Memorial Collection at the University of Pennsylvania, so in some respects Smith finally got his wish for an American publication devoted to the history of chemistry and fittingly sponsored by his own institution. But the University had agreed to fund it for only three years. The fourth volume was produced through private donations, but no financial support could be found after that, and the publication ceased after the 1951 issue.

Late in 1956 HIST revisited the idea of a journal of the history of chemistry, but in the end decided to revive *Chymia* instead. The ACS Board approved the joint venture with the University of Pennsylvania, "with the understanding that the ACS would assume no financial responsibility and that the approval would continue "only so long as *Chymia* is the official organ of the Division (81)." HIST now appeared on the masthead along with the Smith Collection; Henry Leicester was the editor-in-chief, and Sidney Edelstein was on the editorial board. That meant that for HIST to revive *Chymia*, it would need to infuse the venture with hard cash. It is tantalizing to suppose that HIST was not particularly flush to support such a venture, but there had to be some financial support forthcoming from somewhere. There is no official record to show just how HIST did finance this venture. However, one name that has been mentioned is Denis I. Duveen, an independently wealthy book collector who often spent time in the Smith Collection. Another is Sidney Edelstein and the Dexter Chemical Company. Interestingly, *Chymia* ceased publication again with volume 12 in 1967, just one year after Sidney left his position as secretary of HIST, a move he did not make willingly (82).

In 1988 this issue was taken up once again by William B. Jensen, then serving as HIST secretary. Desktop publishing had reduced production costs, but more importantly, the history of chemistry was finding less acceptance in the more traditional chemical journals, especially the *Journal of Chemical Education*. Jensen had become editor of the Division's newsletter and had produced a few issues when he decided to expand it into a regular publication. As a result, and with the support of the Division and the Department of Chemistry of the University of Cincinnati and the Oesper Collections in the History of Chemistry, Jensen began in 1988 to publish the *Bulletin for the History of Chemistry*, which incorporated the Division's newsletter. He did not intend to compete with established historical journals of science like *Ambix* (83) and *Isis*.

Jensen reasoned that the majority of HIST members were practicing chemists and teachers of chemistry "who enjoyed reading general interest articles on the history of chemistry but [were] prevented by other duties from engaging in full time historical research." Rather than tackle the social and philosophical questions raised by professional historians, Jensen wanted to reach chemists by adding a time and depth-of-content dimension to their understanding of the history of chemistry (84). When an objection relating to a paper in the *Bulletin* escalated to legal issues, the *Bulletin* publication was temporarily suspended until those issues were resolved, although no volumes were omitted. It was revived under the editorship of Paul R. Jones and has evolved over the years. Originally papers were by invitation only, but now the *Bulletin* is a fully refereed journal issued twice a year. The HIST Executive Committee considers the *Bulletin* so important to its members that the entire membership dues are used to support its production. It would be a bargain at twice the price (85).

Awards

In 1992 the Division embarked on a program with the ACS Office of Public Outreach to recognize our scientific and technical heritage through a series of plaques designating a site, artifact or collection as a National Historical Chemical Landmark. Originally conceived as a public outreach effort to bring the achievements of the chemical community to the general public, by 2007 the program had made 56 awards throughout this country as well as international awards in England, Mexico, Canada, France, and India. An advisory board receives nominations from ACS local sections or divisions and selects those that conform to the program criteria.

It is indeed fitting that the Edgar Fahs Smith Collection was one of those accepted for a brass plaque now on display in the University of Pennsylvania library. The Office of Public Outreach has been disbanded and the program is now run out of the Office of Communications. The original advisory committee is now an *ad hoc* committee of the ACS Board of Directors, and HIST is no longer an official sponsor; but from the beginning, HIST members have always constituted a part of the membership of the advisory committee (86).

In 2006 the Division embarked on another program that involves the awarding of plaques, but with a different purpose in mind (87). While the ACS Landmarks Program focuses on nominations that would be of interest to the general public and with plaques prominently displayed in public areas, the new HIST award, called Citation for Chemical Breakthroughs, is addressed to chemists. The program recognizes publications, books, and patents worldwide in the field of chemistry that have been revolutionary in concept, broad in scope, and long-term in impact. The award consists of a plaque that is placed near the office or laboratory where the breakthrough had been achieved. The program was initially funded by the ACS Innovative Grant Program, the ACS Corporate Associates, and a private donation.

In 2006 one book, three patents, and six scientific publications were honored, including the pH meter invented by Arnold O. Beckman, the discovery of Teflon by Roy J. Plunkett, Moses Gomberg's paper on free radicals, and F. Sherwood Rowland and Mario J. Molina's paper on the destruction of the ozone layer (87). Jeffrey I. Seeman, HIST Chair 2005–2006 and originator of the award, explained that the award is intended to "celebrate great scientific accomplishments and motivate, through shared pride of achievement, all who walk by and see the plaques. We hope they'll say, 'Wow! That was done here (87, 88)!'"

A third award program sponsored by HIST is simply called the Outstanding Paper Award (89). It originated in 1984 with a grant from Raymond B. Seymour, a HIST member who had for many years sponsored HIST symposia on the history of polymer chemistry. The funding came from the proceeds of his book resulting from a HIST symposium, *The History of Polymer Science and Engineering* (90). Originally given for the best oral presentation at a HIST meeting, since 1989 it has been given annually to the best paper published in the *Bulletin for the History of Chemistry* for the previous three-year period. The award consists of $100 and $150 in books from the Chemical Heritage Foundation. It is noteworthy that the 2006 award recipient shares a first name with a charter HIST member, Lyman C. Newell, and his topic was in part one that was discussed at the first HIST meeting 85 years ago (91).

Other Activities

In addition to giving awards, the Division has in recent years won two ChemLuminary Awards from the ACS. The first, in 2003, was for "See and Be Seen," a program to help support HIST members giving a historical paper at a regional meeting and thus help promote history at regional meetings, an area that frequently is omitted from regional meeting programming (92). The second, in 2006, was given for the initiation of the Citation for Chemical Breakthroughs program and the piano concert (93). The latter was a HIST-initiated and -organized event at the Washington ACS meeting (cosponsored with CHF). It was the first Annual Fall ACS National Meeting Music Concert, attended by 400 people and reviewed by the *Washington Post*. The concert featured chemist-pianist Victoria Bragin playing works by chemist-composer

Alexander Porf'irovich Borodin. A second concert was held in the fall of 2006 at the national meeting in San Francisco.

Other Divisional Activities

A piano concert is not the first unusual project undertaken by HIST. Beginning in Chicago in 1985, HIST started producing postal cachets that featured ACS presidents and a logo of sorts identifying the city in which the meeting was held. The cachets featured past stamps with a chemistry theme—the 1951 ACS stamp, the 1976 chemistry stamp, and the 1983 Priestley stamp. HIST was sharing a booth with the Chemical Heritage Foundation in the exhibit hall at the time, and adjacent to the booth was a U.S. Post Office substation. The cachets were hand-cancelled by the post office with a cancellation designed by a HIST member and featuring the ACS logo. Cachets were sold as a fund raiser for the division, and even now they show up on eBay. This was done for every ACS meeting for about ten years, when the problems in dealing with the United States Post Office became insurmountable and the program was discontinued. But there was a serious side to this program as well. While the cachets were being sold at the HIST booth, a paper was being presented in the HIST general session on the ACS president featured on the cachet. These papers covered the ACS presidents in a different light from the standard biographical sketches published in ACS histories. The focus was not just on their chemical achievements, but in fact and more importantly, what led to their being considered worthy of the ACS presidency and what they accomplished as ACS president. This resulted in a set of interesting papers covering the first quarter-century of the society. Unfortunately, none of these papers has been published, which is too often the case with HIST ACS meeting papers.

Back in 1937 Browne, noting this fact, lamented that much of the original scholarship that went into the early HIST papers was never published, and quite frequently the manuscripts were lost forever when their author died (34):

> In order to prevent such occurrences from arising in the future, I would like to suggest that copies of all unpublished papers read before the Division be deposited for safe-keeping and future reference with the Edgar Fahs Smith Collection at the University of Pennsylvania. Users of such material would…give the same credit for their sources of information as in the case of published articles.

It is difficult to tell in retrospect which authors did that, and even today valuable scholarship presented before the Division is being lost. However, in 2006, the Division signed an agreement with the Chemical Heritage Foundation to preserve its archives, which, unfortunately, are extremely meager in the early years. A records retention policy has been established by HIST and a grant received from the ACS Division Activities Committee in 2006 will provide support to organize the HIST archives and develop a model that might be followed by other ACS divisions. This work has just begun. Perhaps the Division may yet fulfill Browne's wish about disappearing unpublished papers.

The Chemical Heritage Foundation

In 1965 the American Institute of Physics (AIP) approached the ACS about jointly forming a facility for documenting the history of the physical sciences. The ACS in turn gave the proposal to HIST, which under the leadership of Wyndham D. Miles as Chair and Sidney M. Edelstein as Secretary rejected the idea. Instead, they wanted a History of Chemistry Center that would look at the entire history of chemistry, not just the recent or current history that interested the AIP. Nothing came of the idea until it was revived by HIST chair William J. Wiswesser and chair-elect John H. Wotiz in 1979. They convinced ACS President Gardner W. Stacy to fund a task force that would visit a number of potential sites for such a center, study other such facilities, and formulate objectives. The group reported to the HIST membership at the fall 1979 ACS meeting in Washington, and the membership responded with an enthusiastic and unanimous resolution calling on the ACS to establish a Center [Office] for the [Contemporary] History of Chemistry.

This was followed by a symposium at the ACS meeting in Houston in March, 1980 organized by Wotiz and titled "The Chemistry Profession Needs a Center for the History of Chemistry." Since this was intended to be an ACS operation, most thought it would be run out of the Washington headquarters. However, at Houston Arnold Thackray made an alternative suggestion—to house the new center at the University of Pennsylvania in Philadelphia. He noted that Penn already housed the Edgar Fahs Smith Collection in the History of Chemistry, housed the editorial offices of *Isis,* and had an outstanding History and Sociology of Sciences Department.

Ensuing discussions at various levels within HIST and the ACS culminated in final approval at the December, 1982 ACS Board of Directors' meeting that formed the Center for the History of Chemistry at the University Pennsylvania with Arnold Thackray as its director. In 2007 the Center, which has now become the Chemical Heritage Foundation, will celebrate its 25th anniversary. While CHF owes much of its current organization to the energies and talents of Thackray and others associated with him, there is no question that the efforts of John Wotiz and HIST were instrumental in nurturing and giving birth to the idea of a history center until it came to fruition (94).

HIST in the 21st Century

This brief review of HIST and its activities for the past 85 years brings us to the earlier question of what have we done with the legacy passed on to us by our founders, Edgar Fahs Smith and Charles A. Browne. What would they say of our stewardship 85 years later? The answer is found in two major documents. The first is the HIST Mission statement, adopted in 2006, which can be found elsewhere in this issue. The second is a State-of-HIST statement issued by HIST Chair Jeffrey I. Seeman as he completed his two-year term of office at the end of 2006. He points out that: HIST is on a sound financial basis with resources leveraged to their maximum value. There is a modern web site which is constantly evolving to provide its membership current information as well as serve as a historical resource tool (95). There is a new logo which is on all HIST documents and is explained elsewhere in this issue. Programming continues to be vibrant, including ACS Presidential symposia and others that have on some occasions received coverage in *Chem. Eng. News*. External funding continues to enhance division activities. The Division's *Bulletin* continues to be one superb issue after another. Strong relationships have been developed with the ACS and CHF at several different levels.

"HIST's greatest strength," Seeman concludes, "is its members and our diversity of interests, experiences, and talents. At the same time, the Executive Committee shares a commitment to HIST, a passion to support HIST's mission, and our promises to our members and the ACS."

I submit that this reflects accurately what Smith and Browne would have wanted the division to achieve when they set us out on that long path 85 years ago. They would, I believe, be proud of what we have done, are doing, and will do in the future. "After all, we study the past that we may understand the present and judge wisely of the future (7)."

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2. There are a number of past accounts of the early history of HIST. See, for example, L. C. Newell, "Historical Sketch of the Division of the History of Chemistry, American Chemical Society," *J. Chem. Educ.,* **1932**, *9*, 667–669; C. A. Browne, "The Past and Future of the History of Chemistry Division," *J. Chem. Educ.,* **1937**, *14*, 503–515; and C. A. Browne, "Contributions of the Divisions: History of Chemistry," in C. A. Browne and M. E. Weeks, *A History of the American Chemical Society: Seventy-five Eventful Years*, American Chemical Society, Washington DC, 1952, 288–290.
3. The official name was the World's Columbian Exposition. See http://users.vnet.net/schulman/Columbian/columbian.html (accessed May 30, 2007).
4. Chemistry and the ACS played an important role in this grand affair. See J. J. Bohning, "A Center of Crystallization in a Molecular Mélange: The 1893 World's Congress of Chemists," *Bull. Hist. Chem.,* **1989**, *No. 3*, 16–21.
5. T. Thomson, *The History of Chemistry,* H. Colburn and R. Bentley, London, 1830–31. For high-quality scanned images from these volumes, see http://othmerlib.chemheritage.org/search?/athomson%2C+thomas/athomson+thomas/1%2C1%2C20%2CB/frameset&FF=athomson+thomas+1773+1852&11%2C%2C20 (accessed May 30, 2007).
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10. This concept was later emphasized by A. J. Ihde, "Let's Teach History of Chemistry to Chemists," *J. Chem. Educ.,* **1971**, *48*, 686–687.
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1955, *32*, 183–185; and D. M. Knight, "Teaching the History of Chemistry to Non-chemists," *J. Chem. Educ.,* **1971**, *48*, 285–286.

12. For more about Sarton and the formalization of the study of the history of science, see E. Garfield, "George Sarton: The Father of the History of Science. Part I. Sarton's Early Life in Belgium; Part II. Sarton Shapes A New Discipline," in *Essays of an Information Scientist*, ISI Press, Philadelphia, PA, 1985, Vol. 8, 241–253. See also I. B. Cohen, "George Sarton," *Isis,* **1957**, *48*, 286–300.
13. Details of the 1920 Chicago meeting, from which these excerpts are taken, may be found in *J. Ind. Eng. Chem.,* **1920**, *12*, 730 ("The Chicago Meeting"); *J. Ind. Eng. Chem.,* **1920**, *12*, 918–920 ("Setting the Scenes for the Chicago Meeting"); *J. Ind. Eng. Chem.,* **1920**, *12*, 938–941 ("The Chicago Meeting"); and *J. Ind. Eng. Chem.,* **1920**, *12*, 1022–1025 ("Program of Papers").
14. For more on Charles A. Browne, see J. J. Bohning, "How Sweet It Is: Charles A. Browne, Sugar Chemist and Historian of the American Chemical Society," *Chem. Heritage*, **2002,** *20*, No. 3, 10–11, 34–38.
15. For more on Edgar Fahs Smith, see The Edgar Fahs Smith memorial issue of *J. Chem. Educ.,* **1932,** *9*, 607–666 (various authors).
16. A valuable resource from which many quotes have been taken is C. A. Browne, "Reminiscences of Professor Edgar Fahs Smith," original typescript in the Edgar Fahs Smith Collection, University of Pennsylvania Library, call number 540.92 Sm52B. This is taken from Browne's extensive notes and journals that he kept throughout his career. See also C. A. Browne, "The Past and Future of the History of Chemistry Division," *J. Chem. Educ.,* **1937**, *14*, 503–515.
17. At the time HIST was organized, a group specializing in a specific topic could form as a Section of the ACS. "After these sections held a sufficient number of successful meetings to prove the need for them, they were given Divisional status with the right to elect their own officers, to draw up their own by-laws…and to collect, control and manage funds." See C. A. Browne and M. E. Weeks, *A History of the American Chemical Society: Seventy-five Eventful Years*, American Chemical Society, Washington, DC, 1952, Ch. 7 and 17.
18. For a picture of the shade trees under which Browne and Smith held the first HIST meeting, see *J. Ind. Eng. Chem.,* **1920**, *12*, 919.
19. Browne's papers are at the Library of Congress, LC Control No. mm 78014134. The 36,000 items are only loosely catalogued. In his correspondence with Smith, Browne kept Smith's original plus a carbon of his reply, thus providing a reasonably complete picture of their exchange. All quotations of the Smith-Browne correspondence are from letters found in this collection.
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24. Browne to Smith, November 15, 1920 and Smith to Browne, November 19, 1920 (19).
25. At this time the ACS president was elected by the ACS Council, not the membership; see *J. Ind. Eng. Chem.,* **1921**, *13*, 2. Election by the general membership did not occur until after the constitutional revision of 1948 (F. W. Walworth, private communication, March 19, 2007). See also F. P. Venable, "The Society's President for 1921—Edgar Fahs Smith," *J. Ind. Eng. Chem.,* **1921**, *13*, 106–107.
26. Browne to Smith, January 11, 1921 and February 8, 1921 (19).
27. Smith to Browne, January 5, 1921 (19).
28. Smith to Browne, January 21, 1921 (19). Browne never published a book on Winthrop, but see C. A. Browne, "Scientific Notes from the Books and Letters of John Winthrop, Jr., (1606–1676)," *Isis,* **1928**, *11*, 325–342.
29. Langmuir was then 40 years old and had been at General Electric for 12 years. His Nobel Prize in Chemistry was still eleven years away.
30. *Bibliotheca chemico-mathematica: catalogue of works in many tongues on exact and applied science, with a subject-index***,** compiled and annotated by H. Z. [Heinrich Zeitlinger] and H. C. S. [Henry Cecil Sotheran] with 127 plates, containing 247 portraits and facsimiles, H. Sotheran, London, 1921.
31. Browne to Smith, April 7, 1921 (19).
32. Smith to Browne, April 11, 1921 (19).
33. Browne to Smith, April 14, 1921 (19).
34. See C. A. Browne, "The Past and Future of the History of Chemistry Division," *J. Chem. Educ.,* **1937**, *14*, 503–515. There are two different Priestley bookplates shown in this paper. A preprint of this paper in the author's collection shows a different pagination and a somewhat different organization than the final published version. The second Priestley bookplate is missing from the preprint. For more on the "missing" bookplate see C. A. Browne, "The Bookplate of Dr. Joseph Priestley," *J. Ind. Eng. Chem.,* **1920**, *12*, 611. Browne's bookplate apparently was in a book in Priestley's library before the Birmingham riots of 1791.
35. *J. Ind. Eng. Chem.,* **1921**, *13*, 398.
36. Anon., "Plans for the Spring Meeting," *J. Ind. Eng. Chem.,* **1921**, *13*, 166. Other advance information about the meeting can be found in *J. Ind. Eng. Chem.,* **1921**, *13*, 266–267, 282, and 352–354.
37. *J. Ind. Eng. Chem.,* **1921**, *13*, 404. About 2,000 people attended the banquet, which was followed by vaudeville entertainment and moving pictures shot by Eastman

Kodak of the previous three days of the meeting. This was still a novelty at the time, but the archival fate of the movie is unknown.

38. The general report of the events of the Rochester meeting are in *J. Ind. Eng. Chem.,* **1921**, *13*, 378–405. Several candid photographs of Smith at this meeting are included in this report. For the "Program of Papers" see in *J. Ind. Eng. Chem.,* **1921**, *13*, 480–483.
39. Ref. 16, p 6. Browne does not record any surprise at this announcement, which seems to conflict with Smith's idea that everything should be kept informal.
40. Anon. counted 25 (*J. Ind. Eng. Chem.,* **1921**, *13*, 397); Browne counted 30 (*J. Chem. Educ.,* **1937,** *14*, 505); and L. C. Newell counted 50 (*J. Chem. Educ.,* **1932,** *9*, 667).
41. More details of this meeting may be found in C. A. Browne, "The Past and Future of the History of Chemistry Division," *J. Chem. Educ.,* **1937**, *14*, 505–507. This is a faithful reproduction of Browne's notes (16).
42. Starkey has received considerable attention in recent years. See W. R. Newman, *Gehennical Fire: The Lives of George Starkey, an American Alchemist in the Scientific Revolution*, Harvard University Press, Cambridge, MA, 1994.
43. When the author worked for the Center for the History of Chemistry at the University of Pennsylvania, he found a note that this lock of hair was in the Edgar Fahs Smith Collection at the University (presumably when Browne made a significant donation to the Collection in 1945). The curators conducted an extensive search but were unable to locate this specimen.
44. J. M. Stillman, *Theophrastus Bombastus von Hohenheim called Paracelsus; his personality and influence as physician, chemist and reformer*, Open Court Publishing Co., Chicago, IL, 1920.
45. *J. Ind. Eng. Chem.,* **1921**, *13*, 398.
46. See "Program of Papers," *J. Ind. Eng. Chem.,* **1921**, *13*, 951–955 and "Reports of Meetings of Divisions and Sections," *J. Ind. Eng. Chem.,* **1921**, *13*, 956–960. For additional information about the meeting, see *J. Ind. Eng. Chem.,* **1921**, *13*, 667, 733–737, 750, 752, 844–848, and 862–892.
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48. For the events leading up to the formation of the Division of Chemical Education, see J. J. Bohning, "Crystallizing Classroom Chemists: From Isolated Disorder to Organized Interaction in the Teaching of Chemistry. A History of the Effort To Create a National Chemical Education Organization," *J. Chem. Educ.,* **2003**, *80*, 642–650.
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51. Reported by N. Gordon in *J. Ind. Eng. Chem.,* **1921**, *13*, 960.
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53. C. F. Chandler, "Chemistry in the United States," *J. Ind. Eng. Chem.,* **1921**, *13*, 391–397. This is the complete text of his talk, accompanied by a picture of Chandler on the steps of Havemeyer Hall at Columbia University.
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55. J. J. Bohning, "The History of HIST. II. The Probationary Years," 198th National Meeting of the American Chemical Society, Miami Beach, FL, September 1989 (Abstract HIST 011); manuscript in preparation.
56. Browne covers some of this in Ref. 34. See also the Priestley issue of *J. Chem. Educ.,* **1927**, *4*, 145–213, and especially C. A. Browne, "Priestley's Life in Northumberland and Discussion of the Priestley Relics on Exhibition in the Museum," *J. Chem. Educ.,* **1927**, *4*, 145–172, and "Joseph Priestley as an Historian of Science, with Some Account of his Historical Apparatus Existing at the Present Time, *J. Chem. Educ.,* **1927**, *4*, 184–213.
57. *J. Am. Chem. Soc.,* Golden Jubilee Number, Part I. Origins and Developments of the American Chemical Society 1876-1926, Part II. Reviews of Progress in Various Branches of Chemistry in America, No. 8, August, 1926, 254 pp. This was a separate printing from the regular *Journal*. There is a regular No. 8 issue containing technical papers consistent with the pagination of No. 7 and No. 9.
58. Ref. 14. See also J. J. Bohning, "How Sweet it is: Charles A. Browne, Sugar Chemist and Historian of the American Chemical Society," 220th National Meeting of the American Chemical Society, Washington, DC, August, 2000 (Abstract HIST 009).
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60. L. Farrington, "Alchemy, Metallurgy, and Pharmacy: Edgar Fahs Smith and the History of Chemistry," http://www.library.upenn.edu/exhibits/rbm/at250/science/lf.pdf (accessed June 3, 2007). Browne made a significant donation to the Smith Collection in 1945. See J. J. Bohning, "Sweetening the Pot: the Library of Sugar Chemist Charles A. Browne and its Impact on the Edgar Fahs Smith Collection," 223rd National Meeting of the American Chemical Society, Orlando, FL, April, 2002 (Abstract HIST 015).
61. This section on Sidney Edelstein is taken from J. J. Bohning, "Ruling the Roost: Sidney Edelstein and HIST," 224th National Meeting of the American Chemical Society, Boston, MA, August 2002 (Abstract HIST 031); manuscript in preparation.

62. Quotations attributed to Edelstein are taken from: Sidney Edelstein, interview by J. L. Sturchio and A. Thackray at New Orleans, LA and West Palm Beach, FL, August 31, 1987 and February 24, 1988 (Chemical Heritage Foundation, Oral History Transcript #0075). See also J. J. Bohning, "Moonshine Whiskey and Japanese Shoestrings: The Making of a Textile Chemist," *Beckman Center News,* **1989**, *6*, No. 1, 3–4, reprinted with permission on the HIST web page at http://www.scs.uiuc.edu/~mainzv/HIST/awards/Edelstein%20Papers/Edelstein_bio_Bohning_CHF_2-header.pdf (accessed June 4, 2007).
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95. See http://www.scs.uiuc.edu/~mainzv/HIST. This web page makes a lot of the Division's information accessible in one place, including meeting papers and abstracts, awards, division business, and the like

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HIST MISSION STATEMENT (2007)

The Division of the History of Chemistry (HIST) of the American Chemical Society (ACS) seeks to advance knowledge and appreciation of the history of the chemical sciences among chemists, students, historians of science, and the broader public by

- Encouraging research and scholarship in history of the chemical sciences;
- Providing a welcoming environment for the discussion of history of chemistry in a variety of venues, particularly in symposia at national ACS meetings;
- Serving as a resource for chemical scientists in general, and members of the ACS in particular, who seek to understand the roots of their discipline, sub-discipline, or interdisciplinary subject;
- Recognizing major achievements from the past in the chemical sciences and the individuals who made those achievements;
- Publishing a scholarly journal in history of chemistry;
- Interacting with other organizations interested in the history of science; and
- Adding value to the ACS by helping it achieve its vision and missions.

COMMUNICATING THE HISTORY OF CHEMISTRY*

Paul R. Jones, University of Michigan

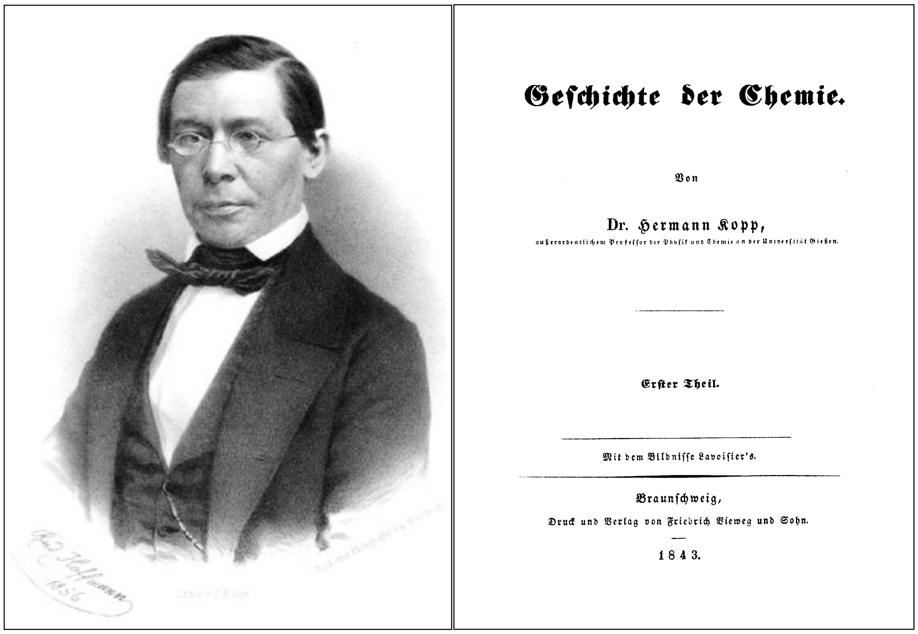
Introduction:

It is fitting that we examine the beginnings of the recording of history of chemistry, in view of our observing the 85th birthday of the founding of HIST, first conceived by its two founders, Edgar Fahs Smith and Charles Albert Browne, at an American Chemical Society meeting at Northwestern University in 1920. The motivation for this move was later stated by Smith in one of his published books (1):

> The author's hope is that sometime—how soon he knows not—but, sometime—the history of chemistry will be given its place in the curriculum of studies in every institution of learning where the science itself is studied; for it is, indeed, a very comprehensive subject.

Books

Yet the challenge of communicating chemical history begins in conveying the subject in the written word for the benefit of other historians of chemistry and, eventually, for their students. This essay will be focused on communication between scholars of the history of chemistry. The need, already recognized early in the 19th century, resulted in the publication of a two-volume set of books on the history of chemistry in England by T. Thomson in 1830-1831 (2). Hermann Kopp became recognized as the authority in history of chemistry, with his publication of four classic sets of books over a period of forty-three years, beginning in 1843 (3). His first, four-volume, extensive survey was succeeded by two volumes on "contributions" to the history of chemistry some 20 years later. At the same time he was assembling a two-volume set covering the development of chemistry in modern times. Only in the 1880s did Kopp turn to a detailed coverage of the earliest period of alchemy.



Geſchichte der Chemie.

Von

Dr. Hermann Kopp,
außerordentlichem Profeſſor der Phyſik und Chemie an der Univerſität Gießen.

Erſter Theil.

Mit dem Bildniſſe Lavoiſier's.

Braunſchweig,
Druck und Verlag von Friedrich Vieweg und Sohn.

1843.

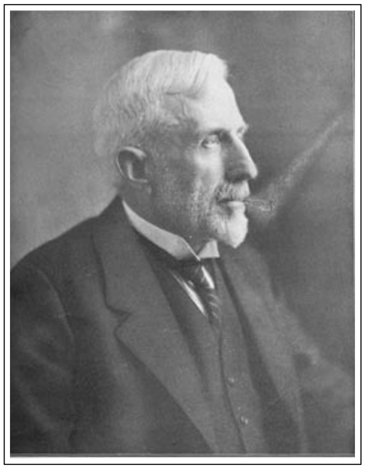
A young Hermann Kopp and his first history, courtesy the Oesper Collection

***Table 1.* Chronological Appearance of Periodicals in History/Philosophy of Science or Chemistry ***

1823	Annals Lyceum Nat. Hist. NY
1846	*Daedalus* (*J. Am. Acad. Arts Sci.*)
1853	*The Annals of Science* (AAAS)
1877	*Chemiker-Zeitung*
1877	*Annals NY Acad. Sci. (*succeeds Ann. Lyceum)
1877	*Revue des questions scientifiques*
1888	*Angewandte Chemie*
1912	*Isis*
1924	*J. Chem. Educ.*
1936	*Annalsl of Science (London)* [English, German, French] *Osiris*
1937	*Ambix*
1947	*Revue d'histoire des sciences*
1948	*Chymia*
1969	*Hist. Stud. Phys. Sci.*
986	*Hist. Stud. Phys. Biol. Sci.*
1960	NTM: *Schriftenreihe für Geschichte der Naturwissenschaften, Technik, und Medizin* [German, English, French]
1962	*Brit. J. Hist. Sci.*
1962	*Hist. Sci., Cambridge*
1962	*Angew. Chem. Int. Ed. Engl.*
1967	*Chemie in unserer Zeit*, Weinheim, Wiley, online
1970	*Studies Hist. Philos. Sci.*
	Kagakushi, J. Jpn. Soc. Hist. Chem.[Japanese]
1977	*Kultur und Technik* [German]
1980	*Historia Scientarum*, Japan [English]
1982	CHOC News (-1987); News, Beckman Ctr. (1988-1992)
1986	*Nuncius, Ist. Museo di Storia della Scienza*[Italian]
1988	Bulletin for the History of Chemistry
1992	*Chem. Heritage*
1995	*Chem. Intelligencer* (terminated 2000)
1997	*HYLE*, (Int. J. Philos. Sci.), online
1999	*Foundations of Chemistry*

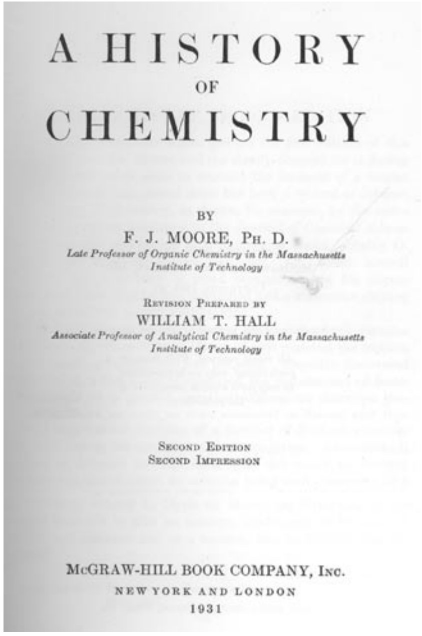
* Underlined titles cover exclusively chemistry and history; others cover sciences and may include philosophy.

A book by HIST cofounder E. F. Smith, *Chemistry in America,* was one of the earliest written on chemical history in the 20th century in the US (4). It might more appropriately have been titled "Early Chemistry in Philadelphia: the Chemical Society of Philadelphia," for he compiled minutes and reports from that society, including only a few "other chemists." In Smith's *Old Chemistries,* a set of biographical sketches and photographs from Geber (ca 1300) to mid-1800 Americans, the reader is provided a conversational walk through Smith's own extensive collection housed at the University of Pennsylvania (5). Probably the most widely acclaimed early survey of chemical history written in the US was a book by F. J. Moore, chemistry professor at MIT (6). It covered the broad span from ancients to early American chemists and was replete with 80 illustrations. The Kopp legacy in Germany was sustained by the publication of two chemical histories by G. Graebe (1920) and P. Walden (1941), but now they were focused on organic chemistry (7). Walden published a general history of chemistry in 1947 and a revision in 1950 (8).



F. J. Moore

In the second half of the 20th century general histories of chemistry were appearing in both Britain and the US. Most extensive and invaluable as a reference is Partington's multi-volume set, which covers the field from its beginnings through the first half of the 1900s (9). Leicester had presented a historical background of chemistry from ancient times to the present, including radioactivity, a decade earlier (10). In 1964, the same year in which Partington's first segment appeared, the history by Ihde (11) was published; this has been recognized as the most detailed one-volume source, rich with citations, photographs, and appendices. Brock's *Fontana History of Chemistry* (12), also a general survey, appeared in 1992 and has been followed in 2005 by *Chemical History. Reviews of the Recent Literature*, edited by Russell and Roberts (13).



A HISTORY
OF
CHEMISTRY

BY
F. J. MOORE, Ph. D.
Late Professor of Organic Chemistry in the Massachusetts
Institute of Technology

Revision Prepared by
WILLIAM T. HALL
Associate Professor of Analytical Chemistry in the Massachusetts
Institute of Technology

Second Edition
Second Impression

McGRAW-HILL BOOK COMPANY, Inc.
NEW YORK AND LONDON
1931

Meanwhile, in East Germany, the history of chemistry was growing as a specialization. Two publications in 1986 and 1989 originating from authors in Leipzig, Halle, Merseburg, and Jena (14) serve as encyclopedias of chemistry from ancient times to the present.

Morris has recently enumerated the myriad specialized histories of chemical industry in the second half of the 20th century (15). One entirely differently focused historical treatment deserving special mention is Hufbauer's account of the growth of German chemistry in the 18th century, published in 1982 (16).

Periodicals

Over two dozen periodicals have originated since 1823, in which subjects on history or philosophy of science (including chemistry, of course), have been published. Many of these are listed in Table 1 in chronological order of their appearance. Because the coverage in most of these journals is much broader than chemistry and history, only a small part of their contents deals with either or both of these topics. E. F. Smith recognized the value of the creation of a journal devoted exclusively to the history of chemistry. Soon after the founding of HIST, he attempted to raise sufficient funding to start such a journal but was ultimately unsuccessful (17). Had he lived longer, he well may have accomplished his goal. The result in the US was that chemists with a professional interest in their history sought outlets for publication and exchange of information with other chemist historians. The appropriate periodicals fall into two categories: periodicals devoted only to chemistry and closely related fields and to those covering only history of science. In the former category are *Angewandte Chemie, J. Chem. Educ., Chemie in unserer Zeit, and Chem. Heritage*; most of the remaining periodicals in the Table fit into the second category. Up until 1988 *Ambix* was the only periodical limited both to chemistry and history.

***Table 2.* Publications in History of Chemistry in Selected Periodicals**

Periodical	Publication Profile	Time Span
Annals Sci. (London)	4 papers./year	1991-2005
NTM	13 papers/year	1993-2003
Angew. Chem.	4 papers/year	1930-1972
Kultur Technik	45 pp/year on chemically related history	2002, 2003

Comparisons and Contrasts

The primary outlet for publications in chemical history for Americans, at the time of the founding of the HIST division and for several succeeding decades, was the *Journal of Chemical Education*. Smith published over a dozen biographies and several articles on the history of chemistry in the journal up to his death in 1928. C. A. Browne, the cofounder of HIST, authored nearly 30 articles, mostly lengthy, on subjects of chemical history. The record is probably held by Ralph E. Oesper for his publication of scores of short biographies of chemists, mostly German and French, and of a dozen longer papers on broader topics in chemistry. From its founding in 1924 through 1980, *J. Chem. Educ* typically provided 35-50 pages on chemical history annually, while only about ten pages on history appeared each year from 1981-1990. This reflects a change in editorial policy, not a diminution in scholarly activity by US chemist historians, who, like Smith back in the early 1920s, hoped for the establishment of a periodical devoted to history.

Ambix, the publication of the (British) Society of the History of Alchemy and Chemistry, founded in 1937, has provided a limited outlet for chemist historians, albeit mainly British.. In three issues per year, totaling on average 180 octavo pages, for example, nine papers were published (1991-2000). Some other periodicals, although not limited to chemistry topics, may have been overlooked by aspiring authors. These include *Annals of Science (London)*, NTM, *Angewandte Chemie (Angewandte Chemie Int. Ed. Eng.* after1962), and *Kultur und Technik*, published by the Deutsches Museum, Munich. Some features of these publications are assembled in Table 2. In the first three examples papers are published in English, but also German and French. It is noted that the extent of coverage of chemical history is indeed particularly modest in *Annals Sci. (London)* and *Angew. Chem.*

By 1980, with over 800 ACS members belonging to HIST, chemist historians in the US could reaffirm the need for alternative outlets for publication. An approach to a solution was to expand the HIST Newsletter to include essays or short papers describing research in chemical history. From this initiative there evolved the *Bulletin for the History of Chemistry*, published by the History of Chemistry Division, its creation largely a reflection of the imagination and perseverance of the founding editor, William B. Jensen. Issue No. 1 appeared in 1988, and the journal continues, now with two issues per year, Volume 32 being the 2007 offering.

Conclusion

Chemist historians seeking an attentive audience for their scholarly publications in English have two periodicals at their disposal: *Ambix* and *Bull. Hist. Chem.*, both of which are dedicated solely to the publication of subjects on the history of chemistry. Thus, 84 years after Edgar Fahs Smith expressed the strong hope for an American journal for the history of chemistry, we can say his vision has finally been realized.

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ABOUT THE AUTHOR

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CALL FOR NOMINATIONS FOR THE 2008 EDELSTEIN AWARD

The Division of History of Chemistry (HIST) of the American Chemical Society (ACS) solicits nominations for the 2008 Sidney M. Edelstein Award for Outstanding Achievement in the History of Chemistry. This award, first given in 2002, honors the memory of the late Sidney M. Edelstein, who established the Dexter Award in 1956, and continues the tradition of the Dexter Award for Outstanding Achievements in the History of Chemistry, which was discontinued after 2001. Lists of previous recipients of the Edelstein Award and its predecessor Dexter Award are available at the HIST webpage (http://www.scs.uiuc.edu/~mainzv/HIST/).

The Edelstein Award is sponsored by Ruth Edelstein Barish and family and is administered by HIST. The recipient chosen to receive the Edelstein Award is presented with an engraved plaque and the sum of $3500, usually at a symposium honoring the recipient at the Fall National Meeting of the ACS, which in 2008 will be held in Philadelphia, Pennsylvania, August 17-21. The award is international in scope, and nominations are welcome from anywhere in the world. Previous winners of the Dexter and Edelstein Awards include chemists and historians from the United States, Canada, Germany, France, the Netherlands, Hungary, and the United Kingdom.

A complete nomination consists of

- a complete curriculum vitae for the nominee, including biographical data, educational background, awards, honors, list of publications, and other service to the profession;
- a letter of nomination summarizing the nominee's achievements in the field of history of chemistry and citing unique contributions that merit a major award; and
- at least two seconding letters.

Copies of no more than three publications may also be included. Only complete nominations will be considered for the award.

All nomination materials should be submitted in triplicate to Peter J. T. Morris, Chair of the Edelstein Award Committee for 2008, Science Museum, London SW7 2DD, UK (email: peter.morris@nmsi.ac.uk) for arrival no later than December 31, 2007.

INFLUENCES OF HIST AND THE HISTORY OF CHEMISTRY ON THE COURSE OF CHEMISTRY, EXAMPLES OF SYNERGY (1, 2)

Jeffrey I. Seeman, SaddlePoint Frontiers

Introduction

> "Every journal, as soon as it's published, is history of chemistry" (3).
>
> "C&EN is the product of the work of 55 full-time journalists, most of them trained as chemists, pursuing their profession. We're proud of the first draft of history we produce each week" (4).

Happy 85th Birthday to the Division of the History of Chemistry (HIST) of the American Chemical Society (ACS)! For many decades, HIST has served as a facilitator of the study of history of chemistry. I now postulate that HIST and historians of chemistry (5) are participants in the evolution of chemistry itself.

At occasions such as this, reflection is reasonable, useful and even obligatory. We have honored HIST's 85th birthday by hosting a special symposium at the 233rd American Chemical Society National Meeting in Chicago on March 27, 2007. As we look back at our activities, we serve as historical tools for our Division. Together, the presenters have communicated their visions and their fundamental understandings for HIST and for the history of chemistry to HIST's members and others. We were pleased to have the event covered by *Chemical & Engineering News* (6). By virtue of this specially dedicated issue of *Bulletin,* we also are providing an archival resource for future members of the Division and other scholars to understand who HIST is and was in 2007. Wouldn't it have been wonderful to have had documents authored by Edgar Fahs Smith, HIST's co-founder and first Chair, from the 1920s or by Ralph Oesper from the 1940s or by Sidney M. Edelstein anytime during his long rein (1948-1964) as Secretary-Treasurer, describing their views of the mission of HIST and the role of history of chemistry in the largest context of the chemical profession?

These are fitting concepts for me to explore, and it is a fitting time and occasion to do so. Having just completed two years as HIST's Chair and, previous to that, two years as HIST's Chair Elect, I have been immersed in the strategic planning, operational planning, and leadership of HIST's activities for more than four years. I have been an eager participant in HIST's activities for many years as well. I have also been active in the field of history of chemistry for over 25 years, publishing my first study—a review of the history of conformational analysis and chemical reactivity—in 1983 (7).

Explicit and Implicit Influences of HIST and the History of Chemistry on Non-Historians

HIST's first mission statement, reprinted on page 81 of this issue of the *Bulletin* (8), was adopted on February 14, 2006 by the HIST Executive Committee (EC). HIST's mission statement describes what the Division's leadership group considers to be HIST's major goals and objectives. This statement is both reflective and forward-thinking. In 2006, the HIST EC consisted of individuals who had been past HIST Chairs and ACS Councilors over a time period of several decades. Thus, the EC's view of what HIST ought to be in the future certainly reflected what HIST had been doing – successfully, under their stewardship – in the recent and not-so-recent past. In addition, the mission statement reflects the insights of the EC based on the current environment including the new ACS mission and vision statements. When I assumed the Chair of the Division in 2005, HIST had just received the prized ACS Division Activity Committee's ChemLuminary Award for achievements by a technical division. HIST was further honored to receive a second ChemLuminary Award for its performance in 2005 and is nominated for a ChemLuminary Award for its achievements in 2006! Thus, from the perspective of its technically-oriented non-historian peers, HIST is and has been making significant contributions to the American Chemical Society and to its members for many years.

How do the activities of HIST and of historians of chemistry affect the course of science? We have all heard—and many of us have expressed—the standard "Mother and Apple Pie" value of history. That thesis is as follows: "Science is not performed in a vacuum. History helps us understand where we are and where we have come from. We must learn from the past, not repeat the mistakes of the past." As Mary Virginia Orna, a past Chair of HIST and one of HIST's ACS councilors for many successive terms, recently stated in reference to the "Proud to be a chemist – ask me why" program of the ACS, "Placing recent chemical achievements into a historical context underlies the enormous contributions that the chemical sciences have made to the well-being of billions worldwide" (9).

For all of us in the chemical sciences, the broader our knowledge and understanding of the historical context of our work, the better we are to obtain the necessary funding and other required resources, the better we are to perform our research, the better able we are to communicate our research results to others, and the better they are to understand and value our work. The shift of interest in the history of chemistry from the classical period(s) to the 20th Century makes historical context even more relevant and consequently more available psychologically and scientifically to today's research chemists. This shift, of course, is due to at least two related factors. First, that research chemists – often in the latter stages of their careers – become actively involved in the history of their own fields of chemistry. Second, younger chemists have become interested in the history of their own fields of chemistry and validate, through their enthusiasm, the history of chemistry.

I posit that the influence of HIST and of the history of chemistry on the chemical profession extends beyond these traditional roles. These influences can be either (a) explicit activities that have direct and specific intent to influence research in chemistry; or (b) implicit activities that subconsciously provide influence. In our lives, background activity is often subtle yet can be substantive and consequential. HIST's activities extend past HIST members and touch many individuals worldwide. I shall review both explicit and implicit influences, with the understanding that there are instances of overlap, i.e., the distinction I am making is often more gray than black-and-white.

Explicit Influences of HIST on Non-Historians of Chemistry

I believe that few HIST members consider themselves to be historians of chemistry (but many could, considering my definition (5) of this term!). Clearly, many HIST members have a serious interest in and value and enjoy learning history of chemistry. By virtue of their choice of HIST membership, they explicitly seek HIST's benefits to enrich and broaden their lives. I believe that one of HIST's most valuable tangible products is *The Bulletin for the History of Chemistry.* This technical journal, published twice a year, contains a wide range of high quality scholarly research articles, essays, and book reviews. It is fully appropriate that the major source of income to the Division – the yearly membership dues – goes directly and exclusively to the publication and distribution of *The Bulletin*. Of course, other explicit influences of HIST include the consequences of HIST activities, from participation in technical meetings and symposia to serving in leadership positions and involvement in HIST awards nomination and selection committees. As the Immediate Past Chair, I know that the HIST Executive Committee is eager to welcome participation and new ideas at all levels.

Implicit Influences of HIST and History of Chemistry on Non-Historians of Chemistry. Examples

Table 1 lists a range of implicit influences of HIST on non-historians of chemistry. Also included in Table 1 are actions that HIST can undertake to increase the effectiveness to these stimuli. I shall now provide a detailed discussion of two examples of implicit influences in which I was personally involved. One involves the new HIST award program, the Citation for Chemical Breakthrough award (10, 11); the second deals with my own recent research on the Woodward-Doering/Rabe-Kindler total synthesis of quinine (5, 12, 13).

A. Citation for Chemical Breakthrough Award Program

Work on this award program began several years before the first Citation for Chemical Breakthrough award was given (2006). It was absolutely necessary to touch a number of bases before a definitive proposal for the Citation award program could be brought before the HIST Executive Committee for formal approval. Each step raised the awareness and credibility of HIST and indirectly the awareness and credibility of the history of chemistry (see Table). Numerous members of the ACS staff, at the highest levels, and Chairs of ACS committees were contacted regarding the draft of the award program. Proposals were written and funded by two ACS grant programs, the ACS Division Activities Committee's Innovative Grants and the ACS Corporate Associates. One individual also made a substantial donation. In total, $13,000 was raised, sufficient to fund four or more years of awards. HIST's ability to influence the profession of chemistry increased with the strong support received from these financial sources and other supporters.

A majority of the members of the first two years' Citation award committee are chemists, not historians of chemistry. The majority of the nominations came from chemists, not historians of chemistry. Several of the 2006 awardees had symposia at their institutions which highlighted the Citation award program. A few hundred chemists attended the first award presentation at Harvard's Department of Chemistry and Chemical Biology. As can be seen from the Harvard symposium agenda (Fig. 1), the audience was educated and entertained by lectures by three Nobelists and several other illustrious chemists, including members of the U.S. National Academy of Sciences and that year's Priestley medalist. History was the topic but it was presented in the form and shape of science. Linda Wang, Associate Editor of *Chemical & Engineering News* wrote an article (10) on the event and thus, the award program reached countless other chemists. A subsequent program to honor Bruce Merrifield's development of solid phase peptide synthesis also was reported in *Chemical & Engineering News* (11). Award ceremonies of different types and flavors were held for all of the awardees (14), always arranged by the awardees' institutions, usually with the active assistance of HIST.

The Citation award plaques have been designed to reflect the image of the actual paper, patent or book being honored. For example, the first award plaque (Fig. 3), now placed prominently at Harvard, speaks to the history of chemistry. Excerpts from the front page of the 1965 Woodward-Hoffmann paper (15) form the thrust of the plaque. A carefully selected graphic from the publication adds to the visual appeal of the design. The plaque honoring Molina and Rowland's 1974 publication in *Nature* on the destruction of ozone by fluorochlorocarbons includes the title exactly as it appeared in the original article: with its typographic error! In response to seeing a draft of the plaque design, Rowland said, "I have no objections to the inscription for the plaque. I will comment specifically that the article in *Nature* was printed with a misprint in the title, as reproduced here" (16). The Gomberg plaque, honoring his evidence for and postulate of the free radical, includes the words "This work will be continued and I wish to reserve the field for myself," exactly as they appeared in *The Journal of the American Chemical Society* over 100 years ago.

An important aspect of the Citation award program is that the award is presented to the institution at which the breakthrough science was performed, not to the scientists or inventors themselves. Consequently, the plaques are being placed permanently in highly visible permanent, public locations rather than in the offices or, ultimately, homes of individuals. In addition to the motivational component of seeing these plaques, there is an educational component as well. Students will want to know more about the achievements of faculty and other students at their own institutions. Thus, history can stimulate and enhance the educational process.

Countless students, faculty, and visitors will pass by and see these plaques for years into the future. Pride will be an overt response, boosted by this historical perspective. Our hope and HIST's intent is that energy and enthusiasm for chemistry, in particular, and science, in general, will be stimulated by seeing these plaques. Some evidence is already in hand to support these goals. I have attended several receptions that followed the award ceremonies. Students and faculty alike spoke to me in glowing terms of history of chemistry and the human side of chemistry, perhaps without themselves even realizing their own excitement.

I conclude that awards, award plaques, award ceremonies, and the consequential publicity are all powerful and yet subtle influences of the history of chemistry on chemical education and chemistry itself.

Table. Areas of Implicit Influence of HIST on Non-Historians of Chemistry and the Field of Chemistry

Area Actions that HIST Can Take to Increase the Influence

The Bulletin for the History of Chemistry

- Increase circulation of The Bulletin, including to institutional libraries; encourage readership outside HIST members and historians of chemistry.
- Increase the number of reviewers for submissions, to include non-historians.
- Encourage non-historians to submit articles. Identify high value author-candidates, e.g., senior scholars, researchers who often author review articles.
- Publish ‘”modern history” special topic-driven dedicated issues of The Bulletin.
- Encourage authors to distribute copies of their published paper to non-historians. The Bulletin allows authors free rights to distribute their papers.
- Increase the number of papers that have chemical structures, thereby attracting chemists who have an interest in history.

HIST awards

- Create and strongly support awards that have significant and continuing public visibility. For example, the Citation for Chemical Breakthrough award plaque is intended to be placed in the hall outside a laboratory where a major chemical breakthrough occurred. Passers-by recognize the importance of that event and the connection with their own lives (e.g., their own undergraduate or graduate school).
- Increase nomination awareness; invite nominations from non-historians.
- Place non-historians on award selection committees.
- Have public award ceremonies. Involve non-historians in the award ceremonies, e.g., incorporate lectures or an on-site symposium into the ceremony. Encourage publicity of the award by recipient institution.

Scientific symposia

- Initiate and participate in the organization, planning and hosting of symposia outside the HIST ACS National meeting agenda. Symposia held in the meeting rooms of other ACS technical divisions will be most effective in communicating history to scientists.
- Invite non-historians to participate in history-oriented symposia, thereby creating interest in the invitees and in the audience.

Chemical & Engineering News and other technical and non-technical media

- Inform and encourage journalists to cover historical events and HIST awards.
- Interact with journalists (and congratulate them!) regarding articles they have written that either have or did not have (inform them, graciously) substantive historical context.a

As part of writing an article in the history of chemistry

- Interview and otherwise interact with non-historian chemists involved in this area of chemistry.
- Invite non-historian(s) to be collaborators in the project.
- Include sufficient chemistry and chemical structures to attract the attention of non-historians.
- Distribute reprints liberally.

A related educational experience was recounted by Jim Bohning, my fellow co-organizer of this symposium, past Chair of HIST, current HIST Historian and Archivist, and noted scholar, who recalled (17):

> I wanted students to become more aware of their surroundings and their history. The intent was the same: to increase awareness in students of their heritage and that discovery doesn't happen in a vacuum. I used to have a display on the 'Chemist of the Month' to achieve the same thing that the Citation for Chemical Breakthrough award does. Students prepared the displays as part of their course assignments. The display case was in a hallway where a lot of students passed, not just chemistry majors. I used to watch to see if anybody was reading the display, and I was not disappointed. When I taught the chemical literature course, I told the students that every time they did a literature search they were doing history, and that cumulative history would then influence how they would proceed.

In 2005, Chemical & Engineering News received HIST's Certificate of Appreciation award. See: http://www.scs.uiuc.edu/~mainzv/HIST/awards/ChemEngNews_Certificate_of_Appreciation.pdf

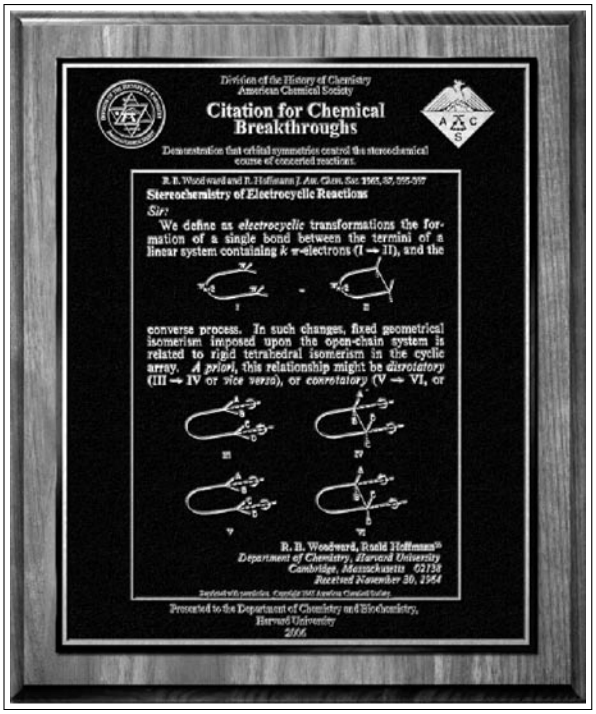
The Division of the History of Chemistry
American Chemical Society
Citation for Chemical Breakthroughs
Harvard University
Department of Chemistry and Chemical Biology
Friday, June 16, 2006
Introduction and Description of the Award
Jeffrey I. Seeman, Chair, Division of the History of Chemistry
American Chemical Society
Introduction of Frank Westheimer
George Whitesides
Message from Jeremy Knowles
Congratulatory Remarks from the University of Chicago
H. F. Fisher, E. C. Conn, B. Vennesland, F. H. Westheimer
J. Biol. Chem. 1953, 202, 687–697
R. Stephen Berry
Frank H. Westheimer

Presentation of Plaque
Introduction of Roald Hoffmann, Memorial to R. B. Woodward
R. B. Woodward and R. Hoffmann J. Am. Chem. Soc. 1965, 87, 395-397
William Lipscomb
"Breakthrough into Collaboration"
Roald Hoffmann
Presentation of Plaque to Harvard
Classics in Science and in Life
Dudley Herschbach
Reception to Follow

Figure 1. Symposium agenda for the presentation of the Citation award to Harvard University, June 16, 2006. As Frank Westheimer was on the faculty of the University of Chicago when he submitted his Citation award-winning 1953 paper cited above, he was "handed" the plaque, received applause and recognition, and then Westheimer gave the plaque to the University of Chicago where it now hangs. Frank Westheimer passed away less than one year later on April 14, 2007 at the age of 95 (36). A Citation award ceremony was held at the Department of Chemistry, University of Chicago on March 26, 2007.

Many textbooks and some teachers insert historical context into their courses. One motivation is to add the human side to the atoms and bonds and reactions of chemistry. Perhaps more historical context would be more frequently used in undergraduate teaching, and perhaps even in high school teaching, if there were more abundant, well-organized and compact, relevant free internet-based sources of information. My vision is that educational material would be provided with increasing levels of detail, technicality and sophistication. This could be done by hyperlinks clearly indicated with descriptive labels just as hikes at National Parks are described with labels of increasing difficulty! Perhaps this is an opportunity for the educational arm of the Chemical Heritage Foundation or for those academic scholars such as Carmen Giunta (18) who already have developed and maintain useful and informative history-of-chemistry websites.

Figure 2. *Roald Hoffmann and Frank Westheimer holding the Citation plaques that honor their breakthrough publications, with Jeff Seeman, at the reception following the award ceremony, Harvard, June 16, 2006. Photo courtesy of Linda Wang, Chemical & Engineering News. Copyright 2006 American Chemical Society*



Division of the History of Chemistry
American Chemical Society

Citation for Chemical
Breakthroughs

Demonstration that orbital symmetries control the stereochemical
course of concerted reactions.

R. B. Woodward and R. Hoffmann *J. Am. Chem. Soc.* 1965, 87, 395-397

Stereochemistry of Electrocyclic Reactions

Sir:

We define as *electrocyclic* transformations the formation of a single bond between the termini of a linear system containing *k* π-electrons (I → II), and the converse process. In such changes, fixed geometrical isomerism imposed upon the open-chain system is related to rigid tetrahedral isomerism in the cyclic array. *A priori*, this relationship might be *disrotatory* (III → IV or *vice versa*), or *conrotatory* (V → VI, or

R. B. Woodward, Roald Hoffmann[1b]
Department of Chemistry, Harvard University
Cambridge, Massachusetts 02138
Received November 30, 1964

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Presented to the Department of Chemistry and Biochemistry,
Harvard University
2006

Figure 3. *Citation for Chemical Breakthrough award plaque presented to the Department of Chemistry and Biochemistry, Harvard University, on June 16, 2006 to honor the first Woodward-Hoffmann paper on orbital symmetry. The design of this plaque (raised metal etching on a walnut base, 15" x 18") is similar to others presented in this award program. These plaques are intended to be placed in the halls near the location of the original discoveries.*

B. The Woodward-Doering/Rabe-Kindler Total Synthesis of Quinine

There is much to be gained by a thorough knowledge of history, philosophy and sociology of science—be it from a formal educational experience or years of participation and study of these fields. Examples of implicit influences on non-historians of chemistry can be cited as a result of publications that have historical content. At a basic level of analysis, all scientists practice some history of science when they write the introduction section of their publications and grant applications. Certainly review articles – and the entire series *Organic Reactions* -- are written by many research chemists who would never consider themselves historians of chemistry, yet these reviews fit within the umbrella of history of science. *Chemical & Engineering News* includes historical context in many of their feature articles and publishes numerous articles and even special issues focusing on the history of chemistry. C*&EN*'s readers are scientists and engineers within the broadly defined discipline of chemistry. The series of autobiographies of eminent organic chemists, *Profiles Pathways and Dreams* that I edited was produced primarily for the practicing chemist, not for the historian of chemistry, though over the long term, the latter will be the eventual beneficiary.

Consider the implicit influences of my recent publication "The Woodward-Doering/Rabe-Kindler Total Synthesis of Quinine: Setting the Record Straight" published in the scholarly research journal *Angewandte Chemie* (12). The most evident theme of this paper is the question, did R. B. Woodward and William Doering achieve the total synthesis of quinine (**1**) as they claimed in their 1944 communication and 1945 full paper entitled "The Total Synthesis of Quinine"? This reported synthesis shown in Fig. 4 was universally accepted until 2000-2001 when the eminent chemist Gilbert

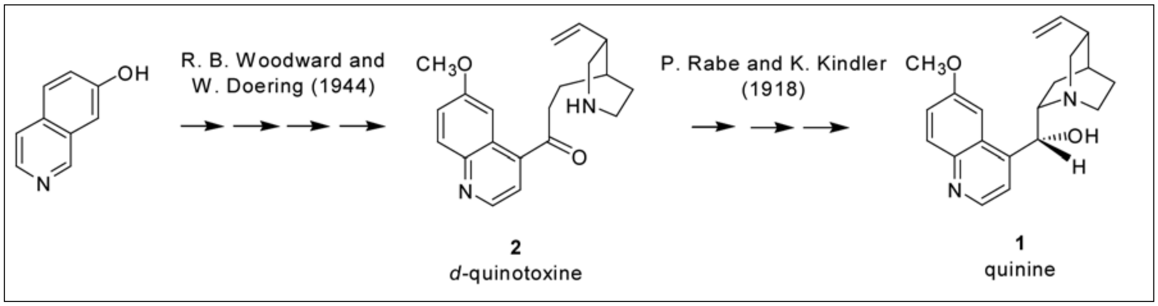

Figure 4. *A summary of the Woodward-Doering formal total syntheses of quinine and the relationship between quinine and d-quinotoxine.*

Stork asserted that it was a "myth" (19-21). Based on interviews with Gilbert Stork and others, *Chemical & Engineering News* reported in 2001 that Stork "Set the Record Straight" (22). But as Rudy Baum, current editor of *Chemical & Engineering News*, recently stated, "Journalism is often described, accurately I think, as the first draft of history" (4). Is the Woodward-Doering representation a fact or a myth?

In order to illustrate the influences of this historical study of quinine on the sociology of chemical research, I needed to use chemical structures, the universal language of chemistry. Atoms, bonds, molecules and reactions are the vocabulary of chemists! I reflect with amusement that the *Bulletin's* current Instructions to Authors states that "Chemical formulas [are] to be kept to a minimum" (23). In fact, I now assert that encouraging prospective *Bulletin* authors to include more structures would positively influence the substance of some of the articles, possibly increasing the number of submissions, and possibly increasing the readership of the *Bulletin* and membership in HIST. Chemists often first look for structures when examining a journal, not titles! In fact, many journals now include a graphical table of content in a front section of each issue.

Woodward and Doering clearly stated in their publications in 1944 (24) and 1945 (25) that they did not prepare any quinine. Rather, they synthesized *d*-quinotoxine (**2**) from simple starting materials (Fig. 4). As they said in the last sentence of their full paper, "In view of the established conversion of quinotoxine to quinine [by Rabe and Kindler] (26), with the synthesis of quinotoxine the total synthesis of quinine was complete." The use of a so-called "relay compound" such as *d*-quinotoxine and the combination of experimental work produced in two or more laboratories (Laboratory #1 converts A → M; Laboratory #2 converts M → Z) together is today termed a "formal total synthesis" (i.e., A → Z).

The problem was, and will remain forever, that Rabe and Kindler never reported the experimental details for their conversion of *d*-quinotoxine to quinine. In 1918, Rabe and Kindler (26) provided the reagents used and some important physical properties of the products. In 1911 and 1932, Rabe (27, 28) provided experimental details for analogous reactions. These facts led Stork to conclude that the assertion of a total synthesis of quinine is a myth (19-22).

There would have been no polemic had Woodward and Doering converted *d*-quinotoxine to quinine following the either information provided by Rabe and co-workers (26-29) or their own imaginative sequence—or had Rabe and Kindler reported their own experimental details. My conclusions on this controversy can be found in both my publication (12) and in the news article that appeared in *Chemical & Engineering News* (13). With this background, I now indicate several influences on chemical research that stem from my historical research.

a) I have been told that several organic chemists are considering or have begun to "repeat" the Rabe-Kindler conversion of *d*-quinotoxine to quinine.

b) As reported in *Chemical & Engineering News*, Princeton's Eric Sorensen stated, "After reading the Seeman article, I don't think I would rely on a formal synthesis. I would reproduce what was reported. I think any scientist would want to avoid any suspicion that what they did wasn't reproducible" (13).

c) Several academics have indicated to me that they intend to use my historical article as a teaching tool. For example, Sorensen wrote, "This will be required reading in my graduate course on complex chemical synthesis" (30).

d) Paul Rabe was the Professor of Chemistry at the University of Hamburg when he published his research with Kindler. I contacted the current Professor of Organic Chemistry at Hamburg, Wittco Francke, inquiring if Rabe and Karl Kindler's notebooks were in their archives. While these could not be located, Professor Francke wrote me, "Now among the legacy of [the department chemical archives], I recently found a collection of voucher samples of quinine derivatives etc. (carefully sealed in glass ampoules) which may be important as they carry the names of Rabe and Kindler (apart from some others which I suppose may be names of PhD students etc.). I think this may be a rather exciting (and important) discovery; several of these samples look as if they are in perfect shape, and one could analyze at least part of them by using modern NMR etc. What do you think" (31)? When I recently inquired of these samples, Francke wrote, "I'll have a look at the samples" (32).

e) I recently received an email from Robert Volkmann, Chair-Elect of the Division of Organic Chemistry (ORGN) of the ACS. He wrote, in part (33):

> 2008 is the 100th anniversary of a number of divisions in the American Chemical Society including the Society's largest division, the Organic Division. I along with others have been wondering how best to commemorate 100 years of scientific advances and discoveries in the field of organic chemistry. One thought would be to have a symposium and to invite leaders in the field to give presentations/stories which capture advances in their particular areas over the years and thoughts about the future. The reason for this email is because of your interest and success in capturing the rich history of our discipline. I personally think that this project might be a lot of fun and captivate the interest of our ACS membership.

This invitation to participate in the celebration of ORGN's 100th Birthday and bring the history of organic chemistry to its members is a very welcome indication of the interest and receptivity that studies in the history of chemistry can make on the research community.

Conclusions

Many of the activities of HIST and of historians of chemistry can have significant influences on the practice of chemistry and on chemical education at all levels. Including non-historian chemists into history-oriented projects can have mutually beneficial, symbiotic values. The quality and value of the projects themselves will be enhanced. Non-historians will have a broader understanding of the context in which their work has evolved. Historians will gain a broader understanding of the details of the science. Adding history content to the classroom will have similar benefits. The stimuli for these associations can come from HIST, from chemical historians, and from practicing research chemists and engineers. HIST has traditionally been oriented an outlet for its more active members. By proactively seeking collaborations with the non-history communities, HIST and its membership as well as chemical historians can have the opportunity to enhance their own activities and those of the broad chemical profession. Through collaborations among these disciplines and as a consequence of both explicit and implicit types of influences, the practice of history of chemistry and chemistry itself are being and will further be enhanced.

Coda

A reviewer of this paper stated that

> "You should clarify the difference between 'historians' and 'chemists.' I used to have a colleague in the history department at my university who publicly said that as a chemist, I had no business doing history, an attitude I have encountered more than once. Many 'professional' historians of science actually had earned undergraduate degrees in science before they went the history route. And if a scientist has done history for 20 years, is his work any less than that of the 'professional' just because he doesn't have the formal training? I think many professional historians actually look down on the work of HIST because we are 'just' chemists."

As described in Ref. 5, I believe that the most appropriate measure of a scientist is his/her professional accomplishments rather than the specifics of a scientist's formal educational heritage. In fact, one can be guilty of scientific McCarthyism (34) if one judges scientific performance on personal facts such as where an individual studied or works or was born or one's age or race "or by any other characteristic other than the content" (34). Formal education and experience merge as teachers about our universe.

I do acknowledge that I often find treatises in fields such as philosophy of science extremely difficult to understand. I attribute my difficulties in part to the 'language' used as well as to the complexities of the substance; I do wonder if my lack of formal education in these fields contributes to my bewilderment. Similarly, I can well understand that historians may not understand the 'language' of chemistry: the names of compounds, the meaning of functional groups, the capabilities of analytical methodologies, the specificities of different theories, and the vocabularies of different disciplines. Communication barriers exist between chemists from different sub-disciplines. I believe that one needs more than a passing knowledge of chemistry to fully understand, appreciate, evaluate and communicate the history of chemistry.

Surely there is a relationship between one's education experience and one's perspective. At least initially, a chemist interested in historical studies may not have the broad understanding of place and context. However, there is likely to be as much variation about the mean value of knowledge and performance among a subgroup of historians of chemistry, all of whom having history-oriented educational backgrounds, as among all historians of chemistry regardless of their educational background.

The strengths of any one discipline—indeed, of any one person—may well be the weaknesses of another. The ability to solve today's most complex problems in science and technology requires multidisciplinary teamwork, a skill not typically taught in the educational paradigms of the past but the focus of current theories in chemical education (35). Indeed, graduate students and non-tenured faculty members are taught the reverse: how to perform independent research and how to gain tenure by building one's own reputation and standing in the community.

The synthesis of these ideas strongly supports the call for collaborations among the various disciplines. Indeed, it is that call that serves as the underpinning of my paper. I believe in the value of synergy and have experienced and witnessed the development of trust and respect as a result of interdisciplinary research programs.

ACKNOWLEDGMENTS

The author thanks Professors Jim Bohning, Carmen Giunta, Seymour H. Mauskopf, and E. Thomas Strom for very helpful discussions on this manuscript and one reviewer for his frank and revealing comments quoted in the Coda.

REFERENCES AND NOTES

1. Based on a presentation at a symposium "HIST at 85: Looking Back and Looking Ahead" (J. J. Bohning and J. I. Seeman, co-organizers) before the Division of the History of Chemistry at the 233rd American Chemical Society National Meeting, Chicago, IL, March 27, 2007, HIST 41.
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HIST 2007 State-of-the-Union
A Report from the Immediate Past Chair

With the completion of my two-year term as Chair of HIST, it is both my pleasure and my responsibility to inform you as to the health and well-being of our division. By a fine coincidence, my brief report of the present and recent past HIST's activities is in a special issue of the *Bulletin* celebrating HIST's 85th birthday. Elsewhere in this issue, four members of HIST's Executive Committee (EC) provide greater detail about the division's longer term past and speculations about HIST's future.

HIST was the recipient of the ACS Divisions Activity Committee's ChemLuminary award for 2003. And HIST received a second ChemLuminary award for technical division achievement in 2005, further demonstrating the vigor of the Division of the History of Chemistry and the respect HISTis receiving from its peers. In the summary that follows, I shall list many of HIST's achievements during the past two years along with many of the individuals who are responsible for these achievements.

The years 2005 and 2006 have witnessed numerous accomplishments by HIST, including

- The development of a sound financial base and budget. We have carefully evaluated our financial position, made some hard decisions including raising dues for the first time in many years, and redirected funds with the principle of leveraging our resources to their maximum value for our membership (Vera Mainz, Jeff Seeman, and the entire HIST Executive Committee, EC).
- A completely redesigned website (Vera Mainz), up-to-date and containing many new features including historical information and important HIST archival information. For example, biographies of all Dexter and Edelstein awardees and many HIST officers are available (Jim Bohning, Tom Perfetti, Tony Travis). Updated present and recent past programming information is also included (Joe Jeffers) as well as minutes from recent EC meetings and numerous financial reports (Mainz).
- A facilitated means of communication with our members through our website regarding current division business, in particular, programming, meeting abstracts, and financial information (Mainz, Jeffers, Bohning).
- HIST's new Mission Statement (Carmen Giunta and the EC).
- HIST's new logo (Mainz) which is found on our website and HIST's official communications.
- Outstanding meeting programming (Jeffers), both in terms of a number of novel Presidential symposia (Mary Virginia Orna, Seeman) and also many other remarkable national meeting and regional meeting symposia (Books and CFC's and Priestley and the Edelstein 50th celebration, to name a few; Bohning, Roger Egolf, Giunta David Lewis, Orna).
- Obtaining external funding for many proposals (Bohning, Giunta, Orna and Seeman).
- The *Bulletin for the History of* Chemistry is stronger than ever and continues its appearance on schedule, issue after issue, with high quality publications, essays and reviews (Paul Jones and his three associate editors, Giunta, Mary Ellen Bowden,and Leo Slater). The editors and the EC made and implemented a major decision regarding copyright ownership and author submission requirements.
- HIST's awards programs are vibrant and healthy. HIST's new award, the Citations for Chemical Breakthrough award, has resulted in immediate meaning and impact to our extended community (Seeman).
- The EC determined that HIST's archives required a permanent, safe, scholarly and accessible location. Consequently, following detailed discussions with CHF and ACS (no ACS facilities were available, per discussions with Madeleine Jacobs, Executive Director of the ACS) the EC gifted HIST's Archives to CHF. The archives are now being catalogued (Bohning).

We have developed very strong working relationships with senior leadership at ACS: staff and ACS presidents. We have also nurtured important synergies with the Chemical Heritage Foundation. Two HIST-CHF music concerts at the ACS fall national meetings have spread our wings and added value to the entire ACS and given HIST special visibility (thanks to chemist-pianist Vicki Bragin; Seeman).

We are pleased to note the frequent coverage of HIST activities by *Chemical & Engineering News*.

HIST's greatest strength is its members and our diversity of interests, experiences, and talents. Perhaps this can be best exemplified by the remarkable differences of our past few Chairs, the current Chair, and our newly elected Chair-Elect. But the greatest similarities, which we all share, are our commitment to HIST, our passion to support HIST's mission, and our promises to our members and to the ACS.

It has been a pleasure for me to have helped lead HIST forward these past few years. It has been an honor to work with and share many experiences with so many of you.

Jeff Seeman

LOOKING AHEAD: KEEPING HISTORY OF CHEMISTRY RELEVANT TO THE FUTURE OF CHEMISTRY (1)

Carmen J. Giunta, Le Moyne College

Introduction

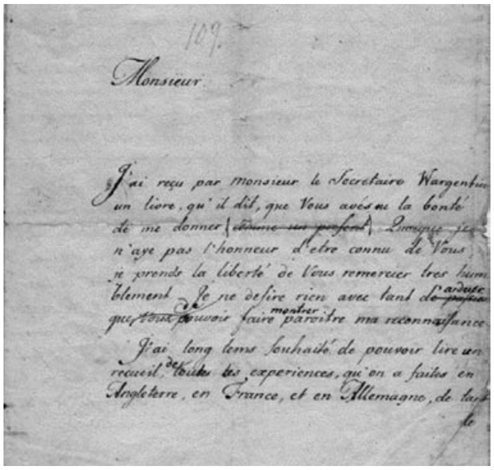
Where is HIST headed? What lies in its future? These were some of the questions I was invited to contemplate as a participant in the symposium, "HIST at 85: Looking Back and Looking Ahead." Rather than embrace such a notoriously difficult task as prediction, I turned to a more tractable and in many ways more interesting question: the relationship of a past-oriented organization like HIST to the future. For clearly HIST *is* interested in the future. Part of the concern of HIST members with the history of chemistry is a sense of stewardship of the legacy of chemistry to pass on to the present and future of the discipline. Furthermore, part of the mission of HIST is to help the larger American Chemical Society (ACS) achieve its vision and missions (2), and those are certainly future oriented (3).

How, then, do HIST, its members, and similar organizations focused on the past engage the future? This paper will touch on three answers: using new media to disseminate historical content, providing historical perspective on current issues and events of the recent past, and interpreting the past in educational materials for future chemists.

New Media

The internet has put powerful publishing and broadcasting tools into the hands of the many. Websites are no longer new media, but they will continue to be important means of distributing information of all sorts. In recent years, the HIST website (2) has dramatically increased its content. In addition to serving as a portal for divisional information, such as the newsletter, programs for national meetings, and Executive Committee records, it is a repository of records relating to the division's journal (this journal), awards, and other business. The website includes indices of this journal and biographical sketches of Dexter and Edelstein award winners. Electronic access to back issues of this journal is a likely development before too much longer. Electronic communication for and among the HIST membership continues to be a topic of discussion among the executive committee, and the division website will continue to serve as an important vehicle for that communication.

Looking at the internet beyond HIST, one can see that some large electronic databases now available include raw materials of history of chemistry. The emergence of digital libraries of archival materials is a welcome development. Sites such as Panopticon Lavoisier (4) at the Institute and Museum of the History of Science in Florence and the Ava Helen and Linus Pauling Papers at Oregon State University (5) have made images of notebooks, letters, and the like available on line. Among the images one can find at Panopticon Lavoisier is a 1774 letter (Fig. 1) in which Carl Scheele thanks Antoine Lavoisier for a book and gives a recipe for an air modern readers would know as oxygen. A 1952 telegram from the American State Department to Linus Pauling (Fig. 2) regarding a passport application



109.

Monsieur

J'ai reçu par Monsieur le Secretaire Wargentin
un livre, qu'il dit, que Vous avés eu la bonté
de me donner (~~comme un present~~) Quoique je
n'aye pas l'honneur d'etre connu de Vous
je prends la liberté de Vous remercier tres hum
blement. Je ne desire rien avec tant d'ardeur ~~de passion~~
que de ~~tous~~ pouvoir faire montrer paroitre ma reconnoissance.
J'ai long tems souhaité de pouvoir lire un
recueil de ~~toutes~~ les experiences, qu'on a faites en
Angleterre, en France, et en Allemagne, de ta
le

Figure 1. *1774 letter from Carl Wilhelm Scheele to Antoine Lavoisier, from Panopticon Lavoisier. Carl Wilhelm Scheele's archives, F1:1, Center for History of Science, Royal Swedish Academy of Sciences. Copyright © Royal Swedish Academy of Sciences*

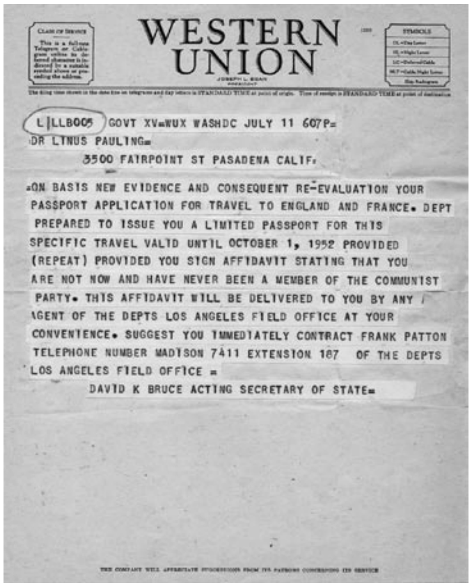
is among the documents displayed at the Pauling papers site in an exhibition about the competition to find the structure of DNA; it illustrates the role of loyalty oaths in US anti-communist efforts. These and other digital library projects allow scholars and students to examine unique materials like these without having to travel.

More broadly based digital libraries such as Gallica (6) at the French National Library, Google Books (7), and several patent libraries already contain much of interest to historians of chemistry and history-minded chemists, and that content will continue to expand. In addition to what one might expect to find in Gallica, such as a long run of *Comptes rendus* and books by French chemists, there is also relevant material in English, including a long run of the *Philosophical Transactions of the Royal Society* (London). Thanks to its partnership with several of the world's premier research libraries, Google Books contains many older works of chemistry and history of chemistry. Both of these libraries contain page images of complete books and journals, mainly from the early 20th century and earlier. Coverage at present is hit and miss. For example, at the time of writing, John Dalton's *A New System of Chemical Philosophy* (1808) was not available at Google Books but Humphry Davy's *Elements of Chemical Philosophy* (1812) was. Google Books aims eventually to produce a comprehensive resource.

Patent images have been available on the Internet for some time from US and European patent office sites (8, 9). These databases have extensive search utilities for patents issued after 1976. Users of these databases can access images of patents issued before then—but only if they know the patent number from some other source. Chemists have long had another source, *Chemical Abstracts*, at least for chemical patents issued after 1907. Google's database of US patents (10) can be searched in full text, at least to the extent that the scanned patents have been properly translated into text by optical character recognition.

New digital media include blogs, wikis, podcasts, and video. A blog (from the phrase "web log") is a website containing serial entries, often on a particular theme or subject. A wiki is a website that allows many users to edit its content and sometimes its form. A podcast is a digital file or series of such files, usually audio or video, distributed over the Internet for playback on a personal computer or portable media player (such as an iPod). Video is not a new term, but the ease with which digital video can now be created and distributed on the Internet makes it in many ways a new medium.

There is not much history of chemistry content currently available in these forms, but there is some. The Chemical Heritage Foundation (CHF) supports a weekly podcast called Science and Society (11). The series is not devoted exclusively to chemical issues—let alone history of chemistry—but there is certainly a lot of chemistry in the topics the series uses to define itself, namely "medical breakthroughs, energy and the environment, space exploration, nanotechnology, and K-12 science educa-



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=ON BASIS NEW EVIDENCE AND CONSEQUENT RE-EVALUATION YOUR PASSPORT APPLICATION FOR TRAVEL TO ENGLAND AND FRANCE. DEPT PREPARED TO ISSUE YOU A LIMITED PASSPORT FOR THIS SPECIFIC TRAVEL VALID UNTIL OCTOBER 1, 1952 PROVIDED (REPEAT) PROVIDED YOU SIGN AFFIDAVIT STATING THAT YOU ARE NOT NOW AND HAVE NEVER BEEN A MEMBER OF THE COMMUNIST PARTY. THIS AFFIDAVIT WILL BE DELIVERED TO YOU BY ANY AGENT OF THE DEPTS LOS ANGELES FIELD OFFICE AT YOUR CONVENIENCE. SUGGEST YOU IMMEDIATELY CONTRACT FRANK PATTON TELEPHONE NUMBER MADISON 7411 EXTENSION 187 OF THE DEPTS LOS ANGELES FIELD OFFICE =

DAVID K BRUCE ACTING SECRETARY OF STATE=

Figure 2. *1952 telegram from the US State Department to Linus Pauling, from the Ava Helen and Linus Pauling Papers, Special Collections, Oregon State University.*

tion." The podcast has featured two presidents of ACS (Catherine Hunt in February 2007 and William Carroll in October 2006) and several guests from CHF.

The best known example of a wiki is Wikipedia (12), the web-based encyclopedia written and edited by volunteer contributors from around the world. There are many Wikipedia articles on scientific topics, and some of these include historical material. Evaluating the reliability of resources is a persistent problem for users of all sorts of Internet media, and Wikipedia has been a prominent lightning rod for this sort of criticism. The very democratization of the means of production and distribution of media that excites many scholars and educators about the Internet means that these media are highly uneven in quality. (Wikipedia's slogan "the free encyclopedia that anyone can edit" epitomizes both the promise and the problem.) One wiki that is both a reliable Internet site and a guide to reliable sources in chemistry is the chemical information wiki at Indiana University, prepared largely by chemistry librarian and chemical information specialist Gary Wiggins (13). Knowledgeable guides like Wiggins and his colleagues provide valuable direction in the forest of chemistry resources (Internet and otherwise), and their wiki includes some sources on chemical history.

Although there is currently precious little history of chemistry in blogs, wikis, podcasts, and video, it would be foolish to extrapolate that paucity into the future. After all, the few **websites** that had history of chemistry content ten years ago were not a reliable indicator of the sites available today.

Engaging the Present: Perspectives on Current and Recent Events

One straightforward prediction about the future of history of chemistry is that historians will have new stories to tell as the present turns into the past. The 2007 and 2006 recipients of the Edelstein award for outstanding achievement in the history of chemistry, Anthony Travis and Peter Morris respectively, have both concentrated on chemistry and chemical industry of the 20th century, which has itself only recently receded into the past.

Stories of the recent past can capture the attention of chemists of the present. The current generation of chemists can identify with stories of their teachers, mentors, and older colleagues, even if they are not more generally interested in the history of their field. A recent example of chemical history that made a big splash in the contemporary chemical community is Jeffrey I. Seeman's article on the first formal synthesis of quinine (14). Seeman's first public venue for this story was rather modest, a presentation in the HIST program at the 232nd National Meeting of ACS in San Francisco in 2006 (15). Meanwhile, his paper was working its way through the peer review process at a leading journal devoted to current chemical research, *Angewandte Chemie*; that paper was published early in 2007. Shortly thereafter, *Chemical and Engineering News* picked up the story, thereby reaching a still larger audience through a news article (16) and an editorial (17). In fact, *Chemical and Engineering News* had previously devoted a fair amount of ink to quinine synthesis, as well as to other topics of recent chemical history. Both Seeman and Rudy Baum, editor-in-chief of *Chemical and Engineering News,* addressed the relationship of history of chemistry to the present at the symposium, "HIST at 85: Looking Back and Looking Ahead" (18, 19).

HIST programming at ACS national meetings has often included symposia that focused on the recent past or that treated a subject over time up to the present. Recent examples include a symposium Seeman organized with the eminent analytical chemist Daniel Armstrong for the 226th National Meeting in New York (Fall 2003) on 100 years of chromatography. The primary sponsor of that event was not HIST but the Division of Analytical Chemistry. The anniversary was the jumping off point for a series of talks that included very recent developments. At the Spring 2005 meeting (229th National Meeting in San Diego), Carmen Giunta organized a symposium on the rise and fall of chlorofluorocarbons, which told the story of those compounds from their invention as safe refrigerants to their ban as threats to the global ozone layer and their subsequent replacement. That symposium included accounts by prominent participants as well as recent studies on replacement compounds.

Mary Virginia Orna is responsible for the most recent examples. "Going with the Flow: Water Sustainability Past, Present, Future" fit into one of the multidisciplinary themes of the 233rd National Meeting (Spring 2007, Chicago), namely sustainability. It was the third of four symposia Orna organized around areas of current or recent applied chemical research. All were intended to further aims articulated by ACS presidents Ann Nalley and Catherine Hunt when they first took the office of President-Elect, and all were designated Presidential Events at their respective meetings. The previous two were on "Health Materials and Techniques" (232nd National Meeting, San Francisco, Fall 2006) and "Pharmaceutical Research and Development" (231st National Meeting, Atlanta, Spring 2006). The subtitles for both 2006 symposia, "Investment in Basic Research Leading to Benefits for Society," were designed to complement Nalley's emphasis on stories about chemistry's benefits to society. The remaining symposium in the series, which will celebrate 100 years of Chemical Abstracts, is set for Boston in fall 2007 (234th National Meeting).

CHF's Center for Contemporary History and Policy is another example of an organization devoted to the history of chemistry engaging in recent and current issues. Its areas of major initiative are biotechnology, electronic materials, environment and risk, innovation, and pharmaceutical policy and politics.

In addition to being current, these areas are all highly interdisciplinary. Interdisciplinarity is an important current aspect of the practice of chemistry, one that has attracted highly visible notice within ACS. Attention to interdisciplinarity appears to reflect a growing awareness that nature does not recognize disciplinary boundaries. Different disciplines bring different approaches, tools, and assumptions to problems. Conventional wisdom holds that multiple perspectives can be combined to generate more robust understanding.

Chemistry has long had porous boundaries with several other scientific disciplines, so historians of chemistry are used to dealing with scientific interdisciplinarity. At the moment, the boundary with biology attracts the most attention, raising questions such as whether one has to be a chemist to win the Nobel Prize in chemistry and what is a chemist anyway? For example, the 2006 laureate in chemistry, Roger D. Kornberg, is Professor of Medicine at Stanford University Medical School's department of Structural Biology. He was awarded the prize "for his studies of the molecular basis of eukaryotic transcription" (20). Historical perspective can shed light on what makes the current chemistry-biology boundary unique and what about it resembles other interesting boundaries in chemistry's past. (Recall, for instance, that nearly 100 years ago, the winner of the Nobel Prize in Chemistry was someone who certainly did not consider himself a chemist, namely Ernest Rutherford.) Historical case studies can shed light on interdisciplinary interactions, including successful syntheses and instances of interdisciplinary ignorance and disciplinary blinders. Such studies can cast light on interdisciplinarity, a topic of current interest, without necessarily drawing upon current or recent cases. Historical approaches to science can be valuable in understanding interactions between established disciplines as well as the emergence of new disciplines. Both phenomena are of current interest, but neither is an entirely new phenomenon.

Educational Materials

History of chemistry has a role in educating future chemists and citizens. The National Science Education Standards promulgated in 1996 include standards on the history and nature of science. In 1997 ACS published a reader and resource manual for high school teachers called *Chemistry in the National Science Education Standards*. Mary Virginia Orna wrote the chapter on the history and nature of science standards (21). A new edition is in preparation, and the corresponding chapter is being written by Seth Rasmussen, Carmen Giunta, and Misty Tomchuk (22).

The standards on the history and nature of science include understanding science as a human endeavor. This is an area in which classroom materials produced by CHF excel. From *Chemical Achievers* (23) to "Her Lab in Your Life" about women in chemistry (24) to "The life and science of Percy Lavon Julian" (25), CHF has put well researched history of chemistry and inspirational characters and stories into resources and activities that teachers can use. In addition to materials specifically designed for the classroom, websites that include photos and biographical sketches of chemists further this educational aim by providing easy access to supplementary information for teachers and students.

Making historical materials available for chemistry education will continue to be a way that chemists and educators interested in their history attempt to affect the future of chemistry. Translating history into educational materials seems to be a perennial activity, done in different ways and with different media in each generation. It is also an area in which individual teachers or small groups produce materials for their own classrooms and possibly for wider distribution as well.

Here are just two examples of such individual projects. James and Virginia Marshall have been working for several years on a project called "Rediscovery of the Elements." The ultimate product of the project, expected around 2010, is a DVD tour of sites associated with the isolation and characterization of the elements (26). Classic Calculations, available on Carmen Giunta's Classic Chemistry website, is a collection of quantitative problems that use data from historically important chemists, an attempt to bring history into the chemistry classroom in a way that supports the main learning objectives of introductory chemistry courses (27).

Conclusion

HIST members and the broader community of chemists interested in their history may well have one eye fixed firmly on the past, but they are interested in the present and future as much more than eventual fodder for history. At least in part, that interest in the present and the future is a consequence of this community's commitment to chemistry. *Chemists* interested in their history are still chemists, committed to their discipline. This paper has touched on a few ways in which this community is and can be engaged with the present and talking to the future. HIST and other history-oriented chemists can use new technologies for communicating as they come along, not for the sake of novelty but to the extent that they serve the purposes of scholarship and education (28). They can influence the future by engaging and informing the present generation of chemists about the past, particularly about their own past—about fields in which they are active and about the achievements of their teachers, their mentors, and their institutions. They can influence the future by instructing the next generation of chemists about their heritage.

ACKNOWLEDGMENTS

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28. One of the referees expressed a concern that the availability of materials on the internet could have an adverse effect on the quality of historical scholarship. In effect, this referee cautioned that the availability of such low-hanging fruit might divert scholarly attention from more thorough scholarship by way of traditional sources. It remains the duty of scholars, including conscientious reviewers, to uphold the values of solid scholarship. I am grateful to this referee for providing a concrete example of a way in which new digital media may not necessarily serve our purposes.

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WEBSITES

HIST: http://www.scs.uiuc.edu/~mainzv/HIST

CHEMICAL HERITAGE: http://www.chemheritage.org

HISTORY OF SCIENCE SOCIETY: http://www.hssonline.org

LETTER TO THE EDITOR: "Cheese or Flu?"

I would like to comment briefly on some of the points raised by Masanori Kaji in his recent review (*Bulletin*, **2007**, *32,* 58-59) of my book *Mendeleev on the Periodic Law*. Anyone who has studied the literature on Mendeleev knows that it is filled with contradictory claims concerning even the most rudimentary facts of his life, such as his birth order or the number of his siblings. Thus the older English language sources (1-3) on Mendeleev are unanimous in claiming that the reason Menshutkin read Mendeleev's paper at the 18 March 1869 meeting (4) (all dates are westernized) of the Russian Chemical Society was because Mendeleev was ill at the time, not because he was away on a trip inspecting cheese cooperatives as claimed by Gordin (5). According to Leicester, who was able to directly read the Russian literature, the story of Mendeleev's illness is traceable to a 1908 biography of Menshutkin by his son (1)—a claim which the son also repeated in a letter to the British journal *Nature* in 1934 (6):

> Mendeléeff prepared his first essay, "Correlation of the Properties with the Atomic Weight of Elements" in early March 1869 intending to communicate it to the Russian Chemical Society ... at the meeting on March 6 [i.e. March 18]. Illness prevented him from attending, and the paper was read, at his request, by my father, Nikolai Alekansdorvic Menschutkin, at the time Professor of analytical chemistry at the University of St. Petersburg.

Gordin, on the other hand, fails to document the source of his claim concerning the cheese cooperatives and the earliest source I could trace for this story is a 1974 article by Kedrov (7). However this author contradicts this claim in an earlier and more detailed account in which he explicitly states that the cheese cooperative inspection trip was actually scheduled to occur between 1 March and 12 March 1869, thus allowing Mendeleev plenty of time to attend the meeting of the Russian Chemical Society on the 18th of March (8). According to this account, the cheese inspection trip was pertinent not because it interfered with Mendeleev's presence at the meeting but because it interfered with his resolution of the final form of his periodic table, which he completed on 1 March and rushed to the printer, thus causing him to delay his departure for the inspection trip by one day.

Thus on the whole, I concluded that the illness story was the better documented of the two. Though there may well exist Russian documents that prove otherwise, no mention of them was provided by either Gordin or Kedrov, nor could I find any in the available German or translated Russian literature. In any case, since my book was not a biography of Mendeleev, I did not feel it was pertinent for me to wallow in this kind of trivial detail in either the body of the text or in an extended footnote.

I might further note that Gordin's otherwise valuable biography is not as infallible as Kaji implies. Thus Gordin also follows the 1974 article by Kedrov in translating the title of Mendeleev's first flyer on the periodic table of 1 March 1869 as "An Attempt at a System of Elements Based on their Atomic Weights and Chemical Affinity." Though it is true that the term affinity is a rough synonym for "kinship" or "likeness," the term "chemical affinity" also has a very specific technical meaning in the chemical literature, where it refers instead to the relative stability of a chemical compound. Since in his paper of 18 March Mendeleev specifically rejected the use of chemical affinity orders as the basis for chemical classification, the Kedrov-Gordin translation is very misleading; and the intent is much better conveyed by Kamensky's 1905 translation as "An Attempted System of the Elements Based on their Atomic Weights and Chemical Analogies." On the other hand, Mendeleev's own translation of his title into French as "Essai d'une système des éléments d'aprés leurs poids atomique et fonctions chimiques" suggests instead the idea of chemical properties or behavior rather than affinity or analogy (9).

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“MOLECULAR” VERSUS “COLLOIDAL”: CONTROVERSIES IN BIOLOGY AND BIOCHEMISTRY, 1900–1940*

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Introduction

The development of molecular concepts in biology is contrasted mainly with the morphological research of cell biologists and the work of classical geneticists. This has overshadowed a major controversy that, for several decades, shaped the pre-history of what was later called molecular biology: namely the controversy between the molecular and the colloidal conception of the micro-structure of cell components. Whereas the molecular conception was based on the notion developed in the 19th century of macromolecules—that is, large molecules with physical and chemical individualities, the competing colloidal theory treated antibodies, enzymes, other proteins and DNA not as macromolecular entities but as colloidal aggregates of a changing composition.

Historians’ assessments of the controversy are at variance. According to biochemist-historian Marcel Florkin, the impact of biocolloidy was primarily detrimental because of the pseudoscientific nature of its theories. The search for deeper information on the relations of structure and function was alleviated, according to Florkin, in “irrelevant theories” related to surface actions, electric charges, and adsorption. Thus biocolloidy retarded the development of scientific biochemistry. Florkin called the period in which it strongly influenced biologists’ and biochemists’ work “the dark age of biocolloidy” (1). Florkin’s clear-cut negative assessment has not been shared by other historians. Joseph Fruton, like Florkin a biochemist and historian, despite highlighting basic flaws of biocolloidy, contradicted Florkin, because in his opinion Florkin dismissed the importance of the physical-chemical approach, in particular the introduction of the concept of adsorption by colloid chemists (2). Surprisingly, however, Fruton also pointed out that the concept of adsorption had been developed long before it was used by colloid chemists (3). Robert Olby considered the emphasis on surface phenomena a positive result of colloid chemistry, though he admits that most of the colloidal work before 1930 has been rejected (4).

Florkin, Fruton, and Olby are in agreement that scientific factors decided the eventual “victory” of the concept of macromolecules over that of colloidal aggregates. In contrast, Pauline Mazumdar emphasizes the role of social factors in determining the outcome of colloidal–molecular controversies that she studied in the history of immunology (5).

What follows is an examination and analysis of major controversies between protagonists of a *colloidal* and a *molecular* biology and biochemistry from 1900 to 1940 (6). They deal with the crucial question, over these four decades, of biological specificity; that is, the specificity of antibodies, enzymes, and what was shown to constitute both, proteins. Controversies in chemistry over the existence and properties of macromolecules largely focused on carbohydrates, though by the late 1920s proteins were included. Notwithstanding the fact that the controversies took place mostly in separate scientific communities, the influences were mutual. Thus the controversy among chemists over the existence of macromolecules, and in particular its eventual outcome, strongly influenced the controversies in biology.

Here, I highlight the scientific and nonscientific aspects of the controversies, review their roles in determining the outcome, and evaluate their impact on the advance of science and the attitude of scientists. I begin with a short review of the early history of macromolecular and colloidal chemistry insofar as it is related to developments in biology and biochemistry (7).

Changing Notions of the Constitution of Biologically Active Macromolecules—a Short Overview

1. The idea of polymeric organic molecules in the 19th century and its decline

Kekulé's theories of the four-valent carbon atom and the existence of linear C-C chains (1858) mark the beginning of structural organic chemistry, in which spatial structures were attributed to molecules. During the following decades physiologists and chemists came to believe in the existence of large organic molecules that were held together by what we now call covalent forces. By 1900 chemists had obtained several protein molecules such as hemoglobin in crystalline form. But because of the uncertainty of evidence concerning protein size and structure—there were no appropriate methods to determine their molecular weight and study their structure—and because of seemingly promising alternative hypotheses, the concept of large molecules lost support after 1900 (8). Some protein chemists, however, among them Thomas B. Osborne at Yale, continued work on the preparation of crystalline proteins and their chemical compositions on the assumption that proteins were discrete large molecules.

The rise of physical chemistry and its focus on electrochemical explanations turned attention away from Kekulé's "main valency bond" (later covalent bond), stressing instead the importance of the much weaker physical bonds. The creation of the theory of *Haupt-* and *Nebenvalenzen* (primary and secondary valences) in chemical coordination theory by the inorganic chemist Alfred Werner (1902) also encouraged the viewpoint that polymers were not large molecules but rather aggregates of small molecules held together by secondary physical bonds. The first X-ray studies of polymers were regarded as supporting the view that the so-called high molecular weight substances were in fact aggregates of small crystalline units (micelles, see below).

Emil Fischer, arguably the most eminent organic chemist at the turn of the 20th century, contributed to chemists' neglect and rejection of the concept of macromolecules. He would have accepted the notion of giant organic molecules but did not consider the available evidence for their existence conclusive. Moreover, he did not consider them necessary in order to account for the assumed diversity of proteins, given the large numbers of possible isomers. Fischer's hypothesis that proteins consist of 30 to 40 amino acids, with molecular weights of probably no more than 4000 to 5000 (9) remained prevalent even after his death in 1919. At the same time, the eminent British biochemist Frederick G. Hopkins advised biochemists to deal "not with complex substances which elude ordinary chemical methods, but with simple substances undergoing comprehensible reactions"(10). Following Fischer and Hopkins, most organic chemists focused on the study of small molecules. The study of biologically active "high molecular weight substances" increasingly became the domain of physical chemists and biochemists, who were strongly influenced by colloid chemistry.

2. The development of the chemistry of "colloids"; the ris e of "biocolloidy"

The term "colloid" was coined by the British chemist Thomas Graham in 1861 to describe the "pseudosolutions" such as silver chloride or starch described by Francesco Selmi in 1854. Colloids were characterized by a low rate of diffusion through membranes that were permeable to salt solutions, a lack of crystallinity and sedimentation, and a size of at least 1 nm in diameter (in modern terms), and an upper size limit of approximately 1μm. Until around 1900 colloids remained, as Servos has emphasized, an esoteric topic (11). Then a new interest in colloids arose which led to a flourishing of colloid science in various areas of research until around 1930.

Several reasons may account for the fact that the concept of large molecules lost support after 1900. The advance of novel techniques, for example the availability of new filtration methods and the ultramicroscope, led to a new interest in colloidal phenomena, at first primarily by inorganic chemists. Biologists and biochemists turned to colloidal science because it offered seemingly promising alternative explanations for those basic life phenomena that structural organic chemistry either did not deal with at all or tackled with complicated methods and uncertain results. For example evidence concerning protein size and structure was ambiguous, relying on difficult methods to determine molecular weight, and without appropriate methods to study their structures (12). Many biologists and biochemists were attracted by the assumption that the phenomena of life followed colloidal laws and could not be explained by structural organic chemistry. Apart from the lack of convincing and convenient alternative practices and the appeal of the new concepts that promised rapid results without the tedious study of the chemistry of substances and processes, the missionary zeal of zoologist-turned-colloidal-scientist Wolfgang Ostwald and his success as discipline builder contributed strongly to the growth of biocolloidy.

Industry was another area where colloid chemistry was in vogue for many years. It was applied in the soap, tobacco, and textile industries. Colloidal chemists empirically succeeded to improve, for example, surface coatings, ceramics, and emulsions. In Germany, at least, the institutionalization of colloid chemistry at universities was in part funded by industry (13).

Among the biologists or physiologists who pioneered the use of colloid chemistry were Carl Nägeli, Franz Hofmeister and, a little later, Wolfgang Ostwald in Germany, and Wilder Bancroft in the United States. A central concept was that of colloidal aggregates, often called micelles, relating to the *Micellartheorie* of Carl Nägeli (14). According to this speculative theory, the “organized substances” in the protoplasm, such as proteins, consisted of crystalline “primary” particles—micelles—that were surrounded by hydration shells. Physiological chemist Franz Hofmeister compared living systems to gelatin that was considered to be a colloid. As colloids, these systems would not possess osmotic properties nor follow the chemical laws of solution.

The main promoter of colloid chemistry in Germany was Wolfgang Ostwald, a son of the physical chemist and Nobel laureate Wilhelm Ostwald (15). His 1915 textbook *Die Welt der vernachlässigten Dimensionen* (The World of Neglected Dimensions) marked a breakthrough for his “biocolloidy.” Ostwald here developed further the theory of the colloidal state of matter, applied to substances that in other respects have nothing in common, such as proteins, gold, soap solutions, solutions of tannic acid, etc. All biochemically relevant substances of the cell such as proteins, enzymes, and nucleic acids were regarded as biologically active colloidal aggregates of undetermined composition. Physiological processes, such as muscle contraction, were explained by increasing or decreasing dispersion of colloids.

In the United States the physical chemist Bancroft became a leading figure in colloid chemistry (16). The physiologist Martin Fischer, an obscure scientist with strong ties to Wolfgang Ostwald, not only strongly propagated colloid-chemical explanations of biological phenomena but also used them in medical treatments in his private practice (17).

During the early 1920s the German organic chemist Hermann Staudinger initiated the concept of macromolecular chemistry. His demonstration of the existence of large molecules held together by covalent bonds, first opposed by almost all of his colleagues, became increasingly accepted after the late 1920s. Biocolloidists were dealt a particularly severe setback when the Swedish colloid chemist Theodor Svedberg, between 1926 and 1930, demonstrated (by means of the ultracentrifuge) the macromolecular nature of hemoglobin and other proteins (and “converted” to macromolecular chemistry). Later findings that many macromolecules include subunits linked by noncovalent bonds did not upset the notion of individual large molecules with well defined physical and chemical structures.

Controversies

1. Controversy over the nature of antibody specificity (around 1900)

The controversy over the nature of antibody specificity started after the publication of Paul Ehrlich’s “side-chain theory of cellular action” (1878) and his further development of the concept of selective affinity, which later became his receptor theory (18). These were inspired by Otto N. Witt’s 1875 theory of color and constitution (19). Ehrlich explained biological specificity, including antibody specificity, entirely in terms of structural chemistry. The idea of a chemical “receptor” gained further prominence through Emil Fischer who, impressed by the stereospecificity of the cleavage of glycosides by enzymes, in 1894 coined the “lock and key” analogy of enzymes and their substrates (20). Ehrlich’s theory implied a sharply defined immunological specificity and a tight binding of the antigen to the antibody. He assumed that lymphocytes possess receptors that combine with antigen and that these receptors are specific because of preformed atom groupings. The theory was rejected by those who preferred explanations based on quantitative differences and denied the existence of sharp specificity in nature. One of them was Max Gruber, professor of hygiene at the University of Vienna, a student of Carl Nägeli, and Max Pettenkofer, both of whom were also strongly opposed to another concept of sharp specificity, the notion of distinguishable bacterial species, put forward by Ferdinand Cohn and, later, Robert Koch (21).

Colloidal chemists, whose explanations relied on physical concepts such as adsorption and electrical properties, which allowed only for weak specificity, joined Ehrlich's critics. Most outspoken was Wolfgang Pauli in Vienna who turned the controversy into a battle for superiority of colloid chemical over structural chemical explanations of biological specificity. Pauli considered Ehrlich's structural chemistry to be outdated and claimed that there was a colloid-chemical explanation for every single phenomenon in biology and medicine, including immunology. He presented the electrochemical theory of immune affinity developed by Karl Landsteiner as superceding Ehrlich's side-chain theory (22). Landsteiner, a student of Max Gruber, who had received additional training in structural chemistry with Emil Fischer, early on applied colloid chemistry to immunology. He assumed that the affinity between antigen and antibody was due to their electrical charges. For him, however, electrical explanations did not conflict with chemical ones, because he realized the dependence of adsorption on the chemical nature of the adsorbing substance (23).

Collaborators or former students of Ehrlich, such as Heinrich Bechhold and Leonor Michaelis, strongly rejected Pauli's and other colloidists' one-sidedness and their far-reaching claims, criticizing Pauli's disregard of the importance of organic chemistry for biomedical research. Responding to Pauli's reproach that the chemical groups of Ehrlich's side-chains were only fiction, they invoked the authority of Emil Fischer and pointed to the similarity of the "lock and key" and the "receptor" (side-chain) concepts (24).

The controversy ended in an interesting way. The far-reaching claims of the biocolloidists became forgotten after some years. An originally strong supporter of colloid chemical ideas, Landsteiner, by abandoning colloid chemistry, played a crucial role in the further development of immunochemistry in the 1920s. By synthesizing a variety of small organic molecules (haptens) and conjugating them with large carrier molecules to form antigens, which specifically reacted with antibodies, he demonstrated the chemical specificity of the antigen-antibody reaction, thus confirming a central aspect of Ehrlich's assumption (25). But Ehrlich's theory had to be modified later on: the chemical bonds between antigen and antibody were not, as he had thought, "primary valency bonds" (covalent bonds) but consisted of a combination of various weak bonds such as ionic, hydrogen, hydrophobic, and van der Waals bonds. At first sight, this development might suggest that in the end, molecular and colloidal concepts converged. But a closer view shows that this was not the case. Colloidal chemists did not contribute at all to the examination of the weak bonds that were so important for their aggregate theory. Rather the clarification of the role of these forces in biologically relevant compounds and reactions started from the concepts of molecules and specificity. Theoretical chemist Linus Pauling, who explained the covalent bonds between atoms and molecules in terms of quantum mechanics (26), also contributed greatly to the clarification of the role of weak bonds in macromolecules. Applying X-ray diffraction studies and calculations, he was instrumental in elucidating the role of weak bonds in macromolecules. In 1936 he suggested that hydrogen and other weak forces determine the three-dimensional structure of proteins and were thus a pre-requisite for their function and biological specificity (27). His hypothesis, notably, was based on the notion of proteins as macromolecules (28):

> Our conception of a native protein molecule (showing specific properties) is the following: The molecule consists of one polypeptide chain which continues without interruption throughout the molecule (or in certain cases of two or more such chains); this chain is folded into a uniquely defined configuration.

In 1952 he proposed the α-helix as a structural element in globular proteins, with hydrogen bonds as the most important weak association in the structures of molecules; a year later James Watson and Francis Crick suggested the double helix structure of DNA, with the two chains linked by hydrogen bonds. The paths to the clarification of the important role of weak forces in the compounds and reactions underlying biological specificity successfully bypassed colloid chemistry.

2. The nature of enzymes

The history of enzymology is another area of biochemical and biomedical research that was shaped by a number of controversies including, those over molecular and colloidal explanations for enzyme action. After the concept of biocatalysis was first proposed by Berzelius in 1837, a bitter dispute arose between Louis Pasteur, who claimed that alcoholic fermentation was based on whole cells, and chemists, in particular Justus Liebig, according to whom living cells were not required in any fermentation. Liebig believed that ferments (from around 1900 they were called enzymes) were substances in a state of decomposition or combination that acted by transmitting vibration and oxidation (29).

In 1859 Moritz Traube, rejecting Liebig's as well as Pasteur's hypotheses, anticipated the existence of intracellular as well as extracellular enzymes as causes of the most important "vital-chemical" processes in lower and higher organisms (30). But he did not succeed in isolating enzymes from cells. The dispute was finally resolved in 1897 by Eduard Buchner, who demonstrated that alcoholic fermentation also took place in cell-free yeast extracts. Buchner concluded that these extracts contain a fermentation enzyme, zymase, and that fermentation is a chemical process related to specific enzymes. But the controversies between molecular and nonmolecular theories of the mode of action of enzymes were continued.

In the late 19th century leading chemists, among them Bunsen, Buchner, and Fischer, were convinced that enzymes were proteins. Bunsen and Gustav von Hüfner proposed that enzymes were capable of undergoing temporary combinations with fermentable substances (31). Fischer's lock-and-key analogy implied this view. But because of the methodological problems of studying macromolecules and the quickly growing influence of colloid chemistry focused on surface phenomena at colloidal particles of protoplasm as responsible for catalysis, it took 30 years after Buchner's discovery before enzymes were recognized as definite chemical species of the nature of proteins.

Antimolecular and antisubstantialist views remained strong. Nägeli, probably influenced by Liebig, proposed that enzymes act by molecular vibrations communicated to the substrate. His theory (1890) that enzymes were not definite substances but properties of material substances became popular among biocolloidists during the following decades. So did E. Herzfeld's theory (1915), according to which enzymatic actions resulted from certain mixtures of common compounds, e.g. amino acids and peptides (32). Rejecting these antisubstantialist views, Willstätter assumed that small organic molecules, as chemically active groups bound to large nonspecific colloidal material, were responsible for the catalytic process. According to him diluted solutions containing enzymes gave none of the tests for proteins, and he concluded as late as 1926 that enzymes were not of a protein-like nature (33).

A member of Emil Abderhalden's school in Halle, Andor Fodor in the 1920s proposed a colloid chemical theory, according to which cellular colloids such as proteins, lipids, and polysaccharides build a gelatin system in the protoplasm on whose numerous interfaces enzymatically active substances or groups operate (34).

The controversy on the nature of enzymes ended with the publication of new experimental evidence in favor of the old theory that enzymes were distinct proteins: In 1926 the American physiologist James B. Sumner, through purification and crystallization of the enzyme urease from jack beans, showed that this enzyme was a pure protein (35). Opponents to this view, in particular Willstätter and members of his school, at first tried to dismiss the achievement as being the crystallization only of a protein "carrier" of the real enzyme. But when John Northrop and Moritz Kunitz purified and crystallized several digestive enzymes as discrete proteins with the active center being part of the protein molecule, the view that enzymes were proteins gradually became generally accepted (36).

The end of the controversies over the nature of enzyme action was thus marked by an experiment that may be called crucial even though its implications were not immediately accepted by all workers in the field. This experiment and those that confirmed its results shortly thereafter became the starting point for research on the mechanism of enzyme action.

3. Controversy over the molecular nature of proteins

The recognition of enzymes as proteins coincided with the recognition of proteins as molecules. After 1900 biocolloidists considered proteins, which became a focus of their research, to be aggregates of small molecules (37). Despite the fact that many protein chemists, in particular in the United States, continued to crystallize proteins and were convinced of high molecular weights of proteins (38), protein chemistry, too, was affected by colloid chemistry (see section 4 below). One of the strongest supporters of colloid chemical concepts in protein and enzyme research was Emil Abderhalden in Germany (39). Biocolloidists claimed that the physicochemical laws of solution were inapplicable to proteins and that they did not form salts in chemical reactions but adsorbed substances such as dyes and hydrogen ions at their surfaces.

A different view was taken by those protein chemists and physical chemists, who at the beginning of the 20th century examined the importance of the ionic environment, a core concept of early physical chemistry, in protein chemistry. Mainly due to the work of Soeren Soerensen and Leonor Michaelis, the concept of the hydrogen ion concentration became the foundation of an exact physical chemistry of proteins, including enzymes. Subsequently Loeb played a crucial role in criticizing colloidal theories concerning proteins, showing that they were obsolete. Loeb was a physiologist who, rejecting vitalistic and metaphysical explanations, became a pioneer in the search for physical and chemical explanations of biological phenomena. Like some other critics of colloid chemistry, such as Landsteiner and Michaelis, Loeb at first considered the colloidal approach potentially useful though he was not happy about its vague explanations. But his opposition to biocolloidists' far-reaching claims, which he rejected for scientific, philosophical, and political reasons, grew rapidly when colloidal chemistry gained influence. The scientist who strongly contributed to rendering these claims popular in the United States was Wolfgang Ostwald, whom Loeb knew well. Because of his good relationship with Ostwald's father, Wilhelm, Loeb invited Wolfgang to spend two postdoctoral years (1904-6) at his laboratory in Berkeley. Ostwald, however, disdained hypothesis-driven experimentation such as Loeb's "schlagende Versuche," preferring instead descriptive research such as "schematical series of experiments" (40). Much to the dislike of Loeb, he began to explain physiological phenomena entirely in terms of colloid chemistry, that is by coagulation and degrees of dispersion of cell components. His neglect of experiment and tendency towards speculation generated a conflict with Loeb; Ostwald's stipend was not extended (41).

During 1913-14 Ostwald was invited to give an extended lecture tour in the United States, where he was warmly received (42). He attempted to convince his audiences of the importance and fundamental new character of colloid chemistry, arguing that colloids constituted the most universal and common of all things we know; they formed a world of neglected dimensions, a middle country between the chemical and microscopic levels, following special yet undiscovered colloid-chemical laws. Therefore colloid chemistry deserved the right "to existence as a separate and independent science" (43). Five of these lectures were included in Ostwald's popular *The World of Neglected Dimensions* (44). Arguments such as that physical chemistry could not account for membrane phenomena since biological systems could not be described in mechanistic terms, were explicitly directed against Loeb's basic convictions. Despite initial enthusiasm for Ostwald's colloid chemistry in the United States, there were critical voices, among them the physiologist Albert P. Mathews who condemned the confusion of descriptive and explanatory terms (45). The most fundamental attack came, however, from Loeb.

According to Loeb's biographer Philip Pauly, Ostwald's views formed for Loeb "a particularly pernicious basis for 'metaphysical romance.'" Moreover, Ostwald exemplified the links Loeb saw between romantic attitudes in science and militaristic nationalism. The assertion that a "'neglected middle country' of colloids had 'a right to exist' paralleled German claims for the defense of 'middle Europe'" (46). The preface, written "from the trenches" in France, supported this view (47). Similarly, Loeb resented Wilhelm Ostwald's rabid nationalism during the war, which aimed at the unification of Europe under German supremacy, and their friendship turned cool (48). Loeb's aversion to the vague, speculative, and inherently vitalistic concepts of colloid chemists and his concern about their increasing acceptance by American scientists such as Bancroft prompted him to refute these claims by experiment. When in 1917 a coworker of Arrhenius and Soerensen brought a new electrical pH meter to the Rockefeller Institute, of which Loeb was by then a member, Loeb gave up all his other projects and produced a long series of publications on proteins and membrane equilibria, culminating in his 1922 *Proteins and the Theory of Colloidal Behavior* (49). Here he showed, first, that the colloidist concept of aggregation was superfluous because proteins obeyed the stoichiometric laws of chemical combination. Second, he made it clear that the physical properties of colloidal proteins, such as osmotic pressure and electrical potential, could be derived from existing theories of physical chemistry, such as the Donnan equilibrium and the theory of solution, if the influence of different pH values was taken into account. In principle, Loeb saw the chemistry of proteins as no different from that of small molecules. He concluded a paper in German in *Die Naturwissenschaften* the following year with the statement (50):

> It is possible to explain quantitatively the colloidal behavior of proteins on the basis of theoretical mathematical derivations. The socalled colloid chemistry that initially gave the impression of a new chemistry appears to have been based only on the non-observance of a condition of equilibrium of classical chemistry, at least insofar as proteins are concerned.

The editor of *Die Naturwissenschaften,* Arnold Berliner, was strongly criticized by German colloid chemists for publishing this article (51). The *Kolloid-Zeitschrift* published several counterstatements, including one by its editor Wolfgang Ostwald, who rejected Loeb's "purely chemical theory" for the behavior of proteins as a mere "fallacy". Though outside Germany Loeb's view became increasingly accepted (52), the controversy over proteins was not resolved because Loeb could not present conclusive evidence for their molecular weights or specify their structures. Only when Svedberg, using an ultracentrifuge, demonstrated the macromolecular nature of proteins in sedimentation studies at the end of the 1920s, did the concept become generally accepted.

Shortly thereafter, the molecular nature of proteins was used with great success in applied research. As Creager has shown, the "molecularizing" of protein chemistry brought about one of the most celebrated advances in military medicine during WWII, namely the development of blood plasma fractions as therapeutic agents (53). The scientist responsible for this success was Edwin Cohn, head of the Department of Physical Chemistry at Harvard Medical School, who together with Edsall during the 1930s systematically studied proteins as macromolecular polyelectrolytes (54). According to Creager (53):

> …the transition in military medicine from reliance on colloidal chemistry [in WWI] to physical chemistry of solutions (as assimilated into biochemistry) [in WWII] reveals the process of molecularization which had occurred within biomedical research in the intervening decades.

Cohn's and Edsall's textbook on proteins (1943) became the standard work for protein biochemists during the next decades (55).

4. The controversies over the existence and properties of macromolecules in chemistry

With a focus mostly on cellulose and other chain molecules comprised of small repeating units such as caoutchouc, the controversy over macromolecules in chemistry in the 1920s took a different course from that over antibodies, enzymes, and proteins. Chain molecules do not possess a uniform molecular weight and have a much less specific structure than globular proteins. Thus the controversies were not about specificity but about the existence of large molecules, the range of chain lengths, and physical properties of the molecules. As with the other controversies, the disputes were shaped by the personalities of those involved. Following is a summary of the main levels of the controversies (56).

The controversy over the existence of macromolecules was initiated by Staudinger, who in 1920 stated—still without experimental evidence—that various artificial products of polymerization, for example polymeric ketenes, "can be explained sufficiently by normal valency formula" (that is they are linked by covalent bonds), thus contradicting the widespread opinion that these products were compounds linked by secondary valencies (weak bonds) (57). The article was generally neglected. A year later, Freudenberg, as a result of his analysis of cellulose degradation studies, published the hypothesis that cellulose consisted of ca. 100 equally bound glucose units, the first evidence for the existence of long chain molecules in cellulose (58).

A few years later Staudinger provided experimental evidence for his theory and introduced the term "Makromolekül" for giant molecules whose subunits were linked by covalent bonds (59). Chemists from various subdisciplines opposed his theory (60). It did not help that he was already a much respected member of the community of organic chemists, with contributions on ketenes and organic phosphorus compounds. Responding to methodological criticism, he began to investigate simple synthetic products whose monomeric components were well known, using them as models for polymeric substances and transferring his experimental results by way of analogy to natural products such as caoutchouc and cellulose.

Most of Staudinger's colleagues at the ETH in Zürich, where he was a professor until his call to Freiburg in 1926, rejected the macromolecular theory. At a meeting of the Zürich Chemical Society in 1924, the mineralogist Paul Niggli, the organic chemist and future Nobel laureate Paul Karrer, and the physicist Paul Scherrer were among his many opponents. Different methodologies played an important role, the main objections coming from X-ray crystallographers such as Scherrer who considered Staudinger's claim a contradiction of their own interpretations.

Likewise, Staudinger was attacked, in particular by organic chemists, at the 1926 Düsseldorf meeting of the Society of German Scientists and Physicians, which was chaired by Willstätter. Fritz Haber was actively involved in choosing participants. According to Yasu Furukawa, Haber chose advocates of the aggregate theory, such as Max Bergmann, Hans Pringsheim, and Hermann (later Herman) Mark to take a stand against Staudinger and his macromolecular view (61). Since Haber did not conduct research in this field himself, he might have been motivated by the fact that members of his institute (in particular Herbert Freundlich) and colleagues from other KWIs (such as Bergmann, Reginald Herzog and Kurt Hess) then supported the aggregate theory. Moreover, one cannot exclude the possibility that he may have wished to see Staudinger publicly criticized, given the fact that their erstwhile friendship came to an end when Staudinger attacked the use of poison gas during WWI.

Several hundred chemists listened to Bergmann, Pringsheim, and Ernst Waldschmidt-Leitz present arguments for the aggregate theory of polysaccharides and proteins. Staudinger, with his paper "Die Chemie der hochmolekularen organischen Stoffe im Sinne der Kekulé'schen Strukturlehre," alone defended the macromolecular view. Somewhat surprisingly, however, the position of Mark, who had supported the aggregate theory earlier, was almost neutral; according to him small unit cells in the X-ray pictures did not preclude the existence of large molecules. Moreover, after a long and fierce discussion of Staudinger's contribution, Willstätter, who chaired the session, became convinced of the possibility that large molecules of a molecular weight of 100,000 existed, despite the fact that this was still a "terrifying concept" to him (62).

During the next couple of years some scientists changed their opinions and started to conduct research based on the concept of macromolecules. Among them were Bergmann, Mark, and Kurt H. Meyer. Bergmann in 1932 succeeded (with Leonidas Zervas) to develop a method to synthesize large specific peptides (carbobenzoxy method), thus opening up a new era in the growing field of peptide synthesis. Mark and Meyer became leading polymer researchers in Germany, focusing on physical chemical methods, in particular X-ray diffraction.

The macromolecular concept received a crucial confirmation through Svedberg's demonstration of the macromolecular nature of proteins. By 1930 most organic chemists were no longer opposed to the idea of macromolecules, even though they were not interested in undertaking research on them. It was the X-ray crystallographers who began to examine the structure of organic macromolecules. The controversy with Staudinger was, however, not ended. On the one hand, it was continued with colloid chemists, in particular Wolfgang Ostwald and Kurt Hess, who remained opposed to the concept (63). On the other, Staudinger opened up, surprisingly, an even fiercer controversy with his erstwhile opponents Mark and Meyer despite the fact that they were among the first to support his theory. Meyer and Mark in 1928 accepted Staudinger's view in principle but modified it. Confirming through X-ray crystallography Freudenberg's hypothesis, according to which cellulose consists of long chains of glucose molecules (they assumed 30 to 50) linked by glycoside bonds, they suggested using the term *Hauptvalenzkette* (main valency chain) instead of macromolecule preferred by Staudinger, because the term molecule connoted discrete compounds of a specific size that were not linked with each other (64). The glucose chains in cellulose were, however, of various lengths and linked to each other by weak forces (van der Waals forces). Questioning Staudinger's life-long assumption that macromolecules were rigid and did not bend, Meyer explained the elasticity of caoutchouc by the tendency of its isoprene compounds to bend and twist (65). Whereas later research proved Mark and Meyer correct in their assumption of intermolecular forces and molecular flexibility, it showed that cellulose molecules were much larger than they had assumed.

The new controversy centered around several issues: terminology (Staudinger's *Makromolekül* prevailed over *Hauptvalenzkette*, but Mark's later suggestion of *polymer* became widely accepted); methodology (Staudinger remained skeptical of physical chemical methods); contradictory interpretations of experimental results; dogma (the rigid molecule); and, perhaps most important for the fierceness of Staudinger's attacks, priority. The arguments were played out between 1928 and 1936 in the *Berichte der deutschen chemischen Gesellschaft*. The dispute was continued after 1932 when Mark moved to Vienna and Meyer to Geneva. Only in 1936 did the editorial board officially bring it to a close, following the 140th communication by Staudinger on macromolecules, an uncompromising attack on Meyer (66).

What followed was what might be called a bad play. Staudinger who at the same time had a fierce dispute with Kurt Hess, whom he reproached for having distorted his (Staudinger's) statements, tried to persuade industry to intervene (both men were beneficiaries of IG Farben funding). Among other things Staudinger presented himself as a victim of the Jews, that is of Mark and Meyer (Mark's father was Jewish). But his sponsor from IG Farben, Georg Kränzlein, a member of the NSDAP and SS, considered Staudinger's ongoing controversies with erstwhile opponents as senseless and counterproductive, and advised him to stop "quarreling with Jews" and "ignore them," in accord with the Nuremberg Laws (67). Moreover, Staudinger's good relationship with IG Farben suffered when he criticized the firm for having allowed Meyer to attack his (Staudinger's) viewpoint in the 1920s when Meyer was a member of the firm's board (68).

This short account shows that various factors contributed to the generation and conduct of the controversies over the existence and properties of macromolecules, among them dogma (organic chemists' "large molecules do not exist," and Staudinger's "macromolecules do not bend"), conflicting interpretations over the use of different methods, change of attitudes, and personal features. The controversy in the 1920s was largely one with Staudinger. He stood his ground with grim determination despite the fact that he was attacked fiercely from all sides and had few allies (he even seemed to prefer to fight alone, and did not try to join forces with Freudenberg). His stubbornness and dogmatic way of arguing prolonged, however, the controversy at a time when his view had long been accepted, preventing him from conducting more productive research later on.

Summary and Conclusion

The historically interesting questions dealt with in this paper concern: (1) the impact of the controversies dealt with on scientific advancement; (2) their influence on scientists' opinions and practices; and (3) the role played by social and scientific factors. What follow are a summary and comment on the conclusions drawn in the four case studies.

(1) Impact of the controversies on scientific advancement

This study largely supports the view of Marcel Florkin that the controversies over the claims of biocolloidists had primarily a retarding impact on the development of biology and biochemistry. At a time when clear cut evidence for large specific molecules was not yet available, these claims discouraged further research on the molecular structure of enzymes, other proteins, and DNA and its relationship to function, that is, research that eventually provided explanations for biological specificity. Chemists focused their work on the structures of small subunits, that is, peptides and nucleotides. To give an example from DNA research: The Swedish cytochemist, Einar Hammarsten was one of the few researchers who dealt with polymeric, nondegraded DNA. But at that time he did not consider it necessary to examine its properties. Instead he explained the biological action of DNA through its ability to act on small environmental changes as a colloid, that is, to increase or reduce its state of aggregation and thus influence the physico-chemical properties, such as osmotic pressure, of the nucleoplasm (69). (Nevertheless, in 1938 he was one of the researchers who established the macromolecular nature of DNA.)

Despite the fact that some laboratory technologies developed by colloid chemists, most importantly the ultracentrifuge, proved highly fertile, the conduct of colloidal chemical research in the area of biology and biochemistry did not lead to any major scientific success. Even the nature of the weak forces, a key element of colloid chemistry, and their role in biologically relevant substances such as proteins, were elucidated by chemists on the basis of the macromolecular concept. The influence of biocolloidy and the often associated focus on applied research in medical biochemistry, at least in Germany, was detrimental to the development of biochemistry by marginalizing excellent biomedical scientists who had abandoned colloid chemistry. According to Michaelis, it was impossible for a biochemist in Germany to receive an academic position, if one did not aim at "so called 'practical' successes" (70). In Europe it appeared to be advantageous to pursue a colloidal chemical approach in order to receive an academic position in biochemistry (71). Lipmann was another leading biochemist active in the 1920s and 1930s who shared Florkin's view of a negative impact of colloid chemistry on the development of an exact scientific biochemistry (72):

> It seemed sufficient to call [the protoplasm] colloid in order to give the impression to understand something about it.

According to Fruton, the most significant feature of the debate about proteins and enzymes between 1900 and 1930 was the tension between biologists and biochemists, using the physical chemical approach of the colloid chemists, and organic chemists and biochemists following the tradition of Emil Fischer (73). These tensions were indeed a main feature of the controversy about Ehrlich's side-chain theory. But with organic chemists hardly participating in the controversies in protein chemistry and biochemistry later on, the main controversies here took place between colloidal chemists on the one side and a few physical chemists and biochemists on the other, all using physico-chemical methods. By applying the ionists' concept of physical chemistry *and* following the tradition of Fischer (and also of Ehrlich), certain researchers fought the prevailing claims of colloidal chemists engaged in biological and biochemical research, most notably Loeb, Cohn, and Edsall.

(2) Influence of the controversies on scientists' opinions and practices

It is not possible to assess the amount of learning brought about by the arguments exchanged in the controversies analyzed above. As in other controversies, many scientists remained intransigent; others changed their attitudes either quietly or explicitly. In some cases, scientists' readiness to change their opinions and practices proved crucial for their own success and for further scientific developments. Among them are the following:

- Karl Landsteiner, an active early supporter of colloid chemistry, later changed his approach, accepting the existence of sharp specificity and including organic chemistry as a tool in his most fruitful studies on the chemistry of antigenic specificity.

- Hermann Mark, in the controversy about the existence of macromolecules, changed his research program and subsequently conducted physical chemical studies on macromolecules (or polymers, as he preferred to call them) that proved of high theoretical and practical value.
- Otto Warburg conducted his most outstanding and internationally pioneering research on enzymes only after he had changed his research program and based it on the notion of enzymes being proteins, following evidence provided by Sumner and Northrop in the late 1920s. In the 1930s Warburg isolated and characterized chemically and physically a number of enzymes active in the oxidative degradation of glucose in the cell, that is glycolysis and the respiratory chain.

(3) The role of scientific and social factors

The cases presented here show that social and political factors influenced the course and duration of the disputes. But they also show that the controversies over the existence of macromolecules and the molecular or colloidal nature of major biologically relevant substances and reactions ended by the late 1920s because there was new, convincing scientific evidence in favor of the macromolecular theory, which soon became generally accepted. Most historians agree that scientific factors played a major role in closing the disputes, but they disagree about the importance of social factors, some, like Mazumdar, attributing a determining influence to them. She here followed the "Denkstil" concept of Ludwik Fleck, pointing to the fact that probably this concept itself had grown out of the colloidal-molecular controversy in immunology. Fleck in 1935 rejected the treatment of toxins and receptors as chemical entities, calling Ehrlich's "Denkstil" "a primitive scheme," which "is being progressively discarded in accordance with current physico-chemical and colloidal theories" (74). Fleck's awareness of the differences between the "Denkstile" prevalent in various schools—that is the concept of absolute specificity in the Ehrlich school and his own "Denkstil" based on colloid and the rejection of sharp specificities—according to Mazumdar, induced him to set up the claim of the socially conditioned nature of scientific facts. Following Fleck, Mazumdar concludes that ideas must be fitted into a social power structure in order to acquire authority.

I argue that this assumption cannot explain major features of the colloidal – macromolecular controversy and their closure. It is true; there was no scientifically "logical" path to the general acceptance of the macromolecular concept. Social and psychological factors, for example the influence of schools, the seductive property of colloidal concepts described by Florkin, the organizational skills and initial high reputation of Wolfgang Ostwald (related to the renown of his father), and the support provided by industry played important roles in the rise of colloidal chemistry. But social factors cannot explain the rise of macromolecular chemistry and biology and the end of colloidal chemistry (as far as biology and biochemistry are concerned). In general, social factors do not explain the fact that new concepts or theories, developed by people without power or prominence, sometimes replace predominant concepts and theories. Macromolecular chemistry, initiated and promoted by the organic chemist Hermann Staudinger, who in doing so became an outsider of his discipline, is a case in point.

In order to explain the outcome of the controversies and the fact that it was molecular biology which has been advancing rapidly during the past half century and not colloidal biology, we need to compare the scientific quality of these two approaches. By applying the criteria of epistemological reliabilism (75) *molecular* biology can be shown to be superior to *colloidal* biology, because

- the existence of macromolecules and the macromolecular nature of biologically active molecules like proteins or DNA have been abundantly confirmed by *reliable* experiments, conducted with the help of a vast range of different methods;
- the macromolecular approach has been *relevant* for answering pertinent questions in biology such as that of the nature of the genetic material, its replication and mutation, because it could be combined with the fruitful concepts of the gene and the chromosome theory, whereas the colloidal approach with its concepts of "ideoplasm" (Nägeli) and adsorption of hereditary enzymes to chromatin (R. Goldschmidt), had already lost its relevance during the development of classical genetics.
- the macromolecular concept has become *fruitful* in further empirical and theoretical biological, biochemical, and genetic research where it has led to major scientific advances. It has become

> a basic unit in biological, pharmaceutical, and medical applications as the increasing importance of "molecular medicine" indicates.

It is true, to quote Linus Pauling, that there are "many aspects of life that are not yet accounted for in a detailed way on a molecular basis" (76). Yet, it is also true, as Pauling continued, that within a relatively short period of molecularization in biology—he spoke of thirty years culminating in the elucidation of the DNA double helix structure by Watson and Crick—"a thoroughly satisfying understanding of many of the properties of living organisms in terms of the structure of the molecules of which they are composed" had been achieved.

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REFERENCES AND NOTES

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1. M. Florkin, *A History of Biochemistry*, Elsevier, Amsterdam, London, 1972, 279-280. "Its supporters claimed that many biological phenomena such as parthenogenesis, muscle contraction, production of action currents in nerves, heart activity, ciliary movements, etc. were influenced by inorganic ions according to a series of degrees of influence of the same series of ions on heat coagulation, on lecithin precipitation, etc. The induction that biological phenomena were the results of changes introduced by ions in agglutination, lytic processes, dispersion, hydration, or dehydration of colloid micelles believed to compose the protoplasmic 'gel' became widespread."
2. J. S. Fruton, *Proteins, Enzymes, Genes. The Interplay of Chemistry and Biology*, Yale University Press, New Haven, CT and London, 1999, 158.
3. Ref. 2, *Proteins*, pp 203-204; see also J. S. Fruton, *Molecules and Life. Historical Essays on the Interplay of Chemistry and Biology*, Wiley-Interscience, New York, London, 1972, 140-1.
4. R. Olby, "Structural and Dynamical Explanations in the World of Neglected Dimensions," in T. J. Horder, J. A. Witkowski, and C. C. Wylie, *A History of Embryology*, Cambridge University Press, Cambridge, 1985.
5. P. Mazumdar, *Species and Specificity*, Cambridge University Press, Cambridge, 1995.
6. Since "molecular" in this context relates to macromolecules, for reasons of simplicity, I will use in the following, the term molecule instead of macromolecule. Angela Creager has used the term molecule in the same way in "Producing Molecular Therapeutics from Human Blood. Edwin Cohn's Wartime Enterprise", in S. de Chadarevian and H. Kamminga, Ed., *Molecularizing Biology and Medicine. New Practices and Alliances, 1910s–1970s*, Overseas Publishers Association, Amsterdam, 1998, 107-138.
7. For a comprehensive history, see, for example a) H. and M. Staudinger, *Die makromolekulare Chemie und ihre Bedeutung für die Protoplasmaforschung*, Springer, Wien, 1954; b) *H. Staudinger, Arbeitserinnerungen*, Dr. Alfred Hüthig. Heidelberg, 1961; c) J. T. Edsall, "Proteins as Macromolecules: An Essay on the Development of the Macromolecule Concept and some of its Vicissitudes," *Arch. Biochem. Biophys.*, **1962**, Suppl. 1, 12-20; d) C. Priesner, H. Staudinger, H. Mark, K. H. Meyer. *Thesen zur Größe und Struktur der Makromoleküle*, Verlag Chemie, Weinheim, 1980; e) Y. Furukawa, *Inventing Polymer Science. Staudinger, Carothers, and the Emergence of Macromolecular Chemistry*, University of Pennsylvania Press, Philadelphia, PA, 1998 f) Y. Furukawa, "Macromolecules. Their Structures and Functions," in M. J. Nye, Ed., *The Modern Physical and Mathematical Sciences*, Cambridge Univ. Press, Cambridge, MA, 2003, 429-445; Ref. 3.
8. J. T. Edsall, "Physical Chemistry of Proteins 1898-1940," in P. R. Srinivasan et al., "The Origin of Modern Biochemistry: a Retrospect on Proteins," *Ann. N. Y. Acad. Sci.*, **1979**, *325*, 53-73 (54-57).
9. See for example E. Fischer, "Isomerie der Polypeptide," *Z. Physiol. Chem.*, **1917**, *99*, 54-66.
10. Ref. 2, pp 159-160.
11. J. W. Servos, *Physical Chemistry from Ostwald to Pauling. The Making of a Science in America*, Princeton University Press, Princeton, NJ, 1990, 300.
12. As far as scientists' motives are concerned, we also may, following Pauline Mazumdar (Ref. 5), invoke a predilection for research styles. The fact that colloidal science emphasized graded differences and smooth transitions may have appealed to scientists with a predilection for this type of explanation over that in which specificity and diversity in nature were prevalent (see Ref. 25). But motives cannot explain the long-term continuation of practices.
13. U. Deichmann, *Flüchten, Mitmachen, Vergessen. Chemiker und Biochemiker in der NS-Zeit*, Wiley/VCH, Weinheim, 2001, Ch. 7.2.
14. Ref. 7a, p 10.
15. Ref. 13.
16. J. W. Servos, "A Disciplinary Program that Failed: Wilder D. Bancroft and the *J. Phys. Chem.*, 1896-1933", *Isis*, **1982**, 73, 207-232.
17. Ref. 13, Ch. 7, part 2.
18. Mazumdar has dealt in detail with the controversies in immunology (Ref. 5).

19. A. S. Travis, "Science as Receptor of Technology. Paul Ehrlich and the Synthetic Dyestuffs Industry," *Science in Context*, **1989**, *3*, 383-408.
20. E. Fischer, "Einfluß der Konfiguration auf die Wirkung der Enzyme, I." *Ber. Dtsch. Chem. Ges.*, **1894**, *27*, 2985-2993.
21. U. Deichmann, "Empiricism and the Discreteness of Nature: Ferdinand Cohn (1828-1998), the Founder of Microbiology," in U. Charpa and U. Deichmann, *Jews and Sciences in German Contexts. Case Studies from the 19th and 20th Centuries*, Mohr-Siebeck, Tübingen, 2007, 39-50.
22. "Festival lecture" to the K.K. Gesellschaft der Ärzte in Wien of 1905, cited in Ref. 5, p 227.
23. Ref. 5, p 231.
24. Ref. 5, pp 220-236.
25. According to Mazumdar, Landsteiner preferred the "unitarian" thought style of graded differences and smooth transitions prevalent with colloidal chemists over the thought style of the "pluralists," the "analytical thinkers," and "dissectors" to whom understanding consists in the uncovering of diversity. Yet his main work followed the "pluralists'" style: he contributed strongly to concepts of sharp specificity, through his immunochemistry as well as his work on human blood groups (for which he received the Nobel Prize). A thought style may have influenced Landsteiner's work, but it did not determine it. Capabilities seem to have been more important than a thought style. Because of his broad scientific education which included organic chemistry, he was able to create a very reliable and fertile new research program and maintain it even though the results contradicted his preferred thought style.
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28. Ref. 27.
29. Ref. 2, p 131.
30. Ref. 2, p 143.
31. Ref. 1, p 271.
32. Ref. 1; he lists as followers E. Baur, Panzer, H. von Euler, and Svanberg, who, except for von Euler, are largely unknown today.
33. R. Willstätter, "Zur Frage der proteinartigen Natur der Saccharase," *Ber. Dtsch. Chem. Ges.*, **1926**, *59 II*, 1591-1594.
34. U. Deichmann and A. S. Travis, "A German Influence on Science in Mandate Palestine and Israel: Chemistry and Biochemistry," *Israel Studies*, **2004**, *9.2*, 34-70.
35. J. B. Sumner, "The Isolation of the Enzyme Urease," *J. Biol. Chem.*, **1926**, *69*, 435-441.
36. J. H. Northrop, M. Kunitz, and R. M. Herriott, *Crystalline Enzymes*, Columbia University Press, New York, 1948. Willstätter's international scientific authority as one of the most renowned German natural products chemist was such that biochemist Hugo Theorell, a Nobel laureate of 1955, assumed that the objections raised for many years in particular by Willstätter and his school against Sumner's and Northrop's work contributed to the fact that the Nobel Prize for the latter two was awarded only 20 years later, in 1946 (H. Theorell, "The Nature and Mode of Action of Oxidation Enzymes," in *Festschrift für Prof. Dr. Arthur Stoll zum siebzigsten Geburtstag 8. Januar 1957. Arbeiten aus dem Gebiet der Naturstoffchemie*, Birkhäuser, Basel, 1957, 36).
37. Around 1900 chemists still assumed that proteins were large, crystallizable molecules; see, for example, F. N. Schulz, *Die Krystallisation von Eiweißstoffen und ihre Bedeutung für die Proteinchemie,* Fischer, Jena, 1901; *Die Größe des Eiweißmoleküls*, Fischer, Jena, 1903. The controversies in the history of protein research have been dealt with in detail in Ref. 1, 2, and 8. For research in Germany, including political aspects of the controversy, see also Ref. 13.
38. C. Tanford and J. Reynolds, "Protein Chemists Bypass the Colloid/Macromolecular Debate," *Ambix*, **1999**, *46*, 33-51. These authors mention in particular Thomas B. Osborne in New Haven, CT, who through "compulsive attention to meticulous purification, reproducibility, error analysis, etc." was "head and shoulders above most of his contemporaries" (p 41).
39. Ref. 38 and Ref. 13, Ch. 7.1. Early other supporters of colloid chemistry include the protein chemists Ernst Waldschmidt-Leitz and, for several years, even renowned protein chemist Max Bergmann.
40. Wolfgang Ostwald to Wilhelm Ostwald, November 14, 1915, Wilhelm Ostwald Gedenkstätte Großbothen, file Wolfgang Ostwald.
41. For Wolfgang Ostwald's efforts in disciplinary building and his rise to the leading colloid chemist in Germany, see Ref. 13, Ch. 7.2.
42. J. W. Servos has reviewed Ostwald's lectures and his reception in the US (Ref. 11, pp 300-308).
43. Wolfgang Ostwald, *Die Welt der vernachlässigten Dimensionen, Eine Einführung in die moderne Kolloichemie mit besonderer Berücksichtigung ihrer Anwendungen*, Steinkopff, Dresden, 1915, Vorwort and passim.
44. Ref. 43; the book appeared in the United States as W. Ostwald and M. Fischer, *An Introduction to Theoretical and Applied Colloid Chemistry: 'The World of Neglected Dimensions*,' John Wiley, New York, 1917.
45. Ref. 11, p 306.
46. P. J. Pauly, *Controlling Life. Jacques Loeb & the Engineering Ideal in Biology*, Oxford Univ. Press, New York, Oxford, 1987, 151-2.

47. As far as politics is concerned, it should be added that during the Nazi era, Wolfgang Ostwald, a member of the NSDAP, used chemical metaphors to describe euphemistically the expulsion of Jews. Between 1937 and 1941 he made several visits to England, the United States, Yugoslavia, Hungary, and Rumania, in which he made a determined effort to convince his audiences that the changes in Germany, i.e. the purge of Jews from German universities and society resembled a "recrystallization," necessary to gain purity. Returning from one of these travels, he reported to German authorities, "As chemists they understood me best when I spoke about our renewal as of a 'recrystallization' that is purification, stabilization, and restructuring." ('Rekristallisation', d.h. Reinigung, Stabilisierung, Neuformung); Wolfgang Ostwald to the German Research Council in reports about his journeys, (Ref. 13, p 390, translation by the author).
48. Ref. 13, Ch. 1.
49. J. Loeb, *Proteins and the Theory of Colloidal Behavior*, McGraw-Hill, New York, 1922.
50. J. Loeb, "Die Erklärung für das kolloidale Verhalten der Eiweißkörper," *Naturwissenschaften*, **1923**, *11*, 213-221(translation by the author).
51. For details of the disputes in Germany see Ref. 13, Ch. 7.2.
52. It is worth mentioning that Herbert Freundlich, a colloid chemist at the KWI for Physical Chemistry, conceded that Loeb's conclusion as far as proteins were concerned was probably right and that, in any case, all further research on proteins had to be based on it (H. Freundlich, "Jacques Loeb und die Kolloidchemie," *Naturwissenschaften*, **1924**, *12*, 602-3).
53. Ref. 6, pp 107-138.
54. Two other contributions to the physical chemistry of proteins were of crucial importance for their work, the Debye-Hückel theory of the behavior of ions in relation to their charge and radius, other ions and the dielectric properties of the environment, and Niels Bjerrum's demonstration of the Zwitterion (dipolar ion) state of amino acids in their isoelectric state (Ref. 2, pp 205-6).
55. E. J. Cohn and J. T. Edsall, *Proteins, Amino Acids and Peptides as Ions and Dipolar Ions*, Reinhold Publishing Corp., New York, 1943.
56. The disputes about the existence and properties of macromolecules in Germany in the 1920s have been tackled in detail by Priesner, who also analyzed the ensuing polemic between Staudinger and Hermann Mark and Kurt H. Meyer (Ref. 7d); see alsoRef. 7e, Ch. 2, and Deichmann, who focused on the disputes about Staudinger and macromolecules during the Nazi era ((Ref. 13, Ch. 6.1.2 and 7.3).
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58. K. Freudenberg, "Zur Kenntnis der Cellulose," *Ber. Dtsch. Chem. Ges.*, **1921**, *54*, 767-772.
59. H.Staudinger, "Über die Konstitution des Kautschuks (6. Mitteilung über Isopren und Kautschuk)," *Ber. Dtsch. Chem. Ges.*, **1924**, *57*, 1203-1208. During the following years Staudinger published a large number of articles in which he developed and expanded the view that that there are large molecules which consist of many subunits held together by covalent bonds and defended the term macromolecule; for example, H. Staudinger, "Über die Konstitution der hochmolekularen Stoffe," *Naturwissenschaften*, **1929**, *17*, 141-144.
60. Staudinger described the opposition he faced in particular by organic chemists in Ref. 7b, pp 77-79.
61. Ref. 7e, pp 69-73.
62. Ref. 7e, p 74.
63. Ref. 7d.
64. K. H. Meyer and H. Mark, "Über den Bau des krystallisierten Anteils der Cellulose," *Ber. Dtsch. Chem. Ges.*, **1928**, *61*, 593-614.
65. K. H. Meyer, "Neue Wege in der organischen Strukturlehre und in der Erforschung hochpolymerer Verbindungen," *Z. Angew. Chem.*, **1928**, *41*, 935-946.
66. H. Staudinger, "Über hochpolymere Verbindungen, 140. Mitteil. Zur Entwicklung der makro-molekularen Chemie. Zugleich eine Antwort auf die Entgegnung von K. H. Meyer und A. van der Wyk," *Ber. Dtsch. Chem. Ges.*, **1936**, *69*, 1168-1185. The editorial board added that it considered the dispute between Mr. Staudinger and Mr. Meyer to be over.
67. Ref. 13, p 405.
68. For details see Ref. 13, Ch. 7.3.
69. E. Hammarsten, "Zur Kenntnis der biologischen Bedeutung der Nucleinsäureverbindungen," *Biochem. Z.*, **1924**, *144*, 383-466.
70. Loeb papers, Library of Congress, Manuscript Division, Michaelis to Loeb, March 4, 1921.
71. Michaelis' application for a professorship at the University of Lund (in 1920) was rejected on the grounds that he did not work on colloid chemistry, a predilection which, according to Jacques Loeb, was not shared in the United States at the time, where scientists like "Lawrence Henderson, Van Slyke, or Levene have not much respect for colloid chemistry left" (Loeb to Michaelis, Jan 27, 1921, Loeb papers, Library of Congress, Manuscript Division).
72. F. Lipmann, in H. Krebs and F. Lipmann, "Dahlem in the Late Nineteen Twenties," in H. Kleinkauf, H. von Döhren, and L. Jaenicke, *The Roots of Modern Biochemistry. Fritz Lipmann's Squiggle and its Consequences*, de Gruyter, Berlin, New York, 1988, 119 (translation by the author).
73. Ref. 2, p 159.
74. Ref. 5, p 380.
75. These criteria, proposed by Alvin Goldman and further developed for scientific research by Paul Thagard and Ulrich Charpa, comprise the reliability, relevance, efficiency, and fertility of a research practice (see, for example, P. Thagard, "Collaborative Knowledge," *Nous*, **1997**, *31*, 242-261).

76. L. Pauling, "Fifty Years of Progress in Structural Chemistry and Molecular Biology," *Daedalus*, **1970**, *99* (4), 988-1014 (1010).

ABOUT THE AUTHOR

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The Partington Prize 2008

The Society for the History of Alchemy and Chemistry has established the Partington Prize in memory of Professor James Riddick Partington, the Society's first Chairman. It is awarded every three years for an original and unpublished essay on any aspect of the history of alchemy or chemistry. The prize consists of three hundred and fifty pounds (£350).

The competition is open to anyone with a scholarly interest in the history of alchemy or chemistry who, by the closing date of 31 December 2007 has not reached 35 years of age, or if older has completed a doctoral thesis in the history of science within the previous three years. Scholars from any country may enter the competition, but entries must be submitted in English and must not have been previously submitted to another journal. The prize-winning essay will be published in the Society's journal, *Ambix*. One hard copy of the entry, word processed on one side of the paper, should be submitted, along with a copy of the entry on disc. We prefer files to be in Microsoft Word 2000, if possible. Essays must be fully documented using the conventions used in the current issue of *Ambix*. Essays must not exceed 10,000 words in length, including references and footnotes. All entries must be submitted with a word count.

All entries should be sent to Dr Gerrylynn K. Roberts, Department of the History of Science, Technology and Medicine, The Open University, Walton Hall, Milton Keynes, MK7 6AA, UK, with the words "Partington Prize" written clearly on the envelope. Each entry should contain a **separate** title page giving the author's name, institution, postal address, email address and date of birth (and if relevant the date of completion of their thesis). The author's name and contact details **must not** appear on the pages of the essay as the identity of the author will not be made available to the judges. Essays (no more than one from each competitor) must be received no later than 31 December 2007.

The decision of the judges appointed by the Council will be final. The Society reserves the right to divide the prize between two or more entries of equal merit, or not to award a prize should no essay be deemed of suitable standard.

The name of the winner will be announced by 30 April 2008, and all essays will be returned to competitors soon after that date.

THEODOR VON GROTTHUSS (1785-1822) – A TRAIL BLAZER

Bruno Jaselskis, Carl E. Moore, and Alfred von Smolinsk, Loyola University of Chicago

Two hundred years ago Theodor Grotthuss published a major paper entitled "*Mémoire sur la décomposition de l'eau et des corps, qu'elle tient en dissolution a' l'aide de l'élecrticite galvanique.*"(1) In a short time this document was reprinted in other journals (2, 3) and was translated into English (4). In this paper Grotthuss presented an electrolysis concept based on physical-chemical phenomena rather than on the prevailing established electrostatic interpretation of the galvanic process. He perceived the battery not only as a galvanic generator but also as a polarized system composed of particles (molecules) of opposing electrical nature. This landmark document laid out new directions for the interpretation of galvanic phenomena. It influenced future investigations carried out by Sir Humphry Davy, John J. Berzelius, Michael Faraday, and others.

This paper was published five years after Alexandro Volta's invention of the electric pile, a device that could provide a continuous electric current for a prolonged period of time. Volta reported this discovery in a letter, written in French, to Sir Joseph Banks, the president of the Royal Society of London (1800). This report of the pile "*On Electricity Excited by Mere Contact of Conducting Substances of Different Kinds,*" appeared in the *Philosophical Transactions* (1800) written in the French language. It was followed by an English translation in September of the same year (5). Volta's paper was a gigantic step from the electrostatic mode of creating electricity and brought with it the challenge to explain the mysterious action of the voltaic pile.

This challenge engaged the best minds of science throughout Europe, in particular those working in the field of static electricity, and especially those who were trying to explain the mechanism of the production of static electricity. Out of the ferment of the times came a young novice scientist, Theodor Grotthuss, a student, eager to absorb knowledge and resolute in his desire to form his own opinions and interpretations uncluttered with the dogma of the established thinking.

J. A. Krikštopaitis, in his interesting book written in the Lithuanian language, titled "Pralenkęs Laiką" (Ahead of the Times), describes the life of Theodor Grotthuss and his scientific contributions. He states (6):

> If Alfred Nobel would have instructed to award a prize for works done a century earlier, T. Grotthuss would have been awarded twice-once for his electrolysis theory and the second for his photochemistry postulates.

Grotthuss was fortunate to be born at the right time, with a high rank in society, and to be endowed with a brilliant inquisitive mind.

Intellectual Environment at the End of the 19th Century

At the time of Grotthuss's birth the works of natural philosophers of the enlightenment period and the intellectual features of the French Revolution promoted a spirit of confidence in the human mind and ushered in the scientific revolution. Especially, ideas by J. W. Goethe, F. W. Schelling, G. W. F. Hegel, and Immanuel Kant encouraged investigation of nature and existing forces. Added to these insights were Lavoisier's brilliant experimental observations that all processes obey a mass conservation law and that something cannot be created from nothing. These insights and observations merged into a radical view of the world that was quite contrary to that from any previous age and required the application of analytical methodology to grasp the nature of the natural world. In the beginning of the 19th century Newtonian mechanics dominated the description of the natural world. Kant and Schelling tried to question "Newton's despotism" in their discourses on natural philosophy. They treated the world as a dynamic summation of differences that continuously underwent changes. Schelling viewed nature as a precise unity and summation of attractive forces. Force could not exist alone without a counterpart, hence matter was continuously undergoing disproportionation, recombination, and neutralization. Thus, young Grotthuss grew up in a period open for inquiry and discovery.

Grotthuss's Early Childhood

Ewald Dietrich von Grotthuss and his wife Elisabeth Eleonor, the parents of Theodor, belonged to an old and distinguished family of Kurland (Courland) chancellery nobility. In 1784 the family went for an extended trip to Western Europe. While the parents were traveling through Germany, their son was born in Leipzig on January 20, 1785. Soon after his birth he was baptized and given the names Christian Johann Dietrich. As an adult he chose to use Theodor as his first name. His godfather, a well known German writer, Felix Weisse, interceded with Samuel Friedrick Morus, the Rector of Leipzig University, to obtain a student's matriculation certificate entitled "*Inscriptionis Diploma.*" Thus, Grotthuss, just a five-day old baby, became the youngest student with a title "*C. J. D. Grotthuss Lipsiensis.*" In a letter to the child's father the godfather stated (7):

> Here are all opportunities for your young beloved son, to acquire a great education, a better education than that of his father and ending this letter I say "Amen.

The Grotthuss family decided to stay in Leipzig for a while. Young Theodor's father was an amateur composer and collector of natural science materials. Unfortunately, the father's health was fragile, and while in Leipzig, it took a turn for the worse. In 1785 the family returned to their estate, and few months later his father, Ewald Dietrich von Grotthuss, died from a stomach ailment.

Theodor Grotthuss grew up on his mother's estate in Gedučiai (Geddutz), which on today's map is located in the northern part of Lithuania at the border with Latvia. He was a lonely child who had relatively limited contact with the children in the village. He had to use his mind and imagination to fill in the time. Tutors instructed and trained him at home, as was customary, in the skills of languages, mathematics, art, and literature. Thus, he obtained a sound basic education enabling him to pursue higher education at various universities. At this time it was not necessary for him to get a diploma. The position within the nobility rank determined the success in a career, not a diploma from a university.

Grotthuss as a Young Scholar

At the age of 18, in May of 1803, the young Theodor Grotthuss left his home for studies abroad, enrolling at the University of Leipzig. However, after attending some lectures there, he became disillusioned because of the lack of new ideas and the shortage of laboratory facilities suitable for experimentation. While in Leipzig some works by Schelling influenced his inquisitive mind. In the fall of the same year Grotthuss went to Paris, where he started studies at *L'Ecole Polytechnique*. He attended lectures of famous professors: C. L. Berthollet, A. F. Fourcroy, Rene-Just Haüy, L. N.Vauquelin, L. J. Thenard, and others. These lectures and laboratory experience had a great influence on his future scientific work. Fourcroy conveyed important ideas on electrolysis, Berthollet on equilibrium, Haüy on crystallization and motion of particles; Vauquelin and Thenard helped to develop his laboratory skills. At the same time the discovery of Volta's battery in 1800 stimulated intensive research efforts at universities and fascinated the minds of scholars including Grotthuss, even as a new student. These discoveries and contacts became an important driving force in his future career.

Grotthuss was influenced by the French revolutionary ideas of rationalism and the democratic way of life. Although of the Kurland nobility, in his public life and in his publications he chose not to use the title **von** and dropped it when he was in Rome in 1806.

Grotthuss's studies in Paris were interrupted when Napoleon declared himself emperor and started preparations for war with Prussia and Russia. Troubled by a recurring stomach-related illness inherited from his father, Grotthuss set sail from Marseilles to Italy with the hope of improving his health. This move provided him much needed rest from his intensive studies in Paris.

In Italy he went to Rome and then to Naples, where he joined a group of scientists, J. L. Gay Lussac, A. Humboldt, and L. Buch, who were investigating volcanism of mount Vesuvius. Under Gay Lussac's guidance he analyzed volcanic gases. While with this group, a British doctor, Thomson [name not available] gave him a Volta's battery pile. This gift was to play an important role in electrical science in that it enabled Grotthuss to study electrolysis, in particular that of water. These studies put in place the basis for his critical paper on the electrolysis of water and provided a broad background for his future scientific activity. While in Italy his health improved, which gave him the impetus to pursue his goals.


Theodor von Grotthusr

Grotthuss's Fundamental Paper

In the fall of 1805 at the age of 20, Theodor Grotthuss set about writing his first fundamental paper, on the electrolysis of water. Ttitled "*Mémoire sur la Décomposition a'l'Aide de l'Electricite Galvanique"* it was published in Rome in 1805. This paper distinctly presented a new approach to the explanation of the role of the electrical current during electrolysis, and some think it, in itself, could have been worthy of a Nobel Prize, if such had existed at that time. Years later Oswald translated this paper into German and made this strong comment (8):

> *Es ist die Schrift, durch die der Name Grotthuss vor allem berümt wurde, und die den grössten Einfluss auf die theoretischen Vorstellung über Elektrolyse ausgeübt hat*. (By this paper the name of Grotthuss will be very famous, and this paper has had a very, very great influence on the theoretical interpretation of electrolysis).

In this important paper, he explained why, during the electrolysis of water, hydrogen was generated at one pole and oxygen at the opposite pole, rather than throughout the whole solution as had been expected. This phenomenon, observed by A. Carlisle and J. W. Nicholson shortly after Volta's discovery of the pile, became known as the "Nicholson Paradox." It presented a challenge to scientists to explain satisfactorily this puzzling effect. It should be noted that, at the time of discovery of the pile by Volta, the conception of voltaic phenomena was based on the existing paradigm of static electricity, which in turn was based on the measurement of voltaic effects by electrostatic instruments.

Volta, by placing highly polished iron and copper plates in contact with each other (1799), had observed that these plates acquired opposite charges. This important observation led his contemporaries to believe that the electrical currents were of static nature as a result of the contact of the metal surfaces, which acquired opposite static charges. Thus, Volta stated that the galvanic and static fluids were identical.

John Baptiste Biot, in a well known summary report (Raporte) in 1803, tried to explain the generation of the electric current in a voltaic pile. He stated that the pile was generating two types of "fluids" which were electrostatic in nature because of the two different metals. He thought that insertion of the conductive pasteboard between the plates prevented the direct interaction of opposite fluids and allowed them to flow in opposite directions. The flow of current was perceived to be due to a chain of impulsive stimuli resulting in a large avalanche of short imperceptible discharges. Biot, even in 1824, did not appear to acknowledge the successive decomposition and recombination described by Grotthuss, Davy, and Berzelius, but attributed the effect to opposite electrical states of the portions of the decomposing substance at the poles.

Rene Just Haüy interpreted Volta's battery as being of purely electrostatic origin, believing that the constant current produced by Volta's battery was a result of a process of successive fast discrete impulses (9).

Grotthuss as a student in Paris stepped in at the time when the electrostatic model was the only way to explain the galvanic phenomena. He was able to recast established facts into a new perspective relating the chemical and physical phenomena. These original ideas presented by Grotthuss can best be understood by an examination of his original paper, which was translated into English and appeared in the *Philosophical Magazine in* 1806 (4). It is of interest to follow the ideas presented by Grotthuss, which have had such far reaching influence on the future research and that put to rest the electrostatic interpretation of the mechanism of voltaic battery.

In the first section of this paper "Action of Galvanic Electricity upon Certain .Bodies Dissolved in Water" Grotthuss set out to investigate the electrolysis of various salt solutions. In the opening sentence he sets the tone (10):

> Without wasting time on the discussion of the multitude of imaginary hypotheses invented to explain the decomposition of water by the electrometer apparatus, I shall give a general theory of the decomposition of liquids by galvanic electricity, which, in my opinion, brings the effects of the latter to a simple and satisfactory explanation.

He confirmed that certain metals were deposited at the negative pole (connected to the copper disc) and formed crystal growth in the direction of the galvanic current while oxygen was evolved at the positive pole, connected to the zinc disk. With metals that did not deposit at the negative pole, hydrogen was evolved; and at the positive pole a precipitate of oxide was formed. He observed that metals have different affinities, as was later observed by Davy and Berzelius. He stated (11):

> All metals in solution are not equally decomposed by galvanic electricity. From nitrate of manganese I obtained gaseous bubbles at the negative pole in place of a metallic deposit; and it seems that when in similar circumstances, the metal, in solution has more affinity to oxygen than hydrogen has for this principle, it is the water which alone suffers the decomposition.

In the case where the arborisation (deposition of metal) takes place at the negative pole, Grotthuss observed no gas evolution and concluded that (11):

> …either that hydrogen arising is combined with the oxygen of the metallic oxide, or that the action is only exercised upon this oxide and not upon the water. This last conclusion ought to be a true one; for we can scarcely admit that the hydrogen is able to carry off completely the oxygen from the oxides of zinc and iron, as well as from certain acids their solvents, in which these two metals are not dissolved, except after having produced an effect contrary to this admission, by decomposing water.

In addition Grotthuss states (12):

> When the current of galvanic electricity acts upon water either pure or when charged with some soluble substance, the positive pole attracts the *oxygenating* principle, while the negative pole attracts the *oxygenated* principle of the liquid. If the proportion of the components at the latter is variable, it becomes oxygenated at the extremity of the wire in communication with the disk of zinc, and deoxygenated at the extremity of the wire in contact with the disk of copper.

Grotthuss perceived that the battery acted not only as a galvanic generator, but also as a dynamic polarized system composed of particles; i.e., molecules of opposite nature as an extension of the pile.

In the second part of the paper, titled "Theory of the Decomposition of Liquids by Means of Galvanic Electricity," Grotthuss attempted to resolve and to reconcile the views of previous researchers with the theory of the nature of water. He states (13):

> It is first necessary to know if the two products of the galvanic poles come from one and the same molecule of water, or rather from two different molecules; and in the latter case we may ask what becomes of the hydrogen at the place where oxygen is perceived? And in return, what becomes of the oxygen where hydrogen is perceived?

Volta's pile as well as the philosophical ideas presented by Schelling and by others probably gave Grotthuss the idea to develop the concept of polarization. He states (14):

> The column [pile] of Volta, which will immortalize his name, is an electrical magnet, every element of which (i.e. each pair of disks) possesses its negative and positive pole. The consideration of this polarity suggests to me the idea that it might establish a similar polarity among the elementary molecules of the water solicited [attacked] by the same electrical agent; and I confess that this afforded me a spark of light on the subject… Thus, when the galvanic current traverses a quantity of water, each of the two component principles of the latter is solicited [attacked] by an attractive force and by a repulsive force, of which the centers of action are reciprocally opposite, and which, by acting in the same manner, determines the decomposition of this liquid.

In addition, Grotthuss observed that the molecules of water were affected by the action of attractive and repulsive forces in the direction of the galvanic current. The reciprocal action of elementary molecules in contact resulted in recombination, and only terminal water molecules underwent electrolysis. In an illustration in this paper [p 336] he showed that water molecules are polarized in the presence of a galvanic current. Although the molecular formula of water had not been established, it was known that water was composed of two elements—oxygen and hydrogen and that oxygen was more negative than hydrogen. Grotthuss stated (15):

> At the moment of establishing a current of galvanic electricity in this water, the electrical polarity mani-

> fests itself among its elementary molecules in such a manner, that the latter seem to constitute the complement of the pile in action.

In summary, this paper presented an original explanation of the electrolysis of water. This explanation postulated that during electrolysis, the molecules of water and salt were polarized and formed polar chains into a unified system. Thus the polarized molecules became the continuation of the copper-zinc couples constituting the pile. He further explained that under the influence of the electrode poles, there formed, in parallel lines in the solution, polarized molecular chains whose members at each end were discharged at the opposite poles. The water molecules touching the electrodes split into the component parts of the water molecules. Thus, at the negative electrode, hydrogen gas was evolved, while oxygen was evolved at the positive pole.

The water molecules continuously exchanged their component parts between their nearest neighbors as well as with surrounding members of the chain. This also meant that leaping interactions were proportional to the transfer of electrical fluid in the microscopic world and that it proceeded in discrete and finite portions. This exchange took place by relays along the molecular chains suggesting the idea of a leap-frog interaction that arose from the transfer process during electrolysis along parallel lines. [Faraday, using Grotthuss's model some forty years later, developed the force line model. (16).] Furthermore, this line of thought led to a concept resembling ionization. In addition, the concept of leaping interactions led to a principle of atomism, discreteness of material objects, divisibility, and a change from static structure to a dynamic representation of matter. The majority of the scientific community accepted the electro-conductivity mechanism proposed by Grotthuss.

For his contribution to the theory of electrolysis he was elected an honorary member of the Galvanic Society of Paris in 1808. The same year he was named a corresponding member of the Turin Academy, and in 1814 he was elected as a corresponding member of the Munich Academy. These high honors were granted to Grotthuss, while he was in his early twenties, despite the fact that he had no formal diploma from any university. Even though the greater part of the scientific community recognized his contribution, some chose not to give credit to Grotthuss. There were prominent researchers such as Davy, who used Grotthuss's original ideas in developing chemical affinity theory, and Berzelius, a proponent of electro-chemical dualism, who never mentioned Grotthuss's original paper in his 1823 chemistry textbook. [Berzelius merely cited him in a minor contribution. (17)]

Davy, in a publication a year and half after the original publication (18), again took the ideas of Grotthuss without giving him credit. In fact, Grotthuss stated that Davy took his paper and left out his drawings. Some 30 years later, in a reference to Davy's work Faraday tried to correct matters by stating (19):

> He mentions the probability of succession of decompositions and recompositions throughout the fluid, agreeing in respect with Grotthuss and supposes that the attractive and repellent agencies may be communicated from the metallic surfaces throughout the whole of the menstruum being communicated from one particle to another particle of the same kind.

The qualitative mechanism of electrolysis of liquids presented by Grotthuss influenced Faraday in his electrochemical studies. Faraday, in his publications (as summarized in Experimental Researches in Electricity) recognized Grotthuss's original contribution on the electrolysis of liquids, and by quoting this work he restored it to the mainstream of research. Grotthuss's original paper provided Faraday with valuable ideas, as indicated in his analysis of the Grotthuss's original paper (20):

> Grotthuss, for instance, describes the poles as centres of attractive and repulsive forces, these varying inversely as the squares of the distances, and says, therefore, that a particle placed anywhere between the poles will be acted upon by a constant force. But the compound force, resulting from such a combination as he supposes, would be but anything but a constant force; it would evidently be a force greatest at the poles, and diminishing to the middle distance. Grotthuss is right, however, in the fact, according to my experiments, that the particles are acted upon by an equal force everywhere in the circuit…but the fact is against his theory, and is also, against all theories that place the decomposing effect in the attractive power of the poles.

Some 40 years later, Faraday confirmed Grotthuss's original polarization concept in an experiment with small pieces of silk threads that became polarized and aligned in solution during electrolysis.

Grotthuss's Investigations at his Home Laboratory (1808-1812)

In 1808 Grotthuss left Paris and on his return to his home estate he stopped at Munich, where he met A. F. Gehlen, the famous German editor of *Physik, Chemie und Mineralogy*. His acquaintance with Gehlen enabled him in later years to publish his original works and to keep contacts with other scientists. Upon returning to his mother's estate, Grotthuss continued to work feverishly in his home laboratory. He acquired glassware from the neighboring apothecary, his childhood friend, Bidder. He built needed equipment such as galvanometers and volumetric flasks for the gas volume measurements. He worked enthusiastically as a hermit far away from scientific communities, having only an occasional contact through journals and correspondence. He regretted not being able to be in direct contact with other scientists except by mail, which was slow and often subject to loss. Periodic bouts of an inherited stomach ailment hampered his work. In the period 1808-1822, while in his mother's estate, Grotthuss carried out numerous studies. During his lifetime he published 76 papers on original research, observations, and proofs, most of which appeared in Western European journals. Some of his very important papers, published in Eastern European journals. were rediscovered after his death.

In 1805, in Naples, he joined von Humboldt and Gay Lussac, who studied volcanic gases and investigated the effect of pressure on the course of gas mixture reactions and at what degree of rarification detonating mixtures would cease to ignite (21). Grotthuss, on his return to Paris (1808) and to his estate (1808-1812), pursued the studies of flames of gas mixtures such as oxygen and hydrogen and other gas mixtures (22-24). He made a fundamental observation that the mixture of gases in narrow tubes will not ignite.

In 1816 Sir Humphry Davy, while studying the mixtures of hydrocarbon gases with oxygen, showed that flames could not pass through narrow tubes, as had been previously reported by Grotthuss. He came upon the idea of screening flames in the lamp with a metallic screen and thus developed the miner's safety lamp—without giving credit to the previous work of Grotthuss. Stradinš, a member of the Latvian Academy of Sciences, in his extensive historical review described the discovery of the miner's safety lamp and Davy's references to Grotthuss (25). In his lecture before the Royal Society in 1817 Davy, for the first time, named Grotthuss as his predecessor in this field; in the same year, however, he stated that his conclusions were very different from those presented by Grotthuss.

Grotthuss responded to Davy's criticism (26):

> Mr. Davy in his paper on flames … mentions me only when he tries to refute or correct my findings on conclusions, at the same time, when he confirms them he takes for himself not only the suggestions of my experiment, but even the phenomena which I had found much earlier than he.

L. W. Gilbert, the editor of the *Annalen der Physik und Physikalische Chemie*, in a letter to Grotthuss dated April 28, 1818, which was found in the archives of Kurland Provincial Museum and published by O. Clement, described the grievances of Grotthuss (27):

> It is incomprehensible how such an honorable man as Davy can have so little fairness and so willingly appropriate everything.

The Last Ten years of Grotthuss Life: 1812-1822

The war between France and Russia (1812) disrupted Grotthuss's normal work, but it enabled him to establish new contacts in St. Petersburg and to open new research opportunities. Before Napoleon's occupation of the region Grotthuss spent six months in St. Petersburg, where he became a friend of A. F. Scherer, a member of the St. Petersburg Academy of Sciences. Scherer provided him with some phosphorescent crystals that influenced Grotthuss in the studies of light and phosphorescence. A founder of the journals *Allgemeine nordische Annalen der Chemie* and *Allgemeine nordische chemische Blätter.* Scherer devoted a considerable amount of space to contributions by Grotthuss in these publications. They described in detail Grotthuss's research dealing with phosphorescence, the influence of alkalis on the electrolytic conductance, and the chemical interaction of light and electricity. Because these publications were not widely circulated in Western Europe, some of his research results were rediscovered and recognized only after his death. Grotthuss published some of his research results in the journals edited by Gehlen, J. E. Gilbert, J. B. Trommsdorff, and J. S. C. Schweigger. In his journal Schweigger included Grotthuss along with the names of Berzelius, H. C. Oersted, T. J. Seebeck, and other famous scientists. Schweigger also published a collection of Grotthuss's proportional weight and material tables, which were widely used by chemists and pharmacists.

After his stay in St. Petersburg Grotthuss returned to the estate, where he continued his research, in particular in the areas of electrolysis and interaction of light. Professor G. F. Parrot at the Dorpat University, Estonia, offered Grotthuss the position made available after the retirement of his associate, Grindel, in 1814. The University Appointment Committee voted by a great majority (15 to 2) to offer the professorship to Grotthuss, who was deeply touched. He was reluctant, however, to commit himself for six years of service because of his health. The final approval of this position would have had to come from Moscow, where it got sidetracked for political reasons.

Grotthuss then joined the activities at the Peter's Academy in Jelgava (Mitau), close to his mother's estate. Here he became an active member of the Kurland's Literature and Arts Association, where he presented papers on research and shared various views with the association members. His papers and those of E. Echwald, M. G. Paucher, K. F. Gauss, and others were summarized in two books published under the auspices of the Peter's Academy. Here he worked closely with his childhood friend Bidder, a well established pharmacist. Heinrich Rose worked in Bidder's apothecary in Mitau from 1816-1819. Here he met Grotthuss and carried out electrolysis experiments of metal ion solutions as guided by Grotthuss. In 1820 Rose contributed a paper titled "Über die Theorie Metallreductionen des Herrn Grotthuss" to the collection of research papers by Grotthus, which appeared in *Physisch-chemische Forschungen* in 1820. Thus, it appears that Grotthuss had a great influence on Rose.

In this period Grotthuss formulated some original ideas pertaining to the absorption of light, which included phosphorescence, fluorescence, and photochemical reactions. He was interested in the physiological aspects of chemical interactions of light with polarized molecular particles. He at first accepted Newton's model of light. However, after studying the crystals exhibiting phosphorescence in 1812, he observed that the phosphorescent light was different from the absorbed light, which was contrary to Newton's mechanistic theory. He concluded that the phosphorescence phenomenon was associated with the motion of light and the structure of the body being irradiated. He proposed that the light at the surface of the fluorescent crystal split into +E and –E components, which, upon interaction with polarized molecules in the crystal, were separated and caused the emission of light of a different color from that used for irradiation, Grotthuss concluded that interaction of light with matter causes specific motions characteristic of colors and thus can dampen or enhance different colors (28). In so doing, he presented the theoretical foundations for luminescence, which were later elaborated in the second half of the 19th century by A. E. Becquerel, D. Brewster, and A. D. Stokes.

In 1818 Grotthuss attempted to develop a unified electromolecular concept of physical and chemical phenomena, and published a paper "Über die chemische Wirksamkeit des Lichtes und der Elektrizität" in 1819 (29). Here he proposed that the combination of different types of electricity (+E and –E) produces heat; and, if the combination of different kinds of electricity is prevented by the atmosphere or an insulating layer, the light is emitted—the color of which is determined by its vibrations. He concluded that "+E and –E" are the original [energy] sources for light, heat, and electricity, which are different modes of [energy] manifestation. In the last part of this paper he elaborated on his ideas presented in the first paper dealing with the electrolysis of water. He stated that in solution even without electrical current there is a continuous exchange between molecules and their elementary parts. These are closed circles of exchanging charged entities (30). This cannot be observed in solution because of the established equilibrium between electrical forces. An external force destroys the equilibrium and closed circles are opened. Molecular chains are formed and are stretched between the poles. At the end of the chains, at the poles, the cascading molecular elements of water molecules are separated. Here also Grotthuss proposes that in solution there exist molecular fragments. Fifteen years later Faraday, in presenting his electrolysis theory, established these fragments as being ions. In studying the tendency of metals to be oxidized or reduced during electrolysis, Grotthuss concluded that –E in all metals is chemically bound. We quote this powerful statement verbatim (31):

> Auf jedem Fall halte ich es für gewiss das alle Metalle –E chemisch gebunden enthalten… [In every case I hold it for certain (I am firmly convinced) that all metals contain a minus E which is chemically bound.]

Almost 80 years later in 1895 J.J. Thompson established the presence of the electron as a basic component of nature.

In the study of iron (III) thiocyanate alcoholic solutions, Grotthuss observed that the solution faded when exposed to light and that the rate of fading was proportional to the duration of the exposure and the intensity of the light. Consequently, in the study of thiocyanate and cyanide complexes of iron (III) and cobalt (II) he discovered the basic laws of photochemistry: that the photochemical reaction could be caused only by the light absorbed by the substance and its rate was proportional to the time of exposure and to the intensity of the light. These observations by Grotthuss were confirmed some 20 years later by J. F. W. Herschel and J. Draper. Eventually, these conclusions became known as the Grotthuss-Draper first and second laws of photochemistry. Some think that these studies in the field of photochemistry would have been worthy of a Nobel Prize if such an award had existed at that time.

Grotthuss pursued research in many areas outside of electrolysis and interaction of light with matter. He synthesized potassium thiocyanate salts of iron, mercury, silver, and gold by fusion of sulfur with the corresponding cyanide salts. He separated iron (III) chloride from manganese (II) chloride by taking advantage of the solubility difference of these salts in alcohol. He also, at the request of the academician Scherer, who was collecting data on mineral springs in the Russian empire, analyzed the mineral springs in his neighborhood (Smardone). Here he used ammoniacal silver solution for the determination of sulfide rather than copper chloride as was customary at that time. While J. W. Goethe was investigating the sulfur sources in the mineral springs of *Bad Berka*, Germany, Grotthuss suggested that the reduced sulfur in mineral springs was the result of the reaction of organic matter with gypsum. He also analyzed the composition of the meteorite, Lixna, which had recently fallen in the estate of Liksna. He concluded that the meteorite was formed in a waterless environment.

Also, in the period 1816-1818 he studied the properties of thiocyanate and thiocyanic acid and developed analytical methods for iron (III) and cobalt (II). At this time Schweigger published a collection of Grotthuss's proportional weights and materials tables, which became widely used by chemists and pharmacists (32).

In his wide ranging investigations, he observed and reported the phenomenon of electrostenosis: namely, that silver dendrites were formed at the very narrow cracks of the glass at the anode because of an electrocapillary effect. This effect was rediscovered 70 years later and elaborated on by F. Braun (1891) and E. J. Kohen (1898), who called it electrostenosis.

In correspondence with Berzelius Grotthuss expressed a wish to work in the renowned Swedish chemist's laboratory, but his failing health prevented the fulfillment of his wish. He continued to work enthusiastically in spite of his failing health and increasingly painful setbacks. In the last year of his life, he was disappointed and felt left out of the main stream of the scientific community. As a result of great suffering, he committed suicide on March 20, 1822 at the age of thirty-seven. He was buried in his mother's estate. However, after the estate was sold to new owners, Grotthuss remains were moved to a new resting place which is unknown. It can be said that even at the moment of his death he searched for the secrets of nature and thereafter he has remained a beacon of light. As he wrote (33):

> Lux lucet in tenebris quamvis nihil obscurius luce. [A literal translation: Light shines in darkness, however, nothing darkens the light.]

Reverberations after Grotthuss's Death and Rediscovery of his Works

Bidder, a life long friend, commented after Grotthuss's death, that in the last years of his life Grotthuss worked sporadically, and that he made a number of rash statements because of his lack of energy to carry out necessary experiments (34). As time passed, his physical existence became more and more difficult for him to bear. Before his death he commented to his friend (34):

> What is the value of a sad life, if I am not able to work and if I dwell only on my errors?

He left his archives and the library to Peter's Academy in Jelgava. By freeing his serfs in his will, he went against the accepted norm of the Kurland high society. His will was contested, and the Kurland Literature and Art Association declared that Grotthuss was mentally ill. Ironically, at one time time he was one of its most prominent members. His will was annulled. With this action, the Grotthuss family and the Kurland high society closed the chapter on the life of Theodor Grotthuss, who dared to challenge the established code of society. Yet as the family tried to forget this episode, his contributions to science sustained his memories and survived the trials of time.

Almost a century later in 1906 his fellow country man, the famous scientist and editor, Wilhelm Ostwald, dedicated a special edition, compiled by Luther and Ottingen, of *Ostwald's Klassiker der exakten Wissenschaften, Nr.152* to Grotthuss. Ostwald commented (35):

> Grotthuss war ein Forscher mit ausserordentlich stark ausgebildeter wissenschaftlicher Phantasie, und so ist es erklärlich, daβ manche seiner Ahnungen sich nicht verwirklicht, manche seiner Beobachtungen sich als unrichtig erwiesen hat. Immerhin geht es auch bei seinem kühnsten Spekulationen stets von experimentellen Tatsachen aus, und seine erkentnistheoretischen Anschaungen klingen häufig an die Kirchoff-Machsche Theorie der Wissenschaften an. [Grotthuss was a researcher of exceptionally well developed scientific vision, and it is obvious that some of his perceptions have been shown incorrect in respect to some of his observations. However, there are in his ingenious speculations always experimental facts, and his perceived theoretical insights resonate repeatedly with Kirchoff-Machsche theory.].

It is interesting to note that Arrhenius in his Nobel Prize lecture in 1903 echoed a view very similar to that presented by Grotthuss a century earlier (36):

> The general tendency in scientific research appears to attach more and more importance to electricity, the most powerful factor of nature, and developments in this direction are now proceeding very rapidly.

Grotthuss in his paper of 1805 stated (37):

> The admirable simplicity of the law to which this phenomenon is submitted coincides, to our astonishment, with the laws of the universe. Nature can neither create nor destroy; since the number of bodies is never augmented or diminished, but all without exception are subject to mutual exchange of their elements; and when we consider the wonderful effects of electricity, which acts often in secret, although spread over the universe, we cannot refrain from pronouncing it to be one of the most powerful agents of grand operations of Nature.

In a lecture at the University of California in Berkeley in 1906 Arrhenius stated (38):

> No simple formula conveys the whole sense of structure of the hydronium ion in water because protons transfer rapidly one H_20 molecule to another…The explanation which is called THE GROTTHUSS mechanism is in the migration of the proton is in fact not an actual movement of the ion through the solvent but a cooperative rearrangement of the atoms: a proton jumps from one 0 atom to the next along a hydrogen bond, the receiving molecule becomes the cation, and one of its protons can now migrate to another neighbor in the same way. The migration is a cooperative process that takes place through a network of several hydrogen bonded H_20 molecules.

The Lithuanian and Latvian Academies of Science in the period of 1960 to the present have undertaken extensive investigations of Grotthuss's contributions to science and have organized special conferences dealing with his life and scientific works. In 1994 the Lithuanian Academy of Science and von Grotthuss family established a special fund to support further investigations into Grotthuss's life. Also, in 1997 in Germany there was established the Förderverein T.v. Grotthuss. The contributions of Theodor Grotthuss have cast his imprint on many large areas of science for over two centuries, and his light is still shining brightly in the darkness.

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CLEAR WATERS AND A GREEN GAS: A HISTORY OF CHLORINE AS A SWIMMING POOL SANITIZER IN THE UNITED STATES

Kevin Olsen, Montclair State University

In a piece in the *New York Times* in 2003, Garbarino wrote (1):

> To most people swimming pools conjure summer afternoons dedicated to carefree indulgences like lime daiquiris and a satisfying bad novel. Pools are pleasure ponds and symbols of suburban arcadia.

According to the US Bureau of the Census, swimming is the second most popular recreational activity next to walking in the United States, with more than 350 million persons participating each year. (2) Today in the United States some eight million families own a residential pool (3), and residential swimming pools are a $6.9 billion industry. (4)

Unlike a lake or a river, a pool has no source of fresh water or a means of flushing out the impurities. Some method of sanitizing the water is needed to make swimming safe and pleasurable. For almost a century, chlorinated compounds have provided it. Even for professional chemists, the chemistry of pool chlorination can be a mysterious art. Little or nothing has been written about the history of this process, and most pool owners have little understanding of its whys and wherefores.

Pools BC (Before Chlorine)

Although it is commonly believed that the ancient Romans invented the swimming pool or public baths, they were actually latecomers to the idea. The earliest known sacred baths date to about 3000 BCE in the Indus Valley. The Greeks built swimming pools near their gymnasiums about 500 BCE, and in ancient Israel public baths were available for ritual washing (5). The Romans built hundreds of bath complexes throughout their empire. The typical complex included swimming pools, warm baths, steam baths, and recreational facilities. With the fall of the Roman Empire the construction of swimming facilities declined in the west, although they remained popular in several eastern civilizations such as India, Turkey, Japan, and Ceylon (6).

The nineteenth-century British enjoyed public baths in India and Japan and brought the swimming pool back home to England. Elaborate swimming baths (as swimming pools were called in Great Britain) quickly spread throughout England and the European continent, especially at fashionable spas. In the 1860s, the local municipalities of Boston, Massachusetts started a program for salt-water bathing. By 1901, the city operated fourteen floating baths, ten public beaches, and two swimming pools. The number of Bostonians bathing nearly tripled between the years of 1897 and 1898, increasing from 657,275 to 1,920,368 (7). The first public swimming pool to open in the United States was in the town of Brookline, Massachusetts, in 1887 (6).

The Victorian era was a time when unprecedented advances in medicine, science, public infrastructure, and industrial technology raised the standard of living for millions of people. Yet, medical science was still unable to prevent outbreaks of diseases such as typhoid, cholera, and dysentery in both the United States and Europe. In the crowded cities of the nineteenth century large numbers of poor people living in tenements had no access to bathing facilities. Beginning in the 1890s, a hygienic reform movement grew in larger cities both in the United States and Europe, the goal being to promote health through cleanliness by providing the urban poor with public baths. Thus the American Association for the Promotion of Hygiene and Public Baths was founded in 1912 in New York City (8). A short time later Dr. Simon Baruch, one of the founders of the Association, persuaded New York City to open several public pools with showers and dressing rooms (8). By the summer of 1936, 80,000 people were using New York's public pools (9). During this era most cities had either built or were building bath facilities, and in time the organization focused less on promoting new facilities and more on serving as a forum for professional bath operators. Only later did the organization begin to promote the swimming pools and serve as a professional organization for pool operators (8). By the 1920s, there were several thousand pools in operation in the United States. About two thirds of them were operated by municipalities, YMCAs, schools, colleges, and Boys' Clubs, the rest being commercially operated (5). At the start of the 1930s, an estimated 42% of high schools in the United States had swimming pools (10).

Table

Sterilization Methods used for High School Pools in the United States, 1930 (Ref. 10)

Method	**Number of schools in the survey**
Chlorine	20
Filter	9
Violet ray	4
Chlorinated lime	3
Gravel and chlorozene	1
Alum and sand	1
Chlorine bubble	1
Chloro-clock machine	1
Ozone	1
Vacuum	1

Jones' paper does not provide details of the processes listed and it may be supposed that his readers would not have needed them. The Chloro-clock machine was probably a simple granulated chlorine feeder fitted with a timer. It probably dispensed "chlorinated lime." The most popular choice "Chlorine" was probably a solution of sodium hypochlorite.

The numbers of home swimming pools also grew during the years between WWI and WWII, but they were only for the wealthy. In 1920, a 20 x 40 foot residential pool (called an "estate pool" for obvious reasons), complete with filter and recirculation system, cost $12,000 to $15,000 in the east and $8,000 or more on the west coast. In 1940 a technique introduced in California of applying concrete pneumatically revolutionized the residential pool market since it cut the construction cost by as much as three fourths (6).

Prior to the introduction of sterilization chlorine, bromine, ozone, or ultraviolet light, most swimming pools were filtered to keep them somewhat clean, and the water was changed frequently (5, 9). Many residential pools were built on sloping ground to facilitate drainage, and some were equipped with storage tanks to hold the replacement water. Because many impurities floated on the top of the water, almost all pools were fitted with "scum gutters" along the edges from which these could be drawn off (11, 12).

Industrial Production of Chlorine at the Dawn of the Twentieth Century

Bleaching powder had been produced in Europe throughout the 19th century for use in the textile and paper industries. The process used at the time consisted of generating chlorine from manganese oxide and HCl and then passing the gas through a solution of potash. In his multi-volume *American Chemical Industry*, Williams Haynes writes that a major impetus for the production of chlorine in the United States was the passage of a favorable tariff on bleaching powders, though he did not specify which particular tariff bill had the most influence on the industry. Favorable tariffs, general industrial growth, and inexpensive electricity created an environment in which domestic production of bleaching powder would rise from 10,979 short tons in 1899 to 155,190 short tons in 1914 (13).

Bleaching powder, calcium oxychloride ($CaOCl_2$), often erroneously called calcium hypochlorite [$Ca(OCl)_2 \cdot 4H_2O$] (13), was made by treating chlorine gas with lime. The chlorine was supplied as a byproduct of the electrolytic production of sodium hydroxide. Passing an electric current through a concentrated solution of sodium chloride liberates chlorine and hydrogen gases and leaves behind concentrated sodium hydroxide, commonly known as caustic soda. It was, and still is, used in a great variety of industrial processes including soap and glass manufacture. In 1895 the opening of the hydroelectric plant at Niagara Falls, New York, made inexpensive electricity available to any chemical manufacturer that chose to locate nearby. In 1906, The Roberts Chemical Company began supplying chlorine to the Electro Bleaching Gas Company, which was the country's first producer of liquid chlorine (13). The area quickly attracted additional electrochemical process plants, which by 1912 included Mathieson and the Hooker Electrochemical Company (14). Niagara Falls is still a major center of chlorine production. As recently as 2006, Olin Corp. expanded this plant by investing $6.5 million to double chlorine production. (15)

Anyone producing caustic soda needed to find a market for the chlorine byproduct. Each time there was an imbalance between the production of sodium hydroxide and the market for chlorine, chemists were set to work discovering new uses for the latter. The history of this search, as well as its economic and environmental consequences, is discussed in Thornton's book *Pandora's Poison, Chlorine, Health, and a New Environmental Strategy* (16). Outside the Niagara Falls area, the major chlorine producers were Michigan firms: Dow Chemical in Midland and the Pennsylvania Salt Manufacturing Company in Wyandotte (14).

The First World War dramatically increased the demand for all types of alkaline materials as well as for chlorine. Aside from military uses of chlorine for chemical warfare, it was in demand by the dyeing and bleaching industries. The loss of German supplies of chlorine forced European manufacturers to buy the material in the United States (17, 18). As more chemical production was dedicated to the Allied war effort, electrolytic chlorine producers found they could easily scale up production because their chief limiting factor was the supply of electricity. This gave them a huge competitive advantage over companies using other technologies such as the lime-soda method (19). The First World War also exerted another beneficial effect for the chlorine industry; namely the experience gained in the manufacture, handling, and shipping of chlorine-filled gas shells that would prove invaluable in peacetime.

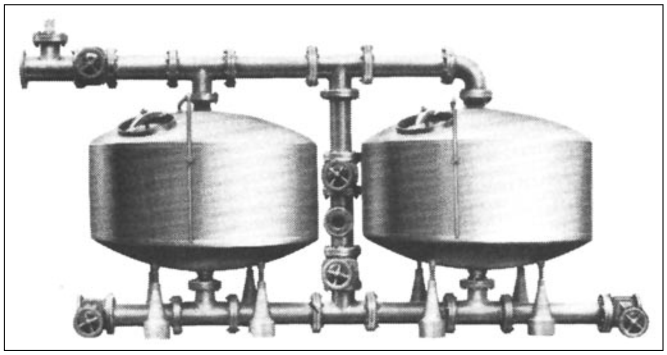

Figure 1. *These two sixty inch high flow-rate filtration tanks date to the late 1960's. They were designed to hold a single filter medium and could accommodate 46,800 gallons per hour. Courtesy Council for National Cooperation in Aquatics*

Early Use of Chlorine in Water Disinfection

Credit for the first use of chlorine to disinfect potable water goes to the British scientist Sims Woodhead, who used "bleach solution" as a sterilizing agent during an 1897 typhoid outbreak in Maidstone, Kent. This temporary measure entailed introducing the solution at the distribution mains (20). The first regular use of chlorine for potable water treatment in the United States began at the Jersey City Boonton Reservoir in 1908 (21). In 1914 the US Department of the Treasury promulgated the first bacteriological standard for potable waters in the United States. The limit of 2 coliforms per 100 mL of water applied only to interstate water supply systems.

From the time when chlorination of potable water first began, there were alternative methods for water sanitation available, such as the use of ozone. This technology was first used in the Netherlands in 1893 (22). High start-up and equipment costs for systems like ozone disinfectant, when contrasted with an abundant supply of chlorine from caustic soda manufacture, meant that few alternative technologies could compete with the price and convenience of chlorine.

Destroying bacteria is only the first half of potable water purification the other critical element being a filtration system. Originally incorporated into waterworks as a means of removing sediments, color, and the organic materials causing odors, sand filtration was soon recognized by engineers as a useful means for removing harmful bacteria. The first modern city with a water supply purified by filtration was Paisley, Scotland, in 1804. It was built by Joseph Gibb to supply his bleachery with water. Although the city of Poughkeepsie, New York, had one of the first sand filtration systems in America in 1872, serious research in the United States did not began until 1887. By

1899 a number of filtration units were operated throughout the United States (23), by which time it was estimated that filtration systems were used to supply potable water to 20,000,000 people in Great Britain and the European mainland (23).

Filtration technologies were of two types. Slow sand filters allowed water to percolate through a thick bed of sand at the rate of 1.5 to 2 million gallons per day per acre of filter bed. This method was inexpensive but required large filter beds. In the Rapid Sand Filtration technology a coagulating agent was first added to the water prior to filtration. This allowed the filtration to proceed an estimated fifty times faster than with the slow sand technology and the system required only about 2% of the filter bed area necessary for the slow sand (24, 25).

Into the Pool

By the early 1900s waterworks engineers had mastered the use of chlorine and filtration. Educators and health professionals appreciated the value of swimming for physical fitness. All of the elements of the modern swimming pool were in place. As near as the author can determine, the first attempt to sterilize a pool in the United States with chlorine was made at Brown University in Providence, Rhode Island. When Brown University's Colgate Hoyt Pool was opened on March 2, 1903, and filled with city water, the university quickly discovered that while "pure enough for drinking purposes," a large mass of the water exhibited a brown color. Once a mechanical filter had been installed, the 70,000-gallon pool could be filled in about 18 hours with filtered water (26). The 1911 chlorination experiment was reported in the *American Journal of Hygiene* and summarized in an item appearing in the *New York Times*.

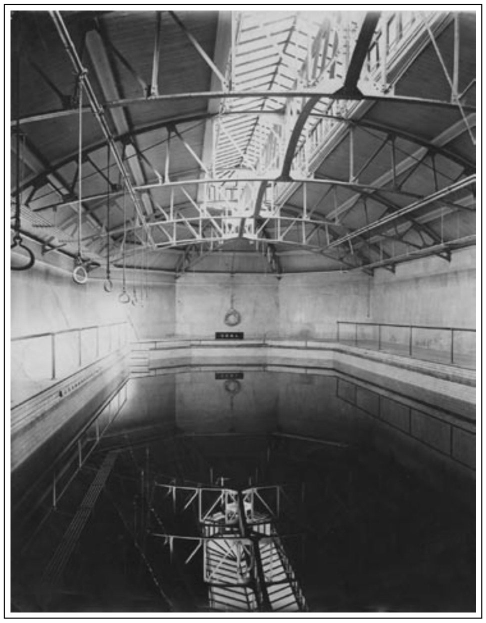

Figure 2. *Colgate Hoyt Pool, courtesy John Hay Library, Brown University*

The experiment was performed by John Wymond Miller Bunker, a member of the class of 1909. Bunker graduated with honors in biology and then went on to earn a M.S. in 1911 and a Ph.D. in 1912. He subsequently held positions as instructor in sanitary biology at Harvard University and director of the biological division of Digestive Ferments Co. in Detroit. Bunker was appointed Assistant Professor of Physiology & Biochemistry at MIT in 1921 and made Professor in 1928, where he remained until his retirement in 1952 (26). Although the Brown University pool had never been the site of a serious infection, some ear-aches and minor nasal infections had been reported by members of the swim team. Prior to the initial chlorination experiments, when the pool water was still, Bunker measured bacterial counts of 300 to 500 bacteria per cc (incubated on agar at 37°C for 24 hours). When the deep end of the pool was stirred up by use, the counts rose as high as 1000 (27). Bunker's first application was "hyperchlorite of lime" (sic) to 2 liters of pool water at a concentration of 1 ppm. The results were spectacular. Bacteria counts went from 700 to 0 in only 15 minutes. The experiments were duplicated with an application at 0.5 ppm. The full-scale application came next (27). The powdered compound was placed in a cheesecloth bag, was dragged over the pool until the material was distributed, the final concentration of chlorine being 0.5 ppm. Surface bacteria counts fell from 500 to 30 in only 15 minutes, to 10 in 30 minutes, and "complete sterility" after an hour (27). According to the *Times* report, the pool remained sterile for four days. (The *Times* report did not indicate whether anyone was swimming in it during those four days, but it does not seem likely.) (28). The news item went on to report that there was "no odor and no perceptible taste." The report concluded by saying "hypochlorite (sic) of lime is an effective sterilizer of swimming pools (28)." (Hyperchlorite of lime and hypochlorite of lime are actually the same material.)

About the same time as the Brown University experiment the *Lancet* was publishing papers related to the bacterial contamination of swimming baths and means to sterilize them, including chlorination (29, 30). A decade later the *Lancet* reported that chlorine levels of 0.5 to 1.0 ppm were sufficient for this purpose (31).

By 1923 seven states had passed regulations for the control of swimming pool sterilization (32). The technology of pool chlorination was still not fully developed, however. During the 1920s pools still had to be drained periodically and the entire volume of water replaced (33).

During the 1920s there were no pumps or filter devices designed for use with residential pools. Contractors usually adapted pumps from the marine industry and filters from either the chemical or dry cleaning industries. The first specialty pool supply company was founded in 1925 by John Mudge, a former chemical engineer. In 1929 Mudge also founded the first pool maintenance company, Chemtech, in California. One of Mudge's equipment inventors, Dave Cavanah, was the first head of Chemtech (33).

A survey of high school pools in 1930 revealed that most used "Chlorine" or "chlorine in one of its forms" for sterilization. The results of the survey are listed in Table I (10). The pool manager of the 1930s not only had to contend with the issues of chlorine levels, alkalinity, and water clarity, but the state of swimwear. In 1937, the pioneering women's physical education promoter, Mabel Lee, offered a number of suggestions. Because wool bathing suits tended to shed lint that clogged filters and the fabric dyes came off in the water, a standardized "ugly, gray-cotton uniform" was preferred for collegiate swimming. Lee wrote that in the 1930s treated wools and less soluble dyes made suits available in "Gay colors, pleasing styles, and materials that hold their shape". Even so, she recommended that the school purchase swim suits for students so that no one would enter the pool with an older or poor quality suit that would shed lint and leak dye (34).

Figure 3. WPA Poster titled "Swim for health in safe and pure pools," Library of Congress.

The Common Methods of Chlorination

For most of the twentieth century there have traditionally been four major forms of chlorine used for swimming pool sterilization. All of them were in use by the middle of the century and, except for tanks of chlorine gas, continue to be widely used today. When certain chlorine compounds (or any oxidizer) are added to a pool, it oxidizes the organic materials and combines with certain inorganic species. The amount of oxidizing agent required to destroy the impurities present at the time of addition is the "chlorine demand" and the unchanged material is the "residual chlorine." Most pool chemical suppliers recommend keeping the residual chlorine at a concentration of no more than 2 ppm. "Shocking" a pool refers to adding excess chlorine (2.0 to 5.0 ppm) so that once the chlorine demand is met, a massive excess of chlorine "shocks" the water.

For very large pools the least expensive source of sterilizer has been compressed chlorine gas, which passes into the water at a measured rate. The tank usually sits on a scale so that the operator would know when it is running low. The problem with this method is that chlorine gas (Cl_2) is deadly and the tanks have to be checked regularly for leaks. Pool staff must know how to handle the steel tanks and need respirators in case of emergency (35). Over the years there have been a number of accidents involving this system. On July 14, 1965, 23 children were treated for chlorine gas exposure at the Rochelle Community Hospital in Rochelle Illinois, because excess gas had been released into the water after a chlorine tank valve had been left open (36). The other drawback to this system is the formation of HCl, in addition to hypochlorous acid, HOCl, when chlorine gas is added to water. HCl lowers the pools pH without appreciably contributing to sterilization. This system requires the addition of about one pound of soda ash (Na_2CO_3) for every pound of chlorine. (35)

The second method is "liquid chlorine," or more commonly, bleach. This material was sold in small bottles for home pools or in large drums for bigger operations. It is an approximately 10% to 15% solution of sodium hypochlorite. (37) Sodium hypochlorite is made by treating calcium hypochlorite with a solution of sodium hydroxide. After the reaction is complete, an excess of sodium hydroxide prevents breakdown of the product and the release of chlorine gas. The typical liquid chlorine product contains 13 g of NaOH per liter. This may or may not be a good thing. Sometimes this negates the need to add alkalinity-increasing chemicals to the water, but when the water is already alkaline, the pH may become unacceptably high (38). Because it is a liquid this product is easier to handle. It can be automatically fed into the water or simply poured in by hand. It does deteriorate in sunlight and warm temperatures, which are of course exactly the conditions where pools receive the most use. The other major drawback is that sodium hypochlorite cannot be used with hard water since it leaves calcium deposits. When these clog the automated feeders, a rinse with hydrochloric acid is necessary to clear them (37).

Figure 4. *Installed at the New Hyde Park Municipal Pool, in Hyde Park, New York, this late 1960s system automatically monitored and controlled chlorination, alkalinity, filtering, and water levels. Courtesy Council for National Cooperation in Aquatics*

Calcium hypochlorite, used in most private pools, has a number of advantages. A dry, white compound, it will release 70% of its weight as free chlorine when dissolved in water (37). Unlike chlorine gas and sodium hypochlorite, this compound will not appreciably change the pH of the pool water. Calcium hypochlorite is sold as a granular powder or pressed into slowly dissolving tablets (37).

By the middle of the twentieth century a new class of chlorinating compounds was available. These were compounds consisting of chlorine with cyanuric acid. Early experiments demonstrated that sodium dichloroisocyanurate and potassium dichloroisocyanurate are the most stable and have the best solubility of this class of compounds. Like earlier compounds, these materials function as a source of hypochlorous acid. The disadvantage of these materials is that the pool water must contain some cyanuric acid as a stabilizer. This prevents the chlorine from being lost to ultraviolet radiation. On a sunny day, as much as 70% of chlorine may be dissipated from an unstabilized pool (38). When used correctly the combination of dichloroisocyanurates and cyanuric acid stabilizer provides long lasting chlorine, good solubility, ease of application, and is unaffected pH (38). Today the typical residential pool owner uses a combination of isocyanurates pressed into slowly dissolving tablets for steady, long term chlorination and calcium hypochlorites for periodic "shock treatments."

Alternatives to Chlorine in Swimming Pools

From the outset there were those who objected to the odor of chlorine in their pools, and so a search began for alternatives. It should be pointed out that the so called "chlorine" odor found in pools is not in fact chlorine. The odor is that of chloramines, which are the reaction products of chlorine and nitrogen-containing organic compounds.

Ultraviolet sterilization was an early alternative to chlorination. In 1919 the Hotel Pennsylvania was advertising that its two swimming pools were "filtered clean, and then purified by violet rays. No Chlorine, no Chemicals (39)." (The hotel's women's bath was open daily except Sunday 10 to 7, and the men's bath was "always open.") The hotel also offered "all electric treatments," baths, manicures, chiropody, and massage, all by "Highly expert operators (39)."

W.A. Manheimer, Ph.D., was the secretary of the American Association for Promoting Hygiene and Public Baths. In the early 1920s he conducted water purification experiments at the research laboratories of the New York State Department of Health. As a result of these experiments and subsequent field trials, Manheimer concluded that ozone was superior to chlorine for swimming pools. Chlorine was unsuited to waters with high concentrations of organic matter because of the odor problem. He went on to point out that since ozone is insoluble in water, there were no upper limits on the amounts that could be introduced. (40)

In 1934 a chemist named C. H. Brandes developed a method of introducing silver ions into a pool as a sterilizer (41), but the cost of treating pools by the electrolytic production of silver ions was considerably higher than that with chlorine. The effectiveness is also limited by the presence of interfering compounds in the water (42). The use of an electric current to create ions for bacteria and algae control is gaining in popularity, and there are a number of these systems available to pool owners today.

The Postwar Period

How many Americans knew how to swim at the start of WWII? Although an exact count is impossible, some statistics are available. A survey of seniors at Kansas City high schools revealed that 90% of the boys and 72% of the girls could swim (43). Swimming as a competitive sport, especially for women, gained popularity after the establishment of the modern Olympic Games in 1896. The sport received another boost when, during WWII, thousands of men and women were taught to swim as part of their military training.

Aside from advances in chlorine chemistry, the most important innovation in pool purification technology was the introduction of diatomaceous earth as a filter medium. It gained widespread use after WWII. The tiny skeletons of one-cell organisms make an excellent filter medium; it has been estimated that as much as 95% of the bacteria in water can be removed by filtration through this material (44). Another important change to filtration technologies was the introduction of new types of skimmers. The Paddock Pool Company of San Mateo, California, invented a floating skimmer during WWII. This device was followed by a skimmer built into the pool wall, introduced in 1952. The impetus behind these innovations was the introduction of the free-form pool, which could not be fitted with scum gutters. (12)

With the outbreak of the Korean War in June, 1950, the economy went back on a wartime footing, and strategic materials were once again rationed. In January, 1951 the National Production Authority ordered producers to supply public health authorities (waterworks and sewage treatment plants) with the same quantities of chlorine as they had in 1950. The availability of chlorine for pools was described as "doubtful." The needs of swimming pool operators and owners were specifically excluded from the definition of public health (45). At the time, US chlorine production was 6,000 tons per day, while chlorine demand was estimated at 10,000 tons (45). Twenty years later US chlorine production would reach 26,000 tons per day, and in 2005 it was up to 35,000 tons per day (46).

Polio was the dark cloud hanging over what should have been the carefree summer days of the immediate postwar period. Although the disease was first identified at the end of the 1800s, and the first serious outbreaks in the United States occurred at the time of WWI, polio was most common in the period between 1942 and 1953. The epidemic peaked in the summer of 1952 with 60,000 cases reported. (47) Because polio outbreaks were most common in the summer months, anything associated with summer were suspect: flies, mosquitoes, sunshine, heat, strenuous exercise, and even drinking cola (48). Swimming was especially suspect because people remembered that President Roosevelt first exhibited symptoms of polio after swimming in an icy bay at his family's vacation home in Maine. Lakes, beaches, and public pools were frequently closed (49). Some people felt safe swimming in pools while others distrusted them. Cold water was suspect as was cloudy water. Some parents forbade swimming in pools but allowed children into woodland creeks and lakes (49). For many, it must have been a relief when in 1946 announcements were made clearing the swimming pool as a potential source of the virus. The University of Michigan School of Public Health announced that conventional means of sterilization, including chlorine or "chlorine dioxide" rendered the virus inactive. G. M. Ridenour and R. S. Ingols studied the viability of the poliomyelitis virus in waters used for both drinking and bathing. They concluded that chlorine levels low enough to keep the water palatable were still high enough to inactivate the virus (50). In January, 1946 *The Journal of Pediatrics* published a study on the means by which polio was transmitted. Although polio would continue to resist all prevention efforts of both laypersons and the medical community, a few useful facts did emerge. The authors concluded that there was no evidence that "water supplies, milk supplies, or swimming pools were means by which the disease was disseminated (51)."

Some of the historians with whom the author has corresponded think the polio outbreaks of this period were responsible for replacing the natural swimming hole in favor of the man made pool. In the final analysis though, postwar affluence had far more to do with the rising popularity of swimming pools. The increasing popularity of pools prompted both the American Public Health Association and the US Public Health Service to develop standards for their care. The latter agency developed a model ordinance governing the construction and use of public pools. It was intended to be adopted by municipalities but was not binding. The Health Service hoped soon to complete a comparable ordinance for private residential pools (52). The American Public Health Association devoted much of its annual meeting to creating a set of guidelines for swimming pool care. Meeting in Detroit's Cobo Hall in November, 1961, the association invited representatives of the pool industry, a decision that sometimes resulted in heated debate (52). Eric W. Mood of the New Haven, Connecticut, Health Department chaired a subcommittee on water disinfection. The Committee issued a recommendation that residual chlorine should be maintained at 0.4 ppm and that for killing algae, copper sulfate should be added at a level of 1.4 ppm. Mood warned that copper sulfate should not be used daily, lest it turn blonde hair green (52)! Dr. Walter L. Mallmann, professor of bacteriology at Michigan State University, reported at the meeting that streptococcus was discovered at "otherwise adequately chlorinated pools." The conditions, however, were those of crowded pools and none of the bacteria was found in empty pools. Since the bacteria were characteristic of the respiratory tract, Mallmann believed that they escaped from the noses and mouths of swimmers and survived long enough to infect others (52).

The health recommendations came at an opportune time. According to *Life* magazine, Americans were spending $250,000,000 on private pools in 1960 (53). Chlorinating these pools with tablets had become easier when, in 1956, Olin Mathieson introduced a floating polyethylene mesh basket that could be suspended in a pool and allow chlorination tablets to dissolve (54). The chemistry choices confronting the pool owner or operator of the early 1960s were about the same as those of today. The five categories of products were those for disinfection, pH control, algae control, stabilizers, and flocculating agents (55). Liquid chlorine (or more properly an alkaline solution of 10 to 15% sodium hypochlorite) was among the most popular types of chlorine at that time, but an average sized pool required as much as one gallon per day. Lithium hypochlorite had also recently come onto the market. This material released only 35% available chlorine compared to the 70% released by calcium hypochlorite. These two materials were the most popular types of solid chlorine products (55). The chlorinated isocyanurates were also relatively new; and while they were more resistant to photodegradation than other forms of chlorine, their effectiveness as biocides was still being debated. Bromine, iodine, and silver ions were available but not widely used. Chlorine in one form or another was used to disinfect some 95% of pools in 1963 (55). Algae control formulations based on quaternary ammonium compounds were also introduced, but they did not entirely replace copper sulfate. (Swimmer's hair continued to turn green.) Pool owners were advised to maintain pH between 7.2 and 7.6, as they still are today (55).

By 1963 advances in pool construction techniques lowered the price of residential pools from $10,000 - $20,000 to an average of $4,000. Homeowners willing to buy do-it-yourself kits could have a respectable backyard pool for as low as $1,000. About 15% of pools were lined with plastic (53). It was estimated that by 1970, there would be 1,000,000 pools in the US and 550,000 would be in private hands (53). The estimates proved low. Over 1,000,000 above-ground pools were installed in the country by 1969, and it was projected that another 275,000 above-ground pools 12-feet and larger would be sold that year (56). In 1986 there were 2,569,000 in-ground residential pools and at least another 2,000,000 above-ground residential pools (57). According to the market research firm of PK Data of Duluth, Georgia, in the early years of the 21st century, 8,000,000 US households have swimming pools, about half being above-ground units. (Sales of above-ground pools have tripled since 1980.) There are another 5,000,000 hot tubs in the United States (3, 58).

Methods of automatic chlorination, especially for the homeowner, became a popular accessory and a number of inventors produced devices for this purpose. Dr. Frank Schneider of Part Washington, Long Island, was a retired professor of chemistry at the time he was awarded US Patent 3,622,479 for a small-scale electrical device that turned sodium chloride into chlorine. Schneider's partner in the project was a retired chemical manufacturer, Albert Young, of Fort Lauderdale, Florida (59). Called the Electrochlor, the device was one of the first to use salt water as its feed stock; it was intended for use in potable water sterilization as well as swimming pools. What made the Electrochlor unique was that it "fragmented" the water before it came in contact with the electrodes (59). According to the patent application, spinning disks threw droplets of water from a continuous stream into a chamber with a hydrophobic coating. This chamber's interior had a hydrophobic coating. This prevented the droplets from coalescing and thus provided a continuous electrical path from the electrodes to a swimmer (60). At the bottom of the chamber the salt solution is allowed to coalesce and come in contact with the electrodes. Hydrogen gas is drawn off from one aperture and the dissolved chlorine is drained off through another. (60). Operation of the device required that dilute sodium chloride be dissolved in the pool water and when used for potable water treatment, a separate source of salt water had to be provided (60). The Electrochlor was an early version of a system that is widely popular today. Presently in Australia, more than 95 % of homeowners use a salt water-chlorine system for their residential pools. The benefits of this system are said to include lower maintenance of the pool, cleaner, silkier water, and reduced skin and eye irritation compared to a conventional system of chlorination (61). However, in 2005, the Los Angeles County Sanitation District banned the use of salt water systems in Santa Clarita. According to officials, the waste salt pollutes freshwater when it is drained into local sewers (62).

Chlorine Safety and Chlorination Byproducts

The first fissures in the summer love affair between chlorine and swimming came in 1974 when chemists first discovered that halogens could react with organic material in drinking water to create chloroform and other trihalomethanes (63). As research continued into this subject, additional reaction products were discovered including known carcinogens such as bromodichloromethane, chlorinated acetic acids, and 3-chloro-4-dichloromethyl-5-hydroxy-2(5H)-furanone (64). It was discovered in 1990 that shortly after exercise, swimmers using indoor pools had elevated levels of chloroform in their blood (65). The hunt for a "chemical free" pool took on added urgency, but the question remained whether the average swimmer should be concerned about chloroform exposure. Speaking in 1993, Dr. Eric W. Mood, by then an Associate Professor at Yale University Medical School, stated that while it was possible to generate chloroform in swimming pools, it was not possible to "form much of it in connection with sanitizing swimming pool water (66)." In the same year Joe Thornton, a research analyst at Greenpeace's Seattle office, warned that chloroform and chlorinated acids are very toxic. Greenpeace was not, however, lobbying against the use of chlorine in swimming pools. Instead the group was concentrating its efforts on industrial uses. (66)

While chronic effects were being debated, everyone agreed that acute effects from chlorine misuse are potentially serious. In the mid-1980s, the New York City Department of Health Poison Control Center received about a dozen calls every summer resulting from misuse of pool chemicals. People have been known to splash the chemicals in their eyes, be overcome with fumes while mixing chemicals in their pool houses, and burn their legs by standing in the pool while adding chemicals to it (66). The acute effects of chlorine exposure from swimming in pools are generally not considered serious health risks. Hair can turn dry and brittle. Chlorine from pools, or salt from the ocean, can dry out the skin (67). Dr. Jeffrey R. Haag and Dr. Richard G. Gieser of the Loyola University Medical Center reported that two-thirds of swimmers exposed to chlorine in pool water experienced a swelling of the cornea, and almost all of them showed some erosion of the cornea. This problem can be avoided with the proper use of high quality goggles (67). A recent Belgian study found that regular attendance at indoor chlorinated pools can increase the risk of developing asthma in children. The main cause is believed to be trichloramines (68).

Environmental Effects

According to Scott Klarich, an environmental protection specialist with the Colorado Department of Public Health and Environment Water Quality Control Division, when swimming pool or hot tub water runs off into ponds, lakes, rivers, and oceans, the high chlorine concentration can prove to be harmful to fish and other aquatic life. Even a concentration as low as 0.011 ppm can be detrimental to organisms in the water. Therefore, owners are urged to take precautions when draining the chlorine-contaminated water from pools (69).

A combination of the concerns about health effects from exposure of chlorine byproducts and from pool water discharge has prompted the creation of biological treatment systems. These have been popular in continental Europe for 20 years and are becoming increasingly common in the United Kingdom. At this time only a few have been installed in the United States (70). This system consists of two artificial pools that are connected. One is for swimming and the other is for growing plants that purify the water (70). Pumping water between the two basins is required as are occasional chemical treatments. Plants used for purification are divided into three categories. The emergent category includes sedges, lesser cattails, aquatic irises, and rushes. Submergent plants such as the common waterweed and Hornwort are valued for their high oxygen output. The floating plants category includes pondweeds and common duckweed (71).

A Final Note

There can be no more enthusiastic endorsement of the public's faith in chlorine than the attempt made in 1989 to sterilize an ocean beach by using chlorine tablets. At 4 am on Thursday, July 20th, 1989, the mayor of Wildwood Crest, New Jersey, received a request from five local businessmen to throw chlorine tablets into the ocean. Because it was not clear at the time exactly what the men were planning to do, the call was referred to the town's Public Works Director, Arthur Schard (72). Although chlorine tablets were not mentioned in the conversation, Schard gave the men permission to accompany the town's work crew on their early morning daily beach cleanup. At around 5 am, county inspectors spotted the men throwing chlorine tablets into the ocean. Six days later, Robert Drewnowski, Joseph Salerno, Daniel MacElrevey, Robert Belansen, and Joseph Jablonski were presented with a civil summons for placing "deleterious substances" in tidal waters (72). Wildwood Crest has always been proud of its 100-yard wide beach, but that summer business was down by some 28 to 30% from pervious years. Health inspectors had spent the two days gathering water samples. Some of the samples revealed bacteria levels that were high enough to force four town beaches to close (72). Everyone agreed that the five men were responsible local businessmen who were trying to do what was best for the town, but beyond that, opinion was divided. "It seems like a senseless thing to do," said one resident; and "They tried to do right for the whole town," said another. Mayor Joyce P. Gould said that the tablet throwing "really wasn't sanctioned by the town government." She went on to call the action "futile and ludicrous," but she strongly defended the men in her public statement. "They genuinely felt a concern for the beach," according to the mayor (72). The decision to use chlorine tablets in the surf was not entirely without precedent. Twice a week the town's public works crews would suspend baskets of tablets in the storm drains. Any bacteria accumulating in there would be killed before rainwater washed it out to sea. It is not clear from the press reports whether the five men took their idea from this practice (72).

Conclusions

Chlorine has been providing a means of protecting human health by sterilizing swimming pool water for almost a century. During that time effectiveness and ease of use have been steadily increasing. While initial opposition to chlorine use was due to objectionable odors, since the 1970s there have been an increasing number of concerns over human exposure to chlorinated organic byproducts as well as the release of inorganic chlorine compounds to the environment. There will certainly be an increase in the number of large pools purified with ozone technology but it remains to be seen what alternatives will prove popular among residential pool owners. Despite the increasing concerns over safety, chlorine in one form or another is expected to remain in widespread use in the foreseeable future.

APPENDIX: How does Pool Chlorine Work?

"Chlorine" in swimming pools is something of a misnomer. The active sanitizing agent is hypochlorous acid. When chlorine gas is bubbled into water, or some other source of hypochlorous acid is dissolved in the water, an equilibrium forms with Cl_2, HOCl, and HCl:

$$H_2O + Cl_2 \rightleftharpoons HOCl + HCl$$

The active sterilizing agent is the hypochlorous acid and not the diatomic chlorine. This equilibrium is pH-dependent and the available sanitizer is maximized under slightly acidic conditions. However, even slightly acidic water would damage the pool and so a pH 7.4 to 7.6 is considered optimal. The formation of hydrochloric acid may require addition of an alkaline material to increase the pH.

Meanwhile solar radiation of the water is causing the hypochlorous acid to break down:

$$HOCl \rightarrow Cl_2 + O_2 + \text{small amounts of } ClO_3$$

For this reason stabilizers are added to chlorine tablets, which function as a sort of sunscreen to prevent the photodegradation reaction. The Cl_2 escapes from the surface of the water; when pools are overly chlorinated, it can cause breathing difficulties. Shock treatment formulations contain a source of hypochlorous acid but often without any stabilizer, which is why many manufacturers recommend adding them at night.

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Kevin Olsen is the instrumentation specialist on the support staff of the Chemistry and Biochemistry Department at Montclair State University, Montclair NJ, 07043. As both an owner of a swimming pool and an historian, he wondered who invented chlorine treatments and when they became popular. *Illustrations*

FUTURE ACS MEETINGS

April 6-10, **2008**—New Orleans, LA

August 17-21, **2008**—Philadelphia, PA

March 22-26, **2009**—Salt Lake City, UT

August 16-20, **2009**—Washington, DC

March 21-25, **2010**—San Francisco, CA

August 22-26, **2010**—Boston, MA

March 27-31, **2011**—Anaheim, CA

BOOK REVIEWS

The Boyle Papers: Understanding the Manuscripts of Robert Boyle. Michael Hunter, Ashgate Publishing, Aldershot, England, Burlington, VT, 688 pp, ISBN 978 0 7546 5568 8, $99.95.

Robert Boyle (1627-1691) has never lacked for scholarly attention. An eclectic and prolific author, his books were widely disseminated in his lifetime and remained accessible long after his death. Of his scientific books, *The Sceptical Chemist* has been almost continuously available and was even included in Everyman's Library in 1911 with an eloquent but not entirely reliable introduction by M. M. Pattison Muir. Boyle's "collected works" were first published by Thomas Birch in 1744. They comprised 5 volumes. J. R. Partington quotes one commentator to the effect that "no one but the proof reader had ever been through the whole of Boyle's works in the immense folios of Birch." The normally indefatigable Partington slyly adds, "I am in no position to contradict him." One wonders what either would say about the 14 volumes of *The Works of Robert Boyle* that appeared in 1999-2000.

Indeed there has been an efflorescence of scholarly books on Boyle during the last two decades. Many of these are thesis-driven monographs; the rest are bibliographic and documentary such as the 6 volumes of *The Correspondence of Robert Boyle*. Unfortunately, no one so far has attempted a biography of Boyle to bear comparison with Robert Schofield's magisterial treatment of the equally complex and polymathic Joseph Priestley.

The present volume is for the most part strictly for Boyle scholars. It focuses on a large cache of Boyle manuscripts that have been at the Royal Society of London since the 18th century. Approximately one half of the book is comprised of a detailed, fine-print catalogue of the 20,000 items in the collection. One wonders if even the proof reader has read and digested all of this material.

Commentary is to be found in five chapters written by the principal author, Michael Hunter, and several coauthors.

1. Robert Boyle and his Archives.

2. The Lost Papers of Robert Boyle (with Lawrence M. Principe)

3. The Workdiaries of Robert Boyle: A Newly Discovered Source and its Internet Publication (with Charles Littleton)

4. Robert Boyle's *Paralipomena*: An Analysis and Reconstruction (with Harriet Knight and Charles Littleton) [A "paralipomena" is essentially a supplement.]

5. The Making of Robert Boyle's *Free Enquiry into the Vulgarly Receiv'd Notion of Nature* (1686) (with Edward B. Davis)

With the exception of Chapter 1, these are very heavy going and are fodder only for a Boyle specialist. Chapter 1 does give a useful overview of the progress of Boyle studies over the centuries. In addition it provides a vivid account of the trials and travail of mastering such a huge archive.

One might think that this would exhaust the Boyle legacy but, there are intimations that more is yet to come. *Derek A. Davenport, Purdue University, W. Lafayette, IN 47907.*

The Periodic Table: Its Story and Significance. Eric R. Scerri, Oxford University Press, Oxford, 2007; hardcover, xxii + 286 pp, ISBN 0 19 530573 6, £ 19.99, $xx.xx..

New Ideas in Chemistry from Fresh Energy for the Periodic Law. Henry Bent, Author-House, Bloomington, IN, 2006; softcover, xxv + 195 pp, ISBN 1 4259 4862 6, $xx.xx.

After its inception in the 1860s, the Periodic Table soon began to occupy a position of central importance in chemistry. Following its rise to prominence, the virtues of the Table have been so widely extolled that it has become one of the great scientific icons of our modern age. The Periodic Table can certainly hold its own alongside other notable monuments to our scientific prowess such as the doubly helical coil of DNA or the frequently quoted Einstein equation that relates mass and energy: $E = mc^2$. In spite of its celebrity, however, the responses to the Periodic Table of those most involved with it—the chemists—have often been strangely conflicted. Although chemists, like other scientists, generally applaud the monumental achievement that the Table represents and are only too happy to have it adorn their workplace, the Table has somewhat surprisingly been the object of an inordinate amount of wrangling and dissension over the years; and the quarrelling has unfortunately continued down to the present day.

Just what is it about the Periodic Table that makes it so contentious? It is probably fair to say that most chemists hold the Table in such high esteem that it is accorded almost quasi-religious status. The Table is viewed by many as something akin to an object of veneration, and it is embraced by virtually everyone as the foundation stone of the doctrine of chemistry. But chemists who uncritically admire the Table tend to be those who make comparatively little direct use of it, such as teachers of chemistry. For others, especially those whose research has a direct bearing on or relevance to the Periodic Table, the story tends to be quite different. Simple admiration for the Table all too often becomes transformed into a notable zeal to modify the Table in ways that match the needs of individual researchers. The latter thus become advocates of a restructuring of the Table, the changing of its shape, or the repositioning of certain elements, or alterations of some other kind that will render the Table more appropriate for their specific area of application.

Since it is now part of the folklore of chemistry that we have continual disputes and confrontations over the Periodic Table, a situation has been reached in which we have currently in existence something approaching 1,000 different versions! Perhaps it is only to be expected that the sense of conflict surrounding the Table forms a leitmotiv that runs through both of the books under review here. In each book much of the text is taken up with a focus on issues, problems, and challenges that have arisen with the Table, some of which are still ongoing. Rather unexpectedly, however, books that address such controversies and cover in depth the early history and the subsequent development of the Table are a great rarity and in fact only three such books have ever been published in the English language. These three exceptional books are Venable's *The Development of the Periodic Law* (1896), van Spronsen's *The Periodic System of the Chemical Elements: The First One Hundred Years* (1969), and Scerri's *The Periodic Table: Its Story and Its Significance* (2007) under review here. If we compute the rate of appearance of these rare books since the formulation of the Periodic Table, we find that on average only one book of this kind appears every fifty years—a rather startling conclusion that might suggest that Scerri's book is something of a special treat.

In reality Scerri's book does little more than continue and elaborate upon the story told in the two pioneering works that preceded it in 1896 and 1969, and especially that of van Spronsen. Scerri seems to adopt and take for granted many of van Spronsen's ideas, arguments, and even his illustrations. In most cases, he makes use of them without special acknowledgment. Thus, Scerri admits van Spronsen's contentions that there were so-called "precursors" as well as actual "discoverers" of the Periodic Table, that there were precisely six independent discoverers, that the six discoveries were made during the years 1862-1869, and that the specific discoverers in chronological order were the Frenchman Alexandre Beguyer de Chancourtois, the two Britons John Newlands and William Odling, the Danish American Gustavus Hinrichs, the German Lothar Meyer, and, finally, the Russian Dmitri Mendeleev. Scerri also supports van Spronsen's assertion that Mendeleev was by far the most important discoverer in terms of his lasting impact.

In addition to covering this familiar terrain, Scerri also delves into a range of contentious topics, though he usually ends up by reaching conclusions that might be described as hedging one's bets. On the issue of the placement of hydrogen and helium in the Periodic Table, for instance, he rather confusingly states that "[p]erhaps there is a 'fact of the matter' as to the optimum placement . . . Perhaps this question is not a matter of utility or convention that can be legislated . . ." Not surprisingly, he never goes on to reach any verdict on this issue. Similarly, on the question whether chemistry—and specifically here the Periodic Table—has been reduced to and fully interpreted by quantum mechanics, we are again given an equivocal answer. Scerri states that the "reduction of chemistry to quantum mechanics has neither failed completely, . . . nor has it been a complete success." When it comes to the matter of the most optimal representation of the Periodic Table, Scerri is equally tentative. On this subject he declares "with some trepidation" that he advocates "general adoption of the left-step periodic system," namely the system first put forward in 1929 by the chemist Charles Janet, in which hydrogen is placed above lithium and helium above beryllium.

In moving on to Henry Bent's self-published book, one is immediately struck by the fact that it contains none of the hesitancy and uncertainties that characterize Scerri's book. Bent knows what he believes and believes what he knows, and there are no two ways about it. In particular, Bent is very strong in his advocacy of the left-step periodic system, the one that Scerri so diffidently suggested as the optimal system. Bent asserts that it would be in everyone's best interest if scientists were to start using the left-step system forthwith. To buttress his support for this system, Bent comes up with dozens of reasonable sounding arguments in its favor. In fact, Bent offers us an astonishing 57 reasons why helium must be located above beryllium rather than neon. The fundamental reason for his assertion appears to be that this placement of helium represents a natural classification of atoms whereas placing helium above neon amounts to an artificial classification of simple substances. Throughout his book, Bent never ceases to try to convince us that the left-step system is by far the best way to exhibit both the primary kinships that exist among the elements within the same group as well as the secondary kinships that exist among the elements of differing groups. Many of his arguments are well constructed and thought provoking.

Although Bent's book is outrageously polemical in nature, it has the great advantage that it is much more forthright and engaging than Scerri's work. Moreover it is also witty, colloquial, informal, and on occasion even a little wacky. But one always knows where one stands with Bent: his arguments are persuasive, his explanations voluminous, and his confidence is boundless. Bent's book is certainly a very unusual one and I can't help wishing there were more like it. His enthusiasm for his topic and the sheer joy he has in discussing chemistry are infectious and he comes across as an author who has thought long and hard before ever putting pen to paper. Typical of the style he adopts is the reason he gives for the great proliferation of Periodic Tables referred to above. He speculates that "God made the initial conditions for the Big Bang such that the evolution of the universe would lead to many-electron atoms and a wacky s-block, in order that chemists and physicists would not be bored." We can safely say that neither chemists nor physicists nor any other interested parties will ever be bored by reading Bent's book. *Dennis H. Rouvray, University of Georgia.*

From Alchemy to Chemistry in Picture and Story. Arthur Greenberg, Wiley Interscience, Hoboken, NJ, 2007. Cloth, xxiii + 637 pp, $69.95.

As indicated in the preface, this book is the result of having consolidated the author's previous two books, *A Chemical History Tour* (Wiley 2000) and *The Art of Chemistry* (Wiley 2003). In so doing, the author has eliminated redundant contents, revised others, and added a few new items as well. The net result is a series of 84 mini-essays or brief historical vignettes, mostly between two and five pages in length, each of which is inspired by various graphics or drawings taken from old chemical books, pamphlets, or advertisements and organized into ten thematic sections in roughly chronological order. Since the selection of subjects is driven by the novelty of the art work rather than the art work serving to illuminate necessarily important historical subjects, the result falls far short of being a proper history of chemistry and more closely approximates a collection of chemical curiosities whose purpose is less to illuminate history than to exploit it as a source of amusement and entertainment. The closest approximations in the older history of chemistry literature would probably be Edgar Fahs Smith's 1927 volume, *Old Chemistries*, and John Read's 1947 volume, *Humour and Humanism in Chemistry*.

To the author's credit, each vignette is properly referenced, though the historical commentary is generally based on standard textbook sources, such as the general histories of chemistry by Ihde and Partington, rather than on specialty articles found in the history of science literature. Indeed, judging by the number of allusions to such writers as John Emsley, Oliver Sachs, and Pierre Laszlo, the author's eye in writing this volume was directed more at the successful writers occupying the popular science section of Barnes and Noble than at the scholarly university community, though both the book's textbook-like format and high cost almost automatically preclude it from ever reaching the same audience.

The book reflects the author's obvious love of all things chemical and especially of those which are in some fashion either novel or even bizarre. Though it has little to offer the serious historian of chemistry, it has much to offer the student or chemist who has only a passing interest in the subject. Indeed, it is a book that the reviewer would have been delighted to have discovered as a student in high school or college when his own interest in chemistry and history of chemistry was just beginning to mature. My only serious criticism is the low quality of many of the black and white line drawings. Several, such as those of Hales' apparatus on pages 270-271 or those of Scheele's apparatus on page 293, are of such poor quality as to be illegible in places, whereas in other cases the images are either dark and murky through overexposure (pp 477-478), out of focus (p 564), or full of extraneous marks betraying their origin on a photocopying machine (p 142). This is almost tragic in a book so explicitly driven by its art work, and one can only wonder that a publisher such as Wiley was willing to tolerate such a low technical standard. *William B. Jensen, University of Cincinnati.*

IG Farben and ICI, 1925-1953: *Strategies for Growth and Survival*. Kim Coleman, Palgrave/Macmillan, Houndmills, UK, New York, 2006, xxiv + 225 pp, ISBN 13:978-0-230-00329-3), £ 50, $74.95.*

The period between World War I and World War II was a time of tremendous change in the European chemical industry. To a large extent, the story of this evolution is related to the formation and growth of two massive chemical conglomerates, IG Farben in Germany and Imperial Chemical Industries (ICI) in England. Coleman does an excellent job of analyzing the economic history of these two companies during the period from the end of WWI to post WWII, providing a useful insight into the factors that affected their development and did so much to create the modern chemical industry.

Armament production for World War I forced the chemical industry to expand on a scale that would previously have been inconceivable. Following the war, the situation changed drastically and many companies were confronted with severe overcapacity. The German chemical industry in particular seemed to face the most severe challenges because it had lost many of its overseas markets, the national economy was in disarray, and the peace treaty that ended the war was specifically intended to cripple German ability, especially in chemicals, to support a new war effort. Although the chemical industry in England faced a less obvious set of problems, there was a series of mergers in both countries that ultimately formed the two giant companies, which are the subject of this book.

Both German and English companies shifted focus from armaments to producing products like dyestuffs and nitrogen compounds, which were required for the peacetime economy. Germany lacked many natural resources and so focused on synthesizing substitutes for materials that were either expensive or not readily available. English industry was constrained by a combination of internal management problems, poor planning, and German competition. Ultimately, economic conditions became so bad in both nations that it became necessary to pursue cartels and secret noncompetitive agreements to divide up what markets were still available. Of course, as the threat of another World War loomed, these economic problems were superseded by a need to support the rearmament efforts.

In both countries the governments recognized that the chemical industry would play a key role in a war, and that this would require the large-scale application of pure science to practical problems. Both companies cooperated with their respective governments, but IG Farben formed an especially close working relationship with the Nazi government. Farben created much of the arms and resources that supplied the German war machine. Coleman suggests that German industry may have been better prepared for this combination of pure and applied science than was industry in England because the leadership of IG Farben was more sympathetic to scientific research than were the directors of ICI. The English failure to recognize the importance of basic research may have placed them at a continuing disadvantage after WW II.

The end of the war seemed to be a disaster for IG Farben. Aside from the economic problems, extensive disruption, and challenges to overseas markets, world opinion also judged Farben to be a major contributor to the German war machine as well as a willing partner in the use of slave labor and the creation of the death camps. Not only did the postwar partition of Germany effectively dismember IG Farben, but the Allies took specific steps to further dismantle the old industrial structure. Despite these problems, Coleman agrees with the conventional wisdom that the German chemical industry rapidly reestablished the significant lead over their English competition that had prevailed throughout most of the period covered by this book. The greater success of German chemical companies, especially IG Farben, has long been recognized; but there has been less agreement about why this happened. The cause of German success is a continuing focus of academic research, in part because answering this question may offer helpful insights into what social and political policies facilitate the success of the chemical industry.

A number of reasons have been suggested to explain the greater success of Germany, but thus far none of the proposals seems totally convincing. The traditional explanation, mentioned above, that the German chemical industry formed stronger alliances with academic research is probably accurate but not sufficient by itself. Another suggested explanation is that Germany's facilities, destroyed by the bombing during WW II, were rebuilt with U.S. money from the Marshall plan, while the British industry had to make do with outdated equipment. Coleman points out (pp 159-60), that this argument is unpersuasive, since English industries received twice as much Marshall Plan aid as their German counterparts.

Coleman does suggest some other factors that may have been important. Since IG Farben made it a priority to find ways to synthesize artificial substitutes for materials that were unavailable, the German chemical industry invested more effort on chemical innovation to create specialty products. The English seemed to continue to produce bulk chemicals that were less research intensive but had a smaller profit margin. In addition, she also points out that IG Farben was led by chemists like Duisberg and Bosch, whereas ICI was led mainly by men who were more experienced in business than science. Despite these suggestive hints, Coleman finally concludes that the lack of adequate documentary evidence, in combination with the complexity of the process, makes it difficult to offer a completely satisfactory explanation.

Economic analysis provides a powerful tool for analyzing the chemical industry, but economics is by no means the only important measure that needs to be considered. A complex combination of other factors is involved, such as government policies, feedstock availability, investment in new technologies, and availability of a strong local market. Coleman might have profitably given more consideration to these other factors; but even so, this little book is an interesting case study for those interested in industrial policy in general or, more specifically, in the history of the chemical industry. Despite the fact that this book does not emphasize chemistry, industrial chemists and historians of chemistry will probably find it to be interesting. *Harry E. Pence, Department of Chemistry and Biochemistry, SUNY Oneonta, Oneonta, NY 13820, USA, Pencehe@oneonta.edu*

* The publisher's web site indicates that publication of this title has been cancelled, but it is still available for purchase.

Der Briefwechsel von Johann Bartholomäus Trommsdorff (1770-1837), *Romershausen-Sertürner*, Vol. 9. Hartmut Bettin, Christoph Friedrich, and Wolfgang Götz, Ed., Wissenschaftliche Verlagsgesellschaft mbH, Stuttgart, 2006, 309 pp, incl. name index for Vol. 1-9 + bibliography & subject index for vol. 9, €19.95. *Acta Historica Leopoldina*, Nr. 18: Lfg. 9 (2006).

Although many of the *Bulletin*'s readers may not know his name, Trommsdorff gained renown in Germany between the mid-1790s and early1830s as editor of the *Journal der Pharmacie* and director of a "chemical-physical-pharmaceutical boarding school" attended by more than 300 students. These endeavors put him at the center during his day of an already vigorous movement there to transform pharmacy from a tradition-based craft into a science-based profession. Trommsdorff's immense correspondence provides an excellent window on an important stage in this reforming movement. Realizing its value in the early 1980s, Wolfgang Götz and others took up the large job of editing and publishing this epistolary treasure. The volume under review is the ninth in a series that commenced in 1987 and will probably wind up—after some 3,500 pages!—circa 2013.

It presents 141 letters—95 from archives and 46 from Trommsdorff's *Journal*—from and, very occasionally to, 39 correspondents. More than half of these letters were from less than a fifth of his correspondents—pharmacists J. C. C. Schrader (20), G. W. Rüde (18), D. P. H. Schmidt (13), and F. W. A. Sertürner (9), chemist A. N. Scherer (9), and parson-inventor E. Romershausen (8). While this pattern accords well with Trommsdorff's orientation to pharmacy, two other patterns suggest that the letters found comprised but a small fraction of his overall correspondence. Well more than half of the letters were written between 1815 and 1830. Moreover, Trommsdorff himself is only represented by letters to physicians H. R. Schinz-Zeller (3) and P. Scheel (2) and to publisher J. L. Schrag (2).

The editing is excellent. The correspondents are reliably introduced with brief biographical essays supplemented by references to existing scholarship and, if need be, archival sources. Only one slip caught my eye—Rüde's year of death is given as 1830 (pp 7, 30-31); but his son's poignant report of his passing reveals that the year was 1831 (pp 72-73). Images, including those of Scherer, Schrader, and Sertürner, accompany several of the thumbnail sketches. The commentaries on details in the letters are useful and well referenced. The volume's foreword ably highlights themes illuminated by the letters presented—Trommsdorff's attention to advances in chemistry and technology, his involvement in efforts to improve the welfare of unfortunate apprentices, his interest in the early gatherings of the Deutscher Naturforscher und Aerzte, and his curiosity about developments elsewhere in Europe. The editors rightly suggest as well that the volume touches on numerous issues beyond Trommsdorff's central concerns. I, for one, was intrigued by the minor place given to religious sentiments when Trommsdorff's correspondents reported the adversities of family and friends and when they offered him comfort in similar circumstances.

To judge from the quality of this volume and from its index to names appearing in earlier volumes, I strongly recommend that individuals interested in German chemistry and pharmacy during the first third of the nineteenth century have their university libraries acquire this series, or do so themselves! *Karl Hufbauer, University of Washington, Seattle.*

Labors & Legacies: The Chemists of Penn State 1855 –1947. Kirsten A. Yarmey, The Pennsylvania State University Department of Chemistry, University Park, PA, 2006, 216 pp, ISBN 1-59971-410-8, $19.95.

This delightful volume is an account about the chemists at one institution, Penn State, as it developed from the Farmers High School in the 1850s into a college and, later, a university. It is a story about the men and women who struggled, dreamed, and sacrificed to build, in words of the author, “A haven of chemical education and research in the middle of rural Pennsylvania.”

Sized 23 x 28 cm, the book easily could find its way onto coffee tables as well as the bookshelves of students of history. It is written in a narrative style that is engaging. Once picked up by the reader, it becomes difficult to put down. The stories bring out the conditions that existed on campus and elsewhere; and they depict the personalities of both faculty and students. There are numerous quotations and excerpts from letters, memos, reports, and newspaper articles (including student publications) written by those who were on the scene at the time. They offer insight into conditions as they existed and tell us much about the individuals who were involved.

The book is divided into five chapters. The first, “Evan Pugh and the Farmers in the Lab,” begins with establishment of the Farmers High School as the second Land Grant College in the country, trailing Michigan State by 10 days. It describes the early beginnings that were crowned by the arrival of Dr. Evan Pugh to be the first President. Fresh from classical training in chemistry in Germany, he had superb abilities and lofty plans. Unfortunately, he died of typhoid fever after only 4½ years. He was an essential player in the founding of Penn State and was responsible for starting a curriculum that emphasized chemical education.

Chapter II describes a period of difficulties for the young institution. Financial woes multiplied. Recognition and acceptance by the public were almost nonexistent. There was a succession of presidents who served short tenures. The chemistry program suffered. At least eight men held the position of Chair of Chemistry and Physics during the period 1846-1888.

Chapter III describes the era of George G. “Swampy” Pond (1888-1920). Colorful, able and effective, he emphasized undergraduate training. Many of his students and associates went on to distinguished careers. Stories about him are legion; many have lasted through the decades. Just prior to his untimely death, at age 57, he had prepared a plaque to be presented to Albert M. Keiser citing him as, “My ten thousandth enrolled student in chemistry.”

Gerald L. Went was Dean for five years during the 1920s. He had strong ties to the chemical industry throughout the country. He led the department into a greatly expanded research program with emphasis on graduate training.

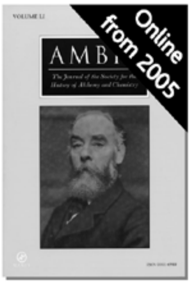
In Chapter V we meet Frank C. Whitmore, who was Dean from 1929 until his death in 1947. Building upon the foundation of an already established graduate program in the School of Chemistry and Physics, Whitmore transformed it into one of the best in the country. He was adept in procuring funds for both teaching and research. He recruited and developed a remarkable group of faculty members. Most worked to exhaustion in efforts to support the nation during World War II. Their contributions were many and of lasting importance.

The account ends with the death of Dean Whitmore not long after the end of the war, except for a 10-page epilogue. It provides a glimpse of what became of a number of the main players who were on board at the time – names such as Chandlee, Simons, Fenske, Dorothy Quiggle, Mary Willard, Aston, and Marker. This reviewer was privileged to know a few of them and to have shared the legacy of numerous others.

There are approximately 160 figures, 80% of them photographs. Emphasis is on people, but appropriate buildings and laboratories are included. Of interest was a photo of the entire college faculty in 1887-88. There were 17 men and 2 women. William Hale Herrick was listed as Professor of Chemistry.

There is a very extensive bibliography, conveniently divided to coincide with the chapters of the volume. Almost all are from the collection of the Pennsylvania State University Archives located in the Special Collections section of Paterno Library.

The book will provide enjoyable reading for all who are in some way connected to or interested in Penn State. For others it provides an accurate account of the birth of a Land Grant institution and of its colorful but often difficult journey toward greatness. Kirsten Yarmey leads us through the epic by depicting the personalities, character, foibles, sacrifices, and accomplishments of the persons involved. *Earl M. Kesler, 534 Beaumont Drive, State College, PA 16801.*



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