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John W. Draper, ACS President, 1876

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The Cover... The first president of the 125-year-old American Chemical Society, an organization whose founding was met with strong opposition. See Bohning's article p 92.

THE 2000 DEXTER AWARD ADDRESS

CELEBRITY CULTURE IN PARISIAN CHEMISTRY

Alan J. Rocke, Case Western University

“Hard as he tried, [Nobelist] Murray Gell-Mann could never make himself into a legend like his rakish colleague and collaborator, Richard Feynman—even if he was the greater physicist.” Thus begins an article in the July 2000 issue of the *Atlantic Monthly*. The author notes that (1):

..there are other factors [besides top honors] that count in the manufacture of fame. Gell-Mann knew how to package ideas, and he had a knack for giving whimsical and unforgettable names to the most abstract concepts in science. Feynman had a more vital gift: he knew how to package himself.

Celebrityhood in science is, of course, nothing new. The earliest obituarist of the French chemist Jules Pelouze (1807-1867) began his article by praising those who successfully raise themselves from modest backgrounds (2):

Even if some allow themselves to be defeated through lack of perseverance, there are others, and more than one would think, who attain prosperity, fortune, and, what is perhaps even preferable to these, celebrity. ... This is the case with the man whom we have just lost, and whom I make bold to call my pupil; for, issu-

ing from my apothecary laboratory, he grew, found fame, and became a celebrity.



Alan J. Rocke

The irony in this particular instance is that in spite of an extremely successful and influential working life, and in spite of this putative celebrityhood in his own day, Pelouze has been almost completely ignored by posterity. Other than five obituaries published at the time of his death, there is virtually no secondary literature on Pelouze. To make matters worse, the nineteenth-century obituaries contradict each other in some of the important dates, and collectively omit some others. Little wonder that the poor writer of the Pelouze article in the *Dictionary of Scientific Biography* ended up by being vague and was sometimes in error (3).

I want to explore here what it meant to be a “celebrity” in nineteenth-century French chemistry, how this status can be correlated with historical significance as measured by posthumous historiography, and what this all meant (and means) in a broader perspective. Confirming the earlier work of Robert Fox and others, we will see that in Orleanist and Second Empire France there developed a distinct culture—or even *cult*—of savant-celebrities, which tended to replace the institution of so-called “no-

tables” of the First Empire and Restoration. This celebrity culture may have played a significant role in shaping nineteenth-century French science, and not always for the best. To instantiate the discussion, I use two pairs of French chemists. The first pair is Pelouze and a slightly older and much better known contemporary, Jean-Baptiste Dumas (1800-1884); the second part will focus on Adolphe Wurtz (1817-1884) and Marcellin Berthelot (1827-1907).

Pelouze and Dumas

Born in the provinces, Pelouze arrived in Paris in 1825 to take his apprenticeship in pharmacy (4). He was desperately poor, living in a garret and surviving at times on bread and water. About 1827 a chance encounter determined his future. Caught in a driving rain walking from Charenton toward Paris, Pelouze flagged down a passing cab, not realizing that it already held a passenger who had hired the driver to take him back to Paris. The driver was reluctant to stop until the passenger, the eminent J. L. Gay-Lussac, insisted on taking the young man in. The two chemists found common interests during the trip, and Gay-Lussac immediately offered Pelouze a place in his laboratory in the Paris Arsenal (5). This was only the second time that Gay-Lussac had taken a private student; the first had been the young Justus Liebig, in 1824. Pelouze and Liebig first met when Liebig returned to Paris for a few weeks in 1828 and visited his mentor’s laboratory. Liebig and Pelouze became fast friends.

In 1831 Gay-Lussac hired Pelouze as his *répétiteur* at the École Polytechnique. Here Pelouze found himself in daily contact with Dumas, who had been *répétiteur* for Gay-Lussac’s colleague L. J. Thenard since 1824. For the next five years, Dumas and Pelouze occupied neighboring laboratory benches at the École Polytechnique. Both also corresponded regularly with Liebig. In the summer of 1836 Pelouze traveled to Giessen to work with Liebig and in subsequent years acted essentially as Liebig’s agent in Paris. In their correspondence with Liebig, Pelouze and Dumas both offered high praise of the rising German chemical com-

munity, often using virtually the same language; they regularly disparaged Parisian academic chemistry.

They also lamented their lack of facilities. Liebig not only had a fine laboratory and a growing group of *Praktikanten*, but his residence was conveniently upstairs from the laboratory, and there were no urban distractions in the tiny town of Giessen. He could virtually live in his laboratory, devoting himself heart and soul to his research and his students. By contrast, life in Paris was immensely complicated. Unlike Liebig, Pelouze and Dumas had no laboratories in their residences, and each had two workplaces (aside from the École Polytechnique, Dumas was also professor at the École Centrale des Arts et Manufactures; and from 1834 Pelouze also worked as an assayer at the Paris Mint). Although this practice of multiple positions (called “cumul”) prevented scholars from starving, it created logistical havoc with their daily lives. Moreover, chemical laboratories in Parisian academic institutions were sadly deficient, and most were scandalously bad.



Jules Pelouze, Oesper Collection

In 1836 Dumas was promoted to professor at the Polytechnique, and simultaneously, with Thenard’s help, he became professeur adjoint at the Sorbonne. Two years later, a vacancy at the Faculté de Médecine was announced. Dumas wrote Liebig to tell him that he had no wish to apply for this position, since he already held professorships at three different institutions. But the Sorbonne had no laboratories at all, and his ill-heated Polytechnique laboratory had become virtually uninhabitable during the Paris cold wave of January 1838. Dumas was desperately unhappy. He formed a new plan (as he subsequently explained it to Liebig): to win the medical professorship, resign the Polytechnique, and then use the resulting higher income to run a private laboratory, which he could also use for his personal scientific research. Connected with this plan, Dumas’s wealthy father-in-law, Alexandre Brongniart, generously built him a house for the laboratory on the Rue Cuvier (6). Dumas wrote Liebig (7):

Since I came to Paris, I have been seeking a way to create a laboratory broadly constituted under my di-

rection. I think I have finally succeeded in this, and that gives me some consolation. In two or three months I will be able to put ten selected students to work in my house, and I will be able to devote four or five thousand francs per year to their experiments. Only then will I be in a position to resume my experiments in competition with yours. At the moment I can't keep pace with you.

The Rue Cuvier laboratory operated for a total of ten years, financed entirely from his own pocket, before Dumas was forced to close it in the chaotic conditions of the Revolution of 1848. However, in this decade the Dumas laboratory played a major role in the history of 19th-century French chemistry.

Many years later, in his *éloge* for Pelouze, Dumas commented that their destinies had been closely linked for forty years, and there is much truth to the statement (8). Dumas had been Thenard's *répétiteur* at the École Polytechnique, while Pelouze was performing the same function for Gay-Lussac. Just as Gay-Lussac and Thenard, once close friends, had fallen out, so also their respective protégés became unfriendly rivals. Both men also taught at the École Centrale and both also provided substitute lectures for Thenard at the Collège de France. Both were elected to the Académie des Sciences at an early age (Dumas at 32, Pelouze at 30). In their parallel early letters to Liebig, both professed abhorrence of the politics of *cumul*, and pledged not to engage in such behavior (9). Of course, both later did just that. When Dumas took the professorship at the Faculté de Médecine, Pelouze succeeded Dumas as professor at the École Polytechnique. Just as Dumas used his new professorship to enable him to open his private laboratory in the Rue Cuvier near his residence, Pelouze simultaneously used his new professorship at the Polytechnique to enable him to construct a private teaching and research laboratory adjacent to his official residence in the Rue Guénégaud at the Mint (10). Pelouze's and Dumas's letters to Liebig leave no doubt that they were both consciously following the model of Liebig's Giessen laboratory.

Many fine chemists were trained in Pelouze's Rue Guénégaud laboratory, or pursued their own original research there, including Claude Bernard and Charles Gerhardt. Seven years after opening this laboratory, Pelouze closed it in order to create a much larger private laboratory school in the Rue Dauphine, near the Mint (and the Seine). This expanded enterprise, lasting twelve years, was highly successful. He had enrollments of around thirty at a time, consisting mostly of young

men from the provinces and from abroad, preparing for future roles in their families' chemical businesses. Pelouze's enterprise could only have benefited when Dumas was forced to close his own laboratory in the spring of 1848, and Gay-Lussac, his patron, was induced to retire (11). In mid-career, Pelouze became successively professor at the Collège de France (1846), president of the Commission of the Mints of France (1848), and chief consultant of the great state-chartered Saint-Gobain chemical works (1850). With these positions, together with his private chemical laboratory school, which operated in two successive locations for nearly twenty years without much competition (12), Pelouze had more wealth and influence than he could have dreamed (13).

Nevertheless, Dumas far exceeded Pelouze in career success. Like Pelouze, as a young man from the provinces Dumas had arrived in Paris without means; and like Pelouze he had also risen quickly. As already mentioned, in 1838 Dumas traded his professorship at the École Polytechnique for the Faculté de Médecine. Upon Thenard's move to the vice-presidency of the Conseil Royal de l'Instruction Publique in 1841, Dumas was promoted from professeur adjoint to professeur at the Sorbonne; and the following year he succeeded Thenard as dean of the Faculté des Sciences. Thenard,

raised to the baronage in 1825, became the *de facto* academic czar of France in the 1840s and relied heavily thereafter on his protégé's recommendations for chairs and promotions. In this way Dumas became the most powerful academic scientist in Orleanist



Jean-Baptiste Dumas

France. Parisian chemical students began to refer to Dumas jocularly at this time as "l'être suprême (14)." His private laboratory became the French analogue to Liebig's.

Politically, Dumas was a center-right conservative, and he had a few queasy moments during the brief Second Republic. However, during the Second Empire he became even more powerful than he had been in the July Republic. For a few years he was Napoleon III's Minister of Agriculture, then was appointed Senator, and also was Inspector General of Higher Education—all while retaining his two professorships. After Pelouze died, he gave up the Inspector General position and his Sorbonne professorship in order to become Pelouze's successor as head of the Mint (a time-consuming but very lucrative post). Finally, in 1867 Dumas was named *Secrétaire Perpétuel* of the Académie des Sciences, the most exalted honor in the French scientific community.

Hence, despite all the parallels between these two men, and contrary to the obituarist's claims with which I began this paper, Pelouze was no savant-celebrity, and Dumas most certainly was. What did it mean to be a celebrity? Scholarly renown was the first criterion, and it is true that Dumas's scientific work, especially in his early career, far outshone Pelouze's. However, wealth, social connections, and (what is more important) the habitual exercise of influence were certainly a part of the mix. The mature Pelouze was modest and retiring, just as he had been in his youth, while the mature Dumas was proud, confident, expansive, and powerful. Pelouze had reputation and position, but Dumas was both world famous and locally powerful in a way that Pelouze never became. Aside from his very real merit as one of the finest scientists of his generation, Dumas had always been careful to cultivate celebrityhood. Pelouze never did.

Wurtz and Berthelot

Let us now move forward a half-generation and take up the case of Adolphe Wurtz and the slightly younger Marcellin Berthelot. Wurtz's background was middle-class and provincial (Alsatian), and he studied under both Liebig and Dumas. Dumas's support was important in gaining Wurtz his principal professorial chair, that of organic chemistry in the Faculté de Médecine in Paris (1853). Later he added a second position, at the Sorbonne.

Unlike all my other protagonists, Berthelot grew up in Paris, the son of a physician. He was successively Pelouze's *préparateur* in his private laboratory school, then Antoine Balard's *préparateur* at the Collège de France; and he gained both men's patronage. In 1859 he was awarded a new chair of organic chemistry at the

École de Pharmacie. Then, a group of influential intriguers succeeded in engineering the creation of a new chair of organic chemistry at the Collège de France; and in 1865 they managed to have Berthelot installed in it. Thus, it can be said that both Wurtz and Berthelot were cumulards, but to a much lesser extent than Dumas and Pelouze had been.

Wurtz and Berthelot had an uneven personal relationship right from the beginning. Part of the conflict was religious and cultural, for Berthelot was an atheist and materialist whereas Wurtz was an idealist and a devout Lutheran. Mostly, however, they clashed over chemical theory. Berthelot was powerfully influenced by Pelouze's anti-theoretical attitudes, which Pelouze had imbibed from his own mentor, Gay-Lussac. Wurtz, on the other hand, adopted Dumas's and Liebig's strong orientation toward theory. In 1854 Wurtz embraced the atomic-molecular reforms in chemistry that had been advocated by such chemists as Auguste Laurent, Charles Gerhardt, Alexander Williamson, and August Kekulé. For the remaining thirty years of his life, Wurtz was the principal French advocate of atomistic chemistry, but those who regarded themselves as anti-atomists—led by Berthelot in the first instance—were successful in opposing him. The Gerhardt-Laurent reforms, rapidly adopted in other European countries, especially in Germany and after 1860, failed to win acceptance in France until near the end of the century.

As already mentioned, between 1853 and 1865 three new professorships dedicated to organic chemistry were established in Paris. However, none was used to promote the Gerhardt-Laurent reforms or the emergent theory of chemical structure. The chairs at the École de Pharmacie and Faculté de Médecine, held by Berthelot and Wurtz respectively, had practice-oriented pedagogical restrictions that excluded any systematic teaching—or even extended discussion—of chemical theory. The Collège de France, on the other hand, was explicitly devoted to pure scientific research, including advanced theory; but molecular theory was the last thing either Berthelot or Balard wanted to consider. Wurtz naturally found this situation frustrating, particularly since the chemical reforms did not seem to be making much headway in France. His chair at the Faculté de Médecine gave him a secure professional position and his teaching/research laboratory was popular and successful; but what he dearly desired was a rhetorical platform from which he could make the case for the new chemistry to a wider circle. His efforts met with mixed results, at best.

By any measure, Wurtz was one of the greatest French scientists of the nineteenth century; his research spanned the entire science of chemistry and was notable for its volume, significance, and influence. Nonetheless, Wurtz's election to the Académie des Sciences was delayed, largely because the number of members was fixed, and there happened to be a dearth of deaths in the Académie after the Dumas/Pelouze generation. The first election in the Académie's Chemistry Section after Wurtz's arrival in Paris was in 1857, after Baron Thenard died. Wurtz was deeply disappointed that Edmond Frémy (professor at the *Muséum d'Histoire Naturelle*) was chosen in preference to himself. There were reasons for this unrelated to intrinsic merit: not only was Frémy senior to Wurtz, but there can also be little doubt that Frémy's mentor and patron Pelouze had pulled some strings.



Adolphe Wurtz

The next vacancy in the Section occurred ten years later, when Pelouze died. Wurtz easily won the election, by a vote of 46 to 3 over Berthelot. The same year, an anonymous ministerial report addressed to the emperor assessing candidates for Dumas's vacated office of *Inspecteur Général de l'Enseignement Supérieur* was frankly critical of Berthelot. The most eminent chemists in France, the writer declared, were clearly Pasteur, Deville, and Wurtz, and Balard was senior to all of them (Balard was chosen) (15). In the following year another vacancy was created in the Académie's Chemistry Section, upon Dumas's elevation to *Secrétaire Perpétuel*. For a third time Berthelot was a candidate, and for a third time he lost, this time to Auguste Cahours (Pasteur and Deville were already members of the academy). Berthelot finally won election to the Académie in the

Physics Section, when a vacancy occurred in 1873. Even then he was third on the Section's nomination list, tied with nine others; nonetheless, he prevailed in the general election (16).

All this suggests that Wurtz's research standing exceeded Berthelot's, but Wurtz's contemporary renown did not translate to posterity. There are no Wurtz memorials in Paris, and until the last eight years there was only a little more Wurtz historiography than that devoted to Pelouze. Marcellin Berthelot, by contrast, lived and died larger than life. The fiftieth anniversary of his first publication (1901) was celebrated by 3,000 invited guests in the Great Hall of the new Sorbonne. His state funeral six years later was marked by speeches by the President and Prime Minister of the Republic and the presence of hundreds of other dignitaries. Interment, for both Berthelot and his wife, was in no less elevated a place than the Panthéon—an unprecedented honor made possible only by a special legislative act. His

statue was erected prominently in the Rue des Écoles, in the square opposite the Collège de France, a square that bears his name today. The centenary of his birth in 1927 was the occasion of elaborate celebra-



Marcellin Berthelot

tions, including the preparation of a sumptuous commemorative volume. No fewer than fifty schools in France are now named in his honor (17).

Wurtz was not a star in the French scientific firmament. Berthelot was. So was Pasteur, for whom an eponymous research institute was created long before

his death. So was Claude Bernard, in whose honor the French government conducted the first state funeral for any scientist (1878). In an earlier generation, so were Thenard, Gay-Lussac, Dumas, Arago, and Cuvier. What made a scientist into a celebrity? It was not just contemporary research renown, for Wurtz had that; and conversely Thenard fell far short of the research productivity of Gay-Lussac, Dumas, or Wurtz. Rather, what one needed in addition was a collection of certain other human qualities, including the ability successfully to promote oneself.

Three qualifications are in order. I do not wish to imply that Berthelot's scientific work was weak, or that he was undeserving of his fame. Partington wrote, accurately, that (18):

Berthelot's work is astonishing in its volume, originality, and importance. ... There must be few chemists of my generation, whatever their interests, who have not more than once turned up Berthelot's publications.

What I do want to affirm, however, is the judgment that the great advantage Berthelot enjoyed over Wurtz, both in reputation among the literate lay public in his own day (though not in specialist collegial circles!), and in posthumous recognition, was not in proper proportion to their respective scientific merits. I believe, on the contrary, that Wurtz's lifetime scientific accomplishments must be judged as significantly greater than his rival's; moreover, I believe that the majority of *expert* authorities contemporary with Wurtz and Berthelot agreed with this judgment. Jean Jacques's recent demythologizing biography of Berthelot (16) is generally on the mark, in my view.

My second qualification is the following. In partially ascribing this unjust disproportion to what I am calling a "celebrity culture," I do not mean to imply that scientific prowess was unimportant to contemporary Parisian opinion, or that other countries refused to engage in hero worship. The "research ethic" was not a German monopoly; the route to success in 19th-century French science was always through research. In particular, Berthelot could not have gained the heights without a superb research record. Moreover, in my first case study, that of Dumas and Pelouze, I hope that I have made my opinion clear that Dumas fully deserved his fame, and also that he deserved to be more celebrated than Pelouze. One might certainly argue that the other savant-celebrities of nineteenth-century Parisian culture, such as Gay-Lussac, Thenard, Cuvier, Claude Bernard, and Pasteur, were also deserving of their "star" status,

on substantive grounds. In fact, the argument I want to make does not concern so much the fate of individuals, but rather the fate of the national community.

Finally, one might legitimately suggest that the ability to create excitement about one's scientific contributions and to persuade others of their importance is an essential element of what it means to practice science successfully. In writing of the "private science" of Louis Pasteur, Gerald Geison (19) rightly notes that "past scientists are great insofar as they persuaded their peers to adopt their ideas and techniques.." However, he adds the important qualification that it is also necessary that "those ideas and techniques [be] fertile in the investigation and resolution of important research problems." Geison affirms that Pasteur's work fully meets both criteria, and therefore "he deserves his reputation as one of the greatest scientists who ever lived." The work of the celebrities Richard Feynman and Jean-Baptiste Dumas met Geison's second criterion by universal agreement. Such agreement is less unanimous in certain other cases—Thenard, Balard, and Berthelot, for example.

Context and Consequences of Celebrity Culture

In many respects, 19th-century Parisian science worked well. In chemistry, there was a galaxy of talent in the middle decades of the century. After the retirements of Gay-Lussac and Thenard, students could attend the lectures of Regnault, Cahours, or Frémy at the École Polytechnique, or those of Dumas and Balard at the Sorbonne. The latter men were succeeded by Pasteur and Deville, and then Wurtz was added, as well. The École Normale also boasted Pasteur and Deville; and at the Collège de France were Pelouze, Balard, Regnault, and Berthelot. Frémy and Chevreul held the two chemical chairs at the Muséum d'Histoire Naturelle, and Dumas and then Wurtz were professors at the Faculté de Médecine. Even this is not a full list of the chemical talent available in Paris, for private laboratory schools were run at various times by Gerhardt, Laurent, Wurtz, and Pelouze; and the private École Centrale des Arts et Manufactures had considerable importance.

Furthermore, in many respects opportunity for advancement ran on admirably egalitarian principles, a legacy of Napoleonic reforms. Admission to the *grandes écoles* was by competitive examination, and the university system was essentially free and open. During the July Monarchy, professorships in the French university system were awarded as the result of complex—and

more-or-less impartial—competitions. The most powerful scientific institution of all was the Académie des Sciences, which has been accused, from the 1830s on, of operating as an oligarchic clique to control access to careers and political power in the scientific community. There is truth to this criticism, but Maurice Crosland has also rightly emphasized the essentially meritocratic nature of admission to the Académie, and its emphasis on research renown (20).

The system worked, in its own fashion, and much evidence points to its health. The fact that success was possible for poor youths such as Dumas and Pelouze, and for others from lower middle-class backgrounds such as Berthelot, Wurtz, and Pasteur, is an obvious positive indicator. Politics was ever-present, of course, but the ardent republicans Pelouze, Arago, and Berthelot, for instance, succeeded in relatively conservative times. Those who exercised the greatest influence, such as Gay-Lussac, Thenard, and Dumas, usually were able to discern talent and excellence in the next generation, and in most cases acted appropriately to promote careers. (The unfortunate cases of Gerhardt and Laurent, ostracized by the powerful in the 1840s and early 1850s, are striking, but fortunately somewhat anomalous.) The quality of research produced by the French chemical community in the middle years of the century was excellent.

Nonetheless, there were pathologies in the system, and some of these had unfortunate consequences. One of these is the characteristic upon which I focus here, a culture of celebrity. Early in the century, influence in France was exercised predominantly by a small privileged elite denominated by the word “*notables* (21).” These men used old aristocratic connections, social position, and wealth to exercise political power; they included a number of scientists—such as Berthollet, Laplace, and Cuvier. In the middle decades of the century, this structure yielded in the broadest terms to the growing power of the bourgeoisie and, in science, to a movement toward increasing professionalization. In the late 19th century, power was no longer wielded by *notables* distinguished largely by their social standing, but rather, in the scholarly world, by top university researchers, academicians, and high-level bureaucrats. This change has been well studied, at least in its broad outlines. What I want to suggest, however, is that certain cultural attitudes survived this socio-economic shift. In the following generation, celebrities now played some of the cultural functions that *notables* had earlier. The notability of the beginning of the century had been con-

ferred principally by social position and wealth; by the middle of the century the first criterion was fame.

This issue is closely related to what Robert Fox has referred to as a “radical change” in French cultural life in the decade or two after Napoleon’s fall (22). Fox observes that a new “declamatory” style of higher education, where oratorical and dramatic effects were emphasized often at the expense of serious treatment of difficult issues, became fashionable in Restoration French culture. This new style was particularly visible at the Sorbonne and the Collège de France, where huge audiences consisting mostly of interested laypeople were attracted to lectures in all fields of scholarship. This situation developed partly because there was no attendance requirement for registered students, and contrariwise there was much interest in elevated subject matter among the educated public. Education became a variety of theater; one had to come early to get a seat, spectators expected to be entertained, and professors hoped for applause. These practices were already noticeable (and commented upon as novel) in the 1820s; by the 1830s a few worried voices were raised, and by the 1860s many reformers viewed them as deleterious to the future of French science. The very public meetings of the Académie des Sciences had gone in a similar theatrical direction, which critics regarded as damaging to the institution’s *raison d’être*—pure science and serious research.

In 1864, the philologist Ernest Renan published a biting essay on the French system of higher education, comparing it unfavorably to that of Germany (23). A German visiting courses in Paris, he wrote, is “very surprised.” The lack of dignity and respect, the coming and going of the students during the lecture, the inattention of the auditors, the theatrical style of the professor, and above all, the applause at the conclusion, strike the German student as curious. “An attentive listener has no time to clap. This bizarre custom shows him once more that the purpose of the exercise is not to instruct, but to shine.” The intellectual danger that France was running, stated Renan, was of becoming “a nation of orators and editors, without concern for essential matters and for the real progress of knowledge.”

The Minister of Public Instruction at this time, Victor Duruy, agreed with Renan. Duruy complained of the predilection among French academic historians of his day for “the depiction of personalities and passions, the analysis of the human heart, [and a] brilliant style of light reading (24),” and he was well aware that the sci-

ences shared the same histrionic style as the humanities. Duruy played the leading role in the creation of a new teaching institution in Paris, the *École Pratique des Hautes Études*. This was to provide a means for the pursuit of careful scholarship, not “brilliance,” and for the creation of new knowledge instead of the recounting of moving stories.

Renan and Duruy were not alone in these opinions. A generation earlier, Liebig expressed a number of similar thoughts, pronounced in his characteristic hard-edged fashion. His 1832 paper, published in his own proprietary journal, charged most French scientists with arrogance, chauvinism, rhetorical bombast, and thievery. The harshness of these judgments was only slightly ameliorated by his suggestion that much of the behavior he described was an inevitable product of certain *structural* characteristics. In particular, he thought that the monopolistic power of the *Académie des Sciences*, “the source of all remunerative positions,” led almost inevitably to an unseemly scramble for success. This was why French scientific papers seemed so arrogant and self-promoting, Liebig thought (25). (Liebig, of course, was not immune to the very faults he imputed to his foreign rivals. No one was more skilled at cultivating celebrityhood than he!—and many accused him, with some justice, of the very same list of crimes: arrogance, chauvinism, rhetorical bombast, and thievery.)

The *Académie* was, indeed, a powerful organization, not *de jure* but rather *de facto*. Given the centralization of French science in Paris, combined with the highly cumulated structure of professional positions, it was almost inevitable that power in the community would be concentrated in a few hands; and the geographic/institutional locus of that clique was the *Académie*. To nearly everyone outside of that clique, and even to some on the inside, this was an unhealthy structure. This accurate perception was the source of the attacks on *cumul*, which we have cited in the early letters of Dumas and Pelouze; of course, both ceased attacking the system after they were brought into the elite. *Cumul* continued to be criticized throughout the century, without, however, being dismantled.

Cumul was made almost inevitable by the resistance of the governing authorities to raise academic salaries to a decent level and to provide chairholders with appropriate facilities for their work. The understandable response of a scientist offered a professorship at the Sorbonne that included no laboratory and a deficient salary was to seek a second professorship.

When teaching duties became overwhelming because of the multiplicity of posts, the less desirable or more exhausting positions could be farmed out to youthful *suppléants*, with whom the chairholder shared half the salary in exchange for all of the real work. The same underlying cause, namely the refusal of the government for proper support of higher education and research, meant that scholars were diverted in their middle and later years into lucrative state consulting posts or politics, rather than continuing their teaching or research. All of this also fed into the celebrity culture that I have described.

Of course, none of this was healthy for the Parisian scientific community. The structure of that community became even less salutary during the Second Empire, when Napoleon III abolished the meritocratic system of competitions for major academic positions. It was a developing sense of imminent crisis in the 1860s that led such leading figures as Wurtz, Renan, Pasteur, and Duruy to sound the alarms. Unfortunately, on the brink of success the Franco-Prussians dealt a temporary defeat to all of those efforts. Only in the new environment of the early Third Republic could effective measures finally be taken for thorough going reform.

Conclusions

I want to caution that my account of French “celebrity culture” is intended at this stage merely as suggestive. Even stipulating the existence of this phenomenon, neither its etiology nor its consequences are clear. Comparisons across national boundaries, which I have not even attempted, are vital to judge whether this really was a “pathology” that damaged the development of French science relative to rival nations. This, of course, requires much more investigation. Contemporary historians of 19th-century French science have done extraordinary service in elucidating the historical developments dealt with here, and my work would have been inconceivable without that foundation. However, this field is still relatively young and undeveloped, compared to others even within the history of science. I would like to point to a few topics I think are worthy of further investigation, under three headings: personalities, institutions, and practices.

Other than Lavoisier, Claude Bernard, and a small number of other examples, French chemists have not been well investigated by historians. Speaking only of my cast of characters, I have already noted that Pelouze has been almost completely neglected, and the same was

true of Wurtz until a decade ago. Dumas has been studied, at times with care, but I think it is fair to say that the Dumas literature is still quite meager, compared to his importance. Even Berthelot, about whom much good work has been written, is still imperfectly understood. As for others contemporary with these personalities, there is almost nothing in the modern historical literature on Thenard, Balard, Frémy, Chevreul, Cahours, or Deville. This is the case, despite the existence of masses of archival materials at the Archives Nationales and the Archives de l'Académie des Sciences.

We also need to know a great deal more about the institutions of science in the nineteenth century. Maurice Crosland has done us a great service with the publication of his important monograph on the Académie des Sciences (26), and we also have fine recent studies by John Weiss, Craig Zwerling, and Terry Shinn, among others, of the École Centrale, École Polytechnique, and the École Normale; but this is only a beginning (27). One obvious desideratum is a study of science instruction at the Sorbonne (28). A second is the Collège de France, the details of which are far too little understood (29). A third is a proper study of laboratories and facilities for research, both official and private. And very much to be desired is a broad study of the politics of science funding in the crucial middle decades of the century, where so little attention has been directed—the generation before the rise of what Harry Paul calls the “science empire” in France (30).

Finally, we would benefit greatly from a study of practices and customs in 19th-century French science. One such example is the system of *cumul*; it would be nice to know more about how it actually operated, from both sides of the lectern. There are all sorts of details that we know little about, including remuneration, actual duties, procedures of selection of—and attitudes of students toward—the *suppléants*, for example. A statistical analysis of how the *degré* of cumulation of Parisian (or French) science changed over time would also be very revealing.

A second area of interest under the rubric of practices is the doctrinal control of pedagogy. I have recently finished a study for which this topic was an important element, but I was frustrated by the limits to what I was able to learn. The usual complaint, then and now, is that French “anti-atomists” threw up roadblocks that effectively prevented the teaching of atomistic chemistry from the 1830s until the 1890s. In general I have found this impression to be reasonably accurate, but many puzzles remain unresolved; and it would be very

helpful to know more about both the effectiveness and the tools by which this influence was exerted (31).

This story must, of course, be embedded within a larger account of the science politics of the day. We know that both Wurtz and Pasteur cried foul in 1863 when Berthelot was awarded a new chair created expressly for him at the Collège de France. They quite reasonably suspected that there had been some behind-the-scenes influence, but it would be gratifying to see the actual details revealed in this and many other similar episodes. It is, of course, possible that the real action happened in face-to-face encounters, or by a written trail that has vanished or is otherwise unrecoverable. This exemplifies one of the difficulties for the historian in dealing with Parisian science of the past. German scientists, spatially separated as they were in the decentralized German states, wrote thousands of letters to each other and to their governments, many of which still exist; Parisian scientists, by contrast, could do much business orally, leaving fewer tools by which the historian can reconstruct the action. Celebrities in particular, the focus of this paper, often do their best promotional work in person.

The situation may not be quite as desperate as I appear to be suggesting. Anyone who has sampled the richness of yet unexploited resources at the Archives Nationales and the Archives de l'Académie des Sciences knows that the historical study of 19th-century French science is still young. These are, of course, only two of a great network of archives, and much still exists also in private hands. Exciting work lies ahead.

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3. The five obituaries are Chevallier (Ref. 2); C. von Martius, *Neues Reporatorium für die Pharmacie*, **1868**, 17, 506-10; A. Cahours, *Moniteur scientifique*, **1868**, 16, 502-8; W. De la Rue, *J. Chem. Soc.*, **1868**, 21, xxv-xxix; and J. B. Dumas, in *Discours et éloges académiques*, Gauthier-Villars, Paris, 1885, Vol.1, 125-98. Pelouze also enters substantively in F. L. Holmes' biography of Justus Liebig in the *Dictionary of Scientific Biography*, and in his *Claude Bernard and Animal Chemistry*, Harvard University Press, Cambridge, MA, 1974. Alex Berman's three-paragraph Pelouze article in the *Dictionary of Scientific Biography* (1974) is excellent consid-

- ering the limitations under which he labored. There is little on Pelouze in Partington's *History of Chemistry*. Celia von Lindern (University of Regensburg) is currently preparing a full edition of the Liebig-Pelouze correspondence.
4. The information on Pelouze in this paper is derived from the sources cited in the preceding note and the following: the government annual *Almanach royal [national, impérial]* (later editions of which add the words *Annuaire de la République Française*); the Pelouze correspondence in the Liebigiana Collection of the Bayerische Staatsbibliothek; and the Dossier Pelouze in the Archives de l'Académie des Sciences, Paris. Some of the material in this paper appears in slightly different form in A. Rocke, *Nationalizing Science: Adolphe Wurtz and the Battle for French Chemistry*, MIT Press, Cambridge, MA, 2001.
 5. This story, related in the Chevallier and Dumas éloges, certainly derives directly from Pelouze, but the date is not certain. Gay-Lussac was a consultant at the large iron works in Charenton, and Pelouze's father lived and worked there. The vehicle specified in the story is a "coucou" (cuckoo), a two-wheeled carriage for hire that soon thereafter went out of fashion. I take it to be similar to the later English hansom cab.
 6. Dumas to Liebig, n.d., postmarks December 19, 1837, January 21, February 25, May [?], and May 21, 1838, Liebigiana IIB.
 7. Dumas to Liebig, n.d., postmarked April 21 and May [?], 1838, Liebigiana IIB. "Depuis que je suis à Paris, je cherche un moyen d'avoir un laboratoire largement constitué sous ma main. Je crois y être enfin parvenu et c'est là ce qui me fait quelque consolation. Je pourrai dans deux ou trois mois faire travailler dix élèves choisis chez moi et je pourrai consacrer à leurs expériences quatre ou cinq milles francs par an. Alors seulement, je serai en mesure de reprendre des expériences en concurrence avec les vôtres. Je ne puis pas aller votre pas dans ce moment."
 8. J. B. Dumas, *Discours* (Ref. 3), pp 196-97.
 9. Dumas wrote Liebig (n.d., late 1831, Liebigiana IIB), "Je suis parfaitement résolu à ne jamais plier le dos devant personne, pour obtenir une place meilleure. Il en résulte que je serai longtemps sans en avoir ... Jamais je n'ouvrirai la bouche contre ceux qui cumulent, jamais je ne consentirai à la création d'une place en faveur de mon chétif individu." Pelouze wrote Liebig (22 November 1836, Liebigiana IIB): "Vous êtes vraiment heureux d'être éloigné d'aussi viles passions que celles qui agitent la grande majorité des gens que l'on appelle savant. Le spectacle de tout cela me rend triste et m'engage plus que jamais à vivre éloigné de toute société. Pour mon compte au moins, je n'ai pas même sollicité la suppléance dont M. Thenard me charge [at the Collège de France]; c'est lui-même que me l'a proposé."
 10. Pelouze had a small laboratory installed in (or attached to) his new lodgings soon after taking the position of assayer in 1834. In his letter to Liebig of August 22, 1835, he wrote, "On m'a donné deux chambres à côté de mon logement. J'en fais dans ce moment un laboratoire et j'espère bien travailler plus que par le passé et d'une manière d'ailleurs moins fatigante." Three years later (May 1, 1838), however, he announced to Liebig the completion that day of "un magnifique laboratoire de chimie qui va remplacer le petit trou qui j'occupais au bout de mon appartement" (Liebigiana IIB). Various witnesses rate the capacity of this laboratory (Rue Guénégaud) as being between six and twelve workers; it is apparently the laboratory that Laurent inherited in 1848. Many students and guest workers accommodated in Pelouze's laboratory can be documented prior to the opening of the better known laboratory treated in the following paragraph.
 11. The best (and almost the only) source of information on Pelouze's laboratories is an untitled, undated 3,300-word anonymous document prepared (probably at Dumas's request) shortly after Pelouze's death (Dossier Pelouze, Académie des Sciences). It is precise, carefully researched, and seems authoritative. The most likely author candidate is Aimé Girard, who supervised the laboratory during the final five years of its existence.
 12. As noted, Dumas's Rue Cuvier laboratory operated from 1838 until 1848. Wurtz ran a similar school from 1850 until 1853, and Gerhardt ran one from 1851 until 1855. Neither of the latter was a success, and the probable reason for that is competition from Pelouze's operation.
 13. According to the information in M. P. Crosland, *Gay-Lussac, Scientist and Bourgeois*, Cambridge University Press, Cambridge, 1978, 230-31, Pelouze's positions at Saint-Gobain and the Mint alone must have brought in about 26,000 francs, at a time when most professorships in major academic institutions earned about 5,000.
 14. A. Lieben, "Erinnerungen an meine Jugend- und Wanderjahre," in *Adolph Lieben Festschrift*, Winter, Leipzig, 1906), 5. Lieben was a student in Wurtz's laboratory in Paris in the years 1856-58 and 1862.
 15. L. Velluz, *Vie de Berthelot* Plon, Paris, 1964, 119-20. The writer was probably Duruy himself.
 16. J. Jacques, *Berthelot 1827-1907: Autopsie d'un Mythe*, Berlin, Paris, 1987, 105-11; A. Carneiro, The Research School of Chemistry of Adolphe Wurtz, Ph.D. Thesis, University of Kent/Canterbury, 1992, 69. Deville had been elected in the mineralogy section of the Académie as early as 1861; Pasteur was elected to the same section in the following year.
 17. Jacques, *Autopsie* (Ref. 16), "Un test de la popularité de Berthelot: Son nom dans la géographie urbaine," and B. Javault, "Berthelot, héros de la IIIème République," both in J. Dhombres and B. Javault, Ed., *Marcelin Berthelot: Une vie, une époque, un mythe* SFHST, Paris, 1992, 121-31 and 113-19, respectively. Jacques has determined

- that the only scientists who have had more French city streets named for them than Berthelot are Pasteur and the Curies.
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 23. E. Renan, "L'instruction supérieure en France, son histoire et son avenir," *Revue des deux mondes*, 1864, [2] *51*, 73-95.
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ABOUT THE AUTHOR

Alan J. Rocke is Henry Eldridge Bourne Professor of History, Case Western Reserve University, Cleveland, OH 44106-7107. He is the author of *Chemical Atomism in the Nineteenth Century: from Dalton to Cannizzaro* (Ohio State University Press, Columbus, OH, 1984); *The Quiet Revolution: Hermann Kolbe and the Science of Organic Chemistry* (University of California Press, Los Angeles, CA, 1993); and *Nationalizing Science: Adolphe Wurtz and the Battle for French Chemistry* (MIT Press, Cambridge, MA, 2001).

JOHN H. WOTIZ

HIST Chair, Dexter Award Recipient

April 12, 1919 – August 21, 2001

OPPOSITION TO THE FORMATION OF THE AMERICAN CHEMICAL SOCIETY ⁽¹⁾

James J. Bohning, Lehigh University

On March 27, 1876, Professor Charles F. Chandler of the School of Mines at Columbia College and seven of his colleagues issued a short but specific invitation to members of the chemical profession (2):

Dear Sir: A meeting for organizing the American Chemical Society will be held on Thursday evening, April 6, 1876 at 8 o'clock P.M. in the lecture room of the College of Pharmacy, University Building, corner Waverly Place and University Place. Your attendance is earnestly required.

In spite of such short notice, 34 men assembled with Chandler at the appointed time on the New York University campus and promptly elected him president of the meeting (3). The subsequent motion by Isidor Walz "that we proceed to organize a national Chemical Society, which shall be called the American Chemical Society," provoked a vigorous discussion which finally resulted in an affirmation that was marred by three dissenters who submitted negative votes (4).

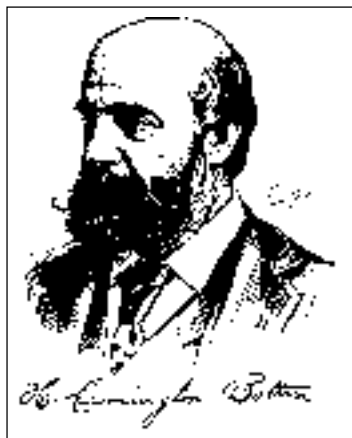
This lack of unanimity in the formation of the American Chemical Society (ACS) did not come as a complete surprise to the meeting's organizers. In fact, it was a continuation of action initiated almost two years earlier during the Centennial of Chemistry celebration held at a public school in Northumberland, Pennsylvania. At that magnificent meeting, generally considered to be the first national meeting of chemists held in the United States, the participants gathered with high spirits in a picturesque setting to honor the discoverer of oxygen, Joseph Priestley, whose last home and burial place were in Northumberland. In keeping with the name chosen for the meeting, they also examined the

development of chemistry in the United States over the centennial years 1774 to 1874 (5). The occasion of this meeting was prompted by a suggestion from Professor Henry Carrington Bolton of Columbia College, who proposed the centennial significance of the year 1874 (6), and by Professor Rachel L. Bodley of the Women's Medical College of Philadelphia, who later suggested the location (7). When Bolton urged the Chemical Section of the New York Lyceum of Natural History to undertake the organization of the affair, it was only natural that President J. S. Newberry appoint him chairman of the General Committee (8).

When the Centennial of Chemistry meeting opened promptly at 9 A.M. on Friday, July 31, 1874, Bolton acted as the temporary chairman. Although the assembly subsequently elected Charles F. Chandler as president and presiding officer for the two-day meeting, Bolton's efforts did not go unrecognized. A special resolution, passed just prior to adjournment on August 1, commended Bolton for his "considerable attention to details" that resulted in "a memorial gathering" to which all could "look back with the greatest satisfaction (9)." While much of that satisfaction derived from the physical location and the Priestley legacy, at least an equal amount must have been derived from the professional contact of 77 chemists previously scattered over 16 states and two foreign countries, and representing at least 25 academic institutions as well as many industrial firms.

For Professor Persifor Frazer of the University of Pennsylvania, there appeared to be a very logical course

Opponents



Bolton



F. W. Clarke



Egleston



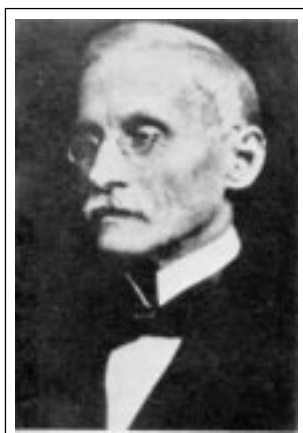
Fraser



Eben N. Horsford



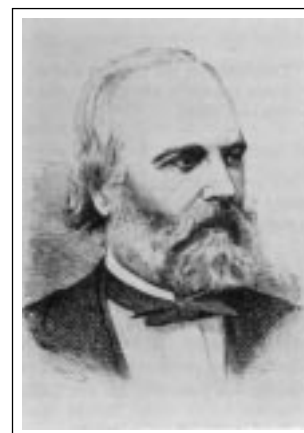
T. Sterry Hunt



Samuel W. Johnson



Benjamin Silliman, Jr.



J. L. Smith

of action to assure the scheduling of similar events in the future. Towards the end of the Friday afternoon session, Frazer proposed (5):

..the formation of a chemical society which should date its origin from this centennial celebration, and urged the importance of the fact that, while American chemists have done perhaps a larger amount of work in their own department proportionately than has been done in the world within the last century in any other branch of science, they have as yet in this country not a single society to represent the chemical thought of the country.

Frazer then moved (5):

..that a committee of five be appointed by the president, to whom shall be referred the advisability of calling a representative committee of chemists of the United States to form a chemical society, and all questions relating to the organization of the society.

While this urge to form an organization of chemists was, in part, a direct consequence of the camaraderie prevalent throughout the meeting, it was not new to the Centennial of Chemistry celebration. In a resolution adopted on May 11, 1874, the New York Lyceum of Natural History recognized that a "social reunion of American Chemists, for mutual exchange of ideas and observations, would promote good fellowship in the brotherhood of chemists (8). The organizing committee continued in its circular to stress that "a reunion of American Chemists ... would ... foster a feeling of fraternity among us." This sentiment was also echoed by correspondents responding to Bolton's first suggestion of the meeting in the *American Chemist* (6).

The New York *Daily Graphic* was even moved to capture this spirit in an unusual chemical metaphor, projecting that (10):

If the chemists who were at Northumberland ... had combined in certain definite proportions to accomplish what was really the obvious purpose of their merely mechanical mixture, ... the world would have cause to rejoice in their synthesis [for] hitherto America has done but little for the science, each chemist being but an isolated molecule giving but little show of affinity for others.

Frazer's motion, therefore, was a rational and natural reaction made with optimism and in anticipation of constructing an organization that would be directly related to the origins of modern chemistry through the Centennial of Chemistry celebration.

The first to respond to this proposal was the eminent mineralogist J. Lawrence Smith of Louisville, Ken-

tucky. Frazer must have been dismayed when Smith immediately stated that there were many difficulties in forming such an organization. "One formidable objection was that this country was too large, and that it would be impossible to centralize its chemical research." Continuing, Smith pointed out that "the very strength of the country is in decentralization. We want all of our scientific institutions dispersed far and wide."

Even Smith must have realized the weakness of this argument, for he then proceeded to present a more direct and specific premise:

We already have two great institutions in the country – the American Scientific Association and the American Academy of Sciences [11] – which undertake to embrace in their proceedings everything connected with chemical research, and it would be more creditable to the chemical talent of this country if an attempt were made to secure its better representation in the chemical section of the former association.

To support this line of reasoning, Smith included examples of foreign organizations:

Even the meetings of the Chemical Society of London, where there exists a great centralization of chemists, are very meagerly attended, the members preferring to read their papers before the more distinguished Royal Society. The same is true of the French Chemical Society, while the attention of the Academy of Sciences of France is constantly asked for papers of the highest importance relating to chemistry.

Smith's biased opinion was only partially correct, for in 1874 chemical societies were not only established in England, France, Germany, and Russia; they were also publishing journals devoted solely to chemistry (12). Yet, in the lengthy and "somewhat heated debate" that followed, only one speaker, Professor William H. Chandler of Lehigh University (and the younger brother of Chairman Charles F. Chandler) "presented forcibly many cogent arguments in favor of the formation of a national chemical society." He was outnumbered, however, by five other speakers who "advocated the earnest co-operation of the chemists as a body with the American Scientific Association, and that if a national chemical society were formed, it should be a permanent section of that body."

An evaluation of the effectiveness of these comments is best made by examining the stature of those who made them. William H. Chandler (age 32) was known to his audience as the co-editor of the first American chemical journal, *The American Chemist*, which he

had started with his brother Charles in 1870 (13). However, those speaking against the motion were also individuals whose reputations had attained national significance.

J. Lawrence Smith (age 55), who held the M.D. degree from the Medical College of South Carolina, was the first person from the United States to study under Justus von Liebig at Giessen. A former professor of medical chemistry and toxicology at the University of Louisville, he had published extensively on mineral analysis and had developed a process for the separation of alkali metals from silicates that bore his name. A cofounder in 1846 of the *Southern Journal of Medicine and Pharmacy*, Smith had recently published his treatise on "Minerals and Chemistry: Original Researches" in 1873. His services to foreign governments brought him decorations from France, Turkey, and Russia (14).

Benjamin Silliman, Jr. (age 57) had published textbooks in both physics and chemistry that were immensely popular in colleges throughout the country and was currently serving as editor of the *American Journal of Science and Arts*, a publication that had been founded by his father in 1818 (15).

Frank Wigglesworth Clarke (age 27) had just assumed his position as professor of chemistry and physics at the University of Cincinnati after previous positions at Cornell, Boston Dental College, and Howard University. In spite of his youth he had published many articles in the popular press. A series in Silliman's and Chandler's journals became "The Constants of Nature. Part I," which had just been published by the Smithsonian Institution in 1873 (16).

Eben Norton Horsford (age 56), having obtained a B.S. from Rensselaer Polytechnical Institute in 1836, was the second person from the United States to study with Liebig. He developed the first laboratory in America for analytical chemistry in the Lawrence Scientific School at Harvard, serving as Rumford Professor from 1847 to 1863 and Dean from 1861 to 1862. In 1863 he resigned his academic position to pursue industrial chemistry, founding the Rumford Chemical Company in Rhode Island from the profits of his invention of the phosphate baking powder as a yeast substitute (17).

Edward Travers Cox (age 53) grew up in New Harmony, Indiana, where he received his early training in chemistry and geology from David Dale Owen, assist-

ing Owen in the U.S. government field studies of the Upper Mississippi Valley and the geological surveys of Kentucky and Arkansas before the Civil War. He made an extensive survey of mining opportunities in New Mexico in 1864 and identified important coal deposits in southern Illinois. Appointed State geologist in 1869 by the Governor of Indiana, he immediately began his series of annual reports on the geology of Indiana. As State Geologist, Cox automatically filled the chair of geology at Indiana University (18).

Peter Henri Van der Weyde (age 61) was a physician with an M.D. from New York University, who held a faculty position with the Women's Medical College in New York. Previously, he had held faculty positions at New York Medical College, Cooper Union, and Girard College. Founder and editor-in-chief of *The Manufacturer & Builder*, which started in 1869 as a "practical journal of industrial progress," Van der Weyde obtained patents in 1867 and 1869 relating to a petroleum distillate product (called "Chemogene") and a compression ice system, which was used to construct artificial refrigeration systems throughout the south and in Philadelphia (19).

Sensing that Frazer's original motion would be crushed under the weight of such heavy opposition, Bolton offered a compromise in the form of an amendment:

That a committee of five be appointed from this meeting to cooperate with the American Association for the Advancement of Science [AAAS] at their next meeting, to the end of establishing a chemical section on a firmer basis.

The assembly gladly and quickly adopted the modified resolution, and Chairman Charles F. Chandler appointed Bolton, Silliman, Smith, Horsford, and Professor T. Sterry Hunt of the Massachusetts Institute of Technology as committee members. This strong allegiance to the AAAS effectively blocked the formation of the American Chemical Society at a time when it could have directly related its origin to the centennial of modern chemistry as it was celebrated in Northumberland.

In order to understand the rationale behind this virtually unanimous rejection of the Frazer proposal, it is necessary to examine the relationship of the objectors to the AAAS. For example, every member of the committee appointed by Chandler and all of the antagonists who spoke against the Frazer proposal were members of the AAAS; three of them were charter members dating back to the formation of the AAAS as a reorganiza-

tion of the American Association of Geologists and Naturalists in 1848 (20). Most were active members as well, holding a variety of offices at the annual meeting that was generally held in the fall of the year. Thus, Silliman had served as assistant secretary (1841-1843), secretary (1847-1848), and chairman (1841-1842), while Horsford had been general secretary (1849-1850). Most significantly, J. Lawrence Smith had served as president of the AAAS at the 21st Annual Meeting held in Dubuque, Iowa in August, 1872, with his term of office ending at the 22nd Annual Meeting held at Portland, Maine in August of 1873 (21).

At the Dubuque meeting only two chemical papers can be identified from the five presented before Section A on "Physics and Chemistry." The following year, however, in Portland, six chemical papers were presented in Section A, including one by F. W. Clarke. It was at the conclusion of this meeting that Clarke and others met informally to present "laboratory notes and informal papers." They found the mutual exchange of ideas so satisfactory that they adopted resolutions for the formation of a separate chemical subsection, which were to be presented to the Steering Committee at the 23rd annual meeting scheduled for Hartford, Connecticut on August 12-19, 1874 (22).

It is possible to identify two groups with different motives among those who united to support the AAAS. Through the end of the Civil War, American scientists were primarily generalists, with interests and publications spanning a wide range of topics and applications. Thus, in 1874, individuals such as Smith, Horsford, Silliman, Van der Weyde, and Cox (with an average age of 56) became part of the old guard who were reluctantly thrust into the beginning age of specialization, which was developing as a result of the industrial revolution. For those who were accustomed to following their own curiosity in "natural philosophy," the formation of a chemical society would be contrary to their belief that specialists would stifle the exchange of scientific thought by imposing boundaries for scientific investigation (23).

This attitude was expressed by the secretary of the Hartford AAAS meeting, who wrote (24):

The action taken by the ... formation of a Permanent Subsection of Chemistry was ... in accordance with the objects of the Association in bringing together scientists in all departments, that this expression on the part of ... special branches can only be regarded as most favorable towards the annual centralization

of scientific thought in the country during Association week, and it cannot be long before the American Association will draw within its folds ... many special organizations now existing, which ... working for one common end would thus still more greatly aid in the Advancement of Science in America.

This concern did not dissipate readily, for many years later, in describing the fiftieth anniversary of the AAAS, Daniel S. Martin complained that (25):

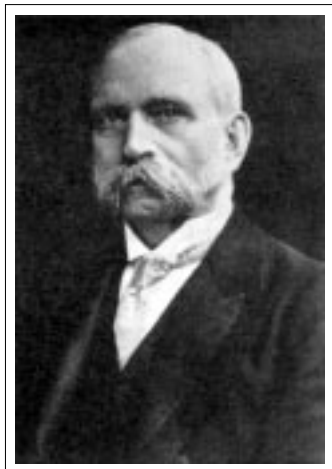
The increase of specialism has led not only to a division of the association into nine sections, in place of the two to three of its early years, but to the formation of several separate organizations of specialist which have been looked upon as tending to weaken, or even disintegrate, the main body. The American Chemical Society, the American Mathematical Society, and the Geological Society of America may be cited as leading examples.

For the younger chemists whose careers were just beginning, the concerns were much different. As the first group of specialists, their efforts had become more sophisticated and consequently more definite with regard to purpose. While their older colleagues often rambled through their scientific investigations with little cohesive planning, the new specialists were more careful to explore a topic in detail, not being averse to spending much of their lives on a single sub-specialty. This new attitude brought with it an increasing desire to exchange information with others who had similar special interests. It was within this framework that Clarke's group, whose average age was just 33, were preparing to finalize the new chemical subsection of the AAAS just two weeks after the Centennial of Chemistry meeting in Northumberland (26).

It is not surprising, therefore, that there was little enthusiasm for the possibility of forming a new society of chemists as proposed by Frazer. Further, since 20 of the 77 who came to Northumberland were already members of the AAAS (20), Bolton's amendment, seemingly the logical choice, was accepted without further comment. Subsequently, during the week of August 12, 1874, a Permanent Subsection of Chemistry of Section A of the AAAS was established, with Professor S. W. Johnson of Yale as Chairman and F. W. Clarke as Secretary.

Following almost immediately after the Centennial of Chemistry celebration, the Hartford meeting of the AAAS was the beginning of a long period of intense activity for chemists and the new subsection. The Bolton amendment was quite effective, for eight people who were in Northumberland a few weeks earlier became

Proponents



Charles F. Chandler



William H. Nichols

members of the AAAS at Hartford (27). The number of papers of chemical interest increased to 25, including six by J. Lawrence Smith, two by F. W. Clarke, and three by P. H. Van der Weyde (28).

In 1875 Clarke sent a letter to the editor of *Popular Science Monthly*, describing the formation of the subsection of “chemistry, chemical physics, chemical technology, mineralogy, and metallurgy,” and urged interested readers to attend the 24th meeting of the AAAS in Detroit, Michigan, during the week of August 11. Concerned that the fledgling subsection needed full attendance “in order to make it a success,” Clarke pointed out that (29):

Chemistry has been but little represented in the proceedings of the Association, and the time now seems to have arrived in which some good work can be done.

In Detroit, three more attendees from the Northumberland meeting became AAAS members. Clarke presented three papers and Smith gave five, while H. C. Bolton was elected to replace Clarke as the secretary of the chemistry subsection at the next annual meeting (30).

According to its constitution, the chief object of the AAAS was “by periodical and migratory meetings to promote intercourse between those who are cultivat-

ing science in different parts of the United States.” These migratory meetings “gave American chemists for the first time an opportunity of obtaining periodic scientific contacts of a national character in different sections of the country (23).” As the new chemical subsection of the AAAS continued to attract new members and prepare for the 25th annual meeting in Buffalo, New York, in August of 1876, the concept of a separate national chemical society lay officially dormant.

Some of the New York chemists who were at Northumberland began privately to discuss the formation of their own chemical society. Their leader was Charles F. Chandler of Columbia University, who as head of a self-appointed committee of seven issued an invitation in January of 1876 to the chemists of New York to see whether there was sufficient interest to form a local chemical society. “Widely scattered as the chemists in this neighborhood are,” Chandler said, “such an association would become a centre [sic] of pleasant personal intercourse, and of an interchange of views, experience and researches which would benefit all concerned (31).” Chandler and his colleagues were not following up on the Frazer suggestion at the Priestley house, for their thinking was strictly focused on what would be beneficial to New Yorkers. Chandler’s circular was mailed to about 100 chemists in the New York City area. Less than half this number responded favorably, but Chandler and

his associates boldly forged ahead. This number, they said, "was so unexpectedly satisfactory" that it would be "deemed opportune" to attempt the formation of a national rather than a local organization. On March 22, 1876, the Chandler group sent another circular announcing their intentions, but this time the mailing list was a different group of 220 chemists throughout the country. To encourage this group, who would be considered "non-resident" members, that is, outside of a 30-mile radius of New York City, Chandler promised that at least one meeting a year would be held outside of New York, "at such a time and at such a place as to make attendance on the part of non-resident members more convenient and representative." Within six days, lured by Chandler's rhetoric, some 60 non-resident chemists returned a favorable response. Armed with what he considered to be a significant response to his polls, Chandler issued the call to meet on April 6, 1876, for the purpose of forming the American Chemical Society (32).

As chairman of this organizational meeting, Chandler began the proceedings by stating that:

the ultimate object was to bring chemists together in scientific and social intercourse, to secure rooms which would be open in the day and evening, and to establish there a library of reference and a chemical museum (33).

It fell upon Isidor Walz, acting as the secretary of the meeting and the organizing committee, to describe in some detail the steps that had brought the group together on that April evening. At the conclusion of his presentation, Walz referred to the existing opposition to the formation of a national society. Noting that the organizational committee did not share that opinion, he nevertheless reported that the opposition had been promised "the fullest opportunity to lay their views before the group," and he urged that the audience give them "earnest and careful attention." Walz then offered his motion to organize; and after a second to the motion, Chairman Chandler "declared the subject open to discussion."

The first to respond to the Walz motion was Professor Thomas Egleston, who started on a semi-positive note by stating that it "might be advisable to organize such an association," but not in 1876. His first objection was based on experience with the American Institute of Mining Engineers (AIME), which had been formed five years earlier. Noting that the \$6,000 income from the 600 members of the AIME was used entirely to publish its *Proceedings*, Egleston concluded that the 100 potential members of the new chemical society (those responding favorably to the Chandler polls of

January and March) would bring an income of \$500 to \$1000, which would not be sufficient to "publish *Proceedings* in a creditable and prompt way." His fear, that "it is hard to get money now—when it will be easier to obtain it, the success of such a society would be better assured" was not unusual but it was rather feeble. As with Professor Smith at Northumberland, Egleston then launched a more pointed argument that was immediately reinforced by the second speaker, Henry Carrington Bolton. Both men supported no immediate action by the group, claiming that it would be unfortunate to have a division of forces. Instead, they favored cooperation with two existing societies: the Chemical Section of the New York Academy of Sciences and the Chemical Section of the AAAS.

It is ironic that both of these opponents were colleagues of Chandler at Columbia. Bolton (age 39), a Columbia graduate who obtained the doctorate in 1866 with Friedrich Wöhler at Göttingen (just ten years after Chandler), was an assistant in analytical chemistry at the School of Mines and a faculty member at the Women's Medical College of New York. His extensive bibliographic compilation of the literature of uranium chemistry that appeared in 1870 was a direct result of his many research papers published on uranium compounds between 1866 and 1870 (34).

Professor Thomas Egleston (age 44) was the founder of the School of Mines at Columbia. When he presented his proposal to the Columbia Trustees in March of 1863, it marked the first attempt at a new form of education in mining and metallurgy in the United States and was strongly influenced by Egleston's experience at the École des Mines in Paris several years earlier. In fact, it was Egleston who personally recruited Charles F. Chandler to become the chemist on the new faculty of three when the school opened in November of 1864 (35).

The third person to comment negatively on the Walz motion was Professor Albert R. Leeds of Steven Institute of Technology. Leeds (age 42) acted as secretary of the Centennial of Chemistry celebration in Northumberland, and it is his vivid account in the *American Chemist* that still remains the most authoritative description of that meeting. Leeds was not emphatic in his comments, preferring simply to state that he "did not think the movement timely (36)."

The appeal of Egleston, Bolton, and Leeds on behalf of the existing organizations resembled that made in Northumberland in 1874. There were several differ-

ences, however, that were important to the outcome of the vote on the Walz motion. This time the speakers were local individuals who were not of the same national caliber as those in Northumberland. There was also much more of a direct concern that there would be a keen rivalry between similar organizations for the attention of a small number of potential members. Of particular interest was the effect on the New York Academy of Sciences. Although it had originally been founded as the Lyceum of Natural History in 1817, the new name had just been adopted in February of 1876 when an extensive revision to revitalize the organization was completed. As part of these changes, a section devoted entirely to chemistry had been established and was holding monthly meetings. The architect of these changes, which required "revising and remodeling the entire constitution and by-laws," was the vice-president, Thomas Egleston (37). His plea, however, did not have much effect on his listeners, for of the 35 present on April 6, only six were members of that organization, including Chandler, William Habirshaw (a member of Chandler's organizing committee), Bolton, Egleston, and Leeds (38).

In a similar manner Bolton, who was scheduled to replace Clarke as secretary of the chemistry subsection of the AAAS at the Buffalo meeting in August 1876, would certainly have been remiss had he not reminded the group about that existing organization and its chemical activities. Contrary to the response in Northumberland, his point had little effect this time, for there were only three AAAS members present: Bolton, Leeds, and Chandler.

After the lengthy remarks by Egleston and Bolton, Chandler appealed for help from his colleagues, suggesting "that it would almost seem as though we had met for the purpose of deciding not to organize a chemical society." In utter exasperation, Secretary Walz answered that "the apparent reluctance to discuss the subject was probably due to the fact that the chemists present had the subject before them so long and had discussed it so thoroughly in private that a prolonged expression of views at this meeting was deemed unnecessary." Nevertheless, he proceeded to present a brilliant rebuttal, aided by Herman Endemann (D. phil., Marburg, 1866, later to be the first editor of the *Journal of the American Chemical Society*), and Chandler's assistant at the New York Department of Health, and Meinhard Alsberg, also at the Health Department.

It was, however, the statement of William H.

Nichols, who at age 24 had just begun his career in the chemical industry, that was the most noteworthy as well as prophetic:

We did not come here expecting to find a society ready formed, with a library and a fine building; those would come in time. We have much intelligence assembled here, and that is better than a library. Much benefit would accrue to all branches of the profession from such a society as that proposed. *Let us begin this society small, let it do its work well, and it will undoubtedly grow.* [emphasis added]

On that note of optimism, Chandler called for the motion, which passed with three nays, presumably by Egleston, Bolton and Leeds. Moving quickly to the organizational business at hand, Chandler began to deal immediately with some of the opposition. The election of John W. Draper as the first president was precisely calculated to minimize the influence of those who might be tempted to continue attacking the new society (39). At the same time, the first nominating committee returned 21 other names for offices mandated by the newly adopted constitution, including H. Carrington Bolton (corresponding secretary), Albert R. Leeds (committee on papers and publications), and the absent J. Lawrence Smith (vice-president). Only Bolton refused to serve, preferring instead to continue his AAAS activities.

It would be many years before the conflicts between the AAAS chemistry subsection, the New York Academy of Science, and other chemical societies that materialized after 1876 would be resolved (40). When the ACS finally emerged as the national professional organization of chemists, many of those who had initially supported other societies became active and influential ACS members (Fig. 1). Unfortunately, the person who first publicly raised the concept of a national society in Northumberland did not remain committed to his own proposal. Persifor Frazer followed Bolton's suggestion and joined the AAAS in August, 1874. Later, he joined the ACS in July, 1876, but shortly thereafter resigned in January, 1877, during the initial stages of the prolonged controversy when detractors claimed the ACS was not a true national society but was in reality a local New York organization. Apparently, Frazer did not renew his ACS membership at a later date.

There remains one interesting and unanswered question. In the period from August 1, 1874 to January 22, 1876, there is no record of what those in New York who favored the original Frazer motion might be considering. Only three of Chandler's "self-appointed committee" were at the Northumberland meeting. Yet, ac-

Figure 1. ACS Activities of Those Who Initially Opposed ACS Formation

<u>Name</u>	<u>ACS Membership</u>	<u>ACS Office</u>
A. Speakers at the Centennial of Chemistry, August 1, 1874		
1. J. Lawrence Smith	April 1876	President, 1877
2. Benjamin Silliman	April 1876	Vice-President, 1878
3. Frank W. Clarke	1877*	President, 1901
4. Eben N. Horsford	1877	None
5. Edward T. Cox	?	None
B. Petitioners for the AAAS Chemistry Subsection, August 12, 1874		
1. Samuel W. Johnson	July 1876	Vice-President, 1877 President, 1878
2. T. Sterry Hunt	April 1876	Vice-President, 1877, 1886, 1887, 1889 President, 1879
3. George F. Barker	April 1876	President, 1891
4. Harvey W. Wiley	November 1876	President, 1893, 1894
5. Charles E. Monroe	April 1876	Vice-President, 1889, 1890, 1891, 1895 President, 1898
6. William McMurtie	April 1876	Vice-President, 1897, 1898 President, 1900
C. Speakers at the ACS Organizational Meeting, April 6, 1876		
1. Thomas Egleston	April 1876	None
2. Henry Carrington Bolton	?	Vice-President, 900
3. Albert R. Leeds	January 1878	Vice-President, 1879 – 1888

* Clarke resigned after only two months of membership and did not rejoin the ACS until 1891. See J. J. Bohning, "Fighting City Hall: The Role of Washington Chemists in the Nationalization of the American Chemical Society," 220th National Meeting, American Chemical Society, Washington, DC, August 21, 2000; HIST 006.

ording to Isidor Walz, the subject was “before them so long” and “had been discussed thoroughly in private.” Evidently, none of the participants found it sufficiently important to make any notes of their informal gatherings (41). Yet, they continued in the face of very unfavorable conditions. On a local level, there was the New York Academy of Sciences, which was holding monthly meetings for chemists in New York City. On the national level, there was the AAAS chemistry subsection, which was growing and becoming increasingly active. During this period, however, Chandler and his committee continued to pursue the formation of a new organization when existing societies might have served their purpose. They set their sights on a local organization at first but quickly jumped to the national concept on the basis of a poll in which less than half of the people contacted were in favor of the idea of forming just a local group. Except for the bold determination and imaginative thinking of eight chemists from the New York City area, the American Chemical Society might not have come into existence for some time after 1876, if at all (42).

ACKNOWLEDGMENTS

The assistance of William Jarvis (Lehigh University) and Jane Hartye (Stevens Institute of Technology) is gratefully acknowledged. The author is indebted to Jeanne Kravitz (Wilkes College), whose knack for uncovering obscure citations is surpassed only by her uncanny ability to expedite accession to them. This work was supported in part by a grant from the Wilkes College Research Fund.

REFERENCES AND NOTES

1. Presented in part at the 184th National Meeting of the American Chemical Society, Kansas City, MO, September 13, 1982; HIST 008.
2. Original copies of this invitation are rare.
3. The March 27 invitation should not have come as a complete surprise, however, since the self-appointed organizing committee headed by Chandler had previously distributed inquires for interest in an organization of chemists on a local (January 22, 1876) and national (March 22, 1876) scale. See Ref. 4.
4. Identical detailed records of the first meeting of the American Chemical Society and the events that led to it may be found in a) *Am. Chem.*, **1876**, 6, 401-406; b) *Proc. Am. Chem. Soc.*, 1876, 1 (Part I), 1-20. This and all subsequent quotations pertaining to the first meeting are taken from the account in the *American Chemist*.
5. The most complete record of this meeting and the events that led to it were made by Professor Albert R. Leeds of Stevens Institute of Technology, who served officially as Recording Secretary. His faithful account was published in *Am. Chem.*, **1874**, 5, 35-44. The editors of the *American Chemist* later collected from their pages all of the material related to the meeting that was published in different issues (see following references) and produced a separate volume: *Proceedings of the Centennial of Chemistry*, Collins, Philadelphia, PA, 1875.
6. The meeting in Northumberland is often mistakenly identified as a meeting that celebrated the centennial of the discovery of oxygen, or “The Priestley Centennial (See, for example, H. Skolnik and K. Reese, *A Century of Chemistry: The Role of Chemists and the American Chemical Society*, American Chemical Society, Washington, DC, 1976, 4). However, Bolton’s now famous letter (*Am. Chem.*, **1874**, 4, 362) was quite clear in considering Priestley’s achievement as just one of many events that marked the year 1774 “as the starting point of modern chemistry.” The letter concluded by proposing “that some public recognition of this fact should be made this coming summer. Would it not be an agreeable event if American chemists should meet on the first day of August, 1874 [Priestley discovered oxygen on August 1, 1774], at some pleasant watering place, to discuss chemical questions, especially the wonderfully rapid progress of chemical science in the past 100 years?” In agreement with Bolton’s assessment of the centennial significance, the editors of the *American Chemist* used the “Centennial of Chemistry” banner for Bolton’s letter, as well as the responses his letter produced (*Am. Chem.*, **1874**, 4, 441-443), (only one correspondent disputed the Centennial of Chemistry label, but still supported the meeting concept), the general circular from the organizing committee (*Am. Chem.*, **1876**, 5, 11-13), and the Leeds report of the meeting’s minutes (Note 5). While the Northumberland gathering did pay homage to Priestley (*Am. Chem.*, **1874**, 5, 43-51), the organizing committee remained committed to Bolton’s original suggestion and scheduled several speakers to review the centennial aspects of chemistry. Thus, T. Sterry Hunt discussed “A Century’s Progress in Theoretical Chemistry” (*Am. Chem.*, **1874**, 5, 56-61); J. Lawrence Smith presented “The Century’s Progress in Industrial Chemistry” (*Am. Chem.*, **1874**, 5, 61-70); and Benjamin Silliman prepared a monumental listing of the “American Contributions to Chemistry” (*Am. Chem.*, **1874**, 5, 70-114, 195-209, 327-328). These three speakers were given positions of prominence, front row center, in the group photograph that was taken in front of a local building. Therefore, it is appropriate and desirable to use the “Centennial of Chemistry” descriptor rather than the

- more restrictive "Priestley Centennial" in reference to this meeting.
7. *Am. Chem.*, **1874**, 4, 441. Bodley proposed that "the centennial gathering be around this [Priestley's] grave, and that the meetings, other than the open-air one on the cemetery hilltop, be in the quaint little church built by Priestley." (The gathering did visit Priestley's grave, but the meetings were held in the local school rather than the church.) Bodley also noted in her letter that in her valedictory address delivered to the 22nd graduating class of the Women's Medical College on March 14 she had also called attention to 1874 as the centennial year of chemistry.
 8. Bolton made his proposal at the May 11 meeting of the Chemical Section of the Lyceum. Other members of the committee were Charles F. Chandler, Henry Wurtz, Albert R. Leeds, and Charles A. Seeley. It was this committee that was responsible for all the subsequent preparations for the Centennial of Chemistry celebration. See *Proc. Lyceum Nat. Hist. New York*, **1874**, 2nd. Ser., No. 4, 144-145.
 9. This and all subsequent quotations pertaining to the Centennial of Chemistry celebration are taken from Leed's account (Ref. 5).
 10. *New York Daily Graphic*, August 6 1874, p 252. See also M. G. Waring, "The Priestley Centennial, Turning Point in the Career of W. George Waring," *J. Chem. Educ.*, **1948**, 25, 647-652.
 11. Smith was referring to the American Association for the Advancement of Science (formed in 1840 as the Association of American Geologists and Naturalists) and the American Academy of Arts and Sciences (formed in 1780).
 12. The Chemical Society (London) was the oldest of the four organizations, beginning in 1841 with 77 members (coincidentally the same number present on the official list at the Centennial of Chemistry celebration). By 1874 it had almost 800 members and had been publishing a journal since 1847, first as a quarterly, and then, in 1861, as the monthly *Journal of the Chemical Society*. The Society received considerable support from Justus von Liebig; and by 1874 notable members who had held official positions included Hofmann, Faraday, Crookes, and Perkin, while Dumas and Cannizzaro had presented the memorial Faraday lectures. (See T. S. Moore and J. C. Philip, *The Chemical Society: 1841-1941*, London, 1947.) On the continent, the Chemical Society of Paris began in 1855 and started publishing the *Bulletin de la Société Chimique de Paris* in 1858. Shortly after Hofmann returned to Germany he founded the Deutsche Chemische Gesellschaft in 1866, and publication of *Berichte* began in 1868. The Russian Physical-Chemical Society was organized in 1869, and its journal commenced in the same year. (See A. J. Ihde, *The Development of Modern Chemistry*, Harper and Row, New York, 1964, 274-275 and references therein.) In spite of his comments, Smith was not adverse to using chemical journals for his own papers, having published 17 articles in the *American Chemist* between 1870 and 1874.
 13. For more information on the Chandlers, See R. D. Billinger, "The Chandler Influence in American Chemistry," *J. Chem. Educ.*, **1939**, 16, 253-257.
 14. For more information on Smith, see a) C. A. Elliott, *Biographical Dictionary of American Science*, Greenwood Press, Westport, CT, 1979, 238; b) D. H. Wilcox, Jr., in Wyndham D. Miles, Ed., *American Chemists and Chemical Engineers*, American Chemical Society, Washington, DC, 1976, 447-448; c) H. S. van Klooster, "Liebig and His American Pupils," *J. Chem. Educ.*, **1956**, 33, 493-497.
 15. For more information on Silliman, see Ref. 14c and a) C. C. Gillispie, *Dictionary of Scientific Biography*, Charles Scribner's, New York, 1970-1979, Vol. 13, 434-437; b) Ref. 14a, p 237; c) Ref. 14b, pp 438-440.
 16. For more information on Clarke, see a) Ref. 14b, p 82-83; b) Ref. 15a, pp 292-294; c) C. E. Monroe, *J. Am. Chem. Soc.*, **1935**, 57, 20-30.
 17. For more information on Horsford, see a) Ref. 14b, p 230-231; b) Ref. 15a, Vol. 6, pp 517-518.
 18. For more information on Edward Travers Cox, see a) James Grant Wilson and John Fiske, Ed., *Appleton's Cyclopaedia of American Biography*, D. Appleton & Co., New York, 1887, Vol. 1, 757; b) *The National Cyclopaedia of American Biography*, James T. White & Co., New York, 1904, Vol. 12, 328.
 19. For more information on Van der Weyde, see W. R. Woolrich, *The Men Who Created Cold*, Exposition Press, New York, 1967, 118-119.
 20. Smith, Silliman, and Horsford were charter members of the AAAS. The "official" list of the attendees at the Centennial of Chemistry celebration ("As far as I have been able to procure their names," according to Leeds) is given in Ref. 5. Detailed membership lists of the AAAS, including the dates at which the member was elected, were published as part of the annual *Proceedings*. See, for example, *Proc. Am. Assoc. Adv. Sci.*, **1874**, 23, xxvii ff.
 21. For a complete list of officers of the AAAS from 1841 to 1882, see *Proc. Am. Assoc. Adv. Sci.*, **1882**, 31, xix ff.
 22. *Proc. Am. Assoc. Adv. Sci.*, 1874, 22, 424. Clarke's brief recollection of these events can be found in C. A. Browne, *A Half-Century of Chemistry in America*, American Chemical Society, Philadelphia, PA, 1926, Chapter 3. More details are given by M. Benjamin in *Twenty-Fifth Anniversary of the American Chemical Society*, Chemical Publishing Co., Easton, PA, 1902, 86-98.
 23. For the early history of the AAAS and its relationship to American science, see S. G. Kohlstedt, *The Formation of the American Scientific Community: The AAAS 1848-1860*, University of Illinois Press, Urbana, IL, 1975; and S. G. Kohlstedt, *The Establishment of Science in America: 150 Years of the American Association for the Advancement of Science*, Rutgers University Press, New

- Brunswick, NJ, 1999. For the development of specialization in the nineteenth century, see R. S. Bates, *Scientific Societies in the United States*, MIT Press, Cambridge, MA, 1965, 3rd ed., Ch. 3.
24. *Proc. Am. Assoc. Adv. Sci.*, **1874**, 23, 153-154.
 25. D. S. Martin, *Popular Sci. Monthly*, **1897**, 51, 829.
 26. Clarke's colleagues included G. F. Barker, T. S. Hunt, S. W. Johnson, W. McMurtie, C. E. Monroe, and H. W. Wiley. Only Hunt was present at the Centennial of Chemistry celebration. Earlier, Hunt had responded to Bolton's idea of a centennial meeting by suggesting that it be held concurrently with the AAAS meeting in Hartford starting August 12 (See Ref. 5).
 27. Centennial of Chemistry attendees who joined the AAAS at Hartford were E. B. Coxe, E. T. Capen, A. H. Elliott, A. R. Leeds, T. R. PUNCHON, C. W. Roepper, E. H. Swallow, and E. Waller (See Ref. 20).
 28. See Ref. 20 for a complete listing and publication of some of these papers.
 29. F. W. Clarke, *Popular Sci. Monthly*, **1875**, 7, 365.
 30. See *Proc. Am. Assoc. Adv. Sci.*, **1874**, 23, xlvii, 99, 121. The three new members from the Northumberland meeting were S. H. Douglass, S. A. Goldschmitt, and Persifor Frazer.
 31. *Twenty-Fifth Anniversary of the American Chemical Society*, Chemical Publishing Co., Easton, PA, 1902, 39. At the time that Chandler issued this call, the migratory AAAS meetings, where all the national chemical activity was then taking place, had never been held in New York City since the AAAS had started 26 years earlier. The first meeting of the AAAS in New York City took place in 1887.
 32. See Ref. 4; the two ACS histories in Ref. 22; and C. A. Browne and M. E. Weeks, *A History of the American Chemical Society: Seventy-Five Eventful Years*, American Chemical Society, Washington, DC, 1952, for additional information on the preliminaries to ACS formation.
 33. Chandler's design for the new society was patterned after the European societies. See Ref. 12.
 34. For more information on Bolton, see Ref. 14 b, pp 40-41.
 35. For more information on Egleston, see D. Malone, *Dictionary of American Biography*, Charles Scribner's Sons, New York, 1930, Vol. 6, 50.
 36. In spite of these comments, Leeds became a faithful and active ACS member. For more information on Leeds, see *Proc. Am. Chem. Soc.*, **1902**, 24, 53-57.
 37. See the biographical notice by G. G. Kunze in *Trans. Am. Inst. Min. Eng.*, **1902**, 31, 14.
 38. For the membership list of the Academy, see H. L. Fairchild, *A History of the New York Academy of Sciences*, H. L. Fairchild, New York, 1887, 132ff.
 39. J. J. Bohning, "Prestige vs. Practicality in Selecting the First President of ACS," *Chem. & Eng. News*, **1982**, 60, March 8, 1982, 31-34.
 40. See the ACS history of Ref. 32 for more details.
 41. When C. A. Browne began preparing the 50th anniversary history of the ACS (Ref. 22), he asked Charles Chandler for details of the August 1874–April 1876 period (uncataloged letters in the Charles F. Chandler Papers, Butler Library, Columbia University). Chandler's only response was to submit a copy of the chapter he wrote for the 25th anniversary volume (Ref. 22).
 42. Drawing on the history of the first 20 years of the ACS, Sturchio has described the organization as a "gentleman's club" that "was the centerpiece of a network of metropolitan clubs and societies" which "served the social interests of the contingent of members with an interest in urban improvement and close ties to local commerce." See J. L. Sturchio, "Charles Chandler, the American Chemical Society, and Club Life in Gilded New York," presented at the Annual Meeting of the History of Science Society, New York City, December 27-30, 1979.

ABOUT THE AUTHOR

James J. Bohning is Professor of Chemistry Emeritus at Wilkes College and a past chair of HIST. He directed the oral history program at the Chemical Heritage Foundation for many years and was a science writer for the News Service of the American Chemical Society. He is currently a Visiting Research Scientist and CESAR Fellow in the Department of Chemistry, Lehigh University, 6 E. Packer Ave., Bethlehem, PA 18015, jjba@lehigh.edu.

NIEPCE DE SAINT-VICTOR AND THE DISCOVERY OF RADIOACTIVITY

Fathi Habashi, Laval University, Quebec, Canada

The discovery of radioactivity is usually credited to the French physicist Antoine Henri Becquerel (1852-1908) who in 1896 discovered the rays that were emitted from uranium salts (1). Becquerel placed fragments of several uranium phosphorescent substances on photographic plates wrapped in two sheets of black paper. In about 24 hours, when the plates were developed, a silhouette of the phosphorescent substance appeared on the plate. Hence, it was inferred that *..the phosphorescent salts of uranium must emit radiations which are capable of passing through black paper opaque to ordinary light, and of reducing the silver salts of the photographic plate, even when the uranium compound has been completely sheltered from the light.* The radiations emitted by the phosphorescent substance were called "Becquerel rays." At first, Becquerel thought that these radiations were a kind of invisible phosphorescence, which was afterwards shown to be wrong. For this discovery Becquerel was awarded the Nobel Prize for physics shared with Pierre and Marie Curie in 1903.

Mellor (2) in 1929, however, mentioned briefly that Niepce de St. Victor had already observed thirty years earlier that uranium salts could affect photographic plates in the dark. Mellor stated that Becquerel repeated some experiments of Niepce de St. Victor in order to find "if the property of emitting very penetrative rays is intimately connected with phosphorescence. In other words, does the principle of reversibility apply? If Röntgen's rays make a fluorescent substance shine in the dark, will a fluorescing substance emit invisible penetrative rays?"

In his report to the French Academy of Science published in 1858, i.e., 38 years before Becquerel's report, Niepce de St. Victor stated that (3):

A drawing traced on a piece of carton with a solution of uranium nitrate...whether or not exposed before to light, and applied on a piece of sensitive paper prepared using silver chloride will print its image...If the drawing made on the carton with the uranium nitrate solution...is traced with large strokes, it will be produced even at 2 or 3 cm further away from the sensitive paper.

With this statement one may conclude that Niepce de St. Victor had discovered radioactivity before Becquerel.

Studies on the effect of uranium salts on a photographic plate were started after the discovery of the metal by Klaproth in 1789. A few years later, since the new metal gives vivid color to glass, a small uranium industry was started by the Austrian Government to produce uranium salts in Joachimsthal in Bohemia. Thus A. F. Gehlen (1775-1815) in Germany in 1804 reported on the color change when an ethereal solution of uranium chloride was exposed to light (4). Other workers attempted to use this phenomenon in a copying process in which paper was soaked with uranium salts then dried. The picture to be copied was then made fully visible after exposure to light by soaking in a solution of silver nitrate then washing. The picture was formed as the result of the photochemical reduction of the uranium salt to uranium oxide which then produced metallic silver by reduction of silver nitrate. Niepce de St. Victor showed such pictures at the Third Exhibition of the Société Française de Photographie in Paris in 1859. He

was able later to give a colored tone to his copies. For example, a violet color was produced when the paper was treated with chlorine, a green color when iron salt was present, and a brown color when potassium ferrocyanide was present. The process was widely publicized as the "uranium paper" or "uranium copying process" and pictures produced by this process were exhibited at the First Photographic Exhibition in Vienna in 1864 (4).

Claude Félix Abel Niepce de St. Victor (1805-1870) was a cousin of Joseph Nicéphore Niepce (1765-1833), the amateur French scientist who, together with Louis Daguerre (1789-1851), developed the photographic picture. He was born in Saint Cyr near Chalon-sur Saône. He attended the school for cavalry at Saumur and became Lieutenant of Dragoons in 1842. In 1845 he was transferred to the Paris Municipal Guard quartered in the barracks of the suburb of Saint Martin, where he equipped a chemical laboratory. His first work, presented to the Academy of Sciences in Paris in 1847 dealt with the condensation of iodine vapors on a copper plate engraving and the reproduction of the iodine vapor image onto metal. In the same year, he made his invention of photography on glass.



Niepce de St-Victor (1805-1870)

In 1848, the barracks in which he lived were burned and his laboratory was destroyed. In the same year he became Captain of his regiment and was elected Chevalier of the Legion of Honor and received also a prize of two thousand francs from the Société d'Encouragement. He improved the asphaltum process of his cousin Nicéphore Niepce and greatly advanced etching on steel. When he was appointed Squadron Leader and Commander of the Louvre in Paris, he had time for his experiments and it was during this period that he investi-

gated photography with uranium salts. He was pensioned when Napoleon III came to the throne, and in his retirement he continued his research on scientific photography. He authored: *Traité pratique de gravure heliographique sur acier et sur verre*, Paris, 1856 and *Recherches photographiques*, Paris, 1858 (5).

In conclusion, the fact that Mellor had pointed out that Niepce de Saint-Victor, an amateur photographer, should be credited with the discovery of radioactivity and not Antoine Henrie Becquerel, prompted the present writer to pursue the matter further. By reviewing the history of uranium salts in photography during the nineteenth century, it could be concluded that Mellor's point of view is valid.

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ABOUT THE AUTHOR

Fathi Habashi is professor of extractive metallurgy at Université Laval, Faculté des Sciences et de Génie, Département de Mines et Métallurgie, Cité Universitaire, Québec, G1K 7P4, CANADA. He authored *Principles of Extractive Metallurgy* in 4 volumes and edited *Handbook of Extractive Metallurgy* in 4 volumes.

THE HARE-CLARKE CONTROVERSY OVER THE INVENTION OF THE IMPROVED GAS BLOWPIPE

Martin D. Saltzman, Providence College

Credit for the initial invention of the gas blowpipe has been marked by an unusually large amount of controversy (1), involving bitter charges of plagiarism and intellectual dishonesty. An example of this involves the American chemist Robert Hare, 1781-1858 (2), who claimed that Edward Clarke, Professor of Mineralogy at Cambridge University (3), had falsely taken credit for the invention of the improved gas blowpipe. Edward Daniel Clarke, LLD (1769-1822), had published, in 1819, *The Gas Blowpipe; or, Art of Fusion by Burning the Gaseous Constituents of Water* (4), in which he staked out his claim for the primary credit for the invention of this apparatus. Hare maintained that he had published the details of its construction fifteen years earlier than that of Clarke. This led Hare to ask the question in 1820, "Will Briton's tolerate such conduct in their professors?"

Hare maintained that he had developed a similar gas blowpipe (5) in 1802 and therefore deserved sole credit. Benjamin Silliman Jr., Professor of Chemistry at Yale University, in his 1874 history of chemistry in the United States as part of the celebrations marking the centennial of Priestley's discovery of oxygen, described the development of the Hare blowpipe as follows (6):

Probably no chemical discovery made in this country has been more generally cited or less generally understood in its scientific significance, than the oxygen-hydrogen blowpipe of Dr. Hare.

Who deserves priority for the invention of this most important piece of laboratory equipment? Did Clarke

knowingly appropriate the work of Hare without giving him the proper credit? An examination of the claims and counter claims will shed some light on this question.

The development of the mouth blowpipe has been attributed to Florentine glass blowers in the middle of the seventeenth century. In the works of Robert Boyle, according to Partington, one can find the following description of the blowpipe and its uses (7):

The small crooked pipe of either metal or glass, such as tradesmen for its use call a blowpipe gives a jet of air which when directed on the flame of a lamp or candle produces a pointed flame which melts silver and even copper.

The blowpipe was first introduced into analytical chemistry in 1750 by the Swedish chemist A. F. Cronstedt (1722-1765). Tobar Bergman (1735-1784) was the first to describe the blowpipe extensively as a tool for the analytical chemist. He also gave directions for its use with various reagents such as soda, borax, and phosphate for the study of earths, salts, combustible materials, metals, and ores. Bergman's student J. G. Gahn (1745-1818) made further improvements in the design and use, and Berzelius was the foremost advocate of its use in the early nineteenth century.

According to Hare's own account, he developed his improved blowpipe in Philadelphia in 1801. The impetus behind this invention was the need to produce higher temperatures than could be obtained by any contemporary apparatus. The principal means of producing high temperatures at this time were through the use of mag-

nifying glasses or furnaces (1). Lavoisier in 1782 had constructed an apparatus that allowed him to direct a stream of oxygen or hydrogen or a combination of both at a hollow piece of charcoal, to produce an intense source of heat. In a dramatic demonstration in April, 1782, Lavoisier, using a jet of oxygen directed at charcoal containing a piece of platinum, was able to melt it: a feat that had never been possible before. This experiment was repeated again on June 6, 1782 at Versailles to an audience that included Louis XVI, Marie Antoinette, The Grand Duke Paul of Russia, and Benjamin Franklin. The details of the apparatus and the experiments performed were reported to the *Academie* in a paper read in 1782 (8). Although Lavoisier mentioned the possibility of using the oxygen-hydrogen mixture, there is no description of this type of blowpipe in his *Traite de Chemie* of 1790 (9).

Gonzalez contends that the credit for the invention of the oxy-hydrogen blowpipe belongs to Lavoisier's contemporary, J. B. G. Bochard de Saron (1730-1794) because of Lavoisier's mention in his 1782 paper of the device having been made by Bochard de Saron. Bochard claimed that if two streams of gas—one being oxygen and the other hydrogen—were directed at charcoal, a very intense flame could be produced. However, Bochard de Saron never published his work, as he was to meet the same fate as Lavoisier. The apparatus designed by Lavoisier was too complex and expensive to be of any real practical value; and the details of the Bochard device, which may have been simpler and easier to use, were never published. Thus a practical instrument which could produce the intense heat needed for many laboratory situations was not available when Hare began his work in 1801.

What led Hare to believe that he could combine hydrogen and oxygen together under controlled conditions to produce a flame hotter than any known at the time? The inspiration for the improved blowpipe came

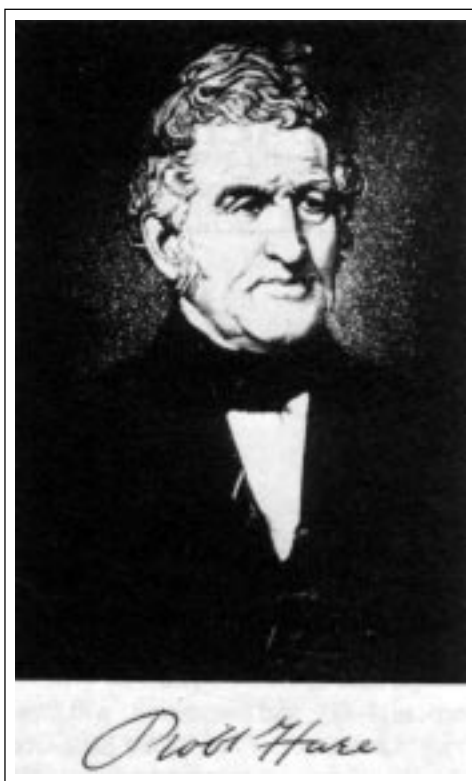
from the lectures Hare attended, beginning in 1798, given by James Woodhouse (1770-1809) at the University of Pennsylvania Medical School (10). Hare, the son of a Philadelphia brewer, had become interested in chemistry at a very young age. This interest was further peaked when he attended the lectures of Woodhouse, one of the small group of American chemists who had wholeheartedly embraced the new chemistry of Lavoisier. He was one of the leading critics, along with John Maclean, of Joseph Priestley, proponent of the "old chemistry." Although Priestley disagreed with Woodhouse, he nevertheless greatly admired his experimental abilities and considered him as the equal in skill and dexterity of any chemist he knew in England and France. Maclean may also have influenced Woodhouse to adopt the new chemistry as the result of his lectures on the new chemistry, published in 1797 as *Two Lectures on Combustion, which contained Considerations on the Doctrine of Phlogiston and the Decomposition of Water* (11). Woodhouse performed numerous studies on the synthesis and decomposition of water following the principles laid down by Lavoisier. The commonly accepted belief was that, when hydrogen combined with oxygen, a large amount of caloric (heat) was released. Hare probably heard of this explanation in one of Woodhouse's lectures, and this provided the inspiration for his blowpipe.

Hare believed that if he could somehow introduce a stream of pure oxygen and hydrogen, the ignition of

these two gases would produce temperatures not previously obtainable. Lavoisier had developed a gasometer that would allow the use of oxygen and hydrogen but was inconvenient, as Hare noted (5):

(It) is too unwieldy and expensive, for ordinary use...Being sensible of the advantage which would result, from the invention of a more perfect method of supplying the Blow-Pipe, with pure or atmospheric air, I was induced to search for means of accomplishing this object.

Hare's years in the brewery were put to good use, as he realized how the humble brewer's keg, being rugged, tight, and cheap, could form the basis of the new im-



Robert Hare

proved blowpipe. Hare divided a barrel approximately eighteen inches wide and thirty-two inches high into two compartments, the upper being fourteen inches and the lower sixteen inches. A sheet of copper containing a copper tube and had a set of water tight leather bellows comprised the bottom compartment. An iron rod was attached to the bellows via the copper pipe, which could be raised or lowered by a handle attached to the rod and thus act as a pump. The upper portions of the apparatus were separated into two chambers that could be filled with oxygen and hydrogen, which could be compressed by the built-in pump. Two pipes were used to carry the gases, and the streams met at a burning candle or lamp placed on a stand. Examples of the tips used by Hare are illustrated in Footnote 12 of Ref. 1. Hare had designed and constructed the prototype of what would become the oxygen-hydrogen blowtorch, still used today for welding and cutting metals (12). Miles writes of Hare's invention as follows (13):

...New methods of attaining high temperatures had been interesting chemists for a third of a century; oxygen and hydrogen had been known for a quarter of a century; and Hare lived in an age of giants.

Lavoisier, Priestley, Black, and others had the facts needed to construct an oxy-hydrogen blowtorch, but none of them experienced the flash of genius that came to Hare.

Hare estimated that his blowpipe could be made for about twenty dollars, and a simplified version that would be more efficient than the mouth blowpipe or the enameler's lamp for about four dollars. The significance of this invention and its usefulness prompted the reprinting *verbatim* of Hare's 1802 *Memoir of the Supply and Application of the Blowpipe* in Tilloch's *Philosophical Magazine* in London (14) and in the *Annales de Chemie* (15) in France. Thus, there can be little question that the claims made by Hare have a great deal of validity. It should be noted that Hare's description of the blowpipe was also printed in pamphlet form, and there is no way to judge how many copies were published and what the extent of the distribution was at the time. Just how many read the description of Hare's device in Tilloch's publication or in the *Annales de Chemie* is also impossible to gauge.



E. D. Clarke

Daniel Edward Clarke, the second son of a country vicar, entered Jesus College, Cambridge in 1786. He graduated in 1790 and was then employed as the tutor and companion to the sons of several wealthy families. As a student, Clarke developed an interest in mineralogy, which he fostered as the result of the travels in Europe in his role as companion and tutor. He collected many different types of specimens of minerals and plants and produced several volumes describing his travels.

The mouth blowpipe would have been known to Clarke as it was a primary tool used in mineralogical analysis. Clarke's travels took him also to Greece, Egypt, Turkey, and Palestine; and he was abroad when Hare's paper describing his blowpipe was published. Clarke donated many of the artifacts he had collected, which included several marble statues, to Cambridge University. Cambridge University awarded Clarke the degree of LLD in 1803, and Jesus College appointed him as a senior tutor in 1805. In the same year Clarke became an ordained priest in the Anglican Church, which was a usual prerequisite for appointment to a professorship at Cambridge and Oxford Universities.

Clarke was the vicar of two parishes until 1808, when a university professorship in mineralogy was created and he was appointed to the chair. It is most unlikely that Clarke may have come upon the Hare's original 1802 paper as well as subsequent papers of Hare and his collaborators. Abstracts as well as indexes were nonexistent in this era and it would have been only by chance that Clarke may have been aware of Hare's work.

It seems clear that the motivation for Clarke's interest in the blowpipe came as the result of his research in mineralogy rather than, in the case of Hare, as a result of his study of chemistry. In the preface to Clarke's *The Gas Blowpipe*, he writes the following (3):

The public is already in possession of the principal facts, which have led to the history of the Gas Blowpipe. The different claims made on the part of the Chemists of this Country and of America, as to the originality of the invention, have rendered it desirable to remove a few existing doubts, and to show,

by a summary memorial, the progressive steps by which the philosophical apparatus, here delineated and described, has reach its present state of utility.

Clarke stated that his particular blowpipe design was the result of a conversation he had with the instrument maker, Mr. Newman of Lisle Street, Leicester Square, London, in early 1816. Mr. Newman had constructed a blowpipe for a Mr. Brooke, who described the design in the May, 1816 *Annals of Philosophy* (16). Brooke stated that he had produced this new blowpipe because of the great inconvenience he had experienced with the common mouth blowpipe. Brooke's design consisted of using either a copper or iron vessel in which air was forced by a plunger and allowed to escape through a very narrow stopcock. This stream of air when ignited then produced an intense source of heat. Newman modified Brooke's original design so that gases like oxygen could be introduced. This, Brooke concluded would (16):

render it more extensive in its application to chemical purposes, and probably so as to supersede the use of the common gasometer.

In his design Clarke used the principle "of an explosive mixture of gases propelled through a common aperture found from a common reservoir (3)." This principle, Clarke stated, was well-known and the result of an investigation made of gas illumination by Professor Tennant and Dr. William Wollaston and presented in a public lecture delivered in Spring, 1814. Thomas Thomson in a letter to Clarke dated April 9, 1817 wrote that he, Thomson, had done experiments in 1800 that formed the basis of Hare's blowpipe. Thomson had abandoned his work because of the problem of explosions, which wrecked his apparatus; and the work was never published. In addition, Clarke stated that the principle of the oxy-hydrogen blowpipe had been demonstrated in chemical lectures at Cambridge for at least a dozen years prior to the publication of his book. As to who was demonstrating the blowpipe is not clear from Clarke's book.

The danger of an explosion from a retrograde movement of the flame was well known, and Wollaston had warned Clarke that his experiments could put him in great danger. Clarke "persisted in making them, narrowly escaped being killed by frequent bursting of his apparatus (3)." Clarke also consulted Sir Humphrey Davy in May, 1816 with his idea of mixing oxygen and hydrogen and passing the mixture through a capillary tube prior to ignition. Davy, who had developed the miner's safety lamp using the narrow capillary principle,

replied on July 8, 1816. Davy stated that he had tried the experiment and that "there would be no danger in burning the compressed gases by suffering them to pass through a fine thermometer tube, 1/80 of an inch diameter, and three inches in length (17)." The inherent explosive nature of the oxygen-hydrogen mixture required a container that could withstand an explosion. A colleague of Clarke at Cambridge, the Rev. J. Cumming, the Professor of Chemistry, developed a safety cylinder, which made the device much safer to use (3):

It becomes therefore a duty of gratitude to lay greater stress upon that part of the invention to which, beyond all doubt, he is indebted for his present safety. Had it not been for the circumstance, it would have fallen to the lot of some other person to have written the history of the Gas Blow-pipe, and to have rendered it rather tragical than amusing.

Just because Hare had reported experiments he performed in 1802 with a device of his own design, which happened to use hydrogen and oxygen, was not sufficient in Clarke's opinion to negate his own claim of originality.

The significant difference in the Clarke apparatus is that the oxygen and hydrogen are premixed. In Hare's blowpipe they were in separate containers and flowed through two different apertures before combining. As Clarke stated (3):

But the intensity of the heat is incomparably greater when the gases, after compression, are propelled and burned in a mixed state; because the due proportion necessary for forming water is then constantly and equally maintained: whereas an excess, whether on the side of the hydrogen or of the oxygen, not only tends to diminish the temperature, but, if it be much increased on the side of the oxygen, infallibly extinguishes the flame.

This greatly improved device with all the improvements that had been made by Clarke, he insisted, should have the name of "Gas Blowpipe." Robert Hare cried foul because in his 1802 paper he had not only described the design of the blowpipe but also reported the results of many of the experiments that had been performed with the blowpipe. Further modifications of the original design of the blowpipe as well as additional experiments were the subject of a paper read by Hare on June 17, 1803 at the Chemical Society of Philadelphia and published in 1804 (18). Hare pointed out in 1820 that he had reported experiments using his blowpipe in 1802 that Clarke claimed were new results (4):

Hence, until plagiarism had given them a new shape, and perhaps false gilding, they were totally over-

looked in his compilations. He neither treated of the pure earths as susceptible of fusion, nor of platinum as susceptible of volatilization, until many years after I had proved them to be so, and promulgated my observations.

One example from Hare's work that was also reported by Clarke as his own original work was the fusion of lime and magnesia (magnesium oxide) using anthracite coal as the reducing agent. By exposure to the gaseous flame of the coal, both magnesia and lime exhibited strong symptoms of fusion. The former assumed a glazed and somewhat globular appearance; the latter became converted into a brownish semi-vitreous mass.

Benjamin Silliman in 1812 (19) reported that with Hare's blowpipe he was also able to fuse lime and magnesia ignited in a covered platinum crucible (20). Clarke obtained similar results but failed to credit the work of Hare and Silliman. Hare noted (4):

Notwithstanding the previous publicity of these results obtained by my friend and myself, Dr. Clarke in the following note endeavors to convey an impression of the incompetency of my apparatus to fuse lime and magnesia. Note 5, page 46. Professor Hare in America could not accomplish the fusion either of lime or magnesia by means of his hydrostatic blowpipe. See *Annales de Chimie*, tome 45 page 126. But why overlook Silliman's experiments? It is moreover strange that an English writer should refer his readers to the French *Annales* in preference to a London magazine, for a memoir which he knew to be published in both.

What is one to make of these conflicting counter-claims concerning the blowpipe? Benjamin Silliman, Jr., writing in 1874 (6), made the point that a distinction needs to be made between discovery of the principle and the actual invention. Certainly, Hare deserves credit for the development of a device based upon the use of sound chemical principles. The production of copious amounts of heat by the combination of oxygen and hydrogen also formed the basis of Clarke's invention. However, Clarke's rationalization based upon the chemical nature of volcanism was certainly wrong.

Chemical theories of volcanism were very much in vogue in the first half of the nineteenth century. Clarke, as well as many of his contemporaries, believed that the volcanic fire, as he referred to it, led to the decomposition of water. According to him, these gases were compressed and their subsequent combustion produced the energy associated with the violent events that occur in a volcanic eruption. Thus the volcano is really a giant blowpipe, according to Clarke. Humphrey Davy sug-

gested that the origin of volcanic activity and subterranean heat was the action of water on sodium and potassium in the interior of the earth

Hare also realized that Lavoisier had overlooked an important point in his investigations, in that he considered the greatest amount of caloric would be released if the substance were placed upon charcoal and only a jet of oxygen gas introduced. Hare had reasoned that an even greater amount of heat would be produced if the substance were burned on a solid support in a stream of hydrogen and oxygen gas. As Hare wrote in 1802 (5):

It soon occurred that these desiderata might be attained by means of a flame, supported by the hydrogen and oxygen gasses; for it was conceived that, according to the admirable theory of the French chemists, more caloric ought to be extricated by this than any other condition.

In hindsight we can see an error in Hare's work concerning the concept of heat. It must be remembered that Rumford's paper on the origin of heat had not been published until 1800 in the *Transactions of the Royal Society* and was only slowly being accepted. Hare can be faulted on this point but not his basic chemical insight, which Clarke lacked.

Clarke deserves credit for the design of his blowpipe. The relative ease of the use of the Clarke blowpipe and the contributions stemming from its use in chemistry and mineralogy redeem somewhat his scandalous disregard and diminution of the work of Hare.

ACKNOWLEDGMENT

I am most grateful to Dr. W. A. Smeaton, Emeritus Reader in the History of Science, University of London, for all of his help in preparing this paper. Bill Smeaton passed away on January 22, 2001. He will be missed by all his friends in the chemical history community.

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ABOUT THE AUTHOR

Dr. Martin D. Saltzman is Professor of Natural Science, Providence College, Providence, RI 02918.

NATIONAL HISTORIC CHEMICAL LANDMARKS

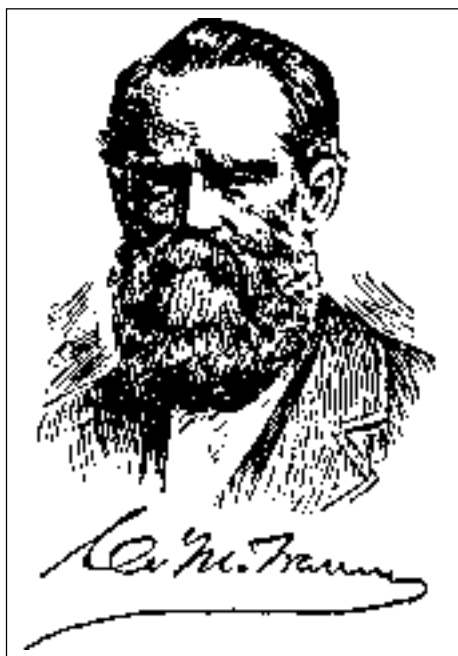
<http://www.acs.org/outreach/landmarks>

THE CONTRIBUTIONS OF CYRUS MOORS WARREN TO THE ANALYSIS OF HYDROCARBONS

Martin D. Saltzman, Providence College

Saturday, August 27, 1859 was a day that profoundly changed the world, for on that afternoon in the small town of Titusville, Pennsylvania, the first well drilled specifically to produce liquid oil came in. Petroleum offered a simpler and cheaper way to produce kerosene than the method of that time, which involved the destructive distillation of certain types of coal (1). Kerosene, introduced in the early 1850s, had revolutionized industrial societies by offering a source of light that allowed for a longer working day. Even the humblest homes could be lighted in a way that was previously unavailable.

Hydrocarbons that had been produced by the distillation of various substances such as tars, pitch, and coal remained a mystery because of the failure to separate and analyze these mixtures(2). Cyrus Moors Warren (1824-1891) made significant contributions to the development of techniques for fractional distillation and their application to the analysis of complex hydrocarbon mixtures. He did his work in the early 1860s in his own private laboratory in Boston and reported the results at meetings of the American Academy of Arts and Sciences (3). Much of the work was later published in the academy journals (7,13,15,17,18,19,20).



Warren's life combined careers as a basic research oriented chemist and as a successful industrialist.

Warren was born on January 15, 1824 in West Dedham, Massachusetts, the fifth son and eighth of eleven children born to Jesse and Betsey (Jackson) Warren. His father was a blacksmith by trade and an inventor by avocation, who for all his ability was never very successful financially. In 1829 the family moved to Peru, Vermont, in hopes of improving their situation. This was followed by another move to Springfield, Vermont, where his father started a foundry business. The foundry was destroyed by fire in 1839, leaving the family in dire financial straits.

Cyrus' formal education to this point was spotty at best, but both he and his older brother Samuel spent as much time as possible continuing their education by self-study. The brothers pledged to each other that if they became successful, first one and then the other would be able to complete his education. In 1846 Samuel Warren perfected a process to improve the manufacture of a waterproof roofing material made by coating paper with coal tar. He opened a plant to make the "tar paper" in Cincinnati in that year, and Cyrus joined him as a partner in the business in 1847.

Coal tar is the byproduct of the process used to produce coal gas for lighting. This process had been invented by William Murdoch (1754-1839) in the late 18th century, and plants to manufacture this gas were found in cities all over Europe and the United States after 1810. When the Warrens entered the tar paper business, coal tar was thought to have little commercial value; and the brothers were able to enter into long term contracts to purchase it at very low prices.

The tar paper business proved to be an almost immediate success. Samuel was the first to leave and pursue his education (4). As the business prospered, other Warren brothers were brought in; and finally, in 1852, at the age of 28 Cyrus, by this time married with a family, began his higher education. Because of his interest from childhood in the natural sciences, he chose to pursue his studies at the Lawrence Scientific School, affiliated with Harvard University. His initial interests were both in zoology and chemistry, and his studies were supervised by the eminent Swiss naturalist Louis Agassiz (1807-1873). Although attracted to natural history, Warren decided his interest really lay in chemistry. Warren received his S.B. degree in 1855 with high distinction and was elected to Phi Beta Kappa. He had the unique honor of being the first graduate of the Lawrence Scientific School ever elected to this scholarly society.

Wishing to complete his education and being of independent means, Warren embarked on the chemical grand tour of Europe with stops in London, Paris, Heidelberg, Munich, and Berlin. He carried out various research projects in the laboratories of such titans as Emil Kopp, Justus Liebig, Robert Bunsen, and Heinrich Rose. His work in Rose's laboratory on titanium and zirconium resulted in the publication of several papers in 1857 (5). This European sojourn was to last for several years before Warren finally returned to America in 1858. There were few American chemists who had achieved as extensive training in various aspects of chemistry as Cyrus Warren had by 1858.

In 1855 the Warrens had moved their industrial operations to New York, establishing the Warren Chemical and Manufacturing Company to produce coal tar products. Contracts had been secured from the numerous gas works in the city to provide the raw material at very low cost. When William Henry Perkin's synthetic aniline dye mauve, introduced in early 1858, became an instant success, the Warrens profited handsomely by being a major source of benzene, aniline, and other coal tar intermediates used by the aniline dye industry (6).

Cyrus Warren remained in New York until 1861, when he moved to Boston with the intention of producing the highly profitable aniline dyes himself. As the center of the American textile industry, New England was a logical location for a dye plant. With the raw materials available at very low cost from the Warren coal tar distilleries in New York, this venture seemed a logical extension for the Warren Company. However, this was not to be. When it became obvious to the English manufacturers such as Reed & Holliday that an American firm was going to compete in the lucrative American market, they proceeded to unload their aniline dyes on the American market at prices with which the Warren Company could never compete. With failure of the dye venture, Warren turned his attention from business to basic research at the laboratory he had built as an addition to his Boston home. It had always been Warren's intention to devote himself to chemical research as a part of this new venture in Boston. Warren was able to outfit his laboratory with the latest and best equipment available and set to work. One of the first problems to which Warren turned his attention was the development of better methods of fractional distillation, with an eye towards its application to the analysis of hydrocarbon mixtures. As Warren wrote (7):

...simple fractional distillation...affords but very imperfect and unsatisfactory results, and not infrequently leads to gross errors and misconceptions... The want of a more efficient process for effecting such separations has long been recognized. There are numerous natural and artificial products, of the highest scientific interest, such as petroleum...of which it may at least be said that we have but very imperfect knowledge

The innovation that Warren introduced was the control of the temperature in the still head. This was achieved by a worm coil whose temperature was controlled independently and through which the distillate had to pass. Figure 1 is the drawing of the apparatus shown in Warren's paper (7):

In the belief that no process of fractioning at all analogous to mine has ever been employed in scientific research, and that I am not in any way directly indebted to any devices of my predecessors,...I may say, however, that I have found no record of any one's ever having employed the oil bath and a separate fire to regulate a heated condenser, this being the essential feature on which the superiority of my process is based; adapting it at once to both high and low temperatures, and for the most delicate work.

The worm coils used by Warren were made of copper in various sizes from 10 feet by 1/2 inch to as small as 1

foot 6 inches by 1/4 inch in length. A complete description of the procedures used to conduct fractional distillation is given in the paper. Warren concluded his paper with the following remarks (7):

...I can say that as regards bodies not decomposed by heat in distillation, I have not yet found a mixture so complex that it cannot be resolved by this process into its proximate constituents so completely, that these shall have almost absolutely constant boiling points.

The fractional distillation technique was so successful that Warren was able to separate benzene from coal tar naphtha in such a state of purity after several distillations that it froze upon cooling in the receiver into a solid mass of crystals. Warren's fractional distillation technique would be used in the research laboratory as well as in the chemical industry particularly in Europe (8):

...an expert traveling in Europe in 1870 found the process in common use there in the distilleries of tar. In some instances, the managers of these works knew that they were using Warren's invention, while others professed ignorance as to its origin, while freely admitting its excellence.

Warren had received a patent for his technique (9), but this did not seem to deter those who infringed upon it without paying any royalties.

Having developed this efficient method of fractionation, Warren began to apply it to various types of hydrocarbon mixtures. Among the first studied were the volatile hydrocarbons found in light coal tar naphtha. The first attempt to analyze these low-boiling fractions (bp 80-175°C) had been done in 1849 by Charles Mansfield, a student of Hofmann at the Royal College of Chemistry in London (10). Mansfield reported (11) that there were four major components that showed a constant incremental difference of 30° in boiling point. Mansfield believed these four components to be aromatic hydrocarbons, with the first of the series being benzene. Faraday had isolated benzene in 1825 from oil gas, and Hofmann had shown it to be present in coal tar naphtha in 1845. Mansfield proposed the other three components, in order of increasing boiling point, to be toluene (113°), cumene (140-145°), and cymene (170-175° C). The only compound that could be definitively identified in the mixture was benzene, the lowest-boiling component. The others had not been obtained in a sufficient state of purity for any kind of definitive analysis. In 1855 Church, reporting his analysis of this mixture, proposed (12) that there are five components, each having a constant difference in boiling point of 22°.

Warren hoped to produce a definitive analysis of the light coal tar naphtha by using his fractional distillation technique. In addition, he had developed what he thought to be a better and more accurate method for determining boiling points. To ensure the accuracy of his study he used a mixture of coal tars produced by six different gas works, derived from different varieties of coal, both imported and domestic. Since the gas works produced coal tar residues in quantities of upwards of 50,000 barrels per annum, Warren firmly believed that the naphtha he was to fractionate contained all the possible components (13):

...fractioning in this case was conducted in all respects as there described, and continued until the whole of the naphtha taken, boiling between 80° and 170° C had accumulated at the four points...80°, 110°, 140°, and 170°, or so nearly the whole that the intermediate quantities had become too small to admit of being further operated upon...I may here remark that each of the sample gallons employed, when subjected to my process of fractioning, was found to contain, in variable proportion, all of the constituents of the naphtha.

The compounds isolated by Warren were identified in order of increasing boiling points as benzene (80°), toluene (110°), xylene (140°) and cumene (170° C). This paper seemed to verify the preliminary work that had been done by Mansfield and cast doubt upon the analysis published by Church.

An early empirical discovery was that the hydrocarbons in a homologous series had boiling points that differed by a constant increment. Hermann Kopp (1817-1893) reported in papers published in 1842 and 1845 (14) that there was a definite 19°C increment for each member of the homologous hydrocarbon series he studied. In 1855 (15), Kopp, revisiting the question of the correlation of boiling points in homologous series of hydrocarbons, noted that there seemed to be certain exceptions for a difference of C₂H₂ in some cases. Other contemporaries of Kopp had suggested various rationales for calculating the differences in boiling points by using certain structural assumptions. Warren was concerned with the lack of reliability of boiling points reported in the literature and hence the confusion concerning the effect on physical properties in an homologous series (15):

It may be hoped, however, that the superior means which my process furnishes for separating mixtures of liquids, will lead to the accumulation of reliable facts of sufficient number and variety for a profitable review of this question in its different bearings, which, from its importance, it clearly merits.

Warren obtained his boiling points by immersing the bulb of the thermometer in the liquid rather than in the vapor, as was the standard procedure. He argued that the temperature of the boiling liquid and the vapor should be the same as long as there is a regular pattern of boiling. To ensure uniform boiling Warren used as boiling stones pieces of sodium when possible or else coke (15):

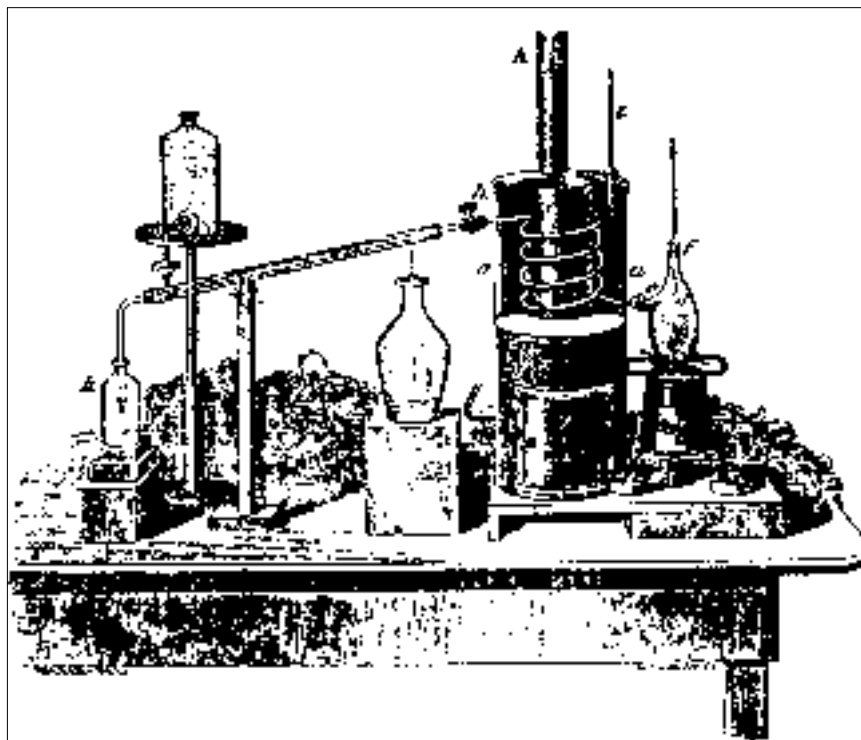
My experience has shown that, when irregular ebullition is effectually prevented, the temperature of the vapor from a boiling liquid is more liable to lead to an erroneous determination of the boiling-point, than the temperature of the liquid itself.

Warren used Pennsylvania petroleum as the source of homologous series of hydrocarbons in one of his studies. Prior to the beginning of Warren's study in 1861 there had not been any complete analysis of the volatile hydrocarbons from petroleum. The more volatile components had little commercial value at this time and would only become important in the 20th century with the development and increasing use of the internal combustion engine. Warren thought it would be of great interest to fractionate the most volatile hydrocarbons from crude petroleum as well as from the synthetic coal oil, which had been used to make kerosene prior to the availability of liquid petroleum. The question was whether the mixture of hydrocarbons found in Pennsylvania crude and that produced by distillation of coal were the same or different in view of the contrasting sources. In all of his studies Warren pointed out that each of the fractions isolated boiled within, at most, a 1.5° range and left no residue. Two identical series of hydrocarbons were obtained from both sources, which differed by 30° for each increase of CH_2 , thus showing that the petroleum (also known as rock oil) and coal oil were identical mixtures of hydrocarbons. These hydrocarbons were identified as pentane, bp (30.2°), hexane (61.2°), heptane (90.4°), octane (119.5°) and nonane (150.8°C). In a truly Baconian manner, Warren wrote of his accomplishment as follows (15):

As no one had preceded me in the investigation of these substances, my mind was as far as possible un-

biased as to the boiling points of the constituents of these mixtures. I was, however, aware of the beautiful relation between elementary constitution and boiling point, which Kopp had discovered.

Thus it would appear that no matter the source of the hydrocarbon mixture the homologous series of hydrocarbons were identical and had an incremental boiling



Warren's distillation apparatus

point of 30°. Seeking to extend his investigations further, Warren studied the boiling points of the nitro compounds of the aromatic series. These showed a difference of only 14°, much less than the 30° for the hydrocarbons. Warren had no proper explanation for these results.

The fractional distillation technique was also used to determine the composition of petroleum produced in Burma, known in the trade as Rangoon petroleum. Crude oil that seeped to the surface in Burma, just as in Pennsylvania, was collected and distilled at a refinery in Rangoon to produce kerosene for the Asian market. The British chemists Warren De La Rue and Hugo Muller had attempted to determine the composition of the crude Burmese petroleum but without success. They had reported (16) their failure in being able to separate the naphtha fraction into its components in 1857. Warren wrote (17):

The labors of De La Rue and Muller at once occurred to us as furnishing an extreme instance, and it was determined to test the new process with materials which, as these chemists had shown, could not be unraveled by the old processes of analysis.

Volatile components from the Rangoon petroleum were collected in a range from 150° to 270°. The light naphtha fraction was not included in the sample Warren had obtained from a London merchant, and therefore the volatile components started to boil at 150°. A sample of the light naphtha fraction could not be obtained, and thus only the fraction above 150° was subjected to fractional distillation. This produced seven distinct fractions boiling between 170° and 240°C. Each of the fractions was treated with sodium until no further reaction occurred and then analyzed for carbon-hydrogen content. The entire volatile fraction had approximately 86% carbon and 14% hydrogen, corresponding to an empirical formula of CH_2 . As to the composition of this mixture Warren speculated that it was a series of homologous aliphatic hydrocarbons, C_{10} - C_{14} . The presence of naphthalene in the mixture was also inferred.

Warren was not satisfied with the methods available for the analysis of carbon and hydrogen content by combustion in air. He modified the usual method of combustion, substituting pure oxygen for air for the analysis (18):

By a very simple device I entirely obviate the danger of explosion; viz. the combustion tube is closely packed with asbestos, or other inert substance, and yet so loosely as to leave free passage for gases through the interstices.

Warren provided a thorough and detailed description of the apparatus and procedures that led him to conclude that "the results obtained are extremely accurate and uniform." This method, Warren believed, was superior to the conventional methods in use (18):

...its greater convenience, economy of time, avoidance of excessive heat, neatness, etc.; it will, at least, not be found inferior to other methods; but, on the contrary, I think preferable, as affording greater security against failures and errors.

In the course of his work on petroleum, Warren isolated several sulfur and chlorine containing compounds. The present methods for the analysis of chlorine and sulfur in organic compounds were not satisfactory in Warren's opinion. Sulfur analysis involved a problem that some of the element present in the compound was converted to sulfuric acid rather than sulfate. Warren turned this problem to his advantage by employing the reaction of

sulfur compounds with lead peroxide (PbO_2) to ensure that sulfuric acid would be the only product formed. With some modification Warren was able to use the same apparatus he had developed for carbon-hydrogen content to analyze organic compounds simultaneously for carbon, hydrogen, and sulfur. The method gave excellent results for the known compounds such as carbon disulfide (19).

The analysis of chlorine in organic compounds presented the problem of the conversion of the whole of the chlorine content, but without allowing absorption of the carbonic acid and water produced from the carbon and hydrogen in the sample. Oxides of the heavy metals were known to have a strong affinity for chlorine, but they did not interfere with the carbon analysis. After trial and error, Warren found that the "brown oxide of copper" ($\text{CuO}_2 \cdot 2\text{H}_2\text{O}$) was the best reagent to use for the analysis of chlorine. He also obtained excellent results by using known compounds (20).

In 1866 Warren accepted an appointment as Professor of Organic Chemistry at the Massachusetts Institute of Technology, but he resigned the position after two years because it involved excessive demands upon his time. His ability to continue his research and his continued involvement in the chemical industry as a consultant were seriously hampered by his duties at MIT.

The chaotic business cycles that followed the Civil War finally claimed all of his time, and so the grand program for the separation and analysis of petroleum was never realized. Though independently wealthy, he felt a continuing obligation to his partners, especially after the death of his brother in 1880. The strain of overwork broke his health and eventually led to a debilitating stroke in 1888. He died on August 13, 1891 at his home in Manchester, Vermont.

In his will he generously provided funds for the American Academy of Arts and Sciences as well as Harvard University for the funding of basic chemical research. In addition, his will provided funds toward the construction in Boston of a permanent library and meeting hall for the American Academy of Arts and Sciences. Although Cyrus Warren's career in basic research spanned only a few years, he made significant inroads into an understanding of complex hydrocarbon mixtures, but his contributions do not appear to be widely known by the American chemical community.

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RECIPROCAL SOLUBILITY INFLUENCE IN SALT MIXTURES. THE CONTRIBUTIONS OF WALTHER NERNST AND OF ARTHUR NOYES

John T. Stock, University of Connecticut

In 1887, Walther Hermann Nernst (1864-1941; Nobel Laureate 1920) began his highly successful career as an assistant to Wilhelm Ostwald (1853-1932; Nobel Laureate 1909) at the University of Leipzig. Ostwald strongly supported the ionic theory and had extended it, especially to the dissociation of weak electrolytes. By 1889, Nernst had established the principles of electrode potential, familiarized in the textbook "Nernst equation," and hence of the emf of a reversible cell. That the solubility of a salt is diminished by the addition of another salt having an ion in common with the first was well known. While briefly at the University of Heidelberg, Nernst developed a quantitative theory of this common ion effect, supported by experiments with uni-univalent strong electrolytes (1).

Nernst's simplest case assumes that the salts are completely dissociated in solution. If to the saturated solution of MX (molar concentration m_0) MY or NX is added to concentration x , then the now smaller solubility m is given by

$$m(m+x) = m_0^2 \quad (1)$$

The symbols are those used by Nernst. The quantity m_0^2 became known as the *solubility product* of MX. (Nowadays, K_{sp} is the usual symbol for a solubility product). Unless the solubility of MX is very small, equation (1) must be modified to allow for incomplete dissociation:

$$ma(ma + xa\phi) = m_0^2 a_0^2 \quad (2)$$

Here a_0 is the degree of dissociation of MX when saturated in water and a the value after the addition of, e.g., NX, which is dissociated to the extent $a\phi$. Svante Arrhenius (1859-1927) had pointed out that the mixing of two solutions with one ion in common does not alter the degree of dissociation of the salts (2). For example, a mixture of equivalent solutions of a pair of alkaline halides has a conductivity equal to that of the mean of the conductivities of the individuals (3). Therefore when solving equation (2) with respect to m , Nernst felt justified in making the assumption that a and $a\phi$ were equal, so that m is given by:

$$m = -\frac{x}{2} + \sqrt{\frac{m_0^2 a_0^2}{a^2} + \frac{x^2}{4}} \quad (3)$$

Then, with CH_3COOAg (solubility 0.0603M at 16°C) as MX and known concentrations of AgNO_3 or CH_3COONa as additive, Nernst found that the measured and the calculated solubilities were similar. The largest concentration of additive was 0.230 M, when the solubility of CH_3COOAg fell to approximately one-third of its solubility in water.

Nernst commented that solubility measurements might throw light on the existence in solution of both M^+ and MX^- . He also theorized that, when two common-ion salts form a single saturated solution, their solubilities, m_1 and m_2 , must be less than their m_0 values.

Nernst provided no experimental support for these ideas. Before his move to the University of Göttingen in 1890, Nernst was able to place the verification and extension of mutual solubility problems in the hands of Arthur Amos Noyes (1866-1936).

Noyes graduated from the Massachusetts Institute of Technology (MIT) in 1886 and continued research in organic chemistry to obtain his M.S. and an assistantship in 1887 (4). By the summer of 1888, Noyes had planned to study under Johann Friedrich Wilhelm Adolf von Baeyer (1835- 1917) in Munich, but there was no laboratory space for him. Instead, he went to Leipzig, aiming to study organic chemistry under Johannes Wislicenus (1835-1902). However, having heard Ostwald's lectures on physical chemistry, Noyes decided to work in his laboratory. Eventually, Noyes became the first American to obtain a Ph.D. under Ostwald's guidance.

Noyes began his studies with a survey of the principles of mutual salt solubilities (5). He pointed out that a consideration of the undissociated portion of MX leads to a very simple alternate expression for m :



Walther Nernst

$$m = m_0 (1 - a_0) / (1 - a) \quad (4)$$

However, this simplicity is offset by a greater sensitivity to any error in a . A positive error obviously yields a value of m that is too large; Noyes commented that if the same value of a is inserted in equation (3), m is found to be too small.

To extend Nernst's studies, Noyes chose the systems listed in Table 1. The substrates were chosen to have qualitatively similar low solubilities. Experiments with TlBr were made at 68.5° C because the solubility of this salt was unacceptably low at ambient temperatures. The solutions used were thermostatted, usually at 25° C. Classical gravimetric and volumetric methods were used for the analyses. Equation (3) or its modifications was used to calculate the expected solubilities.

Noyes tabulated the results obtained with the various pairs of electrolytes. In all cases, the observed solubility was greater than the calculated value. Noyes noted that the difference between the two values became greater as the concentration of additive was increased. This is, of course, an expected result of a greater total ionic strength. Noyes felt that the differences might be caused by the use of conductivity measurement to find degrees of dissociation. Certainly the "zero concentration" equivalent conductivity values were at that time the best estimates. The Kohlrausch square root rule for finding such values by linear extrapolation from measurements made at finite concentrations did not appear until 1900 (6).

Noyes chose the pair TlCl and TlSCN for a quantitative examination of Nernst's conclusions concerning the solubilities of two common-ion salts in a single saturated solution. He found decreases in solubility of approximately 26% for the more soluble TlCl and 28% for TlSCN.

Nernst had indicated that solubility measurements might throw light upon the state of dissociation of ternary salts. For example, is AgSO_4^- present in a saturated solution of Ag_2SO_4 ? To find an indirect answer to this kind of problem, Noyes added equivalent amounts of TlNO_3 , BaCl_2 , and Tl_2SO_4 to saturated solutions of TlCl. He found that the solubility of the latter salt was lowered by the same extent with each of the additives. Because Tl^+ and Cl^- , but not TlSO_4^- and BaCl^+ , control the solubility of TlCl, he concluded that there were no significant amounts of the double ions. Otherwise, less Cl^- or Tl^+ would be available from the additives.

Table 1. Common-ion Systems

Substrate	Solubility, m_0	Additive
PhCH:CBBr.COOH ^a	0.0176	PhNH.CO.COOH
AgBrO ₃ ^b	0.00810	KBrO ₃ or AgNO ₃
TIBr ^c	0.00869	TiNO ₃
TISCN	0.0149	TiNO ₃ or KSCN
TiCl	0.0161	TiNO ₃ or HCl

^a Common ion, H⁺^b24.5° C ^c68.5° C

Noyes turned to systems that have no ions in common. He examined the effect of KNO₃ and of CH₃COONa on the solubility of TiCl and found that these increased the solubility, as we would expect from the present-day concept of ionic strength. However, Noyes attributed the increase to the formation of some undissociated KCl by an exchange reaction with TiCl, thereby increasing the solubility of the latter. At additive concentrations greater than about 0.03M, the solubility was less than expected from his mode of calculation. He could not explain this but suggested that it might be due to inaccurate dissociation values, or to the assumption that all of the salts had equal dissociation constants.

Having examined the common-ion effects in solutions of sparingly soluble salts, Noyes considered such effects in solutions containing only freely soluble salts. He critically surveyed the solubility results obtained by numerous earlier workers, pointing out certain peculiarities such as the formation of double salts. He concluded that the mutual solubility principles were also obeyed in the necessarily more concentrated solutions, although the results might lack quantitative exactitude. Noyes considered that the application of solubility measurements to the determination of degrees of dissociation was one of the most important results of his work. With the elimination of m_0 by combining equations (3) and (4) he obtained the relationship:

$$a = [(m_0 - m) / x] [1 + (1 + x / m)^{1/2}] \quad (5)$$

At this point he stressed that a is the dissociation of each salt in the presence of the other and is equal to the dissociation undergone by each salt at concentration $(m + x)$. For a single salt, the application of the law of mass action leads to the relationship $(1 - a).n = k.a^2.n^2$,

where n is the normality of the solution and k is the factor that Noyes termed the "dissociation constant" of the salt. This relationship, a form of the Ostwald "dilution law," is applicable to solutions of weak electrolytes. When, as was usual, the degree of dissociation a was obtained from conductivity measurements, attempts to apply the above relationship to solutions of strong electrolytes resulted in failure. Noyes illustrated this by the results obtained with solutions of TiNO₃. These results, along with those found when a was calculated from solubility measurements, are listed in Table 2.

Although not completely independent of concentration, the results in column 2 show a degree of constancy that is completely absent from the results obtained from conductivity data. Noyes concluded that the determination of dissociation from solubility measurements was the most reliable method then known. Solutions of pairs of salts that differ completely, i.e., have no ion in common, such as the pair TiCl and KNO₃, were examined. The results of several experiments showed that the solubility increase agreed approximately with that calculated from known dissociation constants. In validating and extending Nernst's concepts, Noyes produced much valuable solubility data. He showed decisively that the common ion effect was a highly significant phenomenon in quantitative chemistry.

After receipt of his Ph.D. in 1890, Noyes returned to MIT, to become a great teacher of chemistry in its widest sense (4). This activity was accompanied by extensive research, especially on solutions of electrolytes. Noyes' doctoral studies must have led him to suspect that rather more than the law of mass action and the Arrhenius ionic theory were needed to explain some of the phenomena encountered. By 1903 he had begun to consider the possibility that anomalies in electrolytic conductivity might be attributable to electrical charges on the ions and not to specific chemical affinity (7,8).

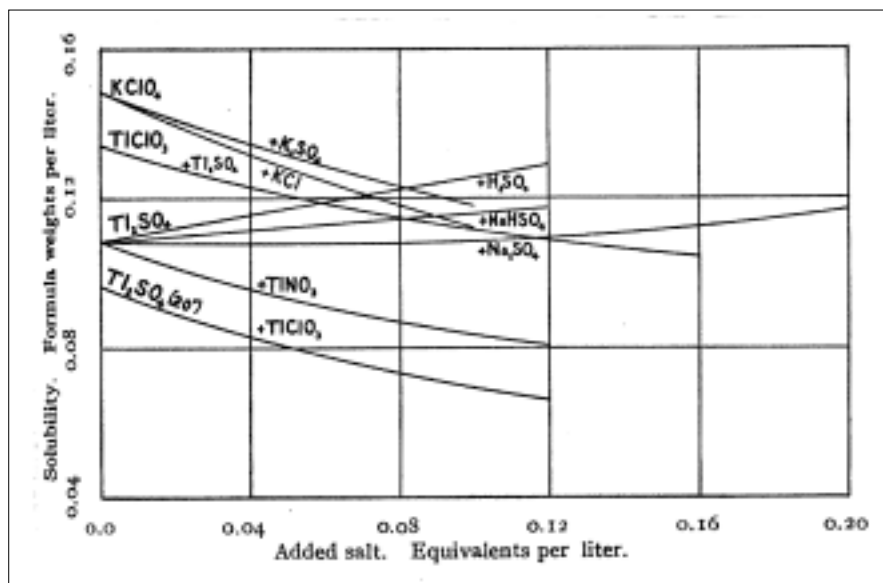
Table 2. "Dissociation Constant" (k) of TiNO₃

Concn.	k , solubility	K , conductivity
0.0161	5.45	7.11
0.0366	5.45	4.84
0.0617	5.21	4.03
0.100	5.07	3.32
0.150	4.81	2.90

He retained his interest in the solubilities of electrolytes and the properties of their solutions. This included the effect of salts on the solubility of other salts, an effect that Noyes had investigated in his doctoral studies.

In 1911, Noyes collaborated with Research Associate (later, Assistant Professor) William Crowell Bray (1879-1946) to produce a set of papers that critically examined and greatly extended his earlier work (5). The first of these papers showed that the solubility principles initially adopted by Noyes are subject to considerable deviations (9). Additional studies since 1890 had shown that, in a solution saturated at 40°C with both TlCl and TlSCN, the concentration of nonionized TlCl is about 15% less, and the ionic product $[Tl^+][Cl^-]$ about 5% greater than in a solution of TlCl alone. Further, the solubility-product principle failed badly when a salt with a common bivalent ion was added. For example, although the solubility of $PbCl_2$ was decreased slightly by a small addition of $Pb(NO_3)_2$, further additions caused the solubility to become greater than in water. The authors proposed to make use of the thermodynamically related concept of *activity*, A , introduced by Gilbert Newton Lewis (1875-1946) in 1907 (10). In a solution in equilibrium with solid salt BA, the relationships $A_B \times A_A = \text{constant}$ and $A_{BA} = \text{constant}$ are strictly true. The *activity coefficient*, *i.e.*, the ratio of activity to concentration, A/C , is assumed to be unity at infinite dilution.

The experimental work described in the second paper was shared by Noyes's three coauthors (11). Great care was taken to ensure the purity of the various salts and the temperature was maintained at $25 \pm$



Effect of salts on the solubility of other salts

0.02°C (or 20°C, where indicated). The results of the numerous solubility determinations are summarized as in Fig. 3. The lowered solubility of Tl_2SO_4 by the presence of $TlNO_3$, a salt with a univalent common ion, agreed qualitatively with the ionic product principle.

However, with the bivalent common ion salt Na_2SO_4 , the solubility, reduced by only 0.3% in 0.1 N Na_2SO_4 , was actually increased in higher concentrations of this additive.

Research in the third paper, which dealt with the effect of other salts on the solubility of TlCl, was directed by Bray (12). The solubility of TlCl itself, found to be 16.07 mM per liter, was only 0.13% lower than the original value found by Noyes (5). The increased solubility caused by the presence of KNO_3 or of K_2SO_4 , salts without a common ion, was attributed to the formation by metathesis of nonionized $TlNO_3$ or Tl_2SO_4 . At comparable concentrations, the



Arthur Amos Noyes

Table 3
Effect of Added Salts on the Dissociation of TlCl

Added salt	Log $10^3 \times [\hat{A} i]$	Log $10^4 \times [TlCl]$	Log $10^5 \times [Tl^+][Cl^-]$
None	1.1559	1.2443	1.3115
Tl ₂ SO ₄	1.3703	1.6158	1.3183
	1.5911	1.0931	1.3339
	1.8090	1.0362	1.3642
TlNO ₃	1.4649	1.1446	1.3345
	1.6709	1.0962	1.3690
	1.9141	1.0386	1.4096
K ₂ SO ₄	1.4819	1.1265	1.3243
	1.7157	1.0492	1.3401
	1.9296	0.9850	1.3612
	2.3030	0.8854	1.4126

latter salt is less highly ionized than TlNO₃, so that Tl₂SO₄ has the greater effect. The decrease in solubility caused by the addition of salts with a common ion is in accord with that expected from the ionic product principle.

Bray undertook the discussion of the results described in the three foregoing papers (13). Included in the various tables is a listing of the degrees of ionization, at concentrations from 0.01 to 0.25 N, of the seven salts that were used in the studies. The values were obtained by precise conductometric measurements.

The composition of the various solutions saturated with TlCl was calculated on the assumption that, for each salt, the values in mixtures depend only on the equivalent ion concentration (Σi). To show the relationships in dilute solutions more clearly, Σi , the corresponding concentrations of nonionized TlCl, and the values of the ionic product $[Tl^+][Cl^-]$, were expressed logarithmically. Examples are given in Table 3; the effects of the additives BaCl₂, KNO₃ and KCl were also examined.

In all cases, an increase in Σi caused a decrease in the concentration of nonionized TlCl and an increase in the ionic product $[Tl^+][Cl^-]$. Although the experiments involved univalent and bivalent salts, with and without a common ion, the results were remarkably similar. This supported the assumption that the total ionic strength in a mixture primarily determines the ionization of univalent salts. In the case of less soluble salts, it was concluded that their solubility products would be practically constant in the presence of small amounts of other salts.

Further analysis of the results led to the conclusion that, in the case of TlCl, deviations of the ionization from the law of mass action are due more to the abnormal behavior of the nonionized salt than to that of the ions. In more concentrated solutions of a single salt, the activity coefficient, A/C, decreased more rapidly with further increase in concentration. Available measurements of the emf of Tl⁺-ion concentration cells supported this conclusion.

Theories and equations that were based upon the concept of the complete dissociation of strong electrolytes in solution were eventually developed by others (14). Then Noyes was able to use his acquired experimental results to check these developments.

In 1913, Noyes began a part-time association with Throop College, which became the California Institute of Technology (Cal Tech). This association became full-time in 1919, when Noyes moved from MIT, with the intention of making Cal Tech a great center for education and research. This he certainly achieved. Troubled by ill health during the latter part of his life, Noyes died on June 3, 1936. He had never married; his estate was bequeathed to Cal Tech for support of research in chemistry.

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ABOUT THE AUTHOR

John T. Stock, recipient of the HIST Dexter Award in 1992, is Professor Emeritus of Chemistry, University of Connecticut, Storrs, CT 06269-4060.

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A PHILOSOPHICAL COMMENTARY ON GIUNTA'S CRITIQUE OF NEWLANDS' CLASSIFICATION OF THE ELEMENTS

Eric R. Scerri, University of California, Los Angeles

Introduction and Motivation

In recent years there has been a resurgence in interest in philosophical aspects of chemistry, dating from the early 1990s (1-3). It is gratifying to see that this development has not been confined to analytical philosophy but has frequently spilled over into issues concerning the history of chemistry. Giunta's recent article (4) is therefore of great interest because it represents an example of work that approaches philosophy of chemistry from the historical direction. Giunta's article on Newland's periodic system, or whether indeed it can be called a system, is a bold attempt by a chemist-historian who is willing to venture a philosophical analysis based on an episode in the history of chemistry.

There has been a good deal of discussion in the literature over whether chemical periodicity should be referred to as the periodic table, periodic system, or periodic law. In addition there have been articles in philosophy of chemistry which have attempted to clarify the terms theory, model, or law in the context of chemistry as distinct from physics (5, 6). Giunta's article, which provides an analysis of these terms, should therefore be of interest to philosophers as well as historians of chemistry.

While accepting that a chemical audience may not wish to agonize over the use of terms like system and self-consistency, I believe that the clarification of terms is one area in which a philosophical analysis of chemi-

cal concepts has a powerful role to play. In addition, it cannot be denied that Giunta's main intention is to examine whether Newlands produced a "system" or not. I therefore make no apologies for undertaking an analysis of precisely what Giunta means by the word "system" in this context. After doing so I turn to Newland's work and conclude that he deserves more credit than he has been accorded by Giunta.

Commentary

I believe that Giunta makes some important points about Newland's overall role in the discovery of the periodic system but also that he introduces some misconceptions with which I shall express some friendly disagreement. Although Giunta states, at the outset, that he intends to examine the work of Newlands "from a contemporary point of view," I think there may be some problems with this proposal as I hope to show. Giunta also says that he is not concerned with reconstruction of the process of Newlands' discoveries but only with appraising the validity of his writings. He appears to want to concentrate on the logic of discovery, rather than the context of discovery, to use a distinction that was once popular in philosophy of science. Such a goal is of course laudable, especially given the excessive emphasis on context, and in particular the social context, of scientific discoveries that one finds in recent science scholarship.

System, Organization, and Self-consistency

Although chemists might have an intuitive feel for terms like system, organization, and internal consistency, I ask for their indulgence in pausing to analyze these notions. I feel justified in doing so since Giunta has made them major criteria in his critique of Newlands.

Giunta begins his critical analysis by stating that whereas Mendeleev and Newlands referred to a “periodic law” he, Giunta, intends to follow the author van Spronsen in preferring to use the term “periodic system (7).” However, Giunta believes that van Spronsen’s definition of periodic system is inadequate and in need of strengthening. He tells the reader that van Spronsen defines a periodic system as (4):

...a system of all the (known) elements arranged according to increasing atomic weight in which the elements with analogous properties are arranged in the same group or column.

but that earlier in his work van Spronsen refers to (7):

“facets of a true periodic system” including additional criteria, for example a distinction between main groups and sub-groups, and provision of vacant spaces for undiscovered elements.

Giunta proposes to define a periodic system as something lying between these two versions, given by van Spronsen, namely (7):

a periodic system of the elements consists of a self-consistent arrangement by atomic weight of all the known elements, which systematically displays groups of analogous elements.

Giunta claims that his own definition places considerable emphasis on “organization” and “internal consistency” although he fails to provide any additional criteria to indicate just what these features might mean in this context. Finally, he asserts that he does not require his own sense of system to be one “free from error.”

Giunta’s attempt to improve on van Spronsen’s definition(s) of the term periodic system is, I believe, somewhat problematic (8). Whereas Giunta implies that his own definition is stronger than van Spronsen’s first definition, it is, in fact, weaker. By failing to include the word “increasing,” as a qualifier for atomic weight, Giunta unwittingly admits even earlier systems such as that of Gmelin. In 1843, a remarkable 26 years prior to the first of Mendeleev’s published systems, this chemist classified all the then known elements and obtained a very successful grouping of analogous elements (9). Gmelin’s only failing was that he did not strictly adhere

to increasing atomic weights, something which Giunta does not explicitly specify as an important criterion, although this omission may well have been accidental.

On the other hand, the claim by Giunta that his own definition of “system” is weaker than van Spronsen’s second definition also appears to be mistaken because van Spronsen does not require a system to be free from error. Had van Spronsen done so, he would have excluded many of the precursors of the modern periodic system, which he has so painstakingly documented in his book while considering them as genuine systems.

I turn to considering what Giunta claims to have added to van Spronsen’s definitions. Giunta requires that the qualities of being “self-consistent” and “systematic” should be present in a system displaying all the known elements. However, we are not told what self-consistency actually implies in the context of a classification of the elements.

As for the second requirement, I believe this may be circular. Since the definition given by Giunta was intended to define “system,” it can hardly be illuminated by the statement that a system shall be systematic! The further stipulation that his own definition “places considerable emphasis on organization and internal consistency” does not appear to clarify his position since nothing in the definition actually states how this claim is to be realized.

Is the Periodic System a Theory?

Giunta then introduces a further requirement, namely that a periodic system should also fulfill the criteria given by one George Lachman of what constitutes a theory. Whereas Giunta promises, in his title, to analyze why the work of Newlands does not represent the discovery of a system, I believe he proceeds to cloud the issue by invoking a further set of criteria which are not intended for “systems” but for scientific theories. Whereas up to this point the discussion had focused on whether Newlands had produced a system, Giunta then appears to suggest that “theory” and “system” are synonymous terms. I would like to explain why I believe these terms to be far from synonymous, especially in the context of chemical periodicity.

The term system is very frequently used to describe chemical periodicity, but to the best of my knowledge chemical periodicity has never been regarded as a theory. In fact, according to some authors, chemistry does not possess any genuine theories of its own (10). This inci-

dentally is a reason sometimes given for the lack of interest in chemistry shown by philosophers of science. Chemical periodicity is instead referred to almost exclusively as a system because it is essentially a classification system and, as such, does not depend upon any theoretical underpinning for its success or otherwise. This is why I believe that Giunta may be mistaken in invoking Lachman's criteria for theories in a context where one is simply not dealing with any theory.

Indeed Giunta then introduces into the reckoning some even further criteria, due to Thomas Kuhn, without discussing whether they are consistent with the views of Lachman as well as his own previously stated definition of "system." Having thus set up at least three types of criteria, and without, I would claim, *any* supporting arguments involving examples from Newlands work, Giunta is prepared to declare (4):

Although Newlands' work does not meet the criteria for a periodic system set out above, his contributions were substantial.

Perhaps the most charitable interpretation for this conclusion would be that Giunta intends to show later in the article how Newlands fails to meet the criteria for a system, but this turns out not to be the case. Instead of giving any form of analysis of why Newlands fails to meet his own criteria or those of Lachman and Kuhn, Giunta begins pursuing what he himself states as being of secondary importance, namely the fact that Newlands' "contributions were substantial." I will return to this analysis which takes up the next two pages of Giunta's article, in due course; but first let us turn to the main purpose of the article, namely whether Newlands did or did not produce a "system."

Predictions

In returning to the promised main theme, Giunta begins by stating that it is not so much that Mendeleev produced a better system than Newlands, but rather that Newlands failed to produce anything that might warrant the label of a "system." First of all Newlands' rather remarkable prediction of the existence of an unknown element, which subsequently became known as germanium, is dismissed by Giunta. This is done on the grounds that the prediction was made before Newlands had formulated his law of octaves and that it was carried out with atomic weights instead of ordinal numbers. In doing so Giunta seems to overlook the fact that Mendeleev's spectacular predictions of germanium, gallium, and scandium were also based on atomic weights.

Contrary to Giunta's reading, and regardless of whether or not Newlands called his earlier classification a law of octaves or not, it cannot be denied that he did in fact predict germanium a number of years before Mendeleev, as many historians concur.

In any case it is difficult to see why Giunta is placing so much importance on predictions when he had promised earlier to concentrate on the criteria of "self-consistency" and "organization" in order to assess the worth of a periodic system. Of course, it may well be that Newlands' law of octaves is inconsistent with his prior prediction of the element germanium, but this is a quite separate issue from whether the system itself is self-consistent. As I see it, self-consistency, in any form of system, such as a mathematical system, for example, does not necessarily imply predictive power.

Giunta then proceeds to criticize Newlands on the grounds of failing to accommodate newly discovered elements into his classification. Once again this is a separate criterion that is covered neither by self-consistency nor organization of any system since the latter criteria are not necessarily connected to the possible discovery of new elements. The failure of Newlands' classification to allow for accommodation of new elements is an important drawback but one which I believe is mischaracterized by Giunta's analysis which supposedly hinges on "self-consistency and organization."

Giunta then claims that Newlands' "attempts of systematization" made in 1878 and 1884 came too late. What features make these attempts more systematic, in Giunta's view, is not something that he discusses, except to say that Newlands was now "providing a checklist of specific instances in which he was applying the law." I suggest that Giunta has once again shifted ground in that now an attempt to apply the law of octaves is taken to represent another criterion for deciding whether or not Newlands' classification represents a "system" (11).

Giunta then moves on to praise Mendeleev's superiority over Newlands for making an (4):

...extensive list of deductions which accompanied his predictions from the start.

This is unfortunately not quite the case. Admittedly, the three famous predictions of Mendeleev are hinted at in his original paper of 1869, by the fact that he leaves empty spaces for these elements. But it was not until two years later that Mendeleev was prepared to make detailed predictions on the properties of these elements

and their compounds in his system of 1871 (12, 13). One might argue that two years is not a long time between Mendeleev's vague predictions of 1869 and his detailed version of 1871, but Giunta's remark suggesting that Newlands is the only person whose views on classification of the elements evolved over time seems to be excessive.

There is no denying Giunta's statement that some of Newlands' published ideas showed a deterioration as time progressed. But there are some not too well known aspects of Mendeleev's work which, if examined in isolation, would also lead the reader to realize that the much lauded Russian chemist also falls short of the adulation he is usually accorded. One example is the case, seldom discussed in the literature, concerning the element gallium. In his paper of 1871 Mendeleev predicted that eka-aluminum, subsequently known as gallium, would "in all respects" have properties intermediate between those of the elements above and below it, namely aluminum and indium. However, the melting point of gallium (30°C) is nowhere close to being intermediate between those of aluminum (660°C) and indium (155°C). In 1879 Mendeleev gave the following *ad hoc* rationalization of the anomalously low melting point for gallium (14):

...we should pay heed to the fact that the melting point of gallium is so low that it melts at the temperature of the hand. It might appear that this property is unexpected; but this is not so. It suffices to look at the following series -

Mg	Al	Si	P	S	Cl
Zn	Ga	...	As	Se	Br
Cd	In	Sn	Sb	Te	I

It is evident that in the group Mg, Zn, Cd, the most refractory metal has the lowest atomic weight; but in the groups beginning with S and Cl, the most difficultly fusible simple bodies are, on the contrary, the heaviest. In a transitory group such as Al, Ga, In, we must expect an intermediate phenomenon; the heaviest (In) and the lightest (Al), should be less fusible than the middle one, which is as it is in reality. I turn attention to the fact that properties such as the melting point of bodies depend chiefly upon molecular weight, and not on atomic weight. If we were to have a variety of solid sulphur not in the form of S_6 (or, perhaps, of still heavier molecules S_n), but in the form S_2 , which it assumes at 800°C, then its temperature of melting and of boiling would undoubtedly be much lower. In just the same way, ozone, O_3 , condenses and solidifies much more readily than does ordinary oxygen, O_2 .

Not only had such an argument never been given before by Mendeleev, as a means of predicting trends in properties, but it also runs contrary to the spirit of his method of simple interpolation which he used so successfully in many other instances. The completely *ad hoc* nature of the argument is compounded by the fact that it is by no means clear that this truly represents "an intermediate phenomenon" to those in the other groups mentioned and indeed why this somewhat contrived trend should begin at this particular place in the periodic table. In spite of his use of the word "must" there is nothing in the least bit compelling about Mendeleev's argument.

Nobody would consider denying Mendeleev his triumphs because of such indiscretions. Furthermore these *ad hoc* moves by Mendeleev would seem to be more serious than Newlands' desperate bid to assert his claim to priority in the case of germanium by referring to all his articles rather than, as Giunta would seem to wish, just those published following the announcement of his law of octaves.

Back to Newlands

Contrary to the message in Giunta's title, I believe that Newlands did indeed produce a good periodic system and more importantly perhaps, that he was the first to emphasize the importance of the periodic law, or the law of octaves as he termed it. Indeed, as Giunta points out and documents, the often heard dismissals of Newlands on the grounds that he mistook the repeat distance to be eight elements instead of nine, in the short periods, is something that Newlands himself fully anticipated.

Newlands' contribution lies in having been the first to recognize that the crucial feature lay in the approximate repetition, or periodicity, of the elements and that this behavior is law-like. Whether this repetition occurs after seven, eight, or even nine elements is beside the point. I believe that Giunta's arguments for criticizing Newlands' system because of what he regards as inconsistencies have missed this important aspect. But I agree with Giunta's drawing attention to the fact that some of Newlands' later systems did not leave any gaps for undiscovered elements and thus negated his periodicity of eight.

This mention of leaving gaps raises the vexing question of just how important predictions are in science, something that Giunta does not discuss in spite of the extensive literature on the subject and the fact that this

debate is still being actively pursued and precisely in the context of the periodic system (15-20). Whether or not prediction is an especially important aspect of scientific developments is open to question, as the current debate continues to show. On the other hand, the discovery of laws as a very important scientific activity is accepted with less controversy. To return to a theme I alluded to earlier, Newlands deserves perhaps more credit than Giunta is giving him, precisely because he was the first to recognize a law-like behavior in the way that elements seem to recur after certain intervals.

Although Giunta recognizes that law-likeness is important, he seems to be prepared to ignore this aspect in the course of pronouncing judgment on Newlands' scientific contributions. Instead, as the title of the article in question indicates, Newlands is being criticized for failing to discover a "system," according to Giunta's rather idiosyncratic criteria for what constitutes a system.

I suggest that in attributing merit it is not the ability to capture the small details that should be valued most, but rather to grasp the existence of a general law. If this is accepted then, contrary to Giunta's position, Newlands should be lauded rather than faulted. Admittedly, Newlands was mistaken in not realizing that this repeat distance was variable. But in terms of announcing the existence of a law of regularity, which would have very important ramifications, he was the first to do so.

Atomic Number

Finally, I turn to Giunta's critique of Newlands over the question of atomic number since I believe that the arguments proposed are to some extent misplaced and rather Whiggish. Giunta contradicts Wendell Taylor's statement (21), that Newlands might have been "a pioneer in atomic numbers" because as Giunta puts it (4):

For several reasons that number is not the same as the atomic number known today.

The first such reason for Giunta is that the discovery of elements unknown to Newlands would cause some of Newlands' higher atomic numbers to be too low. Although this is indeed the case, I believe it to be a trivial objection to the general principle of using an ordinal number to order the elements rather than their individual atomic weights. Clearly, Newlands could not have known the correct atomic numbers of all the elements at the time at which he was writing.

Giunta's second reason, the fact that Newlands assigned the same ordinal number to some elements, is a more serious problem although it only occurs six times in as many as 56 entries in Newlands' table of 1866.

The final reason given by Giunta for rejecting the notion that Newlands foresaw atomic number is also disputable (4):

Finally, Newlands was not aware of the physical basis for atomic number first elucidated by Moseley more than half a century later.

If the issue is whether Newlands in some sense anticipated the notion of atomic number, then he could only have done so in the absence of the knowledge of its physical basis. One cannot help wondering whether Giunta might also want to diminish Mendeleev's discovery of chemical periodicity itself because he was not aware of its "physical basis" until this was provided by Niels Bohr, in the form of electronic configurations of atoms, also about half a century later.

Conclusion

Giunta is to be applauded for trying to bridge the unfortunate gap between the study of historical and philosophical aspects of chemistry. He has begun to analyze the term "system" in the work of John Newlands, while drawing on the historical record. I hope the comments raised here will stimulate a deeper analysis of the issues involved.

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ABOUT THE AUTHOR

Eric Scerri, Department of Chemistry & Biochemistry, University of California at Los Angeles, Los Angeles, CA 90095; E-mail: scerri@chem.ucla.edu. Scerri's research interests lie in the history and philosophy of chemistry and in chemical education. He is the founder-editor of the interdisciplinary journal *Foundations of Chemistry*. <http://www.wkap.nl/journals/foch>

HISTORY OF CHEMISTRY DIVISION

<http://www.scs.uiuc.edu/~maintzvHIST/>

A RESPONSE TO SCERRI'S COMMENTARY

Carmen J. Giunta, Le Moyne College

Dr. Scerri raises several thoughtful points, both historical and philosophical, in his commentary (1) on my paper (2) about J. A. R. Newlands' classifications of the elements. I would like to respond to several of those points, to agree with some of them and to highlight some which we view differently.

I agree that my definition of periodic system was ambiguous and somewhat circular as regards "system." Let me try again. What constitutes a periodic system? It must be periodic and it must be systematic. In my article, I believe I specified what I meant by periodic: arrangement by atomic weight and grouping of elements with common properties; blank spaces for new elements and main group/sub group distinctions were not necessary. I was much less definite on what it meant to be systematic, specifying only internal consistency. Clearly more was needed. What I had in mind but did not explicitly define can be described as clarity of exposition and of classification: a classification system ought to be clear about which elements constitute a group of related elements.

Several other criticisms of my criteria for what constitutes a periodic system are, I believe, less well founded. Scerri states that I introduced an additional requirement, that a periodic system fulfill the criteria of Sheldon Lachman about "what constitutes a theory." Lachman's criteria are for theory preference, not for what constitutes a theory (3). I did judge Newlands' work by Lachman's criteria in addition to my (ambiguous) criteria for a periodic system; these were separate analyses that addressed different questions. Scerri comments that I introduced even further criteria (due to

Thomas Kuhn) without mentioning whether they are consistent with either Lachman's or my earlier criteria of periodic system. On the contrary, my purpose of listing Kuhn's criteria was not to introduce another set, but to show that Lachman's list—which I find to be particularly clear—is not anomalous in philosophy of science. Indeed, I quoted Kuhn: "Together with others of much the same sort, they provide the shared basis for theory choice (4)."

Dr. Scerri suggests that I implied that the periodic system is a theory. I must admit to having had no conscious intent to imply such a thing; however, I recognize that my paper can be fairly read as suggesting just that, or at least as blurring the lines between theories on the one hand and classification systems on the other. Upon reflection, I do not wish to make such a suggestion, and I recognize that distinctions between theories and classification systems can be useful. Still, I think that classification systems and empirical laws are amenable to analysis under criteria for theory preference such as Lachman's, Kuhn's, or the like.

This may be a fruitful point for discussion. Must a theory be explanatory? predictive? or may it be simply descriptive? Clearly, a report of raw observations or experimental results is not a theory; such a report is descriptive, but not well organized. Empirical laws and empirical classification systems are also descriptive, but in a more organized way, correlating the observations upon which they are based; however, they need not be explanatory (*i.e.*, state why a relationship holds). An empirical law asserts a relationship between quantities generalized from individual instances and at least im-

PLICITLY predicts relationships that can be measured in the future between the same variables. A classification scheme is also assertive, if not at least implicitly predictive, if it claims that it is a natural classification; its groupings assert that in one respect or another, A is like B and unlike C. Empirical laws and classifications are susceptible, at least in principle, of being formulated in alternative ways. In that they are assertive generalizations susceptible of alternative formulations, empirical laws and classification systems are similar enough to theories that Lachman's or Kuhn's criteria are appropriate for preferring one to another. This is not to deny that a distinction between theories on the one hand and classifications or empirical laws on the other may well be useful.

One main point of disagreement between Dr. Scerri's analysis of Newlands' work and my own is over my treating all of Newlands' work as a whole, which I found incoherent. He seems to read Newlands' classifications as ideas evolving over time. This interpretation is not unreasonable; however, I believe that the historical record supports my interpretation (5). One can conceptually separate triad-based classifications (which include predictions of undiscovered elements, including one of the element now called germanium) from order number classifications (including the "Law of Octaves" and later classifications). If one were to consider these two phases separately, then the triad phase is certainly not a periodic system because it did not embrace all elements. The law of octaves is a periodic system by my definition, although it is not as clear as I would like about what elements constitute a group of related elements. (The law of octaves falls short of the more elaborate versions of Mendeleev's classifications on several of Lachman's criteria, but those criteria are for preference, not for periodic system.) In short, if I regarded order-number classifications as displacing triad-based classifications, then I would admit that the latter constitute a periodic system. I do not think the historical record supports such an interpretation, though. Newlands' monograph, written years after the classifications, seems to embrace both of these phases simultaneously. In addition, the first of the order-number papers (August 8, 1864) was written soon after the last of the triad-based papers (July 12, 1864) without any explicit break.

Beyond these differences in interpreting the historical record, I believe Dr. Scerri's analysis misses the mark in several particulars. For example, he asserts that I failed to show how Newlands fell short of my criteria

for periodic system or the criteria of Lachman and Kuhn, and that instead I followed a summary judgment on this matter by pursuing secondary issues, namely Newlands' substantial contributions. In fact, the section to which Scerri refers was titled "The Case for Newlands," and it treated those aspects of Newlands' work that *satisfy* parts of my definition of a periodic system. The next section ("Why Newlands' Insights Do Not Constitute a Periodic System") asserted that Newlands' work is not a periodic system because it is not systematic. While I admit the inadequacy of my definition of periodic system, this section certainly addressed the definition, criticizing the internal inconsistency of Newlands' writings. Finally, the last section of the paper ("Assessment Using Lachman's Criteria") rated Newlands' work on all six of those criteria, one by one. Scerri seems to think that I unjustly dismissed Newlands' prediction of germanium, made on the basis of atomic weight relationships, and overlooked the fact that Mendeleev's predictions were also based on atomic weights. Far from dismissing the prediction of germanium, I emphasized it because predictions of new elements were logically inconsistent with the later periodic classification (the law of octaves) which left no room for new elements. Furthermore, the basis of Newlands' (and Mendeleev's) predictions was not wrong but it was not the basis of Newlands' periodic classification. Scerri contradicts my assertion that Mendeleev's system included an "extensive list of deductions ... from the start," characterizing predictions in his original 1869 paper as merely "hinted at" by leaving empty spaces in his table. In fact, the 1869 paper contained several explicit deductions, including explicit predictions of two new elements (6). Finally, Scerri finds my "critique of Newlands over the question of atomic number" to be "to some extent misplaced and rather Whiggish." I do not consider my words on atomic number to be a critique: I credited Newlands for the ordinal number concept and quoted without contradiction Taylor's assessment of Newlands as a "pioneer in atomic numbers (7)." I went on to describe the differences between the modern concept of atomic number and Newlands' ordinal number, with no criticism stated, intended, or implied (but apparently criticism was inferred). Of course this section is Whiggish in that it describes current understanding of a past proposal.

Dr. Scerri offers some observations that I believe would have improved my paper if I had incorporated them; he raises some issues that thoughtful scholars can fruitfully debate and over which they may disagree, but he makes some points that I believe are not well founded.

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ABOUT THE AUTHOR

Carmen Giunta is Associate Professor of Chemistry at Le Moyne College, Syracuse, NY 13214. A physical chemist by training, he is particularly interested in applying history of chemistry to chemical education. He maintains the Classic Chemistry web site: <http://webserver.lemoyne.edu/faculty/giunta>

Paul Bunge Prize

The Paul Bunge Prize 2001 has been awarded to Dr. Jim Bennett, Museum of the History of Science, Oxford, in recognition of his complete historical works on scientific instruments. The prize of DM 15,000 will be presented on September 25, 2001 in Würzburg, on the occasion of the annual meeting of the Gesellschaft Deutscher Chemiker. Deadline for applications for the Paul Bunge Prize 2002 (7,500 Euro) is September 30, 2001: Contact German Chemical Society, Public Relations Department, PO Box 900440, D-60444 Frankfurt am Main: pr@gdch.de.

CALL FOR NOMINATIONS FOR THE EDELSTEIN AWARD

The Division of the History of Chemistry (HIST) of the American Chemical Society (ACS) solicits nominations for the 2002 Sidney M. Edelstein Award for Outstanding Achievement in the History of Chemistry. This recently established award honors the memory of the late Sidney M. Edelstein, who established the Dexter Award in 1956, and it also continues the outstanding tradition of the Dexter Award, which came to an end with its posthumous presentation to Dr. William A. Smeaton in 2001.

The Edelstein Award is sponsored by Ruth Edelstein Barish and Family and is administered by HIST. In recognition of receiving the Edelstein Award, the winner is presented with an engraved plaque and the sum of \$3500, usually at a symposium honoring the winner at the Fall National Meeting of the ACS, which for 2002 will be held in Boston, Massachusetts, August 18-22. The award is international in scope, and nominations are welcome from anywhere in the world. Previous winners of the Dexter Award have included chemists and historians from the U.S., Canada, Germany, France, Holland, Hungary, and Great Britain.

Nominations should consist of a complete curriculum vitae for the nominee, including biographical data, educational background, awards, honors, publications, presentations, and other services to the profession; a nominating letter summarizing the nominee's achievements in the field of the history of chemistry and citing unique contributions that merit a major award; and at least two seconding letters. Copies of no more than three publications may also be included if they are available. All nominations should be sent in triplicate to Prof. Seymour Mauskopf, Chair of the Edelstein Award Committee, Department of History, Duke University, Durham, NC 27707 (e-mail: shamus@acpub.duke.edu), by January 31, 2002.

BOOK REVIEWS

Instruments and Experimentation in the History of Chemistry, F. L. Holmes and T. H. Levere, Ed., MIT Press, Cambridge, MA, 2000. xvii. + 415 pp, Cloth, ISBN 0-262-08282-9. \$50.00.

This volume is part of the new "Dibner Institute Studies in the History of Science and Technology" series and contains 14 essays dealing with the evolution of chemical apparatus and laboratory techniques. Part I, entitled "The Practice of Alchemy," contains three essays covering the evolution of early distillation apparatus (Robert Anderson), the relation between alchemy and assaying (William Newman), and the problems of replicating alchemical apparatus and experiments (Lawrence Principe).

Part II, entitled "From Hales to the Chemical Revolution," contains six essays covering the evolution of apparatus for the generation and isolation of gases (Maurice Crosland), the development of the eudiometer (Trevore Levere), the evolution of Lavoisier's chemical apparatus (Frederic L. Holmes), the development of hydrometers (Bernadette Bensaude-Vincent), the development of 18th-century thermometers (Jan Golinski), and 18th-century uses of platinum and ground glass in apparatus design (William Smeaton).

Part III, entitled "The Nineteenth and Early Twentieth Centuries," contains five essays covering a reassessment of the experimental work of Wollaston and Thomson on multiple proportions (Melvyn Usselman), the development of organic combustion analysis in the period 1811-1837 (Alan Rocke), the experimental study of gun powder (Seymour Mauskopf), apparatus innovation in the work of Edward Frankland (Colin Russell),

and apparatus usage in the career of Michael Polanyi (Mary Jo Nye).

One could hardly ask for a more sterling cast of contributors, nine of whom are former Dexter Award Winners. Consequently, it comes as no surprise that all of the contributions are well done and of great interest nor that many of the essays are amplifications of subjects in which the authors already have well established reputations. Regrettably this also means that there is little or no substantive coverage of events after about 1840, as the two essays by Russell and Nye, which post-date this period, are really biographical vignettes rather than focused studies of significant developments in apparatus and experimental innovation. Given that the period 1840-2000 contains some of the most spectacular advances in chemical instrumentation and laboratory technique, this omission is unfortunate and appears to be tied to another quirk of this volume (shared also by its sister volumes in the same series)—namely the total absence of any chemical historians (i.e., chemists) among the lists of contributors. Though the Division of the History of Chemistry has made many publication and symposia opportunities in the history of chemistry available to professional historians of science, this generosity in the sharing of resources appears to be a one-way street.

However amateurish some of their attempts to do chemical history may appear by modern standards, chemists have always excelled at documenting the history of their apparatus and procedures. One needs only mention the pioneering work of John Stock, L. S. Etre's studies on the development of chromatography, or the heroic efforts of John Ferraro and his collaborators to document the post-Second World War instrumentation revolution.

With the possible exception of Hans Jenemann's recent (1997) monograph on development of the chemical balance, the individual authors appear to have done a good job in citing this literature in their individual contributions, where appropriate. The same, however, cannot be said of the book's introduction. Surely one of the responsibilities of an editor is to establish continuity between the book in question and the older literature in the field, even if this older literature no longer reflects current historiographic standards. Yet one scans the main introduction and the shorter section introductions in vain for any explicit mention of the work of John Stock, of Ernest Child's 1940 study, *The Tools of the Chemist*, or of the even earlier volume, *A Pictorial*

History of Chemistry, by F. Ferschl and A. Süssenguth (1939). Despite its title, this latter work is essentially a pictorial history of chemical apparatus from alchemy through about 1850 based on the extensive displays developed by Süssenguth at the Deutsches Museum in Munich before the Second World War.

But despite these minor defects (and the reviewer readily admits to being inordinately curmudgeonly for harping on them in the first place), this volume as a whole represents a valuable and worthwhile commentary on the development of chemical, albeit "early" chemical, apparatus. *William B. Jensen, Department of Chemistry, University of Cincinnati, Cincinnati, OH, 45221-0172.*

Instrument-Experiment: Historische Studien, C. Meinel, Ed., Verlag für Geschichte der Naturwissenschaften und der Technik, Berlin-Diepholz, 2000. 423 pp, Cloth, ISBN 3-928186-51-5. 34 Euro.

This collection of 37 essays dealing with the development and impact of scientific instrumentation was commissioned by the German Society for the History of Medicine, Natural Science and Technology (Deutsche Gesellschaft für Geschichte der Medizin, Naturwissenschaft und Technik). The editor, Christoph Meinel of the University of Regensburg, has divided the contributions into six groups, entitled "Historiographic and Methodological Perspectives" (four essays), "Instruments and the Manufacturing of Reality" (eight essays), "Establishing Instrumental Procedures" (six essays), "Microscopic Views and Scientific Knowledge" (twelve essays), "Instrumentation and Social Practice" (eight essays), and "Towards the Materiality of Instruments" (seven essays).

All but three of the essays are in German and only two deal explicitly with topics relevant to the history of chemistry (Nikos Psarros, "Was sah Ostwald (als er die Brille von Frantisek Wald ablegte)?" and Anthony Travis, "Surrogate Instruments: Industrial Chemical Reactors and Organic Chemistry"). The vast majority of the remaining essays deal with topics in the history of physics, medicine, or physiology.

As can be seen from the translations of the section headers, the organization of the book is heavily influenced by current fads in the sociology and philosophy of science, and practicing scientists are likely to find themselves puzzled by some of the bizarre terminology and superficial metaphors employed in some of the more "theoretical" contributions. Nevertheless, several of the authors have managed to present straightforward factual accounts, which, despite their brevity, may prove useful to those interested in the history of instrumentation and its role in the construction and verification of scientific theories. *William B. Jensen, Department of Chemistry, University of Cincinnati, Cincinnati, OH, 45221-0172*

Gold aus dem Meer; Die Forschungen des Nobelpreisträgers Fritz Haber in den Jahren 1922-1927, Ralf Hahn, GNT-Verlag, Diepholz, 1999. ISBN 3-928186-46-9, paper, 101 pp. DM 24.60.

For those who may have read about Haber's quest for gold from the ocean in his biography (D. Stoltzenberg, *Fritz Haber: Chemiker, Nobelpreisträger, Deutscher, Jude*, Wiley-VCH Verlag GmbH, Weinheim, 1994; see review, *Bull. Hist. Chem.*, **1999**, *24*, 77-78.), this booklet by Hahn will provide 100 pages of detail about the ambitious, unsuccessful venture. This gold-covered publication, the result of a master's degree study (*Magisterarbeit*), contains an introduction by Lutz Haber (1920-), youngest son of Fritz Haber and a science historian living in Bath, England.

A highly abbreviated but richly annotated biography of Fritz Haber serves as a preliminary chapter. In the following ten-page chapter, Hahn presents the background of the status of research on extraction of gold from seawater at the time Haber considered undertaking this project in the 1920s. The first proposal by J. L. Proust that the sea might contain significant amounts of gold appeared surprisingly early, in 1787. A. Wurtz suggested it in a lecture in 1866, and the first recorded experiments on the subject are attributed to the British scientist E. Sonstadt, in 1872. The author describes in some detail the various methods devised for the separation and analysis of noble metals, from 1872 up to 1918, as reported by British, Norwegian, French, Swiss, and New Zealand experimentalists. This includes a comparison of the sensitivity of analyses and practicality of the methods. When Haber initiated his program, he elected to use the technique described in 1918 by H. Koch, consisting of adsorption of aqueous solutions on wood charcoal, followed by ignition and cupellation, to afford gold in the form of pellets.

The major portion of this booklet is taken up with Haber's research project on gold, carried out at the Kaiser-Wilhelm Institute in Berlin, from 1922-1927, in "Abteilung M." There is a suggestion—although undocumented—that Haber may have been given the idea to pursue extraction of gold from seawater by Arrhenius in Stockholm in June, 1920 when Haber received the Nobel Prize. The motivation for such an ambitious undertaking was the idea that recovery of gold would serve as a source of repayment of Germany's huge war debt, amounting to over 200 billion

Goldmarks. Even reduced to 132 billion by 1921, this sum would have been the equivalent of 50,000 tons of gold. The research got underway by 1922; carried out by Haber's institute associates, it led to six doctoral dissertations in the next six years. One of those doctoral students, Johannes Jaenicke, published a short description of the project in *Die Naturwissenschaften* in 1935 but also provided bountiful documents, which are housed in the archives at the Max-Planck-Gesellschaft, Berlin-Dahlem. Like the biographer Stoltzenberg, Hahn depended heavily on these documents for his research. Preliminary experiments were directed toward effective workup and analysis of gold-containing samples, mostly synthetic. The author provides substantial detail on various separation and analytical designs, which are not included in this review. Results from the sparse number of real seawater samples were disappointingly lower than those reported by earlier researchers. Perhaps Haber sensed the project would entail more elaborate support to be successful, for he turned to external sources for financial backing. Ever skilled in such collaborations, he eventually arranged the establishment of a consortium with Degussa and Metallgesellschaft. Haber was to provide the scientific expertise, while Degussa and Metallgesellschaft would finance the project up to \$100,000. Moreover, in all decision making, Haber would hold two votes to one each for the other two participants. Any financial gain to be realized would be distributed, 50% to Haber, and the other half equally divided between Degussa and Metallgesellschaft. Author Hahn notes that archival material from Metallgesellschaft served as a valuable resource for his historical research and that his request to use records from Hapag, Hamburg, were denied. No mention is made of any materials from Degussa.

By the summer of 1923 plans were in place for an expedition in the Atlantic Ocean. On board the *Hansa*, a ship of the Hamburg-America line, two cabins were outfitted as laboratories and sleeping quarters for Haber and three coworkers, who took samples and analyzed them during the round trip from Hamburg to New York in July and August. A second expedition in September afforded fewer results than the first, which had been erratic. In October a third expedition, on board the *Württemberg*, also a ship of the Hamburg-America line, headed for Buenos Aires, this trip having been financed by the German Navy and "Notgemeinschaft." As with the first *Hansa* expedition, Haber was on hand, along with three doctoral students who took samples and performed analyses in a newly designed and improved labo-

ratory. The values for gold content were even lower than those from the two earlier excursions; this raised questions about the efficacy of the ever changing methods and the possible uneven distribution of gold in the oceans. Still fairly optimistic, Haber gave a lecture in Buenos Aires before the German Club; and in the spring of 1924 in Dahlem he spoke at the second conference on "Seejod." There followed yet two more sea voyages, one on board the *Poisedon* in the North Sea in May, 1924, the other in April, 1925 on the *Meteor*, which made 14 round trips between Africa and South America, while the scientists on board took copious samples. Most were analyzed back at the Kaiser-Wilhelm Institute. Haber arranged for seawater samples to be sent to Berlin for analysis from Iceland, Greenland, the San Francisco Bay, and even from the Rhein River near Karlsruhe. From the *Meteor* expedition 1,635 samples were collected and 85% of them eventually analyzed back in Berlin. The average gold content was 4×10^{-3} mg/ton of seawater; this was deemed to be 1/1,000 the amount

for a viable extraction process. At this point Haber pronounced the project hopeless. In a lecture in May, 1926, entitled "Gold in the Sea," Haber asserted he had given up searching for the needle in the haystack!

In a brief summary, the author brings us up to the present day. During the quest for gold between 1920 and 1926, Haber experienced no competition; yet other reports of gold detection came out between 1927 and the early 1940s, all values being low and in the same range as those of Haber. With the development of new analytical techniques, such as neutron activation, the field is still being studied, as indicated by various patent applications. Yet the average concentration reported in the 1990s— 10^{-2} mg/ton is not far from the final average figure from Haber's work.

Hahn concludes his richly documented and illustrated booklet with Roald Hoffmann's poem, "Fritz Haber (1993)." *Paul R. Jones, University of Michigan.*

Science and Engineering in Ireland in 1798: A Time of Revolution, P. N. W. Jackson, Ed., Royal Irish Academy, Dublin, 2000. viii + 81 pp, paper, ISBN 1-874045-77-1. IR £5.00.

This collection of five essays is the outcome of the proceedings of a symposium organized by the National (Irish) Committee for the History and Philosophy of Science, held in November, 1998, as a bicentennial recognition of the 1798 rebellion of the United Irishmen. Authors are affiliated with Queen's University, Belfast; Trinity College, Dublin, or University College, Dublin. Three of the chapters cover the status of some of the sciences at the time of the rebellion: biology; geology; and science, engineering and the military. Of particular interest to chemists will be the remaining two chapters on two chemists, both of whom ended up in careers in the United States: William James MacNeven (1763-1841) and John Patten Emmet (1796-1842). Neither chemist is indexed in Ihde's *Development of Modern Chemistry*; only MacNeven appears in Partington's history, although both are included in Miles and Gould, *American Chemists and Chemical Engineers*, 2nd ed. (1994). While MacNeven's professional career at Co-

lumbia College of Physicians and Surgeons, both in medicine and later in chemistry, is well covered by Miles, the Irish publication includes MacNeven's involvement in the abortive rebellion and other background before he emigrated to the US. Emmet migrated with the rest of his family to the US as a child; it was his father who participated in the rebellion and was imprisoned for a time. In New York he was educated by a Trinity College graduate, Richard W. Thompson, and then studied medicine under MacNeven. Emmet became adept in laboratory skills and was also a talented artist. Eventually he became Professor of the School of Natural History at the University of Virginia, publishing several articles in analytical chemistry and electrochemistry, among other topics. A complete list of his publications is included in this essay, along with some of his original sketches. Both articles on MacNeven and Emmet are enhanced with figures from the Irish Rebellion, photographs of the two chemists and Emmet's wife, of title pages from MacNeven's popular texts, of the original Rutgers Medical College (co-founded by MacNeven), of the monument to MacNeven in New York, and a reproduction of Thomas Jefferson's hand written invitation to Emmet for the faculty position in Charlotte. *Paul R. Jones, University of Michigan.*

The Biographical Dictionary of Women in Science: Pioneering Lives from Ancient Times to the Mid-20th Century, Marilyn Ogilvie and Joy Harvey, Ed. 2 vol., Routledge, New York and London, 2000. xxxviii + 1499 pp, \$250.

This extensive compilation is an expansion and updating of Marilyn Ogilvie's earlier biographical dictionary, *Women in Science, Antiquity Through the Nineteenth Century* (1986). Embracing a broad range of women taking part in scientific or science-oriented work, from public health activists and alchemists to writers and zoologists, it covers a span of 2500 years and includes people from all five continents. In addition to the narrative texts that form the body of the work, there are three sets of lists of women by occupation, time period, and country or geographical region. A very good bibliography of about 200 standard sources covering material up to 1998 is also included. The volumes are handsomely bound; type is clear on glossy paper; names are easy to spot on the page; and there is a good general index.

Entries follow a uniform format but vary in length depending on the prominence of the subject and the amount of information found by the writers. Thus, on looking at chemists in particular, several pages are devoted to Marie Curie; early Massachusetts Institute of Technology instructor Ellen Swallow Richards (also known for her pioneering work in home economics) is discussed in an article of medium length. Lesser players in the drama and many for whom information is less readily accessible are afforded coverage in only a paragraph or two—such as the tantalizingly brief glimpse of Leonora Bilger (born 1893), a successful early chemist, Garvan medalist, and department head at the University of Hawaii from 1943 to 1954. A short but interesting sketch is offered of the career of Mary Peters Fieser, wife and research colleague of Louis Fieser and the co-author of the famous Fieser and Fieser texts so well-known to many of us. Greek-born bacteriologist and social activist Amalia Coutsouris Fleming, second wife of Sir Alexander Fleming of penicillin fame and his co-worker in investigations on streptomycin, is also included. Overall, the work shows signs of haste with many typographical errors and other more important oversights which have the unfortunate effect of shaking one's confidence in the work as a whole. The reader with scientific background especially may well be somewhat disappointed. To this reviewer the stan-

dard does not match that of Marilyn Ogilvie's 1986 *Dictionary*, either in the overall quality of the articles or the factual accuracy. Unfortunately there are occasional difficulties with basic information. For instance late nineteenth-century Italian bacteriologist Giuseppina Cattani's last name is spelled consistently as Catani, and early twentieth-century British chemist Alice Emily Smith is designated as having the working life 1850-1905. In Smith's case the confusion arose from the fact that a scholarship she held as a student commemorated the Great Exhibition which took place in London in 1851; she herself published her first paper jointly with W. H. Perkin Jr. in 1902, and she remained very active in research and publication until at least 1909.

A number of entries that have been carried over essentially unchanged from the earlier edition remain very adequate, such as those on Marie Curie and Ellen Swallow Richards. However, in some cases updating would have been appropriate, as in the discussion of the work of French mathematician Sophie Germain. It is no longer the case (as it was in 1986) that Fermat's Last Theorem is unproven, the task having been accomplished by Andrew Wiles in 1995; and although Germain's work remained an important contribution referred to by others for many decades, Wiles's proof does not depend on it but rather on tools developed long after her day. New entries written by scientists and contributors who have made extensive studies of the person under discussion are good, such as that on early twentieth-century German physicist Hertha Sponer-Franck, professor at Duke University for almost thirty years and remembered for her quantum mechanical studies, including investigations of structural properties of complex molecules.

One might wish that the assistance and guidance of specialists in the fields had been used more extensively so as to reduce difficulties with discussions of technical work and provide somewhat fuller coverage. A number of articles on women chemists published during the last fifteen years in the *Bulletin for the History of Chemistry* have not been consulted, and neither has Volume 2 of *American Chemists and Chemical Engineers* (W. Miles and R. Gould, 1994). These sources could have provided needed additional information. However, the editors will undoubtedly correct in later editions such rather serious errors as those which detract from the presentation of the work of American bacteriologist/biochemist Rebecca Lancefield ("Lancefield found evidence that countered the accepted belief that type-specific virulences [*sic*] were carbohydrates of [*sic*] polysaccharides," p 739). It is perhaps something of an exaggeration

tion to claim Mt. Holyoke botanist and general sciences instructor Lydia Shattuck as “one of the founders of the American Chemical Society (p 1182).”

As noted in the editors’ introduction, the more ready accessibility of source materials biases the coverage in favor of the United States. Nevertheless, women of France, Germany, and especially Britain are also fairly well represented. Some entries for which the available source material is plentiful but in non-English language publications are disappointingly brief—which is perhaps understandable in a work of this size since the labor of translating can be time consuming. The articles on Russian women, while a welcome addition to the somewhat meager coverage of this national group in English language sources, hardly reflect the tremendous increase in the participation of women in scientific work during the Soviet era.

A few words concerning the interpretation of the lists of women by occupation, by time period, and by nationality are perhaps in order since these lists are important in fulfilling one of the editors’ stated hopes: namely, that this dictionary will provide a unique opportunity to view subjects “longitudinally within fields over a long period of time or horizontally across fields within a restricted time period (Introduction, xi).” The lists run into the problems typical of such categorization attempts. While their lengths offer approximate indications of the growth of women’s participation in scientific work over time, the extent to which women penetrated into particular fields, and the countries in which they have been most successful, the overlaps between divisions within each list mean that simple enumeration might well give one a somewhat exaggerated idea of the extent of women’s activity in science. This also holds for a comparison of fields within individual countries. (Here the overall bias towards the United States is not a complicating factor.) Thus chemist Charlotte Roberts of Wellesley College appears in both nineteenth- and twentieth-century lists; mid-westerner Laura Linton, who during her years in chemistry carried out a mineral analysis in addition to her rather notable, but not mentioned work in petroleum chemistry, appears under both chemists and mineralogists (as well as under physicians and educators, reflecting other periods of her career). There are also some unexpected and unexplained editorial choices. For instance, why present two lists of Dutch women separated under the headings “Holland” and “Netherlands”? And why place nineteenth-century St. Petersburg chemist Anna Volkova, a protégée of Mendeleev, in the listing headed “USSR” rather than

in that headed “Russia”? Even with the lists and the impressively large number of subjects included, it is somewhat difficult to see this dictionary as meeting the editors’ hope of offering a “unique perspective” for international and cross disciplinary comparisons; its basic style and methodology keep it too firmly anchored in the standard pattern for dictionaries of women in science.

Critical judgment of the work of some of the more eccentric women appearing in this very inclusive collection is rarely offered. Mary Boole, widow of the famous nineteenth-century British mathematician George Boole, developed and published pioneering ideas on the teaching of mathematical concepts to young children, ideas of continuing interest to educational psychologists; however, it might have been advisable to acknowledge that at the same time, caught up in current psychic research and related fads, Mary Boole brought out a considerable amount of what is best described as nonsense. Likewise, while the entry on nineteenth-/twentieth-century Paris-based feminist and writer Céline Renooz offers a fairly realistic assessment of Renooz’s wildly extravagant conjectures about current science and the development of human cultures, the discussion of Renooz’s contemporary Clémence Royer is somewhat less balanced. Science writer and commentator Royer, author of the first French translation of Darwin’s *Origin of Species*, has her enthusiastic supporters among historians, but others advocate a more cautious approach. Some mention might have been made of the fact that Royer was a convinced Lamarckian who failed to grasp the essential difference between Lamarck’s ideas of evolution and Darwin’s theory of natural selection—a failure which distorted her translation. As well as making the dictionary articles more bland and less interesting than they otherwise might have been, this very delicate approach will make it difficult for the uninitiated reader to form a realistic idea of the importance of the person under discussion.

Readers already familiar with the field of women in science will find here new names; specialists will also quickly realize they need to take the precaution of checking the details provided. On the other hand, those who have little background in the subject, particularly beginning students, should approach these volumes with caution. Although the basic information of nationality, time period, and area and general extent of scientific activity of this large collection of women of science provides a useful point of entry, careful scrutiny of additional information is recommended.

Despite the caveats, the fact remains that this dictionary is a noble effort, the product of a very considerable amount of labor on the part of the two editors, who have themselves written a large fraction of the articles. They are to be commended for making available in one

location a great many names of women scientists; the work will unquestionably remain an essential, standard reference in American libraries for many years. *Mary R. S. Creese, Hall Center for the Humanities, University of Kansas, Lawrence, KS 66045.*

Auf der Suche nach dem Stein der Weisen: Die Geschichte der Alchemie. Hans-Werner Schütt, Verlag C. H. Beck, Munich, 2000. 602 pp, DM 68.50.

The history of alchemy has come into its own in recent years, the Renaissance and early modern periods having been particularly well covered by such eminent scholars as Allen Debus, Betty Jo Dobbs, Karin Figala, Owen Hannaway, William Newman, Lawrence Principe, and Pamela Smith. What has been lacking is an up-to-date recounting of the entire span of alchemical history, from antiquity to the modern period. The most recent such surveys are now nearly a half century old. (Readers should be aware, however, of the admirable *Alchemie: Lexikon einer hermetischen Wissenschaft*, edited by Claus Priesner and Karin Figala, and published by Beck Verlag in 1998.)

In the book under review, the outstanding historian of chemistry Hans-Werner Schütt provides us with such a history. Here, in 600 pages and close to a hundred short chapters, is an examination of the full range of alchemical lore; beginning “in the shadows of the pyramids,” Schütt progresses through “foreign worlds” (the Arabic period), then “into monasteries and elsewhere” (the middle ages), and finally “into the new world of Europe.” In an afterword, Schütt notes that it was not his intent to provide a scholarly investigation of the subject that seeks novel understanding. Rather, his aim was to get under the skin and into the minds of the alchemists of various times and places. This included, for Schütt, mining matters “anecdotal, philosophical, psychological, and political,” in the hope of presenting, as Golo Mann put it, “what is uncommonly entertaining in history.” In this way, he concluded, he

could steer safely between the Scylla of “professorial incomprehensibility” and the Charybdis of “cheap popular showmanship.”

Schütt’s navigation was up to the task. The book is well organized, lively, and chock filled with interesting matter. The writing is effective and often suffused with a deliciously wry sense of humor. In some respects Schütt was overly modest in his protestations, for there is much that is novel and sometimes even profound here. One example is a wonderful passage on what might be called the teleology of everyday life, an effective piece of rhetoric that allows entrée into the teleological psychology of alchemy (pp 63-64).

In another fascinating passage (pp 495-497), Schütt relates his experiments attempting to reproduce some of the observations of the alchemists. Similar to Lawrence Principe’s work of a few years ago, Schütt succeeded in showing that various puzzling reports in the alchemical literature really do make sense as a result of laboratory operations. His report, interlaced with precise details of time, place, and witnesses (or lack thereof), is strikingly reminiscent of some of the alchemical narratives themselves.

Schütt wanted to appeal to the broadest possible audience and so kept his scholarly apparatus to the minimum: about 300 footnotes in all (most of them textual rather than source citations), and four pages of bibliography. This poses a disadvantage for scholars of the history of alchemy, which is ameliorated by Schütt’s maintenance of a website associated with his book (<http://www.tu-berlin.de/fb1/alchemie>) that provides a much fuller bibliography.

Still, it must be clearly stated that Schütt has performed an enormous service for the field. His history of alchemy will immediately become the standard general treatment and will go far to raise interest in the field

among general readers. One can only hope that this admirable book will soon be translated into English. *Alan J. Rocke, History of Technology & Science, 1141 East Blvd., Case Western Reserve University, Cleveland, OH 44106-7107*

Arnold O. Beckman: One Hundred Years of Excellence, Arnold Thackray and Minor Myers, Jr. Chemical Heritage Foundation, Philadelphia, PA 2000, 379 pp. \$65.00.

What a grand celebration! A celebration of 100 years of inexhaustible contributions to the advancement of science, technology, education and the quality of life. A true attainment of the American Dream! (And this reviewer knows something about centennials and celebrations, currently experiencing the centennial year of the National Institute of Standards and Technology.)

Chronologically organized, this book is more than just the life and times of Arnold Beckman; it is a chronicle of chemistry in the 20th century. No, it is more than just about chemistry, it covers all of science and engineering. Whom are we kidding? This book is about life in the United States of America!

Beginning with his early years in Illinois, we see an engaging youth filled with curiosity (these days often this same spirit might be viewed as non-PC, if not illegal!). Forced to learn piano by a loving mother, he proved these talents to be invaluable in his summer jaunt across the country via freight train! The reader learns about his first chemistry set, his woodworking skills, his toils on a threshing machine, and much more. This level of intricate and personal detail persists throughout the book, as Dr. Beckman's life is traced from birth through his schools years, military service, and his three "careers" – from professor to entrepreneur to philanthropist. His devotion to his wife and

family is not neglected and is neatly woven into this amazing legend. The pictures and vignettes interspersed throughout the book (a veritable Who's Who in America) bring to life and personalize the warm friendly discourses of the authors. If a picture is worth a thousand words, this book amounts to hundreds of volumes. Truly these authors know Arnold Beckman. It is absolutely impossible, as much as the ardent reader might want, to read this book from cover to cover in one sitting. There is just too much for the mind to absorb. No, one has to allow each section to sink in, sometimes letting days or weeks pass before picking it up again. But one will be drawn back to read more until the end, then beg for more.

And there is more. Tucked neatly before the hardbound back cover is a compact computer disk. This CD is the icing on the cake. The CD adds additional auditory and visual stimulation to the "reader." Without the CD the book is a magnificent work of art. With the CD, the reader becomes all that more familiar with Arnold Beckman. When finished, the reader truly feels as if he knows Arnold Beckman and, given the opportunity, would greet him as a friend.

If there is a downside to the book, it is that it is too big and heavy. This review would have reached you much sooner, could I have carted it along with me through airports. Even as a coffee-table book, I am afraid it is a bit large. So I am concerned that it will be relegated to the bottom shelf of the bookcase (where all the over-sized books are placed) and forgotten. This would be a terrible tragedy.

Let me end with a quote from Dr. Beckman: "I'd like to get young kids interested in science... The young mind is inquisitive enough that you don't have to worry

about scaring up enthusiasm; you simply need to keep them interested and excited about science.” This book should be required reading for all aspiring scientists and engineers (especially chemists), as well as modern history majors—not as a text book, but as a philosophical

and inspirational reading. This book will keep them interested and excited about science. *William F. Koch, National Institute of Standards and Technology, Chemical Science and Technology Laboratory, Gaithersburg, Maryland 20899-8300.*

The Partington Prize

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 two hundred and fifty pounds (£250).

The competition is open to anyone with a scholarly interest in the history of alchemy or chemistry who has not reached 35 years of age by the closing date, December 31, 2002. Scholars from any country may enter for the competition, but entries must be submitted in English, typewritten or wordprocessed and double spaced on one side of the paper. Essays must be fully documented with the conventions used in recent issues of *Ambix*. Essays must not exceed 5000 words in length, excluding references and footnotes. All entries must be submitted with a word count. The prize winning essay will be considered for publication in *Ambix*, but publication cannot be guaranteed.

All entries should be sent to the Hon. Secretary of the Society, J. A. Hudson, Applied Sciences, Anglia Polytechnic University, East Road, Cambridge CB1 1PT, England, with the words 'Partington Prize' written clearly on the envelope. Each entry should contain a separate title page with the author's name, institution, address, and date of birth; this information will not be made available to the judges. Essays (only one from each competitor) must be received no later than December 31, 2002.

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 April 30, 2003, and all essays will be returned to competitors soon after that date.

John Hudson, Hon. Secretary

FUTURE ACS MEETINGS

- April 7-11, **2002**—Orlando, FL
August 18-22, **2002**—Boston, MA
March 23-27, **2003**—New Orleans, LA
September 7-11, **2003**—New York, NY
March 28-April 1, **2004**—Anaheim, CA
August 22-26, **2004**—Philadelphia, PA
March 13-17, **2005**—San Diego, CA
August 28-September 1, **2005**—Washington, DC
March 26-30, **2006**—Atlanta, GA
September 10-14, **2006**—San Francisco, CA
March 25-29, **2007**—Chicago, IL
August 19-23, **2007**—Boston, MA
April 6-10, **2008**—San Antonio, TX
August 17-22, **2008**—Philadelphia, PA
March 22-26-, **2009**—Salt Lake City, UT
August 16-21, **2009**—Washington, DC
March 21-26, **2010**—San Francisco, CA
August 22-27, **2010**—New York, NY
March 27-31, **2011**—Anaheim, CA
August 28-September 1, **2011**—Chicago, IL
March 25-29, **2012**—San Diego, CA
August 19-23, **2012**—Boston, MA

INSTRUCTIONS FOR AUTHORS

Articles of 4-12 pages, typed, double-spaced (excluding references) should be submitted as hard copy (2 copies) and also on diskette, preferably formatted in Word or Wordperfect, to **Editor, Bulletin for the History of Chemistry**, at the University of Michigan. Chemical formulas, to be kept to a minimum, should be computer-generated and printed on separate sheets, with a clear indication of their location in the ms. Authors are encouraged to provide photographs (black and white glossy prints) and drawings (black ink) to enhance the publication. Include a legend for photos, drawings, graphs and credits if appropriate. Diskettes, photographs, and drawings will be returned at the authors' request.

The title of the article should be of reasonable length (up to 15 words); a subtitle may be included if appropriate. Authors should strive to make the title descriptive of the specific scope and content of the paper, bearing in mind that the title will determine entries in the subject index. Subheadings within the paper may be used if authors feel their inclusion will enhance clarity.

Format as found in recent issues of the **Bulletin** should serve as a guide. Authors should consult J. S. Dodd, Ed., *The ACS Style Guide*, American Chemical Society, Washington, DC, 2nd ed., 1997. Format the document throughout with standard 1" left and right margins. Direct quotations, which in the journal will be indented, will be repositioned by the graphic artist.

REFERENCES AND NOTES should appear at the end as part of the main document and not buried as footnotes. References should conform to the format as illustrated below. Standard **Chem. Abstr.** abbreviations are to be used (see CASSI); title of the article is in quotes. Books and journals are italicized, as are volume numbers. The year of publication of periodicals (not books) is boldfaced. Inclusive page numbers are given for an article or partial section of a book. Note the placement of commas and periods. It is imperative to recheck the accuracy of references before submitting the ms. In the text references are identified by Arabic numbers within parentheses—no superscripts.

1. O. T. Benfey, "Dimensional Analysis of Chemical Laws and Theories," *J. Chem. Educ.*, **1957**, 34, 286-288.
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6. J. B. Conant, "Elmer Peter Kohler," *Biogr. Mem. Nat. Acad. Sci. USA*, **1952**, 27, 264-291.

Please provide a short biographical sketch, to be included as **ABOUT THE AUTHOR(S)** at the end of the article.

Readers are encouraged to submit short notes, where appropriate, and letters to the Editor, as well as articles. We would welcome hearing from those who have an interest in refereeing papers and/or preparing book reviews.

BULLETIN FOR THE HISTORY OF CHEMISTRY

William B. Jensen, Founding Editor

Paul R. Jones, Editor
Department of Chemistry
University of Michigan
930 N. University Avenue
Ann Arbor, MI 48109-1055
prjones@umich.edu

Herbert T. Pratt, Ed. Board
23 Colesbery Drive
Penn Acres
New Castle DE 19720-3201

Dr. Peter Ramberg, Ed. Board
Science Division
Truman State University
100 E. Normal
Kirksville, MO 63501
ramberg@truman.edu

HIST OFFICERS, 2001

Richard E. Rice, Chair
PO Box 1090
Florence, MT 59833-1090
charrice@juno.com

Ben B. Chastain, Councilor
538 Hampton Drive
Birmingham, AL 35209
bbchasta@samford.edu

Vera V. Mainz, Sec/Treas
School of Chemical Sciences
Un. Illinois
Urbana, IL 61801
mainzv@aries.scs.uiuc.edu

Mary Virginia Orna, Councilor
16 Hemlock Place
New Rochelle, NY 10805
mvorna@mvorna.cnr.edu

Roger A. Egolf, Program Chair
Dept. of Chemistry
Penn. State. Univ.
Fogelsville, PA 18051
rae4@psu.edu

Albert S. Kirsch
10245 Collins Avenue #12F
Bal harbour, FL 33154
askirsch@compuserve.com

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Dr. Paul R. Jones, Editor

University of Michigan

Department of Chemistry

930 N. University Avenue

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