

10.1000
BU

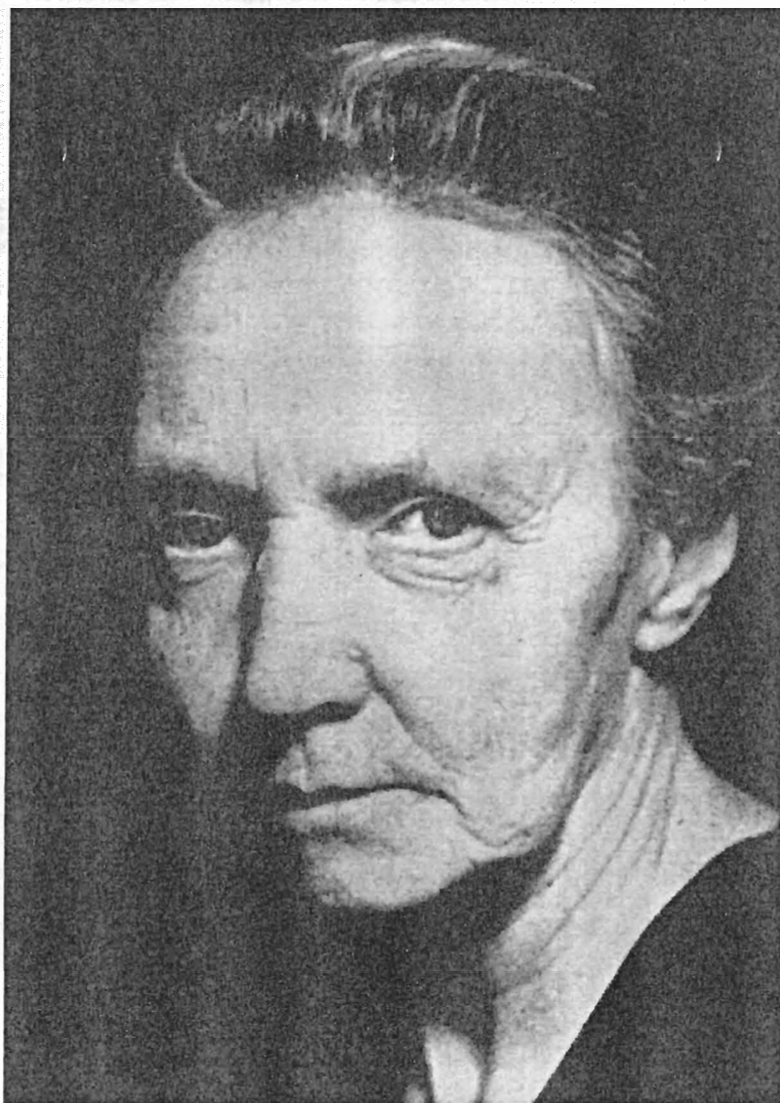
CIA

BULLETIN FOR THE HISTORY OF CHEMISTRY

Division of the History of Chemistry of the American Chemical Society

NUMBER 20

1997



IRENE JOLIOT-CURIE (1897-1956)

CHEMISTRY LIBRARY

DEC 15

UNIVERSITY OF CALIFORNIA
AT URBANA-CHAMPAIGN

**BULLETIN FOR THE HISTORY OF
CHEMISTRY, NO. 20, 1997**

Editor: *Paul R. Jones*
Department of Chemistry
University of Michigan
Ann Arbor, MI 48109-1055

EDITORIAL BOARD:
Herbert T. Pratt
23 Colesbery Drive
Penn Acres
New Castle DE 19720-3201

Dr. Peter Ramberg
Department of Chemistry
Johns Hopkins University
3400 N. Charles Street
Baltimore, MD 21218

CORRESPONDING EDITORS:
Peter J. T. Morris
The Science Museum, London, England
Yasu Furukawa
Tokyo Denki University, Tokyo, Japan

The BULLETIN FOR THE HISTORY OF CHEMISTRY (ISSN 1053-4385) is published by the History of Chemistry Division of the American Chemical Society. All matters relating to manuscripts, book reviews, and letters should be sent to Dr. Paul R. Jones, Editor. Subscription changes, changes of address, and claims for missing issues, as well as new memberships, are handled by the Sec./Treas.

HIST OFFICERS, 1997:
Chair, Harold Goldwhite
Sec./Treas., Vera Mainz
Program Chair, Richard E. Rice
Councilors: Mary Virginia Orna, Ben B. Chastain
Alternate Councilor: Albert S. Kirsch

The Cover...

Irene Joliot-Curie. See Margaret W. Rossiter's article, p. 33

CONTENTS

1994 Dexter Award Address by <i>Frederic L. Holmes</i>	1
Thomas Martin Lowry and the Mixed Multiple Bond by <i>Martin D. Saltzman</i>	10
The Doctoral Theses of Pierre Adolphe Bobierre by <i>John T. Stock</i>	18
Old Chemistries by <i>William D. Williams</i>	24
A Morrill Hall for Chemistry by <i>Paul R. Jones</i>	27
The Young Johannes Wislicenus in America by <i>Paul R. Jones</i>	28
"But She's an Avowed Communist!" L'Affaire Currie at the American Chemical Society by <i>Margaret W. Rossiter</i>	33
Martha Annie Whiteley: Chemist and Editor by <i>Mary R. S. Creese</i>	42
The Hungarian National Museum for Science and Technology by <i>Lyman R. Caswell</i>	46
A Stereochemical Achievement of the First Order: Alfred Werner's Resolution of Cobalt Complexes, 85 Years Later by <i>George B. Kauffman</i>	50
William Draper Harkins: An Early Environmental Chemist in Montana by <i>Richard E. Rice and George B. Kauffman</i>	60
Book Notes	68
Author Index, Numbers 1-20	73
Subject Index, Numbers 1-20	74

"Instructions for Authors:" see Number 15/16

1994 DEXTER AWARD ADDRESS

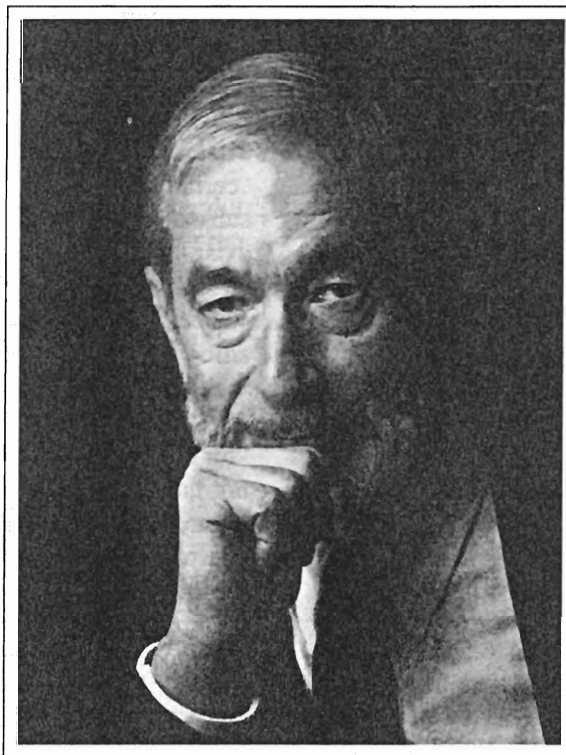
What Was the Chemical Revolution About?

Frederic L. Holmes, Yale University

Two centuries after the death of Antoine Lavoisier, we might expect historians to be able to agree on the nature and the boundaries of the great revolution in chemistry for which he is celebrated. In 1988, however, the editor of a volume entitled "The Chemical Revolution: Essays in Reinterpretation" wrote that "there is at present a striking lack of consensus as to what happened in the Chemical Revolution." Arthur Donovan argued there that intense scholarship during the last four decades had shown the "received" view of the chemical revolution as the overthrow of the phlogiston theory to be "inadequate and misleading," but had not yet supplied a "commanding replacement" for it (1). Chemists who have never doubted that the chemical revolution was about the replacement of the phlogiston theory by Lavoisier's oxygen theory, may well wonder if Donovan's statement is an admission that historians are unable to settle anything. Are they victims of their own revisionist tendencies, perpetually reopening questions that ought to have been resolved long ago, if they only possessed the rigorous methods that scientists do? Or does the collapse of a formerly accepted story represent progress in the historical un-

derstanding of a complex set of events played out a little more than two centuries ago?

Some of the proposals of the last three decades for redefining the chemical revolution offer new perspectives on the event and on its relations to the chemistry that preceded or followed it. Some of the changes in outlook about this particular revolution have been forced by broader recent debates about the existence of revolutions in science in general, especially in the aftermath of Thomas Kuhn's enormously influential *Structure of Scientific Revolutions*. Some of the disagreements that still engage historians, however, are continuations of debates that have existed from the time of the chemical revolution itself. They involve perennial issues about continuity and discontinuity that surround, in one form or another, the interpretation of any radical change in science, or in society at large. In the case of the chemical revolution, this debate often surfaces as one or another variation on the question, was chemistry already a science before Lavoisier's "reforms," or did it become a science only through his revolution?



F. L. Holmes

Ambiguity over this question began with the leader of the revolution himself. Sometimes Lavoisier characterized earlier chemistry as so fraught with error that it would be preferable to begin anew; but on other occasions he viewed himself as continuing in directions followed by his predecessors, only carrying their analyses a step further. During the nineteenth century the question was heavily influenced by the attitudes of chemists toward the heritage that they felt they had received from Lavoisier. Because his chemical system had been perceived, by both supporters and opponents, as a "French" chemistry, nationalistic rivalries played a strong part also in these judgments (2).

The most famous of these opinions was expressed in 1869 by the French chemist Adolph Wurtz, in the introduction to his *History of Chemical Doctrines since Lavoisier* (3):

Chemistry is a French science: it was constituted by Lavoisier, of immortal memory. For centuries chemistry was only a repository of obscure, often misleading recipes used by alchemists, and later by iatrochemists. A great mind, Georg Ernst Stahl, had tried in vain, at the beginning of the 18th century, to give it a scientific foundation. His system could not withstand the test of the facts and the powerful criticism of Lavoisier. The work of Lavoisier is complex: he was both the author of a new theory, and the creator of the true method in chemistry.

In his chapter on Lavoisier, Wurtz described knowledgeably both the phlogiston theory and the main features of Lavoisier's "new doctrine (4)". For contemporaries, however, these details were overpowered by his emotionally charged initial declaration that Lavoisier had single-handedly constructed the science of chemistry, and that its origin was, therefore, French. The corollary, implied in the declaration itself, but also made explicit in the rest of the that I have included in the preceding quotation, was that chemistry before Lavoisier was a rudimentary, pre-scientific affair; that the only previous attempt to make it scientific, Stahl's phlogiston theory, had been a failure.

To some German chemists, Wurtz's claim appeared outrageous. Several of them wrote historical rebuttals, emphasizing continuities between the doctrines of Stahl and later chemistry, and diminishing the contributions of Lavoisier (5). In the midst of these polemics the great German historian of chemistry, Hermann Kopp, published a one volume history of *The Development of Chemistry in Modern Times*, in which he assessed judiciously, and with remarkable freedom from national bias, the question (6):

How far had chemistry advanced, up to ... the time at which Lavoisier acted so powerfully on its further development and led it onto the track pursued continually since then? Had chemistry at that time already secured the claim to be regarded as a science, or did it, according to what it strove for and had achieved, not yet merit that designation? Did what a later time brought forth as the so-called modern chemistry come from the further development of what was already known and scientifically integrated, or does chemistry as a science really date from Lavoisier? Very different answers have been given to this question.

In treating this question himself, Kopp noted first that by the end of what he called the era of the "domination of the phlogiston theory," all chemists were in agreement that chemistry was "the doctrine of the composition of bodies: how they are composed, and how they become composed (7)". This criterion was especially important for Kopp, because, as the quoted statement suggests, he defined a science in general according to its goals, not merely its completed achievements (8). The knowledge already acquired by the end of that era was, however, impressive enough. After enumerating the metals, earths, salts, and acids newly identified during the last decades of this era, Kopp commented on "how rich in discoveries of particular substances the era preceding the overthrow of the phlogiston theory was." These discoveries relied on a general understanding that "certain substances are contained in other composed substances as constituents, in which they continue to exist. Such constituents were not merely hypothetical elements, but actually producible [*darstellbare*] substances." Most of these pre-Lavoisier views of composition were afterward translated into the language and viewpoint of the chemistry of Lavoisier (9).

This "wealth of knowledge" was, Kopp conceded, "for the most part empirical knowledge," but the era did not lack "more general perspectives" capable of leading to "more comprehensive, important views." As one approaches the most basic levels of chemical thought of the time, however, one encounters in greater degree, opinions that were "erroneous." In particular, "with respect to the elementary composition of bodies, and most especially their ultimate constituents, erroneous representations dominated." He reviewed the four element theory, a commonplace of chemical textbooks near the end of the phlogiston era. This theory represented a revival of the Aristotelian elements of 2000 years earlier. Unlike some more recent historians, Kopp emphasized that under the names of these elements 18th century chemists thought of very different entities than the Greek

philosophers had. "The meaning of these names was adapted, above all else, to what later chemical investigations appeared to reveal (10)".

Why, in spite of the wealth of knowledge about other aspects of composition that he credited to pre-Lavoisier chemistry, did Kopp characterize the era by precisely that theoretical system which failed to survive the chemical revolution? One answer that he himself gave was that, "For a long time chemistry had viewed its chief task as to understand and explain the action of fire on various bodies (11)". Kopp understood thoroughly both the strengths and limitations of the phlogiston theory. On the one hand, "In the recognition of which substances are intermediate between others, and in which order, chemistry was considerably advanced under the influence of the phlogiston theory (12)". Much of the knowledge organized in this way "was directly translated into the new system." By the end of the era, however, efforts to hold to the theory in the face of changing circumstances had already led, before Lavoisier intervened, to "considerable divergence, and rapid changes of views. ... The recognition of one error maintained until then led one only to tumble into a new error."

Despite these negative notes, Kopp had no doubt that the answer to his question, "whether chemistry, already conceived then as a science, was pursued scientifically," was "Yes." Much of the knowledge acquired before Lavoisier provided foundations for the system that Lavoisier formulated. Chemistry as a science did not date from Lavoisier, even if he had proven that the previous answers to "fundamental questions" had been wrong, and even if he had created a new method of investigation. The problems of chemistry remained the same, but the methods to solve these problems were "perfected by Lavoisier", and a new doctrine of the composition of bodies and their most important processes introduced. "A transformation of views was caused by Lavoisier within an existing science (13)".

Hermann Kopp had studied 18th century chemistry more thoroughly than any historian before him had, and knew more about the subject than most historians since him. He was able to conclude that pre-Lavoisian chemistry was a genuine science, even though he distinguished "correct" knowledge from "error" in typical 19th century fashion. Current historians of science believe that past scientific ideas and knowledge should be judged according to the standards of their own time, not that of later eras. It is ironic that they seem, nevertheless, to have more reservations than Kopp had about

the scientific character of chemistry before the chemical revolution.

There are, I believe, a number of explanations for why the question of whether chemistry before Lavoisier was a science has remained an unresolved issue. One reason is that historians of science, like scientists themselves, often forget their own past. Until well into the twentieth century, Hermann Kopp was known among historians of chemistry as a towering scholar in their field. In 1932 the journal *Archeion* published an appreciation of Kopp as a historian, by Edmund von Lippmann, which concluded that "Until the present day no younger scholar has combined the talent, the knowledge (of the subject and the languages) and the hard work to such a degree" as Kopp had done to produce his massive studies in the history of chemistry (14). The generations of historians of science who have come of age during the 1950s and later, however, tend to treat the older histories written by scientists about their own fields as pre-professional and outmoded. In the recent *Osiris* volume on reinterpretations of the chemical revolution, to which I alluded at the beginning of my talk, I found only one reference to Kopp, significantly by a Dutch historian who has been in the field longer than the rest of the contributors to that volume (15). I confess that I, too, ignored Kopp until recently, and now realize that some of what I have written about 18th century chemistry that I thought to be novel, he had already discussed. Undoubtedly those participating in debates about the meaning of the chemical revolution today would not fully agree with Kopp's position, but current discussions have, nevertheless, been impoverished by the loss of insights that he brought to the field more than a century ago.

Another reason that the same questions continue to be reopened is that the criteria by which they are answered have changed with changes in the structure and aims of the history of science. During the post war period, as the field was moving away from the model of the scientist-historian, and toward attachments with professional historians, there was a strong tendency to view the history of science as a branch of intellectual history. One consequence of such alliances was a focus, never so exclusive as later portrayed, but nevertheless strong enough, on scientific ideas. From this perspective, much of the work of eighteenth century chemists that someone like Kopp could treat with empathy, appeared barren. Moreover, the pre-Lavoisier chemists were very difficult to read, because they wrote in a language that had been made opaque by the reformed nomenclature which emerged during the chemical revolution. Histo-

rians began during the 1960s to define 18th century chemistry less in terms of the activities of practicing chemists than of the views of influential figures of the period who wrote about chemistry, or topics related to chemistry. A good example was a pioneering paper written in 1963 by Maurice Crosland. Having been requested to "discuss the development of chemistry in the eighteenth century with particular reference to the history of ideas," Crosland found it "confining" to restrict his "attention to men who might reasonably be called chemists and ignore others whose chief interests were in other branches of science or even outside science altogether." Among the ideas that appeared to Crosland to have affected the development of chemistry most significantly were the atomic view of matter, elective attractions, schemes of classification, the "physical approach to chemistry," and the phlogiston theory. Only the latter emanated primarily from chemists. The "influence of Newton" played a predominant role, and the attitudes of the philosophes toward chemistry became more visible than the views of chemists whose careers took place in the laboratory (16).

Within the framework of this genre of history of scientific thought, the question of whether chemistry before Lavoisier was "still more of an art than a science" was displaced from criteria such as Kopp had used, to those resting on the status of an intellectual structure largely defined by outsiders to, or dilettantes within, chemistry itself. A similar orientation can be seen in other prominent historical studies of this period, including, for example, Arnold Thackray's monograph *Atoms and Powers*, published in 1970, whose subtitle was *An Essay on Newtonian Matter-Theory and the Development of Chemistry* (17). By such standards it was easy to find 18th century chemistry lacking in the kind of theoretical structure one would expect of a "modern science." The wealth of discoveries that had so impressed Kopp were readily passed over as merely empirical.

In 1955 the French historian of chemistry Maurice Daumas pointed out that 18th century physicists and mathematicians believed that chemistry had stagnated in outmoded doctrines. It could only be saved, according to them, by applying mathematical methods and by explaining chemical reactions by extending the principle of universal attraction to the molecular scale. They also condemned chemical conceptions of heat as a substance. But what they had to substitute was of no use to chemists. "The chemists remained true to themselves. Practitioners above all else, they kept their faith in the only guide that had led them so far, that of experiment (18)." Daumas's view can serve also as a warning to histori-

ans not to lean too heavily on the writings of Newtonians, natural historians such as Buffon, or the philosophes, to characterize the chemistry of that time. Relying on the testimony of outsiders and amateurs to the field, historians risk missing the internal dynamic of what was taking place within its laboratories, in the Academies in which the leading chemists of the age participated, and in the specialized literature within which its advances were recorded.

Another displacement of the question, when did chemistry become a science, began in the 1970s with the emergence of a vigorous social history of science. Within this framework historians looked neither for evidences of the progressive acquisition of empirical knowledge, nor for the advent of a strong theoretical structure, but for the formation of a "discipline-oriented community." In the best known study of this type, Karl Hufbauer found that by the 1770s a national community had coalesced in Germany. Its signs were the number of active chemists holding institutional positions, and regular communication among them, particularly with the founding of the first specialized chemical journal (19).

One of the main obstacles to reaching a consensus about what happened in the chemical revolution is a persistent conflation of two images of Lavoisier. From his own time he was recognized, by followers and opponents alike, as the leader of a great revolution in chemistry. During the nineteenth century he came to be viewed by many also as the "founder" of modern chemistry, an image conveyed most vividly in the patriotic hues of Adolph Wurtz. These ought to have been separable viewpoints. The chemical revolution was a historical event, bounded in time, whereas the foundation of modern chemistry is a far more complex phenomenon, dependent on variable judgments about the essential features of modern science in general, of chemistry in particular, and of its differences from what had preceded it. The distinction has, however, attracted little notice.

Nineteenth century writers sometimes used the term "revolution," as Lavoisier himself did, to describe what he had introduced into chemistry, but did not give priority to the term. Kopp, for example, headed his chapter on that topic "the reform of chemistry by Lavoisier (20)," and Wurtz talked about the "triumph" of the "system of Lavoisier" over that of Stahl (21). Twentieth century historians have fixed on the phrase "chemical revolution" and have, in spite of the questionable status of all "founder" myths, continued to treat it as the defining event in the formation of modern chemistry. Why has that happened?

One explanation I would propose is that the chemical revolution was fitted into a more encompassing story of how modern science in general emerged from what has come to be known as "The scientific revolution." David Lindberg has recently surveyed the historical process through which the belief of seventeenth century scientists, and of spokesmen for science such as Francis Bacon, that they were making a sharp break from the past, evolved into the twentieth century notion of a great scientific revolution. The characterization of this revolution was in part a rebuttal to the assertions of Pierre Duhem, who had found evidence of continuities between Galilean physics and ideas of the late middle ages, and claimed that modern science had evolved continuously out of medieval science. A culmination of the reassertion of discontinuity was expressed in Herbert Butterfield's *The Origins of Modern Science* (22), which began with the famous statement that the scientific revolution "outshines everything since the rise of Christianity and reduces the Renaissance and Reformation to the rank of mere episodes." According to Butterfield, that revolution was "the real origin both of the modern world and of the modern mentality (23)." Encompassed within this overarching event was the formation of the modern sciences.

To account for so much, Butterfield had to enlarge the boundaries of a development "popularly associated with the sixteenth and seventeenth centuries," to extend from 1300 to 1800 (24). The latter date enabled him to include within its scope what he called "The postponed scientific revolution in chemistry."

"It has often been a matter of surprise," he wrote, "that the emergence of modern chemistry should come at so late a stage in the story of scientific progress." He attributed much of the delay to the "hurdle" raised in the eighteenth century by the "interposition of the phlogistic theory." After recounting the canonical story of the events, from Joseph Black to Lavoisier, which finally permitted chemists to get around this "block" to "scientific progress," Butterfield concluded, "the chemical revolution which he [Lavoisier] set out to achieve was incorporated in the new terminology as well as in a new treatise on chemistry. ... Over a broad front, therefore, he made good his victory, so that he stands as the founder of the modern science (25)."

First published in 1949, and in a second edition in 1957, Butterfield's little volume provided an accessible overview of the momentous events which shaped the emergence of "modern science" in just that period in which the history of science was coalescing, particularly in the United States, as an academic discipline. It

appears to have played a disproportionate role in shaping the early historical perceptions of the young scholars who began entering the field at that time, and despite the extensive new scholarship in each of the areas it covers, it is still used in introductory courses. It is reasonable to surmise that Butterfield's account of the chemical revolution has helped to condition both the negative image of pre-Lavoisier chemistry that is still prominent in the field, and the close association still assumed between victory in the chemical revolution and the foundation of modern chemistry.

The consequences of the retrospective expansion of the meaning of the chemical revolution to somehow cover the foundation of modern chemistry as a whole can be plainly seen in recent revisionist reinterpretations of the revolution. There is not time here to enumerate the specific redefinitions of its scope and themes that have been proposed in the past few years, but I think that Donovan summarized their general thrust accurately when he wrote that they make it (26):

clear that focusing on the overthrow of the phlogiston theory provides too narrow a base for investigations of any of the major developments associated with the Chemical Revolution, investigations such as those that attempt to reconstruct Lavoisier's research career in chemistry, to describe the theoretical revolution he effected, to explain the ways in which his new theories were received, and to construct a more comprehensive account of the founding of modern chemistry.

The pitfall here is that to account for all of these developments, in particular the founding of modern chemistry, as aspects of the chemical revolution, is to impose a crushing interpretative overburden on the meaning of the historical event that was from the beginning seen by contemporaries as a "revolution in chemistry." What has happened is that historians have inverted the part-whole relationship that should apply. The founding of modern chemistry is not contained within the chemical revolution, but is a much larger story, in which the revolution plays its part.

Once we free the chemical revolution from the burden of explaining too much, then, I believe it becomes quite clear that the older view that it centered on the overthrow of the phlogiston theory was a realistic one. I have elsewhere argued that Lavoisier and his most strategic convert, Guyton de Morveau clearly had that in mind when they invoked the word revolution to describe what had transpired (27). If, by revolution we mean a radical break with the past, a struggle to replace authoritative positions with something new, and a victory in

this struggle, then there can be no doubt that the contest was between the phlogiston theory and the theory of combustion with which Lavoisier sought to supplant it. All of the prominent opponents of Lavoisier's new chemical system resisted it as defenders of phlogiston. It is no accident that they named Lavoisier and his followers "antiphlogistonists."

Historians have long maintained that Lavoisier also overthrew the prevailing views about chemical elements. In the famous preliminary discourse of his *Traité élémentaire de chimie*, Lavoisier dismissed "the tendency to see all natural bodies composed of three or four elements," as a metaphysical prejudice dating from the Greek philosophers. He replaced such ideas with his pragmatic definition of elements as "all those substances that we have not yet been able to decompose by any means (28)." It is true that in the period just before Lavoisier entered the field, there had been a revival of the four element theory, but, as Kopp recognized, its basis was much different from the original Greek foundations. Moreover, it was, in the eyes of the eighteenth century chemists who favored it, only a tentative scheme, subject to the same criterion of validity that Lavoisier later asserted. In the article on the "elements" in his *Dictionnaire de chimie*, Pierre Joseph Macquer named in 1766 "the purest fire, air, water, and earth" as elements, because "all the efforts of the art" had so far been "insufficient to decompose them." But he quickly added that "It is very possible that these substances, although reputed to be very simple, are not, that they are even very composed, that they result from the union of many other, simpler substances (29)." These "reputed" elements did disappear in the chemical revolution. Because they had never been firmly embedded within the operating structure of pre-Lavoisier chemistry, as phlogiston had been, however, they were not defended with the tenacity that phlogiston was, and their demise was more a by-product of, than a central issue of the chemical revolution.

I said earlier that I have come belatedly to the realization that there are strong resemblances between the way I have recently written about eighteenth century chemistry and what Hermann Kopp wrote about it long ago. To summarize my view of the chemical revolution I could well adopt his statement that, "A transformation of views was caused by Lavoisier within an existing science." There is, however, a distinction between Kopp's treatment and my position that I believe is significant, the exploration of which can reveal further insights about the nature of the pre-Lavoisier era and recast the boundaries of the chemical revolution itself.

The wealth of discoveries of acids, bases, earths, metals and neutral salts about which Kopp wrote with such deep familiarity appeared to him to constitute "empirical" knowledge. For the "broader views" of the era he turned to eighteenth century ideas about the elements, affinities, and above all, the phlogiston theory. I have argued that the systematic experimental investigations through which the discoveries Kopp describes were made were guided by a simple, but powerful theoretical structure.

The emergence of the concept of what was known at the beginning of the century as the "middle salt," and by the middle of the century as the neutral salt, had profound consequences for the chemistry of that era, and for chemistry ever since then. The idea of the neutral salt was, in part, an empirical generalization growing out of the gradual realization during the seventeenth century that acids could be combined with the various known alkalis, metals, or calcareous earth (the only "earth" recognized at the time to have this property) to produce salts which did not display the properties of these constituents, but from which the constituents could be recovered with their original properties restored (30).

The reason that this generalization was more than empirical, however, is that it required chemists to conceive of the two constituents of the salt as present within it, even though the properties by which the constituents were defined and identified were not present. That was not only counter-intuitive, but in conflict with the general theories of composition prevailing in the seventeenth century. The four element theory of that period, which was, unlike its eighteenth century revival, directly traceable to its Aristotelian roots, the Paracelsian salt, sulfur, mercury triad, and the various compromise systems derived from them during that century, all attributed the generic properties of tangible substances—their fluidity or solidity, volatility or non-volatility, combustibility or non-combustibility, their sharpness or insipidness of taste and other qualities—to one or more of the "principles" which entered into them.

As is well known, this general conception persisted long after the seventeenth century schemes in which it had been embodied had receded into the past. Its most prominent manifestation in the eighteenth century was the principle of flammability, phlogiston. But Stahl and other chemists of the time looked for others, such as that of acidity, under the guidance of the deeply embedded idea that every prominent property common to a class of substances should ultimately be traceable to one of its generic principles (31).

Some historians argue that the chemical revolution replaced this traditional view of composition with a

“combinatorial” one, in which the levels of composition were defined by the tangible substances that could be separated by successive layers of analysis, regardless of the relationship of the properties of the constituents and those of the bodies composed of them. Maurice Daumas emphasized, on the other hand, the continuity between Lavoisier and his predecessors in this respect. “The chemistry of principles” with which Lavoisier came into contact in his youth was represented in new forms in his “principle of acidity,” his presumption that a corresponding “principle of alkalinity” would be found, and in caloric (32).

I would like to argue that there was also a continuity across the revolutionary rupture that was the inverse of this one - that is, that there was a strong precedent for the “combinatorial” view of composition thought to be introduced by Lavoisier. This view was expressed with particular clarity in the article “Mixte et Mixtion” in the influential French *Encyclopédie* in 1765 (33):

An essential characteristic of the chemical *mixt*, at least in its most perfect form, is that the particular properties of each of the principles that unite in the formation of the *mixt* perish, or are at least so far masked that it is as if they did not exist at all, and the *mixt* is truly a new substance, specified by its own properties, and different from those of each of its principles.

The prime support for this generalization came from the observed relations between acids, bases, and the neutral salts that they formed. I would submit, however, that this was no simple generalization of laboratory experience. It was a hard-won *understanding* of that experience. Seventeenth century chemists had generally believed that the acid and alkali which reacted together so energetically destroyed each other in the process, because those very distinctive properties of the reactants could no longer be detected in the product. In the early eighteenth century, chemists in the French Academy of Sciences carried on a heated debate about whether the iron extracted from plant matter could have been performed in it, or created in the process. The main obstacle to their acceptance of the first option was their difficulty in conceiving how the iron could be present without imparting magnetic properties to the plant.

In the face of such a mental barrier, the most forceful countervailing force in eighteenth century chemistry was the success of the chemistry of salts. As the number of known salts, acids, and bases grew, research guided by this compositional framework flourished on an ever accelerating scale. It was the principal source of that great “wealth of discoveries” that so impressed

Hermann Kopp. Lavoisier had already recognized as much in 1778, when he wrote that “the theory of neutral salts, which has fixed the attention of chemists for more than a century, ... is so perfected today that one can regard it as the most complete part of chemistry (34).”

Viewing the chemistry of salts, not as Kopp did, as empirical knowledge alone, but as Lavoisier had seen it, as a domain of chemistry built around a *theory* of neutral salts, enables us to define the old question of continuity and discontinuity in the chemical revolution in a new way. Within a well-defined problem domain, defined by Lavoisier as “combustion, the calcination of metals, and in general all the operations in which there is an absorption and release of air (35),” he came into sharp confrontation with those who believed that the phlogiston theory adequately explained these phenomena. He attacked the defenders of the prevailing view, overwhelmed them with his reasoning, his experimental evidence, his effective rhetoric, and an organized campaign to win over those who resisted. He believed that his revolution was nearly completed by 1790, because chemists all over Europe were “gradually dropping the doctrine of Stahl (36).”

When Lavoisier looked at the chemistry of salts, however, he did not see himself in opposition to his predecessors, but as the follower of a long tradition. “In accordance with the state in which the science of chemistry has been transmitted to us,” he wrote in 1778 (37):

It remains for us to do for the principles comprising neutral salts what the chemists who were our predecessors did for the neutral salts themselves: to attack the acids and bases, and to push back by another degree the limits of this type of chemical analysis.

That is just what he achieved. It was through his oxygen theory of combustion that the acids and bases were found to be composed each of a particular constituent combined with oxygen. Where there had formerly been established a single level of composition in terms of what Kopp called “presentable” component substances, there were now two successive levels understood in the same manner. The whole of the second part of Lavoisier’s *Traité* is testimony to the ease with which the simpler levels of composition that he had defined could be integrated with the previous knowledge and theory of neutral salts.

My version of Kopp’s statement, therefore implies that the revolution Lavoisier led took place not only within an established science, but in a bounded domain within a broader science. Kopp viewed the phlogiston

theory as the dominant theoretical feature of that established science because of the great importance traditionally attached to explanations for the action of fire on substances. I view that concern as less dominant by the second half of the eighteenth century, when distillation and other analyses depending primarily on heat no longer dominated the operations of the chemical laboratory to the extent they once had. The chemistry of salts relied more and more on solution and precipitation, differential solubilities, purification and identification through crystallization, and the expanding capacity to define a specific salt, acid, or base through a broad combination of physical and chemical properties. In short, chemistry was no longer a narrow, monolithic set of concepts and procedures, but a complex science comprising an array of increasingly specialized knowledge and problems. Lavoisier clearly recognized that state of the field when he referred to the various "parts" of chemistry.

To reaffirm that the chemical revolution was about the overthrow of the phlogiston theory is not to imply that the changes Lavoisier introduced into chemistry were limited to that domain. The list of his achievements that had major impacts on the future development of the various subfields of chemistry which emerged during the nineteenth century is astonishingly long. They range from his thoroughgoing quantitative style of experimentation and the introduction of a whole new level of complexity of instruments and apparatus, to calorimetry, the elementary analysis of organic substances, and a theory of fermentation which provided the first description of a chemical process as a balanced equation, to the list of pragmatically defined elements from which all subsequent tables of the elements have evolved, and the reformed nomenclature.

Some of these achievements changed the theory and practice of chemistry rapidly, others took many years to exert their full effects. The more dramatic effects might be called "revolutionary," if we follow the popular tendency to describe highly visible, rapid changes of any kind as revolutions. To do so as historians, however, only blunts our use of the language and diminishes the precision of our interpretations. It is useful to distinguish scientific revolutions which require ruptures involving radical change and the overthrow of something essential to the pre-revolutionary state, from other kinds of major scientific transformations. Lavoisier caused a revolution focused on the phlogiston theory. He produced or laid the ground work for many additional transformations in chemistry. But some well established parts of chemistry, in which there was rapid progress contemporary with Lavoisier, but to which he contributed little, continued

to develop in the same directions in which they had been heading when he arrived on the scene. They too played their parts in the founding of modern chemistry.

REFERENCES AND NOTES

1. A. Donovan, "Introduction," *Osiris*, **1988**, *4*, 5-7.
2. For discussion of this period, see B. Bensaude-Vincent, *Lavoisier: Mémoires d'une révolution*, Flammarion, Paris, 1993, pp. 344-417.
3. A. Wurtz, *Histoire des doctrines chimiques*, Hachette, Paris, 1869, pp. 1-2.
4. *Ibid.*, pp. 7-34.
5. Ref. 2, pp. 393-407; See also H. Metzger, "Le role de Lavoisier dans l'histoire des sciences," *Archeion*, **1932**, *14*, 33-38.
6. H. Kopp, *Die Entwicklung der Chemie in der neueren Zeit*, Oldenbourg, Munich, 1873, p. 85.
7. *Ibid.*, p. 92.
8. See H. Kopp, *Geschichte der Chemie*, Vieweg, Braunschweig, 1843, Vol. 1, pp. 3-8.
9. Ref. 6, pp. 91-104.
10. *Ibid.*, pp. 108-118.
11. *Ibid.*, p. 117.
12. *Ibid.*, p. 124.
13. *Ibid.*, pp. 132-133.
14. E. O. von Lippmann, "Hermann Kopp als Historiker," *Archeion*, **1932**, *14*, 4.
15. H. A. M. Snelders, "The New Chemistry in the Netherlands," *Osiris*, **1988**, *4*, 126.
16. M. Crosland, "The Development of Chemistry in the Eighteenth Century," in T. Besterman, Ed., *Studies on Voltaire and the Eighteenth Century*, Institut et Musee Voltaire, Geneva, 1963, Vol., 24, pp. 369-411.
17. A. Thackray, *Atoms and Powers: An Essay on Newtonian Matter-Theory and the Development of Chemistry*, Harvard Un. Press., Cambridge, MA, 1970.
18. M. Dumas, *Lavoisier: Théoricien et expérimentateur*, Presses Universitaires, Paris, 1955, pp. 159-161.
19. K. Hufbauer, *The Formation of the German Chemical Community*, Un. Cal. Press, Berkeley, CA, 1982.
20. Ref. 6, p. 134.
21. Ref. 3, pp. 1-34.
22. D. C. Lindberg, "Conceptions of the Scientific Revolution from Bacon to Butterfield: a Preliminary Sketch," in D. C. Lindberg and R. S. Westman, Ed., *Reappraisals of the Scientific Revolution*, Cambridge Un. Press, Cambridge, 1990, pp. 1-26.
23. H. Butterfield, *The Origins of Modern Science: 1300-1800*, Clarke, Irwin, Toronto, 2nd ed., rev., 1968, pp. vii-viii.
24. *Ibid.*, p. vii.
25. *Ibid.*, pp. 191-209.
26. Ref. 1, p. 6.
27. F. L. Holmes, "The Boundaries of Lavoisier's Chemical

- Revolution," *Revue d'histoire des sciences*, 1995, 48, 9-48.
28. A. L. Lavoisier, *Traité élémentaire de chimie*, Cuchet, Paris, 1789, Vol. 1, pp. xv-xvii.
29. [P. J. Macquer], *Dictionnaire de chymie*, Lacombe, Paris, 1766, Vol. 1, p. 399.
30. F. L. Holmes, *Eighteenth Century Chemistry as an Investigative Enterprise*, Office of History of Science and Technology, Berkeley, CA, 1989, pp.33-35.
31. For a recent review of this topic, see J. G. McEvoy, "Continuity and Discontinuity in the Chemical Revolution," *Osiris*, 1988, 4, 199-205.
32. Ref. 18, pp. 158-163.
33. "Mixte et Mixtion," *Encyclopédie, ou dictionnaire raisonné des sciences, des arts, et des métiers*, Neufchatel, 1765, Vol. 10, pp. 586-587.
34. A. L. Lavoisier, "Considérations Général sur la nature des acides et sur les principes dont ils sont composés," *Memoires de l'Académie des Sciences*, 1777, 535.
35. A. L. Lavoisier, "Reflexions sur la phlogistique," *Oeuvres*, p. 623.
36. A. L. Lavoisier to B. Franklin, translated in M. Beretta, *The Enlightenment of Matter: the Definition of Chemistry from Agricola to Lavoisier*, Science History Publ., Canton, MA, 1993, p. 258.
37. Ref. 34, p. 535.

ABOUT THE AUTHOR

Frederic L. Holmes, Avalon Professor at Yale University, is also Chairman of the Section of the History of Medicine, School of Medicine at Yale, PO Box 208015, New Haven, CT 06520-8015. The Dexter Award recognized Professor Holmes' extensive research and publication record in science and medicine in the eighteenth and nineteenth centuries.

COMING EVENTS

214th ACS Meeting, Las Vegas, NV, September 7-11, 1997

Fifth Chemical Congress of North America, Cancun, Mexico, November 11-15, 1997

215th ACS Meeting, Dallas, TX, March 29, April 2, 1998

International Symposium, "The Science and Technology of Rudolfinian Time," National Technical Museum, Prague, Czech Republic, August 24-28, 1997. For details contact: Dr. Jaroslav Foltá, "R-11" Nat. Tech. Museum, Kostelni 42, 170 78 Prague 7. iso@ntm.anet.cz; FAX: ++ 420 2 379151.

50th Anniversary Conference, British Society for History of Science, "The History of Science as Public Culture?" September 9-11, 1997, University of Leeds. For details contact Dr. Frank A. J. L. James, Royal Inst. Centre for the History of Science & Technology, Royal Institution, 21 Albemarle Street, London, W1X 4BS. fjames@ri.ac.uk; FAX: ++ 0171 629 3569.

7th Italian Congress in History of Chemistry, L'Aquila University, Abruzzo, Italy: October 8-11, 1997. For details contact: Prof. Giorgio Corradini, Dipartimento di Chimica, Ingegneria Chimica e Materiali, Montelucio di Roio, L'Aquila, Italia. FAX: + 39 862 434203.

THOMAS MARTIN LOWRY AND THE MIXED MULTIPLE BOND

Martin D. Saltzman, Providence College

Thomas Martin Lowry (1874-1936) is a name familiar to all chemists primarily for his theory of acids and bases. He also discovered, named and elucidated the mechanism of mutarotation as well as being a pioneer in the study of optical rotatory dispersion. Deeply interested in the history of chemistry he published a widely used text *Historical Introduction to Chemistry* which went through three editions (1). Trained originally as an organic chemist his interest gravitated to physical chemistry. Lowry was one of the pioneers in the developing hybrid discipline of physical organic chemistry. With his student William Alec Waters (1903-1985) he produced in 1935 the first book in this area, *Physical Aspects of Organic Chemistry*. Others by Herbert Ben Watson (1894-1975), Louis B. Hammett (1894-1987), and Gerald Branch (1896-1954) and Melvin Calvin (1911-1997) followed in 1937, 1940, and 1941, respectively. One of Lowry's most significant contributions to physical organic chemistry was his early advocacy of the use of Lewis-Langmuir theory in the interpretation of organic reactions (2). This paper will review and assess the significance of his role in this formative period of physical organic chemistry.

Lowry was born October 26, 1874, in Bradford, England. His father was a Methodist minister as had been generations of Lowrys before. His life was dominated by a pious devotion to his religion; and his biographers Allsop and Waters (3) state that his scientific career was always guided by his devotion to his religion. A clue as to why Lowry would gravitate to physical organic chemistry comes from his academic training. In 1893 he entered the Central Technical College of the City and Guilds Institute in London to embark upon his chemical training. There he came under the

influence of Henry Edward Armstrong (1848-1937), an iconoclastic teacher and researcher. Armstrong, an organic chemist, is best remembered for his centric formula for benzene; but he also did significant work on



T. M. Lowry

the stereochemistry of natural products and chemical crystallography. Armstrong was one of the first to cross the traditional boundaries between physical and organic chemistry. On his graduation in 1896 Lowry became Armstrong's assistant and remained so until 1913. During this period he obtained his D.Sc. degree (1899) and

taught himself physical chemistry (4). He held a series of teaching positions in this period which culminated in 1912 in a Lectureship in Chemistry at the Guy's Hospital Medical School. His reputation as an innovative researcher led to his election as a Fellow of the Royal Society (1914) and his promotion to the rank of Professor. In 1920 the newly created physical chemistry chair at Cambridge was offered to Lowry, and he spent the rest of his life at Cambridge. In summarizing his work his biographers have written (3):

Lowry gained scientific eminence as a physical chemist, and for the last twenty years of his life held one of the senior professorships of physical chemistry in England; but at the outset of his scientific career he was an experimental organic chemist busily engaged in elementary teaching. His early work in both these directions deeply influenced his outlook throughout his whole life, for his approach to physical chemistry was always that of the experimenter rather than that of the theorist, possibly because his mathematical knowledge was limited, though in this direction he was enthusiastic in stimulating the efforts of collaborators.

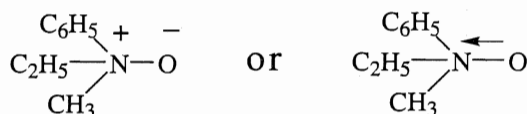
Lowry became one of the most enthusiastic advocates of the Lewis-Langmuir theory. His original contributions to the field were to occur in the period only from 1923-1925, but his impact was to be long lasting.

The characterization of the electron by J. J. Thomson of Cambridge University (1856-1940) in 1897, as a fundamental sub-atomic particle in 1897 was followed by initial speculations on the role of the electron in chemical bonding. Thomson's first paper on the subject appeared in 1904, but it seemed to have had little impact on the British chemical community. Thomson's conception of bonding was a modern revival of Berzelius's dualism. His ideas were enthusiastically adopted by a group of American organic chemists who attempted to explain chemical properties as well as reaction mechanism based upon the Thomson model. This American effort was doomed to failure and would in turn in the United States retard the adoption of the newer ideas developed by the American chemists G. N. Lewis (1875-1946 in 1916 and Irving Langmuir (1881-1957) in 1919. American organic chemists with a few exceptions such as James B. Conant (1893-1978) and Howard Lucas (1885-1963), did not get involved in the application of Lewis-Langmuir theory. British chemists who had not been bruised by the battles fought earlier in America were far more receptive to new ideas.

Lowry's interest in the Lewis-Langmuir theory began sometime after 1921. Langmuir had spoken at length on his octet theory at the 1921 British Association for

the Advancement of Science meeting in Edinburgh. J. J. Thomson had over the years modified his views from a strictly electropolar approach to accommodate electron sharing and covalence. These ideas were to appear in papers in 1907 (5), 1914 (6), and in 1921 (7). Thomson could not envision bonding as a gradation of the extent of electron pair sharing. To him polar and nonpolar bonds were distinctly different. Lowry's first contribution to the electronic theory occurred in 1922, when he read a paper before the Faraday Society on November 20, entitled "The Electronic Theory of Valency. Part I. Intramolecular Ionization" (8). This paper appeared in the February, 1923, issue of the *Transactions of the Faraday Society*. Most of the paper is devoted to examples from inorganic chemistry, with a few examples from organic chemistry. The key premise is that "a condition of intramolecular ionization exists in a large number of compounds, where nothing of the sort has been suspected previously" (8). With respect to amine oxides Lowry writes (8):

It therefore really looks as if the obvious method of representing this compound; as one in which the oxygen is linked to the nitrogen by a mixed double bond, including one covalency and one electrovalency, thus may perhaps be novel.



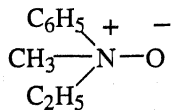
Lowry suggested this structure in response to that postulated by Langmuir for amine oxides (8):

Langmuir states definitely that in this compound the nitrogen is quadricovalent, and it is entirely unnecessary to assume a quinquivalent nitrogen. It is difficult to believe that Langmuir can have overlooked the fact that in this system the nitrogen atom is positively charged (and thereby converted into a cation), whilst the oxygen is negatively charged, and so converted into a bound anion.

On November 27, 1922, shortly after the Faraday Society meeting paper, Lowry wrote to G.N. Lewis. The letter was an acknowledgment by Lowry of reprints of recent work by Lewis. Plans to confirm a visit by Lewis to England in the summer of 1923 were also included. The post-script is the most important part of the letter and is reproduced below (9):

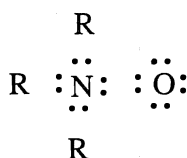
I should be interested to know your opinion in reference to Langmuir's electronic formula for N_2 . My

own view is that it is an anomaly for which I cannot find any sufficient justification; and I certainly prefer the interpretation which you gave in your 1916 paper. I have also been interested myself in the discovery of electric charges on the atoms in the electronic formulae of quite a lot of simple compounds, e.g. methylethylaniline oxide.



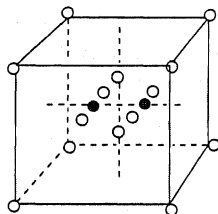
All this appears very obvious, but I cannot find that it has been published before, and it certainly leads to some interesting results both in inorganic and in organic chemistry.

Lewis in his own hand wrote the following structure:



Neville Sidgwick (1873-1952) in 1927 was to describe the bonding in amine oxides as a coordinate covalent (or dative) bond. He writes "this is the semi-polar link of Lowry and others which is written $\overset{+}{\text{A}}-\overset{-}{\text{B}}$ or $\overset{+}{\text{A}}-\overset{-}{\text{B}}$ (10). Lowry never used the terms "coordinate" or "dative" in his writings.

Langmuir had presented a rather odd structure for the nitrogen molecule in his 1919 paper "Arrangement of Electrons in Atoms and Molecules" (11). Seeking to use the Lewis cubical atom, he presented the structure shown below, in which an electron pair is embedded in the octet that surrounds the two nitrogen atoms in order to accommodate the 10 electrons of the two nitrogen atoms. This in essence was a quadruple bond.



Lewis had realized in 1916, that using the cubical atom concept would make it impossible to represent triple bonds or to show the concept of free rotation, a basic tenet of stereochemistry. He states (12):

On the other hand, the group of eight electrons in which the pairs are symmetrically placed about the center gives identically the model of the tetrahedral carbon atom which has been of such single utility throughout the whole of organic chemistry.

Thus in N_2 there are three bonds, the maximum number Lewis believed could exist, and he represented the molecule as $:\text{N}:::\text{N}:$.

Langmuir seemed never to have mastered the difficulties and problems of using the cubical atom in more complex situations in organic chemistry. His 1919 paper is replete with attempts to use the cubical atom instead of dot formulas to represent structures. Lowry was adept enough at this point to see that by using the electron-pair and octet concepts it was not possible to write a formula for amine oxides without formal positive and negative charges. Lewis never adopted Lowry's suggestion, although he comments in his 1923 monograph on "Valence and the Structure of Atoms and Molecules" as follows (13):

Presumably such a substance as amine oxide is therefore considerably polarized.

Lewis was probably loathe to use signs because it would look like a revival of the electropolar theories he had so thoroughly discredited. Lowry's representation of amine oxides is, of course, used in all textbooks today.

Over the next few months Lowry extended his ideas on the electronic interpretation of multiple bonds in organic compounds. These were to become his most significant contribution. In a letter dated January 3, 1923, to Lewis, Lowry set forth his ideas on the nature of these bonds. Before giving his rationalization for multiple bonds he returned to the problem of formal charges in amine oxides (14).

If so, you will see that the electric charges that I have postulated are arrived at by dividing equally between the two atoms the duplet which constitutes the bond between the oxygen and nitrogen in the amine oxides. The mere balancing of the electrons against the nuclear charges then gives evidence of an excess or a deficiency of electrons on the individual atoms.

With regard to multiple bonds he states the following (14):

As regards the formula for N_2 , I am inclining more and more towards the view that unsaturation nearly always means the presence of electrovalencies, usually in the form of a 'mixed' double or triple bond containing at least one valency of each type. Thus the unsaturation of ethylene and acetylene can be ascribed to the association of one electrovalency with

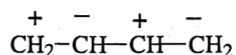
one or two covalencies, respectively, as in $\overset{+}{\text{C}}\text{H}_2-\overset{-}{\text{C}}\text{H}_2$

and $\overset{+}{\text{C}}\text{H}=\overset{-}{\text{C}}\text{H}_2$. On the other hand, the saturated character of O_2 and N_2 would be ascribed to the presence of double and triple covalencies.

Lewis in his structure for ethylene which he presented in 1916 has two formulas: $\text{H}:\text{C}::\text{C}:\text{H}$ and $\text{H}:\text{C}:\text{C}:\text{H}$. In a cryptic note Lewis writes, "I shall postpone a discussion of the important bearing of such formulae upon the problem of the conjugate bond" (15). Unfortunately this seemed to have been a permanent postponement! Certainly Lowry's conception was different from Lewis' as to the nature of the double bond.

Who were some of the persons and the ideas that influenced Lowry to produce his "novel" conception of unsaturation? Certainly J.J. Thomson was one. On January 6, 1923, Lowry wrote to Thomson the following concerning a lecture Thomson had recently given in which he discussed the electronic nature of unsaturation and conjugation. Thomson presented a model in which the reactivity of the double bond was the result of dividing the bond equally, in essence producing a septet at each carbon atom. Thomson used the cubic model of Lewis to show how this happened (16).

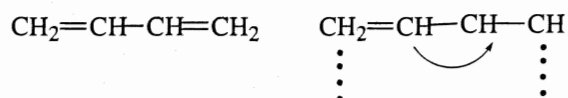
I was interested to hear your references to conjugated compounds in your lecture on Thursday morning. There are two possibilities as regards the opening of the hinge between two doubly bound carbon atoms. I think your scheme involved the formation of two septets: but I have been following up the alternative hypothesis that one carbon atom takes both electrons giving rise to an octet and a sextet. This makes the alternate atoms positively and negatively charged so that the formula for butadiene becomes



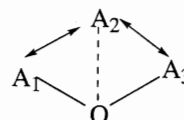
J. J. Thomson in reply seemed to accept this idea of Lowry as a possible electronic interpretation of the double bond. This is one of the earliest known examples of what became known as the electromeric effect.

Another important influence was the work of Johannes Thiele (1865-1918). Thiele, as distinct from most German organic chemists of his time, was very much interested in the theory of organic reactions. In 1899 he proposed his theory of partial valencies to explain the phenomenon of conjugation and the apparently anomalous chemistry that results. All atoms had inherently some residual affinity for reaction, according to Thiele. Thus in butadiene the inner two carbon bonds

became saturated from the residual affinity on C_2 and C_3 , leaving partial valencies on the terminal carbons to produce enhanced reactivity



Hugo Kauffmann (1870-?) in a 1908 paper in the *Physikalische Zeitschrift* (17) interpreted Thiele's theory in terms of the electron theory of valence. He argued that valence was divisible and that the lines of force associated with the electron were divided among three atoms. Thus although three atoms A_1 , A_2 , and A_3 shared a pair of electrons to form a bond, the valence was distributed over three atoms.

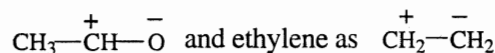


(after Stranges, Ref. 2)

Kauffmann's electronic structures bear strong similarities to those of Thiele. His dashed and dotted lines were equivalent to an electron bond in the former and a partial bond in the latter. To explain the reactivity of unsaturated molecules which was not obvious from the structural formula, Kauffmann proposed that in these systems there were more numerous lines of force more widely extended out in space, thus creating enhanced reactivity. Kauffmann's interpretation did not seem in retrospect to elicit much support or use in the chemical community. It was only a small step for Lowry to translate the formula of butadiene of Thiele to the one he proposed to J. J. Thomson.

In a paper received on March 14, 1923, by the Chemical Society and appearing in the April, 1923 issue, Lowry expanded on his view of the double bond in organic chemistry. The paper opens as follows (18):

The object of this paper is to suggest that, whilst a single bond may be either a covalency or an electrovalency, a double bond in organic chemistry usually reacts as if it contained one covalency and one electrovalency. Acetaldehyde is therefore written as

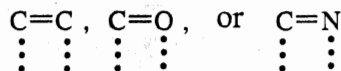


Bonds of this character are described as mixed double bonds.... Each (-) sign then indicates an excess of one planetary electron above the net nuclear charge, and each (+) sign a deficit of one electron.

According to Lowry an electrovalency enhanced the reactivity of any substance, regardless of the elements.

The enhanced reactivity of double bonds versus single bonds is the result of the mixed bond in the former. Thiele's ideas of partial valencies are easily translatable on an electronic basis by using the mixed bond:

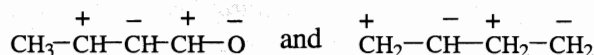
The superposition of an electrovalency on a covalency therefore provides a convenient explanation of the subdivision of affinity, which Thiele indicated by dotted lines in unsaturated groups such as



These may be written as C=C, C=O, and C=N, where oxygen and nitrogen are usually negative relatively to carbon, and oxygen relatively to nitrogen.

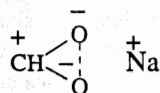
Perhaps the most important insight Lowry had in this paper deals with conjugated compounds. These were the subject of much theoretical speculation in the 1920's by the British chemists Arthur Lapworth (1872-1941), Robert Robinson (1886-1974), and Christopher Ingold (1893-1970) (18):

Crotonaldehyde and butadiene are formulated as



It will be seen that under this scheme the distinction between single and double bonds in a conjugated system disappears completely. There is therefore no longer any need to postulate a wandering of the double bond when butadiene is brominated, since the central double bond is already in position.

From our present perspective we could read into this statement the concepts of mesomerism as well as electromerism. These would become part of the system of tautomeric effects involving polarization and polarizability that Ingold was to develop beginning in 1926. Similarly Lowry's explanation of the structure of the ions of carboxylic acids is very much as used today. For example in sodium formate "The distinction between the single and double bonds disappears, just as in the case of conjugation" (18).



Other applications were made by Lowry by using the mixed bond which were to lead to controversy. Lowry firmly believed that there were formal charges in the systems he discussed and he compared his \pm sign as the same as that used in compounds such as sodium chloride. Statements of this kind were bound to cause Lowry problems. Samuel Sugden (1892-1950) of Birkbeck College, London, almost immediately called

Lowry to task in a short note received by the *Journal of the Chemical Society* on June 21, 1923, entitled "Electron Valency Theory and Stereochemistry" (19). Sugden points out that Lowry's conception of the double bond would lead to free rotation and the lack of geometrical isomerism. Lowry quickly replied on July 10, 1923 and this followed Sugden's note (20):

In reading my paper on 'The Polarity of Double Bonds' I laid considerable stress on the fact that the formulae there set out represented the reactions of the various compounds rather than the resting-states of the molecules. The metaphor used was that these formulae represented 'the dog standing up and barking' and that he might assume a very different attitude 'when curled up and at rest.'

In some cases such as the amine oxides there is no distinction between the ground and activated states. Lowry implied that the mixed double bond is only operative in the activated state and until a molecule is activated ordinary considerations will apply. The use of the \pm signs by Lowry and his qualification in this reply to Sugden created confusion. Lowry recalled in a letter dated May 9, 1935, to William Albert Noyes (1857-1941) of the University of Illinois (21):

These proposals met with such fierce opposition that for some years it was unwise to write a + or - sign on the blackboard of the Chemical Society for fear of open ridicule by Ingold and others.

In the midst of writing these papers Lowry was organizing what was to become a landmark conference in the application of electronic theory to organic chemistry. A two-day meeting on July 13-14, 1923 was held at Cambridge under the auspices of The Faraday Society. The symposium entitled "The Electronic Theory of Valency" consisted of three sessions. G. N. Lewis gave the introductory address entitled "Valence and the Electron" (22). Among those attending were J.J. Thomson, William Bragg, William Albert Noyes, Arthur Lapworth, Robert Robinson, R. G. W. Norrish, Jocelyn Thorpe, Ian Heilbron, George Norman Burkhardt, and Bernard Flurscheim. Many of these were already the leaders or soon to be leaders in British organic chemistry. In particular Lapworth and Robinson had been very active in the area under discussion.

Lowry presented the introductory address to the second part of the symposium which dealt with applications to organic chemistry. Lowry defended his concept of the mixed bond as a convenient way to explain the reactions but probably not the structure of a variety of unsaturated compounds and conjugated systems (23):

For good or evil, it possesses one feature which distinguishes it from some other electronic theories. Thus, although it was definitely suggested by and is firmly based upon the electronic theory of valency, it is capable of being expressed by familiar symbols, the meaning of which is readily grasped by all chemists. It therefore presents a specially easy target for criticism, of which it has received at least a full share. These criticisms appear to me to have done no damage to the target; and, whilst questions may be raised as to the conditions under which the structure that I have discussed is developed, I am confident that the mixed double bond represents a real alternative to the well-recognised double covalency.

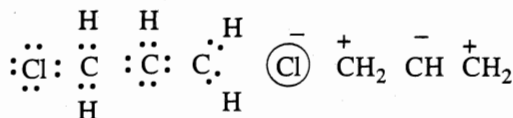
Lowry's original contributions to the symposium were two papers: "Intramolecular Ionisation in Organic Compounds" (24) and "The Transmission of Chemical Affinity By Single Bonds" (25). In the former he presented evidence showing that a polar catalyst is required to cause ethylene to react with bromine which was in line with the idea of an activated double bond being a mixed bond. In the latter paper he addressed the question of the types of valency and methods for the transmission of these. All the papers presented at the symposium, including the discussion that ensued, were printed in *The Transactions of The Faraday Society*.

In many of the papers and the discussions which followed exception was taken to Lowry's interpretation of the double bond. Lowry proposed that the mixed bond has to be thought of in the context of the problem of structure and reactivity. In the ground state the electron pair will be equally shared, but during the reactive phase there may be an ionization of the bond to produce the mixed bond. Lowry stated (26):

At the end of the discussion I am still convinced that the mixed double bond, in which one of the links has been completely ionised, really exists and plays an important part in many branches of organic chemistry.

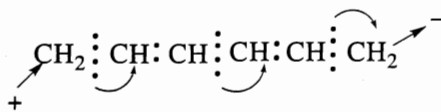
At this point it would be instructive to compare the ideas of Lowry with those of Robinson, who was also initially influenced by Thiele's residual affinities concept as well as by the alternating polarities of his colleague Arthur Lapworth. In a 1922 paper written in collaboration with William O. Kermack (1898-1970) entitled "An Explanation of the Property of Induced Polarity of Atoms and an Interpretation of the Theory of Partial Valencies on an Electronic Basis" (27), the attempt was made to translate Thiele's and Lapworth's ideas by using Lewis-Langmuir theory. In discussing the reactions of conjugated systems Kermack and Robinson produced the concept of the mobility of the octet in response to

some internal or external agent. This would lead to an alternation of polarities represented by + and - signs. These signs were not to be taken in the same light as Lowry's. For example, allyl chloride could be represented in the following way (27):



Here the stable octet surrounding the chlorine atom produces an unstable system about the adjacent carbon atom, and therefore the formation of a stable system in the CH group is facilitated by the aid of two of the electrons held in common with the second unsaturated carbon atom. It is accordingly quite natural that the product of the addition of H Br is trimethylene chlorobromide $\text{Cl-CH}_2\text{-CH}_2\text{-CH}_2\text{Br}$.

In discussing chemistry of conjugated systems such as hexatriene, Kermack and Robinson chose the following representation by incorporating the curved arrow:



Robinson's view of multiple bonds was far more general than that of Lowry. He viewed the enhanced reactivity in terms of the mobility of the octet, not an implicit electrovalency. For this and other reasons Robinson proved to be far more successful. Colin Russell, in his classic study of valence, best summarizes the contributions made by Robinson and Lowry; (28):

Thus the covalent bond was no longer conceived as a rigid entity with two electrons symmetrically shared between the atoms. A molecule was subject to strains and stresses unimagined twenty years previously, and the valency bonds were more dynamic than static, responding to the demand of a reagent to assist a reaction.

Lowry's involvement with further developments in the electronic theory of valency after 1923 was to be peripheral in nature. In a brief paper published in the *Philosophical Magazine* in 1924 entitled "The Origin of Acidity," he stated his definition of acidity as follows (29):

An acid may be defined as a hydride from which a proton can be detached, e.g. on dissolution in an ionizing solvent, on electrolysis, or by displacement by a metallic ion.

To explain the order of acidity in the series CH_4 , NH_3 , OH_2 , and HF, Lowry adopted ideas from the Bohr model

of the atom. He invoked the idea that the electrons in a molecule are in what he called looped orbits which execute a figure eight between the nuclei in a molecule. In molecules like methane the loops are far removed from the nucleus. There is little probability of repulsion between electrons in these orbits and thus the chances of losing a hydrogen as a proton are minimal. However in hydrogen fluoride for example (29):

...the much smaller loops in which the proton of hydrogen fluoride is held must bring it nearer to the nucleus, and may easily bring it so near that it can be driven outside the loop by the strong repulsion to which it is subjected.

In order to explain the transmission of acidity through a carbon chain, such as in halogenated aliphatic carboxylic acids, the suggestion was made by Lowry that atoms like chlorine tend to change the size of the orbits of the electrons they share with other atoms. Thus in a dynamic way this will cause alterations of properties along a chain whose effect will diminish with distance. Groups that enhance basicity will function in the opposite manner. These novel suggestions indicated the way that Lowry, essentially a product of the nineteenth century, so eagerly embraced new ideas.

Although few chemists were to adopt Lowry's concepts of the mixed or semi-polar bond, his faith in these ideas persisted through 1925. For example, in a discussion at the British Association meeting in Southampton on September 1, 1925, devoted to "The Alternating Effect in Carbon Chains," Lowry offered what he believed was new evidence for his views (30). Sugden had developed a concept in 1924 (31) called the parachor, a measurement of molecular volume by use of surface tension, density measurements of liquid and vapor phases, and molecular weight [$P = M^{1/4}$]. Parachor measurements, according to Lowry, were inconsistent with a nonpolar double bond in such systems as nitro, for example. There must be a nonpolar as well as a polar bond, and a group such as the sulfoxide contains only a polar bond.

In 1925 Phillips reported resolution of the compound ethyl toluenesulfinate into a pair of enantiomers. Lowry saw in this report conclusive evidence for the semipolar bond. As Lowry stated, the S=O bond in these compounds must be semipolar since only when represented by the symbol



could the existence of optical activity be possible. Sugden's measurement of the parachor for this compound also offered additional evidence for the semipolar

bond. Sugden's parachor was succeeded by more sophisticated physical measurements; indeed, the optical activity of the sulfinates is based on an entirely different explanation. Nevertheless, Lowry certainly tried to use all the experimental evidence of his day to validate his ideas.

To his credit, Lowry was one of the few university lecturers in Britain in the 1920's who dealt with the electronic theories of valency in his courses. His student W. A. Waters reminisced (32):

At Cambridge in my time (1923) he gave a wide set of lectures on recent theories of atomic and molecular structure, following Bohr, Lewis, and Langmuir, choosing his examples as much from organic as from inorganic chemistry, and giving the early spectroscopic evidence that led to the quantum theory of atomic and molecular orbitals.

Although Lowry's contributions to the electronic theory of valency may be considered only as a footnote to the work of others, through his prestige he certainly was influential in setting the state for the remarkable developments that occurred in Britain in the era between 1919-1939.

REFERENCES AND NOTES

1. T. M. Lowry, *Historical Introduction to Chemistry*, Macmillan & Co., London, 1915, 1926, 1936.
2. Comprehensive histories of valence may be found in C.A. Russell, *The History of Valency*, Leicester University Press, Leicester, 1971, and A.N. Stranges, *Electrons & Valence*, Texas A & M University Press, College Station, TX, 1982. For a historical study of unsaturation see A. A. Baker, Jr., *Unsaturation in Organic Chemistry*, Houghton Mifflin, Boston, MA, 1968.
3. C. B. Allsop and W. A. Waters. "Thomas Martin Lowry," in A. Findlay and W. H. Mills Ed., *British Chemists*, Chemical Society, London, 1947, pp. 402-418.
4. Private Communication from W. A. Waters, February 23, 1983.
5. J. J. Thomson, *Corpuscular Theory of Matter*, Charles Scribner's Sons, New York, 1907.
6. J. J. Thomson, "The Force Between Atoms and Chemical Affinity", *Phil. Mag.*, **1914**, *27*, 757-781.
7. J. J. Thomson, "On the Structure of the Molecule and Chemical Combination," *Phil. Mag.*, **1921**, *41*, 510-544.
8. T. M. Lowry, "Studies of Electrovalency. Part I. Intramolecular Ionization," *Trans. Faraday Soc.*, **1923**, *18*, 285-295.
9. Letter in the G. N. Lewis papers, The Bancroft Library, University of California, Berkeley.
10. N. V. Sidgwick, *The Electronic Theory of Valency*, Clarendon Press, Oxford, 1927, p. 60.

11. I. Langmuir, Arrangement of Electrons in Atoms and Molecules," *J. Am. Chem. Soc.*, **1919**, *41*, 868-934.
12. G. N. Lewis, "The Atom and the Molecule," *J. Am. Chem. Soc.*, **1916**, *38*, 762-785.
13. G. N. Lewis, *Valence and the Structure of Atoms and Molecules*, The Chemical Catalog Company, New York, 1923, p. 134.
14. Letter in the G. N. Lewis papers, The Bancroft Library, University of California, Berkeley.
15. Ref. 12.
16. Letter in the J. J. Thomson papers, Cambridge University Library.
17. H. Kaufmann, "Electronen and Valenzlehre," *Phys. Z.*, **1908**, *9*, 311-314, 334-42, 539-541.
18. T. M. Lowry, "Studies of Electrovalency. Part I. The Polarity of Double Bonds," *J. Chem. Soc.*, **1923**, *123*, 822-831.
19. S. Sugden, "Electron Valency Theories and Stereochemistry," *J. Chem. Soc.*, **1923**, *123*, 1861-1867.
20. T. M. Lowry, "Note on the Theory of Free Rotation," *J. Chem. Soc.*, **1923**, *123*, 1866-1877.
21. Letter in the W. A. Noyes papers, University of Illinois Library, Urbana, IL.
22. G. N. Lewis, "Valence and the Electrons," *Trans. Faraday Soc.*, **1923**, *19*, 452-458.
23. T. M. Lowry, "Introductory Remarks to Part II: Applications in Organic Chemistry, The Electronic Theory of Valency," *Trans. Faraday Soc.*, **1923**, *19*, 485-487.
24. T. M. Lowry, "Intramolecular Ionization in Organic Compounds," *Trans. Faraday Soc.*, **1923**, *19*, 488-496.
25. T. M. Lowry, "The Transmission of Chemical Affinity by Single Bonds," *Phil. Mag.*, **1924**, *47*, 1013-1020.
26. T. M. Lowry, "The Electronic Theory of Valency: Discussion on Part II," *Trans. Faraday Soc.*, **1923**, *19*, 537.
27. W. O. Kermak and R. Robinson, "An Explanation of the Property of Induced Polarity of Atoms and an Interpretation of the Theory of Partial Valences on an Electronic Basis," *J. Chem. Soc.*, **1922**, *121*, 427-440.
28. C. A. Russell, *The History of Valency*, Leicester University Press, Leicester, 1971, p. 290.
29. T. M. Lowry, "The Electronic Theory of Valency," *Phil. Mag.*, **1924**, *47*, 1021-1024.
30. T. M. Lowry, "Polarity and Activation," *J. Chem. Soc.*, **1925**, *44*, 970-972.
31. S. Sugden, "A Relation between Surface, Tension, Density, and Chemical Composition," *J. Chem. Soc.*, **1924**, *125*, 1177-1189.
32. Ref. 4.

ABOUT THE AUTHOR

Dr. Saltzman is Professor of Natural Science at Providence College, Providence, RI 02918, and is particularly interested in the development of physical organic chemistry in the 20th century in Britain and America.

Letter to the Editor

In Nos 15/16 of the *Bulletin*, the paper by Pierre Laszlo "Georges Darzens (1857-1954): Inventor and Iconoclast" pages 59-64 was marred by a most unfortunate editorial addition: at no time in its history was the Ecole polytechnique headed by a German general! Even though this horrible mistake was corrected on the proofs, Mr. Murphy or the vengeful Gods left it for all to see!

— Pierre Laszlo

THE DOCTORAL THESES OF PIERRE ADOLPHE BOBIERRE (1823-1881)

John T. Stock, University of Connecticut

Apart from his birth on May 7, 1823, information concerning Bobierre's early life seems to be lacking (1, 2). In 1843 he was "Préparateur de Chimie" in l'École Primaire Supérieure in Paris. In the following year he filled a similar position in the Faculty of Medicine, under the direction of Jean Baptiste André Dumas (1800-1884). Bobierre obtained a diploma as "Pharmacien" in Marseilles in 1846; the route into chemistry by way of pharmacy was not uncommon at that time. By 1848 he was in Nantes and eventually died there on October 20, 1881, after a productive and varied scientific career. He appears to have attained recognition early; when he obtained his *licencié es sciences* in 1853 he had already written a number of papers as well as several books (3-5). After the appearance of the book on fertilizers (4), Dumas, who was then Minister of Agriculture, asked Bobierre to reorganize the control of these commodities. A law aimed at the suppression of their fraudulent sale was passed in 1867.

Bobierre became examiner of fertilizers in 1850. Finding that Nantes was lacking in the teaching of science, he started a course on chemistry, receiving no pay. When the Nantes School of Sciences was founded in 1855, he was appointed to the chair of chemistry and occupied this to the end of his life.

Bobierre was obviously well respected during his lifetime; he was awarded the French Cross of the Legion of Honor and received several foreign decorations (2). However, although he was active in Nantes for many years, this town, its university, and the Bibliothèque Interuniversitaire in Nancy have no record of a portrait of him.

Figure 1 shows the title page of Bobierre's doctoral theses. These were approved in the summer of 1858, when he was 35 years old (6). By then he was well established in both scientific and government circles. Dumas, the President of the examining board, was a powerful public figure and, as an internationally famous scientist, had received the Royal Society's Copley Medal in 1843. The examiners were César Mansuete Despretz (1789-1863), a celebrated physicist who was President of the Académie des Sciences in 1858, and Antoine Jerome Balard (1802-1876). Balard, most remembered for his discovery of bromine in 1826, began his professional life at the age of 17 as a laboratory boy at the Faculté des Sciences in Montpellier. From 1851 until his death, he was Professor in the Collège de France, Paris.

The longer of the two theses is devoted to "Physics" and carries the subtitle "Electrochemical phenomena which characterize the alteration, in the sea, of alloys used to sheath ships" (7). However, this thesis describes many analytical methods which have no direct connection with electrochemistry. In fact, the document is particularly valuable because it provides an excellent overview of the state of French analytical chemistry in the mid 1850's. The subtitle of the much shorter "Chemistry" thesis is "Observations relating to agriculture in the West of France" (8). A number of short accounts of agricultural chemistry are given.

Because he was the official examiner of fertilizers, it is not surprising that Bobierre should choose to deal with topics related to agriculture. However, his "Physics" thesis and related publications give no clue as to how he became involved in the completely different and

highly specialized study of metallic corrosion. Biographical (1) and obituary (2) notices do not mention this study.

Although Bobierre had published accounts of the material in his theses, these documents make almost no reference to them. In fact, referencing is minimal; often merely a name (e.g., "Davy") is all that is given. It is hoped that the references found before writing the present account are the ones that Bobierre had in mind.

Bobierre's "Physics" theme was a study of the causes of deterioration in sea water of the metallic sheathing of ships (9-12). Apparently the protection of the hulls of vessels by covering with metal plates goes back to ancient times. Copper sheathing came into use in the late 18th century. Variability of attack under sailing conditions was a problem. Commercial copper was usually impure, but the use of high-purity metal was no guarantee of long life.

Sir Humphry Davy gave much time to this problem, using as a remedy a small sacrificial anode of zinc or iron (13-15). This alleviated attack on the plates but, unfortunately, induced the formation on them of a cal-

cium-magnesium carbonate film which provided anchorage for seaweed, barnacles, and the like.

A study published in 1841 concerned the durability of the sheathing on four ships (16). All were sheathed with copper containing approximately 0.5 per cent of impurities. In two cases, this sheathing proved to be very durable; in the others, attack was severe. Yet chemical analyses of the sheathings gave no clue as to the causes of the differing performances. Factors such as differing voyaging conditions were considered. The report suggested that ships, each with the two sides carrying different kinds of copper, should be sent to sea, so that the more durable material could be selected.

Because it had been shown that some samples of impure copper performed better than the pure metal, the making of deliberate additions to copper was obvious. Thus the copper-tin alloy, bronze, came into use as a sheathing material. Bronze is harder than copper and consequently more difficult to fabricate.

Bobierre surveyed the reports of trials, made in the 1830's, on ships that were sheathed partly with copper, partly with bronze. Results varied, but the loss in weight of copper sheathing tended to be about twice that undergone by bronze. He then began the detailed examination of the bronze sheathing of various ships; one of these was the *Sarah*, the subject of a later re-examination. The sheathing of this ship, installed in 1849, became perforated and had to be replaced about a year later. Damage was not uniform; some plates were not attacked at all. He backed up his visual examination by chemical analyses, finding that the *Sarah*'s sheathing contained an average of 3.1% of tin.

Bobierre surveyed existing methods before choosing his own approach. He considered the determination of tin by Sobrero's method, in which tin is volatilized in

a stream of chlorine (17). Bobierre found problems in addition to the splashing of copper chloride and resorted to classical methods, which he refined.

The weighed sample is attacked with a slight excess of nitric acid. After dilution and filtration, any lead in the filtrate is determined by precipitation as the sulfate. The residue, principally metastannic acid, is ignited and weighed. The residue, which also contains any arsenic from the alloy, is heated in a stream of hydrogen, so that the volatile arsine can be decomposed to yield a deposit of arsenic that could be compared with standards.

Bobierre drew up a table of conclusions drawn from the visual and analytical examination of the sheathings

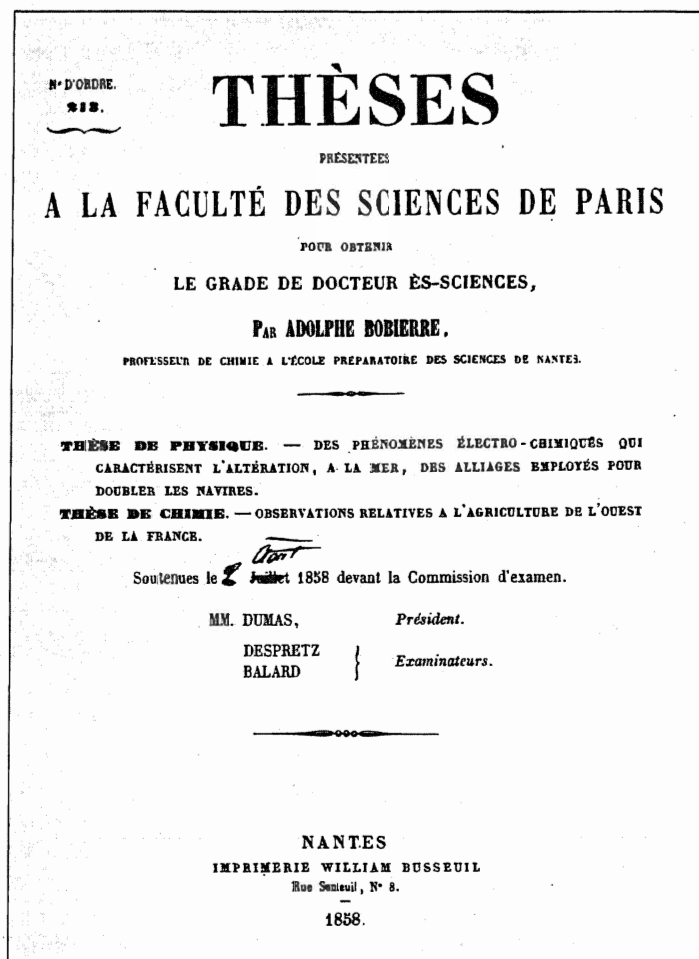


Figure 1 Title page of Bobierre's theses

used for various vessels. Although good performances had been observed with bronzes containing approximately 5% of tin, he pointed out that his conclusion was a generality. He found that attack, more noticeable at the water line, was greater near the prow of the vessel than near the stern. Obviously, sampling could not be confined to a single location.

He realized that electrochemical attack could be induced if the sheathing was not of uniform composition. He sampled bronze ingots of various copper:tin ratios, finding that the percentage of tin was greater at the surface of an ingot than in its center. The likelihood of homogeneity diminished as the average percentage of tin deviated from 5.5. Further, a bronze containing this percentage of tin proved to be very suitable for sheathing.

Bobierre had previously published his observations concerning the sheathing troubles of the ship *Sarah* (9). Obtaining a sample of the new sheathing that had been applied in 1852, he predicted that this sheathing would prove to be unsatisfactory. He deposited this prediction as a sealed letter to the Academy of Sciences (18). When the ship returned to France less than two years later, the sheathing was found to be in poor condition.

Bobierre then turned to sheathings of the copper-zinc alloy, brass, commenting that "...although relatively less durable than bronze or copper, they were in general use." (7) The performances of brass sheathings varied considerably, so Bobierre directed his attention to methods for the analysis of samples of new and of seaworn brass sheathing. Methods for the detection or determination of lead, arsenic, and iron were similar to those used for the bronze samples. Several methods for the separation of copper from zinc were surveyed. In the precipitation of copper by hydrogen sulfide or potassium sulfide, some zinc coprecipitated with copper sulfide, as had already been pointed out by Rivot and Bouquet (19). Titration, in hot ammoniacal medium, of copper with sodium sulfide solution had been described by Theophile Jules Pelouze (1807-1867) (20). However, for the analysis of brass, a difficult filtration of the easily-oxidized $5\text{CuS}\cdot\text{CuO}$ precipitate is required if zinc is to be determined (19).

Bobierre found that Flajolot's method (21) gave good results when used skillfully. He rejected this be-

cause the precipitate of cuprous sulfide, obtained by the action of "hyposulphite de soude" (sodium thiosulfate), required lengthy washing on the filter. In a method described by Louis Edmond Rivot (1820-1869) and Jean Jacques Bouquet (1827-1909), copper oxide is precipitated by the addition of potassium hydroxide. The filtrate is slightly acidified; then zinc is precipitated by means of sodium carbonate. Bobierre found that the re-

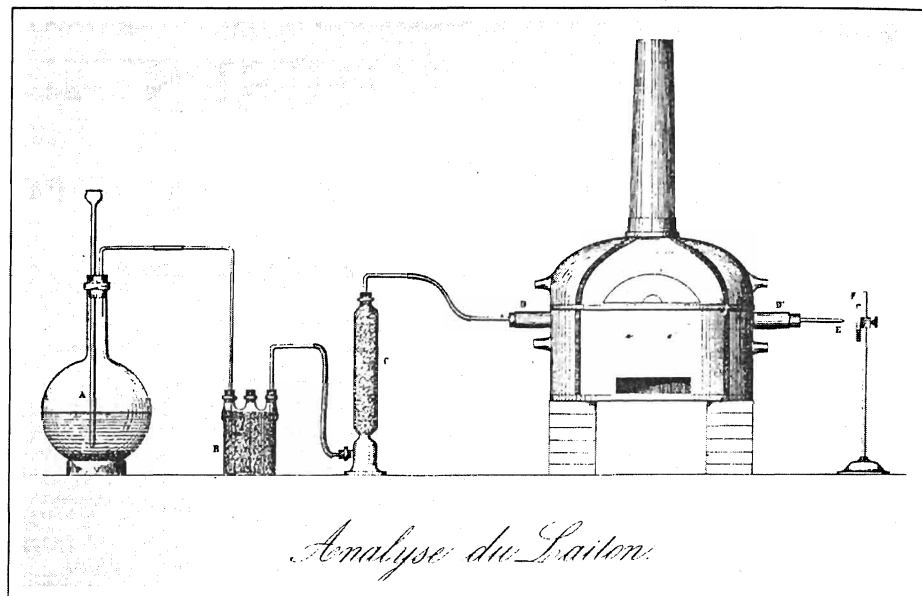


Figure 2 Apparatus for the analysis of brasses

sults for copper tended to be high.

Heating in a stream of hydrogen had been used for selective reduction in mixtures of metallic oxides (22). This principle had been adapted for the analysis of brass and bronze (23). Bobierre found that the results for zinc tended to be high. He next examined an oxidative titration method for the determination of copper (24). Copper in the solution from the sample is reduced to the cuprous state by sodium sulfite. After acidification and boiling to destroy excess sulfite, the solution is titrated with permanganate solution. He passed over this method because of variable results such as can arise from residual traces of sulfurous or nitric acids.

Bobierre next considered the loss-in-weight method used for the assay of brass coinage. Repeated furnace heatings were involved, the aim being to drive off zinc. For his purpose, the method was doubly objectionable. First, if lead is present, it is only partially volatilized. Second, the furnaces of the mint assayer were not available to the general analyst. However, the principle of selective metal volatilization appealed to Bobierre, who set about to devise a suitable method. This involved the strong heating of a weighed brass sample in a stream of hydrogen, when zinc and any lead are driven off, while

any iron or tin remains with the copper. This is obtained and weighed as a spheroidal button at the end of the operation.

The apparatus is shown in Figure 2. Hydrogen, generated in A, is demisted in B and dried by passage through calcium chloride tower C. The gas then flows through porcelain tube D, which is heated to redness. This tube contains a carbon boat into which the sample has been weighed. When the issue of white vapor from tube E appears to have stopped, disk F, which is covered with black velvet, is placed near the exit. If the velvet remains unmarked, heating is stopped and the metal button is weighed when cool. The results quoted by Bobierre indicate that the weights of the buttons thus obtained were within approximately 2-3% of the expected values.

From his 10 years of investigation of the behavior of brass sheathings, Bobierre recognized three distinct types:

1. The thickness of the sheathing diminished uniformly; neither the color nor the malleability changed much. Such behavior was usual with sheathings containing 30-34% of zinc.
2. Although there was little change in color, etc., local attack, often resulting in pitting, occurred.
3. Especially on the side exposed to the sea, the color approached that of copper and the porosity was such that finger pressure could cause fragmentation.

In other cases, otherwise satisfactory sheathing had begun to lift away from the hull, because of the corrosion of the brass retaining nails. These had contained more zinc than the sheathing and consequently suffered anodic attack. With some sheathings that originally contained 40% of zinc, the areas suffering the greatest attack had lost much of this metal. This suggested that the original material was not homogeneous and led Bobierre to survey methods used to manufacture brass sheets.

Owing to volatilization of zinc while the brass is molten, a resulting ingot may become richer in copper than expected from the proportions of the metals taken. During transport from the furnace to ingot mold, zinc tends to rise to the surface, so

the melt is stirred before pouring. However, the first portions rapidly solidify in the cool mold; but, in the slower solidification of the remainder of this pouring, various alloy phases may separate and thus lead to inhomogeneity, as Bobierre found.

Two processes were in use to convert ingots into sheets. Hot lamination, taking 24 hours and requiring only 5 heatings and 15 passes through the rolling mill, was generally adopted for brasses containing about 40% of zinc. A month was needed for cold lamination, which involved 22 reheats and coolings, as well as 66 passes through the rollers. This process was rarely used for 40% zinc brasses. In hot-laminating brass of this composition volatilization of zinc, occurring in the furnace, could be seen continuing during rolling. With brasses containing up to 20% of zinc, a black film of copper oxide formed during heating. This film could break away during rolling. Bobierre found that these effects were not observed with the apparently stable brasses containing above 20% to 33% of zinc.

Cold lamination gave a product more dense and of higher zinc content than obtainable by the hot lamination of a similar ingot. Presumably molecular stabilization and a higher density were induced by the numerous rollings needed for cold lamination. The product was found to be electronegative to one that had been hot laminated. Although they may have similar compositions, brasses from differing sources should not be used to sheath a given vessel.

Bobierre concluded that brass with the atomic ratio $2\text{Cu}:\text{Zn}$; i.e., of zinc content close to 34%, was advantageous in all respects. Insistence by sheathing contractors that the zinc content of the brass must not exceed this figure provided a guarantee against the supply of material that had been made by hot lamination, which requires an alloy with a higher percentage of zinc. Phenomena such as poor homogeneity in hot-laminated brass could cause this to deteriorate in sea water.

Bobierre's thesis on chemistry begins with a short account of the analysis of sugarbeets from various

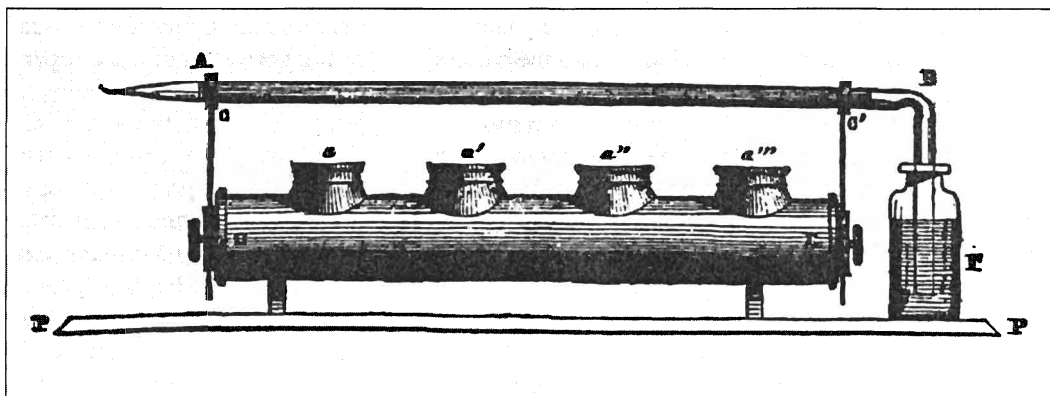


Figure 3 Apparatus for the determination of ammonia liberated from guano

sources (25). Prevailing belief was that the soils of the Loire-Inferieure region were unsuited for sugar production; he proved that this was not the case. Next follow a few pages on the flooding of the Loire and the effect on the composition of the water of this river. The great need for storage and filtration of water destined for human consumption is stressed.

Uncertainties as to quality, even adulteration, of the then important fertilizer Peruvian guano greatly concerned Bobierre. He needed a rapid method for the determination of nitrogen (i.e., available ammonia) to check such uncertainties. The apparatus (Figure 3) that he developed for this purpose was based on a principle

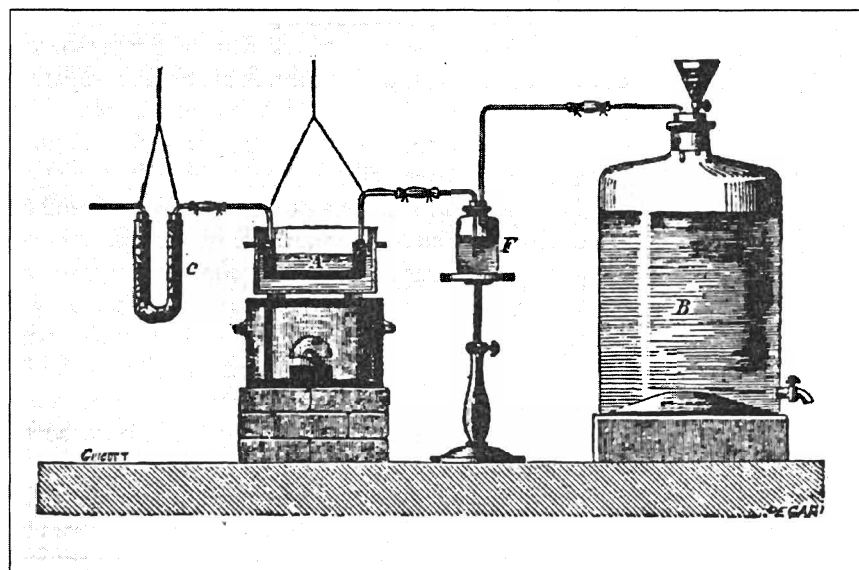


Figure 4 Apparatus for the assessment of the loss of ammonia from guano

described by Eugène Melchior Peligot (1811-1890) (26). The weighed sample is mixed with finely-powdered soda-lime and trapped between columns of the same but coarser reagent. The tube is progressively heated, starting from the end near the beak, when liberated ammonia is driven into standard sulfuric acid solution in flask F. When heating is discontinued, the beak is broken off to prevent sucking back during cooling; and the excess acid is back titrated.

Various additives had been proposed to minimize the loss of ammonia from guano that is in contact with air. Bobierre developed the apparatus shown in Figure 4 to demonstrate that "animal black" (presumably carbonized animal substances, or "animal charcoal") was effective for this purpose (27). Three grams of damp guano are placed in vessel A, which is kept at 50° C in a water bath. Outflow from aspirator B causes a fixed volume of air to be drawn through drying tube C, Vessel A, and flask F, which contains 10 cc of standard sulfu-

ric acid solution. This is back titrated at the end of the experiment. He calculated that a given sample had lost 4.3 mg of nitrogen. In a similar run in which one gram of "animal black" had been mixed with the guano, no loss of nitrogen was found.

In a further experiment, a sample of guano and another admixed with one-third of its weight of "animal black" were placed in a current of air for 10 days, being exposed to sunlight as much as possible. The mixture lost only 0.2% of nitrogen, while the loss from the additive-free guano was 2%.

Bobierre next reported on a rock-like fertilizer termed "guano phosphatique" (28). He then examined "animal black" in more detail, including its use in the refining of syrups (29). He found that the blackening of the copper pipes leading to the filters was caused by sulfur compounds, either present in the "animal black" or absorbed by it. Heating of the "black" with hydrochloric acid, passage of the liberated hydrogen sulfide into copper sulfate solution, and the weighing of the precipitated copper sulfide were used for quantitative studies.

After a comment on the solubility of calcium phosphate in sugar refinery residues (30), Bobierre got to the final topic of his thesis. Here was a direct agricultural problem that involved actual growing trials. It entailed a study of the release to growing vegetation of nominally insoluble phosphate. Laboratory experiments showed that slow solubilization of calcium phosphate can be brought about by a wide variety of salt solutions. The causes of the dissolution or transformation of "insoluble" phosphate in soils appeared to be very complex.

The treatment and scope of the topics in the "Chemistry" thesis are no more than might be expected of an experienced agricultural scientist. However, the study of the corrosion of the copper alloy sheathings on seagoing ships marks Bobierre as an outstanding investigator. Passing from a visual examination of the sheathings to an extended survey of methods for their analysis, he improved and developed such methods until he could be sure of his results. He then transformed himself into an industrial scientist-technologist, searching for factors beyond gross composition that might govern the performance of materials to be used for sheathing. He examined foundry practice, looking for causes of inhomogeneity during the casting of ingots. These had to be rolled into sheets, suitable for sheath-

ing; Bobierre showed that the procedure adopted had a considerable bearing on the expected performance of the resulting sheathing. Eventually, he was able to specify the composition and manufacturing procedure for sheathing which might be expected to have satisfactory durability. This work is an excellent example of the pursuit of a difficult project through all of its stages, with perseverance until a satisfactory and doubtless technically valuable conclusion had been reached.

ACKNOWLEDGMENTS

This work was partly carried out under the Research Fellowship Program of the Science Museum, London. I am grateful to Professor Robert Merrer, of Western Connecticut State University, for the gift of the printed theses.

REFERENCES AND NOTES

1. A. M. Latour, *Dictionnaire de Biographie Francaise*, Letouzy, Paris, 1954, p. 737.
2. L. Maitre, "Notice Nécrologique sur Ad. Bobierre", *Ann. Soc. Acad. Nantes*, 1881, 2, 344-458.
3. A. Bobierre, *Traité de Manipulations Chimiques*, Mequignon-Marvis, Paris, 1844.
4. E. Moride and A. Bobierre, *Technologie des Engrais de L'Ouest de France*, Langlois-Leclercq, Paris, 1848.
5. A. Bobierre, *Lecons Élémentaires de Chimie*, Masson, Paris, 1852.
6. A. Bobierre, *Thèses Présentées à la Faculté des Sciences de Paris. Thèse de Physique: Thèse de Chimie*, Busseuil, Nantes, 1858.
7. Des phénomènes électrochimiques qui caractérisent l'altération, à la mer, des alliages employés pour doubler les navires.
8. Observations relatives à l'agriculture de l'ouest de France.
9. A. Bobierre, "Recherches sur l'Altération des Bronzes Employés au Doublage des Navires," *Compt. Rend.*, 1852, 34, 688-691.
10. A. Bobierre, "Recherches sur l'Altération des Bronzes Employés au Doublage des Navires", *Compt. Rend.*, 1853, 37, 131-132.
11. A. Bobierre, Note sur l'Altération des Bronzes Employés au Doublage des Vaisseaux," *Compt. Rend.*, 1854, 38, 122-123.
12. A. Bobierre, "De l'Altération, à la Mer, des Doublages en Laiton des Navires," *Compt. Rend.*, 1858, 47, 357.
13. H. Davy, "On the Corrosion of Copper Sheathing by Seawater, and on Methods of Preventing this Effect, and on their Application to Ships of War and Other Ships," *Philos. Trans. Roy. Soc.*, 1824, 114, 151-158.
14. H. Davy, "Additional Experiments and Observations on the Application of Electrical Combinations to the Preservation of Copper Sheathing of Ships, and to Other Purposes," *Philos. Trans. Roy. Soc.*, 1824, 114, 242-246.
15. H. Davy, "Further Researches on the Preservation of Metals by Electrochemical Means," *Philos. Trans. Roy. Soc.*, 1825, 115, 328-346.
16. J. Prideaux, "Some Inquiries into the Causes of the Increased Destructibility of Modern Copper Sheathing," *Brit. Assoc. Adv. Sci. Report*, 1841, Pt. 2, 43-47.
17. C. Barreswil and A. Sobrero, *Appendice à tous les Traités d'Analyse Chimique*, Fortin, Masson et Cie., Paris, 1845, pp. 144-145.
18. A. Bobierre, "Depôt d'un Paquet," *Compt. Rend.*, 1851, 32, 728.
19. L. E. Rivot and J.P. Bouquet, "Note sur l'Analyse des Alliages de Cuivre et de Zinc," *Ann. Chim. Phys.*, 1851, 33, 24-33.
20. T. J. Pelouze, "Second Mémoire sur le Dosage de Cuivre", *Ann. Chim. Phys.*, 1846, 17, 393-421.
21. Flajolot, "Sur la Séparation de Quelques Oxydes Métalliques," *Ann. Chim. Phys.*, 1853, 39, 460-469.
22. L. E. Rivot, "De l'Emploi de l'Hydrogène dans les Analyses des Substances Minérales," *Ann. Chim. Phys.*, 1850, 30, 188-203.
23. H. Sainte-Claire Deville, "Méthode d'Analyse des Bronzes et des Laitons," *Ann. Chim. Phys.*, 1855, 43, 473-477.
24. A. Terreil, "Dosage du Cuivre par le Permanganate de Potasse," *Compt. Rend.*, 1858, 46, 230-232.
25. A. Bobierre, "Note sur la Composition Chimique de la Betterave à Sucre, Cultivée dans les Terrains Alumino-silicieux de la Loire Inférieure," *Compt. Rend.*, 1853, 36, 31-33.
26. E. M. Peligot, "Sur un Procédé Propre à Déterminer, d'une Maniere Rapide, la Quantité d'Azote Contenue dans les Substances Organiques," *Compt. Rend.*, 1847, 34, 550-553.
27. A. Bobierre, "Note sur la Déperdition de l'Ammoniaque du Guano Péruvien," *Compt. Rend.*, 1856, 43, 757-759.
28. A. Bobierre, "Note sur un Substance Dite Guano Phosphatique," *Compt. Rend.*, 1857, 44, 1013-1015.
29. A. Bobierre, *Le Noir Animale, Analyse, Emploi, Vente*, Librairie Agricole de la Maison Rustique, Paris, 1856.
30. A. Bobierre, "Sur la Solubilité du Phosphate du Chaux dans le Sucrate de Chaux," *Compt. Rend.*, 1851, 32, 859-860.

ABOUT THE AUTHOR

John T. Stock, winner of the 1992 Dexter Award in the History of Chemistry, is Professor Emeritus of Chemistry, University of Connecticut, Storrs, CT 06269. His major interest is in the history and preservation of chemical instruments.

OLD CHEMISTRIES

Alonzo Gray's "Elements of Chemistry"

William D. Williams, Harding University

Many scientists have been inspired to pursue their careers by reading an old textbook when they were at a very young age. A case in point is Charles Martin Hall (1863-1914), who discovered the first practical, inexpensive method of producing aluminum. He began research on this problem while still a student at Oberlin College, OH. Working with homemade equipment in the family woodshed, he found that molten cryolite would dissolve aluminum oxide and that a carbon anode would allow electrodeposition of metallic aluminum. On February 23, 1886, at the age of 22, Hall produced the first globules of aluminum by this method. Subsequent patents and founding of what became the Aluminum Company of America resulted in a new industry that has touched every facet of modern life.

In 1911 Hall was presented the Perkin Medal for his contributions in electrochemistry. His speech at that occasion recalled his early study of chemistry (1):

My first knowledge of chemistry was gained as a schoolboy at Oberlin Ohio, from reading a book on chemistry which my father studied in college in the forties. I still have the book, published in 1841. It is minus the cover and the title page, so I do not know the author. It may be interesting to see what this book, published seventy years ago, says about aluminum: "The metal may be obtained by heating chloride of aluminum with potassium in a covered platinum or porcelain crucible and dissolving out the salt with water. As thus prepared it is a gray powder similar to platinum, but when rubbed in a mortar exhibits distinctly metallic lustre. It fuses at a higher temperature than cast-iron and in this state is a conductor of electricity but a non-conductor when cold.

Some Hall biographers mention the 1841 chemistry book, but none has sought to identify it (2). After searching through chemistry texts in use in the 1840s, the present author has located Hall's quotation in *Elements of Chemistry* by Alonzo Gray (3).

Gray's *Elements of Chemistry* undoubtedly influenced many young students in addition to Charles Hall. It went through forty "editions" from 1840 to 1858, with a remarkable 28 "editions" in a two-year span, 1846 to 1848. Most of the "editions" were merely reprintings. Only three differences in content and pagination are found: 1840 (1st, 359 pp.), 1841 (2nd, 395 pp.), and 1848 (40th, 452 pp.). Contemporary John Griscom recommended the work as (4):

An exceedingly judicious arrangement of the facts of Chemistry. Its consecutive order is lucid and logical. It indicates a mind accustomed to teach as well as study. It seems to me to hold a happy medium between the brevity which only obscures the subject, and the copious details of works too elaborate and minute for the general student.

Little is known of the life of Alonzo Gray. Born in Townsend, Vermont on 21 February 1808, he received the A.B. in 1834 from Amherst and the A.M. in 1838 from Andover Theological Seminary, Andover, Mass. From 1837 to 1843 he taught natural science, chemistry, botany, geology and mineralogy at Teacher's Seminary, a division of Phillips Academy, at Andover. Following the 1844-5 school year as professor of chemistry at Marietta College, the remainder of his career was spent as headmaster of two schools in Brooklyn, NY: Brooklyn Heights Female Academy and Brooklyn

Heights Female Seminary. He died March 10, 1860, in Brooklyn (5).

Gray's *Elements* was one of several American texts based upon that of British chemist Edward Turner's *Elements of Chemistry* (6). Silliman's *American Journal of Science* noted this trend in its review of Gray's book (7):

Unhappily for our reputation as advancers of science, almost all the works on chemistry which have been issued here have been written on the basis of some foreign treatise. We hope the day is not far distant when American chemists will take a high rank as original investigators.

Gray acknowledged that his work was a compilation from Webster, Liebig, Berzelius, and Griffin, in addition to Turner. He even listed page numbers in Turner for further reading. He followed the same order as Turner, but his judicious simplification of Turner's intimidating treatise was a welcome contribution. A second edition was issued within a year because of "the rapid sale of the first edition and its introduction into several colleges (8)."

The first sentence in the book, "Science is classified knowledge," emphasized Gray's philosophy of teaching. Indeed, a very important attribute of the text was its eye-catching outline format. A 14-page Table of Contents provided a detailed outline of the entire book. That same outline was accentuated in the body of the text with numbers, headings, and frequent changes in type. Experiments were described to illustrate almost every topic—always in the same small type to make them quickly recognizable. Likewise, special paragraphs on "Theory" followed many sections. He encouraged the use of chemical formulas as "the most efficient aid to clear, definite, and easy comprehension of the whole science (9)." Because he believed that teachers should emphasize principles rather than details, he gave more attention to a few very important nonmetals and the imponderable agents (heat, light, and electricity). The remaining elements, compounds, organic chemistry, and analytical chemistry were condensed to almost outline form.

The only personal comments in the volume that yield an insight into Gray's personality were several brief references to God as the "great and ultimate cause" of a nature that is designed for the benefit of man (10). In describing Hare's compound blowpipe, Gray added a footnote that inferred that he had experience using the system (11). Under the topic, "Steam Artillery," the same argument was made for a war deterrent that has been used about nuclear arms in our modern era (12):

A [steam] musket may be made to throw from one hundred to a thousand balls per minute. It is greatly to be hoped that this experiment will prove successful; for, if such engines of death could be brought into the field of battle, few nations would be willing to settle their disputes in that way. Few would fight in the prospect of certain death.

Gray wrote two other widely used texts as companion works to his chemistry: *Elements of Natural Philosophy* (1850-1875) and *Elements of Geology* (1853-1863, with former classmate Charles B. Adams). His *Elements of Scientific and Practical Agriculture* (1842) was one of the earliest American books to promote agricultural chemistry. Written for his class at Phillips Academy, it was a critical digest of the work of European chemists, in which the author urged the introduction of agriculture as a branch of study in high schools (13). Gray acknowledged the principle we now call photosynthesis but disagreed with Liebig's theory that plants derive all their carbon from the air. Although the book gave special thanks to Samuel L. Dana, it was already in press before Dana's *Muck Manual for Farmers* (14) was published the same year.

REFERENCES AND NOTES

1. "The Perkin Medal Award," *J. Ind. Eng. Chem.*, 1911, 3, 146.
2. J. Kendall, *Young Chemists and Great Discoveries*, D. Appleton-Century Co., New York, 1940, pp. 236-244; C. Carr, *ALCOA An American Enterprise*, Rinehart, New York, 1952, pp. 5-21; J. Edwards, *The Immortal Woodshed*, Dodd, Mead and Co., New York, 1955; N. Craig, "Charles Martin Hall—The Young Man, his Mentor, and his Metal," *J. Chem. Educ.*, 1986, 63, 557-9.
3. A. Gray, *Elements of Chemistry*: containing the principles of the science, both experimental and practical, intended as a text-book for academies, high schools, and colleges, Dayton and Saxton, New York, 1841, p. 234.
4. S. A. Allibone, *A Critical Dictionary of English Literature*, J. B. Lippincott, Philadelphia, 1870, Vol. 1, p. 723.
5. J. Wilson and J. Fiske, *Appleton's Cyclopaedia of American Biography*, D. Appleton, New York, 1888, Vol. 2, p. 728; M. D. Gilman, *The Bibliography of Vermont*, Free Press Association, Burlington, 1897, p. 105; R. Dake, "The History of Chemistry at Phillips Academy," *J. Chem. Educ.*, 1939, 16, 403-9.
6. Turner went through seven British editions from 1827 to 1842. Franklin Bache edited six official American editions from 1828 to 1840, exact copies of the British work with footnotes by Bache. A seventh American edition in 1846 was edited by James and Robert Rogers. Other American texts based on Turner were: J. Green, A

- Text Book of Chemical Philosophy* (1828); J. Comstock, *Elements of Chemistry* (over 100 "editions" from 1831 to 1859); L. Beck, *A Manual of Chemistry* (four editions from 1831 to 1844); J. Johnston, *A Manual of Chemistry* (eight editions from 1810 to 1874).
7. "Miscellanies," *Amer. J. Sci.*, **1840**, 39, 391.
 8. Reference 3, p. 3.
 9. Reference 3, p. 5.
 10. Reference 3, pp. 120, 133, 163, 176.
 11. Reference 3, p. 153. Note that the illustration of Hare's blowpipe is a different design from that in Steele's 1868 text, which was used on the cover of *Bull. Hist. Chem.*, **1989**, Number 4.
 12. Reference 3, p. 59.
 13. A. Gray, *Elements of Scientific and Practical Agriculture*, or the application of biology, geology, and chemistry to agriculture and horticulture. Intended as a textbook for farmers and students in agriculture, Allen, Morrill and Wardwell, Andover and Dayton and Newman, New York, 1842. This text was reviewed in *The New Genesee Farmer*, 1842, 3, 134.
 14. S. Dana, *A Muck Manual for Farmers*, D. Bixby, Lowell, MA, 1842. America's enthusiastic acceptance of soil and plant chemistry is discussed in: M. Rossiter, *The Emergence of Agricultural Science. Justus Liebig and the Americas*, 1840-1880, Yale University Press, New Haven, CT, 1975.
 15. No likeness of Alonzo Gray has been found.

ABOUT THE AUTHOR

William D. Williams is Professor of Chemistry, Emeritus, at Harding University, Box 602, Searcy, AR 72149-0001. He is an authority on early American chemistry texts and their authors.



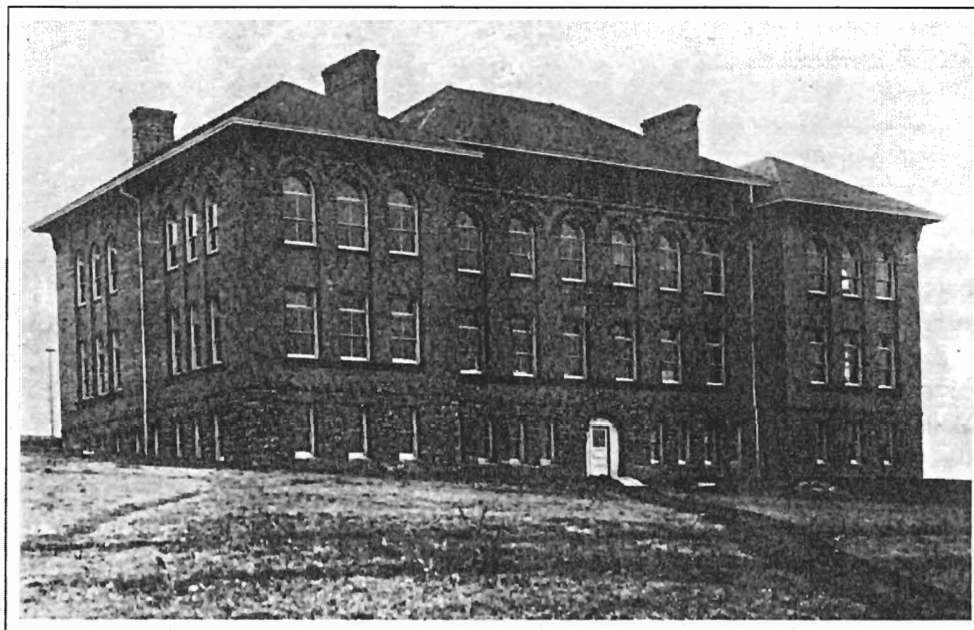
Recipient of the 1997 *DEXTER AWARD* is Professor Bernadette Bensaude-Vincent, University of Paris X. The award will be presented at the Spring 1998 American Chemical Society Meeting in Dallas, TX, March 29 - April 2. See Book Notes, p. 71.

A MORRILL HALL FOR CHEMISTRY

Paul R. Jones, University of Michigan

Senator Justin Smith Morrill first introduced the "Agricultural College Act" into the US House in 1857; after a series of setbacks, the "Morrill Act" was finally approved by close margins in both the House and Senate and submitted in 1859 to President Buchanan, who vetoed the measure. When Senator Morrill re-introduced the bill into the House in 1861, the Committee on Public Lands recommended 'that it not pass.' Once it had been soundly supported in the Senate where it had been introduced by Ohio Senator Wade, however, it was put before the full House, which also passed the bill with a healthy majority. The bill establishing the land-grant institutions was finally signed into law by President Abraham Lincoln on July 2, 1862.

In recognition of the Senator's dedication to the cause, he has been immortalized on thirteen land-grant campuses through the naming of buildings. Often Morrill Hall was the first or one of the very early buildings on the campus, and usually it housed agriculture. The very first at Cornell University is an example. Only one of the 13 buildings, however, was originally built for Chemistry. Morrill Hall at Washington State University, Pullman, was erected in 1903-4, the fourth building on the campus, constructed of red brick with a basalt foundation. No longer housing chemistry, the building was refurbished in 1981, its original wooden windows being retained.

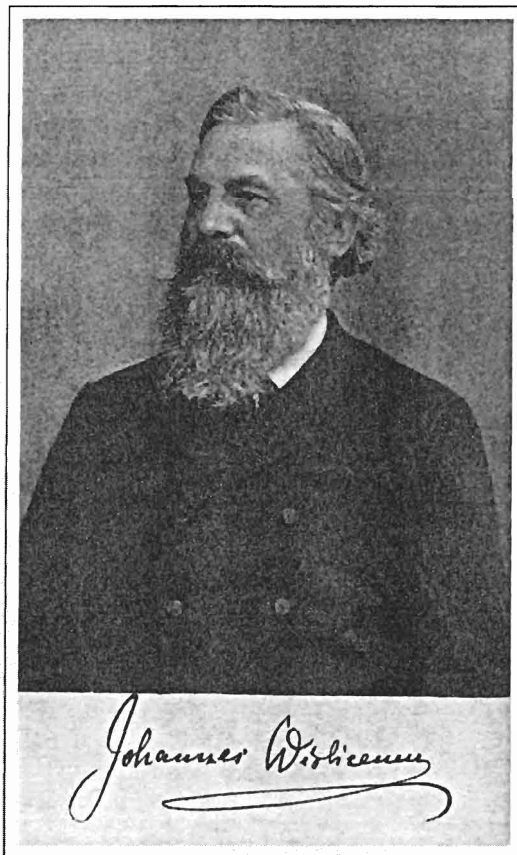


Morrill Hall Chemistry Building, Washington State University, Pullman, WA, erected 1903-1904. Photograph courtesy of Historical Photograph Collections, Washington State University Libraries

THE YOUNG JOHANNES WISLICENUS IN AMERICA

Paul R. Jones, University of Michigan

JOHANNES WISLICENUS (1835-1902) was born in Klein-Eichstedt near Halle, the eldest son of Gustav, a Lutheran minister. He began his studies in chemistry at Halle under Professor Wilhelm Heintz (1817-1880) in 1853 and resumed them between 1857 and 1859. He transferred to Zürich, where he earned his Dr. Phil. under Georg Städeler in 1860. Only one year later he made his *Habilitation*. Wislicenus developed an impressive professional career as chemist, educator, and administrator. He was a chemistry faculty member at the Technische Hochschule (later Eidgenössische Technische Hochschule) in Zürich 1860-1864 and at the University of Zürich 1864-1872, holding both positions simultaneously for part of that period. Then he moved to Würzburg, where he remained from 1872-1885, and finally to Leipzig, from 1885 until his death in 1902. At both Würzburg and Leipzig Wislicenus served as University Rektor. He was instrumental in the founding of chemical societies in Zürich, Würzburg, and Leipzig and served as president of the Chemische Gesellschaft in 1889. In 1898 he was awarded the Davy Medal by the



Chemical Society of Britain. Two of his sons earned doctoral degrees in chemistry under his tutelage: Wilhelm (1861-1922) at Würzburg in 1885 and Johannes (1867-1951) at Leipzig in 1892. Two biographical accounts of Wislicenus' life were written by chemists (1,2). A biographical sketch of Johannes by his son Wilhelm (3) includes some of the same information.

When this writer first became aware of Wislicenus' illustrious career as a chemist, he was struck with the number of English-speaking students who completed doctoral dissertations under his direction at Würzburg and Leipzig between 1878 and 1898 (4). It exceeds the number of any other German chemistry professor, including Wöhler, who was a popular mentor a few decades earlier. Among his progeny was William Henry Perkin, Jr., one of his biographers, who earned his doctorate at Würzburg in 1882. An explanation was sought for the apparently strong attraction to Wislicenus on the part of foreign graduate students. To be sure, his name is less familiar to current chemists and historians of chemistry than that of Liebig, Wöhler, Bunsen, and

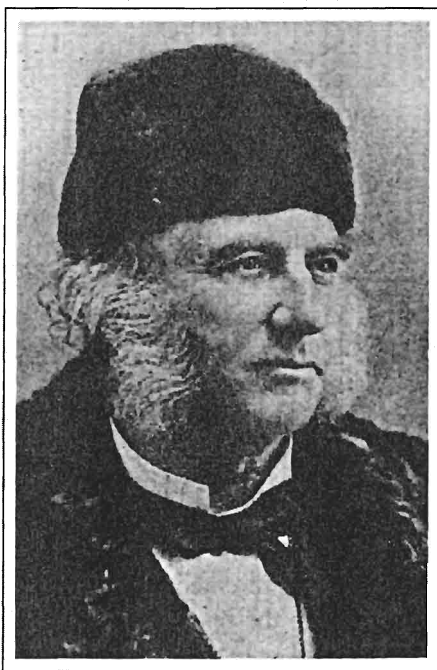
others. Probably his major contribution to chemistry was the promotion of the idea of tetrahedral carbon through his encouragement of a translation of the Dutch edition of van't Hoff's book into German and the design of experiments to test van't Hoff's hypothesis. He can be viewed as one of the earliest of chemists to take a physical-chemical, mechanistic approach to understanding some chemical transformations (5). Yet these accomplishments received only modest recognition during his lifetime and, in fact, were scorned by Hermann Kolbe (6).

For a German chemist in the 19th century, one unique aspect of Wislicenus's life was a two-year residency in the United States, when he was still a very young man. The possibility presented itself that this experience, which might have exerted a lasting impression, may have rendered Wislicenus, later a Professor of Chemistry, particularly sympathetic to young, aspiring chemists who, like himself, found themselves in a foreign land, grappling with a foreign language, and seeking 'parental' guidance.

What about Johannes' two-year stint in America? Why did he go, and what were his experiences? The events leading up to the migration to America are described in an

account by his father Gustav and also summarized in the biographies by Beckmann (1) and Perkin (2). Gustav, the father, was a liberal Lutheran minister, who had been jailed for a time before his marriage because of his radical religious views, which he expressed freely. In 1853, with his publi-

cation of a book entitled *Die Bibel im Lichte der Bildung unserer Zeit* (7), he was charged with undermining religious principles and violating the infallibility of the Bible. Realizing he would not be able to avoid incarceration, he fled Germany for England, where he awaited



Eben Norton Horsford

the arrival of his wife and eight children: nine individuals who had to dispose of their German household belongings and arrange passage to England, the 18-year-old Johannes, the eldest child, making the arrangements. Johannes himself kept a detailed account of his experiences between 1853 and 1856 in a diary, which is often quoted in the biographies by Beckmann and Perkin (8). Once reunited, the family of ten set sail for New York from Liverpool on a three-masted sailing ship, the *Guiding Star*, in October, 1853. A fierce storm along the coast of Ireland destroyed the middle mast. When a cholera epidemic broke out on board, Johannes aided the only physician as interpreter. By some turn of luck, none of the Wislicenus family became ill. After a hectic experience, the ship finally landed in Belfast before being towed back to Liverpool. Eventually the family set sail in November on the mail ship, *Niagara*, headed for Boston, where they landed after two weeks on December 9, 1853. Gustav Wislicenus sent accounts back to his free congregation in Halle. These letters, eventually published as a book entitled *Aus Amerika* (9), portray a rather detailed account of the family's experiences in the New World, including their harrowing sea passage.

The family took up residence in Roxbury, outside Boston, in a house they rented for \$9 a month. Gustav describes the freezing winter of 1854; their association with several Germans; his preaching, which he resumed promptly; attending anti-slavery meetings, often with son Johannes; and a trip to New York, which had been their original destination on the *Guiding Star*.

While the father endeavored to earn a living by preaching and writing, young Johannes somehow secured a position in the Harvard laboratory of Professor Eben Horsford (1818-1893), where he worked six days a week doing iron and meteorite analyses, beginning in March, 1854. His pay was \$1 per day, and he found the work uninspiring, according to a quote from his diary, in which he pined for his home: "Oh, Deutschland!" he wrote, three weeks after beginning his laboratory position. Because the trip from Roxbury to Harvard required one and one-half hours, Johannes took up residence in Cambridge. He became a leader in a gymnastics club and also joined a chorus of fellow Germans.

By May of 1854 the family had moved to West Hoboken, New Jersey, while Johannes stayed behind to continue his Harvard laboratory work for another month. Then, after a scant four months as analytical technician in Horsford's laboratory, Johannes followed his family to New Jersey. His father had already established a private school, and Johannes helped in teaching some of the children while he sought more gainful employment.

To this end he wrote to Horsford in July, 1854, requesting a letter of recommendation (10). An earlier request in late June apparently never reached Horsford, much to Wislicenus's dismay:

I requested you to send me as quick as possible the testimony about my working in Your laboratory, which You promised to be in my possession some days after I left Boston. Now I want it most hastily, because a Professorship for Chemistry in the New York Mechanics Institute has been offered me...

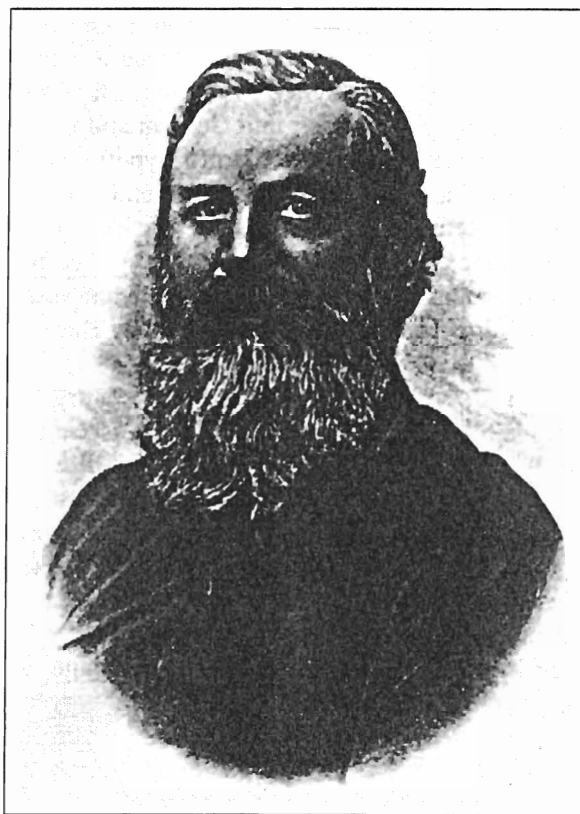
Late in July Wislicenus wrote to Horsford, confirming that he had secured the 'Professorship' at the Mechanics Institute and expected to begin lecturing on practical chemistry in September, 1854. He asked Horsford for the titles of books, particularly on tanning, dyeing, and metallurgy.

The Mechanics Institute had been established in 1820 by the General Society of Mechanics and Tradesmen, an organization which exists to this day, with offices on West 44th Street in New York City (11). The Society Library is in possession of a list of 'lecturers' at the Institute during the span 1835-1868; but, unfortunately, the period 1854-1857 is missing (12). From Johannes' own account in his diary, quoted by Beckmann, he first lectured to a class of 100 on "Chemical Uses of Wood" and later held lectures on tanning. He balanced his responsibilities for lecturing in the evening at the Mechanics Institute with analyses of bark, which he carried out in a factory in Newark in the daytime. He described this work routine in a letter to Horsford dated July 26, 1854 (10). In a fourth letter (13) to Horsford, Wislicenus explains that he was encouraged by a Mr. Detmold, President of the New Jersey Tire Company, to purchase apparatus from the widow of the late Dr. Enderlyn and to set up a chemical analysis business. This caused no end of anxiety, for, as he said to Horsford (13):

...In Germany I scarcely thought of making money. Now these happy days are gone; not for knowledge I work, but only for bread, and the love of science had to succumb to the mere necessities of life....

According to the account in his diary (quoted by Beckmann), Wislicenus had been promised \$80 for a set of analyses but only received \$60; in another instance the expected \$40 amounted only to \$25.

In these letters Wislicenus portrays a distinct disillusionment with the American life, mixed with homesickness for his beloved Germany. Nevertheless, by January 1855, he had been appointed "Professor" at the Mechanics Institute and so had a secure position, if only part-time.



Johannes Wislicenus

In the summer of 1855 Johannes composed a lengthy letter (14) to Luise Rade, a friend of his and of his sister Klotilde. [The use of *Freundin* implies more than just a family acquaintance.] It is a love letter of sorts, in which he reminisces on good times they had enjoyed in Germany; but then he proceeds to describe some of his experiences in New York, most notably his participation in a German songfest held in Manhattan in June, 1855. It must have been a gala affair. He reported that 1,500 German singers disembarked one evening at the Battery and then formed a flag-waving, jubilant procession up Broadway to City Hall. Much to his own delight, Johannes was reunited with some of his acquaintances from Boston, from whom he had departed just a year earlier. The concert was held the following evening in the Metropolitan Theater. Johannes enumerates all the offerings presented by the massive chorus and orchestra, from the opening with Wagner's *Rienzi* overture to a final selection by the New York Singing Club. Later in the week 40,000 attended a picnic in Elm Park.

...There was a great deal of singing and playing of music, dancing, eating, drinking, joking, laughing, chatting, handshaking. All was happy and optimistic, and no one caused any kind of disturbance for which to be ashamed. Among the 40,000 people who were present, I saw no one inebriated; and the only

ones arrested were American pickpockets..That was the wonderful New York singing festival, a genuine German festival!..

In the spirit of his euphoria over the German song festival, Johannes advances a prophetic remark:

...What celebrations we shall hold in Germany, as soon as we have the opportunity. The Germans, so similar to the Greek people, will revive the Olympic Games...

Although the Olympic Games were indeed revived, largely through the efforts of the French, they were nevertheless reinstated—40 years after Wislicenus's suggestion. There is some irony in the fact that Germany's role in the Olympics, however, has been shrouded with misfortune: the 1916 games were canceled during World War I; the 1936 Olympics are identified with the rise of Hitler; and the Munich Olympics in 1972 are remembered more for the massacre of Jewish athletes than for the gold medals.

His homesickness and loyalty for Germany are expressed again and again in this outpouring to Luise. He deplores the fact he has found no such magnificent evening sky nor any such beautiful woods as in his homeland. He describes the Catskills as "...tall and thick but otherwise neglected;...the restful beauty of our woods is totally lacking; one sees only a jumble of everything straight, tall, or downfallen trees..."

In August of 1855 Johannes traveled to Brattleboro, Vermont, to visit the Wesselhoeft family. From there he went with Conrad Wesselhoeft to Boston. In the last known letter written to Horsford (13), which Johannes began in Hoboken on February 22 but finished on March 10, 1856, he offers condolences to Horsford on the death of Horsford's wife, who had passed away in 1855. He then goes on to express his consternation over Horsford's purported low opinion of Wislicenus:

...It has given me great affliction to hear some friends in Boston, and chiefly Mr. Schenkl when he for the last time came to New York, say what an idea you seem to have formed on my long silence to you. Mr. Schenkl, on the authority of Mr. Hoffmann I believe, has spoken at a public place on severe judgements (sic) which you had pronounced on my 'ungratitude' towards you...

Wislicenus describes his attempt to make amends with Horsford during his visit to Boston:

...I went to Cambridge to beg you personally for pardon if the rumor should prove true. In Main Street I found you standing on the sidewalk together with

another gentleman. When I came near to you, you and I only turned the head and looked at us (my friend Conrad Wesselhoeft being at that time with me), but momentarily (sic) you went away taking your way through the college grounds. My hopes vanished. I passed by the laboratory without calling in. Two days after I left Boston with the most painful feeling, and arrived home with remorse that I nevertheless had been at your house and spoken to you...

Although Johannes closes his letter with a promise to write Horsford again soon, there is no record of any further correspondence from the young chemist.

By May of 1856 conditions had changed sufficiently, that the entire Wislicenus family returned to Europe—not to Halle, however, where they were not welcome, but to Zürich, where Gustav, his wife, and several of the children remained. Johannes resumed his chemistry studies in Halle but eventually was awarded the doctorate in Zürich. Johannes faced several difficult obstacles in his career; yet he must have rejoiced in the return to his beloved country. He seldom made reference to the 'Wanderjahr' in America, perhaps because it conjured up painful memories. It seems he did not keep up communication with Horsford, anyone associated with the Mechanics Institute, or with the analytical laboratories where he worked in New Jersey. Horsford makes no mention of the young Wislicenus in his correspondence with Liebig (15). It is as if the two-year period in America were a bad dream for Johannes Wislicenus, which he blotted from his memory.

REFERENCES AND NOTES

1. E. Beckmann, *Ber. Dtsch. Chem. Ges.*, **1904**, 37, 4861-4946.
2. W. H. Perkin, Jr., *J. Chem. Soc.*, **1905**, 87, 501-534.
3. W. H. Wislicenus, *Lebensläufe aus Franken*, **1922**, 2, 500-512.
4. P. R. Jones, *Bibliographie der Dissertationen amerikanischer und britischer Chemiker an deutschen Universitäten, 1840-1914*. Deutsches Museum, Munich, 1983.
5. P. J. Ramberg, *Bull. Hist. Chem.*, **1994**, 15/16, 45-53.
6. A. J. Rocke, *The Quiet Revolution*, U. California Press, Berkeley, CA, 1993. pp.29-330.
7. G. Wislicenus, *Die Bibel im Lichte der Bildung unserer Zeit*, Fabricius, Magdeburg, 1853.
8. Considerable effort to locate Johannes Wislicenus's diary, through inquiries of several of his descendants, has been unsuccessful.
9. G. Wislicenus, *Aus Amerika*, Leipzig, Otto Wigand, 1854; New York, C. F. Guenther.
10. Letters from Johannes Wislicenus to Eben Horsford,

Archives, Rensselaer Polytechnic Institute Archives, Box 6.

11. C. A. Bennett, *History of Manual and Industrial Education up to 1870*, Peoria, IL, Manual Arts Press, 1926.
12. Private Communication, General Society of Mechanics and Tradesmen Library, March, 1994.
13. Letter from Johannes Wislicenus to Eben Horsford, Archives, Rensselaer Polytechnic Institute, Box 8, Folder 3.
14. "Brief von Johannes Wislicenus an Luise Rade", private collection, Wislicenus family.
15. Liebigiana, Bayerische Staatsbibliothek, Munich.

ABOUT THE AUTHOR

Paul R. Jones, Professor Emeritus of Chemistry, University of New Hampshire, is Visiting Scholar in the Department of Chemistry, University of Michigan, where he also serves as Editor of the *Bulletin*.

MAILING LIST HISTORIANS OF CHEMISTRY

A mail distribution list is being set up to exchange information related to history of chemistry. If you would like to be included, communicate by e-mail to MAISER@LISTSERV.NGATE.UNI-REGENSBURG.DE, with the following command: CHEM-HIST. For further details, contact Dr. Christoph Meinel: christoph.meinel@psk.uni-regensburg.de.

1998 PAUL BUNGE PRIZE

Applications are invited for the 1998 Paul Bunge Prize to recognize outstanding publications in all fields of the history of scientific instruments. Papers may be in German, English, or French. The application should include, in addition to the scientific paper, a CV and list of publications of the applicant. Information is available at the German Chemical society, Public Relations Dept. P.O. Box 900440, D-60444 Frankfurt/Main; pr@gdch.de; FAX: + 69 7917 322. Deadline is September 30, 1997.

“BUT SHE’S AN AVOWED COMMUNIST!” L’AFFAIRE CURIE AT THE AMERICAN CHEMICAL SOCIETY, 1953-1955

Margaret W. Rossiter, Cornell University

Introduction

One might have expected that the American Chemical Society (ACS), an organization that claimed to be working for the advancement of chemistry and not using political tests for its membership, would readily accept an application from a Nobel laureate in chemistry. Yet this was not the case with Irène Joliot-Curie in 1953. After some consternation ACS officials rejected her membership application because of her political reputation (strongly linked to the pro-Communist beliefs and activities of her husband, Frédéric Joliot-Curie), informed her of the decision but gave no reasons, and said nothing of their action publicly. When months later her friends questioned and publicized her rejection, the case became a cause célèbre. The extensive commentary and correspondence surrounding the episode make it possible to gauge some contemporary reaction to the wisdom, handling, and significance of this decision. When compared to one of the other anti-communist “witch-hunts” in the United States in the 1940s and 1950s, the public harassment of leading members of the American Association for the Advancement of Science (AAAS), one can see the different reaction. Whereas the AAAS’s board of directors responded stoutly to the anti-communist crusade by electing E. U. Condon and Kirtley Mather presidents (1), the leaders of the ACS refused to elect Madame Joliot-Curie even to membership. “L’Affaire Curie,” as it came to be called, also revealed structural tensions within the ACS between the protective instincts of the members of the Board of Directors and the concern for political liberties among the leaders of some sections. The rank and file readers of scientific publications also revealed a wide range of reactions.

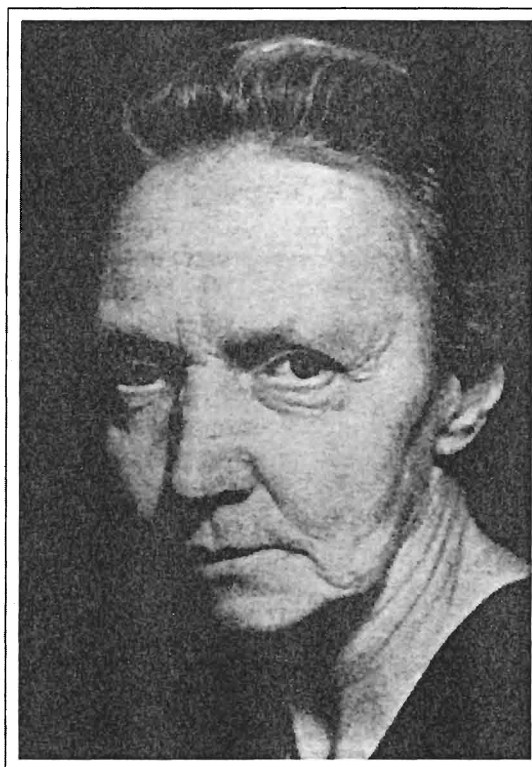


Figure 1 Irène Joliot-Curie (1897-1956). Shown here late in life, Joliot-Curie shared the Nobel Prize in Chemistry with her husband Frederic in 1935. Intensely apolitical in her early life, she became more involved in French women's, socialist, and pro-Communist movements starting in the late 1930s. (Reprinted from Nuclear Physics 4[1957], p. 497)

The Facts

In March, 1953, French chemist and Nobel Laureate Irène Joliot-Curie applied for membership in the ACS, one of the largest scientific associations in the world with 68,000 members, in order, as she claimed, to receive the *Journal of the American Chemical Society* regularly. She filled in the application form and had, as was required, two current members in good standing vouch for her moral character by signing the form. The form did not ask about her political beliefs or affiliations, as the ACS was quite proud that it did not take such factors into consideration. Its admissions committee generally considered credentials like degrees in chemistry as sufficient evidence of an interest in advancing the field. Since the Society claimed that it did not discriminate against applicants on the basis of race, color, religion, or political beliefs, its staff and committee did not investigate these areas (2). But the Society did require that members be of a high moral character. Since the admissions committee could not investigate this aspect of every application, this was to be vouched for by the signatures of two current ACS members, which Joliot-Curie had done.

But, despite her high standing as a chemist (and she was later greatly relieved to learn that the value of her chemical work had never been in doubt), the Committee on Admissions of the ACS rejected her application on July 24, in a letter signed by Norman Bekkedahl of the National Bureau of Standards, the chairman of the committee (3). In September Madame Joliot-Curie wrote Alden Emery, the executive secretary of the ACS, to ask the reason why. (Her friends later told Linus Pauling that she had assumed that they considered her work to be physics rather than chemistry (4). She had been elected to the American Physical Society in November 1950 (5). Emery did not respond, though he could and should have informed her that there was a standard procedure for appeals of admissions decisions (6): she could appeal to the ACS Council's Committee on Membership Standards, which set membership policy and heard appeals. She never pursued this route.

Instead when after a month she had heard nothing from Emery, in October, 1953 a concerned friend of hers, Moise Haissinsky of the Radium Institute in Paris, who had himself in 1950 been refused a temporary visa to attend a meeting in the United States, wrote a letter to the editor of another American journal with a special interest in issues of science and politics, the *Bulletin of the Atomic Scientists* (*Bull. At. Sci.*). Its editor Eugene Rabinowitch notified Emery that he would publish the French letter after January 1, and invited him to respond.

Accordingly Haissinsky's letter appeared in the February, 1954 issue, along with a carefully crafted explanation by Emery, who had been in close communication with several members of the ACS Board of Directors. The *New York Times* had a story about it on February 15, the *Christian Science Monitor* one on February 20, *Time* magazine an item about it on its "People" page February 22, and *Science* re-

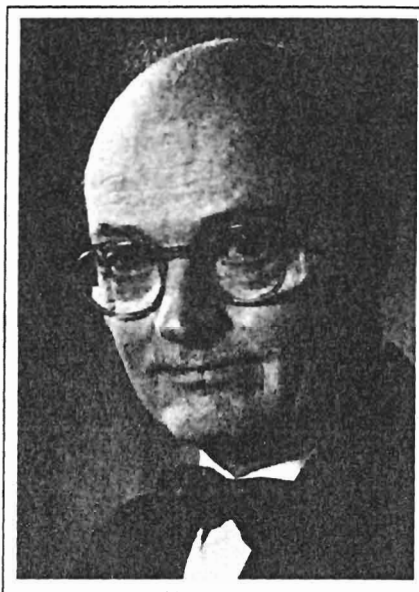


Figure 2 Alden H. Emery (1901-1975). After thirteen years at the U. S. Bureau of Mines, he moved in 1936 to the American Chemical Society, where he served in a series of administrative posts, including that of executive secretary from 1947 to 1965. (With the permission of the American Chemical Society)

printed both letters in its March 12 issue (7).

Yet the controversy was not brought to the attention of ACS members until the March 22, 1954 issue of its weekly *Chemical & Engineering News* (*Chem. Eng. News*), when editor Walter J. Murphy published both a statement by the executive committee of the ACS's Board of Directors that supported the admissions committee's action and three letters-to-the-editor on the subject (8). Over the next two months (that is, April and May, 1954) the issue provoked a considerable response in both journals, with 26 more letters to the editors (twelve in support of the action and fourteen in opposition). Finally in the May 17 issue of the *Chem. Eng. News* Murphy called a halt to it, saying that further comment was unlikely to resolve the issue any further (9). Thus ended the first phase.

The Reasoning

There were several issues involved here. First the timing—the spring of 1953 was the height of a series of congressional committee investigations by the U.S. Senate into communism on American campuses. Several hundred faculty members were being subpoenaed to testify; the hearings were highly publicized; and, as has

been documented recently, administrators at most universities fired their controversial cases as soon as possible rather than rally to any defense of academic freedom (10). Even closer to home — at headquarters in fact — Walter Murphy had fired ACS News Service staff member Robert Norton in April or May, 1953 after he had refused to testify



Figure 3 Walter J. Murphy (1899 - 1959). Trained as a chemist, he worked for a series of chemical companies before becoming one of the first chemical editors in 1930. He served as editor of *Chemical and Engineering News* from 1943 until his death. (With the permission of the American Chemical Society)

before Senator Jenner's committee (11). Thus Emery and the ACS admissions committee suspected that the pro-communist Madame Joliot-Curie was using them as a kind of timely test case. Not wishing to be so used, they turned her down quietly, did not publicize their decision, and did not respond to her inquiry, possibly in hopes that the problem would go away unnoticed—which it did for a while.

Another factor that rankled the ACS executive secretary was that about this time the Society's committee on publications was introducing a new rate structure for the ACS's several journals. Formerly every ACS member had received *Chemical Abstracts* as well as the *Chem. Eng. News* and the *Journal of the American Chemical Society* (*J. Am. Chem. Soc.*). Now *J. Am. Chem. Soc.* was to have a new rate structure: nonmembers and institutions ("subscribers") were to be charged considerably more than individual members of the ACS (12). Emery and his staff had noticed that they were getting increasing numbers of applications from individual chemists whose companies were anxious to evade the new corporate subscription rate (13). This they suspected Joliot-Curie of doing as well. There was not much the

ACS could do about this, which made the staff angry that they were being taken advantage of. Thus Alden Emery had initially retorted that she did not have to be a member of the Society to get the journals; she could just subscribe to the journal, as did many individuals and laboratories (14), and pay the higher fee.

Thus though technically Joliot-Curie was qualified—she had filled in the form fully and she had obtained the necessary signatures—her timing and various possible ulterior motives also irritated the staff and the Committee did not dare admit her. Yet how would they explain rejecting her? Emery's chief justification for rejecting her was that someone on the Admissions Committee had said that she was an "avowed and active Communist." To admit her might not only cause the Society adverse publicity, but it would also violate the Society's federal charter, a matter of greater concern to the Board of Directors than to the Council. The charter's passage by Congress and adoption by the ACS in 1937 committed the Society to advancing chemistry, improving American industries, and cooperating with the federal government when needed. It thereby also gained the Society a tax exemption (not only from federal taxes, which had apparently been

possible under its previous New York state charter, but also from District of Columbia real estate ones as well as. This was important, since in 1940 the Society purchased and remodeled an apartment house for its headquarters there.) Under the charter the ACS became a corporation whose property was owned and controlled by the Board of Directors and it was required to report annually to the U. S. Con-



Figure 4 Charles A. Thomas (1900-1982). After coming to Monsanto in 1936 he held a variety of executive posts including its presidency. In 1948 he was president of the ACS and a member of the board of directors in 1950-53, where he was its chairman in 1953 when Joliot-Curie applied for membership. (With the permission of the American Chemical Society)

gress (15). Yet citing the federal charter was a new factor in admission decisions, for the federal charter had not stopped the Society from admitting foreign members before. In fact, as Rabinowitch pointed out in an editorial on the subject in March, 1954, many American learned societies and professional organizations already had foreign and even Soviet members (16). (The 1951 membership directory of the ACS listed 2,908 foreign members of 64,241 individuals with 88 from France, 71 from eastern Europe, 22 from China, and none from the Soviet Union (17).) But as no one knew or had inquired about their political views, their continued membership was not thought to threaten the Society or violate the charter the way publicly electing even a suspected Communist to the ACS would in 1953.

In early March, 1954, as word of the episode spread but before the *Chem. Eng. News* provided more of the details, S. C. Lind, a former (1940) ACS president, long at the University of Minnesota but now a consultant at the Oak Ridge National Laboratory, wrote the recent president Farrington Daniels of the University of Wisconsin, asking why the Board of Directors had been consulted and not the Council's Committee on Membership Standards, which set criteria for admissions and to which the committee on admissions usually reported. He saw this as one more example of the Board's encroachment upon the Council, which had before 1937 governed the society. Lind thought that under the circumstances losing the federal charter might be a blessing, as he was sure that in this case the Council would have made a better decision than the Board had (18). Daniels was taken aback, since in the haste and panic of the crisis no one had pursued this procedure. [In November, 1953, Emery had mentioned it as one possibility but did not object when Daniels and others wanted to consult with the Board, which was small, would be meeting soon, and had in the past dealt with other touchy issues, as Linus Pauling's passport denial in 1952 (19).] Yet this very bypassing of normal procedures to gather support from the elders of the society rather than the more representative and less predictable Council shows just how worried the ACS leaders felt.

In its March, 1954, statement published in the *Chem. Eng. News*, the executive committee of the Board of Directors fell back on the nuance that membership in the ACS was an honor and not a right. Thus someone known (however it was determined and whether accurate or not) to be committed to the overthrow of the federal government could not be accepted. To admit such a person would, they feared, endanger the Society's very existence. Rejecting Irène Joliot-Curie's application also

offered the advantage that it could be done quietly, or, as statement by the executive committee of the Board of Directors explained, it was standard Society policy to keep the names of persons denied membership private, as it would hurt them unnecessarily to publish them and would open the society to possible slander action (20). Yet Irène Joliot-Curie did not acquiesce to such silence and outmaneuvered both the ACS committee and the board. In fact if her goal had been revenge by exposing the officials' narrow-mindedness, she must have been gratified with the response both privately (for many chemists wrote their sentiments either directly to her (21) or to the ACS and its officers (22) and publicly in the pages of the *Chem. Eng. News*, the *Bull. At. Sci.*, and the newsletters of two important ACS sections.

The Response

A few of the published letters expressed irritation, as one might have expected, about the ACS's delay and procedures in not informing its own members more directly about an action by their own governing boards. The episode had all the signs of a "cover up" finally exposed months later. Rabinowitch deplored the decision and noted in the *Bull. At. Sci.* that the ACS would receive much adverse publicity in the pro-Soviet press, as still more proof of the intolerance of America's so-called democratic institutions (23).

Beyond this, on the whole, the letters Murphy printed in the *Chem. Eng. News* were about evenly divided as to the Committee's action. Those in agreement with the rejection of Joliot-Curie offered a variety of arguments. [One suggested that anyone who did not believe in God should not be a member of the ACS (24).] Chiefly many fervently believed that the Communist Party was not the usual sort of political party that Americans who believed in democratic institutions were used to. Communism was instead a potent even subversive political belief system. (The Supreme Court upheld this view as late as 1959.) If even one well-known Communist were to be elected to the ACS, the other 68,000 members would also be immediately suspect. This political contamination (she did work on radioactivity!) might bring the whole organization under scrutiny, congressional or otherwise, as a hotbed of communism. Then all members would be labeled as potentially subversive, for how could anyone prove that he/she was not a communist, especially when the real communists were under strict party discipline and known to lie when necessary, even under oath (25). It was thus the duty of the members, as represented by its vigilant Committee on Admissions, to protect the good name of the ACS by

rejecting Joliot-Curie, Nobel laureate or not, and the presumed internationality of science notwithstanding (26).

The critics were less fearful and more procedurally minded, pointing out that 1) the Committee on Admissions had no official information on her or any applicants' political beliefs, which were in any case, not permissible criteria for exclusion; 2) as she could not under prevailing conditions get a visa to enter the United States, she was very unlikely ever to come to a meeting; and 3) the ACS committee could not vouch for the Americanism of any of its current members—there might well be many Communists among them already. Paul Flory then of Ithaca, New York, an outspoken advocate of political freedom and himself later a Nobelist, suggested that someone should, in fact, investigate the loyalty of those voting to exclude Madame Joliot-Curie (27)! Others argued that whether or not communism was a subversive philosophy, Joliot-Curie was no threat to other members, precisely because her leftist political beliefs were so well known. One such person was such a small proportion of the whole immense and diverse ACS that she hardly mattered. David Todd of Shrewsbury, Massachusetts, one of her staunchest and most persistent defenders, asserted that she should not be confused with T. D. Lysenko, who held and enforced distorted scientific views in the USSR (28). The real danger to the society was from secret Communists, who would lie about their affiliations and beliefs, if asked, anyhow. Several ACS members reportedly resigned over the incident, including nuclear chemist Charles D. Coryell of the Massachusetts Institute of Technology (29). [If he did resign, he rejoined later, because he was listed in the ACS membership directory for 1956. He also won a prize from the ACS in 1960 and spent a year at the Radium Institute in 1963 (30).]

In retrospect the most judicious immediate reaction to the episode was the editorial on "The Chemist and the Communist" in the April, 1954 issue of the *Chemical Bulletin*, the newsletter of the Chicago section, third largest, of the ACS. Its editor Robert N. Feinstein, an assistant professor of biochemistry at the University of Chicago and so probably known to Eugene Rabinowitch, said that the whole affair had been badly handled. He summarized the dilemma succinctly, "Mr. Emery's statement is reasonable and understandable. And still we deeply regret it." Adding that "We are not here in any way condoning communism," he claimed that because the Society did not consider race, color, creed, or political views as criteria for membership and already had many other foreign members, "We can only regret with all our heart that the American Chemical

Society, by its rigidly conformist interpretation of a debatable section of the ACS Charter, has been instrumental in drawing just a tiny bit tighter the band of continuing encroachments on our sphere of free activities" (31). Endorsing these sentiments, Rabinowitch reprinted the editorial in the June 1954 issue of the *Bull. At. Sci.*, along with several other letters to the editor and a summary of those in the *Chem. Eng. News* (32).

Thus most of the discussion did not revolve around any practical fear of how her ideas or physical presence might threaten members of the society or the field of chemistry. It was generally in the political realm of what the welcoming of a known Communist in 1953 or 1954 would do to the Society's political fortunes, legal standing, and reputation, a greater concern to most of



Figure 5 Farrington Daniels (1889-1972). On the faculty at the University of Wisconsin from 1920 to 1959, he won numerous awards and was president of the ACS in 1953 when the cause celebre unfolded. (With the permission of the American Chemical Society)

the society's officers (whose presidents Arnold Thackray has termed an "elite gerontocracy") than to the more populist members of some of the local sections (33). Rather than feeling strong enough to stand up to outside criticism and select members solely on their scientific fitness, the society's leaders, nearly unanimously (34), felt the need to reaffirm their patriotism—her application had even put their own loyalty under suspicion. Yet ironically far from protecting the integrity and internationality of science at a time of danger, the actions of the ACS staff and leadership seem to have, as one critic pointed, out dropped to the level of the Communists themselves. If a Nobel laureate in chemistry could be excluded from an American society presuming to advance that field, then the integrity of many other American scientific organizations was also put into serious doubt (35).

Aftermath

"L'Affaire Curie," as Rabinowitch dubbed it, dragged on into 1955. In August, 1954 the editor of the *Chem. Eng. News* broke his own earlier ban on the topic by printing a letter from Irène Joliot-Curie to David Todd, who in his efforts to guide the discussions of the issue in the Central Massachusetts section (Worcester), had inquired directly if she had told the ACS admissions committee that she was a member of the Communist Party. She replied that she was not a member, but added that this was a mere technicality as she believed totally in its tenets and her husband was an active member. She then expatiated on how hard she personally strove to overlook other scientists' political views, how warmly she welcomed scientists of all nations (including even the United States) into her laboratory, and how deplorable she felt the recent behavior of the ACS was, coming as it did from fellow scientists, who should be above such things. The *Bull. At. Sci.* reprinted her letter in October, 1954 (36). In it she did not mention that since 1949 she and her husband had been urging Chinese scientists to convince Chairman Mao Zedong's communist regime to build atomic and hydrogen bombs (37).

Meanwhile Alan C. Nixon, a research chemist at Shell Research and Development Center at Emeryville, Chairman of the California section of the ACS, headquartered then in nearby Berkeley, and in the 1970s an ACS president in his own right, kept the issue of Joliot-Curie's exclusion alive in *The Vortex*, the section's monthly journal, throughout 1954. Already known for his liberal views, he thought it was an outrage that she had been rejected and kept pressing the national leadership for more specific reasons. He also engaged Berkeley professor Joel C. Hildebrand, a prominent member and former chairman of the California section as well as the incoming president of the ACS, in several exchanges on the matter in *The Vortex* (38).

Hildebrand was no stranger to such issues, since a few years earlier as dean of the University of California's College of Chemistry he had had to deal with the thorny issue of the Regents loyalty oath. In the spring of 1950 he had even chaired the faculty advisory committee on the issue for the three northern campuses (Berkeley, San Francisco, and Davis) to President Sproul. When he urged recalcitrant faculty members to sign the revised oath, he emerged as an accommodater and compromiser rather than a diehard political purist (39).

In the course of his correspondence with on the one hand the liberal Alan Nixon and on the other with the illiberal ACS staff, Hildebrand struggled with the issues and dangers. Basically he worried less about the federal

charter than had Emery and more that if Madame Joliot-Curie were accepted for membership, she would be able to vote for future ACS officers. Finally in December, 1955 he proposed that the ACS Council and Board of Directors should revise the organization's current constitution to strike a compromise of sorts. The National Academy of Sciences, of which he was also a member, and which also had a federal charter, had foreign members who did not vote for the officers. It thus managed to look international but did not jeopardize its national interests in allowing the foreigners a chance to control the organization. Thus the ACS could do likewise—elect foreign members, whose political loyalty and moral character could not be vouched for, to non-voting membership status. They could then be members and get the journals at the reduced rate, but there would be no chance of their taking over the leadership (40). Thus the ACS began to develop a more open approach to international members, whose numbers were growing rapidly, including 358 applications from Japan alone in 1955 (41). In 1956 it even had one member from the Soviet Union, Anatoli Kreshkov (42).

But by the time the ACS had revised its membership rules, the woman who had started all the fuss had taken to her bed with her final illness. Irène Joliot-Curie died from leukemia in March, 1956 (43).

Analysis and Conclusion

Beneath the particulars of this case are several underlying issues relating to the role of scientific societies in the 1950s: in tying subscription rates to ACS membership status, they could to a certain extent control access to inexpensive scientific information; in choosing some but not other foreign members, they could shape the internationalization of chemistry; in withholding information on association activities, they could protect their leaders from criticism; while in publishing letters-to-the-editor they could air the members' differing political views.

Yet this was a highly unusual case. Most studies in the growing literature on "McCarthyism" in science focus on the harassment of American scientists, especially those in the physical sciences, and including, perhaps even featuring, many very eminent ones, as Nobel laureates, by both political figures, as congressmen, federal bureaucrats, especially passport officials and grants officers, who are usually not scientists, and university administrators, a few of whom were former scientists, even chemists (44). As these accounts document the imposition of loyalty oaths, the denials of federal grants, the withholding of passports, the issuing of subpoenas

to testify before congressional committees, and even job suspensions and firings, they usually have the flavor of pitting the "Davids" (like Condon or J. Robert Oppenheimer) against the "Goliaths" (the government officials or university administrators) (45). "L'Affaire Curie" was an unusual and more disturbing case in that here a scientist—a foreigner as well as a woman and a Nobelist—was rejected not by narrow-minded American bureaucrats [though she had been detained overnight at Ellis Island by State Department officials in 1948 when she came to the United States on a lecture tour(46)] but by her fellow chemists, the officers (all male and no Nobelists) of the ACS, for mere membership in their scientific organization. She in turn was quite angry at her fellow scientists, whose behavior reflected narrow chauvinism and even political cowardice rather than the lofty ideals of science, while at the same time limiting her access to certain valuable informational resources.

Yet in the complex world of American science in the 1950s, the leaders of this one scientific society—admittedly a large one with substantial resources and particularly close ties to the nation-state via its charter and tax-exempt status—felt that they had to protect the best interests of both their society and American chemistry and thus had to accede to prevailing political realities. What may have been different about this case was that, unlike the untold numbers of others that were successfully covered up, Madame Joliot-Curie was able to bypass her censors and bring it to public attention, thus revealing the wide structural and ideological divisions among American chemists. Perhaps the biggest surprise was that she got as much public and private support as did.

REFERENCES AND NOTES

1. Wang, "Science, Security, and the Cold War: The Case of E. U. Condon," *Isis*, **1992**, 83, 238-69; D. Wolfle, *Renewing a Scientific Society, The American Association*, AAAS, Washington, DC, 1989, 16-20, 44-45, and 283-84n14; and K. B. Bork, *Cracking Rocks and Defending Democracy: Kirtley F. Mather*, AAAS, Pacific Division, San Francisco, CA, 1994.
2. The ACS's executive secretary Alden Emery, however, revealed that one of his staff members periodically showed a list of foreign applicants to the scientific attaché of the relevant embassy to find out what he knew about the individuals involved. ("Membership-Foreign," American Chemical Society Papers, Box 26, Manuscript Division, Library of Congress, Washington, DC). The extent of the ACS's ties to the State Department is not clear.
3. On his way to the dedication of the Weizmann Institute in Israel in October 1953, Linus Pauling visited Madame Joliot-Curie in Paris. She showed him her rejection letter, and he wrote Farrington Daniels of the University of Wisconsin and President of the ACS, urging him "to rectify this without delay" (Linus Pauling to Farrington Daniels, October 28, 1953, in Farrington Daniels Papers (#7/6/14-2) at the University of Wisconsin Archives, Madison. Much of the correspondence in the Daniels Papers refers to items deliberately omitted from the official minutes of the meetings of the ACS Board of Directors. Pauling referred to the incident again in a letter to David Todd, October 19, 1959; Linus and Helen Pauling Papers, Oregon State University Library, cited in T. Hager, *Force of Nature, The Life of Linus Pauling*, Simon & Schuster, New York, 1995, 435. It is also of interest that Bekkedahl won a meritorious service award from the besieged Commerce Department in 1954 (*American Men of Science [AMS]*, 12th ed. 1971, vol. 6, 390).
4. Linus Pauling to Farrington Daniels, December 17, 1953, quoted in O. B. Daniels [with considerable help from Anthony Stranges], "Farrington Daniels, Chemist and Prophet of the Solar Age, A Biography," typescript, 1978, 287, copy at University of Wisconsin Archives. I thank Anthony Stranges for telling me of this.
5. *Ibid.*, p. 285n11.
6. Alden Emery to Farrington Daniels, November 6, 1953, Box 5 ("Joliot-Curie Controversy, 1953"), Daniels Papers.
7. M. Haissinsky to Editor, *Bull. At. Sci.*, February, 1954, 10, 56; "U. S. Chemists Bar Mme. Joliot-Curie," *New York Times*, February 15, 1954, 10:1; R. C. Cowen, "Chemical Society Faces Joliot-Curie Controversy," *Christian Science Monitor*, February 20, 1954, 5:3; "People," *Time*, February 22, 1954, 48:1; "Science News," *Science*, March 12, 1954, 119, 340-41. See also H. Skolnik and K. M. Reese, *A Century of Chemistry, The Role of Chemists and the American Chemical Society*, American Chemical Society, Washington, DC, 1976, 42-43, who called it "the most acrimonious debate of the 1950s." It is disturbing that almost all we know about the history of the ACS comes from histories that it commissioned. The three to date make it one of the most written about of scientific societies, however.
8. "Board of Directors' Statement on Election to Membership in the ACS," *Chem. Eng. News*, March 22, 1954, 32, 1146. The chairman of the Board of Directors in 1953, when the statement was drafted, was Charles A. Thomas, President of the Monsanto Company of St. Louis. Among the other members of the Board were John C. Warner, President of the Carnegie Institute of Technology, Ralph Connor, Vice-president for Research at Rohm and Haas, Raymond Kirk, Dean of the Graduate School at the Polytechnic Institute of Brooklyn, and Arthur C. Cope, Chairman of the Chemistry Department at the Massachusetts Institute of Technology. (H. Skolnik and K. M. Reese, *op. cit.*, 395).

9. "Letters—Joliot-Curie Membership," *Chem. Eng. News*, May 7, 1954, 32, 1966.
10. For example, E. W. Schrecker, *No Ivory Tower, McCarthyism at the Universities*, Oxford University Press, New York, 1986; S. Diamond, *Compromised Campus The Collaboration of Universities with the Intelligence Community 1945-1955* Oxford University Press, New York, 1992.
11. Ernest H. Volwiler to Walter J. Murphy, May 8, 1953, in Farrington Daniels Papers, Box 1 ("ACS Board of Directors").
12. For example, "Subscription Rate Increase I," *Chem. Eng. News*, 1955, 33, p. 2836. See also R. Shank, "Physical Science and Engineering Societies in the United States as Publishers, 1939-1964," unpublished D.L.S. dissertation, Columbia University, 1966; and R. G. Lerner, "The Professional Society in a Changing World," *Library Quarterly*, 1984, 54, 36-47.
13. See especially Alden Emery to K. K. Darrow, October 29, 1952, Box 25, American Chemical Society Papers, for a complaint that members are abusing their right to get copies of *Chemical Abstracts*. Other material in Boxes 25 and 26 is relevant to the Joliot-Curie affair and its aftermath, but the collection is far from complete and perhaps even more notable for what is not there. Some memos, for example, contain cross-references to files (such as "Joliot-Curie, I., Reactions Unfavorable") that are not present.
14. Alden Emery, "Letters to the Editor," *Bull. At. Sci.*, February 1954, 10, 56.
15. "An Act to Incorporate the American Chemical Society," U. S. Statutes at Large, 75th Congress, 1937, 50, Pt I, 798-800. See also H. Skolnik and K. M. Reese, *op. cit.*, 24-26, and "Federal Charter Strengthened ACS Role on the National Stage," in J. L. Sturchio and A. Thackray, "Chemistry and Public Policy," *Chem. Eng. News*, March 9, 1987, 65, 22-23. (I thank Jeffrey Sturchio for this reference and other helpful advice.)
16. E[ugene] R[abinowitch], "The American Chemical Society and Madame Joliot-Curie," *Bull. At. Sci.* March 1954, 10, 67. For obituaries of Rabinowitch see *New York Times*, May 16, 1973, 50 and *Bull. At. Sci.*, June 1973, 29, 3, and "A Voice is Stilled," *ibid.*, 4-12 for excerpts from eulogies.
17. "Summary of Geographical Distribution," *American Chemical Society Diamond Jubilee Directory, 1951*, ACS, Washington, DC, 1951, n.p.
18. S. C. Lind to Farrington Daniels, March 3, 1954, Daniels Papers, Box 5. Lind: *AMS*, 10th ed., 1961, 4, 2429.
19. Alden Emery to Farrington Daniels, November 6, 1953, Daniels Papers, Box 5, and Alden Emery to Farrington Daniels, June 10, 1952, Box 1.
20. "Board of Directors; Statement on Election to Membership in the ACS," *Chem. Eng. News*, March 22, 1954, 32, 1146.
21. N. Loriot, *Irène Joliot-Curie*, Presses de la Renaissance, Paris, 1991, 285.
22. Also in the Daniels Papers (Box 5) is a list of "Members who protested [privately to the ACS the] action on Madame Joliot-Curie's application" with 15 names and the national and Philadelphia chapter of the American Association of Scientific Workers, but their actual letters are not there. The only one of the fifteen who also had a letter published in *Chem. Eng. News* or *Bull. At. Sci.* was David Todd. Listed were G. B. Carpenter, Providence, RI; G. S. Christiansen, New London, CT; H. T. Clarke, NYC; W. Cohn, Oak Ridge, TN; M. Heidelberger, NYC; R. J. M. Henry, Cambridge, MA; A. I. Kosak, NYC; D. B. Luten, Berkeley, CA; R. M. Noyes, NYC; W. A. Noyes, Jr., Rochester, NY; A. M. Pappenheimer, Jr., NYC; J. E. Snow, Hasbrouck Heights, NJ; W. J. Stewart, Pittsburgh, PA; D. Todd, Shrewsbury, MA; and V. Weinmayr, Landenberg, PA. Their entries in the *AMS* (Stewart was the only one not listed) show a high proportion of biochemists and radiochemists with many Fulbright and Guggenheim awards among them. They thus might represent a more liberal and international and less industrial orientation than did the members of the Board of Directors and a more elite and academic fraction than the writers of letters-to-the-editor, eleven of whom were not listed in the *AMS*. This list is far from complete, however, for others not on this list wrote directly to Daniels, as S. C. Lind in n17 above, and W. J. Sparks of the Standard Oil Company of New Jersey, who said that under no legal system of which he was aware were wives to be held accountable for their husband's indiscretions (n.d., Box 5). Both he and his wife Meredith were members (*AMS*, 11th ed., 1967, 5, 5068).
23. E. Rabinowitch; see Ref. 16.
24. H. B. Kreider, Jr., Gilsonia, PA, "Letters," *Chem. Eng. News*, May 17, 1954, 32, 1968.
25. G. V. Caesar, Harbor Beach, MI, "Letters—Decision on Joliot-Curie," *ibid.*, April 19, 1954, 32, 1542.
26. S. E. Marugg, Philadelphia, "Letters—Decision on Joliot-Curie," *ibid.*, April 19, 1954, 32, 1542.
27. P. J. Flory, "Letters," *ibid.*, May 17, 1954, 32, 1966.
28. D. Todd, Shrewsbury, Massachusetts, "Letters," *ibid.* April 19, 1954, 32, 1546 and 1548.
29. Ref. 21.
30. *American Chemical Society, Directory of Members, 1956*, ACS, Washington, DC, 1956, 150. Coryell had also been secretary-treasurer of the Federation of Atomic Scientists in 1952 (*AMS*, 11th ed., 1965, 1001); "Dr. Charles D. Coryell," *Chem. Eng. News* April 18, 1960, 38, 73; "Deaths—Charles D. Coryell," *ibid.*, January 25, 1971, 49, 56; and "Coryell, Charles DuBois," *National Cyclopedia of American Biography* 1975, 56, 472-73. Some of his work had been classified research for the federal government.
31. R. N. F., "The Chemist and the Communist," *Chem. Bull.*, April, 1954, 41, (4), 5 and 7. There were responses (generally negative) in the May, June, and September issues. In 1959-60 Feinstein had a Guggenheim fellowship to

- the Radium Institute in Paris (*AMS*, 11th ed., 1965, vol. 2, 1501).
32. "L'Affaire Curie," *Bull. At. Sci.* June 1954, 10, 211-14. One contributor to the *BAtS* discussion, psychologist Mary Mercer of Minnesota, suggested another possible solution: it would have been easier on everyone involved if the ACS had had a fund with which to send journals to controversial people like Irène Joliot-Curie at no charge. ("Bulletin Readers React," *ibid.*, 212.)
 33. A. Thackray, J. L. Sturchio, P. T. Carroll, and R. Bud, *Chemistry in America 1876-1976* D. Reidel Pub. Co., Dordrecht and Boston, 1985, 188 and 189. The ACS Board of Directors was a nursery for future presidents.
 34. Farrington Daniels, who had met the Joliot-Curies in Paris in 1928, was of two minds on the issue, writing even before Madame Joliot-Curie had been rejected that in the short run there would be more criticism for accepting her than for rejecting her, but that in the long run they would find that admitting her would have been the right thing to do. (Farrington Daniels to Alden Emery, July 6, 1953, Daniels Papers, Box 5). He opposed the decision at the time but then worked with Emery, ACS lawyer Elisha Hanson, and the other members of the executive committee of the Board to put the society's action in the best light possible. After hearing from S. C. Lind in early March, 1954, he wished he had opposed her rejection more vigorously. (O. B. Daniels, "Farrington Daniels, Chemist and Prophet of the Solar Age, A Biography," 285-91.)
 35. H. O. Albrecht, Springfield, PA, "Bulletin Members React," *Bull. At. Sci.*, June 1954, 10, 212; J. W. Beckman, Oakland, CA, "Letters," *Chem. Eng. News*, April 19, 1954, 32, 1544.
 36. Irène Curie to David Todd, *Chem. Eng. News*, August 2, 1954, 32, 3026, reprinted in *Bull. At. Sci.* October 1954, 10, 335.
 37. J. W. Lewis and X. Litai, *China Builds the Bomb*, Stanford University Press, Stanford, CA., 1988, 36.
 38. "Alan C. Nixon Chairman for 1954," *The Vortex*, January, 1954, 15, 5; A. C. Nixon, "Chairman's Message, ACS=American Communist Society?" *ibid.*, April, 1954, 147 and 162; *idem*, "Chairman's Message, Madame is Still Here," May, 1954, 187; C. O'Konski, "Minutes of Executive Committee Meeting," May, 1954, 211; J. H. Hildebrand, "A Letter to the Chairman," *Vortex*, June, 1954, 223; A. C. Nixon, "Chairman's Message, Answering the Mail," *ibid.* June, 1954, 225 and 285; C. O'Konski, "Minutes of the General Meeting," *ibid.*, September, 1954, 294; A. C. Nixon, "Chairman's Message," September, 1954, 303; and "Membership Requirements," October 1954, 362 and 364, reprinting a recent letter from J. Hildebrand to A. Nixon.
 39. D. P. Gardner, *The California Oath Controversy*, University of California Press, Berkeley, CA, 1967, 47; more recently, N. K. Innis, "Lessons from the Controversy over the Loyalty Oath at the University of California," *Minerva*, 1992, 30, 337-65; and J. H. Hildebrand, "The Professor and His Public," *Chem. Eng. News*, November 24, 1952, 30., 4934-37, reprinted in *Bull. At. Sci.*, February, 1953, 9, 23-25.
 40. J. Hildebrand to Alden Emery, December 27, 1955, ACS Papers, Box 26. The Joel Hildebrand Papers, his biographical file, and two oral histories of him at the Bancroft Library, University of California, Berkeley, are not helpful. Emery may have learned from the episode, for when in 1961 he was awarded the Gold Medal of the American Institute of Chemists, its citation mentioned his "exceptional intelligence, tact, vision, and responsiveness to the desires of the members." ("Former ACS staff chief dead at 73," *Chem. Eng. News*, March 24, 1975, 53, 7.)
 41. ACS Papers, Box 25. By then the country with the largest number of foreign members of the ACS was Japan with 1,019 up from just 18 in 1951. This was considerably more than the 701 from Canada and almost as many as from Wisconsin (1,089). (*American Chemical Society, Directory of Members, 1956 ACS*, Washington, DC, 1956, 1163-4).
 42. *Ibid.*, 1131.
 43. "Mme. Joliot-Curie is Dead in Paris," *New York Times*, March 18, 1956, 89:1; J. Chadwick, "Obituary, Mme. Irène Joliot-Curie," *Nature*, May 26, 1956, 177, 964-5; F. Perrin, "Irène Joliot-Curie," *Dictionary of Scientific Biography*, 157-59; O. S. Opfell, *The Lady Laureates, Women Who Have Won the Nobel Prize*, 2nd ed., Scarecrow Press, Metuchen, NJ, 1986, 195-212; *Current Biography Yearbook, 1940*, 435-36; Ref. 21; and B. Bensaude-Vincent, "Star Scientists in a Nobelist Family: Irène and Frédéric Joliot-Curie," in H. Pycior, N. Slack, and P. Abir-Am, Ed., *Creative Couples in the Sciences*, Rutgers University Press, New Brunswick, NJ, 1996), 57-71.
 44. See Ref. 33, Table 6.5, pp. 394-6, for a list of chemists who became college and university presidents before 1959.
 45. For example, M. D. Kamen, *Radiant Science, Dark Politics, A Memoir of the Nuclear Age*, University of California Press, Berkeley, CA, 1985, 213-14; and n1 above.
 46. Mentioned in "U.S. Chemists Bar Mme. Joliot-Curie," *New York Times*, February 15, 1954, 10:1.

ABOUT THE AUTHOR

Margaret W. Rossiter is the Marie Underhill Noll Professor of the History of Science in the Department of Science and Technology Studies, Cornell University, 726 University Ave., Ithaca, NY, 14850/USA. Her most recent book *Women Scientists in America, Before Affirmative Action, 1940-1972*, was published by Johns Hopkins University Press in 1995. An earlier form of this paper was presented at the XIXth International Congress of the History of Science, Zaragoza, Spain, in August, 1993.

MARTHA ANNIE WHITELEY (1866-1956): CHEMIST AND EDITOR

Mary R. S. Creese, University of Kansas

Few women chemists whose careers began before 1900 made important contributions to major technical works still to be found in reference sections of science libraries. But Martha Annie Whiteley, coeditor and then editor-in-chief of the eleven-volume fourth edition of *Thorpe's Dictionary of Applied Chemistry* (1937-1954) is an exception.

The original three-volume edition of this work was produced under the editorship of British chemist Sir Edward Thorpe (1845-1925) over the period 1890-1893. During the next four decades it was greatly expanded, a five-volume second edition being brought out by Thorpe in 1916-1919 and two more volumes being added by Foster Morley in 1927-1930 after Thorpe's death. However, within a few years, because of the rapid growth of the field, a complete revision was being



Martha Annie Whiteley, c. 1907, courtesy of the Archives of Imperial College of Science, Technology and Medicine

planned. Supplementary volumes were issued in 1934 and 1935 as a stop-gap measure and the first volume of the revised fourth edition appeared in 1937. The revision was expected to take nine years, a projection which prompted a *Nature* reviewer of volume 1 to remark that the first volumes would be out-of-date before the last came out (1). In fact seventeen years were to pass and World War II to intervene before the work was completed with the publication in 1954 of volume 11; a combined index followed in 1956. Coeditors were Sir Jocelyn Field Thorpe (not a relative of Thomas Edward) and Martha Annie Whiteley, who was already very familiar with the material, having helped extensively in the preparation

of earlier editions. With Sir Jocelyn Thorpe's death in 1940 about the time of publication of volume 4 of the fourth edition, Martha Whiteley, although then seventy-four and somewhat handicapped physically, became editor-in-chief, with help from an assistant editor, A. J. E. Welch. She also had the backing of a distinguished editorial board; the latter consisted of I. M. Heilbron, Professor of Organic Chemistry at Imperial College London, and H. J. Emeléus, Reader in Inorganic Chemistry, at Imperial College, Sir Alexander Todd, Professor of Chemistry at Manchester University, and H. W. Melville, Professor of Chemistry at Aberdeen University. Throughout World War II Whiteley's work was done at Cambridge, where she was given accommodation and library facilities. In 1944 she returned to London and prepared the last four volumes, working from Imperial College and her South Kensington flat, her house having been destroyed by bombing.

This last edition of a chemical encyclopedia on which major effort was put forth over more than sixty years covered almost every topic relating to chemistry. Although it can still be used as a comprehensive source for much basic information, it is of considerable interest now from another point of view as well, namely, the broad historical perspective it can give of the field up to the late 1940s. Many entries are in fact careful, detailed reviews by leaders in the field of progress in particular areas and topics up to that time; the essays on stereochemistry (W. H. Mills) and on tautomerism (J. W. Baker) are two of many such articles of particular note. Also striking is the long essay on synthetic drugs by three chemists from Imperial Chemical Industries, Manchester (S. Ellington, W. R. Boon, and H. C.

Carrington), which sets out in precise detail the relatively limited collection of medicinal compounds available fifty years ago, when, for example, the sulfa drugs were still the major systemic antibacterial agents. Short discussions afford summaries of work on newly discovered isotopes, such as tritium, and recently isolated compounds such as streptomycin, while longer essays cover standard, classical topics such as the theory, construction, and performance capabilities of the chemical balance, including the successive changes and improvements from the instruments of the early nineteenth cen-



Group photograph, c. 1912 - Imperial College of Science and Technology, London, women students and staff - M. A. Whiteley seated third from left

tury to the London-manufactured Oertling micro-assay balance much used by the 1930's.

This huge, ambitious work, in its several editions, is associated primarily with the Thorpes, Sir Edward and Sir Jocelyn. However, Martha Whiteley's efforts as both contributor and editor over almost four decades were remarkable, especially her work on the fourth edition, the labor of her retirement years. She has good claim to a note in the history of the field.

Among the most outstanding women chemists of her time and a notable figure in British academic life, she was one of the first women to hold a full staff position in the chemistry department of a coeducational university in Britain. She was also one of the first to hold a teaching position at Imperial College, where she did

much to gain the acceptance of women students and foster their work in chemistry at both undergraduate and graduate levels (2).

Born in Hammersmith, London, November 11, 1866, the second daughter of William Sedgwick Whiteley and his wife Mary (Bargh), she attended Kensington High School and the Royal Holloway College for Women (London), graduating with a B.Sc. in 1890. She then taught for eleven years, first at Wimbledon High School and after that at St. Gabriel's Training College (London); but by 1898 she was also continuing her studies, having begun work at the Royal College of Science (later part of Imperial College) under the guidance of Professor Sir William Tilden. She received a London D.Sc. in 1902, presenting a dissertation on the preparation and properties of amides and oximes, and the following year was invited by Tilden to join the Royal College of Science teaching staff. Promoted to demonstrator in 1908, she became lecturer in 1914, at age forty-eight, and for fourteen years, from 1920 until she retired in 1934, held the post of assistant professor (a senior academic post at Imperial College, later designated as reader).

Her early research, published in the period up to about 1909, continued her dissertation studies on the amide and oxime derivatives of dicarboxylic acids and related cyclic ureides, including derivatives of caffeine and barbituric acid. She focused particularly on the problem of tautomerism in oximes, an area receiving considerable attention at the time, and a special interest of Sir Jocelyn Thorpe. Although Whiteley was Thorpe's junior colleague at Imperial College and his collaborator in many projects, it is clear from her writings that she had her own research program. Her work on amides and oximes continued after World War I, she and her students and collaborators (including two women, Edith Usherwood and Dorothy Yapp) publishing at least three full papers during the 1920's (3).

Throughout the war, the chemistry staff at Imperial College put their efforts into urgently needed government work. With the cutting off of Britain's pre-1914 supply of German-manufactured chemicals, crash programs had to be started to try to make good the deficit. Whiteley collaborated with Thorpe on the synthesis of drugs badly needed for military hospitals, especially phenacetin, novocaine and b-eucaine, until then imported. She also worked with Thorpe, a member of the government's Trench Warfare Committee, on the production of lachrymatory and vesicant gases for military use.

In 1925 she and Thorpe brought out their *Students' Manual of Organic Chemical Analysis: Qualitative and*

Quantitative, a work which had its origins in the accumulated experience of twenty years' teaching in the organic chemistry laboratories of Imperial College; Whiteley had directed the advanced course throughout much of this period. The 241-page work, a comprehensive and up-to-date compilation of methods used for estimating the more common types of organic materials with full experimental procedures included, was a much welcomed addition to the then meager literature on organic analysis. In addition to presenting clear, practical information, it traced the historical development of some of the most important areas of the subject. Thus the section on the quantitative estimation of carbon and hydrogen included a description of the apparatus devised by Lavoisier 140 years earlier, which, though it failed to give satisfactory results, established as early as 1784 the basic method of burning the organic sample and collecting and weighing the combustion products (4).

One further area in which Martha Whiteley's influence was considerable was in the long campaign to bring women into fellowship in the London Chemical Society (5). She and her friend and fellow chemist Ida Smedley MacLean (1877-1944), a research worker at the Lister Institute of Preventive Medicine, were patient leaders in this effort over almost two decades; and in 1920, when women were finally admitted, Whiteley was one of the first to be formally accepted. The first woman elected to the society's council, she served from 1928 to 1931.

Although several British women from about Whiteley's time were productive research workers, most of them made their contributions as assistants to male chemists. A few developed their own research programs and achieved notable success in newer, nontraditional branches of the field, Ida Smedley MacLean's work in biochemistry being especially outstanding (6). Whiteley, however, was probably the only one who found a place as an independent worker in an established area of chemistry and remained active in research, teaching, and technical writing throughout a long career at a major educational institution—a notable achievement for a woman chemist of her generation.

Her services to chemistry in general and Imperial College in particular were formally recognized in 1945 when she was made an honorary fellow of the college. An able teacher, she was remembered by students and younger colleagues as a source of inspiration and a dependable guide. She died on May 24, 1956, in her ninetyeth year, not long after she had seen the *Dictionary* revision through to its completion.

REFERENCES AND NOTES

1. H. E. W., "A Chemical Encyclopedia," *Nature*, **1937**, *140*, 1076-1077.
2. A. A. Eldridge, "Martha Annie Whiteley. 1866-1956," *Proc. Chem. Soc. London*, June, 1957, 182-183; *London Times*, May 26, 1956, p. 10; information from Anne Barrett, Archivist, Imperial College of Science, Technology, and Medicine, London.
3. Martha Whiteley's papers include the following: "The Oxime of Mesoxamide and some Allied Compounds," *J. Chem. Soc.*, **1900**, 77, Pt. 2, 1040-1046; "The Action of Barium Hydroxide on Dimethylvioluric Acid," *ibid.*, **1903**, 83, 18-23; "The Oxime of Mesoxamide and Some Allied Compounds. Part II. Di-substituted Derivatives," *ibid.*, **1903**, 83, 24-45; "Studies in the Barbituric Acid Series. I. 1:3-Diphenylbarbituric Acid and Some Coloured Derivatives," *J. Chem. Soc. Trans.*, **1907**, 91, 1330-1350; (with Edith H. Usherwood) "Oxime of Mesoxamide (Isonitrosomalonyamide) and Some Allied Compounds. Ring Formation in the Tetrasubstituted Series," *ibid.*, **1923**, 123, 1069-1089; (with Arthur Plowman) "Oxime of Mesoxamide (Isonitrosomalonyamide) and Some Allied Compounds. Structural and Stereoisomerism in the Methyl Ethers of the *p*-Tolyl Derivative," *ibid.*, **1924**, 125, 587-604; (with Dorothy Yapp) "Reaction of Diazonium Salts and Malonyldiurethan," *J. Chem. Soc.*, **1927**, 130, 521-528.
4. See A. T. Schwartz, "Instruments of the Revolution: Lavoisier's Apparatus," *Bull. Hist. Chem.*, **1989**, 5, 31-34.
5. J. Mason, "A Forty Years' War," *Chem. Br.*, **1991**, 27, 233-238.
6. See M. R. S. Creese, "British Women of the Nineteenth and Early Twentieth Centuries who Contributed to Research in the Chemical Sciences," *Br. J. Hist. Sci.*, **1991**, 24, 275-305.

ABOUT THE AUTHOR

Mary R. S. Creese is an Associate at the Hall Center for the Humanities, University of Kansas, Lawrence, KS 66045.

The *OUTSTANDING PAPER AWARD* for 1997 goes to Dr. David E. Lewis for his publication "Aleksandr Mikhailovich Zaitsev (1841-1910), Markovnikov's Conservative Contemporary," which appeared in Number 17/18 of the *Bulletin*.

The 1996 Dexter Award address by Dr. Keith J. Laidler was presented in video format at the San Francisco ACS Meeting in April, 1997. Copies of the video may be purchased by sending \$15 (check made out to HIST) to Dr. Vera Mainz, Secretary-Treasurer (her address may be found on the inside cover).

THE HUNGARIAN NATIONAL MUSEUM FOR SCIENCE AND TECHNOLOGY

Lyman R. Caswell, Seattle, WA

The main building of the Hungarian National Museum for Science and Technology (Országos Műszaki Múzeum) is located at 13-15 Kaposvár Street in Budapest.

The development of this museum and the formation of much of its collections have been realized through the efforts of its first Director, Professor Ferenc Szabadvány, the noted historian of chemistry (1).

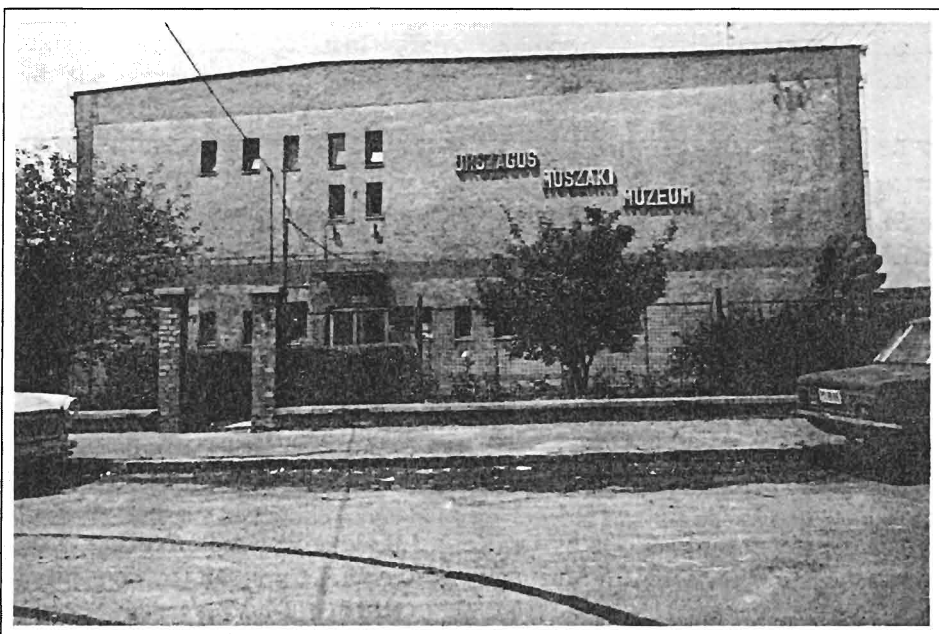
The idea of a technical museum in Hungary is not new. As early as 1807, the royal governor of Hungary, the Palatine Joseph, ordered the Hungarian counties to submit masterpieces of their industrial products. No record of

this accumulation, if it was actually formed, exists today. Many years later the nationalist patriot and reformer Lajos Kossuth, leader of the 1848 independence movement

and provisional governor of Hungary in the revolutionary government of 1848-49, called for the formation of a collection of industrial tools. As the result of the suppression of the independence movement, this collection was never made.

No real collection was formed until the 1870's when the National Museum established a Department of Technology, which accumulated

some 20,000 objects. This collection was used for technical instruction, rather than historical preservation and study. In 1889 the collection was distributed to three organizations: the Industrial Association, the



National Museum for Science and Technology, Budapest, Hungary, photo courtesy of the Museum

Technical University, and the Industrial Museum for Technology. The Industrial Association had evolved into an industrial arts school by 1910. The Museum for Technology, the logical place for the exhibits, failed to develop as a museum, and became instead a center for materials testing. Few identifiable objects from this collection remain.

Another effort to establish a Hungarian technical museum was made in the period between the two world wars, based on a personal collection accumulated by the Chief Librarian of the Hungarian Association of Engineers and Architects, Ede Losy-Schmidt. This was the Hungarian Museum for Technology, established without a building in 1935. After the partition of Czechoslovakia in 1938, this collection was transported to a building which housed a railroad museum in Kassa, in the territory annexed to Hungary. This territory was returned to Czechoslovakia in 1945, and the collection remains there today in Kosce, Slovakia.

The beginning of the present museum stems from Law-Decree No. 4/1954 of the Hungarian Presidential Council, which made provision for the preservation of "industrial relics." Under this decree, factories were ordered to preserve obsolete machines. The Group for Registration and Preservation of Industrial Relics was organized, and a storehouse for the collection was constructed in the University District of Budapest. The Group was reorganized as the National Museum for Science and Technology on January 1, 1973.

The first Director of the new museum was Dr. Ferenc Szabadváry, Professor of Analytical Chemistry at the Technical University of Budapest, who is well-known for his work in the history of chemistry. Professor Szabadváry was born September 1, 1923, in Köszeg, Vas County, in western Hungary. His birthplace is a location of historical significance, being the place where the advance of the Turkish army toward Vienna was stopped in 1532. Perhaps because of the rich history of

his home town, Szabadváry developed a strong interest in history at an early age. It was his original ambition to become a historian(1). Instead, in response to the needs of the family business, a soap factory, he attended the Technical University of Budapest, earning a degree in chemical engineering in 1943. After the post-war Communist government nationalized the soap factory, Szabadváry joined the faculty of the Technical University as Assistant Lecturer in general and analytical chemistry. In 1965 he completed the Doctor of Technology degree and in 1970 received the Doctor of Science in analytical chemistry.

At the time of his appointment as Director of the Museum for Science and Technology (Országos Műszaki Múzeum) in Budapest, he was Professor of Chemistry at the Technical University. He served on the editorial boards of the *Journal of Thermal Analysis* and the *Journal for Radioanalytical Chemistry*. He has also been Chairman of the Hungarian Committee of the International Union of History and Philosophy of Science and Deputy Chairman of the Committee for History

of Sciences and Technology of the Hungarian Academy of Science.

Szabadváry's specialization in analytical chemistry and his interest in history resulted in his work in the history of analytical chemistry, a field that had not previously been explored. According to Professor Szabadváry (1), the inspiration to develop this history occurred early in his teaching career, when a student asked him who first used a permanganate titration for the determination of iron, and he was not able to find the answer in any readily available text. Through his historical research he found that this technique was first described in 1846 by Frédéric Margueritte(2), an employee of the Paris gas works.

After he became Director of the Museum, Szabadváry's chemical research focused entirely on the



Professor Ferenc Szabadváry and a clock built to give the time in the world's major cities.
Clock built c. 1900

history of chemistry, especially analytical chemistry, and on the lives of the persons who made significant contributions to chemistry. A survey of the author indices of *Chemical Abstracts* yielded some 80 titles by Szabadváry in the history of chemistry. The first of these was "A Brief History of Analytical chemistry," published in 1958(3). His major work, the *History of Analytical Chemistry*, was first published in 1960 in Hungarian(4). A German translation appeared in 1966(5) and a Japanese translation in 1988(6a).

The English translation by Gyula Svehla was published in 1966(7) and was reprinted in 1992(8). Szabadváry's other major work, the biography of Lavoisier, was translated from German by Professor Ralph E. Oesper(9) of the University of Cincinnati, where it was published in 1977(10). Another book, *History of Chemistry in Hungary*, a collaboration with Zoltan Szokefalvi-Nagy, was published in 1972(11).

It was the *History of Analytical Chemistry* that earned Professor Szabadváry the 1970 Dexter Award of the History of Chemistry Division of the American Chemical Society. He was the first continental European to receive this award. Since he was unable to travel to the US to receive it, the award was sent to him via the Hungarian Ambassador. Receipt of the award and the international recognitions which accompanied it generated a turning point in Szabadváry's career. As the son of a capitalist manufacturer, he had been labeled a "class enemy" by the Communist Party, which ruled Hungary at the time. For this reason the local Communist Party section had kept him from receiving his doctorate, although he had met all the academic criteria for the degree. These obstacles to his advancement were removed by the prestige of the Dexter Award. In 1991 the new government of Hungary awarded him the Széchenyi Medal, the highest Hungarian scientific achievement(1).

Under Professor Szabadváry's leadership, the Museum has accumulated a collection of some 12,000 objects, which include 3,100 electrotechnical items, 2,100 precision mechanical and optical devices, 900 mechani-

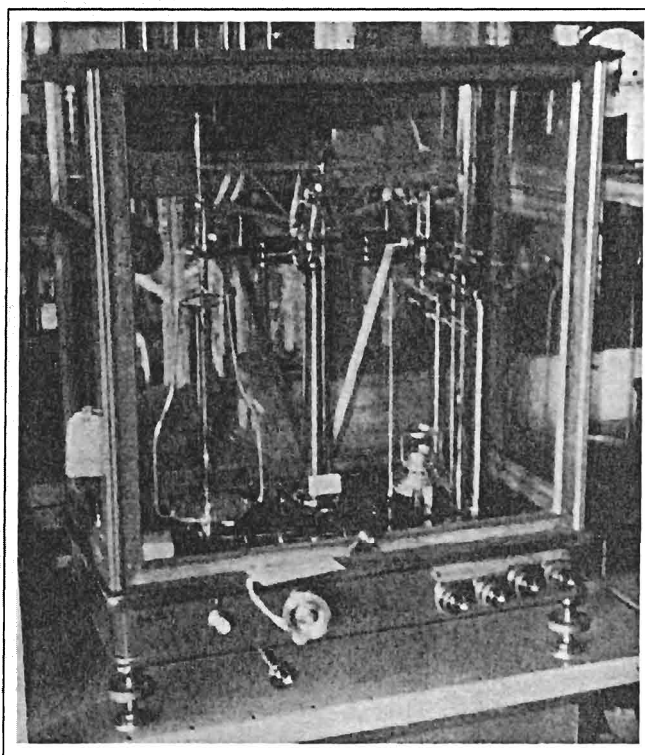
cal engineering objects, 700 items relating to the history of photography, and 600 measuring instruments. The archive collection has some 1,600 items. Of these, the map collection comprises items from the seventeenth century to the present time, including the original maps on river control and canalization dating from the nineteenth century. The photograph collection has some 2,500 items. The library contains some 13,500 volumes. The facilities of the archives and the library are avail-

able to scholars undertaking research in the history of science and technology. In addition to these facilities, there are workshops for the restoration of artifacts held by the museum. There are 27 full-time and 14 part-time employees.

In 1984 a new and larger storehouse for the museum's collections was built at the present location. This building also contains the archives, the library, the workshops, and the administrative offices. Traveling exhibits to be sent to fairs, international exhibitions, meetings of professional societies, and the like are prepared at this location.

The building also houses the editorial offices of *Technikatörténeti Szemle* (*Review of the History of Technics*), the Museum's journal, and *Periodica Polytechnica*, the journal of the Technical University of Budapest, of which Professor Szabadváry was editor..

A small number of cases along one side of the main hallway contain the limited number of exhibits at this facility. Some of these exhibits portray the accomplishments of nineteenth-century Hungarian inventors, some of whom independently invented items for which credit is generally given to better-known persons in other countries. An example is the case of Ányos Jedlik, who invented the self-excited electrical generator in 1861, five years prior to its independent invention by Werner von Siemens. It did not occur to Jedlik, a Roman Catholic monk, that there might be a practical or commercial



Early analytical balance without weights, Vienna, 1910

application for his invention. One of the other exhibits depicts models of nineteenth-century farm machinery invented by Americans. The most unusual exhibit is a century-old picture of a match factory, constructed entirely of the factory's products.

Of chemical interest is the exhibit of balances. A large additional number of balances remains in the storehouse. These include various microbalances and early examples of automatic and electronic balances. As an analytical chemist, Professor Szabadváry has also provided an extensive collection of volumetric glassware.

In addition to the location in Budapest, the Museum has three other facilities. A permanent exhibit entitled "The Hungarian Industry" was established in 1986 in the Széchenyi palace at Nagycenk in West Hungary. Count Istvan Széchenyi (1791-1860), in whose palace the exhibit is located, was a statesman who promoted industrial development in Hungary during the first half of the nineteenth century, and who in 1825 founded the Hungarian Academy of Science, endowing it with the income from his estates. A second permanent exhibit presents the history of phototechniques. It is located at the Forte Photochemical Works in Vác, north of Budapest. A collection of computers is stored in a facility owned by the museum in South Hungary.

The Ministry of Culture, which administers all the museums in Hungary, had promised in 1970 that an exhibit hall for the Museum of Science and Technology "would be constructed under the next five-year plan." When the building was not forthcoming, Dr. Szabadváry discovered that the Minister of Finance had added two letters to the planning document, changing the word for *plan* to the plural, and thus extending the promise to the indefinite future. A site for the future museum has been reserved, however, in the territory of the future university campus in Lágymános in south Budapest. An exhibition hall of 2,500-3,000 square meters of exhibition surface is to be built at this site, while the stores and offices are to remain at the present location. Government financing has still not become available.

Professor Szabadváry retired as Director of the Museum on December 31, 1993, but remains an active scholar of the history of science and especially of the history of chemistry. He was succeeded as Director by Dr. Éva Vámos, who had served as Deputy Director of the Museum for the previous five years. Dr. Vámos is also a prominent historian of science who is particularly noted for her contributions to the history of women in science.

REFERENCES AND NOTES

1. The description of the Museum, its contents, and its history, as well as biographical data on Professor Szabadváry, derive from a visit to the Museum by the author of this article, on October 11, 1993.
2. F. Margueritte, "Sur un nouveau procédé de dosage du fer par la voie humide," *C. R. Hebd. Séances Acad. Sci.*, **1846**, 22, 587-591; *Ann. Chim. Phys.*, **1846**, 18, 244-255.
3. F. Szabadváry, "Kurze Geschichte der Analytischen Chemie," *Periodica Polytech.*, **1958**, 2, 49-57.
4. F. Szabadváry, *Az analitikai kémia módszereinek kialakulása*, Akadémiai Kiadó, Budapest, 1960. 418 pp.
5. F. Szabadváry, *Geschichte der Analytischen Chemie*, Verlag Vieweg, Braunschweig, 1966. 410 pp.
6. F. Szabadváry, *Bunseki Kagaku no Rekishi: Kagaku no Kigen, Tayona Kagakusha, Sho Bunsekiho no Tenkai*, Uchida Rokakuho Publishing Co., Ltd, Tokyo, 1988. 577 pp.
7. F. Szabadváry, *History of Analytical Chemistry*, G. Svehla, translator, Pergamon Press Ltd, London, 1966. 418 pp.
8. F. Szabadváry, *History of Analytical Chemistry*, G. Svehla, translator, Gordon and Breach Science Publishers, Langhorne PA, 1992. 418 pp.
9. Ralph Edward Oesper (1886-1977), Professor of Analytical Chemistry at the University of Cincinnati, was the recipient in 1956 of the first Dexter Award for his contributions to the history of chemistry.
10. F. Szabadváry, *Antoine Laurent Lavoisier: The Investigator and His Times, 1741-1794*, R. E. Oesper, translator, University of Cincinnati Press, Cincinnati OH, 1977.
11. F. Szabadváry and Z. Szokefalvi-Nagy, *A kémia története Magyarországon*, Akadémiai Kiadó, Budapest, 1972. 367 pp.

ACKNOWLEDGMENTS

The author is grateful to Professor Szabadváry for the opportunity to meet with him and to visit the National Museum for Science and Technology, and to Dr. Vámos for providing information on the Museum's history.

ABOUT THE AUTHOR

Lyman R. Caswell is Professor Emeritus of Chemistry at Texas Woman's University, Denton, TX, now residing at 6535 37th Ave. NE, Seattle, WA 98115-7431; e-mail 72712.575@compuserve.com. He is interested in chemical and mineralogical research done in Spain and Spanish America during the colonial period.

A STEREOCHEMICAL ACHIEVEMENT OF THE FIRST ORDER: ALFRED WERNER'S RESOLUTION OF COBALT COMPLEXES, 85 YEARS LATER

George B. Kauffman, California State University, Fresno

Listen, old man; take my advice.
Give me the cobalt in a thrice.
Though Hell and Devil say me nay,
I shall resolve cobalt today (1).

With these words the Old One (*der Alte*, i.e., Werner) challenges the King of the Spirits (*Geisterkönig*). Spurning the tempting gifts offered him by the King, he approaches the giant blue octahedron with a cry of "I want to resolve cobalt" (*Ich will das Kobalt spalten*). He tears one side of it away with his axe, removes the *d*-form, and spins it on his little finger. As everyone joyfully shouts, "It rotates, it rotates!" (*Es dreht, es dreht!*), *der Alte* removes the *l*-form from the other side of the octahedron and spins it on his little finger in the direction opposite to the *d*-form.

Now that the momentous task has been accomplished, all the participants face the audience and triumphantly sing the final chorus to the melody of *Das Studium der Weiber*:

From early morn 'till night so late
We'll just rotate, rotate, rotate,
Until the world and all therein
Rotating spins to oblivion (2).

So ends the anonymous 1911 *Weihnachtskommers* (traditional student Christmas party) playlet in verse, *Drehen und Spalten* (Rotating and Resolving) (3), whose authorship the late Nobel Chemistry laureate Paul Karrer (Ph.D., Universität Zürich, 1911) admitted to me. It



Alfred Werner (1866-1919) from George B. Kauffman, "Alfred Werner-Founder of Coordination Chemistry," Berlin-Heidelberg-New York: Springer 1966, Frontispiece

purports to tell in mock-heroic fashion the story of the first successful resolution of an optically active coordination compound, a problem that had occupied Alfred Werner (4) and a series of his students "over a period of

some nine years" (5), according to his American *Doktorand* Victor L. King (1886-1958) (Ph.D., Universität Zürich, 1912) (6), who finally solved the problem. King, who was accustomed to being greeted on the streets of Zürich with the inquiry, "Nun, dreht es schon?" (Well, does it rotate yet?), recalls how he walked into Werner's office with the long-awaited news. Werner "leaned back in his chair, smiled, and said not a single word" (5).

All the students knew that something extraordinary must have happened when the always punctual Werner failed to appear at his 5 P.M. lecture. A young student appeared, announcing that the lecture had been canceled. Werner, fearing that the optical antipodes might racemize, worked late into the night with King, making many derivatives and measuring their rotations. But his fear was unfounded, for the enantiomorphs proved to be remarkably resistant to racemization in solution, both on prolonged standing and even on heating to incipient boiling (7).

Werner atypically but elatedly accosted casual acquaintances to tell them of his greatest experimental triumph – a "stereochemical achievement of the first order. (8)" Peter Debye offered his own personal account (9):

One early afternoon, when I went from the lake to the Physics Institute after lunch, Werner hailed me from the opposite side of the Rämistrasse. It turned out that he wanted to talk to me about the fact that he had succeeded in making a coordination compound which showed rotation of the plane of polarization. I was very much interested indeed but did not quite understand why he talked to me, since we had had no scientific discussions at all before that time.

The concepts of asymmetry and optical activity, although introduced fairly late into inorganic chemistry, have played a central role in organic chemistry. If modern organic chemistry is considered to begin in 1828 with Friedrich Wöhler's synthesis of urea, then Jean Baptiste Biot's discovery of optical activity in 1812 antedates the very genesis of this field. Furthermore, Le Bel and van't Hoff's concept of the tetrahedral carbon atom in 1874, which constitutes the foundation of stereochemistry, was proposed primarily to explain the optical isomerism investigated by Louis Pasteur and others. It is to Werner, however, that we owe the systematic introduction of the concept of optical activity into coordination chemistry.

Although compounds containing asymmetric atoms other than carbon, *e.g.*, nitrogen, phosphorus, sulfur, selenium, tin, and silicon, had been resolved before

Werner and King's resolution, all these compounds had involved atoms of tetrahedral configuration (10). Werner's work, on the other hand, involved a compound of the octahedral configuration, which he had proposed in his first paper on the coordination theory (1893) (11) but which was still unproven. As a result of this work, "The spatial configuration of the co-ordination complex with six associating units is now as firmly established as that of the asymmetric tetrahedral carbon atom" (12).

The resolution of optically active coordination compounds, a feat that "shook chemistry to its innermost foundations" (13), gained for Werner and his coordination theory the widespread recognition for which he had been striving so long. In 1913, two years later, largely because of this "most brilliant confirmation of [his] stereochemical views" (14), Werner was awarded the Nobel Prize in Chemistry "in recognition of his work on the linkage of atoms in molecules by which he has thrown new light on earlier investigations and opened up new fields of research especially in inorganic chemistry" (15).

Werner's Earlier Research on Optical Activity

Contrary to common belief, Werner did not mention optical activity in his first paper on the coordination theory (11). It was not until 1899, the same year in which he accomplished his first resolution – that of the organic compound, *trans*-hexahydrophthalic acid – with his assistant Hermann Erich Conrad (Ph.D., Universität Zürich, 1898) (16) – that he and A. Vilmos considered for the first time in print the fact that molecular asymmetry for certain types of complexes containing chelate ligands (17) is a geometric consequence of his postulated octahedral configuration (18):

Thus, for the oxalatodiethylenediaminecobalt salts [modern, bis(ethylenediamine)oxalatocobalt(III) salts (19) and similar compounds, we may predict a new type of isomerism which belongs to the class of asymmetry isomerism, of which until now the usual carbon asymmetry and the molecular asymmetry of the inositols [cyclohexanhexanols] are known (19).

The first evidence that Werner had recognized that resolution of such compounds would provide an elegant, definitive proof of his stereochemical views and that he was actually attempting to resolve them is found in a letter of February 20, 1897 to Arturo Miolati antedating this paper by two years (20):

At present we are searching for asymmetrically constructed cobalt molecules. Will it be successful?

King's estimate of the time that Werner had spent on unsuccessful resolutions was thus a conservative one.

More than a decade after this letter, success still eluded Werner, but he had not abandoned his goal (21):

I see from your beautiful paper in the *Berichte* that you have been more successful in resolving propylenediamine than we have. Now I wish to ask you whether you would permit me to use the active propylenediamine in the investigation of compounds $[(O_2N)_2COpn_2]X$, of which we have already obtained five inactive series.

Further proof of continuing activity in the field is a sample in the Werner collection of complexes labeled "Resolution experiment on $[CoCO_3en_2]Br$ [en = ethylenediamine] by means of silver *d*-tartrate, 20/I. 1908, Dubsky" (22). It was on this compound that King began experimental work on first arriving in Zürich (23), but after a year of unsuccessful attempts at resolution, he abandoned it.

Werner and King's Resolution

Werner chose the most widely used method of resolution, Pasteur's diastereomer formation, which often failed because the naturally occurring optically active acids and bases then available for use as resolving agents were weak and their salts were not very stable in solution. Kipping and Pope's synthesis from naturally occurring (+)-camphor of resolving agents that are strong acids or bases, such as (+)-camphorsulfonic and (+)-bromocamphorsulfonic acids, provided Werner and King with the agent that finally brought them success (24).

The resolution involved two series of complexes – the *cis*-amminechlorobis(ethylenediamine)cobalt(III) salts, $[CoCl(NH_3)en_2]X_2$ (with King) and the corresponding *cis*-bromo- salts, $[CoBr(NH_3)en_2]X_2$ (with Werner's *Privat-Assistent* Ernst Scholze, Ph.D., Universität Zürich, 1911) (25). The latter series is easier to resolve because of the greater difference in solubility between the diastereomers that are formed with the resolving agent, silver (+)-3-bromo-9-camphorsulfonate. For both series the (+)-bromocamphorsulfonate of the (+)-antipode is less soluble than that of the (-)-antipode.

According to King's laboratory notebook (Ref. 23, p. 21, undated but sometime between April and June 12, 1911), "With the Bromoammine *Reihe* [series] the dextro salt falls right out and no fractionation is necessary." King later recalled (5):

I shall never forget the day that the optically active isomers were first attained. In connection with this work, I had been carrying out some 2000 fraction-

ations and had been studying Madame Curie's work on radium for that purpose. After having made these 2000 separate fractional crystallizations which proved that the opposite ends of the system were precisely alike and that we had to do something more drastic, I proposed increasing the dissimilarity of the diastomers [*sic*] by using brom camphor sulfonic acid as a salt-forming constituent having extremely high optical activity. When this was tried, the isomers in the form of these salts literally fell apart.

A solution of the chloro- or bromo- racemic salt was treated with a solution of the resolving agent, whereupon the (+)(+)-diastereomer precipitated and was removed by filtration (26). The (-)-dithionate was precipitated from the mother liquor by addition of sodium dithionate. The recrystallized (+)(+)-diastereomer was treated with concentrated hydrochloric acid to yield a precipitate of the (+)-chloride, while the (-)-chloride was precipitated by grinding the (-)-dithionate with concentrated hydrochloric acid, followed by cooling. The procedure is reproducible (27) and has been modified and simplified for use as a classic experiment for the undergraduate inorganic laboratory (28).

The resolution, which Werner considered to confirm "one of the most far-reaching conclusions of the octahedral formula," proved that "metal atoms can act as central atoms of stable, asymmetrically constructed molecules [and] that pure molecular compounds [coordination compounds] can also occur as stable mirror image isomers, whereby the difference between valence compounds [ordinary compounds], which is still frequently maintained, disappears entirely" (26). According to King, it was "the last proof for the octahedral formula assumed by A. Werner" (29).

Werner's numerous contributions to coordination chemistry sometimes tend to obscure the fact that he was trained under Arthur Hantzsch as an organic chemist with an emphasis on stereochemistry, was originally called to Universität Zürich to teach organic chemistry, and was not assigned the main lecture in inorganic chemistry until the winter semester 1902/03 (Ref. 4, pp. 38-39). It was not until 1898, when his reputation was firmly established in coordination chemistry, that the number of his inorganic articles – 21 – reached that of his organic papers (30).

Therefore the question as to why it took Werner so long to succeed in resolving coordination compounds is not only an interesting but a legitimate one (31). Many anionic complexes such as oxalato compounds, which contain the chelate ligand with which Werner first considered the possibility for optical isomerism (17), racemize readily; and he might have achieved resolutions of

these compounds without recognizing them. The most common resolving agents such as (+)-tartaric acid are unsuccessful in the case of most coordination compounds, and the resolving agent that eventually proved to be successful – (+)-3-bromo-9-camphorsulfonic acid – was not readily available at the time. Because its synthesis was a long and tedious process, Werner probably did not try it until he had exhausted all other possibilities. Furthermore, recent improvements in instrumentation may have played a great role in Werner's success. It was only shortly before Werner achieved his long-sought success with the Schmidt & Haensch Model No. 8142 polarimeter that this instrument, whose new features for measuring small rotations such as the Lippich half-shadow (*Halbschatten*) device increased the precision in determining the zero points with the deeply colored solutions with which he had to work. Although at the time optical rotations were measured at the sodium D-line wavelength, the intense colors of Werner's solutions must have made use of this wavelength difficult. Around the time of Werner's success Hans Heinrich Landolt introduced the use of filters to obtain different wavelengths, and Werner reported all his optical rotation values as $[\alpha]_C$ rather than $[\alpha]_D$. Finally, the resolution of optical isomers in the absence of exact directions is as much an art as a science.

Rather than asking why it took Werner so long, perhaps we should marvel that he succeeded in the time that he did. Considering the possible variety of coordination compounds and choice of resolving agents, instruments, and wavelengths as well as the highly specific experimental conditions sometimes required, it is not unlikely that, even for someone with Werner's experimental skill and chemical intuition, many years of hard work might have been needed to solve the problem. In short, an extensive Edisonian trial-and-error approach was necessary, and the large number of possible experiments to be attempted rendered the possibility of success small. In any case, Werner's conclusive proof of an octahedral configuration for cobalt(III) by the resolution of certain coordination compounds was due to his persistent belief in his own ideas and his unrelenting attempts to prove them, despite what others might have found to be insurmountable experimental obstacles.

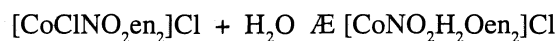
Further Resolutions

It is a repeating historical scenario that whenever anything substantially new is being attempted, much time and effort are required to discover the first successful example; thereafter, numerous examples are quickly and

readily developed. Thus a long period of search is followed by several years of fruitful and prolific productivity. As Paul Karrer expressed it in the present case, "Whenever Werner opened up a new field, he expanded it with unbelievable speed" (13). Once Werner had discovered the means of resolving coordination compounds, a large number of papers describing resolutions of additional complexes – more than forty series within eight years – appeared with great speed from his institute. Together with Michael Basyrin (Ph.D., Universität Zürich, 1915), he even repaid his debt to Pasteur by using optically active inorganic complexes, which he had resolved with organic resolving agents, to resolve an organic compound, dimethylsuccinic acid (32).

In the second article in his 12-part series "Toward an Understanding of the Asymmetric Cobalt Atom" (33) Werner resolved compounds of the *cis*-[Co(NO₂)₂en₂]X (flavo) series by a combination of two resolving agents – (+)-camphorsulfonic acid, which forms a less soluble (-)-salt, and (+)-3-bromocamphor-9-sulfonic acid, which forms a less soluble (+)-salt. In this way he was able to avoid using the sulfonic acid derivatives of the uncommon (-)-camphor. All his attempts to resolve salts of the corresponding *trans*-[Co(NO₂)₂en₂]X (croceo) series were unsuccessful, in support of his octahedral hypothesis, which predicted that only the *cis*- and not the *trans*- isomer should exist in asymmetric forms. It also added resolvability as a new and powerful weapon to the arsenal of the stereochemist seeking to determine the configuration of geometrically isomeric complexes.

In Werner's third paper in the series (34) the resolution of *cis*-[CoClNO₂en₂]X by means of ammonium (+)-camphorsulfonate and ammonium (+)-bromocamphorsulfonate provided an unusual case of mutarotation, similar to that observed with dextrose among organic compounds, caused by a rapid aquation reaction:



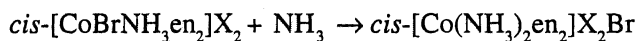
Because coordinated chlorine atoms appear to be replaced by water when silver salts are used as resolving agents, in his fourth paper in the series (35) Werner used ammonium (+)- and (-)-bromocamphorsulfonates to resolve *cis*-[CoCl₂en₂]X. Although stable in the dry state, the active salts racemized in solution to form inactive aquachloro and diaqua salts. Yet in some cases Werner was able to replace coordinated chlorine atoms with other atoms without loss of optical activity. He also observed that some reactions proceed with a change in the sign of rotation, but he realized that the sign of rotation of a

complex is not determined simply by its absolute configuration and that consequently a change in sign need not be attributed to an inversion of configuration.

In the fifth paper in the series (36) Werner resolved $[\text{Coen}_3]\text{X}_3$ salts by crystallization of the chloride tartrate or bromide tartrate, one of the few cases among inorganic complexes in which tartaric acid proved useful as a resolving agent, the other case being that of the structurally similar $[\text{Rhen}_3]\text{X}_3$ (37). He regarded the resolution of tris(ethylenediamine)cobalt(III) salts as particularly important because it furnished conclusive proof for the correctness of the octahedral configuration, in that it eliminated the hexagonal planar and trigonal prismatic configurations, which would possess a plane of symmetry for compounds of type $[\text{M}(\text{AA})_3]$. It also demonstrated that optical activity does not require the linkage of *different* groups to the central atom, and it explained the optical activity of many minerals such as basic polynuclear salts which may be regarded as constitutionally similar to $[\text{Men}_3]$ salts (38).

All attempts to resolve by diastereomer formation with optically active anions the salts with which King began his work, *viz.*, $[\text{CoCO}_3\text{en}_2]\text{X}$, were unsuccessful. Therefore in the sixth paper of the series (39) Werner, together with the late Thomas Potter McCutcheon, who was my general chemistry instructor at the University of Pennsylvania in 1948, synthesized optically active $[\text{CoCO}_3\text{en}_2]\text{X}$ by the action of potassium carbonate on optically active *cis*- $[\text{CoCl}_2\text{en}_2]\text{X}$ salts, a reaction later investigated by Bailar (40) and others. Werner and McCutcheon also prepared optically active $[\text{CoC}_2\text{O}_4\text{en}_2]\text{X}$ by reaction of active *cis*- $[\text{CoCl}_2\text{en}_2]\text{X}$ with potassium oxalate rather than by resolution of the racemic mixture (39). In attempting the latter Werner developed a new method of resolution – preferential crystallization (21b), a procedure currently used in Japan to produce L-glutamic acid in tonnage quantities.

Because all attempts to resolve *cis*- $[\text{Co}(\text{NH}_3)_2\text{en}_2]\text{X}_3$ salts by means of optically active acids had ended in failure, in the seventh paper of the series Werner and Shibata (41) converted optically active *cis*- $[\text{CoBrNH}_3\text{en}_2]\text{X}_2$ salts (26) into the corresponding active diammine salts by treatment with liquid ammonia:



In the eighth paper of the series (42) Werner and Tschernoff resolved *cis*- $[\text{CoBrClen}_2]\text{X}$ salts by precipitation with ammonium (+)- and (-)-bromo camphorsulfonates. In solution the active salts undergo

aquation with complete loss of activity within 30-60 minutes.

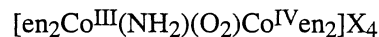
In the ninth paper of the series (43, 44) Werner proved that polynuclear as well as mononuclear complexes could be resolved, demonstrating his theoretically predicted analogy between compounds containing two asymmetric carbon atoms and polynuclear complexes with two metal atoms, another striking confirmation of his octahedral hypothesis. In complete analogy with tartaric acid, Werner prepared



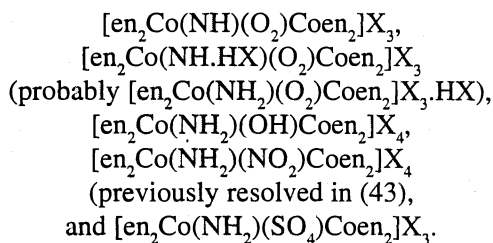
in a racemic (+)(-) form; (+)- and (-)-enantiomers; and an internally compensated, nonresolvable *meso* form.

According to the coordination theory, the two cobalt atoms in these binuclear complexes should not be structurally identical because one is bonded by a principal valence (*Hauptvalenz*) to NO_2 and by a secondary valence (*Nebenvalenz*) to NH_2 and the other vice versa. However, the inactivity of the *meso* form indicates that the two cobalt atoms exhibit no difference even in their action on the plane of polarized light and that the NH_2 and NO_2 groups are symmetrically located with respect to the cobalt atoms. Consequently, Werner concluded that “no essential difference can exist between principal and secondary valence bonds” (*kein prinzipieller Unterschied zwischen Haupt- und Nebenvalenzbindungen bestehen kann*) (43), a statement that he also subsequently made on other occasions.

In the tenth paper of the series (45) Werner, together with Hedwig Kuh (Ph.D., Universität Zürich, 1912) and Paul Wüst (Ph.D., Universität Zürich, 1913), resolved



(previously prepared in (46)) into three forms – (+),(+); (-),(-); and (+),(-) (His formulations with $\text{Co}(\text{IV})$ are now known to be incorrect; the two cobalt atoms are equivalent and tripositive, and the O_2 is a superoxide bridge). By allowing the parent compound to react with various reagents, he also obtained the following optically active compounds:



In the eleventh paper of the series (18b) Werner and Jakob Bosshart (Ph.D., Universität Zürich, 1914) used preferential crystallization to resolve $[\text{CoC}_2\text{O}_4\text{en}_2]\text{X}$, the series in which he first predicted the possibility of optical isomerism (19). By adding a "seed" crystal of (+)- $[\text{CoC}_2\text{O}_4\text{en}_2]\text{Br}$ to a concentrated solution of racemic- $[\text{CoC}_2\text{O}_4\text{en}_2]\text{Br}$, followed by addition of alcohol and ether, they obtained a precipitate of pure (+)- $[\text{CoC}_2\text{O}_4\text{en}_2]\text{Br}$. They then recovered the (-)-isomer from the solution.

The Final and Unequivocal Proof

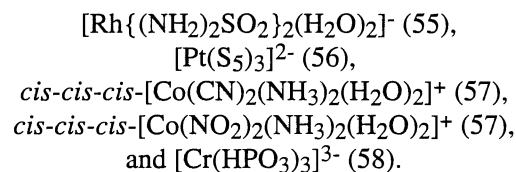
Although the complexes that Werner had resolved up to this time (1914) represented a remarkable variety of compound types, they all possessed one common characteristic – they all contained carbon. Even though the symmetrical carbon-containing ligands (ethylenediamine, bipyridyl, or oxalate ion) are themselves optically inactive, any chemists opposed to Werner's views might maintain that the optical activity of the resolved complexes was in some way due to the presence of these ligands and thus cast doubt on the validity of the octahedral configuration for not only cobalt(III) but also for chromium(III), rhodium(III), and iron(II), whose complexes Werner had also resolved.

For this reason, the twelfth and last article in Werner's series on the asymmetric cobalt atom (47) was a particularly satisfying and welcome one, for it vindicated his octahedral concept for even his most skeptical contemporaries (48-50). In his own words, the resolution proved that "carbon-free inorganic compounds can also exist as mirror image isomers" (47) and that therefore "the difference still existing between carbon compounds and purely inorganic compounds disappears" (47). Finally, he had verified experimentally a conviction that he had held for many years – there is no essential difference between organic and inorganic chemistry.

It is ironic that the completely inorganic tetranuclear tris[tetraammine- μ -dihydroxocobalt(III)]cobalt(III) salts that Werner and his female *Doktorandin* Sophie Matissen (Ph.D., Universität Zürich, 1912) succeeded in resolving were first prepared by Jørgensen (51), whom Werner graciously acknowledged in his Nobel acceptance address. With Ernst Berl (Ph.D., Universität Zürich, 1911), Ernst Zingeler (Ph.D., Universität Zürich, 1902), and Gustav Jantsch (Ph.D., Universität Zürich, 1907), Werner had showed that they possess the constitution $[\text{Co}\{(\text{OH})_2\text{Co}(\text{NH}_3)_4\}_3]\text{X}_6$ (52). They are thus struc-

turally similar to $[\text{Men}_3]\text{X}$ salts with the inorganic $[(\text{OH})_2\text{Co}(\text{NH}_3)_4]^+$ ions in place of the organic ethylenediamine molecules and should therefore be capable of existing in optically active forms.

The resolution of the bromides with silver (+)-bromocamphorsulfonate was very tedious because of the slight differences in solubility between the diastereomers. Optical rotation measurements were difficult because of the dark color of the solutions and were made in 50% aqueous acetone to minimize the rapid racemization. Nevertheless, a modified form of the resolution is reproducible (53) and has been adapted as an undergraduate laboratory experiment (54). Since Werner's classic resolution only five other completely carbon-free inorganic complexes have been resolved, *viz.*,



Conclusion

From the inception of his studies of optical activity, Werner did not limit himself to the complexes of cobalt. He succeeded in resolving coordination compounds of chromium(III), iron(II), rhodium(III), iridium(III), and platinum(IV), which are discussed elsewhere (59). He continued his research with cobalt to include complexes with optically active unsymmetrical ligands. For example, he and his assistant A. P. Smirnoff (60) resolved *cis*- $[\text{Co}(\text{NO}_2)_2\text{enpn}]\text{Br}$; this compound illustrated a new and complicated type of isomerism, which arises from three causes – (1) *cis* (flavo) geometric isomerism, (2) ligand isomerism ((+)-pn or (-)-pn), and (3) structural isomerism caused by the unsymmetrical nature of the pn ligand (*cis* isomer only). It is a tribute to Werner's octahedral model that he could predict the existence of ten optically active isomers of this complex and a tribute to his experimental skill that he was able to isolate them even though he was unable to assign unambiguous structures to all of them.

In his last article (61), published with Jeanne Elisabeth Schwyzer (Ph.D., Universität Zürich, 1919) and Walter Karrer (Ph.D., Universität Zürich, 1919), more than a year after his premature death, Werner confirmed his view that β -diketonate anions occupy two coordination positions (62, 63) by preparing optically active acetylacetonatobis(ethylenediamine)cobalt(III) and propionylacetonatobis(ethylenediamine)cobalt(III) salts.

Thus, during his last years, Werner devoted himself almost exclusively to the optically active complexes that had proved his stereochemical views and had brought him the Nobel Prize in Chemistry. His studies of these complexes underlie much of the more recent and sophisticated studies of the thermodynamics; kinetics; visible, ultraviolet, infrared, NMR, and EPR spectra; rotatory dispersion; circular dichroism; ligand exchange; racemization; absolute configuration; and other aspects of these and similar compounds. Although some of his resolution methods have been improved upon, and his specific rotation values have proved to be too low, he was a true pioneer who first opened a previously unsuspected field. In his last works he stood on the threshold of an extremely complex area of research – the study of optically active coordination compounds containing optically active ligands. If his creative and fruitful life had not been cut short by his death at the early age of 53, Alfred Werner certainly would have made many significant contributions to this field.

ACKNOWLEDGMENT

The author is indebted to the referees for helpful comments and suggestions.

REFERENCES AND NOTES

1. "Hör, Alter, ich rate dir wirklich sehr,
Gib mir das Kobalt gutwillig her.
Nicht Hölle und Teufel sollen mich halten,
Noch heute will ich das Kobalt spalten."
2. "Ja, vom Morgen bis Abend spät,
Wird jetzt nur noch gedreht, ja gedreht,
Bis die ganze grosse Welt
Sich drehend in Ohnmacht fällt."
3. *Drehen und Spalten: Tragikomisches Volksspiel in einem Akt*, Weihnachtskommers der Universitäts-Chemiker, 15. Dezember 1911, Dissert.-Druckerei Gebr. Leemann & Co., Zürich-Selnau, 1911. For excerpts translated into English see G. B. Kauffman and H. K. Doswald, "Paul Karrer—Rotating and Resolving: A Tragicomic Popular Play," *Chemistry*, **1974**, 47(2), 8-12. The entire playlet was performed in English at the van't Hoff–Le Bel Centennial Symposium, 168th National Meeting, American Chemical Society, Atlantic City, NJ, September 11-12, 1974 with George B. Kauffman in the role of Werner and with a cast of internationally distinguished chemists, including a Nobel laureate. It was also performed at the California State University, Fresno, May 14, 1974; the Australian National University, Canberra, December, 1975; and a meeting of the Penn-Ohio Border Section, American Chemical Society, Youngstown, OH, February 20, 1976.
4. G. B. Kauffman, *Alfred Werner: Founder of Coordination Chemistry*, Springer-Verlag, New York, 1966.
5. V. L. King, "A Rough but Brilliant Diamond," *J. Chem. Educ.*, **1942**, 19, 345.
6. For biographical information on King see G. B. Kauffman, *Classics in Coordination Chemistry, Part 1: The Selected Papers of Alfred Werner*, Dover, New York, 1968, pp. 157-158.
7. G. B. Kauffman, "The First Resolution of a Coordination Compound," in O. B. Ramsay, Ed., *van't Hoff–Le Bel Centennial*, American Chemical Society, Washington, DC, 1975, pp. 126-142.
8. J. Read, *Humour and Humanism in Chemistry*, G. Bell & Sons, London, 1947, p. 264. John Read, the historian of chemistry, received his Ph.D. under Werner in 1907 with an organic stereochemical dissertation.
9. P. Debye, letter to George B. Kauffman, December 4, 1963.
10. G. B. Kauffman and I. Bernal, "Overlooked Opportunities in Stereochemistry. Part II. The Neglected Connection between Metal-Ammines (Alfred Werner) and Organic Onium Compounds (William Jackson Pope)," *J. Chem. Educ.*, **1989**, 66, 293-300.
11. A. Werner, "Beitrag zur Konstitution anorganischer Verbindungen," *Z. Anorg. Chem.*, **1893**, 3, 267-330. For an annotated English translation see Ref. 6, pp. 9-88.
12. G. T. Morgan, "Alfred Werner," *J. Chem. Soc.*, **1920**, 117, 1639-1648.
13. P. Karrer, "Prof. Alfred Werner," *Neue Zürcher Zeitung*, No. 1804, Erstes Morgenblatt, November 21, 1919, Feuilleton p. 1; reprinted in *Vierteljahresschrift der Naturforschenden Gesellschaft in Zürich*, **1919**, 64, 851-854.
14. I. Lifschitz, "Alfred Werner," *Z. Elektrochem.*, **1920**, 26, 514-529. Israel Lifschitz received his *venia legendi* (the right to lecture) with a *Habilitationsschrift* (a paper embodying the results of original and independent postdoctoral research) at Universität Zürich in 1911.
15. Nobel Foundation, *Nobel Lectures Including Presentation Speeches and Laureates' Biographies: Chemistry, 1901-1921*, Elsevier, Amsterdam, 1966, p. 253. Werner received the following nominations for the Nobel Prize: 1907, Eugen Bamberger; 1908, Adolf von Baeyer, Luigi Balbiano, E. Grandmougin, Georg Lunge, and Frederic P. Treadwell; 1911, Otto Dimroth, A. F. Holleman, and Paul Walden; 1912, Emil Fischer, Arnold Lang, Johannes Thiele, and Edgar Wedekind; and 1913, Svante A. Arrhenius, who also nominated Theodore W. Richards (the 1914 winner): Emil Fischer, Arthur R. Hantzsch, Werner's *Doktorvater*, and Paul Sabatier (the 1912 winner). All the 1912 and 1913 nominators except Fischer in 1911 specifically cited Werner's work on optical activity. In both 1912 and 1913 Friedrich Rose and Hans Rupe nominated Werner, but because their nominations were received after the February 1 deadlines in both years they were disallowed (information and letters of nomi-

- nation, courtesy of Kai-Inge Hillerud, Secretary to the Nobel Archives Committee, the Royal Swedish Academy of Sciences). See also E. Crawford, J. L. Heilbron, and R. Ullrich, *The Nobel Population 1901-1937: A Census of the Nominators and Nominees for the Prizes in Physics and Chemistry*, Office for History of Science and Technology, University of California, Berkeley, CA, Office for History of Science, Uppsala University, Uppsala, 1987, pp. 174, 176, 177, 186-195.
16. A. Werner and H. E. Conrad, "Über die optisch aktiven Transhexahydroptalsäuren," *Ber. Dtsch. Chem. Ges.*, **1899**, 32, 3046-3055.
 17. Although Werner's isolation of the long-sought, unstable *cis*-tetraamminedichlorocobalt(III) salts (violeo salts), *cis*-[CoCl₂(NH₃)₄]X (A. Werner, "Über 1.2-Dichlorotetrammin-kobaltisalze (Ammoniakvioleosalze)," *Ber. Dtsch. Chem. Ges.*, **1907**, 40, 4817-4825; for an annotated English translation see Ref. 6, pp. 144-154) convinced his primary scientific adversary Sophus Mads Jørgensen (1837-1914) that the Blomstrand-Jørgensen chain theory could not be correct (G. B. Kauffman, "Sophus Mads Jørgensen and the Werner-Jørgensen Controversy," *Chymia*, **1960**, 6, 180-204), Werner's success in preparing two and only two isomers for complexes of types MA₄B₂ and MA₃B₃ was not sufficient to prove the octahedral configuration. In spite of such "negative" evidence, it could be argued that failure to isolate a third isomer did not necessarily prove its non-existence. The resolution of asymmetric coordination compounds would provide Werner with the "positive" proof that he needed.
 18. A. Werner and A. Vilmos, "Beitrag zur Konstitution anorganischer Verbindungen. XVII. Über Oxalatodiäthylendiaminkobaltisalze (CoC₂O₄en₂)X," *Z. anorg. Chem.*, **1899**, 21, 145-158, p. 147.
 19. These salts were not the first coordination compounds to be resolved, but Werner succeeded in resolving them in 1912 and 1914 - (a) A. Werner and T. P. McCutcheon, "Zur Kenntnis des asymmetrischen Kobaltatoms. VII.," *Ber. Dtsch. Chem. Ges.*, **1912**, 45, 3281-3287; (b) A. Werner and J. Bosshart, "Zur Kenntnis des asymmetrischen Kobaltatoms. XI. Über Oxalatodiäthylendiamin-kobaltisalze und eine neue Spaltungsmethode für racemische anorganische Verbindungen," *Ber. Dtsch. Chem. Ges.*, **1914**, 47, 2171-2182.
 20. Werner's former fellow doctoral student, friend, and collaborator on the classical conductance studies of coordination compounds - the first published experimental work in support of his coordination theory (A. Werner and A. Miolati, "Beiträge zur Konstitution anorganischer Verbindungen, I. Abhandlung," *Z. physik. Chem.*, **1893**, 12, 35-55; "Beiträge zur Konstitution anorganischer Verbindungen, II. Abhandlung," *Z. physik. Chem.*, **1894**, 14, 506-521; for annotated English translations see Ref. 6, pp. 92-139). For a biography see G. B. Kauffman, "Arturo Miolati (1869-1956)," *Isis*, **1970**, 61, 241-253.
 21. (a) A. Werner, letter to Lev Aleksandrovich Chugaev (1873-1922), the Russian chemist who was an enthusiastic proponent of his coordination theory, November 15, 1907. (b) For a biography see G. B. Kauffman, "Terpenes to Platinum: The Chemical Career of Lev Aleksandrovich Chugaev," *J. Chem. Educ.*, **1963**, 40, 656-665.
 22. Jean V. Dubsy (1882-1946) received his Ph.D. under Werner's supervision in 1908 and served as his assistant. For a biography see F. Jursík and G. B. Kauffman, "J. V. Dubsy and His Participation in Werner's Coordination Theory," in G. B. Kauffman, Ed., *Coordination Chemistry: A Century of Progress*, American Chemical Society, Washington, DC, 1994, pp. 58-67.
 23. V. L. King, *Nachweis des Oktaheder Formels der Kobaltiake: Arbeit für Ph.D., Zürich, 1910-1912*, p. 2. This laboratory notebook is preserved in the Chemistry Department Library at Dartmouth College, Hanover, NH, from which King received his bachelor's degree in 1907.
 24. F. S. Kipping and W. J. Pope, "Studies of the Terpenes and Allied Compounds. The Sulphonic Derivatives of Camphor," *J. Chem. Soc.*, **1893**, 63, 548-604. Werner used their procedure, but improved syntheses have since been devised, e.g., G. B. Kauffman, "A Rapid, Simplified Synthesis of α -Bromo-*d*-camphor- π -sulfonic Acid and Its Salts," *J. prakt. Chem.*, **1966**, [4] 33, 295-297.
 25. These compounds, like many of the complexes that Werner used to support his coordination theory and discredit the rival Blomstrand-Jørgensen chain theory, was first prepared by Jørgensen ("Ueber Metalldiaminverbindungen. VII. Ammin-Aethylendiamin-Chloropurpureokobaltsalze," *J. prakt. Chem.*, **1890**, [2] 41, 453-459).
 26. A. Werner, "Zur Kenntnis des asymmetrischen Kobaltatoms. I.," *Ber. Dtsch. Chem. Ges.*, **1911**, 44, 1887-1898; for an annotated English translation see Ref. 6, 159-173.
 27. G. B. Kauffman and E. V. Lindley, Jr., "Resolution of the *cis*-Bromoamminebis(ethylenediamine)cobalt(III) Ion," *Inorg. Syn.*, **1976**, 16, 93-97.
 28. G. B. Kauffman and E. V. Lindley, Jr., "A Classic in Coordination Chemistry: A Resolution Experiment for the Inorganic Laboratory," *J. Chem. Educ.*, **1974**, 51, 424-425.
 29. V. L. King, "Über Spaltungsmethoden und ihre Anwendung auf komplexe Metall-Ammoniakverbindungen," Dissertation, Universität Zürich, J. J. Meier, Zürich, 1912, p. 59.
 30. G. B. Kauffman, "Alfred Werner's Research in Organic Stereochemistry," *Die Naturwissenschaften*, **1976**, 63, 324-327.
 31. G. B. Kauffman, "The Discovery of Optically Active Coordination Compounds: A Milestone in Stereochemistry," *Isis*, **1975**, 66, 38-62.
 32. A. Werner and M. Basyrin, "Über die optisch aktiven Dimethyl-bernsteinsäuren," *Ber. Dtsch. Chem. Ges.*, **1913**, 46, 3229-3232.

33. A. Werner, "Zur Kenntnis des asymmetrischen Kobaltatoms. II.," *Ber. Dtsch. Chem. Ges.*, **1911**, *44*, 2445-2555.
34. A. Werner, "Zur Kenntnis des asymmetrischen Kobaltatoms. III.," *Ber. Dtsch. Chem. Ges.*, **1911**, *44*, 3272-3278.
35. A. Werner, "Zur Kenntnis des asymmetrischen Kobaltatoms. IV.," *Ber. Dtsch. Chem. Ges.*, **1911**, *44*, 3279-3284.
36. A. Werner, "Zur Kenntnis des asymmetrischen Kobaltatoms. V.," *Ber. Dtsch. Chem. Ges.*, **1912**, *45*, 121-130.
37. A. Werner, "Über Spiegelbildisomerie bei Rhodium Verbindungen. I.," *Ber. Dtsch. Chem. Ges.*, **1912**, *45*, 1228-1236.
38. A. Werner, "Zur Konstitution basischer Salze und analog konstituierte Komplexsalze. I.," *Ber. Dtsch. Chem. Ges.*, **1907**, *40*, 4441-4449.
39. A. Werner and [T. P.] McCutcheon, "Zur Kenntnis des asymmetrischen Kobaltatoms. VI.," *Ber. Dtsch. Chem. Ges.*, **1912**, *45*, 3281-3287.
40. J. C. Bailar, Jr. and R. W. Auten, "The Stereochemistry of Complex Inorganic Compounds. I. The Walden Inversion as Exhibited by Diethylenediaminocobaltic Compounds," *J. Am. Chem. Soc.*, **1934**, *56*, 774-776. D. D. Brown, Christopher K. Ingold, and Ronald S. Nyholm ("Mechanism, Kinetics, and Stereochemistry of Octahedral Substitutions. Part I. Edge-displacement as a Hypothesis Linking Mechanism with the Stereochemistry of Substitution," *J. Chem. Soc.*, **1953**, 2674-2680) suggested that Bailar should not refer to his discovery as a Walden inversion because it implied an octahedral type inversion analogous to the familiar umbrella inversion of tetrahedral carbon.
41. A. Werner and Y. Shibata, "Zur Kenntnis des asymmetrischen Kobaltatoms. VII.," *Ber. Dtsch. Chem. Ges.*, **1912**, *45*, 3287-3293.
42. A. Werner and G. Tschernoff, "Zur Kenntnis des asymmetrischen Kobaltatoms. VIII.," *Ber. Dtsch. Chem. Ges.*, **1912**, *45*, 3294-3301.
43. A. Werner, "Zur Kenntnis des asymmetrischen Kobaltatoms. IX.," *Ber. Dtsch. Chem. Ges.*, **1913**, *46*, 3674-3683.
44. A. Werner, "Über Verbindungen mit zwei asymmetrischen Kobaltatomen," lecture, Verein Deutscher Chemiker, Freiburg im Breisgau, Germany, May 29, 1912.
45. A. Werner, H. Kuh, and P. Wüst, "Zur Kenntnis des asymmetrischen Kobaltatoms. X.," *Ber. Dtsch. Chem. Ges.*, **1914**, *47*, 1961-1979.
46. A. Werner, F. Salzer, M. Pieper, J. Fürstenberg, S. Malmgren, M. Grigorieff, A. Grün, E. Bindschedler, and E. Welti, "Über mehrkernige Metallammoniake. X.," *Ann.*, **1910**, *375*, 1-144.
47. A. Werner, "Zur Kenntnis des asymmetrischen Kobaltatoms. XII. Über optische Aktivität bei kohlenstofffreien Verbindungen," *Ber. Dtsch. Chem. Ges.*, **1914**, *47*, 3087-3094. For an annotated English translation see Ref. 6, pp. 177-184.
48. A. Werner, "Neue Ergebnisse der Spaltungsversuche mit anorganischen Verbindungen," in *Schweizerische Chemische Gesellschaft: Ansprachen zu Ehre des Herrn Prof. Dr. Werner gehalten bei der Versammlung vom 2. Mai 1914 in Neuenberg*, Imprimerie Th. Eberhard, Lausanne, Switzerland, 1914, pp. 35-47 (in German); *Archives des Sciences physiques et naturelles*, **1914**, *37*, 455 (in French).
49. A. Werner, "Über anorganische Konstitutions- und Konfigurationsfragen," lecture, Naturforschende Gesellschaft Zürich, Zürich, June 8, 1914.
50. A. Werner, "Sur l'activité optique de composés chimiques sans carbone," lecture, Société Chimique de France, Paris, August 17, 1914; *Compt. rend.*, **1914**, *159*, 426-429.
51. S. M. Jørgensen, "Zur Konstitution der Kobalt-, Chrom-, und Rhodiumbasen. X. Mitteilung," *Z. Anorg. Chem.*, **1898**, *16*, 184-197. For a more recent synthesis see G. B. Kauffman and R. P. Pinnell, "Tris[tetrammine- μ -dihydroxo-cobalt(III)]cobalt(III) Sulfate 4-Hydrate," *Inorg. Syn.*, **1960**, *6*, 176-179.
52. A. Werner, E. Berl, E. Zinggeler, and G. Jantsch, "Über mehrkernige Metallammoniake. III. Über Dodekamminhexoltetrakobaltisalze und Hexaäthylendiaminhexoltetrakobaltisalze: $\{Co[(HO)_2CoA_4]_3\}X_6$," *Ber. Dtsch. Chem. Ges.*, **1907**, *40*, 2103-2125.
53. T. Yasui, T. Ama, and G. B. Kauffman, "Resolution of the Dodecaamminehexa-m-hydroxo-tetracobalt(III) Ion," *Inorg. Syn.*, **1992**, *29*, 169-174.
54. T. Yasui, T. Ama, and G. B. Kauffman, "The Resolution of a Completely Inorganic Coordination Compound: A Modified Classic Experiment for the Inorganic Laboratory," *J. Chem. Educ.*, **1989**, *66*, 1045-1048.
55. F. G. Mann, "The Complex Metallic Salts of Sulfamide," *J. Chem. Soc.*, **1933**, 412-419.
56. R. D. Gillard and F. L. Wimmer, "Inorganic Optical Activity," *J. Chem. Soc., Chem. Comm.*, **1978**, 936-937.
57. S. Shimba, S. Fujinami, and M. Shibata, "Preparation of a Novel Amminebromocyno[1,4,7-triazacyclononane]cobalt(III) Complex," *Chem. Lett.*, **1979**, 783-784.
58. J. Podlaha and M. Ebert, "Triphosphitochromic Acid," *Nature (London)*, **1960**, *188*, 657-658; *Zh. Neorgan. Khim.*, **1962**, *7*, 2185-2191 (in Russian); "Triphosphite Chromic Acid," *Russ. J. Inorg. Chem. (Engl. transl.)*, **1962**, *7*, 1130-1136.
59. G. B. Kauffman, "Alfred Werner's Research on Optically Active Coordination Compounds," *Coord. Chem. Rev.*, **1974**, *12*, 105-149.
60. A. Werner, "Über eine neue Isomerieart bei Kobaltverbindungen und Verbindungen mit asymmetrischen Kobalt und Kohlenstoff," *Helv. Chim. Acta*, **1918**, *1*, 5-32; also lecture with the same title,

Schweizerische Naturforschende Gesellschaft, Geneva, September 14, 1915.

61. A. Werner, J. E. Schwyzer, and W. Karrer, "Optisch-aktive Kobaltsalze mit β -Diketonreste im Komplex," *Helv. Chim. Acta*, **1921**, *4*, 113-129.
62. A. Werner, "Über Acetylacetonverbindungen des Platins," *Ber. Dtsch. Chem. Ges.*, **1901**, *34*, 2584-2593.
63. A. Werner and S. Matissen, "Zur Konstitution der inneren Metallkomplexsalze," *Helv. Chim. Acta*, **1918**, *1*, 78-84.

ABOUT THE AUTHOR

George B. Kauffman, Professor of Chemistry at California State University, Fresno, is the author or editor of 16 books and more than 1400 papers, reviews, and

encyclopedia articles on chemistry, the history of chemistry, and chemical education. A Dexter Awardee (1978), he has long been active in ACS affairs, serving HIST as Symposium Chairman (1966, 1968, 1970, 1993), Program Chairman (1967-1970), and Division Chairman (1969-1970). A Guggenheim fellow and Contributing Editor of five journals, he recently became the first recipient of the President's Medal of Distinction, the highest nondegree award presented by CSUF. His other honors include CSU System Outstanding Professor, MCA Catalyst Award, ACS Pimentel Award in Chemical Education, Marc-Auguste Pictet Medal, and Chugaev, Kurnakov, and Chernyaev Medals of the USSR Academy of Sciences.

American Chemists and Chemical Engineers

Volume 2

Wyndham D. Miles and Robert F. Gould, Editors

269 New biographical sketches, each fully referenced by 40 contributors. Includes many early chemists who started teaching "natural science," which later branched into chemistry, physics, botany, and so on; 37 born before 1800; 10 college presidents; 14 original ACS members; 4 charter members of AIChE; 19 who saw Civil War service; 7 historians of chemistry.

360 pages, including 67-page name and subject index.

Price: \$23.00, postpaid in US and Canada. Additional \$2.00 elsewhere.

Name: _____

Address: _____

Send to:
Gould Books
20 Forest Brook Rd.,
Guilford, CT 06437

WILLIAM DRAPER HARKINS: AN EARLY ENVIRONMENTAL CHEMIST IN MONTANA (1900-1912)

Richard E. Rice, James Madison University, and George B. Kauffman, California State University, Fresno

William Draper Harkins (1873-1951) is no longer the household name among chemists that it was earlier this century. As a physical chemist at the University of Chicago for nearly forty years, he was well known for his research in nuclear and atomic structure and isotope separation, as well as surface chemistry, for which he received the Willard Gibbs gold medal from the ACS Chicago Section in 1928(1). In a posthumous tribute to Harkins, the Nobel laureate Robert S. Mulliken recalled (2):

When I came to Chicago as a graduate student in 1918, it was because I had read about Harkins' pioneering work toward the understanding of nuclear structure, a subject ignored at that time by American physicists. In fact, during the period 1913-1928, Harkins and his students were the only Americans engaged in work relating to the structure of the atomic nucleus.

Scientists outside Harkins' area of research also recognized the importance of his work; for example, in 1923 Harvey W. Wiley grouped him with Soddy, Aston, and Rutherford in connection with his knowledge of "the constitution of the atom" (3).

Prior to his career at Chicago, however, Harkins spent a dozen years at the University of Montana in Missoula, where for much of that time his research interests were directed towards what would now be called environmental chemistry. After receiving his B.A. in chemistry from Stanford University in May, 1900(4),

he became Instructor in Chemistry and Physics (1900-1901) and the Professor and Head of Chemistry (1901-1912) at Montana. Chemistry prospered under Harkins' leadership, and his first departmental report outlined the changes he had already made or was planning to make in the curriculum: a new course in physical chemistry; expansion of introductory chemistry from one semester to a full year; new courses in inorganic preparations, organic analysis, and gas analysis once the necessary equipment arrived from Germany; a new course in industrial chemistry in conjunction with the Department of Mechanical Engineering; and a lecture course in analytical chemistry to supplement the existing laboratory work(5).

The University of Montana had begun offering graduate study before the turn of the century—the first master's degree was awarded in geology in 1899 (6)—and in his report for 1902-1903 Harkins mentioned the three graduate students then studying chemistry (7):

Two of these work in the laboratory from eight to ten hours per day, Saturdays included. The third, Mr. Martin Jones, spends most of his time in the chemical laboratory working upon the analysis of Montana Ores, his thesis for the Masters Degree being upon this subject.

Another candidate for the master's degree was George Westby, a 1901 graduate of the University and subsequently Chief Chemist of the Washoe Smelter at Anaconda. The topic of his thesis, which was apparently

never completed, "An Examination of the Waters of Montana," prompted Harkins to write (7):

It is of great importance to the people of any community to know whether the water they are using is pure or impure. If the former, no one need be afraid of moving into the community and using the water in abundance; if the latter, steps should be taken at once to purify the water, or to obtain a new source of supply.

Interestingly, while he was directing graduate students in chemistry, Harkins held no advanced degree himself, though in the summer of 1901 he had enrolled in some graduate courses at the University of Chicago: Advanced Experimental Physics, Special Methods in Quantitative Analysis, and Inorganic Properties. He returned to Chicago again during the summer of 1904 to take three more graduate courses: Kinetic Theory, Hertzian Waves, and Chemical Research (8). During the fall semester of 1905, he took a leave of absence from his teaching duties at Montana to register as a graduate student at Stanford. In his report for that academic year Harkins wrote about himself in the third person (9):

During the last half of the year 1905, the Professor of Chemistry was absent on leave without pay, and spent the time in research work at Leland Stanford, Jr., University. It is impossible, at the present time, for any college teacher of science to keep from growing rusty in his own line of work, unless he is given frequent leaves of absence in order that he may learn what other workers in the same subject are doing. This is particularly true in the Rocky Mountain states, where, especially in the case of Chemistry, there are very few scientists of note. . . . It would be, then, good policy for the institution, as is already done in

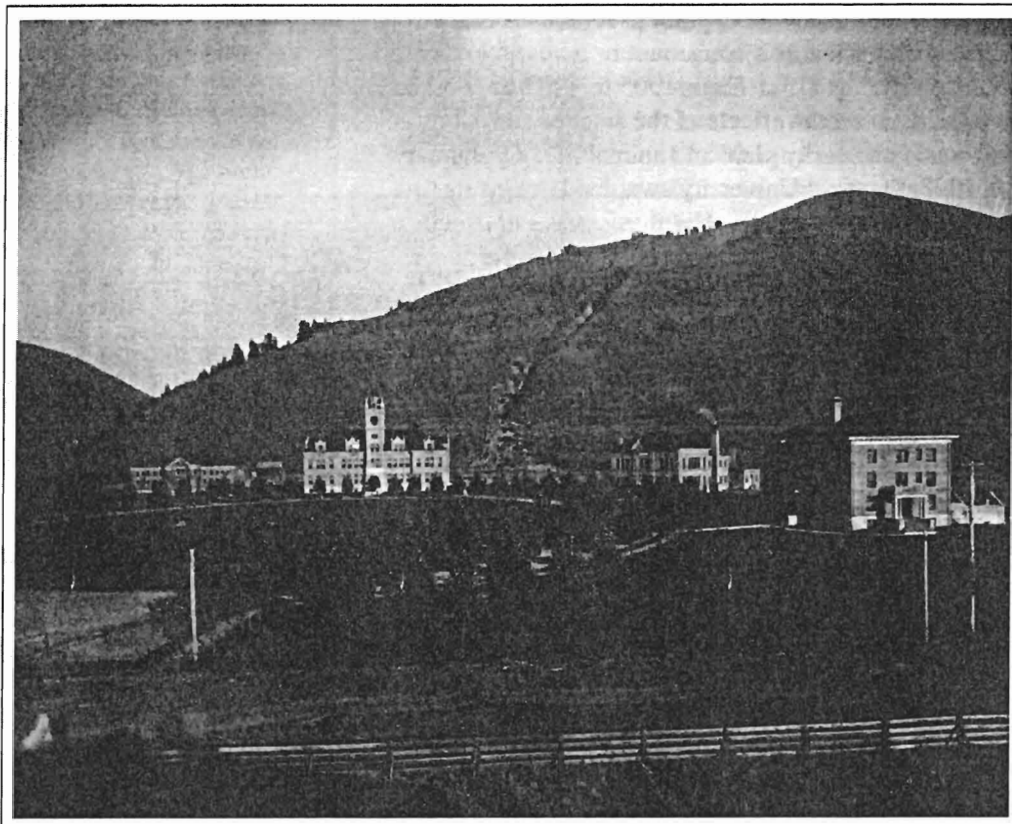


Figure 1 The campus of the University of Montana in 1904. From left to right, Gymnasium (razed in 1965), University (Main) Hall, Science Hall (razed in 1983; the Davidson Honors College recently erected a new building on this site), and Women's Hall. University Hall is still the main administrative building on campus, and Women's Hall now houses Mathematical Sciences (photograph from the K. Ross Toole Archives, Mansfield Library, University of Montana)

most of the other leading institutions, to give the instructors a leave of absence on pay each seventh year, on condition that this year be spent in research or study at some great university, or in some laboratory in which first class research work is done.

At nearly the same time that Harkins wrote this appeal, he was negotiating for the position of Head of Chemistry at the University of Nevada, and one of his major negotiating points, along with a significant increase in salary, was the possibility of paid leaves of absence (10).

As a graduate student at Stanford, Harkins carried out his research under the direction of Robert Eckles Swain (1875-1961), who had received his B.A. from Stanford only a year before Harkins. Awarded a Ph.D. in biochemistry in 1904 by Yale University (11), where he studied with the physiological chemist Lafayette Mendel (1872-1935) (12), Swain had become interested in the effects of industrial pollutants on human health and the environment. He undertook several surveys and

investigations of smelter operations for the Federal government and acted as a consultant to many large-scale pollution studies (13). From 1905 to 1907 he worked with Harkins on the effects of the smelter emissions at Anaconda on nearby plant and animal life. On January 10, 1908, Stanford University awarded Harkins its first Ph.D. in chemistry (14). His thesis was published in 1907-1908 as a series of three papers under the general heading *Papers on Smelter Smoke* (15).

Harkins was again on leave from his regular teaching duties during the fall semester of 1909. In May of that year, he sailed from New York to Liverpool on the *Lusitania*. First, he attended the International Congress of Applied Chemistry in London (16), and then he spent the summer doing research with Fritz Haber at the Institut für physikalische Chemie in Karlsruhe, where he was given a project on surface tension. For the fall semester he was at the Massachusetts Institute of Technology, working with Arthur Amos Noyes and Gilbert Newton Lewis on electrolyte solubility (17). In his acceptance speech for the Willard Gibbs medal nearly twenty years later, Harkins recalled (18):

As an undergraduate, research appealed to me as one

of life's great adventures . . . While the study of the atom and of radioactivity, then a new subject, had an extreme fascination, there were two subjects of investigation in physical chemistry which seemed to me of such minor importance that I took a firm resolution never to be enticed into working on either of them. These two fields of work were surface tension and solubility.

Luckily for Harkins, Haber and Noyes overcame his undergraduate resolution since these two fields proved influential in shaping his later research interests (19).

From MIT Harkins returned to Montana for only two more years before leaving for the University of Chicago. In addition to his academic duties during his twelve-year stay in Missoula, Harkins was also president of the Missoula Board of Health and acted as a consulting chemist for the U.S. Justice Department, the Mountain Copper Company, and the Deer Lodge Valley Farmers' Association (20). The latter two organizations were specifically mentioned by Ebaugh (21) for their roles in the growing "number of damage suits brought against smelting companies" beginning about the turn of the century. In another well-known case involving pollution by two copper companies near Ducktown,

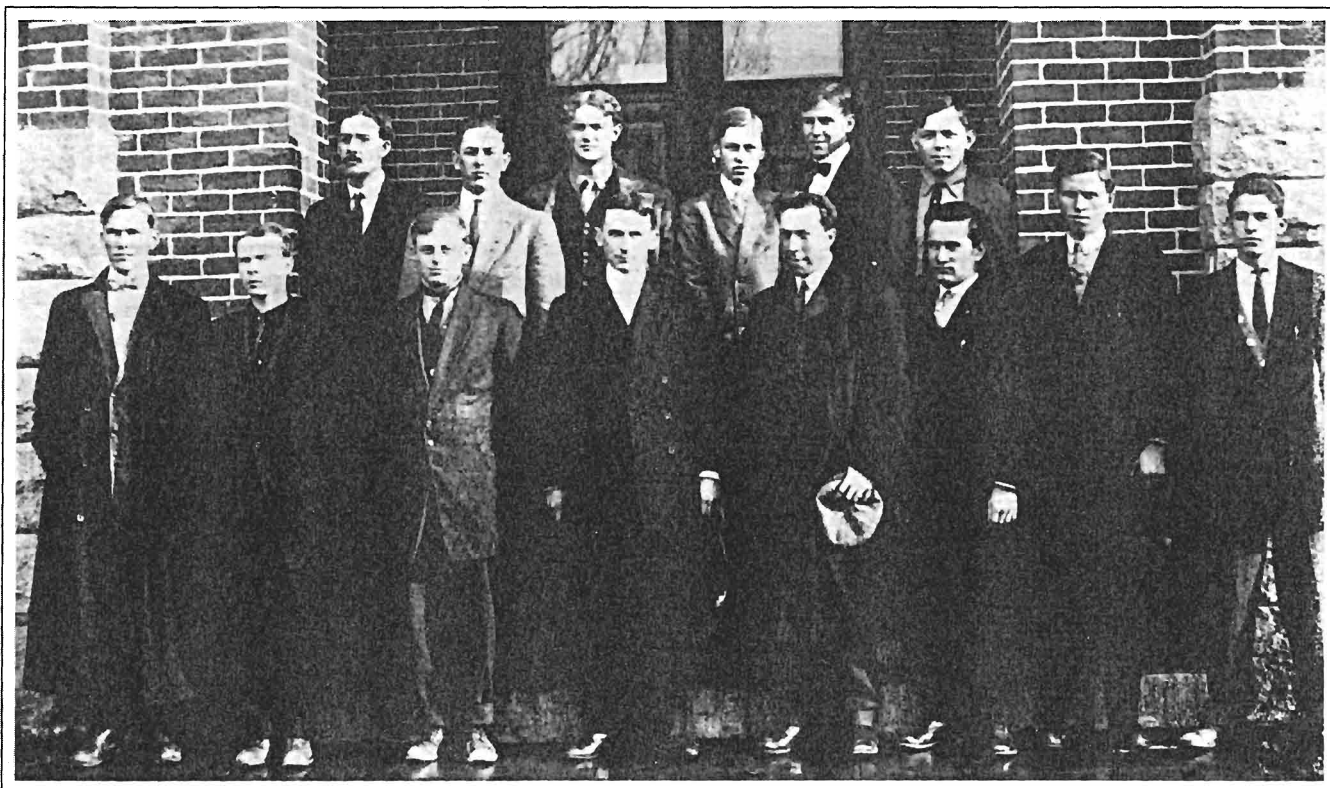


Figure 2 William D. Harkins (front row, left center) as the young Professor of Chemistry at the University of Montana; the others are unidentified faculty and students (undated photograph from the K. Ross Toole Archives, Mansfield Library, University of Montana)



Figure 3 Copper-smelting operation at Anaconda in the early 1900's (undated photograph from the K. Ross Toole Archives, Mansfield Library, University of Montana)

Tennessee (22), Harkins was called to testify as an expert witness (23). Although publications on smelter problems in the U.S. at this time usually cited work carried out in Germany in the second half of the nineteenth century (24), there was increasing scientific interest in such work in America as evidenced by the more than seventy entries of articles and patents under the subject heading "Fumes, smelter" in the First Decennial Index (1907-1916) of *Chemical Abstracts*.

Of all of Harkins' consulting work, however, his longest and most extensive was with the Deer Lodge Valley Farmers' Association, for which he investigated the effects of smelter emissions at Anaconda on plant and animal life during the years 1902-1910 (25). Anaconda had been the site of smelting operations since 1884, when Marcus Daly, Montana's "Copper King," built the first smelter there for the Anaconda Copper Mining Company (26). Emissions from these operations had denuded the surrounding area of vegetation, which consisted mainly of shrubs that were considered useless, so their loss was not seen as a serious problem (27).

In January 1902, however, the Anaconda Company replaced its two existing smelters, which had a com-

bined capacity of 4,000 tons of ore per day, with a much larger single facility, the Washoe Smelter, with a capacity of 12,000 tons per day and four 225-foot smokestacks. In less than a year farmers in the vicinity of the new smelter reported the deaths of hundreds of sheep and cows, along with nearly 2500 horses (28).

In November 1902 Harkins began measuring arsenic concentrations in plants and soils at numerous locations within fifteen miles of the new smelter, and for these analyses he successfully modified the Marsh method for his samples, which contained iron (29). By heating the Marsh generator (Fig. 4) to 100°C, he was able to elimi-

nate the interference of iron in the quantitative reduction of arsenic oxides to arsine. His arsenic determinations on a series of eleven test samples, each containing 1.88 mg of arsenic and varying amounts of iron up to 0.4 g, all show an error of less than about 4% (30).

As a result of the deaths of so many animals and the high arsenic concentrations found in grass and hay in the valley around Anaconda, the company shut down the new smelter from July through September of 1903 in order to install a new system of flues and a 300-foot smokestack, which were expected to reduce the amounts of arsenic, copper, and sulfur dioxide in the emissions.

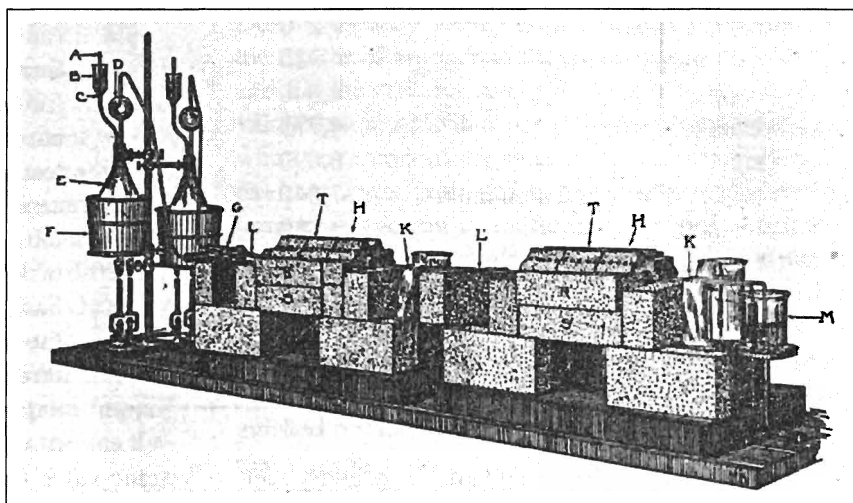


Figure 4 The Marsh generator used by Harkins in arsenic determinations (from reference 29, p. 520)

Animal deaths decreased dramatically, at least for a while, and in early 1905 the company announced that the modifications to the smelter had achieved their objective. The farmers, however, were not so sure; they believed that the problems had merely been altered, not eliminated, as the taller smokestack spread the smelter's emissions over a different—and larger—area than before (31). Their lawsuit against the Anaconda Company, filed in May 1905, eventually resulted in a small monetary award, but the judge refused to issue an injunction against the company, ruling that the smelter operation was of greater importance to the region than agriculture. The considerable attention generated by the case sparked several scientific investigations into the area's problems, including the studies of Harkins and Swain (32).

Beginning in mid 1905, Harkins and Swain carried out velocity determinations and chemical analyses on the smelter emissions from the 300-foot smokestack. On the basis of four samplings in July and August, they found that the smoke moved at an average velocity of 52.88 feet per second and that the stack emitted nearly 2.3 billion cubic feet of smoke per day (measured at an average outside temperature of 17°C). Their chemical analysis of the smoke, based on the same four samplings, is given as an average daily discharge in Table I (33). As an indication of the extreme amounts that these fig-

TABLE I
AVERAGE CONTENTS OF SMOKE FROM THE
WASHOE SMELTER

<u>Substance in Smoke</u>	<u>Discharge (lb/day) from Stack</u>
arsenic trioxide	59,270
antimony trioxide	4,320
copper	4,340
lead	4,775
zinc	6,090
oxides of iron and aluminum	17,840
bismuth	880
manganese	180
silica	10,260
sulfur trioxide	447,600
sulfur dioxide	4,636,000

ures represent, current EPA standards for primary copper smelters require that gaseous emissions contain no more than 0.065% sulfur dioxide by volume (34),

whereas the average percent determined by Harkins and Swain was 1.493% (33). These same EPA standards also limit the particulate matter emitted to 50 mg/m³ and arsenic to 75 kg/h (34), considerably less than the values of 1000 mg/m³ and 848 kg/h, respectively, calculated from Harkins and Swain's data for the Washoe Smelter.

Harkins and Swain (35) concluded that while the new flue system had indeed reduced arsenic and copper emissions to the atmosphere, significant amounts of arsenic trioxide, which condensed to the solid state in the flue, were still being discharged because it was too fine to settle out of the smoke moving at the observed velocities. They also concluded that the taller smokestack had decreased the amount of arsenic deposited in the immediate vicinity of the smelter but spread it over a greater area instead, just as the farmers had claimed. This was true for all the substances examined, and thus the damage to trees and other vegetation from sulfur dioxide extended to areas that had previously escaped its effects. By 1910 emissions from the Washoe Smelter had affected approximately 570 square miles of surrounding territory, a considerably larger area than that affected before the operation of the taller smokestack (36).

Harkins and Swain also investigated the amounts of arsenic and copper in vegetation, especially hay and wild grasses (37), and the arsenic poisoning of herbivorous animals (38) in the area surrounding the Washoe Smelter. Table II (39) lists some of Harkins' analyses carried out prior to the installation of the flue system and the construction of the taller smokestack (samples 1-5), and then shortly after the smelter resumed operation (samples 6-9). Harkins collected samples 2 and 5 from the same farm, but the former had been exposed to smelter emissions for approximately six months, while the latter, cut in July and stacked, had been exposed for less than three months. This difference in arsenic values was "fairly representative of a condition which will be found to prevail with notable uniformity throughout the analyses" (40). Samples 6 and 7 were collected shortly after the smelter had been restarted following its three-month shutdown, while sample 8 was exposed to smelter emissions for an additional month during a very dry period. The large arsenic value for sample 9 suggests that because of "its peculiar matted growth the moss may have collected the arsenic which fell upon it like a natural fiber, and held on to it from the previous year" (40).

TABLE II
ARSENIC AND COPPER IN GRASS AND HAY

Num.	Sample	Location	Date	As ₂ O ₃ (ppm)	Cu(ppm)
1	grass	0.25 E	Nov 1902	1551	1800
2	grass	3.0 W	Nov 1902	166	871
3	grass	4.0 W	Nov 1902	88	708
4	hay	1.5 S	Nov 1902	283	—
5	hay	3.0 W	Nov 1902	36	216
6	grass	4.0 E	Oct 1903	10	128
7	hay	5.0 SE	Oct 1903	13	—
8	grass	15.0 NNE	Nov 1903	52	164
9	moss	15.0 NNE	Nov 1903	405	237

Both by himself and in collaboration with Swain, Harkins continued to collect samples of vegetation from the vicinity of Anaconda for arsenic analysis at least through October 1907. Although these analyses by themselves did not justify any conclusions about the distribution of arsenic throughout the area, they did, in conjunction with arsenic analyses of soils and of the "finely divided dark-gray powder" (41) that collected everywhere in the vicinity of the smelter, lead to "only one interpretation, which is that the smelter smoke is the source of the arsenic found in such excessive amounts in the vegetation of the region around Anaconda" (42).

The final article in Harkins and Swain's series *Papers on Smelter Smoke* (15) focused on the large number of deaths of sheep, cows, and horses around Anaconda after the start-up of the new smelter in 1902. Harkins himself observed the carcasses of hundreds of animals during his travels over 100 square miles of territory surrounding Anaconda in November 1902. Many of these and other animals were dissected, and almost all of them gave evidence of acute or chronic arsenic poisoning (43). After tabulating the arsenic analyses of 82 organs, tissues, and fluids from nearly as many animals, Harkins and Swain tried to relate the amounts of arsenic ingested by some of the animals to the amounts found in their livers. This proved extremely difficult, however, and their attempt was unsuccessful. On average, they found larger amounts of arsenic in those animals kept closer to the smelter, but concluded that the condition of any particular animal was a more important factor in the retention of arsenic in the various tissues (44).

At the time of Harkins' work, there was not much data available on the arsenic poisoning of livestock, and

the little that was available was often contradictory. Therefore, to compare the arsenic concentrations in tissues of the autopsied animals and also to try to determine lethal amounts of arsenic, Harkins and Swain fed known amounts of arsenic to horses and sheep and then analyzed various tissues for their arsenic concentrations after the animals' deaths. These results, as well as those of other experiments they undertook toward the same ends, proved inadequate to provide much insight into their study of the effects

of arsenic on animals in the Anaconda region. As Harkins and Swain somewhat ruefully concluded (45):

The question of the amount of arsenic which will kill a farm animal, if fed daily, is a very important one to the chemist who undertakes to investigate the conditions existing in smelter regions. The effects depend so greatly upon the conditions that even after such an extensive investigation as that carried out by the veterinarians, pathologists, bacteriologists, and chemists, upon the present case, no very definite statements can be made in regard to this point. A study of Table I [Arsenic and Copper in Grass and Hay] of the second paper of this series will give some idea of the poisonous dose, for on almost all of the ranches listed, animals have been supposed to die from arsenical poisoning. On the other hand, there is almost no place in the farming district where some of the animals will not survive.

Despite the inconclusiveness of their investigations into the deaths of animals in the Anaconda region, Harkins and Swain gathered and published a large amount of valuable scientific data about a smelter region at a time when such conditions were not usually recognized as environmental problems nor generally considered appropriate topics for scientific investigations. Their work helped establish Swain's reputation in this field and led directly to his participation in a subsequent study of the Anaconda area for the Department of Justice (46).

For Harkins, however, these investigations represented only the opening chapter in a long and distinguished research career. By 1909 his attention was already shifting toward some of the topics that would occupy him for the rest of his life, and within the next two years his top priority was to find an academic position more conducive to his new interests. In 1912 he moved

to such a position at the University of Chicago, leaving behind the smelter problems at Anaconda and his career as an early environmental chemist in Montana.

REFERENCES AND NOTES

1. W. D. Harkins, "Surface Structure and Atom Building," *Science*, **1929**, 70, 433-442, 463-470; G.B. Kauffman, "William Draper Harkins (1873-1951): A Controversial and Neglected American Physical Chemist," *J. Chem. Educ.*, **1985**, 62, 758-761.
2. R. S. Mulliken, "William Draper Harkins, December 28, 1873-March 7, 1951," *Biogr. Mem. Natl. Acad. Sci. USA*, **1975**, 47, 52-53.
3. H. W. Wiley, "Charles Edward Munroe," *Ind. Eng. Chem.*, **1923**, 15, 649.
4. E. Hutchinson, *The Department of Chemistry, Stanford University, 1891-1976: A Brief Account of the First Eighty-Five Years*, Department of Chemistry, Stanford University, Stanford, CA, 1977, p. 86; K. Miscavage, Student Record Investigator, Stanford University, Letter to G. B. Kauffman, October 28, 1985.
5. W. D. Harkins, "Department of Chemistry," in *The Seventh Annual Report of the President of the University of Montana to the Montana State Board of Education, 1900-1901*, Independent Publishing Co., Helena, MT, 1901, pp. 37-39.
6. E. Douglass, "The Neocene Lake Beds of Western Montana and Descriptions of Some New Vertebrates from the Loup Fork," M.S. thesis, University of Montana, Missoula, 1899.
7. W. D. Harkins, "Department of Chemistry," in *The Ninth Annual Report of the President of the University of Montana to the Montana State Board of Education, 1902-1903*, Independent Publishing Co., Helena, MT, 1903, pp. 38-40. Neither of the master's theses mentioned by Harkins is listed in the holdings of the Mansfield Library at the University of Montana.
8. M. H. Sullivan, University of Chicago, Letter to G. B. Kauffman, October 17, 1985.
9. W. D. Harkins, "Department of Chemistry," in *The Twelfth Annual Report of the President of the University of Montana to the Montana State Board of Education, 1905-1906*, Independent Publishing Co., Helena, MT, 1906, pp. 39-40.
10. Correspondence between Harkins and J. E. Stubbs, President of the University of Nevada, August 1906. Harkins' inquiry about leaves of absence is in the copy of his telegram dated August 12, 1906. Box 1, Folder 3 of the William D. Harkins Papers in the University of Chicago Library Special Collections. Harkins decided against accepting the position at Nevada.
11. R. E. Swain, "The Formation of Kynurenic Acid by the Animal Body," Ph.D. dissertation, Yale University, 1904; *Doctors of Philosophy of Yale University with the Titles of Their Dissertations, 1861-1927*, Yale University, New Haven, 1927, p. 164.
12. H. B. Vickery, "Lafayette Benedict Mendel," in *Dictionary of Scientific Biography*, C. C. Gillispie, Ed., Charles Scribner's Sons, New York, 1974, Vol. 9, pp. 284-286; also see entries in *The National Cyclopædia of American Biography*, James T. White & Co., New York, 1934, current Vol. D, pp. 381-382, and *ibid.*, 1937, vol. 26, p. 424.
13. Hutchinson, Ref. 4, pp. 6-7, 11-13. See also "Robert Eckles Swain" in *The National Cyclopædia of American Biography*, James T. White & Co., Clifton, NJ, 1977, Vol. 57, p. 391.
14. Miscavage, Ref. 4; Hutchinson, Ref. 4, p. 87.
15. W. D. Harkins and R. E. Swain, "The Determination of Arsenic and Other Solid Constituents of Smelter Smoke, with a Study of the Effects of High Stacks and Large Condensing Flues," *J. Am. Chem. Soc.*, **1907**, 29, 970-998; R.E. Swain and W.D. Harkins, "Arsenic in Vegetation Exposed to Smelter Smoke," *ibid.*, **1908**, 30, 916-928; W. D. Harkins and R. E. Swain, "The Chronic Arsenical Poisoning of Herbivorous Animals," *ibid.*, **1908**, 30, 928-946. Harkins' dissertation is cited in *Degrees Conferred by Stanford University, June 1892 - June 1924*, Stanford University, Stanford, CA, 1928.
16. The article "University Honored," *Weekly Kaimin*, April 28, 1909, pp. 1+, contains this information, but also mistakenly reports that Harkins would be "doing research work with Professor Nernst in the University of Berlin." Whether this was simply a mistake on the part of the student reporter or a later change in Harkins' plans for some reason is unknown.
17. W. D. Harkins, "The Effect of Salts upon the Solubility of Other Salts, V. The Solubility of Univalent Salts in Solutions of Salts of Different Types," *J. Am. Chem. Soc.*, **1911**, 33, 1807-1827; W. D. Harkins and W. J. Winninghoff, "The Effect of Salts upon the Solubility of Other Salts, VI. The Solubility of Difficultly Soluble Univalent Salts," *ibid.*, **1911**, 33, 1827-1836; W. D. Harkins, "The Effect of Salts upon the Solubility of Other Salts, VII. Discussion of the Solubility Relations of Univalent Salts," *ibid.*, **1911**, 33, 1836-1873.
18. Harkins, Ref. 1, p. 434.
19. Mulliken, Ref. 2, pp. 53-56.
20. "William Draper Harkins," in *The National Cyclopædia of American Biography*, James T. White & Co., New York, 1958, Vol. 42, pp. 312-313.
21. W. C. Ebaugh, "Gases vs. Solids: An Investigation of the Injurious Ingredients of Smelter Smoke," *J. Am. Chem. Soc.*, **1907**, 29, 951-970.
22. J. K. Haywood, "Injury to Vegetation and Animal Life by Smelter Fumes," *J. Am. Chem. Soc.*, **1907**, 29, 998-1009; "Smelter Smoke," *Science*, **1907**, 26, 476-478.
23. A transcript of Harkins' deposition for the U.S. Supreme Court in this case is in Box 1, Folder 7 of the William D. Harkins Papers in the University of Chicago Library Special Collections.

24. See, for example, the references to German literature given in footnotes in Harkins and Swain, 1907, Ref. 15, pp. 970, 998.
25. A detailed history of the pollution problems in the Butte-Anaconda area is given in D. MacMillan, "A History of the Struggle to Abate Air Pollution From Copper Smelters in the Far West 1885-1933," Ph.D. dissertation, University of Montana, Missoula, 1973; *Diss. Abstr. Int.*, 1974, A34, 6570. For studies and discussions of current problems resulting from the earlier mining and smelting activity in this area, see R. D. Taskey, "Soil Contamination at Anaconda, Montana: History and Influence on Plant Growth," M. S. thesis, University of Montana, Missoula, 1972; G. N. Bissell, "A Survey of Trace Metal Pollution in a Forest Ecosystem Southwest of the Anaconda Copper Smelter," M. S. thesis, University of Montana, Missoula, 1982; and J. N. Moore and S. N. Luoma, "Hazardous Wastes From Large-Scale Metal Extraction: A Case Study," *Environ. Sci. Tech.*, 1990, 24, 1278-1285.
26. A. E. Wells, "Report of the Anaconda Smelter Smoke Commission, Covering the Period From May 1, 1911 to October 1, 1920," n.d. (1920?), p. 2; R. G. Raymer, *Montana: The Land and the People*, 3 vols., Lewis Publishing Co., Chicago, 1930, Vol. 1, pp. 449-450. Raymer discusses the first smelter at Anaconda in a long chapter entitled "Montana's Copper Industry," pp. 434-545.
27. Taskey, Ref. 25, p. 20.
28. Taskey, Ref. 25, pp. 20-22.
29. W. D. Harkins, "The Marsh Test and Excess Potential: The Quantitative Determination of Arsenic," *J. Am. Chem. Soc.*, 1910, 32, 518-530.
30. Harkins, Ref. 29, Table III, p. 522.
31. Harkins and Swain, 1907, Ref. 15, pp. 973, 996.
32. Taskey, Ref. 25, pp. 6, 22; Wells, Ref. 26, pp. 8-11.
33. Harkins and Swain, 1907, Ref. 15, p. 994.
34. Environmental Protection Agency, *Code of Federal Regulations*, 40, Chap. 1, Pt. 60, Subpt. P, §60.162, §60.163; Pt. 61, Subpt. O, §61.172, revised July 1, 1993.
35. Harkins and Swain, 1907, Ref. 15, pp. 995-996.
36. This area is estimated from the extent of damage in different directions from the smelter as given by Taskey, Ref. 25, p. 49.
37. Swain and Harkins, Ref. 15.
38. Harkins and Swain, 1908, Ref. 15.
39. Swain and Harkins, Ref. 15, p. 917.
40. Swain and Harkins, Ref. 15, p. 920.
41. Swain and Harkins, Ref. 15, pp. 923.
42. Swain and Harkins, Ref. 15, p. 926.
43. Harkins and Swain, 1908, Ref. 15, p. 928.
44. Harkins and Swain, 1908, Ref. 15, pp. 932-934.
45. Harkins and Swain, 1908, Ref. 15, p. 939.
46. G. J. Peirce, R. E. Swain, and J. P. Mitchell, "The Effects of Smelter Smoke on Vegetation and the Condition of the National Forests in the Vicinity of Anaconda, Montana," Report to the U.S. Department of Justice, 1913.

ABOUT THE AUTHORS

Richard E. Rice is Coordinator of natural sciences in the General Education Program, James Madison University, Harrisonburg, VA 22807. George B. Kauffman is Professor of Chemistry at California State University, Fresno, CA 93740.

BOOK NOTES

Explorations with Sugars: How Sweet It Was, Raymond U. Lemieux, Profiles, Pathways, and Dreams Autobiographies of Eminent Chemists, Jeffrey L. Seeman, Series Editor, American Chemical Society, Washington, D.C., 1990, pp. 186 + xx.

With each autobiographical subject that enters the reader's mind is the need to ascertain why and how the individual under consideration has arguably become one of the "movers and shakers" of twentieth century organic chemistry. Despite inevitable variation among all the authors, the vast majority do indeed share certain common qualities; capability for performing enormous amounts of work, outstanding intellectual creativity, the capacity to engender motivational inspiration, unusual leadership skills, a shrewd appreciation of the major directions in which their discipline is heading, and common decency. Almost all of the individuals emerged as dominant figures within a decade of completing their formal education. Eminence in their chosen profession of chemistry developed because all of these individuals combine a greatness of character with obvious Olympian ability. Lemieux is no exception to this analysis. Yet, beyond their shared strengths and virtues there is another aspect to these individuals - they are uniquely interesting people. There is no one else in all of science just like Raymond Lemieux. One marvels at how these autobiographical subjects, in spite of so many personal and professional qualities held in common, nevertheless, are so truly individualistic.

From incidents that took place in his youth, one is struck by the complex facets that made up Lemieux's personality. How many other tough kids from Edmonton who similarly grew up in humble circumstances would be capable, as a high school student, of independently discovering the law of Dulong and Petit while "fooling

around" with his chemistry lessons? As is so common with many other emerging scientific personalities, one sees in this teenager's pedagogical experience the future scientist's innate capacity for complex pattern recognition. At a time when other boys his age were learning to drive the family car, Lemieux was already exhibiting that very rare combination of curiosity, intellectual playfulness, and an inner need for insight into the aesthetic aspect of Nature that drives the true scientist's spirit. It was this same interest in tying together loose ends and data misfits that over and over again helped Lemieux to make extremely important discoveries. How many other chemists in trying to empirically fit together proton NMR spectra would have, with hardly a shred of theoretical scaffolding as a basis, been willing to propose negative coupling constant signs to fit their data? How many academic or industrial chemists could combine the ability to be both creative and courageous - unwilling to be discouraged from at least entertaining an unusual hypothesis until it could be replaced by something closer to the truth. Repeatedly, Lemieux juxtaposes the hard work of establishing reliable data against the need to stretch beyond the obvious in order to find a satisfying explanation for a current mystery. To borrow a phrase made popular in a contemporary American television program, Lemieux's style is always an optimistic unfolding of the simple aphorism: "The Truth is out there".

While his original interest in carbohydrates was perhaps partly due to his choice of a thesis advisor, Prof. Purves of McGill, the intellectual excitement contributed by his mentor made him a passionate convert. Lemieux eloquently describes the many fascinating discussions he and Purves had regarding stereochemistry and the structure and synthesis of sucrose, while both of them sat in Purves' office and smoked Purves' tai-

lor-made cigarettes. What better introduction to a career within science than quiet afternoons while master and disciple calmly examine in pure collegial fellowship the mysteries and complexities of the sugars?

Lemieux also gravitated to carbohydrate chemistry through a conscious decision he made on the basis that sugar derivatives play a key role in metabolism. Consequently, the study of sugars should lead to great insights into biochemistry and perhaps directly afford applications to human medicinal chemistry. While his initial investigations might best be characterized as simply meeting the unsolved needs of carbohydrate chemistry in the 1940's, the persistence and individualistic brilliance of Lemieux allowed him to revitalize the discipline. From the perspective of being attracted to organic chemistry via its direct applications to biology and medicine, Lemieux can be counted as an excellent contemporary example of the more traditional organic chemists of the 19th century. While this classical focus was somewhat deflected because of the enormous strides in mechanistic understanding, physical phenomenology, and materials science that both characterized and dominated the period from 1940-1975, a return to this biology/chemistry interface is now very much in the air. One has only to consider the thematic discoveries pouring in a constant stream out of the laboratories of such researchers as Schreiber, Boger, and Nicolaou to see numerous benefits from the marriage of organic chemistry and direct biomedical applications. For an intriguing well-reasoned commentary that touches on this intellectual repositioning (perhaps even worthy of being termed a paradigm re-shift using jargon borrowed from Kuhn), the reader is directed to the comments of Albert Eschenmoser (*Angew. Chem. Int. Engl.* 1994, 33, 2363) made for the purposes of introducing a historical essay by F. W. Lichtenthaler on the contributions of Emil Fischer.

A singular influence on the development of Lemieux's scientific style was his participation in the metamorphosis of ambitious but relatively modest institutions into world-class research operations. This certainly provided a frontier flavor to his choice of research topics as well as allowed for the attracting of a particular sort of graduate student. When resources are severely limited, ideas can become exquisitely focused thereby ensuring that the possibility of squandering time and effort along blind alleys are much less probable. One can see this in the choice of one major initial carbohydrate research goal - an efficient approach to the synthesis of glycopyranosides. After a long series of well-chosen skirmishes at the edges this problem, even-

tually sufficient experimental progress and mechanistic insights were obtained so that attack on the final objectives became possible. Thus, using one beautifully researched experimental methodology, the efficient synthesis of maltose, trehalose, and sucrose was reduced to practice. Indeed, once initiated, the actual synthesis of sucrose was achieved in a very timely fashion. Achievement of this objective was so astounding to the scientific community that it has been accorded almost the status of a chemical epiphany. For example, generations of American undergraduates who have studied from various editions of an immensely popular textbook (*e.g.*, Morrison and Boyd "Organic Chemistry", 6th Ed., Prentice Hall, Englewood Cliffs, New Jersey, 1992, p. 1192) have been intrigued, delighted, and stimulated on reading that "the synthesis of sucrose, by R. U. Lemieux of the University of Alberta, has been described as 'the Mount Everest of organic chemistry'."

As is so often the case with the authors of this autobiographical series, a fixation with stereochemical issues serves both as providing certain favored directions for their research thrust and also providing a scaffolding for relating the evolution of their research themes from one topic to the next. Extracting a point from Eschenmoser's previously cited comments, it might even be argued that much of the evolution of contemporary organic chemistry, biochemistry, and molecular biology is ultimately in the direction of supramolecular science with its strong stereochemical flavor. Prof. Lemieux's body of work is no exception.

Lemieux's early interest in relative configurations rapidly expanded into a general study of stereochemical concepts thereby making his mind unusually receptive to the emerging ideas of conformational analysis. Both historically and in a practical sense, stereochemical issues are paramount in the carbohydrates. For new levels of exploration, chemists needed ever more powerful tools to assist their interpretation of experimental results.

At the very beginning of his independent career, Lemieux examined the behavior of chemical models that straddled the cyclohexane and pyranose worlds. He looked carefully for new tools that would assist him in these endeavors and was rewarded by becoming one of the earliest synthetic organic chemists to appreciate truly the power of magnetic resonance techniques at a time in which he was both still mastering the theory and learning how to make the cantakerous early instruments perform in a satisfactory fashion. Anyone who has ever used a modern computer-controlled FT instrument in which almost everything is automatic except the brew-

ing of the spectroscopist's morning cup of coffee needs to have some sensitivity as to just how difficult it must have been in the early days. As Lemieux notes (p. 31): "I well remember when we learned that our spectra would be much improved by spinning the sample tube." From his initial experiments, there came an understanding in how to assign configurations of axial and equatorial hydrogens on the basis of both chemical shift and, more importantly, coupling constants. The ability to rapidly assess preferred conformations in solution was of enormous significance both to carbohydrate chemistry in particular and to natural products structure elucidation in general. The sophistication of NMR applications continued to increase throughout Lemieux's career eventually leading to a series of beautiful NOE experiments as well as interesting applications of deuterium influences on chemical shifts, hydrogen bonding, and exchange phenomena.

One of the most important themes in Lemieux's work is his concern with what is known as the "anomeric effect." Basically, the anomeric effect concerns the influence that heteroatoms within a cyclic system have on the most stable conformations of nearby epimeric centers. This is a very complex phenomenon that almost certainly still carries locked within itself unexpected surprises. In elementary undergraduate organic chemistry courses, students are correctly taught that substituents about a simple all-carbon cyclohexane ring are thermodynamically almost always considerably more stable when equatorial rather than axial. However, matters become much more complex when one is dealing with a cyclic system that has a heteroatom substituent present, especially an oxygen. Recognition that both the atoms within the molecule as well as atoms in the external environment exercise a major influence on the preferred geometry has been seminal. Furthermore, an understanding of the subtlety of this phenomenon has the potential of giving scientists molecular insights of even more global significance. For decades, scientists have been attempting to calculate *a priori* the folded geometry of polypeptide and protein chains. The body of published work on the geometry of glycosidic linkages in oligosaccharides, again with Lemieux one of the major contributors, provides a valuable model and numerous clues for attacking the much more difficult amide intra- and inter-molecular interactions. For Lemieux, his research has now evolved to the point where computer calculations and molecular modeling permit structure predictions even for such complex carbohydrates as the B human group trisaccharide - an outstanding achievement.

In pursuit of his scientific interests, Lemieux's research has undergone a marvelous progression from the

study of relatively straightforward organic synthetic modifications onward to the investigation of some of the most difficult unsolved mysteries of immunology. In the study of blood group determinants, enormous progress has been achieved through his collaboration with an international cast of fellow scientists. Practical biomedical applications of Lemieux's fundamental advances to the preparation of pure monoclonal and polyclonal antibodies and for the purification of blood products are of the highest rank. Lemieux has performed an elegant series of investigations into the binding of various oligosaccharides to the anti-I-Ma antibody, to lectin 1 of *Ulex europaeus*, to monoclonal anti-Lewis antibodies, and to lectin 4 of *Griffonia simplicifolia*. His excitement (and their obvious scientific importance) is inherent in his description of this work in the latter third of the autobiography. In fact, Lemieux presents a "coda" at the end of the text describing recent work involving elucidation of the molecular structure of lectin 4 at near-atomic resolution and the implications of this information in the binding properties of the molecule. Juxtaposing the coda with a picture of his family, one can appreciate a fatherly sense of pride in science well done. One cannot escape being impressed with the combination of ability and courage that has allowed Lemieux to journey from the highly circumscribed problems of simple organic transformation to an exploration of the mysteries of the vertebrate immune system.

As another theme in common with other subjects of this autobiographical series, one cannot miss the fact that Lemieux was a superb teacher. Choosing just one of his former students as an example - the extraordinarily gifted and charismatic Bert Fraser-Reid - one can see how much motivation and excitement has passed from teacher to pupil, qualities that are apparent to the audience each and every time Fraser-Reid presents his own world-class carbohydrate research results. By the many other gifted scientists that have graduated from his research guidance, Lemieux has left his mark on the development of academic organic chemistry. Of similar importance, it must also be pointed out that, just as he was willing to help academic institutions, Lemieux has also been involved throughout his career in industrial collaborative transfer of chemical technology. This constitutes an activity that surely has brought significant wealth and exciting employment opportunities to Canadian society. As a complement to the combination of brilliance, toughness and persistence, Lemieux also has an admirable reputation within the scientific community as a person of the highest ethics and decency.

His legendary honesty paid handsome dividends when he was given a generous co-authorship by the late W. S. Johnson in recognition of his independent contributions to the development of both the periodate-permanganate and the periodate-osmium tetroxide oxidative reagents for the cleavage of olefins.

In closing, one of the most interesting aspects of these autobiographies is that they provide a forum in which members of our profession with uncommon levels of wisdom can comment on the state of science and its future. Lemieux is no exception. One of his most intriguing monologs appears in pp. 3-4. As part of a general commentary on funding practices of science, Lemieux raises issues that go to the very value foundations of late twentieth century science. He argues his sense that a career in science must transcend finding timely topics with high prospects of funding and publication. Instead, he believes that working in science is far more than just a way to earn a living. Rather it is a commitment to the joy of discovery that requires the most dedicated of individuals. When one looks about at the perplexing state of the U.S. science and engineering professions of the 1990's with their recurrent problems of industrial downsizing, limited academic opportunities, underemployment, and chronic sub-funding,

one can't fail to notice that Lemieux may be on to something. Has there been too much of an emphasis on vocational training in science and engineering over the past few decades? At the obvious risk of oversimplifying, nevertheless, we are all aware that the excitement of excellence and spirited inquiry still held high by the faithful few are at risk of being displaced by faculty viewing careerism as more important than teaching, by minimalist students cynically seeking "dream jobs" simultaneously combining salary, security, and leisure, and by industrialists more concerned with quick fix/bottom line concerns than putting out the very best product. This was not and could not be right. It is left for certain special individuals to teach by the very examples that their lives provide and, in so doing, to permit the rest of us at least to glimpse how it should be. The saga of their lives unequivocally illustrate a pathway to the highest standards of professional dedication and to the internal rewards that can come with such a commitment. The rest of us may have quite a way to go but no excuse for not having at least been shown the path by scientists such as Lemieux.

John L. Belletire, *Ricerca, Inc.*, PO Box 1000,
Paineville, OH 44077-1000

Lavoisier in European Context: Negotiating a New Language for Chemistry. Bernadette Bensaude-Vincent and Ferdinando Abbri, Editors, Science History Publications/USA, Watson Publishing International, Canton, MA, 1995, x + 303 pp. Cloth (typeset), ISBN 0-88135-189-X.

Sometimes a well-researched historical event or period, especially one in our own field, is so familiar that the story seems complete. Then a new book appears, one that provides the unexpected illumination and may even awaken our dozing imagination about the fruitfulness of still further revisiting. Such a book is this collection of fifteen essays about the spread of the new, French chemical nomenclature associated with Lavoisier and the chemical revolution at the end of the

eighteenth century. The book is based on a historiographic workshop organized under the auspices of the European Science Foundation as a complement to the cultural events celebrating Lavoisier's Bicentennial in 1994. Twelve of the essays are in English, three in French. The length of each varies from eight to thirty-six pages, the median length being eighteen pages. The book also includes a thirteen-page, briefly annotated bibliography on the two landmark French volumes *Méthode* and *Traité* and their European translations (1787-1800).

Each essay is written by a different author, including the two editors. The introductory essay, by one of the editors, provides an overview of the context, and the final one gives a summation and concluding remarks. The thirteen essays in between describe "the diffusion of the new nomenclature designed by four French chem-

ists—Louis-Bernard Guyton de Morveau, Antoine-Laurent Lavoisier, Claude-Louis Berthollet, and Antoine-Francois de Fourcroy—all over Europe.” Each essay focuses on a single country: Sweden, Belgium, Poland, Netherlands, Scotland, England (two essays), Portugal, Spain, New Spain (Mexico), France, and Italy (two essays). Notably missing is an essay on Germany, “due to the last moment cancellation of one participant.” Most of these countries are often omitted from accounts about the chemical revolution, and, as one of the authors puts it, this view from the fringe teaches us something about the development of chemistry as a whole at the end of the eighteenth century.

The spread of the new, French chemistry, we read in the introductory essay, helped chemists achieve a sense of belonging to a coherent discipline. The essays examine that spread as it was influenced by national cultural differences or at least the attitudes of some influential chemists that determined each national response. The thrust of all the essays is conveyed by a statement in one of them: “To say that Dutch chemists converted to the ‘new’ chemistry says little if we do not explore what sort of filter their commitments and practices provided for the reception of novelties and what shape those novelties took as they were integrated into the Dutch field.” The names and contributions of several of the cast members on stage in these essays are likely to be familiar only to chemists in the same country, at most. But contributions they did make. “The new language was neither accepted nor refused, but rather debated and negotiated. European chemists rarely showed a passive attitude of simply being ‘receivers’.”

Part of the debate and negotiation—a substantial part, in fact, this book makes clear—was about the new language itself. That is, for example, not only about the role of oxygen in combustion but also about the name oxygen (=acid former) for that substance. “The new language was deeply theory-laden,” and Davy, among others, favored a theory-free language. (For example, his “chlorine” did not imply either element or compound, a matter of some uncertainty at the time.) One assessment of the major effect of some of the new systematic names was “irreparable injury to science.” Subdued echoes of that attitude can be heard even now from some quarters in response to efforts by the nomenclature commissions of IUPAC to make chemical nomenclature increasingly tic. Forsaking the familiar, especially language, is difficult to do. Chemists often seem to grasp and acknowledge improvements in chemical theories more easily than in chemical nomenclature. This book identifies the different kinds and degrees of resistance

to the new, French language of chemistry two centuries ago, country by country, and highlights individual chemists contributing to the outcome of the debate. As *la gniappe* (a south Louisiana term for a pleasing extra), this book provides a capsule history of higher education in Portugal.

Because the book is about language, I had not expected it to be annoyingly inconsistent in its treatment of language; but it is. Most of the twelve English-language essays include numerous quotations from the original, non-English language. One author gives only the English translation, with reference citation to the original Dutch. Some others use the original in the text and provide translations in end-of-the-chapter notes. (The reverse placement—English in the text, original in the notes—would have been far more convenient for the reader without any loss of accuracy or authenticity.) Others translate a quotation of a few words in the text and quickly use a longer one, without translation anywhere, to extend or contrast the point made by the first. In the essay that strongly emphasizes the dominating influence of an estimable translation on the reception of the French proposals in Italy, no translations of the French or Italian passages are provided. Some of the significant points being made by the authors are likely to be missed by many American readers, even older ones who had to pass language examinations as part of their Ph.D. degree requirements.

Even so, the major points will not be missed. The centrality of contentions about nomenclature in the chemical revolution at the end of the eighteenth century will probably be a new viewpoint for many readers. The revolution is presented not as the indomitable triumph of cold, passionless reason set forth by one person, not as “exclusively a French-English *affaire*.” It is, instead, treated as broadly international, dependent on numerous unacclaimed chemists who interacted with passion stemming from a variety of institutional positions, social prestige, and attitudes. Such personal perspectives of the chemists in the development of chemistry are always important but often unacknowledged. Those highlighted in this book are perspectives on authority and liberty, on collegiality and disciplinary boundaries, on empiricism and theorizing, on innovation, on complexity of instrumentation and its effect on lay (audience) participation in scientific experimentation. The diverse humanity of the chemists dominates the story—as it must have dominated the event. *James G. Traynham, Department of Chemistry, Louisiana State University, Baton Rouge, LA 70803-1804.*

AUTHOR INDEX, NUMBERS 1-20

Issue Numbers are given in italics (only the first number for double issues, such as 13/14), followed by page numbers. Biographical sketches of authors are indicated by boldface.

A

Allen, R. O. *1*, 17; *3*, 24, 26

B

Barton, D. H. R. *19*, 43, 47
 Basolo, F. *19*, 66, 71
 Benfey, O. T. *13*, 16, 60, 66; *19*, 19, 24
 Bodner, G. M. *8*, 22, 27
 Bohning, J. J. *1*, 20, 22; *3*, 16, 21; *6*, 15, 21; *13*, 4, 16
 Brock, W. H. *12*, 30, 35
 Bunnett, J. F. *19*, 33, 42
 Buntun, C. A. *19*, 72, 76

C

Caneva, K. L. *13*, 27, 29
 Cantor, G. *11*, 28, 36
 Caswell, L. R. *20*, 46, 49
 Chastain, B. *3*, 7, 11; *4*, 8, 11; *5*, 34, 37; *8*, 8, 12
 Creese, M. R. S. *8*, 15, 18; *15*, 31, 36; *17*, 9, 14; *20*, 42, 45
 Creese, T. M. *8*, 15, 18; *15*, 31, 36; *17*, 9, 14

D

Da Silva Pinto, M. A. *17*, 31, 39
 Davenport, D. A. *5*, 37, 42; *11*, 3, 60, 65, 92, 100; *19*, 13, 18
 Debus, A. G. *1*, 4, 5
 DeLoach, W. S. *9*, 19, 22
 Doheny, J. J. *6*, 21, 23
 Donovan, A. L. *5*, 10, 14

E-F

Espinola, A. *17*, 31, 39
 Fine, L. *2*, 19, 21
 Fruton, J. S. *17*, 1, 8
 Fullmer, J. Z. *11*, 17, 28

G

Gillispie, C. C. *15*, 19, 26
 Goldsmith, R. *1*, 16; *9*, 22, 25
 Goldwhite, H. *11*, 55, 60
 Gonzalez, E. *4*, 11, 15

H

Habushi, F. *2*, 14, 16; *3*, 15, 16; *7*, 13, 16; Habushi, F. *17*, 15
 Haber, L. F. *3*, 4, 7
 Hannaway, O. *12*, 3, 10
 Holmen, R. E. *4*, 15, 23; *6*, 7, 15
 Holmes, F. L. *5*, 24, 31; *20*, 1, 9

I

Ihde, A. J. *1*, 13, 15; *2*, 11, 14; *3*, 11, 15; *4*, 23, 26; *8*, 3, 8
 Ingold, K. U. *19*, 1, 1
 Ireland, K. M. *5*, 37, 42

J

James, F. A. L. *11*, 36, 40
 Jensen, W. B. *1*, 10, 12; *2*, 16, 19; *3*, 21, 24; *5*, 42, 48; *6*, 26, 36; *7*, 26, 33; *10*, 3, 15; *11*, 65, 76; *12*, 49; *13*, 3, 47, 59
 Jones, P. R. *1*, 8, 10; *4*, 3, 7; *9*, 15, 19; *20*, 27, 28, 32

K

Kagan, H. *1*, 22
 Kauffman, G. B. *10*, 21; *20*, 50, 60, 59, 67
 Kessler, A. L. *9*, 7, 15

L

Laszlo, P. *15*, 59, 64
 Lewis, D. E. *17*, 21, 30

M

McKone, H. T. *10*, 25, 31
 Miles, W. D. *8*, 18, 22
 Miller, J. A. *5*, 3
 Morris, K. M. *2*, 23

N

Neno, C. C. *17*, 31, 39
 Newman, W. *13*, 19, 27
 Nye, M. J. *19*, 58, 65

O

O'Brien, J. F. *11*, 47, 50
 Olsen, K. K. *17*, 41, 56
 Orna, M. V. *1*, 17

P

Palter, R. *15*, 15, 17
 Paoloni, L. *12*, 10, 24
 Pratt, H. T. *11*, 40, 47

Q-R

Quane, D. *7*, 3, 13
 Ramberg, P. J. *15*, 45, 51, 52
 Rhees, D. J. *13*, 40, 47
 Rice, R. E. *20*, 60, 67
 Roberts, G. K. *19*, 2, 12
 Roberts, J. D. *19*, 48, 56
 Rocke, A. J. *2*, 4, 6; *7*, 19, 24; *13*, 29, 40
 Ross, S. *11*, 76, 79, 79
 Rossiter, M. W. *20*, 33, 41
 Russell, C. A. *9*, 3, 7

S

Saltzman, M. D. *9*, 7, 15; *15*, 37, 43; *19*, 25, 32; *20*, 10, 17
 Schwartz, A. T. *5*, 31, 34
 Shine, H. J. *19*, 77, 92
 Siegfried, R. *5*, 18, 24
 Smeaton, W. A. *5*, 4, 10
 Stock, J. T. *7*, 17, 19; *8*, 12, 15; *10*, 31, 34; *11*, 86, 9; *15*, 1, 8; *20*, 18, 23

T

Tarbell, D. S. *6*, 3, 7
 Thomas, J. M. *11*, 4, 9
 Towns, M. H. *11*, 92, 100
 Traynham, J. G. *1*, 15
 Tweney, R. D. *11*, 51, 55

U-Z

Usselman, M. C. *11*, 17, 28
 Webb, G. E. *15*, 9, 14
 White, J. E. *5*, 15, 18
 Williams, L. P. *11*, 9, 17
 Williams, W. D. *2*, 6, 10; *6*, 23, 26; *8*, 18, 22; *9*, 37, 39; *10*, 21, 25, 34, 39; *12*, 24, 29, 35, 38; *15*, 27, 29, 55; *20*, 24, 26
 Wotiz, J. H. *7*, 24, 26

SUBJECT INDEX, NUMBERS 1 - 20

Volume numbers are given in *italics* (only the first number for double issues, such as 13/14), followed by page numbers.

A

- AAAS 20, 33
 Aachen, Tech. Un. 4, 4
 Abbott, B. 11, 29, 56
 Abbot, H. 9, 18
 Abbot-Michael, H. 8, 15
 Abderhalden, E. 17, 3, 4, 6
 Abegg, R. 13, 48
 Academie Française 5, 12
 Acad. Sci., French 5, 12-17, 20, 28, 34
 Accum, F. 10, 9, 27, 28; 11, 69
 Acetamide, Kekulé's structure 12, 12
 Achard, F. 4, 12
 Acide nitreux, 3, 8
 Acidity function 19, 38
 Acids, Lavoiser theory of 5, 6, 7, 42-47
 Acree, S. F. 19, 50, 53
 Acylous 19, 37, 40
 Adam, N. K. 15, 2
 Adams, C. B. 20, 25
 Adams, R. 4, 31; 6, 22; 13, 42
 Addison, J. 10, 25
 Adirondack Iron Works 17, 47-49, 54
 Admiralty, 11, 37, 38
 Affinity units 12, 11, 12; 15, 52, 53
 Agassi, J. 11, 16, 66
 Agfa 9, 9
 Agricola, Georgius 8, 5; 12, 3-9; 7, 41, 42
 Agricultural Hall 3, 18, 19
 Air, nature and properties, 3, 21
 Albany Academy 12, 26
 Albemarle Furnace Co. 9, 21
 Albert, Prince 11, 30, 66
 Albright & Wilson 19, 7
 Albumin 17, 2
 Alchemy 13, 19-26
 Alcoa 17, 19
 Alizarin 9, 8, 9
 Allotropism 6, 30, 31
 Aloe 10, 25
 Alpha particle 11, 96
 Altempohl, D. 17, 19
 Alum 3, 5, 7
 Alumina process, Bayer 17, 15-19
 Alumina process, LeChatelier 17, 15-17
 Aluminum, production 20, 24
 Alusuisse 17, 19
 Alyea, H. N. 10, 3, 8, 14
 Am. Assn. Adv. Sci. 6, 15, 16, 18-20, 25; 8, 17
 Am. Assoc. Adv. Women 8, 17
 Am. Chem. J. 6, 17, 19; 13, 41-45
 Am. Chem. Soc. 3, 16, 17; 6, 16, 18; 20, 33-39
 Am. Chem. Soc. NY 6, 15, 17-20
 American Chemist 12, 32
 Am. J. Sci. 15, 9, 12
 Am. J. Sci. Arts 2, 7; 12, 36, 37; 15, 9
 Amherst College 4, 6
 Amine oxides 20, 11, 12
 Amino acids 17, 1-7
 Ammonia, determination 20, 21, 22
 Ampère, A.-M. 11, 56
 Amyott, T. 11, 19
 Anaconda Copper Mining Co. 20, 63-67
 Anaxagoras 5, 18, 44
 Anchimeric acceleration 19, 35, 40, 44
 Anderson, C. 11, 38, 66
 Anderson, R. 4, 26
 Anecdotes, use of 7, 26-32
 Anger, V. 17, 31
 Aniline black 9, 8
 Aniline yellow 9, 9
 Anionoid 19, 35, 37, 39, 40
 Anionotropy 19, 37
 Annals of Science 2, 13
 Antimony, regulus of 1, 3
 Antiphlogistic theory 4, 8-10; 5, 4, 5, 18, 37, 39-41, 44; 8, 5
 Antonoff, G. N. 4, 17
 Apparatus, Lavoisier's 5, 31-34
 Arago, F. 11, 33, 52; 13, 36
 Arago's effect 11, 52
 Arbuzov, A. E. 7, 26; 17, 22, 26, 27
 Arbuzov, B. A. 7, 25, 26
 Arbuzov Museum 7, 26
 Archeological chemistry 1, 17, 18
 Archimedes screw 8, 25
 Archives, HIST 3, 26
 Ardeer 9, 6
 Arendt, R. 10, 10-12
 Argilla vitriolata 3, 8
 Argon 5, 5
 Aristotle 12, 7
 Armstrong, E. V. 2, 11
 Armstrong, H. E. 7, 5, 6, 9; 13, 30; 19, 15, 23; 20, 10
 Arndt, F. 19, 27, 28
 Aromatic substitution 19, 26-29
 Arrhenius, S. 3, 17; 6, 26; 7, 5, 27, 30, 31
 Arsenal 5, 16, 17, 31, 33
 Arsenic poisoning, animal 20, 64, 65
 Artifacts, chemical 1, 8, 9; 2, 19-21
 Assaying, Am. Iron 17, 41-55
 Assoc. Off. Agr. Chemists 6, 15, 16
 Astbury, W. T. 7, 8
 Athenaeum 11, 37
 Atomic Energy Commission 6, 21
 Atomism, 11, 33; 15, 45
 Atwater, W. O. 3, 20
 Atwood's machine 8, 23, 25
 Aughey, S. 17, 9
 Aurora tube 8, 26
 Austin, P. T. 6, 17
 Autenrieth, J. H. F. 13, 29
 Author's Guide 7, 37
 Authors, Instruction for 15, ii
 von Auwers, K. 19, 23
 Avenin 17, 2
 Avogadro, A. 8, 6
 Avogadro constant 11, 93, 95, 96, 98, 99
 Avogadro hypothesis 13, 63, 64
 Azo dyes 9, 9
 Azot 3, 10
 d'Azyr, F. V. 4, 8

B

- Babbage, C. 11, 19, 49, 57
 Babcock, C. L. 6, 9
 Babington, W. 11, 20
 von Babo, C. H. L. 8, 35
 Babo generator 8, 34, 35
 Bache, F. 5, 45; 15, 27, 28
 Bacon, C. W. 9, 17
 Bacon, F. 5, 19; 13, 36; 20, 5
 Bacon, J. 12, 37
 Bader, R. F. W. 6, 10
 von Baeyer, A. 7, 20, 21, 23, 30; 9, 8, 16; 12, 11; 13, 35; 17, 3
 Bailey, E. H. S. 6, 16
 Balance, chainomatic 8, 12-14
 Balance, Watkins and Hill 8, 23
 Balances 20, 48, 49
 Balard, A. J. 20, 18
 Balloon, hot-air 10, 37, 38

- Balmer, J. 8, 6
Bancroft, W. D. 6, 22
Banks, J. 11, 18, 20, 25, 36
Banthrope, D. 19, 77, 78, 81
Banting, F. G. 6, 4
Barbarosa, P. E. F. 17, 33
Barber, S. 6, 9
Bardeen, J. 9, 11
Bargallo, M. 4, 23
Barker, G. F. 12, 35-38
Barnes, W. H. 7, 6
Barreswil, L. C. 13, 36
Barrett, S. 19, 7
Barrow, H. 8, 12; 15, 4
Barrow balance 15, 4, 6, 7
Bartlett, P. D. 4, 31; 19, 45, 48, 50-53
Bartley, E. H. 6, 18
Barton, D. H. R. 6, 12
BASF 9, 8
Basyous 19, 37, 40
Basyrin, M. 20, 53
Bauer, E. 19, 62
Bauer, G. 12, 6
Bauer, N. 6, 12
Bauxite 17, 17-19, 33
Bayer, K. J. 17, 15-19
Bayerische Staatsbibliothek 4, 4
Bayley, J. 11, 19
Béchamp, A. 9, 8
Becher, J. 11, 68
Beck, L. C. 6, 24; 8, 24; 12, 26, 27
Beck, T. R. 12, 26
Becker, C. A. 8, 13
Beckman, A. 9, 11
Beckman Center for History of Chemistry 1, 16; 4, 25, 32
Bequerel, A. H. 8, 6
Beehive 8, 34
Beer, P. 4, 18
Beet sugar 17, 10, 11
Behrend, R. 15, 7
Beilstein, F. 3, 17; 17, 16
Bell Labs. 9, 11
Beloit College 4, 6
Belvedere Iron Co. 17, 49
Benedict, F. G. 10, 11; 17, 6
Benfey, O. T. 13, 3-15, 46
Benjamin, M. 6, 18
Benzene, discovery 11, 5, 6 52
Benzene, stereochemical models 12, 10-21
Benzidine rearrangement
Bergman, T. O. 5, 4, 28; 17, 45
Berkshire Med. Inst. 12, 26
Berl, E. 20, 55
Berlin, T. 6, 8, 10-12
Berlin iron 17, 51
Berlin Un. 4, 4, 5
Bernal, J. D. 7, 8-10
Bernhard, A. 9, 16
Bernières 4, 12
Berthelot, M. 5, 19; 6, 32, 33; 12, 17; 19, 58, 59, 62
Berthollet, C.-L. 3, 7-9; 4, 3; 5, 4, 5, 7-9, 32, 34; 8, 5
Berzelius, J. J. 3, 10; 4, 3; 5, 5; 6, 31; 8, 5, 6, 9-12; 11, 22, 63, 73; 13, 31-33, 37, 52; 20, 11
Bessemer process 17, 48, 51, 53
Best, C. H. 6, 4
Bewin, 3, 23
Bibles, Faraday's 11, 40-46
Bibliography, 1988 4, 27
Bibliography, 1989 6, 37ff
Bibliography 1990 9, 25
Bicarburet of hydrogen (benzene) 11, 6, 52-54, 62, 63
Bielfeldt, K. 17, 19
Biot, J. B. 5, 8; 20, 51
Bird, G. 11, 79
Biringuccio, V. 17, 41, 42
Bischoff, C. A. 15, 35
Bismarck brown 9, 9
Black, J. 5, 6, 19, 20; 10, 5, 6, 35; 15, 4, 15-17; 20, 5
Black, N. H. 19, 51
Blast furnace 17, 42, 44, 48
Blatt, A. H. 19, 51
Bleu de Lyon 9, 8
Bloomery process 17, 43-45, 47-50
Blowpipe 11, 81-83, 85; 17, 45
Blowpipe, oxyhydrogen 4, 11-15
Blowpipe, Plattner 2, 21
Bobiere, P. A. 20, 18-23
Bochard de Saron, J. B. G. 4, 11-14
Bodenstein, M. 4, 16
Bodley, R. 17, 11
Bohr, N. 13, 48
Bologna stone 3, 21
Bolton, H. C. 3, 20; 6, 19
Bonding theory, Lewis-Langmuir 15, 37
Bonhoeffer, K. F. 6, 29
Bonjour, J. F. 5, 4
Bonn Un. 4, 4, 5
Bones and stones 2, 14, 15; 3, 24
Book Notes 1, 14, 15; 2, 22; 3, 26; 4, 29; 6, 41ff; 7, 33-35; 8, 27-30; 9, 32; 10, 39; 12, 39-45; 13, 66-72; 17, 57-64; 19, 94, 95; 20, 68-72
Boone, D. 8, 22
Born, M. 6, 8; 13, 48; 19, 13
Borrichius, O. 13, 19
Boscovich, R. J. 11, 15-17, 54
Bosshart, J. 20, 55
Bossingault 13, 36
Böttger, R. 3, 23
Bouillon-Lagrange, E. J. B. 5, 4, 5
Boulday, P. 8, 10
Bouquet, J. J. 20, 20
Bourdelain, L.-C. 10, 4, 7
Bourguet, M. 19, 59
Bouton, R. 17, 11
Bowman, W. 6, 17
Boxer Rebellion 12, 5
Boye, M. 17, 54
Boyle, R. 4, 12; 5, 5, 19, 20, 26, 46; 8, 5; 13, 19, 24; 15, 20-22
Boyle's air pump 15, 21, 22
Boyle's law 15, 21
Boys, C. V. 11, 88
Bozorth, R. 7, 5
Brady, O. L. 19, 5
Bragg, W. H. 7, 5, 6; 19, 13; 20, 14
Braille, N. 17, 33
Bramson, C. 13, 44
Branch, G. 20, 10
Brande, W. T. 10, 36; 11, 17, 25, 53, 54, 65, 66, 78, 79, 81, 82, 84; 12, 25
Branner, J. C. 12, 3, 6, 9
Brass, analysis 20, 20, 21
Brattain, W. 9, 11
Bravais, A. 12, 20
Bray, W. C. 7, 3, 4
Bread, fermentation in 8, 19, 20
Bredig, G. 4, 16-18
Breneman, A. A. 6, 19
Breslau Un. 4, 4, 5
Brett, R. H. 11, 79
Brevium 4, 18
Bridge, J. L. 9, 16, 18
Bridgman, P. W. 19, 19
Brit. dyestuffs industry 9, 7-11, 14
Brit. Soc. Chem. Ind. 6, 16, 18, 19
Brockway, L. L. 6, 7
Brockway, L. O. 7, 7
Brodie, B. C. 4, 6; 6, 29, 31
deBroglie, L. 19, 62
Bromine, discovery 20, 18
Bronze, analysis 20, 19, 20
Brooke, H. J. 11, 82
Brooke, Simpson & Spiller 9, 8
Brough, J. C. 12, 33
Brown, A. Crum 13, 35
Brown, H. C. 19, 53
Brown, L. 8, 23, 26
Brown, S. 8, 22
Brown Inst. Co. 10, 31
Browne, C. A. 1, 13; 3, 12; 6, 3; 13, 44
Buchenwald 19, 59
Bucquet, J.-B. 4, 8, 11
deBuffon, Conte G. 5, 23
Bullock, J. L. 9, 18
von Bunge, G. 17, 2
Bunsen, R. 4, 3, 5, 6; 7, 19-22; 8, 6; 13, 31, 34; 15, 6, 7; 17, 16
Bunsen burner 11, 70
Bunsen cell 15, 6, 7
Bunton, C. A. 19, 7
Bureau of Mines 13, 41, 42

- Burkhardt, G. N. 20, 14
 Burns, R. 10, 35, 38
 Bush, J. 8, 23
 Buswell, A. M. 7, 7
 Butadiene, electronic structure 20, 13
 Butlerov, A. M. 7, 20, 24-26; 13, 35; 17, 21-25, 27, 28
 Butlerov Museum 7, 24-26
- C**
- Cady, G. H. 7, 8
 Caffeine 17, 33
 Cahours, A. 13, 34
 Calcination 8, 4, 5
 Calcium, reagent for 17, 37
 Caldas, A. 17, 33
 Caldwell, C. 8, 22, 23
 Caley, E. 3, 11
 Calomel electrode 10, 32, 34
 Caloric 3, 10; 5, 4-7, 22, 43; 8, 5; 9, 22
 Calorimeter, ice 5, 32
 Cal. Petroleum Co. 15, 11
 Calvin, M. 19, 49; 20, 10
 Calx, gold 3, 8
 Calx, lead 5, 25
 Calx, mercury 5, 27
 Cambrian Iron Works 17, 52, 53
 Cambridge Un. 4, 3, 6
 Camera lucida 8, 23
 Candle, chem. and phys. of 11, 7, 8, 30, 31
 Cannizzaro, S. 3, 17; 8, 6; 12, 12, 13; 13, 63, 64
 Cannon, oxygen-hydrogen 10, 7, 8
 Caoutchouc 11, 64
 Carbon monoxide 5, 17
 Carburet 11, 84
 Carius, G. L. 12, 13
 Carmack, M. 6, 11
 Carnegie Inst. Wash. 17, 4
 Carnot, S. 5, 6
 Caro, H. 9, 8-10
 Carwheel Mine 17, 51
 Carnot, S. 5, 6
 Cassell Cyanide Co. 19, 2
 Cather, Willa 17, 12
 Cationotropy 19, 37
 Cavello, Tiberius 1, 3, 10; 2, 22, 23; 3, 21
 Cavendish, H. 5, 5, 40; 11, 88
 Celtic antiquities 3, 24, 25
 Center for History of Chemistry 1, 16
 Chadwell, H. 7, 6
 Chadwick, J. 4, 17
 Chair of HIST, message from 3, 27; 6, 43
 Chan, F. L. 17, 34
 Chandler, C. F. 2, cover, 19-21; 3, 16; 4, 6; 6, 19, 20; 12, 32
 Chandler, W. H. 12, 32
 Chandler Chemical Museum 2, 19
 Chaos 13, 25, 26
 Chaptal, J. A. C. 5, 36; 8, 4
 Charcoal 9, 21
 Chargraff, E. 17, 31
 Charles, J. 5, 32
 Chauvinism 13, 29-37
 Chazal, A. 8, 26
 deChazelle, Mme. Comptes 5, 33
 Chelation 7, 6, 9
 Chem. Bull. 6, 22
 Chemical Age 12, 34
 Chemical Foundation 13, 43, 45, 46
 Chemical Gazette 12, 30
 Chemical genealogies 3, 26; 8, 33
 Chemical industry, American 13, 40, 41, 43
 Chemical News 12, 30-34
 Chemical revolution 20, 1-8
 Chemical Revolution, Bicentenn. Symp. 5, 3ff
 Chemical revolution, books of 3, 7-11; 4, 8-11; 5, 34-36
 Chemical Warfare Serv. 13, 41-43
 Chemistry 13, 12
 Chemists' Club 13, 44
 Chem. Soc. (London) 20, 44
 Chem. Soc. Phila. 5, 44
 Chem. Soc. Washington 6, 15-17, 19
 Chester, A. H. 17, 48
 Chestnut St. Female Sem. 17, 10
 Chevalier, C. 8, 24, 25
 Chevreul, M. E. 5, 9
 Chibnall, A. C. 17, 6, 7
 Chicago Exposition 3, 25
 Chicago World's Fair 3, 2, 16-21
 Children, J. G. 11, 20
 Children's voltaic battery 11, 78
 Children's voltaic pile 11, 78
 Chittenden, R. H. 17, 5
 Chladni, E. F. F. 11, 52
 Christian, Prince of Denmark 11, 17
 Christmas Lectures, Faraday 11, 7, 8, 37; 12, 32
 Chrysaniline yellow 9, 8
 Chrysodine 9, 9
 Chymia 2, 11, 13
 Cinchona 11, 93
 Cineres clavellati 3, 8
 Cine-substitution 19, 36
 Clarendon, Earl 11, 30
 Clark, J. 9, 15
 Clark, R. H. 9, 17
 Clark, T. H. 9, 16, 18
 Clark, W. S. 4, 5, 6
 Clark Un. 9, 15-19
 Clarke, E. D. 11, 79-85
 Clarke, F. W. 3, 16, 20; 6, 15, 16, 18-20; 7, 17, 18
 Claus, A. 12, 13
 Claussen, W. H. 7, 8
 Claycop 4, 31
 Cleaveland, P. 6, 24
 Clemmensen reduction 6, 27
 Clepsydra 8, 3, 4
 Cloud, J. 2, 9
 Coal tar 9, 6, 7
 Cobalt (III) ammines 19, 66-70
 Cobalt complexes, resolution 20, 50-56
 Coffee 17, 33
 Cohn, E. 17, 6
 Cohnheim, O. 17, 4
 Colbert, J. B. 5, 12, 13
 Cold fusion 4, 32
 Coleridge, S. T. 11, 32
 Collège de France 5, 9
 Collège Mazarin 5, 12
 Collie, J. N. 19, 3
 Colligation 19, 36, 40
 Collins, E. 10, 26
 Collip, J. B. 6, 4
 Colloids 19, 72-75
 Columbia School of Mines 4, 6
 Columbian Chem. Soc. Memoirs 5, 43
 Columbian Exposition (Chicago World's Fair) 3, 2, 16-21
 Columella, M. 12, 8
 Combustion bulb 5, 33
 Combe, T. 11, 17
 Combustion train 2, 20
 Computers 9, 11-13
 Comstock, J. L. 2, 8
 Comstock Lode 15, 11
 Conant, J. B. 6, 3; 13, 42; 15, 37; 19, 20, 22, 23, 25, 48, 50-52; 20, 11
 de Condillac, Abbé Bonnot 3, 7, 9; 5, 34
 deCondorcet, M.. J. A. N. C. 5, 7, 13
 Conn. Agr. Station 17, 1, 2, 6
 Conrad, H. E. 20, 51
 Conservation of mass 5, 18-24
 Conservation of matter 13, 27-29
 Conservatoire National des Arts et Métiers 5, 31, 33
 Continental Chem. Soc. 3, 16; 6, 15-21
 Cooke, J. P. 2, 17, 18; 6, 19
 Coons, C. C. 10, 33
 Coordinate covalent (dative) bond 20, 12
 Cooper, T. 2, 9; 5, 41; 8, 22
 Cope, J. R. 8, 20, 21
 Copley medal 11, 20; 20, 18
 Corey-Chaykovsky reaction 15, 61
 Cornish crucible 11, 79
 Corpuscular alchemy 13, 19-26
 Coryell, C. D. 20, 37
 Coulson, C. A. 6, 8, 9; 19, 62
 Couper, A. S. 7, 20; 13, 35

- Courtois, B. *11*, 56
 Coxe, J. R. *5*, 44, 45
 Crafts, J. M. *9*, 17
 Crith *2*, 16-18
 Crawford, B. *6*, 12
 Creswell, D. *11*, 19
 Croft, H. *12*, 30
 Croker, J. W. *11*, 37
 Crookes, W. *8*, 6; *11*, 8, 28, 65; *12*, 30-34
 Crosland, M. *4*, 25
 Cross, A. *11*, 29
 Crosse, C. *11*, 29
 Crotenoid *19*, 37, 40
 Crotonoid *19*, 37, 40
 Cryolite *17*, 17
 Cubical atom *20*, 12, 13
 Cullen, W. *10*, 37; *15*, 15
 Cummings, J. *11*, 83
 Cupellation *12*, 8
 Cuprous nitre *4*, 31
 Cupreous nitre *3*, 21
 Curie, I. *3*, 15
 Curie, Marie S. *4*, 16; *6*, 8; *8*, 6; *20*, 52
 Curie, Maurice *4*, 18
 Curie, P. *8*, 6
 Curie, T. J. *3*, 15
 Curtius, T. *4*, 16, 17
 Cutbush, J. *10*, 7
 Cutter, W. P. *6*, 17
 Cuybush, J. *8*, 18
 Cyclotron *6*, 8
- D**
 Dacorso, G. F. *17*, 33
 Daguerreotype *8*, 26
 Dains, F. *3*, 11
 Dalton, J. *4*, 30; *5*, 3, 6; *8*, 5, 6; *11*, 20, 54, 56, 95; *12*, 24; *13*, 37, 61, 62
 Dana, S. L. *20*, 25
 Dancy, F. B. *6*, 17
 Daniell, J. F. *11*, 88, 93
 Daniels, F. *7*, 14; *20*, 36, 37
 d'Arcet, J. *4*, 12
 Darmstadt Tech. Un. *4*, 4
 Darwin, C. G. *4*, 17; *6*, 5, 6
 Darzens, G. *15*, 59-64
 Darzens reaction *15*, 60, 61
 da Silva, C. P. J. *17*, 33
 Daudel, R. *19*, 62
 Daumus, M. *4* 24; *20*, 4, 7
 Davenport, B. F. *6*, 17
 Davis, T. L. *1*, 13
 Davisson, G. *10*, 4; *13*, 61, 62
 Davisson, W. *10*, 3
 Davy, H. *5*, 5-7, 44-46; *6*, 27; *8*, 5; *10*, 6; *11*, 3, 16-23, 25, 36-38, 53-56, 59-61, 63, 65, 73, 80-85, 87, 89; *20*, 19
- Davy, J. *11*, 22, 23
 Davy, Lady *11*, 85
 Davy Medal *13*, 65
 Davy safety lamp *8*, 23
 deBoer, J. H. *13*, 48, 49
 Debray, H. *4*, 14
 Debus, A. *4*, 26
 Debye, P. *7*, 6; *19*, 28; *20*, 51
 Deer Lodge Valley Farmers Assn. *20*, 62, 63
 Dehlinger, U. *13*, 51
 De la Rive, A. *11*, 28-30, 46, 87
 Deleuil, J. A. *8*, 26
 Demant, V. *17*, 31
 Demichel, A. *8*, 13, 14
 Democritus *5*, 19; *8*, 5; *11*, 94; *13*, 60
 Demonstrations, chemical lecture *10*, 3
 de Morgan, A. *11*, 34
 Dephlogisticated air *5*, 22, 27; *6*, 26
 Derbyshire *9*, 6
 Despretz, C. M. *20*, 18
 Deutsches Museum *4*, 3
 Deville, H. S-C. *4*, 14; *6*, 34; *13*, 36
 de Walque, F. *11*, 73
 Dewar, J. *2*, 4, 5; *12*, 13; *15*, 31, 32
 Dewar -complex theory *19*, 78, 80-84, 87
 Dexter Award *1*, 13, 14; *2*, 11-14, 24; *3*, 11-15; *4*, 23-26, 31; *6*, 3-7, 44; *9*, 3; *10*, 16; *12*, 3, 46; *13*, 73; *15*, 1; *17*, 1; *20*, 1, 48
 Dexter Chemical Corporation *1*, 13-15
 Diazotization *9*, 9
 Dickinson, R. *7*, 6
 Dickinson College *5*, 41
 Dihydrol *7*, 7
 Dixon, H. B. *7*, 14
 Döbereiner, J. W. *4*, 3; *13*, 62, 63
 Dobson, T. *10*, 34-37
 Doktorvater *4*, 4, 5, 7
 Dolland, G. *11*, 37
 Donnan, F. G. *19*, 2-5, 7, 8, 72
 Doremus, C. A. *6*, 19
 Druce, J. F. G. *12*, 34
 Drude, P. *13*, 51
 Drummond, Col. P. *11*, 38, 39
 Dualism *8*, 9, 11
 Duckworth, C. W. *1*, 12, 13
 Dudley, C. B. *6*, 18
 Dulong, P.-L. *13*, 36, 64
 Dumas, A. *13*, 62, 63, 65
 Dumas, J.-B. *7*, 20, 22; *8*, 8; *11*, 60; *13*, 32-36; *20*, 18
 duPont, E. I. *5*, 9, 33
 Dupont, G. *19*, 60
 duPont, P. S. *5*, 9, 33
 duPont, V. *5*, 9, 33
 DuPont Co. *13*, 41
 Durham Iron Works *17*, 52, 54
- Duveen, D. I. *2*, 12
 Dyestuffs *9*, 7-10
- E**
 Easley, C. W. *9*, 16, 18, 19
 Eaton, A. *10*, 7, 8
 Ebert, H. *13*, 48
 École Polytechnique *5*, 9
 Edelstein, S. M. *1*, 13, 14
 Edelstein Center for Hist. Phil. Sci. *4*, 32
 Edestin *17*, 2, 4
 Edge displacement *19*, 67
 Edgewood Arsenal *13*, 42
 Edison, T. A. *6*, 16
 Editor, letter from *15*, i
 Editor, letters to *3*, 3
 Editor's Desk *1*, 3; *2*, 3; *3*, 3
 Edwards, M. F. *6*, 17
 Ehrmann, F. *4*, 12, 13
 Einstein, A. *11*, 4, 6, 52; *19*, 13
 Eka-rhenium *3*, 15
 Elderhorst, W. *4*, 7
 Electrochemistry, *11*, 5, 6, 86-92
 Electrodes, *10*, 31
 Electrogravimetry *7*, 17-19
 Electrolysis, *11*, 5, 14, 54, 86-92, 94
 Electrolysis of water *7*, 39
 Electromagnetic induction *11*, 76
 Electromagnetic rotation *11*, 20, 21
 Electromeric effect *19*, 22, 26-29, 33, 35; *20*, 13
 Electrometer, pith-ball *8*, 23
 Electronegativity scale, Allred-Rochow *13*, 56
 Electronegativity scale, Pauling *13*, 53
 Electronic strain *19*, 27, 28, 33
 Electrotypy *19*, 60
 Element *61* *1*, 9
 Element *93* *3*, 15
 Elements, four Greek *8*, 3, 4
 Eli Lilly Co. *6*, 4
 Eliot, J. *17*, 44
 Elixir *13*, 21
 Emeléus, H. J. *19*, 16; *20*, 43
 Emery, A. *20*, 34-38
 Emich, F. *17*, 36
 Emma Silver Mining Co. *15*, 12, 13
 Emmens, S. H. *12*, 33
 Empedocles, four elements of
 Empire Iron & Steel *17*, 52
 Empire Gold & Silver Mining Co. *15*, 11, 12
Encyclopedia Britannica *10*, 34-38
 Endemann, H. *4*, 7
 Eng. Alch. Soc. *15*, 35
 Engelhardt, F. E. *6*, 17
 Engler, E. *3*, 19
 Eolipile *8*, 23
 Epicurus *8*, 5

- Epsom salts 3, 8
 Equivalents, electrochemical 11, 92, 93
 Erdmann, O. L. 4, 3
 Erdtman, H. 19, 45
 Erlangen Un. 4, 4, 5
 Erlenmeyer, E. 3, 17; 13, 34, 35
 Errara, J. 7, 10
 Erzgebirge 12, 6, 7
 Essentialism 6, 5
 Eureka Mine 15, 11
 Evans, M. G. 19, 15
 Evans, P. N. 9, 16
 Evans, W. V. 6, 21
 Everest, G. 15, 6
 Everhart, E. 6, 17
 Ewell, B. S. 9, 25
 Ewell, J. 9, 24
 Ewell, R. S. 9, 25
 Ewell, T. 9, 22-25
 Exposition Universelle, Paris 3, 17
 Externalists 6, 3
 Eyermans, J. 6, 18
 Eyring, H. 19, 15, 48
- F**
- Fackenthal, B. F., Jr. 17, 54
 Fajans, Edgar 6, 12, 15
 Fajans, K. 4, 15-23; 6, 7-15; 13, 48
 Fajans, Stefan 6, 12, 13
 Falcon loom 15, 6
 Falk, K. G. 13, 48; 15, 38
 Falkirk 9, 6
 Fall, P. S. 8, 23, 25, 26
 Farad 11, 91, 98
 Faraday (unit) 11, 91, 92-99
 Faraday, J. 11, 41, 46
 Faraday, M. 10, 6, 8; 11, 3-104; 15, 4, 5
 Faraday, S. 11, 40, 41, 46, 48, 65
 Faraday effect 11, 6
 Faraday Museum 11, 4, 86; 15, 5
 Faraday Soc. 11, 5
 Faraday Soc. Discussions 19, 13-17, 33, 34, 36
 Faraday's Law 11, 95
 Farber, E. 2, 13, 14
 Favorskii, A. E. 17, 28
 Fed. Food, Drug, Cosmetic Act 10, 30
 Fehling, H. 6, 30
 Feigl, F. 17, 31-38
 Feigl, H. E. 17, 32
 Feigl, R. F. 17, 31-33, 35
 Feinstein, R. N. 20, 37
 Fermentation 5, 24, 29, 36
 Fermi, E. 3, 15
 Fertilizers, analysis 20, 18
 Feuchtwanger, L. 17, 55
 Fieser, L. F. 19, 51, 52
 Fischer, E. 4, 5, 17; 6, 6; 13, 44; 17, 2-4, 6, 7
 Fischer, N. W. 4, 3
 Fischer indole reaction 19, 77, 80
 Fisher, D. 6, 17
 Fittig, R. 4, 5
 Fixed air 5, 20; 6, 26; 15, 15, 16, 24
 Fixed nitre 3, 8
 Flavitskii, F. M. 7, 25; 17, 24
 Flory, P. 20, 37
 Fluoric acid 11, 56
 Fluoricum 11, 55
 Fluorine 11, 55-59
 Fluorspar 11, 55, 57
 Flurschheim, B. 19, 14, 26; 20, 14
 Flussaure 11, 55
 Food colorants 10, 25-30
 Fortin, N. 5, 22, 32
 Fortuna 12, 8
 Foster, G. C. 13, 63
 de Fourcroy, A. F. 3, 7-10; 4, 3, 8-11; 5, 4-9, 34-36, 46; 8, 5; 10, 5, 35
Fourteen Weeks in Chemistry 15, 55-57
 Fowler, K. H. 7, 8
 Fowler, R. H. 19, 13
 Fownes, G. 4, 6
 Fox-Talbot camera 8, 26
 Francis, G. W. 10, 8
 Francis, W. 12, 30
 Frankland, E. 4, 6, 7; 9, 4, 5; 13, 30, 33-35, 37; 19, 23
 Frankland, E. P. 4, 7
 Frankland, P. 4, 7
 Frankland's Juvenile Lectures 12, 32
 Franklin, B. 5, 16, 17, 31; 15, 27
 Franklinite 17, 49
 Frazer, P. 6, 16
 Freiburg Un. 4, 4-6
 Fresenius, R. 17, 16
 Fresnel, A. J. 5, 6
 Freud, S. 6, 5
 Freund, I. 13, 6
 Freundlich, H. 4, 16
 Freymann, M. 7, 10
 Freymann, R. 7, 10
 Friedel, C. 9, 17; 19, 58
 Fries, J. F. 13, 29
 Fry, H. S. 6, 9, 31; 13, 48; 15, 38; 19, 15, 26
 Fulhame, Dr. 5, 37
 Fulhame, Mrs. 5, 37-41
 Fuller's Earth Union 9, 4
- G**
- Gadolin, J. 17, 45
 Galen 12, 6
 Galileo 11, 15
 Gallium 17, 18, 19
 Ganz, J. 17, 43
 Gardiner, J. H. 12, 34
 Garrick, F. J. 13, 48
 Garvan, F. P. 13, 43, 46
 Garvan, M. B. 13, 46
 Gas absorption bulbs 5, 32
 Gas burner, Faraday's 11, 70
 Gas silvestre 15, 16
 Gateshead 9, 6
 Gauss, K. F. 11, 96
 Gauze burner 11, 70
 Gay-Lussac, J. L. 4, 3; 5, 6-9, 46; 6, 28; 11, 53, 56; 13, 36, 63
 Gay Lussac tower 9, 6
 von Gebauer-Fullneg, E. 6, 22
 Geber 12, 50, 51
 Geiger, H. 4, 17
 Geijer, B. R. 4, 12
 Geissler, H. 11, 69
 Geissler tube 8, 6
 General Electric 4, 7
 Generator, electrical 20, 48
 Genth, F. 4, 7
 Geoffroy, E. F. 10, 4
 Geol. Survey 17, 46, 47, 51, 52, 55
 Gerhardt, C. 6, 28; 7, 20; 8, 11; 13, 31, 33-37
 Gerhardt, F. 13, 63, 64
 German Chem. Soc. 6, 18; 9, 9
 German dye industry 9, 8-10
 German influence on chemistry 4, 3-7
 Getman, F. H. 7, 32
 Gevaert 17, 32
 Gibb, T., Jr. 6, 10
 Gibbs, J. W. 7, 28, 29
 Gibbs, O. W. 7, 17, 18
 Gibbs Medal 9, 18; 20, 60
 Giessen Un. 4, 4-6
 Gilbert, D. 11, 37, 38
 Gill, A. H. 4, 6
 Gillispie, C. C. 6, 4
 Gilman, H. 19, 77
 Gingham, C. 12, 34
 Ginsberg, H. 17, 19
 Gladstone, J. 6, 31, 32, 34; 11, 12, 13
 Glaig, G. 10, 35, 36
 Glaser, C. 10, 4
 Glass, H. M. 7, 6
 Glass electrode 10, 31, 34
 Glass works, Jamestown 9, 19
 Glauber's spirit of nitre 3, 8
 Glenn, W. 6, 15, 17
 Gliadin 17, 2, 4
 Globulins 17, 2
 Glover tower 9, 6
 Gmelin, L. 4, 3
 Goessman, C. 4, 7
 Göhring, O. 4, 17, 18
 Goldschmidt, V. M. 13, 48
 Gombert, M. 6, 7; 7, 1; 19, 50
 Goniometer 8, 23
 Gooch, F. 15, 35
 Gooding, D. 11, 16, 17, 39
 Goppelsröder, F. 17, 36

- Gorham, J. 2, 9
 Göttingen Un. 4, 4-7
 Göttling, J. 3, 23
 Goudsmit, S. 6, 8
 Gould & Curry Mine 15, 11
 Grabovskii, L. K. 17, 27
 Graebe, C. 9, 8
 Graham, G. 9, 23
 Graham, T. 4, 34; 6, 34; 17, 2
 Granville, A. B. 11, 18
 Grass and hay, analysis for As and Cu 20, 64, 65
 Gray, A. 20, 24, 25
 Great Exposition, London 3, 17
 Green, J. 2, 9
 Greenford Green 9, 8
 Greifswald Un. 4, 4, 5
 Griess, P. 9, 9
 Grignard, V. 19, 59, 62
 Grignard reaction 17, 26; 19, 77, 78
 Grimaux, É. 15, 59, 60
 Grimm, H. G. 4, 19; 13, 48, 51-53
 Griscom, J. 2, 9; 20, 24
 Grjothheim, K. 17, 19
 Groth, P. 4, 19
 Grotta del Cane 1, 10-12; 2, 3; 3, 3
 von Grotthuss, C. 11, 87, 89
 Grubb, A. C. 6, 30
 Grunwald-Winstein equations 19, 72
 Guano 20, 21, 22
 Guano phosphatic 20, 22
 von Guericke, O. 3, 21; 15, 21
 Guerlac, H. 3, 13
 Guettard, J. E. 5, 12
 Guilliton, J. 5, 16, 32
 Gunning, H. 11, 81
 Gunpowder 5, 13, 16, 32; 9, 6, 24
 Guyton, de Morveau, L. 3, 7-9; 5, 4, 34, 45; 8, 5, 10; 20, 5
- H**
 Haber, F. 4, 16, 17, 19; 20, 62
 Haber, L. F. 2, 24; 6, 8
 Hagley Museum 5, 33
 Hahn, O. 3, 15, 16; 4, 18; 6, 8
 Hale, A. C. 3, 16, 17
 Hales, S. 5, 19, 20, 23, 26, 27; 8, 3, 4
 Haliostat 8, 25
 Hall, C. M. 17, 28; 20, 24
 Hall, C. W. 8, 15
 Hall, G. S. 9, 15, 16, 18
 Hall-Heroult process 17, 19
 Halle Un. 4, 4, 5
 Haller, A. 19, 59
 Hallock, E. J. 15, 57
 Hammett, L. P. 19, 15, 17, 19, 20, 31, 48-50; 20, 10
 Hantzsch, A. 4, 5, 7; 6, 11, 12; 9, 17; 20, 52
 Hare, R. 3, 23; 4, 13, 14; 6, 23, 24; 8, 24; 10, 6-8; 11, 68; 15, 27, 28
 Hare's deflagrator 11, 78
 Hare's voltaic trough 11, 78
 Harkins, W. D. 20, 60-66
 Harrington, E. A. 9, 18
 Harris, E. R. 4, 6
 Hart, E. 6, 19, 21
 Hartlib, S. 13, 19
 Hartmann, J. 1, 5; 10, 3
 Harvard College 4, 3
 Hasenclever, P. 17, 44
 Hashimoto, Y. 17, 34
 Hassall, A. H. 10, 28-30
 Hassel, O. 6, 12; 19, 46
 Hatch Act 17, 10
 Hauboldus, G. G. 11, 69
 Hausmann, W. 17, 3, 4
 Havrez, P. 12, 13
 Haynes, W. 2, 11; 3, 4
 Heaveside, O. 11, 13
 Heidelberg Un. 4, 4
 Heilbron, I. 20, 14, 43
 Heintz, W. 20, 28
 Heitler, W. 13, 49
 Hellwig 11, 69
 von Helmholtz, H. 11, 95, 99; 13, 48
 van Helmont, J. B. 1, 4, 5; 13, 20, 25; 15, 16
 Henderson, R., Col. 8, 22
 Hendricks, S. B. 7, 6, 7, 10, 11
 Henninger, A. 12, 17, 18, 20
 Henry, J. 6, 24; 8, 24; 11, 28; 12, 16, 17
 Henry-Beck chem. slide rule 8, 25
 Heroult, P. 17, 18
 Herschel, J. F. W. 11, 19, 20, 22, 37, 85; 12, 19
 Hertz, C. H. 13, 41, 44
 Hessian crucible 11, 79
 Heterolysis 19, 15, 36
 Heumann, K. 10, 9-11
 von Hevesy, G. 4, 17
 Hew Singers 9, 4
 Hexachloroethane 11, 6
 Hexahydrone 7, 5
 Heyl, F. W. 17, 6
 Higgins, B. 4, 31
 Higgins, W. 6, 27
 Hildebrand, J. H. 7, 8, 10, 11, 31; 20, 38
 Hill, R. W. 19, 62
 Hinrichs, G. D. 12, 15
 Hinshelwood, C. 19, 1, 15
 Hippocrates 12, 6
 Hirsch, G. 17, 31
 HIST, founding 13, 44
 Histories, Am. Chem. Depts. 3, 26
 Historiography of science 15, 19
 Hitchcock, E. 6, 24
 Hitchcock, F. 17, 13
 Hobbes, T. 15, 20-23
 Hoechst 9, 8
 Hoefler, F. 7, 22
 Hoffman, G. 12, 4
 Hofmann, A. W. 2, 16; 4, 5, 7; 7, 20-22, 38, 39; 9, 7, 9, 10, 16; 10, 10; 13, 30, 31, 34, 35, 64; 19, 23
 Hofmann elimination 17, 27
 Hofmeister, F. 17, 2, 3
 Hogendahl, K. 19, 28
 Home, E. 11, 17, 18
 Homolysis 19, 15, 36
 Hönigschmid, O. 4, 18, 19
 Hooke, R. 4, 12
 Homberg, W. 3, 21; 5, 19
 Homberg's pyrophorus 2, 22, 23; 3, 21
 Hooker, W. 1, 10, 11
 Hooks and eyes 8, 7
 Hoover, H. C. 12, 3-9
 Hoover, L. 12, 3-6
 Hope, T. C. 15, 4
 Hope's apparatus 8, 23
 Hopewell furnace 17, 46
 Hopkins, F. G. 4, 24; 17, 5, 7
 Horsford, E. N. 4, 5; 20, 29-31
 Howe, H. E. 6, 22; 13, 44
 Hübner, H. 4, 5; 9, 38
 Hüchel, E. 13, 49
 Hudson, J. 11, 17
 Huggins, M. L. 7, 3-5, 7-11; 13, 48
 Hughes, C. E. 6, 4
 Hughes, E. D. 6, 4; 15, 37-42; 19, 3, 4, 8, 15-17, 23, 25, 43-45, 77, 79, 80, 81
 Hughes-Ingold solvent theory 19, 72-75
 Hugo, V. 8, 8-12
 Hume, D. 11, 32
 Hund, F. 13, 49
 Hungarian Acad. Sci. 20, 49
 Hungarian National Museum 3, 12; 20, 46-49
 Hunt, A. E. 6, 17
 Hunt, A. H. 6, 18
 Hunt, R. W. 17, 53
 Hunt, T. S. 12, 35, 37
 Hunter, F. 7, 6
 Hurd, C. B. 9, 19
 Hurd, C. D. 6, 22, 23
 Hutcheson, F. 11, 32
 Hüttig, G. F. 13, 48
 Huxley, T. 11, 7
 Hydrogen bonding 7, 3-13
 Hydrogenium 6, 34
 Hydron 19, 39
 Hydrone 7, 5, 9
 Hydrophobic effect 19, 72, 73
 Hydroxyl Bond 7, 9
 Hydrogen electrode 10, 31, 32
 Hydrogen-calomel electrode 10, 31, 32
 Hygrometer, Daniell's 8, 23

- Hygrometer, Sassure 8, 23
Hyzone 6, 30
- I**
Iatrochemistry 10, 3
Ihde, A. J. 3, 12
Imperial College 20, 43, 44
Imp. Chem. Ind. 19, 4, 16
Indium, discovery 17, 16
Induction 11, 52
Inductive effect 19, 22, 28-30, 33-35, 39
Inductomeric effect 19, 29, 30, 33, 35
Industry, chemical 3, 4-7
Ingenhousz, J. 5, 40
Ingold, C. K. 6, 9; 13, 6, 48; 15, 37-42; 19, 1 ff; 20, 14
Ingold, E. H. 19, 17, 26
Instrumentation 15, 1-7
Insulin 6, 4
Integrated circuit 9, 11-13
Internalists 6, 3
International correspondence course 1, 7
Iodine-azide reaction 17, 37
Ionotropy 19, 37
Iron, assay 17, 41-45
Iron industry 17, 41
Iron manufacture 9, 19-21
Isis 4, 25, 26
Isomorphism 13, 64
Israel 11, 69
- J**
Jackson, C. L. 6, 18; 9, 17
Jackson, C. T. 17, 46
Jacobsen, P. 19, 77, 78, 80, 82, 84
Jaffe, B. 3, 13, 14
Jaffé, H. H. 6, 8
J. Am. Chem. Soc. 6, 19
J. Am. Chem. Soc., first editor 4, 8
James, C. 1, 8, 9
J. Anal. Chem. 6, 19
Janin, L. 12, 4
Jantsch, G. 20, 55
Jardin des Plantes 8, 26
Jardin du Roi 10, 3-5
Jedlik, A. 20, 48
Jefferson, Thomas 3, 11; 8, 19; 9, 21-23
Jeffrey, G. A. 19, 6
Jena Un. 4, 4, 5
Jenkin, H. C. F. 11, 96
Jenkins, R. 3, 12
Joachimsthal 12, 6, 7
Job, A. 19, 59, 60
Joergensen, C. K. 6, 13
Johns Hopkins Un. 4, 6; 9, 15
Johnson, J. 5, 38, 41
Johnson, O. 6, 10
Johnson, S. 12, 50, 51; 17, 1, 2
Johnson, T. B. 17, 6
Johnson-Matthey Co. 4, 14
Johnston, J. 6, 23-25
Joliot, F. 19, 62
Joliot-Curie, F. 20, 33
Joliot-Curie, I. 20, 33-39
Jones, D. V. 17, 6
Jones, H. B. 11, 3, 5, 10, 13, 21, 30, 46, 66
Jones, H. C. 7, 26, 27; 9, 17
Jones, M. 20, 60
Jones, T. P. 2, 9
Joos, G. 4, 19
Joule, J. P. 5, 6; 11, 96
Joy, C. A. 4, 6
Jungreis, E. 17, 34, 37
Justel, H. 4, 12
- K**
Kahlenberg, L. 6, 23
Kant, I. 11, 16; 13, 29
Kaplan, S. 4, 16, 17
Kapulitzas, H. 17, 31
Karlsruhe Congress 13, 335, 63
Karlsruhe Tech. Un. 4, 4
Karlsruhe Un. 4, 17
Karrer, P. 17, 32; 20, 50, 53
Karrer, W. 20, 55
Kastler, A. 19, 59
Kationoid 19, 35, 37, 39, 40
Kauffman, G. 4, 23, 24
Kauffman, H. 13, 48, 50; 20, 13
Kazan Univ. 17, 21, 22
Keating, W. H. 2, 9
Keeler, E. A. 10, 31-33
Kekulé, F. A. 3, 17; 7, 19-22, 27, 28; 12, 10-18, 20, 21; 13, 30, 31, 34-37, 64; 17, 24, 27
Keller, I. 8, 15
Kelvin, Lord 11, 96
Kemmer, N. 11, 82
Kentucky Academy 8, 22
Kentucky Female Eclectic Inst. 8, 23
Kepler, J. 13, 61
Kermack, W. O. 20, 15
Ketelaar, J. A. A. 13, 54-56
Kharasch, M. S. 15, 37; 19, 48, 50, 51
Kieffer, W. 6, 11
Kiel Un. 4, 4
King, C. 10, 22, 23
King, R. 15, 5
King, V. L. 20, 51, 52
Kinnorsley thermometer 8, 26
Kipp generator 8, 35
Kirchhoff, G. R. 8, 6; 17, 16
Kirkwood, J. G. 19, 53
Kirner, W. R. 19, 51
Kirmann, A. 19, 58-63
Kirwan, R. 5, 4, 5, 8
Kaproth, M. H. 11, 82
Klaus, K. 7, 25, 26; 17, 21, 24
Knorr, C. A. 13, 50, 51
Koerner, W. 12, 10-14
Kohler, E. P. 19, 50-52
Kohler, R. E., Jr. 7, 11
Kohlrausch, F. 11, 62
Kolbe, H. 4, 7; 7, 19-24; 12, 10, 13; 13, 30, 31, 33-36; 15, 7, 49; 17, 22-25, 27
Kolthoff, I. M. 15, 2
Kon, G. A. R. 15, 40, 41
Königsberg Un. 4, 4, 5
Königsberger, L. 4, 16
Kopp, H. 4, 5; 7, 19-24; 13, 35, 36; 20, 2-4, 6-8
Körner, W. 2, 4, 5
Kossel, A. 17, 3-5, 7
Kossel, W. 13, 48
Kossuth, L. 20, 46
Koyré, A. 11, 15
Kraus, C. A. 9, 17, 18
Kraus, E. H. 6, 7
Kreshkov, A. 20, 38
Kuh, H. 20, 54
Kuhlmann, C. F. 13, 32
Kühn, O. B. 4, 3
Kunckel, J. 3, 21
Kwart, H. 19, 85, 90
- L**
deLacaille, Abbé 5, 12
Lactic acids 15, 49
Ladenburg, A. 6, 30; 12, 13, 15, 18, 19; 13, 35
L'Affaire Curie 20, 33-39
Lamb, A. B. 6, 11
Landauer, R. S. 6, 30
Landé, A. 13, 48
Landolt, H. H. 20, 53
Langmuir, I. 4, 7; 6, 29; 7, 4; 8, 7; 13, 48; 19, 25, 26
Langmuir Award 7, 1
Lanckester, E. R. 11, 30
Laplace, P.-S. 5, 8, 22, 28, 29, 32, 35; 15, 24
Lapworth, A. 13, 48; 19, 13, 15, 26, 33, 35, 37, 40, 50, 58; 20, 14, 15
deLavergne, L. 5, 16
LaRoche, J. H. 6, 10
Latimer, W. 6, 11; 7, 3-11
Lauraguais, L. L. F. 4, 12
Laurent, A. 6, 28; 8, 10, 11; 13, 31, 33-35, 63, 64
Lavoisier, A. A. 3, 7-9, 21, 22; 4, 8-13; 5, 3-50; 8, 4, 5; 10, 34-36; 11, 55, 68, 69, 77; 13, 27-29, 35, 37, 61; 15, 16, 19, 23-25; 20, 1-8, 48
Lavoisier, A. A., biographies 5, 48-49
Lavoisier, Mme. 5, 4, 6, 9, 12, 31-33, 36, 41

- Lavoisier's mills 5, 9
 Lead, atomic weight 4, 18
 Lead chamber process 9, 6
 LeBas, G. 6, 9
 LeBel, J. A. 12, 10, 11, 14-21; 19, 59, 62
 Leblanc, N. 5, 32
 Leblanc process 9, 5
 LeChatelier, H. L. 3, 17; 17, 15
 LeChatelier, L. 17, 15
 Leeds, A. R. 6, 19
 Leeds & Northrup 10, 31, 34
 LeFèvre, N. 10, 4
 Lehfeldt, R. A. 11, 99
 Lehman, G. 5, 44
 Leicester, H. M. 2, 13; 10, 15-21
 Leipzig Un. 4, 4-6, 16
 Lambert, M. 4, 18
 Lemery, L. 3, 21; 10, 4
 Lemery, N. 10, 3, 4
 Lenard, P. 4, 16, 17; 11, 96
 Lennard-Jones, J. E. 19, 20
 Lenoir, E. 5, 32
 Lepape, A. 19, 62
 Les Forges du Saint-Maurice 2, 15
 Lespieau, R. 19, 58-60, 62
 Letters to editor 2, 3; 8, 30, 31; 9, 34; 10, 45; 12, 45; 13, 72-73
 Leucippus 8, 5; 13, 60
 Levey, M. 2, 14
 Levinstein, I. 9, 9, 10
 Lewin, S. Z. 6, 9
 Lewis, G. N. 6, 23, 31; 7, 3, 4, 6, 8, 11; 8, 7; 13, 48-51; 19, 13, 25-27, 29, 33, 34, 40, 48-50, 52, 60; 20, 11, 14, 62
 Lewis theory 7, 3
 Lewis-Langmuir theory 15, 37; 20, 10, 15
 Leyden jars 8, 26
 Liason hydrogène 7, 10
 Liason monoélectronique 7, 4, 10
 Libavius, A. 10, 3; 11, 68, 69
 Lieber, H. G. 9, 18
 Lieber, L. R. 9, 18, 19
 Liebermann, C. 9, 8
 von Liebig, J. 4, 3-6, 34; 7, 19, 22, 25; 10, 8-10; 11, 49, 60, 64; 13, 30-36, 62; 17, 1, 7
 Lincoln Agr. Coll. 7, 14
 Lind, S. C. 20, 36
 Lindgren, W. 12, 4
 Linnett, J. W. 6, 8
 Linton, L. A. 8, 15-17
 Lipscomb, W. 6, 12
 Liquefaction of gases 11, 22-24, 60, 72
 Liquopodium 11, 53
 Lister Institute 20, 45
 Lloyd, F. 17, 10
 Lloyd, R. 17 9-12
 Lobachevsky, N. 7, 24
 Locke, J. 11, 31
 Lockyer, N. 12, 30, 31
 Lodge, Sir O. 11, 73, 74; 13, 48
 Loeb, M. 9, 16, 17
 Loewi, O. 17, 4
 Lombard Univ. 8, 16
 Lomonosov, M. V. 15, 62
 London, F. 13, 49
 London Co. 9, 19, 20
 London Un. 11, 78
 Long, J. H. 3, 17
 Long Pond Iron Works 17, 54
 Longuet-Higgins, C. 19, 62
 Lonsdale, K. 19, 6
 Lorentz, H. A. 13, 51; 19, 22
 Loring, F. H. 12, 33
 Loschmidt, J. J. 4, 29, 30; 11, 95, 96; 12, 11
 Lossen, W. 15, 45-59, 52, 53
 Losy-Schmidt, E. 20, 47
 Lowry, T. M. 20, 10-16
 Louis XV 5, 15
 Louis XVI 5, 15, 16
 Löwenthal, J. 6, 30
 Lowry, T. M. 7, 5, 6, 11; 13, 48; 19, 13, 14, 26, 27, 33, 34, 37, 40, 60, 61; 20, 48
 Lucas, H. 15, 37; 19, 25, 27, 45, 48, 50, 51
 Luckow, C. 7, 17, 18
 Lucretius 5, 19; 8, 5
 Lunar nitre 3, 8
 Lunar Society 5, 16
 Lunge, G. 3, 19
 Luther, R. 4, 16
 Lutz, R. E. 19, 51
 Lyapunov, M. V. 17, 21, 22
 Lyons, E. 6, 22
 Lysenko, T. D. 20, 37
- M**
 M'Candless, E. V. 7, 17
 McClellan, A. L. 7, 4, 6
 McCutcheon, T. P. 20, 54
 McKenzie, A. 6, 6
 McKenzie, J. D. 6, 9
 McKie, D. 2, 13
 MacKintosh, J. B. 6, 18
 MacLean, I. S. 20, 44
 McMurtie, W. 3, 17, 20; 6, 19
 Maberry, C. F. 6, 18; 17, 10, 12
 Macleod, J. J. R. 6, 4
 Macquer, P. J. 3, 8; 5, 34, 45; 10, 4; 11, 73; 20, 6
 Macquer, P. M. 10, 36;
 Madgin, W. M. 7, 6
 Magdeburg spheres 8, 25
 Magenta 9, 8, 10
 Magnesia alba 3, 8
 Magnesia nigra 3, 8
 Magnetochemistry 11, 5
 Magnus, A. 13, 48
 Magnus, G. 6, 30
 Magrath, E. 11, 29, 37, 67
 Maisch, J. 4, 7
 Makower, W. 4, 17
 Mallet, J. W. 3, 24, 25; 6, 18, 19
 Mallet, R. 3, 24
 Maltby, E. 11, 19, 80
 Manhattan project 6, 8
 Manley J. J. 8, 12
 Manning, J. 10, 26
 Manross, N. S. 4, 5
Manual of Chemistry, Johnston's 6, 23-25
 Marat, J.-P. 5, 14, 17
 Marboe, E. C. 6, 9
 Marburg Un. 4, 4-7
 Marcet, J. 2, 7-9; 5, 41
 Marcet's steam apparatus 8, 23
 Margaric acid 8, 12
 Marggraff, A. 11, 55
 Marguerite, F. 20, 47
 Marignac, J.-C. 13, 62
 Mariposa Mining Co. 15, 11
 Markovnikov, V. V. 7, 25, 26; 17, 21-24, 27, 28
 Marsh, J. 11, 38
 Marsh generator 20, 63
 Marsh test 6, 27, 28
 Marsh's apparatus 11, 78
 Martial chalk 3, 8
 Martin and Synge chromatography 17, 7
 Martius, K. A. 9, 9, 10
 Mass. Coll. Agr. 4, 7
 Masson, G. H. 6, 18
 Matissen, S. 20, 55
 Matteucci, C. 11, 50
 Matthew effect 6, 4
 Mauve 9, 7, 8; 10, 30
 Maxwell, J. C. 11, 4, 8, 13, 94, 96, 97
 Maxwell's equations 11, 13; 19, 21
 Mayer, R. 13, 27-29
 Mayer, T. 11, 37
 Mayo, F. 19, 48, 51
 Mayow, J. 4, 12, 13
 Mechanics Inst. (NY) 20, 30, 31
Mechanics Magazine 11, 12
 Mechanisms, symbols 19, 36
 Medical College of Louisiana 3, 25
 Medicine, routes in chemistry 1, 4, 5
 Megaw, H. D. 7, 9
 Mégnié, P. 5, 21, 32
 Meitner, L. 3, 16; 4, 18
 Meldola Medal 19, 2
 Meldrum W. G. 19, 19, 20
 Mellor, J. W. 6, 31; 7, 13-16
 Melville, H. W. 20, 43
 Mendel, G. 6, 5, 6; 17, 4-7

- Mendel, L. 20, 61
 Mendeleev, D. 3, 17; 5, 7; 7, 27, 28, 30, 31; 8, 6; 13, 62-65; 17, 16, 17
 Mendeleev table 13, 60, 63-65
 Mercurius vitae 3, 8
 Mercury poisoning 11, 47-50
 Merigold, B. S. 9, 16-18
 Merton, R. K. 6, 4
 Merz, V. 17, 10
 Mesabi ores 17, 48
 Mesitylene, Kekulé structure 12, 12
 Mesomerism 19, 14, 15, 22, 25-31, 33, 35, 39; 20, 14
 Metachemistry 12, 33
 Metallurgical chemistry 17, 41, 50
 Metallurgy 17, 41, 50, 52
 Metzger, H. 19, 23
 Methyl violet 9, 8
 Meusnier, J.-B. 4, 13; 5, 28, 29
 Meyer, L. 13, 52, 63-65; 17, 16
 Meyer, V. 4, 5; 15, 34; 17, 16
 Micelles 19, 72-76
 Michael, A. 6, 6; 9, 15, 16; 15, 50; 19, 49, 50
 Microcrith 2, 16-18
 Microprocessor 9, 12, 13
 Midgley, T. J. 6, 22
 Miles, W. 3, 13
 Milk, preserving 8, 18, 20
 Mining engineering 12, 3-5, 7-9
 Miolati, A. 20, 51
 Miranda, L. I. 17, 33
 Mirsky, A. E. 7, 8
 MIT 4, 6
 Mitchell, E. 11, 79
 Mitchell, J. K. 6, 24; 11, 67, 68
 Mitchell, T. D. 5, 42-47; 10, 7
 Mitchell, S.M. 3, 11; 5, 45
 Mitsche, K.R. 17, 19
 Mitscherlich, E. 4, 3; 13, 64
 Mixed multiple bond 20, 10-16
 Mixt 20, 7
 Mixer, W. G. 17, 1
 Mizushima, S. 7, 11
 Moissan, F. F. H. 11, 3, 59
 Mollet, P. 7, 10
 Monge, G. 5, 6, 35
 Monkhouse, C. 11, 13
 Montana Un. 20, 60-62
 Moore, F. J. 6, 3
 Moore, H. 11, 66
 Moore, S. 17, 7
 Moosnick, M. 8, 23
 Morfit & Morfit 11, 73
 Morgan, T. H. 6, 5
 Morhof, G. 13, 19
 Morino, Y. 7, 11
 Morley, F. 20, 42
 Morphia 11, 93
 Morrill, J. S. 20, 27
 Morrill Act 20, 27
 Morrill Hall (Chem.), Wash. State. Un. 20, 27
 Morris, P. 9, 5
 Morse, H. N. 4, 6; 7, 27
 Moseley, H. G. 4, 17; 13, 66
 Moyes, H. 8, 20
 Mozart, W. A. 11, 3
 Muir, M. P. 6, 26, 29, 34
 Mulder, G. 17, 7
 Mulliken, R. S. 9, 16; 13, 49; 19, 62; 20, 60
 Mulliken, S. P. 4, 6; 9, 16
 Multhauf, R. 4, 26
 München Tech. Un. 4, 4
 München Un. 4, 4, 5
 Muncke, G. W. 13, 29
 Munroe, C. E. 3, 20; 6, 18, 20
 Murphy, W. J. 20, 34-36
 Murray, J. 5, 46; 11, 19, 77
 Museum Early Phil. Apparatus 8, 22-26
 Museum of Economic Geology 4, 34
 Muséum d'Histoire Naturelle 5, 9
 Muspratts 9, 4
- ## N
- Nantes School of Sci. 20, 18
 Naquet, A. 12, 15
 Nascent state 6, 26-34
 Nat. Inst. Health 13, 43
 Nat. Museum of Arts and Commerce 4, 24
Nature 12, 30, 31
 Nebraska Section, Am. Chem. Soc. 17, 11
 Needham, J. 4, 24
 Nef, J. U. 9, 16; 19, 50
 Nelson, C. 15, 38
 Neoplatonists 13, 61
 Neptunium 11, 82
 Nernst, W. H. 3, 17; 4, 7, 18; 6, 11; 13, 44
 New Almaden Quicksilver Mine 15, 10, 11
 Newlands, J. A. R. 13, 62, 63, 66
 Newman, J. 11, 82, 84; 12, 25
 Newman-Brooke oxyhydrogen blowpipe 11, 81-83, 85
 Newth, G. S. 10, 11
 Newton, I. 5, 20, 21, 26; 11, 9, 48; 13, 19, 22-26
 Newton's engine 8, 23
 Nichols, W. H. 13, 41
 Nichols Medal 9, 17, 18
 Nicholson, E. C. 9, 10
 Nicholson, H. H. 17, 9-12
 Nicholson, W. 5, 46; 11, 86
 Nitre d'argent 3, 8
 Nitrogen, electron structure 20, 12
 Nitrogene 3, 10
 Nixon, A. C. 20, 3
- Nobel Prize 6, 8, 12; 19, 45, 46; 20, 51, 56
 Noble gases 5, 5
 Noddack, I. 3, 15, 16
 deNollet, Abbé 5, 12
 Nomenclature, chemical 3, 7-11
 Nomenclature, Lavoisier 10, 35, 36
 Nooth apparatus 5, 39, 40
 Norris, J. F. 9, 17; 19, 50
 Norrish, R. G. W. 20, 14
 North Staffordshire Tech. Coll. 7, 15
 Northmore, T. 11, 24
 Norton, L. M. 4, 6; 6, 18
 Norton, R. 20, 35
 Norton, T. H. 6, 16
 Norwood Tech. Inst. 15, 2
 Notice to Subscribers 12, 51
Nouveau Journal de chimie 1, 22
 Noyes, A. A. 4, 6; 9, 17; 19, 51; 20, 62
 Noyes, W. A. 3, 17; 6, 19; 13, 44-46, 48; 15, 38; 19, 13; 20, 14
 Noyes, W. A., Jr. 13, 44, 45
 Nuclear fission 3, 15, 16
 Nyholm, R. 19, 7, 66, 70
- ## O
- Obituaries 12, 48
 Oekonometer 15, 6
 Oersted, H. C. 11, 20, 36
 Oertling, L. 8, 12, 13
 Oertling balance 20, 43
 Oesper, R. E. 1, 13, 14; 2, 11
 Oesper Lecture 4, 3
 Offa Helmontii 3, 8
 Ogg, R. 19, 48
 Oldenburg, H. 4, 12
 Olympic Games 20, 31
 Optical fibers 9, 12
 Orbital symmetry control 19, 85
 Orna, M. V. 2, 23
 Ordway, J. 6, 17
 Organic chemistry, history in US 6, 3
 Orléanis, Prov. Assembly 5, 16
 Országos Műszaki Múzeum 20, 47
 Ortigosa, J. V. 4, 5
 Orton, K. 19, 17, 23
 Osann, G. W. 6, 30
 Osborne, T. B. 17, 1-7
 O'Shaughnessy, W. B. 10, 28
Osiris 4, 25
 Ostwald, W. 3, 17; 4, 5, 16; 6, 26, 33; 7, 5, 27, 28, 30, 31; 10, 12, 13; 11, 98; 13, 44; 15, 7; 19, 50
 Otter, W. 11, 79, 80
 Outstanding Paper Award 1, 18; 2, 24, 25; 4, 32; 6, 44; 12, 47; 13, 74
 Overhead projectors 10, 12
 Overman, F. 17, 54
 Owens, R. 11, 29
 Owens College 4, 7

- Oxford Un. 4, 3, 6
 Oxnard Brothers 17, 11
 Oxygen 5, 15, 16, 18, 22
 Oxygen electrode 10, 32
 Oxymuriatic acid 5, 7
- P**
 Paenecarbanion 19, 38
 Paenecarbonium ion 19, 38
 Paneth, I. 4, 18
 Pagel, W. 3, 12
 Palmgreen, C. 8, 23
 Paracelsus 1, 4; 10, 3; 12, 7, 8; 19, 22
 Parachor 20, 16
 Paris, J. A. 11, 22, 23
 Paris School org. reaction mechanisms 19, 58-63
 Parkhurst, G. L. 6, 21
 Parrish, J. 5, 44
 Parsons, A. 13, 48
 Parsons, C. L. 1, 8, 9; 4, 20; 6, 18; 13, 44
 Partington, J. R. 2, 13, 14; 19, 23
 Pascal, B. 8, 5; 11, 48
 Pasteur, L. 12, 19; 13, 30, 34; 17, 2
 Patterson, H. J. 6, 17
 Pauli principle 7, 7
 Pauling, L. 4, 20; 6, 8, 10-12; 7, 1, 6-11; 13, 49, 63; 19, 17, 25, 29, 30, 48, 52, 62; 20, 34, 36
 Paulze, J. 5, 12
 Pearl ashes 3, 8
 Peckham, S. F. 8, 15, 16
 Peel, R. 11, 17
 Peligot, E. M. 13, 36; 20, 22
 Pelouze, T. J. 13, 36; 20, 20
 Pennington, J. 8, 18-20
 Penn. Un. 4, 6, 7
 Pennington, J. 5, 44
 Penny, F. 4, 6
 Pentahydrone 7, 5
 Peptones 17, 4
 Pepys galvanometer 11, 78
 Pepys voltaic apparatus 11, 78
 Pepys voltaic conductor 11, 78
 Perkin, F. 4, 6
 Perkin, W. H. 9, 7-10; 10, 30
 Perkin, W. H., Jr. 3, 17; 4, 6; 7, 14; 20, 28
 Perkin Medal 20, 24
 Perley, G. A. 10, 33
 Perrin, J. 7, 5, 10; 11, 99; 19, 59, 60, 62
 Peter, A. M. 6, 17
 Peter, R. 8, 23, 26
 Petit, A.-T. 13, 64
 Pettenkofer, M. 13, 62, 63, 65
 pH measurement 10, 31-34
 Philadelphia Exposition 3, 25
 Philalethes, E. 13, 19-26
 Phil. Coll. Pharm. 4, 7
 Phillips, R. 11, 20, 23, 36, 67, 77
 Phillips, W. 11, 77
 Philos. Mag 12, 30
 Philosophers' stone 13, 25
 Phipson, T. L. 6, 30, 31
 Phlogiston 3, 8, 9, 22; 4, 8, 9; 5, 3-8, 17, 18, 21, 22, 24, 27, 37, 39-44; 8, 19, 20; 10, 36; 15, 20, 23, 24; 20, 1-3, 6-8
 Physical org. chem., US 19, 48-53
 Physical org. chem., terminology 19, 33-40
 Pimentel, G. C. 7, 4, 6
 Pinkus, A. 6, 31
 Pitch 9, 21
 Pitzer, K. S. 19, 48
 Plant proteins 17, 1, 2
 Plato 6, 5
 Platonic solids 13, 60, 61
 Plato's square 13, 60
 Plato's triangle 13, 60
 Playfair, L. I. 11; 4, 4, 6, 34
 Plücker tube 1, 9
 Plumbago 11, 57, 58, 84
 Plutonium 11, 81, 82, 84
 Pneumatic chemistry 5, 19, 20, 26
 Pneumatic chemists 8, 3
 Pneumatic trough 8, 3
 Poggendorff's galvanometer 11, 78
 Polanyi, M. 19, 15, 16, 62
 Polarimeter 20, 53
 Polarizability 19, 34, 35
 Polarization 19, 34, 35
 Polar-transition-state theory 19, 81-84
 Politics, Lavoisier's 5, 10-14
 Polonium 8, 6
 Popov, A. N. 17, 24, 27
 Popular Chemistry 15, 57, 58
 Porenski, V. 7, 10
 Potash, Hopkins prep. 8, 21
 Powder Commission 5, 16
 Precipitate washer 15, 5
 Pregl, F. 17, 36
 Prescott, A. B. 3, 19, 20; 6, 16, 19, 20
 Prévost, C. 19, 58-63
 Priestley, J. 4, 11, 13; 5, 15-18, 24, 28, 31, 37-41; 6, 26, 27; 8, 3-5, 21; 11, 77; 13, 23; 15, 23, 24
 Priestley Medal 9, 18
 Principe oxigene 3, 9
 Proctor, H. B. 3, 19
 Program Chair, message from 3, 28
 Protactinium 3, 16; 4, 18
 Prototropy 19, 37, 60
 Proust, L. 3, 23
 Prout, W. 5, 6; 13, 62-65
 Prout's hypothesis 12, 33; 13, 8; 19, 20
 Prussian blue 3, 23
 Pseudophase kinetic model 19, 73, 74
 Pullar & Sons 9, 7
 Puddling process 9, 6; 17, 51
 Pure Food Drug Act 10, 30
 Pure food laws 10, 25
 Pyridine, structure 2, 4-6
 Pythagoreans 13, 60-66
- Q**
 Quanticule 6, 8-13
 Quantification 1, 4
 Quantivalence 2, 16
 Quicksilver 11, 47
 Quinhydrone electrode 10, 31, 33
 Quinia 11, 93
- R**
 Rabinowitch, E. 20, 34, 36, 37
 Radium 8, 6
 Rajman, E. 17, 31
 Rammelsberg, K. F. 12, 19
 Ramsay, W. 5, 5; 7, 29, 20; 13, 48; 19, 3
 Ramsden, J. 8, 12; 15, 2, 3
 Ramsden balance 15, 4
 von Ranke, L. 7, 19, 22
 Raoult, F. 19, 59
 Rappaport, F. 17, 31
 Rayleigh, Lord 5, 5; 7, 29, 30; 11, 97
 Read, J. 2, 12
 Rearrangement, benzidine 19, 45, 77-87
 Rearrangement, benzoic acid 15, 38-40, 41, 42
 Rearrangement, Hofmann 15, 39, 40
 Rearrangements, intramolecular 15, 38-42
 de Reaumer, R. A. F. 17, 45
 Records of chemistry 9, 3-6
 Reed, C. J. 6, 16
 Reformatskii, S. N. 7, 25; 17, 26
 Régie des Poudres 5, 9
 Regulus of antimony 2, 23
 Reich, F. 17, 16
 Reid, D. B. 12, 25
 Reid, E. E. 7, 30; 19, 49
 Remelé, A. 13, 65
 Remick, A. E. 19, 14, 15, 31
 Remsen, I. 3, 17; 4, 3, 6; 6, 19; 9, 15; 13, 41
 Renard Frères 9, 8
 Rensselaer Poly. Inst. 12, 26
 Residual affinities 20, 15
 Resolution, Pasteur 20, 52
 Resonance 7, 8
 Resonance theory 19, 14, 27, 30, 34
 Rhenium 3, 15
 Rhombahedral benzene 12, 15
 Richard, E. S. 8, 15, 16

- Richards, E. S. 3, 20; 17, 12
 Richards, T. W. 4, 18; 9, 16; 11, 9; 19, 19, 51
 Richarz, F. 13, 48
 Richter, T. 17, 16
 Ridgely, F. 8, 22
 Riebau Book Store 11, 5
 Riecke, E. 15, 34
 Ring stand 11, 71
 Rinne, F. 7, 5
 Ritchie, W. 11, 88
 Ritter, J. W. 11, 86
 Ritthausen, H. 17, 1, 2, 4, 5
 Rivière & Rivière 11, 73
 Rivot, L. E. 17, 46; 20, 20
 Roberts, C. F. 15, 31-35; 17, 13
 Roberts, Dale & Co. 9, 9
 Robierre 11, 73
 Robinson, R. 13, 48; 15, 37, 39; 19, 1, 14, 26-28, 33-35, 37, 40, 43-46, 50, 58, 79, 80; 20, 14, 15
 Robinson, T. 6, 17, 18; 8, 12; 15, 4
 Robinson balance 15, 4, 5
 Rochow, E. 6, 10
 Rockefeller Found. 4, 21
 Rocky Hill Copper Mine 17, 44
 Rodebush, W. H. 7, 3-11
 Rolfe, J. 9, 2
 Rolfinck, G. 10, 3
 Rona, E. 4, 18, 19
 Röntgen rays 4, 16
 Röntgen, W. 4, 19
 Rosanoff, L. (see Lieber, L.)
 Rosanoff, M. A. 9, 17
 Roscoe, H. 4, 3, 6, 7; 9, 9; 11, 12, 70, 95; 17, 16
 Rose, G. 4, 3
 Rose, H. 4, 3, 5
 Rose, W. C. 17, 5
 Rossel, R. 17, 34
 Rossi, A. 17, 48
 Rostock Un. 4, 4, 5
 Rouelle, G.-F. 5, 12, 26; 10, 4, 5, 7
 Rouelle, H. M. 10, 4
 Roy. Astron. Soc. 11, 38
 Roy. Coll. Chem. 4, 6, 7; 11, 70
 Roy. Inst. 11, 3-5, 7, 8, 10, 14, 15, 20, 24, 25, 28, 30, 33, 34, 36-40, 44, 53, 65, 66, 73, 76-79, 81, 86, 96, 98
 Roy. Inst., Manchester 4, 34
 Roy. Mil Acad. 11, 38
 Roy. Scottish Mus. 4, 26
 Roy. Soc. 5, 16, 17; 11, 17-26, 36-38, 85, 87
 de Rozier, P. 3, 23
 Rumford, Count 5, 6, 33, 37; 11, 65
 Rumford thermometer 8, 26
 Rush, B. 8, 18, 20, 21; 9, 23
 Russell, A. 17, 19
 Russ. Phys. Chem. Soc. 17, 27
 Ruthenium, discovery 17, 24
 Rutherford, E. 4, 17; 6, 10; 7, 14; 8, 6; 11, 96
 Ryan, H. C. B. 12, 33, 34
- S**
 Sabatier, P. 19, 59
 Sabine, E. 11, 38
 Sadler, J. 11, 24
 Sadtler, S. P. 4, 6; 10, 10, 11
 Safety lamp, Davy's 11, 71
 Salleron, J. 8, 14
 Salt particle 13, 24
 Salt works, Virginia 9, 21
 Samter, Herr v. 1, 6-8
 Sand bath 11, 70
 Sandeman, R. 11, 41, 46, 47
 Sandemanian(s) 11, 29, 32-34, 40-44
 Sanger, F. 17, 7
 Sannié, C. 7, 10
 Sarton, G. 6, 3, 4
 Saugus Iron Works 17, 43, 44
 Sausage symbols 12, 11
 deSaussure, H. B. 5, 8
 Savart 11, 52
 Saytzeff Rule 17, 27
 Schäfer, H. 13, 57
 Scheele, C. W. 3, 22, 23; 11, 55
 Scholze, E. 20, 52
 Schönbein, C. 6, 30, 31; 11, 46, 50
 Schönberg, C. F. 17, 36
 Schorlemmer, C. 4, 7; 19, 23
 Schuckburg, F. 11, 19, 25
 Schuette, H. A. 3, 12
 Schwyzer, E. 20, 55
 Sci. Mus. London 4, 26
 Scoffern, J. 11, 65
 Scott, T. A. 15, 10-12
 Scudamore, C. 11, 19
 Seaborg, G. 11, 82
 Seaman, W. N. 6, 18
 Sears, M. J. 9, 18
 Seaton, C. 9, 6
 Sedgwick, A. 11, 81
 Séguin, A. 15, 24
 Sel nitreux 3, 8
 Sel vitriolique 3, 8
 Semiconductors 9, 11-14; 11, 58
 Semple, F. 15, 10
 Sennert, D. 10, 3
 Serrin, V. 8, 12-15
 Seubert, K. 13, 65
 Seybert, A. 5, 44
 Sharples, S. P. 6, 18
 Shaw, F. R. 19, 28
 Sheffield Sci. School 15, 9, 12
 Shelburne, Earl of 5, 16
 Sherman, A. 7, 9
 Sherman, J. 7, 7; 19, 30
 Shockley, W. 9, 11
 Sidgwick, N. V. 7, 5, 6, 9-11; 13, 48; 19, 13, 20, 29; 20, 12
 Siecke, W. 11, 47
 Siegfried, R. 8, 4
 von Siemens, W. 11, 96; 20, 48
 Sigmatropic shifts 19, 85
 Silicon 11, 81
 Silicon Valley 9, 11
 Silk industry 9, 22
 Silliman, Benjamin, Jr. 2, 7; 12, 35-38; 15, 9-14; 17, 49
 Silliman, Benjamin, Sr. 2, 7; 6, 23, 24; 12, 36; 15, 9
 Silliman chemistry text 12, 35-38
 Sillimans' journal 11, 79
 Simon, W. 6, 17
 Simpson, Maule & Nicholson 9, 8
 Simple substances, Lavoisier's 5, 5, 35, 43
 Slack, E. 5, 46
 Slater, J. 13, 49
 Slide rules, chemical 12, 24-28
 Small, L. 19, 51
 Smeaton, W. 8, 4
 Smelting 20, 62, 63
 Smirnov, A. P. 20, 55
 Smith, A. 6, 32, 34
 Smith, C. S. 4, 24
 Smith, E. F. 1, 13, 20, 21; 3, 17; 4, 6; 6, 3; 7, 18; 10, 21-25; 13, 44; 19, 23
 Smith, I. B. 10, 32, 33
 Smith, Capt. J. 9, 19, 20
 Smith, T. P. 2, 6, 7; 10, 36
 Smithsonian Inst. 4, 26; 12, 26, 27
 Smoke analysis 20, 64
 Sobolev, N. N. 6, 9
 Société d'Arcueil 5, 8
 Soddy, F. 4, 17, 18; 6, 8
 Sodium basket 7, 38, 39
 Sodium spoon 7, 38, 39
 Sommerfeld, A. 4, 19, 20; 7, 6
 Solid-phase functional group analysis 17, 37
 Solvatochromic scale 19, 72, 76
 Solvay Conference 19, 60
 Sommer, L. 19, 51
 Sons of Gwalia 12, 4, 5
 von Soxhlet, F. 3, 17
 Späth, E. 17, 31
 Speakman, J. C. 7, 6
 Spectroscope 8, 6
 Speisewasser 15, 6
 Spence, P. 9, 5
 Spencer, Lord 11, 81
 Spirit of marine salt 5, 19
 Spiritualism, 11, 30, 31
 Spot tests 17, 33-37, 52
 Spotswood, A. 9, 20, 21
 van Spronson, J. 3, 14
 St. Louis Exposition 3, 25
 Staedeler, G. 12, 13; 20, 28

- Staffordshire iron 17, 51
 Stahl, G. E. 5, 3; 13, 37; 20, 2,4,6,7
 Stark, J. 13, 48-51
 Starkey, G. 13, 19, 21
 Stas, J.-S. 13, 62
 Steele, J. D. 15, 55-58
 Stein, W. 17, 7
 Stewart, C. 17, 50
 Steinbeck, R. 7, 17
 Stereochemistry, textbooks 15, 31-35
 Stein, W. 17, 7
 Stewart, C. 17, 50
 Stewart, T. D. 7, 3
 Stieglitz, J. 13, 41, 48; 15, 38; 19, 50
 Stilwell, C. 13, 52-54
 Stirrer apparatus 8, 35
 Stock, A. 11, 47-49
 Stoddard, W. 9, 25
 Stoddard, J. T. 9, 37, 38
 Stoddard test tube clamp 9, 37
 Stone, W. E. 3, 20; 6, 16, 17
 Stoney, G. J. 11, 94-97
 Storey, A. M. 11, 19
 Strassburg Un. 4, 4, 5
 Strassman, F. 3, 15, 16
 Stromeyer, F. 4, 3
 Strutt, R. J. 11, 97
 Strychnine 19, 44
 Sturchio, J. L. 2, 24, 25
 Sturgeon's apparatus 11, 78
 Substitution, Dumas law of 8, 10
 Sugar beets 17, 11; 20, 21
 Sugar of Saturn 3, 8
 Sugden, S. 20, 14, 16
 Sulfide, detection 4, 34
 Sulfinates, resolution 20, 16
 Sulfoxides, discovery 17, 22-24, 27
 Sullivan, F. 6, 22
 Surfactants 19, 73-76
 Sutter, H. A. 17, 34
 Sutton, L. E. 19, 62
 de Suvigny, L. J. 3, 22, 23
 Swain, R. E. 20, 61, 64
 Symbols, chemical, of Hassenfratz
 and Adet 3, 7, 8, 10
 Synartetic acceleration 19, 35, 40, 44
 Synionie 19, 59-62
 Szabadvary, F. 3, 12, 13; 20, 46-49
 Széchenyi, I. 20, 49
- T**
 Tafel, J. 4, 7
 Talbott, B. E. 4, 6
 Tatum, Mr. 11, 82, 83
 Tar 9, 21
 Tarbell, A. T. 4, 31
 Tarbell, D. S. 4, 31
 Tarnowsky, J. 6, 22
 Tartarus citratus 3, 8
 Taubes, S. 17, 31
 Tautomeric effect 19, 28-31, 33-35
- Taylor, H. 6, 29
 Tax Farm 5, 12, 15, 16
 Teachers and hist. chem. 8, 3-8
 Tennant, S. 11, 80
 Tennessee, Un. 3, 26
 Tenteleo Chem. Plant 17, 17
 Terminology, physical organic 19,
 33-40
 Terra anglica rubra 3, 8
 Test tube 11, 72
 Tetrachloroethylene 11, 6
 Tetrahedron 7, 5
 Texts, early American chemistry 2,
 6-10
 Thackray, A. 4, 25
 Tharsis Chem. Co. 9, 4
 Thenard, L.-J. 5, 7, 9; 10, 5, 6; 13,
 36
 Thiele, F. K. J. 13, 50; 19, 26; 20,
 13-15
 Thiersch, K. 13, 36
 Thilorier, M. 11, 24
 Thomas, C. A. 20, 35
 Thomas Iron Co. 17, 52, 54
 Thomists 6, 5
 Thompson, B. (Count Rumford) 5, 6
 Thompson, S. P. 11, 12-16, 67; 17,
 10
 Thoms, G. 3, 19
 Thomsen, J. 6, 29
 Thomson, G. M. 7, 14
 Thomson, J. J. 6, 30, 31; 11, 80, 95-
 97; 13, 48; 15, 38; 19, 13, 21, 22;
 20, 11, 13, 14
 Thomson, T. 2, 7; 5, 7, 41, 46; 9, 3;
 10, 36; 12, 24, 28; 13, 62
 Thomson, W. 15, 5
 Thorpe, E. 20, 42, 43
 Thorpe, J. F. 19, 2, 3, 13, 14, 23, 29,
 34, 43, 49; 20, 14, 42-44
 Thorpe, T. E. 6, 32, 33
 Thorpe-Ingold effect 19, 43
 Tilden, W. 20, 44
 Tillet, M. 4, 13
 Tin, analysis 20, 19
 TNT 9, 6
 Tobacco 9, 20
 Todd, A., Lord 20, 43
 Todd, A. M. 6, 17;
 Todd, D. 20, 37
 Todd, M. 4, 18
 Tollens, B. 4, 5
 Tommasi, D. 6, 29, 33
 Toricelli, 8, 5
 Torrey, J. 6, 24; 12, 26, 27
Traité Élémentaire de Chimie 5, 34-
 37
 Transistor 9, 11
 Transmutation 13, 60
 Transylvania Un. 8, 22
 Traube, M. 6, 34, 35
- Tribe, A. 6, 31, 32, 34
 Trimble, H. 6, 17
 Trudaine lens 4, 12
 Tschirnhausen mirror 4, 12
 Tube chemistry 11, 72, 77
 Tübingen Un. 4, 4, 5
 Turgot, A. R. 5, 13-15
 Turkey red 17, 17
 Turner, E. 6, 23, 24; 20, 25
 Tutenage 11, 53
 Tyndall, J. 11, 3, 5, 7, 11-13, 15, 16
 Types, theory of 8, 11
 Tyrian purple 1, 14; 9, 8
 Tytler, J. 10, 34-38
- U**
 Uehara, Y. 7, 11
 Uhink, W. 8, 14
 Uhlenbeck, G. E. 6, 8
 Un. Coll. London 4, 6; 19, 2-9, 19-
 24
 Union Oil Co. 8, 16
 Universitätsbibliothek, München 4, 4
 Upjohn 17, 6
 Uranium 8, 6; 11, 82
 Uratom 15, 49, 53
 Urbain, G. 19, 59
 Urea, Wöhler's 13, 62; 20, 51
 Uric acid 17, 36
 US Geol. Survey 12, 3, 4
 US Nat. Mus. 4, 26
 Usherwood, E. 20, 44
- V**
 Vagner, E. E. 17, 25-28
 Valence 2, 16
 Valence block set 1, 9
 Valency, history 9, 3
 Valley of death 1, 11, 12
 Vámos, E. 20, 49
 van Arkel, A. E. 13, 48, 49, 54, 55
 van Arkel triangle 13, 47, 54-57
 van der Capellen, H. 17, 44
 van Maets, C. L. 10, 3
 van Rensselaer, S. 12, 26
 Van Slyke, D. D. 17, 6
 van't Hoff, J. H. 3, 17; 6, 26; 7, 5,
 27, 29, 30, 31; 12, 10-12, 16-21;
 15, 31, 34, 35, 45-49; 19, 59, 62
 Varrentrapp, F. 13, 34
 Vauquelin, L. N. 5, 8, 9
 Vermont Acad. Med. 12, 26-28
 Vickery, H. B. 17, 6, 7
 Vieweg, E. 13, 31
 de la Villette, F. 4, 12
 Villette mirror 4, 12
 Vilmos, A. 20, 51
 Violet imperial 9, 8
 Virginia, colonial 9, 19
 Vital air 5, 7
 Vogt, C. 10, 9

- Vollhard, J. 13, 34
 Volta pile 11, 86
 Vulgar chemistry 13, 24
- W**
- Wadsworth-Emmons olefination 17, 27
 Wagner oxidation 17, 26
 Wagner-Meerwein rearrangements 19, 35, 44
 Walden inversion 19, 44, 67
 Wallace, A. R. 6, 5, 6
 Wallach, O. 4, 5
 Wallis, E. S. 15, 38-40
 Walsh, A. D. 6, 11
 Waltham Abbey 9, 6
 Warburton, H. 11, 21-23, 25, 26
 Warden, R. B. 19, 49
 Warder, 3, 20
 Warltire, J. 15, 16
 Warren, W. H. 9, 17, 18
 Washburn, E. W. 7, 32
 Washington, A. 9, 21
 Washington, B. 9, 23
 Washoe Smelter 20, 60-64
 Waters, W. A. 20, 10, 16
 Watson, H. B. 15, 42; 19, 13, 23, 31; 20, 10
 Watt, J. 5, 40
 Watts, H. 12, 33
 Wavell, W. 11, 19, 80
 Weber, W. E. 11, 96
 Webster, J. 6, 23; 10, 5, 6, 9
 Wedgewood, J. 15, 16
 Wedgewood's pyrometer 8, 26
 Weeks, M. E. 3, 11
 Weems, J. 9, 23
 Weems, J. B. 9, 16, 18
 Weidenfeld, L. 17, 31
 Weissberg, K. 17, 31
 Welch, A. J. E. 20, 43
 Wellcome Mus. Hist. Med. 4, 26
 Wellesley College 15, 31, 32, 35
 Wells, H. G. 17, 6
 von Welsbach, A. 17, 16
 Weltausstellung, Wien 3, 17
 Welther's tube 11, 79
 Wendt, G. L. 6, 30
 Werner, A. 4, 23, 24; 19, 26, 67; 20, 50-56
 West, J. 7, 9
 West, P. W. 17, 33, 35
 Westby, G. 20, 60
 Westheimer, F. 19, 48, 50-53
 Wetherill, C. M. 4, 5
 Weyde 11, 73
 Weyl, W. 6, 9
 Wharton, J. 17, 54
 Wharton furnace 17, 52
 Wheatstone, C. 11, 96
 Wheland, G. W. 6, 8; 19, 20, 30, 48, 51, 52
 Whewell, W. 11, 5, 30, 32, 34, 80
 White, G. F. 9, 17
 Whiteley, M. A. 20, 42-44
 Whitmore, F. C. 15, 38-42; 19, 48, 50-52
 Wichelhaus, H. 12, 13
 Wichmann, A. 17, 2
 Widnes 9, 4
 Wiechert, E. 19, 21, 22
 Wieland, H. 4, 20
 Wigner, E. 19, 15
 Wilcke, J. C. 5, 6
 Wiley, H. W. 3, 16-19; 6, 15, 16, 19, 20; 13, 45; 20, 60
 Wilkinson's galvanic trough 11, 78
 Will, H. 7, 19, 22
 Willard, H. 6, 7
 Willard, J. T. 6, 17
 Willcock, E. G. 17, 5
 William, T. I. 3, 14
 Williams, J. 7, 6
 Williams, C. G. 11, 73
 Williams College 4, 6
 Williamson, A. 4, 6; 7, 20; 11, 96; 13, 31, 35, 37, 64
 Willm, E. 6, 34
 Willstätter, R. 4, 17, 19., 20
 Wilson, C. L. 19, 3, 15-17
 Wilson, R. E. 6, 21
 Wine lees, alkali of 3, 8
 Winstein, S. 19, 35, 43-45, 48, 50, 51
 Winthrop, J. 17, 43, 44
 Wislicenus, G. 20, 28, 29, 31
 Wislicenus, J. 4, 5, 7; 7, 20, 21; 9, 16; 12, 10; 15, 45-50, 52, 53; 20, 28-31
 Wislicenus, J.(Hans) 20, 28
 Wislicenus, W. 20, 28
 Wisser, 11, 73
 Wistar, C. 8, 18, 20, 21
 Witt, O. 3, 20; 9, 9, 10
 von Witte, S. 17, 17
 Wittig reaction 15, 61
 Wöhler, F. 3, 24; 4, 3; 7, 22; 13, 31-34, 36
 Wolf, K. L. 7, 11
 Woll, F. W. 6, 17
 Wollaston, W. H. 8, 24, 25; 11, 19, 20, 22, 25, 36, 37, 72, 73, 79, 80, 83; 12, 24-26, 28
 Wood, G. B. 15, 27
 Woodhouse, J. 5, 41; 8, 21
 Woods, C. D. 6, 17
 Woods, H. J. 7, 8
 Woodward, R. B. 19, 43-45
 World War I 13, 40-46
 World's Congress of Chemists 3, 16-21
 Wotiz, J. H. 4, 24, 25
 Woulfe bottles 8, 24, 34
 Wulf, O. 7, 10
 Wurtz, A. 5, 3; 6, 29, 33; 7, 20, 22, 25; 8, 8; 12, 15; 13, 33-35, 37; 17, 23; 19, 58; 20, 2, 4
 Wurtz, H. 17, 46
 Würzburg Un. 4, 4-6
 Wüst, P. 20, 54
- X-Y**
- X-rays 8, 6, 7
 Yale College 4, 3
 Yapp, D. 20, 44
 Yeh, P.-Y. 13, 53-56
 Young, T. 5, 6; 11, 38, 77
 Young, W. G. 19, 45, 48, 51
- Z**
- Zachariasen, W. H. 7, 8
 Zachariasen theory 6, 9
 Zaitsev, A. M. 7, 25; 17, 21-28
 Zaitsev, M. M. 17, 25
 Zaitsev, M. S. 17, 21
 Zamboni's apparatus 8, 26
 Zeeman Effect 11, 96
 von Zeerleder, A. 17, 19
 Zein 17, 2, 4, 5
 Zelinski, N. D. 17, 28
 Zenghelis, C. 6, 33
 Zinc reagents 17, 25, 26, 28
 Zinggler, E. 20, 55
 Zinin, N. N. 7, 25, 26; 17, 21, 23
 Zintl, E. 13, 51, 57