

# BULLETIN FOR THE HISTORY OF CHEMISTRY

Division of the History of Chemistry of the American Chemical Society

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John Johnston  
(1806-1879)

**BULLETIN FOR THE HISTORY  
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Editor ..... William B. Jensen  
Associate Editor ..... James J. Bohning  
Editorial Assistant I ..... Kathy Orchin  
Editorial Assistant II ..... Elaine Seliskar

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***The Cover...***

This issue shows an engraved portrait of John Johnston (1806-1879), Professor of Natural Science at Wesleyan University in Middletown, CT, from 1835-1873 and author of one of the most popular American chemistry texts of the early 19th century. More on Johnston and his textbook can be found in this issue's "Old Chemistries" column.

**DEADLINES**

The deadline for the next issue (Fall 1990) is 30 September 1990. All materials should be sent to Dr. William B. Jensen, Department of Chemistry, University of Cincinnati, Cincinnati, OH 45221, (513) 556-9308.

**CONTENTS**

<b>The 1989 Dexter Address</b> by <i>D. Stanley Tarbell</i>	3
Some musings on the writing of history of science	
<b>Kasimir Fajans</b> by <i>Reynold Holmen</i>	7
This concluding installment deals with Fajans' career in the United States and the controversy over quanticule theory	
<b>The Continental Chemical Society</b> by <i>James J. Bohning</i>	15
The almost demise of the ACS and the compromise that saved it	
<b>Diversions and Digressions</b> by <i>James Doheny</i>	21
The tie that blinds and other satorical satire	
<b>Old Chemistries</b> by <i>William D. Williams</i>	23
Remembering John Johnston's <i>Manual of Chemistry</i> and its author	
<b>Whatever Happened to ... ?</b> by <i>William B. Jensen</i>	26
Once a textbook staple, the nascent state has disappeared - or has it?	
<b>The 1989 Bibliography</b>	37
Even larger than last year	
<b>Book Notes</b>	41
An autobiography, a Russian chemist-composer, and a volume on the history of electrochemistry	
<b>Divisional News</b>	
From the Chair	43
Awards	44
Events of Interest	45
Future Meetings	46
1990 Officer's Directory	47
<b>Future Issues</b>	47
A preview of what's to come	

## THE 1989 DEXTER ADDRESS

### Ways of Writing the History of Science

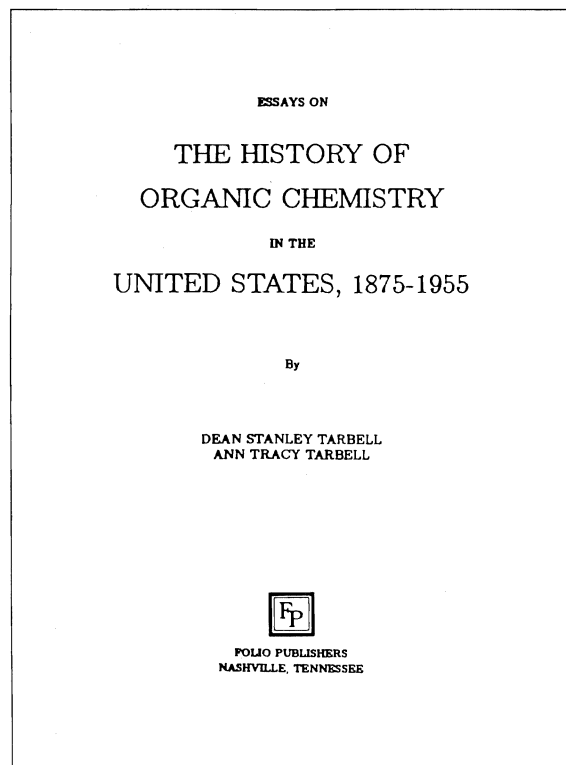
*D. Stanley Tarbell, Vanderbilt University*

I am grateful to the History of Chemistry Division for this award and to the Dexter Chemical Company and Dr. Sidney Edelstein for making it possible. I am deeply indebted to my wife, Ann, without whose collaboration little of my historical writing would have been completed. Some support for our work has come from the National Science Foundation, the Organic Division of the American Chemical Society, and Vanderbilt University.

The history of science, and of chemistry in particular, has developed into a recognized intellectual discipline in the years since 1930, when I entered college. There are a number of academic centers with sizable programs in the field; every established university now tries to have at least one person specializing in this area, usually, although not always, attached to the history department. The HIST division is showing increased vitality, and the accomplishments of Arnold Thackray and his associates in building a vigorous center at Penn for the history of chemistry are familiar to you. A notable number of able young scholars is appearing in the field.

One of the leaders in establishing the history of science in this country was George Sarton, the founder of *Isis* and *Osiris*. Unfortunately, I never heard any of his lectures while I was in college, but I did discover *Isis* and the early volumes of his *Introduction to the History of Science* while poking around in the Widener reading room as an undergraduate (1). I usually read part of *Isis* and, particularly, Sarton's very numerous book reviews. This early interest persisted, and I gradually acquired nearly all of Sarton's published books. James B. Conant, the organic chemist who was president of Harvard from 1933 to 1953, improved Sarton's lot at Harvard very materially; Sarton had hitherto been only a research associate paid by a grant from the Carnegie Institution. Conant gave Sarton a full professorship as well as an honorary degree. Sarton's multivolume *Introduction*, although impressive in its scope and enormous learning, reached only into medieval times, and was in fact an annotated bibliography. His historical skill is better shown in his important two-volume *History of Science*, which unfortunately he was able to carry only to the beginning of the Christian era (2).

The founders of the History of Chemistry Division, among them E. F. Smith, C. A. Browne and F. J. Moore, were all trained as chemists, and their historical studies were an avocation, although Moore wrote a history of chemistry (3). Smith also wrote a number of historical books, but his greatest service was undoubtedly in making his valuable collection (the Smith Collection) of source materials for the history of chemistry. In the terminology of some historians of science, these men were



“internalists”. Their writings required some knowledge of chemistry, were directed primarily to chemists, and showed the internal development of the field by surveying the sequence of ideas and personalities.

The “externalist” historians of science usually have some knowledge of the subject matter of science but are more concerned with placing the science in its social, economic and intellectual context. This classification of “internalist” and “externalist” historians is not clear cut but represents only the extremes of the spectrum.

Internalist history is useful for students and teachers in the field; it is particularly valuable for professional scientists, because many of them do not realize that they are working in a fundamental discipline with a distinguished intellectual and experimental heritage. One of the surprising and gratifying results of our book on American organic chemistry was comment from many people saying that it raised their morale and that of their junior assistants to feel that they were part of something worthwhile and important.

Internalist history can degenerate into antiquarianism, or into the diligent collection of many facts without many organizing ideas. Internalists, like the founders of the History Division, have usually been professional chemists who took up history as a hobby (frequently a full time one).

Externalist historians may have training in a variety of disciplines, such as history, sociology, philosophy. Their writings appeal to other historians and teachers of the history

of science and, unfortunately, to a lesser degree, usually, to practitioners of the science. At its best, externalist history is a valuable contribution to intellectual history, with insights and connections which internalist history may miss. It may play an important role in clarifying and formulating public policy. At its worst, it may become an amiable collection of anecdotes without much solid intellectual content. One of the leading historians of science, C. C. Gillispie, has called for writers to be better informed about the fundamentals of the science they are discussing.

The leading sociological historian of science is undoubtedly R. K. Merton, whose Ph.D. thesis, under Sarton, examined the relation between Puritanism and the development of science in 17th century England (4). Merton has also examined the reward system for scientific work; the "Matthew Effect", which he proposed, can be summed up concisely, if inelegantly, by the sentence "Them as has, gets".

Some externalist historians deal with the economic basis of science and of research support, a topic on which anyone with decades of directing research programs has extensive working knowledge.

A particularly important branch of externalist history is the study of the relation of the federal government to science. As the importance of science and technology grows for the nation, this field becomes ever more significant. Federal support of science is not something new. It goes back at least to the Lewis and Clark Expedition of 1804-1806, sponsored by President Jefferson for scientific as well as for geopolitical purposes. Throughout the 19th century, scientific research by the army, navy, the coast survey, the geological surveys and the Smithsonian were government projects. A recent brilliant book by W. H. Goetzmann of the University of Texas, *New Lands, New Men*, which I commend to all students of 19th century American science, shows that, between 1840 and 1860, Congress supported the publication of 75 reports dealing with scientific exploration (5). Many of these were multivolume and the publications, as well as the expeditions, were expensive. Goetzmann estimates that in some of these years the expense amounted to one third to one quarter of the federal budget. Research on agricultural problems broadly defined was done in the land grant colleges, by state experimental stations, and by federal agencies. This history has been reviewed to 1940 by Hunter Dupree in *Science in the Federal Government* (6), and Don Price has covered the immediate post-war years in *Science and Government* (7). Nathan Reingold, former editor of the papers of Joseph Henry, has also made useful contributions (8).

The role of the federal government in supporting research has of course increased very strikingly since World War II. The examination of its role has therefore become extremely important in recent years.

Some indispensable books, like Aaron Ihde's *The Development of Modern Chemistry*, are difficult to classify as internal or external history (9). Ihde has had experience in studying and

teaching both chemistry and the history of the science, and this book is valuable because of his varied experience.

I would like to mention in more detail two outstanding books, one by a non-scientist, the other by a scientist. The first is *The Discovery of Insulin* by Michael Bliss, Professor of Canadian History at the University of Toronto (10). This fascinating book deals with the most important medical discovery between the advent of antiseptic technique and the discovery of the sulfa drugs. Bliss based his book on much unpublished material, including laboratory notebooks of F. G. Banting and C. H. Best. He received scientific help from a number of people knowledgeable in the life sciences, and his book is a gripping account of the frustrations and triumphs of scientific research. He has a clear understanding of the difficulties of getting reproducible results in isolating and testing biologically active materials. He also has a novelist's talent for describing the development and interaction of personalities, with which the insulin discovery was well supplied.

Suspense is added by the story of the diabetes victims, like Elizabeth Hughes, daughter of the future Chief Justice Charles Evans Hughes. Will enough insulin concentrate become available to save their lives? It did, and Miss Hughes lived, married, had three children, and died of a heart attack 60 years after her first injection of insulin.

Bliss gives a full discussion of the award of the Nobel Prize to Banting and Macleod, an act which aroused endless controversy because C. H. Best and J. B. Collip, both key workers on the problem, could not be honored. This raises the insoluble question, inseparable in any award, of who gets honored and why.

The role of the Eli Lilly Co. was indispensable in making insulin available in sufficient quantity to meet the demand. G. H. A. Clowes of Lilly, noting the first reports of active pancreatic extracts, had followed the Toronto work closely. By enlisting Lilly resources on the isolation of active extracts and eventually of pure insulin, he brought the discovery to clinical usefulness. Lilly's control of the field was a major factor in its growth into a major pharmaceutical company.

Presumably Bliss' book is externalist history, but it is difficult to see how the story could have been told better or more authoritatively by anybody. It is no denigration of his book to point out that it deals with events limited in space and time, even though the results of the insulin discovery were of worldwide significance.

Ernst Mayr's book, *The Growth of Biological Thought*, is internalist history (11). Mayr, the Alexander Agassiz Professor of Zoology Emeritus at Harvard, is one of the leading evolutionary biologists of his time, a fact of which he makes frequent mention. His book is thus based on a detailed knowledge of the field and also on extensive reading on the history of ideas in biology. He started work on it in 1970 when he was 66 and it was published in 1982. His book unfolds against a vast background of the history of the earth and of all

life, from one-celled structures to man.

His volume has an introduction of 147 pages dealing with the nature of biology and an overview of the changing intellectual milieu of biology from antiquity to the present, a summary of the remainder of the book. In the body of the work, he has three main divisions. The first deals with diversity of life, a history of taxonomy and the changing ideas about species and higher taxa. The second division sets forth the history of evolutionary ideas, the leading figure being Charles Darwin, with subsidiary billing to Alfred Russel Wallace, the codiscoverer of evolution through natural selection, and to my mind a more interesting person than Darwin. The third division, on variation and its inheritance, has Gregor Mendel as its hero, with a large supporting cast, among whom is the geneticist Thomas Hunt Morgan, the fruit fly man of Columbia.

Obviously, even if I were qualified, it is impossible for me to review a very long and closely reasoned book like this, written with a clarity of analysis and a vigor which compel respect and even awe. I want to mention, instead, some of the facets of Mayr's thought and give a little of the flavor of his book. Mayr speaks his mind bluntly on many things, believing that making categorical statements "leads more quickly to the ultimate solution of scientific problems than a cautious fence sitting".

There are two themes, almost leit-motivs, which recur constantly in Mayr's discussion. One is the inadequacy of physics as a starting point for understanding biology. "Most physicists seem to take it for granted that ... once one understands physics, one can understand any other science, including biology." Mayr comments on the statement of an eminent physicist that the scientific world view is based on the great discoveries of the 19th century about electricity, heat and the existence of atoms and molecules as follows (p. 33):

... as if Darwin, Bernard, Mendel, and Freud (not to mention hundreds of other biologists) had not made a tremendous contribution to our scientific world view, indeed, perhaps a greater one than the physicists. In biology, one rarely deals with classes of identical entities, but nearly always studies populations consisting of unique individuals. This is true for every level of the hierarchy, from cells to ecosystems ... While entities in the physical sciences, let us say atoms or elementary particles, have constant characteristics, biological entities are characterized by their changeability.

Mayr repeats this point often. It does not mean that one must invoke a vitalistic force to study biological phenomena, but that one must use a population point of view because of the variance of individuals. Mayr does not rule out the possibility that physical and chemical terms may ultimately be sufficient to describe organisms, and this seems very likely to me, although it may take a very long time. As I have written, "structural organic chemistry, with its limited number of basic principles, acts as a bridge between quantum mechanics,

which explains chemical bonding, and the complexities of living systems".

When a well-known Nobel laureate in biochemistry said, "There is only one biology, and it is molecular biology", Mayr comments, "He simply revealed his ignorance and lack of understanding of biology" (p. 33).

The second point which Mayr continually combats is the notion of "essentialism". By this he means the doctrine, due to Plato, that (pp. 38-39):

... the variable world of phenomena ... was nothing but the reflection of a limited number of fixed and unchanging forms, *eide* (as Plato called them) or *essences* as they were called by the Thomists in the Middle Ages. These essences were what is real and important in the world ... Constancy and discontinuity are the points of special emphasis for the essentialists ... Essentialism, with its emphasis on discontinuity, constancy and typical values (typology) dominated the thinking of the western world to a degree that is still not yet fully appreciated by the historians of ideas. Darwin, one of the first thinkers to reject essentialism (at least in part), was not at all understood by the contemporary philosophers (all of whom were essentialists), and his concept of evolution through natural selection was therefore found unacceptable. Genuine change, according to essentialism, is therefore possible only by the saltational origin of new essences. Because evolution as explained by Darwin is by necessity gradual, it is quite incompatible with essentialism. However, the philosophy of essentialism fitted well with the thinking of the physical scientists, whose "classes" consist of identical entities, be they sodium atoms, protons, or pi-mesons.

It can be seen that Mayr's rejection of physical science as a satisfactory approach to biology and of the essentialist philosophy, are very closely related.

Some other aspects of Mayr's thought include his low tolerance for philosophers of science (p. 130-131):

It is still subject to controversy whether or not philosophy has made any contribution to science after 1800. Not surprisingly, philosophers generally tend to answer this question in the affirmative, scientists often in the negative. There is little doubt, however, that the formulation of Darwin's research program was influenced by philosophy ... In recent generations philosophy rather clearly has retreated into metascience, that is, an analysis of scientific methodology, semantics, linguistics, semiotics, and other subjects at the periphery of science.

Externalist science can of course be written from a Marxist standpoint. Presumably all Soviet history of science is based on the Marxist view, and some western historians have used it also. Mayr comments (p. 492):

The Marxist claims that Darwin and Wallace were extending the laissez faire capitalist ethos from society to all nature to make a Weltanschauung out of the new captains' of industry's utopia of

progress through unfettered struggle is not supported by any evidence whatsoever ... Darwin did not live in an ivory tower; he must have seen what went on in England all around him; he read all the relevant literature, and this might have facilitated his acceptance of certain ideas. Yet, if the theory of natural selection were the logical and necessary consequence of the zeitgeist of the industrial revolution, it should have been widely and enthusiastically adopted by Darwin's contemporaries. Actually, just the opposite is true; Darwin's theory was almost universally rejected, indicating that it did not reflect the zeitgeist.

The recent publication of Darwin's diaries allows the development of his thought to be followed in great detail.

Although Mayr disclaims any intention of writing a biographical history of biology, his book contains some memorable sketches of his major figures. Among these are Buffon (who published a 39-volume work on natural history), Cuvier, Larmarck, Mendel, Darwin, Wallace, and many more, including (by implication) himself. After reading 828 pages of text and 30 pages of epilogue in his book, one feels well acquainted with the personality and modes of thought of Ernst Mayr.

The epilogue offers some shrewd comments on sciences and scientists. Mayr emphasizes the "doubts, hesitations, inconsistencies, contradictions, and frequent changes of mind" (p. 830) in the emergence of a finished piece of scientific work, although in the conventional presentations of results the process is made to appear logical and direct. He observes that, with the advantage of hindsight, it is frequently hard to understand why it took so long to reach a seemingly obvious conclusion or to do an obvious experiment. A pertinent example, from the history of chemistry, is the contention of Arthur Michael that addition of bromine to a carbon-carbon double bond is *trans*. He produced some plausible but not conclusive evidence for *trans* addition. It was nearly 25 years (in 1911) before anyone did the conclusive experiment; one which seems obvious now. That year, both Emil Fischer in Germany and Alexander McKenzie in Britain showed that the dibromo acid from (*cis*) maleic acid was resolvable; that is, had no plane of symmetry and therefore must be the result of *trans* addition.

To return to Mayr's epilogue, he has equally wise words on research strategies; one should separate complex problems into components that can be attacked; one should not continue to belabor a point which is already generally accepted. New ideas or discoveries require clear presentation and should not be buried in papers with uninformative titles or which contain highly heterogeneous material. The whole epilogue well repays reading by a scientist in any field.

Not content with the volume at hand, Mayr has written a second volume, which has not yet appeared, dealing with physiology, developmental biology and neurobiology.

Mayr, by writing major books on the history of biology, shows the importance of the history of science in his mind. He

expresses this feeling explicitly in numerous places. The first two sentences of his preface read, "much of modern biology ... cannot be fully understood without a knowledge of the historical background of the problems. Whenever I made this point to my students, they would ask me in what book they should read up on these matters." His book is his answer to this query.

Personally, I feel that some more emphasis on the history of chemistry in the beginning course would make the subject much more attractive to students. Instead, we frequently offer freshmen a dreary melange of topics, without much apparent relevance to their daily lives and with little to arouse their interest.

Students and teachers of science frequently have little appreciation of the long and varied history of the subject, or of careers and personalities of scholars responsible for our present knowledge. Students, in particular, are apt to feel that the science emerged fully grown in the enormous text books which they study. Some sense of the slow development over many decades of the ideas and experimental methods of science might materially enliven their studies. Furthermore, the fact that a science, or any other field of learning, is dynamic, not static, and that important new experimental observations or theories are constantly altering our views is a stimulating and salutary idea that should be emphasized for students. Science and chemistry, in particular, desperately need an informed public view of the nature of the scientific enterprise; a view which the public is not now getting.

All of the varieties of the history of science I have discussed, and others as well, have a real contribution to make to our understanding of science and its place in our culture. I hope that the field will continue to flourish.

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*D. Stanley Tarbell is Professor Emeritus of Chemistry at Vanderbilt University, Nashville, TN, 37235 and is coauthor with his wife, Ann Tracy Tarbell, of "Roger Adams, Scientist and Statesman" and "Essays on the History of Organic Chemistry in the United States".*

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## KASIMIR FAJANS (1887-1975): THE MAN AND HIS WORK

### Part II: America

*Reynold E. Holmen, White Bear Lake, MN*

It was in September of 1936, in the midst of the depression, that I first entered graduate school at the University of Michigan, though for only a year's stay, prior to landing a much needed job. I remember the apparent pride and satisfaction with which Dr. E. H. Kraus, then Professor of Mineralogy and Dean of the College of Letters, Sciences and Arts, told me of the newly arrived Professor Fajans. A study of the chemistry department's staff needs, a study in part induced by Chairman Moses Gomberg's impending retirement, had resulted in several recommendations. Foremost of these was that of securing a leading personage, such as Fajans, to bolster the physical chemistry staff, which some felt was rather inbred. In our analytical chemistry course, Professor Hobart Willard, when introducing the topic of adsorption indicators in argentometry, also reminded us that the man who had founded the field was now a faculty member. Unfortunately, during this first stay at Michigan, I met and talked with Fajans only once and for but a few minutes about the subjects of photochemistry and argentometry.

Fajans quickly made his presence felt. He continually challenged his students to view critically any explanation of chemical phenomena. His 1930 American visit had led him to question some teaching practices common in this country. He could not understand how a student could graduate from high school without having studied physics, a subject he considered even more important than chemistry. His firsthand involvement at Michigan with American chemistry textbooks and teaching practices disturbed him further, particularly after briefly taking over L. L. Brockway's introductory physical chemistry class near the end of World War II. (Brockway was engaged in defense-related research.) He did not readily adjust to this type of classroom task after years of teaching advanced students. The latter were less apt to have a problem with his accented English and were better prepared to benefit from his



Kasimir Fajans, circa 1953

presentation of subject matter; especially when, as in his inorganic chemistry course, the content was fixed by himself rather than being based on someone else's syllabus, thus offering less frequent temptations to digress. In contrast to Brockway's carefully organized and logical presentation of introductory physical chemistry, Fajans' approach was replete with digressions, causing some difficulties for students acquiring their initial foundation in the subject. In spite of this, at least one student, who experienced the lectures of both men, decided that the deeper questions and insights were presented by Fajans (49). Additionally, his office door was always open to any student, freshman or doctoral, having a question or seeking information.

Certain recurrent themes and points of emphasis probably would be recognized by many of Fajans' former students as characteristic of him. Among these were his stress on the importance of the interaction of specific factors in chemical behavior, such as thermodynamics and kinetics, and how our inadequate knowledge of this interaction operates in countless cases to prevent us from making absolute predictions. Typical examples included such statements as:

Our body is unstable thermodynamically relative to CO<sub>2</sub>, but it exists quite comfortably for 70 or more years ... There is no absolute stability, only relative stability ... London in 1927 stated that no oxide of fluorine could exist. The next year one was made ... In chemistry you always borrow and give with respect to energy ... Contrary to valence-bond theory, chemistry does not involve a saturation of a definite number of forces but rather a screening of charges - more or less complete ... Saying that each of two atoms can attain closed electron shells by sharing a pair of electrons is equivalent to saying that husband and wife, by having a total of two dollars in a joint

account and each having six dollars in individual bank accounts, have eight dollars apiece! ... Carbon does not have four valence forces spatially directed to the corners of a tetrahedron ... There are no electron deficient compounds, only deficient theories.

Contrary to the situation in Europe (38), relatively little attention was paid in American chemical circles to such topics as ionic polarization, which had been the focus of much of Fajan's work. The role of these properties in determining the state, reactivity, solubility, and crystal structures of elements and compounds was largely ignored. Instead, "radius ratio" rules were popularly invoked to explain some of the same phenomena, in spite of what Fajans saw as immediate difficulties in the case of such simple compounds as the cesium and thallium halides. Electron pair-octet theory, mesomerism, resonance and hybridization were the common language and supposedly provided a qualitative unification of all chemistry. Textbook authors and investigators, who may have been less grounded than Fajans in fundamentals, often appeared carried away by a not-too-critical enthusiasm for this vocabulary and they often implied a dubiously high degree of quantum mechanical justification for some of their largely qualitative statements. At times warnings against these excesses appeared, such as those by Coulson in 1947 (52), Wheland in 1951 (53), Linnett in 1957 (54), and by Jaffé in 1966 (55). Indeed, the admittedly arbitrary elements of the resonance concept were even noted by its author, Pauling (56). However, these warnings, some mild, some blunt, were largely ignored by many academic and industrial scientists and even by some of their authors.

Most of the initial researches of Fajans and his students at Michigan focused on radiochemistry, the field in which he had first made a name for himself almost a quarter century earlier (57-60). These were facilitated by ready access to the cyclotron deep underground in the physics building, only a block or so distant from the chemistry building, a factor which was important in Fajans' decision to move to Michigan. Shortly prior to this, Michigan's Physics Department had become a summer meeting place for international leaders in theoretical and nuclear physics. In addition, both G. E. Uhlenbeck and S. Goudsmit, formulators of the electron spin hypothesis, had become members of the department.

Fajans was a natural candidate for inclusion in the Manhattan Project, but the fact that he had immediate relatives in Poland ruled this out. In 1940, it also was widely believed that he, like Soddy, Born, Haber, and Hahn before him, would receive the Nobel Prize. However, the prize was discontinued in years between 1940-1942 and, for reasons unknown, he did not receive one later (2c).

The electronic instrumentation introduced into radiochemistry laboratories in the 20 years since his previous involvement was not something with which he had kept abreast, and his interest in the field soon waned (50). Indeed, the last formal

connection for him with the field of radiochemistry was the memorial lecture which he delivered at Columbia University in 1944, on the occasion of the 10th anniversary of the death of Marie Skłodowska Curie (70).

Fajans now gave attention to the subjects which had occupied his Munich years. An initial reinvestigation of adsorption phenomena by means of colorimetry quickly ended, partly because of inadequate instrumentation (50), and he concentrated instead on the study of ionic polarization and its role in determining the structure and properties of molecules and crystals, making use of such diverse properties as optical refractivity and related dispersion phenomena, magnetic susceptibility, solubility, and dielectric polarization (61-64).

It was about five years after Fajans' arrival at Michigan that Theodore Berlin became one of his doctoral students. Berlin was interested in theoretical chemistry and physics and his collaboration with Fajans led to Fajans' last major contribution to physical chemistry, the quanticule theory of chemical binding and structure (65-69).

The term *quanticule* was introduced publicly in papers by Fajans and Berlin at the Spring Meeting of the ACS at Detroit in 1943 (66). It was suggested to Fajans by Berlin as being descriptive of the most characteristic feature of the concept, "A group of electrons quantized with respect to one or more positive charges or cores." Thus, the sodium cation quanticule was represented by the familiar  $\text{Na}^+$  as a valid shorthand for  $(\text{Na}^{1+})1^22^8$ , as was  $\text{Cl}^-$  for the commonly encountered argon-like anion quanticule  $(\text{Cl}^{1-})1^22^83^8$ . The carbon ion quanticule could exist in compounds as  $\text{C}^{4+}$ , as it usually does, or as  $\text{C}^+$ , as in  $(\text{Be}^{2+})_2\text{C}^+$ , depending on its molecular environment. The electrons of a binuclear quanticule were identified by Roman numerals, I, II, as in  $\text{N}_2 = \text{N}^{5+}(\text{I}^2\text{II}^8)\text{N}^{5+}$  (usually abbreviated to  $\text{N}^{5+}e_{10}\text{N}^{5+}$ ), to distinguish them from the familiar Bohr and Hund-Mulliken quantum numerals. The purpose of these numerals was "... to symbolize a discreteness in the structure of the ... system and to associate with ... [these] quantum number[s] average values of electronic energies and internuclear distances." Growing out of Fajans' long study of the polarization and deformation of ions and a comparison of molecules such as  $\text{H}_2$  and  $\text{Li}_2$ , the theory made use of the concepts of continuous versus discontinuous changes in the electron distribution during molecule formation, the latter leading to a change in the quantization of the electron density relative to the positive cores (67).

Berlin (who later became Professor of Physics at Johns Hopkins University and at Rockefeller University) finally overcame referees' objections and published a much delayed but attention-getting paper entitled, "Binding Regions in Diatomic Molecules" in 1951 (69). This dealt with homonuclear and heteronuclear molecules and emphasized the existence of binding and antibinding regions in the space about and between the nuclei. It was an important extension of his work under Fajans. In the process, he also reaffirmed the validity of



the Hellmann-Feynman theorem, which previously had been attacked by Coulson and others.

The most accessible account of quanticule theory was published in German in 1959 in the journal *Chimia* (72). This was also translated into Spanish in 1963, on the occasion of Fajans becoming an honorary member of the Mexican Chemical Society (73). Unfortunately, the most comprehensive account is a book in Polish by Fajans, which was published in 1961 (74). A set of papers, originally presented at the New York Academy of Sciences in December of 1963, included some comparisons with other theories, as well as application of the quanticule approach to some new areas (75).

Between 1945 and 1950, the alternation of properties observed in certain homologous series of carbon compounds was studied by several of Fajans' students and explained on the basis of quanticule theory. The thesis of S. Z. Lewin (76) included references to earlier Fajans students who had also worked on this subject, as well as a good review of nearly four decades of controversy over the existence of alternate polarities in carbon compounds, from the time of H. S. Fry and G. LeBas up to 1949. That year Fajans published an article on the

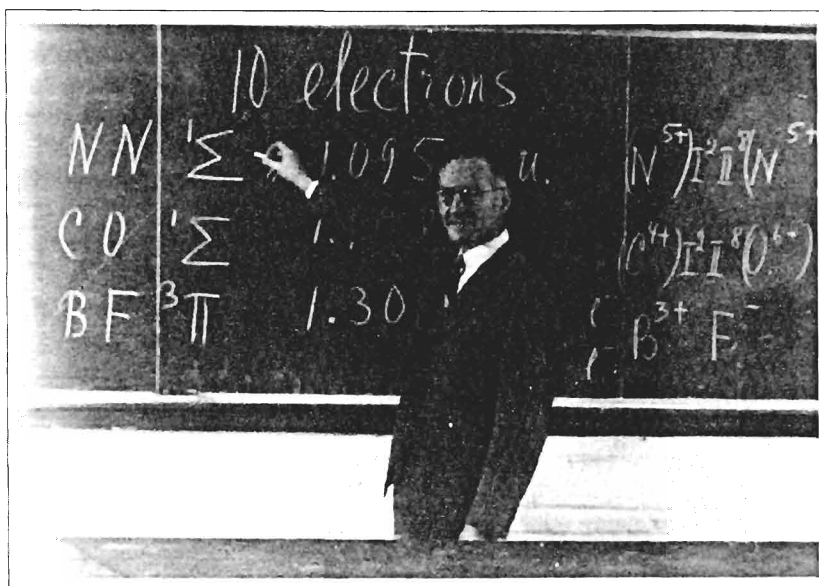
subject summarizing his work in this area (77). Referring to this work, he once remarked that, "Ingold thought that he had abolished alternate polarity, but we revived it."

The years 1945-1960 probably comprise the period of Fajans' greatest influence in this country, largely as a result of his extensive interactions with the industrial chemical community. His collaboration from 1944 - 1947 with the Glass Science Research Foundation, which represented companies in the glass industry, resulted in an article, coauthored with Norbert Kreidl, on the theory of glass formation (78). The impetus for this study came from a growing dissatisfaction with the prevailing Zachariasen network theory on the part of several leading workers in the field of glass science. Wolde-mar Weyl of Pennsylvania State University had noted that the role of lead as a glass former was not consistent with requirements of the Zachariasen theory. The  $Pb^{2+}$  ion is neither highly charged nor small in radius. Weyl suspected that polarizabil-

ity was the key factor and suggested that Fajans be engaged to study this problem. In their paper, Fajans and Kreidl reviewed the problems inherent in the network theory of Zachariasen and stressed the central role of ionic polarizability in developing the necessary viscosity characteristics for a glass. Fajans cited the examples of glycerol and silica, both glass formers but so utterly different in melting point that the "strong forces" called for by the network theory could not be the critical factor. This fruitful collaboration within the glass industry came to an end four years after its inception, for reasons of anti-trust laws.

In 1950 and 1951, Weyl published articles on the subjects of surface chemistry and catalysis, incorporating and expanding on Fajans' concepts (79-81). Weyl, admittedly, at times

wrote for readers ("for executives," Fajans once said) less schooled in science than those to whom Fajans directed his own writing. As a consequence, Fajans was not totally comfortable with some of Weyl's extrapolations, but he greatly appreciated the interest generated in the role of ion polarizability. Weyl and E. C. Marboe again called attention to Fajans' concepts in their multi-volume treatise on the chemistry of glasses (82).



Fajans lecturing on quanticule theory.

Fajans and Stephen Barber, of Owens-Illinois Glass Co., for which Fajans served as consultant from 1948-1955, undertook an investigation of boron oxide glasses. Their results were published in 1952 (83). They again referred to the inadequacy of the network theory in characterizing the states of these glasses at low and moderate temperatures. They proposed a weak molecular structure, gradually changing to a stronger one at higher temperatures. It was suggested that boron oxide behaved in some respects as though it had a structure analogous to that of  $P_4O_{10}$ . Though J. D. McKenzie and others disagreed with this controversial interpretation, Fajans and Barber also found supporters, including N. N. Sobolev and coworkers in Russia (84).

C. L. Babcock, Barber and Fajans also collaborated in examining the data pertaining to vitreous silica. They again discussed inadequacies in the Zachariasen theory and the need for taking polarization effects and the possibility of more than one structure into account (85).

Other kinds of industrial contacts included two weeks of seminars devoted to quanticule theory and its applications, given by Fajans at Shell Development Laboratories in 1953 (86). Following one-day lecture/discussion appearances at the 3M Company in 1952 and in 1956, he presented one week of seminars there in October of 1959, under the auspices of the Inorganic Section of the Central Research Laboratories. By then, three years had then passed since he began his retirement sabbatical at Michigan, where his last doctoral candidate was J. H. La Rochelle (51).

For several years, Eugene Rochow, at Harvard, felt it advantageous to acquaint his first-year students with Fajans' approach to chemical binding, as a worthwhile alternative to the conventional views on bonding. The well-known inorganic text by Fritz Ephraim also incorporated some quanticule descriptions in its later editions (71). In 1959 Thomas Gibb, Jr. presented Fajans' work to the participants at the Tufts Lecture Institute.

The 1951 article by Berlin aroused particular interest on the part of the Canadian theoretician, R. F. W. Bader. A number of publications followed, one of which extended Berlin's work on binding and antibinding regions to polyatomic species and correlated them with the corresponding electron density distributions within the molecules (87-88).

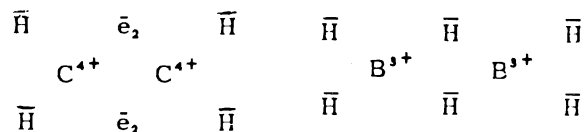
It was in 1970, just before leaving a career in industrial research, that Oliver Johnson, another Fajans' doctoral student, authored the first of a series of papers extending Fajans' concepts to the theory of binding and structure in metals and alloys (89). The resulting "interstitial model" of binding was eventually applied to such topics as heterogeneous catalysis, semiconductors, and superconductivity (91-93). One paper, coauthored with Fajans, used quanticule theory to explain the reason for the difference between  $F_2$  and other halogen molecules (90).

We should not forget that Fajans did not regard his quanticule approach as a sophisticated, quantitative theory of binding and structure, but he was very firm in his belief that it surpassed the then popularly used qualitative concepts of valence bond theory and was more consistent with the fundamental facts of experiment and basic physical theory (including those of quantum mechanics).

In contrast to the open-minded response to Fajans' views in the industrial community, the academic community was largely hostile or, at best, indifferent to quanticule theory. As Hurvic has noted in a recent review of Fajans' later years (2b):

The scientific community for the most part, however, regarded this theory as a great scientist's lapse, as a useless attempt to turn back the course of science. At best the quanticule theory was passed over in silence.

The reasons for this response are complex and involve a combination of scientific, sociological and personal factors.



Quanticule structures for ethene and diborane

Perhaps the major reason for the minimal effect of Fajans' views on his American academic audience was the dominance of organic and organometallic chemistry in American chemical circles and the relative neglect of classical solid-state inorganic chemistry as it was developed in continental Europe after the First World War. This resulted in a feeling that his American colleagues neither appreciated nor understood much of his pioneering work on the subject of ionic polarization. Indeed, the degree of confusion existing in the literature over the nature and extent of Fajans' contributions is vividly depicted through examples discussed in some of his articles (94-95). It is also strikingly evident in a 1962 compilation of over 40 papers, by as many different authors, on the subject of inorganic chemistry (96). Not one of the hundreds of references refers to Fajans' work (106). Even well-intentioned writers sometimes managed to misrepresent his contributions (97) or tried to draw similarities between his approach and some feature of their own, an intended compliment which usually upset him, as he saw it not only as a failure by them to understand his work but as a mixing of his views with those which he considered to be incorrect.

Yet another factor, on a more personal level, was Fajans' admiration for the simplicity of Rutherford's experiments and his statement to Pauling in 1926 that he could not take the time to study the new theory of quantum mechanics. This failure to develop a hands-on, working familiarity with quantum mechanics prevented him from readily putting himself "in the shoes" of its major proponents among chemists. This may have prevented a more constructive interaction on a one-to-one basis with those who were struggling toward a similar goal, albeit on a different track. Commonalities often were overlooked or even avoided, differences were emphasized by both sides, and Fajans admitted having neither time nor energy to follow the "other" literature in later years.

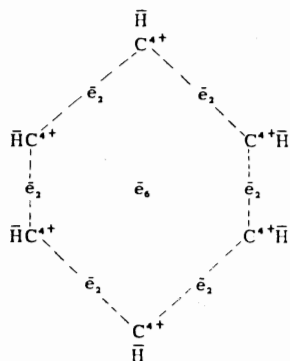
Undoubtedly, however, Fajans' aggressive approach and sometimes bluntly critical remarks to his staff colleagues and professional peers, in contrast to his undiminished consideration for the lowliest inquiring student, bred a growing alienation. This tendency must have been observed even during the Karlsruhe days, at which time he acquired from some of his contemporaries the designation, "The Greatest" (98). Again, at Munich he was approached by representatives of German industry to do some research work for them, work not in his field of expertise. He later related that this was partly because

they told him that at scientific meetings he was "the only one who dared to challenge Walther Nernst" and Nernst certainly was no shrinking violet.

A number of nationally and internationally known scientists, at one time or another over the years, felt the force of Fajans' sharp tongue and pen. On occasion, he was not averse to expressing in public his disagreement with fellow faculty or with visiting lecturers, using words such as, "If you say that, you cannot know chemistry." In 1948, just as Linus Pauling ended the third of three lectures delivered at the University of London, Fajans jumped up, pointed his finger and burst out, "I told you 20 years ago that you couldn't say that" (99). In 1951, A. D. Walsh delivered some lectures at Ann Arbor. In spite of the fact that Walsh went out of his way to acknowledge the work of Fajans, including quanticule theory, as anticipative and representative of much in molecular orbital theory, Fajans became so incensed at Walsh's comingling of the two that he loudly criticized the guest (49). Because of a perceptive blind spot, something which many of us possess in one form or another, it apparently became incomprehensible to him that the recipient of his remarks should view the criticism as anything but an attempt to be helpful. In a manner similar to the incident at Trieste, mentioned earlier, he introduced his paper at the 1950 Gordon Conference on Metals by confronting his peers with the declaration, "There is no such thing as a metallic bond, only a metallic state!"

The extent to which Fajans succeeded in alienating his fellow academicians is further illustrated by an incident which I witnessed at an A.C.S. Meeting in Chicago in the late 1940's. During my brief hallway conversation with another attendee, Wendell Latimer overheard me mention Fajans' name and immediately began swearing.

Fajans' encounters with American journal editors were another source of irritation. Sometimes the best efforts of those who tried to mediate failed completely. The problems were not entirely one-sided. There were some editors, referees and reviewers who either did not understand Fajans' concepts or were reluctant to let his views rock the boat. Even Berlin's key



Quanticule structure for benzene

article in 1951 met with several rejections before finally appearing. A. B. Lamb, the former editor of *JACS*, once had to intervene on behalf of Fajans in one of the publication controversies, declaring that Fajans deserved to be heard, regardless of whether one agreed or disagreed with his views.

On the other side of the coin was Fajans' frequently unbending attitude about form, length, and content. William Kieffer, during his editorship of the *Journal of Chemical Education*, was once interested in publishing a somewhat shorter English translation of Fajans' long German article, which had appeared in *Chimia*. This excellent opportunity for Fajans to gain a wider exposure for his views was lost when he adamantly refused any abbreviation.

Fajans' involvement in reviewing newly published books increasingly developed into contests between him, the editor and the referees. One of these exchanges, over 25 years ago, became a nearly three-year marathon. The related correspondence file reveals a classic example of good, perhaps misplaced, intentions gone awry. A change in editorship was followed by the new editor's move to forego what seemed to Fajans a prior editor's commitment. Additionally, there was Fajans' insistence that the review be published in a form not only bluntly criticizing items in the author's book, but also increasingly promoting Fajans' own views (100). Protests by his family against this insistence were of no avail.

In evaluating this sparring between editor and writer, one must realize that German journals of the pre-World War II era were very tolerant of polemical exchanges. An example is the attack by Hantzsch et al. of Fajans' published doubts about the purity of the compounds used and, therefore, the validity of conclusions reported previously by Hantzsch on the refractivity of electrolytes. Hantzsch finally published an admission of the superiority of the work by Fajans (101).

In 1959 I wrote to T. Berlin to inquire about the poor academic acceptance of Fajans' views and, in particular, the poor acceptance of quanticule theory. In his reply, among other things, Berlin mentioned three factors, all of which are in keeping with the above:

... the way in which Dr. Fajans expressed his opinions ... a general naiveté on the part of chemists ... the general desire by most scientists - chemists and physicists - for recipes.

It was ten years after Fajans' arrival in Ann Arbor and my brief first conversation with him that I made a belated return to graduate school. I was told by an advisor to avoid his inorganic chemistry course, described as so much propaganda. A week later I bowed to curiosity and heeded the earlier advice of Marvin Carmack, then a chemistry professor at the University of Pennsylvania, that I should be sure to avail myself of the opportunity to take a course from Fajans. I dropped one course and enrolled instead in Fajans'. I would be much the poorer for not having done so. It was a stimulating course! It introduced

to me points of view sorely needed to sharpen my own thinking. Unforeseen by me then, that experience would influence the course of my career a decade later.

For about two years I observed one of Fajans' very capable departmental colleagues engage in some highly emotional disagreements with him. Over ten years later, that same colleague told me how he had arrived fully expecting to "cut the props" from under Fajans and at times "wanted to slug him". Instead, after finally controlling his own anger, he decided to benefit from his contact with Fajans and acquired a great respect for the man's knowledge and insight. Another initially outstanding colleague fared less well. He did not have the stamina to stand up to Fajans' attacks, with the result that his productivity progressively diminished.

Sadly, as the time for retirement approached, Fajans' presence in the physical chemistry seminars became increasingly disruptive. He was apt to interrupt and question the speaker at any point. If asked by the presiding staff member to kindly refrain from interrupting the speaker, he was known to reply, "If I had wanted to be polite, I would have gone to a party" (51). His very aggressive criticisms stifled free discussion, with the result that the seminars concluded in relative silence. Even as late as 1961, after Fajans had ceased to attend, the "trauma" persisted in the physical chemistry seminars, to the puzzlement of a newly returned alumnus, and it took a while to restore a normal, stimulating discussion (49).

Fajans' seemingly growing obsession with any aspect of what he sometimes called "the Pauling evangelism" produced a rather humorous incident which I witnessed in 1952. He had just delivered an invited seminar lecture before chemistry faculty and students at the University of Minnesota. After the lecture he stopped to visit briefly with his hosts, Professors Bryce Crawford and William Lipscomb, in Crawford's office. During this visit a sales agent for Freeman, the publisher of Pauling's *General Chemistry* and his later *College Chemistry*, innocently dropped in. Introductions were made, and Fajans immediately welcomed the sales agent with, "I see from your literature that fewer schools are using Pauling's new *College Chemistry*." In spite of this, Fajans valued Pauling's crystallographic studies.

The fact that he, unlike many of his fellow workers in radiochemistry, was not given a Nobel Prize, also probably contributed to Fajans' growing dissatisfaction over the years. Perhaps even more disturbing to him was the award to Odd Hassel, who, for his work in elucidating the structure of intermolecular complexes by means of x-ray diffraction, shared the Nobel Prize in Chemistry with D. H. Barton in 1969. Hassel had been an assistant in Fajans' laboratory in Munich, and was involved there in the study of adsorption indicators (42), but evidently was not considered by Fajans to be one of his outstanding students (49). Of course, neither was Fajans comforted when the award was given to Pauling, with whom he had so often disagreed on the theory of chemical binding and

structure.

Unfortunately, two of the most theoretically inclined of his former students, Norman Bauer and Theodore Berlin, both of whom held university teaching positions, met with untimely deaths too early in their careers. The loss of these men must have been a real blow to Fajans and to the further development of quanticule theory. Both men had continued to collaborate with him after graduation and, undoubtedly, could have done much to improve his interactions with the rest of the academic community.

On the other hand, the outcome of the Hantzsch incident provides an example of Fajans' very commendable professional traits. He demanded experimental work of the highest order, work which would not compromise the resulting conclusions. To his students he would cite examples of data published by respected investigators, and supposedly presenting numerical values of high accuracy, but which were in fact faulted by the use of impure compounds. He ceaselessly sought for more reliable sources of critical experimental data needed to test his conclusions and, when the data contradicted them, he was willing to bow to the experimental evidence. He did so in the case of his predictions about the structures of some of the higher boranes, and also when the use of a model other than his own proved more effective in calculating the dissociation energies of the diatomic halogens (102). His sense of humor extended to his remarks about his rejection of published data which he considered to be questionable. "You see what a bad character I have become - I say the literature is no good; but this is the reason they invited me [to participate]," he would remark knowingly, smiling slyly. His knowledge of the literature pertaining to his research interests was in fact encyclopedic, much to the consternation, even embarrassment at times, of those who expected to find him less aware than they.

Fajans continued teaching Chemistry 283, a review of physical chemistry for graduate students, through the 1955-1956 academic year (103). The "Kasimir Fajans Award" for the outstanding doctoral thesis in chemistry was established at the University of Michigan in 1956 in his honor. After the usual year of retirement sabbatical, Fajans became Professor Emeritus in 1957. As already noted, he continued to actively correspond and write articles about ionic polarization and quanticule theory until the early 1970's. His wry humor did not depart (2c). Deteriorating heart and kidney functions finally led to his death on 18 May 1975. His wife, Salomea, who had played the role of a much needed balance wheel for her more impetuous husband, maintained contact with a number of his former students and friends until her own death in 1982.

Surviving the parents are the two sons, Edgar and Stefan, to whom we referred in Part I. Edgar Fajans retired a number of years ago. He had earlier moved to the United States from England to become Director of Research at American Potash, after prior careers with its "parent", British Borax, and with Imperial Chemical Industries (107). Stefan Fajans secured his

M.D. degree at Michigan and later joined the staff there, where he became Professor of Internal Medicine and Chief of the Division of Endocrinology and Metabolism, specializing in the study of diabetes. He became Professor Emeritus in 1988.

Recently I was startled to have a former colleague, who specializes in the study of the swelling of polymers, inform me that an editor's remarks had prodded him into reexamining Fajans' studies on solubility and alternating polarity. In late 1989 I began to sense a slow awakening of interest in Fajans' work which hopefully will lead to a more positive evaluation of his often keen insights into the nature of chemical binding and structure. The tragedy of this long delay was noted by the Danish theoretical chemist, C. K. Joergensen, when he wrote in 1962, "In a sense it is a pity that the quanticule concept did not [in 20 years] achieve a greater influence on chemists' thinking" (104).

A final incident conveys something of the impact which Fajans had, despite the problems mentioned, on those who came into contact with him. In 1960, a distinguished British professor, who had just completed his invited lecture and discussion period at an American university, was climbing the steps leading out of the auditorium with his host, a well-known physical chemist and dean, when he turned to his host and asked, "What do you think of Fajans and his views?" The answer rang back, "Fajans either is a genius or one of the world's greatest charlatans - and he is no charlatan!" (105).

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99. L. Pauling, letter to R. E. H., 30 May 1984.
100. For examples, see Fajans' reviews of: (a) Y. K. Syrkin and M. E. Dyatkina, *Structure of Molecules and the Chemical Bond*, translated and revised by M. A. Partridge and D. O. Jordan, Interscience Publishers, New York and London, 1950, appearing, along with that of the original Russian edition, in *J. Am. Chem. Soc.*, **1950**, *72*, 4335-4337. (b) L. Pauling, *General Chemistry*, W. H. Freeman and Co., San Francisco, 1947, appearing in *J. Phys. Chem.*, **1951**, *55*, 1107-1108. (Mention also is made of Pauling's *College Chemistry*, same publisher, 1950.) (c) His scheduled review of E. S. Gould's *Inorganic Reactions and Structure*, Holt, Rinehart, and Winston, rev. ed., 1962, to all appearances, ended in an impasse between editor and reviewer.
101. See successively: A. Hantsch and F. Durigen, "Chemical Changes of Acids and Salts in Solution Based on Refractometric Data. I.", *Physik. Chem.*, **1929**, Abt. A, *144*, 147-157; K. Fajans and M-L. Gressmann, "Refractometric Investigations. XIII. Refractometric Methods and the Relation of Concentration to Refraction of Perchloric Acid", **1930**, Abt. A, *146*, 309-313; and F. Durigen and A. Hantsch, "Remarks on the Preceding Work of K. Fajans and Marie-Louise Gressmann", *Z. Physik. Chem.*, **1930**, Abt. A, *146*, 314.
102. O. Johnson, letter to R. E. H., 16 November 1987 regarding the model for halogens.
103. R. C. Taylor, from one who was a student in that class, phone conversation with R. E. H., 12 December 1989.
104. C. K. Joergensen, "Chemical Bonding Inferred from Visible and Ultraviolet Absorption Spectra", in F. Seitz and D. Turnbull, eds., *Solid State Physics*, Academic Press, New York, **1962**, *13*, 461.
105. Conversation overheard by R. E. Holmen.
106. In striking contrast to this neglect, his 1942 paper (62) is cited 30 times, far more than any other by him cited during the period 1974-

1989.

107. Before going to press we were informed that Edgar Fajans passed away (August 1990).

\* **Addendum:** While Part II was being revised, the Editor, W. B. Jensen, advised me of two references new to me. The first is to the significance of Fajans' earliest research (that which won him the Victor Meyer Prize). In 1925, Arthur R. Cushny, Professor of Pharmacology and Materia Medica at the University of Edinburgh, gave the third series of The Charles E. Dohme Memorial Lectures. Earlier he had taught at Universities of Michigan and London. This lecture was published under the title, *Biological Relations of Optically Isomeric Substances*, Williams and Wilkins, Baltimore, 1926. Cushny therein refers to the importance of Fajans' early work in answering some questions raised by Pasteur regarding the action of enzymes and in showing that their stereospecificity in a chemical reaction can be matched by an asymmetric molecule (see especially pp. 10-13, 20-21). Reference to the more complete publication of Fajans' thesis research, "Selective Action of Catalysts on Stereoisomers and Optical Activation by Asymmetric Catalysis", *Z. Physik. Chem.*, **1910**, *73*, 25-96 (German), should have been included with Ref. 3 in Part I of this article. I apologize for citing only the preliminary one. The second, J. Bigeleisen, "Chemistry of Isotopes", *Science*, **1965**, *147*, 463-471, p. 463, credits Fajans with being the first to recognize that the isotopes of an element, although not chemically separable, should exhibit differences in those properties which depend on "the frequencies of atomic and molecular vibrations".

\* **Errata:** In reference 44, p. 23 of Part I, the date of the English translation should be 1938, not 1928. The last two references, same page, are numbered 46 and 47; they should be 47 and 48, respectively.

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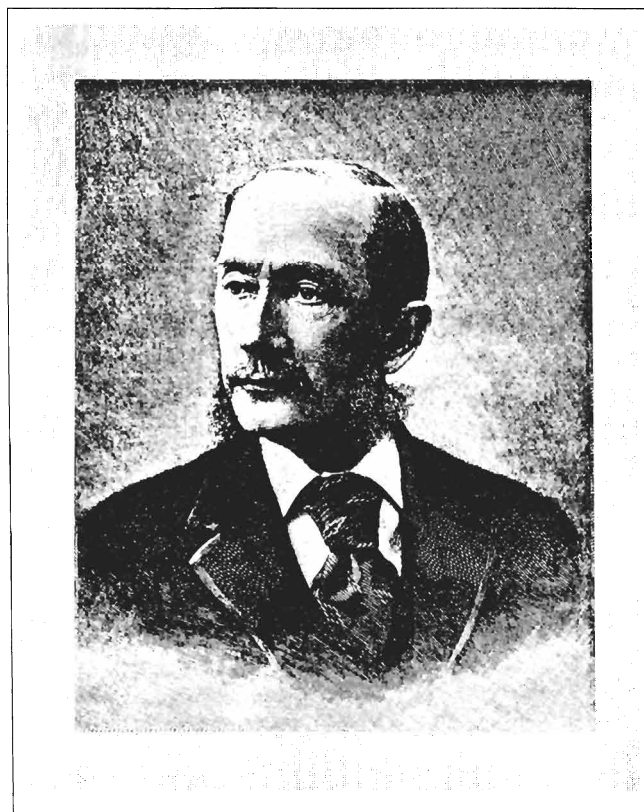
*Dr. Reynold E. Holmen, 2225 Lilac Lane, White Bear Lake, MN 55110, is retired from the 3M Company, where he was employed as an organic chemist. He received his Ph.D. from the University of Michigan, where he had the stimulating experience of taking several courses from Fajans. Part I of this article appeared in the Fall 1989 issue of the Bulletin.*

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## THE CONTINENTAL CHEMICAL SOCIETY

*James J. Bohning, Beckman Center for the History of Chemistry*

Chemists of the U.S. ought to have something better than the Chemical Section of the American Association, the publications of which are next to nil. They ought to have something better than membership in the American Institute of Mining Engineers. They ought to have a national society, including all working chemists in the country -



Frank Wigglesworth Clarke

including the teachers. The American Chemical Society (of New York) would not do, even for a nucleus. It seems to be sort of a Pickwick Club, a joke (1, 2).

Such were the sentiments of William Glenn of Baltimore as he wrote to Frank W. Clarke in Washington, D.C., on 21 June 1890 (3, 4). Glenn's letter was just one of about 100 that Clarke and Harvey W. Wiley received during the summer months from a wide spectrum of chemists in the United States. This spurt of activity was prompted by a circular calling for the formation of the Continental Chemical Society (CCS) and mailed "to the Chemists of America" in early June.

As co-authors, Wiley and Clarke were acting on behalf of the Chemical Society of Washington (CSW), the Chemical Section C of the American Association for the Advancement of Science (AAAS), and the Association of Official Agricultural Chemists (AOAC). Their proposal was brief, attractive in its simplicity, and appeared to provide an obvious course of action. The plan was "to organize a Continental Chemical Society, representative of all North America, by affiliating together as far as possible all existing local organizations. The Society as a whole to hold an annual meeting at such time and place as may be agreed upon from year to year; while local

sections, like sections of the British Society for Chemical Industry, shall have their regular frequent gatherings in as many scientific centers as possible; all publishing their work in one official journal." The broadside recipients were asked to "kindly state whether you regard the project favorably, and if modifications or objections occur to you, will you formulate them? Do you favor a society at all? Do you favor the idea of local sections? Do you favor the publication of a journal?" (3).

Ironically, Clarke publicly opposed the formation of a national chemical society when Persifor Frazer suggested the

idea at the Priestley House in 1874 (5). Even after the American Chemical Society (ACS) was organized, Clarke refused to participate in its affairs. Instead, his efforts were directed to the AAAS, where he was instrumental in the formation of the Permanent Subsection of Chemistry of Section A in 1874, and where he served as an officer and the author of a large number of papers presented at the annual meetings. Fifteen years later, with the number of chemically related organizations increasing, and the

organizational affairs of chemistry in disarray, Clarke succeeded Albert P. Prescott as chairman of a Committee on a National Chemical Society. Prescott, from the University of Michigan, had presented a report to Section C of the AAAS in August of 1889 explaining the desirability of a truly national chemical organization (6). With Wiley's assistance, Clarke obtained unanimous approval of the national society concept from both the CSW and the AOAC. Being careful to exclude the CSW members from the mailing list to avoid charges of bias, Clarke "widely distributed" the circular to obtain more support at the national level (7). It is clear that the intent was

to make Section C of the AAAS serve as the nucleus for the new organization, and the final report for implementation of the project was to be presented to the AAAS during the annual meeting at Indianapolis in August, 1890 (8).

The response to all of Clarke's suggestions was generally quite favorable. A typical reply came from C. J. Reed in the Laboratory of Thomas A. Edison. Acknowledging receipt of "your circular letter of recent date, proposing the organization of a Continental Chemical Society," Reed continued "that the project meets my hearty approval. I would favor 1) the idea

of local sections, 2) the publication of a journal, 3) annual meetings of the whole society." Sixty-nine other letters, most of them equally terse, a few even written in the margin of the original circular, contained similar comments. Twenty-three others, however, took the time to develop more detailed ideas. The diversity of their remarks reflects not only the heterogeneity of the sample obtained from the poll, but also is a measure of the state of Ameri-

## CONTINENTAL CHEMICAL SOCIETY.

### TO THE CHEMISTS OF AMERICA.

The undersigned, representing committees appointed by the Chemical Society of Washington, the Chemical Section of the American Association for the Advancement of Science, and the Association of Official Agricultural Chemists respectfully submit the following statement:

During the past two years the formation of a National, or rather Continental Chemical Society has been much discussed. A committee, of which Prof. A. B. Prescott was chairman, presented a report upon the subject at the last meeting of the American Association, and that report was in the main favorable. A new committee, however, was appointed to secure fuller information, and will report at the next meeting of the Association, in August 1890, at Indianapolis. A larger attendance of Chemists is there expected.

The plan which has so far been chiefly considered is, in brief, as follows:

To organize a Continental Chemical Society, representative of all North America, by affiliating together as far as possible existing local organizations. The Society as a whole to hold an annual meeting at such time and place as may be agreed upon from year to year; while local sections, like the sections of the British Society for Chemical Industry, shall have their regular, frequent gatherings in as many scientific centres as possible; all publishing their work in one official journal.

Bearing this rough outline in view will you kindly state whether you regard the project favorably, and if modifications or objections occur to you, will you formulate them? Do you favor a society at all? Do you favor the idea of local sections? Do you favor the publication of a journal?

Upon the basis of the replies to this circular the committees named in it will prepare their reports to the organizations which they represent. Other existing societies, having appointed similar committees, may take action independently; if so, their views will be considered also, as it is desirable to secure the fullest co-operation among the chemists of America. Complete unity of action is essential to success.

Very respectfully,

F. W. CLARKE.  
H. W. WILEY.

Please address replies to Prof. F. W. Clarke, U. S. Geological Survey, Washington, D. C.

Circular calling for the formation of the Continental Chemical Society, June 1890

can chemistry in 1890.

While not claiming any priority, Thomas H. Norton (University of Cincinnati) gently reminded Clarke "that in 1884 when at Washington I broached the idea to you, but at that time you rather doubted its feasibility." Some were concerned about the name of the new organization. Winthrop E. Stone (Purdue) felt that the name "American Chemical Society" would be more definitive since "we are accustomed to seeing [Continental] used as designating Europe in distinction from England." E.H.S. Bailey (University of Kansas) wondered if "the name American [is] monopolized by the N. Y. Society, so



that we cannot use it?" More directly, Henry Trimble (Philadelphia College of Pharmacy) simply did "not like the name 'Continental', but thought it should be 'American' although such a name could only be adopted by an arrangement with the New York American Chemical Society... The name 'North American' might be used, or the 'United States Chemical Society' which would exclude Canada - a small loss." This dilemma was solved by M. F. Edwards (University of Michigan) who called the project an "American Continental Chemical Society."

Separated by their geography as well as their specialties, most of the chemists had strong feelings about local sections. Divisions "drawn on geographical or on technical grounds" were equally acceptable to Thomas Robinson (Fort Mitchell, Alabama) who could envision either an Eastern Section headquartered at Boston, Montreal or Halifax or sections of Agricultural, Mining, and Biological Chemists. Supporting this view, Alfred E. Hunt (Pittsburgh Testing Laboratory) considered "the idea of the local sections to be a good one; but think that there are at present chemical society organizations, organizations of engineers' societies in which the chemists have sections, or could form sections, that this should represent the local sections." However, Davenport Fisher of Milwaukee felt "so out of the way of intercourse with my fellow chemists that I hardly know how practicable and useful such a society could be." Similarly, F. W. Woll (University of Wisconsin Agricultural Experiment Station) argued that "local sections may be advantageously formed in the East, where a considerable membership of chemists are gathered within a comparatively small area, but I doubt if they would be a success anywhere else, and I should rather see a good strong meeting of the whole society once every year than more frequent weaklings of sectional meetings which cannot help taking away the interest from the mother society." W. P. Cutter (Utah Agricultural College) agreed that his "isolated position would make it practically impossible to attend any such section," yet recognized that "the formation of such local organizations would be the only way to prevent the society from becoming a mere publication firm...".

J. T. Willard (Kansas State Agricultural College) predicted that "not over six or eight local sections could be organized," even though "these would probably embrace the best men of the profession in this country." Willard also presumed it "unnecessary to point out the conditions obtaining here and in Great Britain are vastly different, the area of the latter being scarcely more than that of Kansas. Successful annual meetings which can be attended by a large majority of the members can thus be held there, while here many of us would not be able to attend once in five years." More prophetically, Bennet F. Davenport suggested that "the American Chemical Society at New York City and the Washington, D.C. [Society] could form sections, and we could have one here at Boston." However, John Ordway (Tulane) did not think a local section in Boston

would "flourish, according to past experience, but if the pharmacists could be interested, they might help as they are getting better educated than they used to be." Conflict with the Scientific Association at Johns Hopkins caused William Glenn to doubt the success of a local section at Baltimore, since it "could not live ... unless led by the Hopkins men; it is doubtful if they could find time and inclination to lead it."

In spite of their various approaches to the implementation of local sections, most respondents were in accord with Charles D. Woods (The Storrs School Agricultural Experiment Station) who found "the way in which chemical publications in this country are scattered is to be deplored." Walker Bowman (Virginia Agricultural and Mechanical College) saw the proposed journal merging "the chemical work now appearing in so many different journals in so many different parts of the country." "A journal in which all papers will have a right to appear and not by abstract only" was anticipated by F. B. Dancy (Raleigh, NC). F. E. Engelhardt (Syracuse, NY) looked for an official journal in which American chemists could publish their work, "instead of a dozen or so which is now the case, and quite expensive for a chemist of small means when he takes European journals at the same time." Finances were also the concern of H. J. Patterson (Maryland Agricultural Experiment Station) who did "not think it would be wise to make the subscription to the journal exceed \$5.00 per year."

Quality was the chief concern of Alfred M. Peter (Kentucky Agricultural Experiment Station) who proposed to "purchase or combine" with an established journal to make "the journal the best chemical journal published on the Continent." According to W. Simon (Maryland College of Pharmacy), "it should not be undertaken before a number of prominent writers have promised to make this journal the chief means for publishing and exchanging views." Purdue's Stone complained that "at present publication in American journals is often insufficient to secure a permanent record in the literature." Perhaps this was because Albert M. Todd (Nottawa, MI) believed that many "journals are conducted largely for the purpose of gain; their editorials column as well, being subservient to the interests of gain rather than scientific accuracy and the public good." Peter T. Austin (Rutgers) warned that "the trouble with a journal is that there is not enough work done in this country to fill another. The *American Chemical Journal* might be assimilated, otherwise we should not try to get up another. To devote a large portion of another journal to abstracts would be an outrage on us, for our libraries are now crammed with five or six abstracts of every article that comes out."

Several writers addressed items that were not specifically mentioned in the circular. Based on his experience as a student at the University of Freiburg, Edgar Everhart (University of Texas) thought of "admitting to membership advanced students of chemistry" since "in this way the subscription list would be considerably increased and a livelier interest and enthusiasm in chemistry would be given young men." Simi-

larly, John Eyermans (Easton, PA) wanted to elect "fellows to be composed of college instructors and others doing original work" and "also a second class to be termed associates to include all other classes of chemists." For Charles S. Parsons (Dartmouth) there was a much different concern. "While I recognize the value and beauty of organic chemistry" he wrote, "and wish to keep up with it as far as I can, still I hope that if a Continental Chemical Society is founded it will not make organic chemistry too prominent ... to the exclusion of other branches." Thomas Robinson enthusiastically wanted to "go further and make [the CCS] a section of a world-wide Chemical Society."

The details of organizational problems will evoke as many solutions as there are individuals to propose them. Writing from the Fellowcraft Club in New York City, Marcus Benjamin examined the overall concept of a new society as well. Pointing out that Clarke had copies of his editorials in the *Engineering and Mining Journal*, which advocated a similar plan, Benjamin expressed "doubt that [Charles E.] Munroe's idea of reviving the American Chemical Society by infusing new life into that body" would succeed. Instead, "I believe in a new start with new machinery, officers, etc., and then let the ACS come in... Years ago I proposed that the ACS become a local section of the Society of Chemical Industry, but I was hooted at. In consequence, I resigned and have been \$5.00 ahead each year since." Conversely, J. W. Mallet (University of Virginia) confessed "that I retain an Old World tendency to build upon foundations already laid by others, rather than sweep away the past and set up something entirely new." Others argued for a rational amalgamation of existing societies. It seemed important to A. H. Hunt that the proposed CCS should "use every effort and endeavor, not so much to form new societies as to combine and collate the work of the organizations already formed." A consulting chemist, James B. MacKintosh (New York) thought that "if we could get a society wide enough to embrace the coal tar color men at one end and the mineral chemists at the other with representation both of theory and practice ... we should be able to form a society to be proud of." From the United States Patent Office, William N. Seaman preferred "giving greater autonomy to the sections [of the AAAS], and whether this be done by making the Association a confederation of partially independent societies or by enlarging the powers of the sections ... is ... a question of expediency."

Yet State Assayer S. P. Sharples (Boston) noted that the "AAAS is largely made up of those who are interested as teachers or in theoretical work" while "the chemical side of the Engineering Society at present is a very strong one embracing most of the chemists who are interested in metallurgy and commercial work." Since those "engaged in everyday conflict ... are after results rather than theories" while the teachers are "furnished with abundant material" for their theoretical work from those needs, the new society would serve as a "common

ground" for both groups to meet. However, a group headed by Louis M. Norton (MIT) preferred to have the society "devote its strength mainly to applied chemistry rather than to pure chemistry", looking to the Society of Chemical Industry rather than the German Chemical Society for an example. Citing the Journal of the CCS as the crucial step in implementing the new society, George H. Masson (Trinidad) tied the rise of the professional status of the members "with the progress of the Society, which if worked with earnestness and concord would soon become an association of recognized importance among the scientific institutions of the world." "Organization is the spirit of our time" wrote Charles Munroe (Newport, RI) philosophically, for "it gives force to action and effects an economy in effort while the strength which follows a well matured union of interests is axiomatic and is the fundamental idea of the government under which we live and of the institutions with which we are surrounded. I cannot for my part conceive of any reason ... why every chemist in the country should not wish to become a member of such an organization."

Not everyone shared such optimism. An emotional response came from C. Loving Jackson (Cambridge, MA) who was "opposed to any chemical society on the grounds that the country is too large for meetings, and I do not see that it would be of any advantage to chemistry." Incredulously, Jackson also saw "no field" for a journal, stating that "anyone who does anything toward multiplying the sources of scientific information deserves the curses ... of all scientific men." In a more subdued tone, C. F. Maberry (Case School) concluded that from the "opinions I have heard discussed, I am convinced that this [CCS] is quite out of the question. Such an attempt would receive little help from the men who are carrying on the larger portion of original research in this country. Without such support the scheme would evidently be hopeless." Toxicologist E. H. Bartley (Long Island College) was confident that Clarke did "not expect much support from the members of the ACS." After all, "this society is now in the field as an American Society with members all over the country. Is it likely that another will do better? There are more chemists in and about N.Y. than in any other location in America. Why change the headquarters for such an association when there are but a handful of chemists? To make all of the existing societies subsections would ... aid in building up the Washington or some other Society."

Pennsylvania Railroad Company chemist Charles B. Dudley (Altoona, PA) lamented that "I have done all I could thus far to help on the society whose headquarters are in New York, having constantly paid my dues, although I felt that very poor return was given for the money, since the New York Society has not been as efficient as could be desired." Convinced that "there is not room for two large societies," Dudley contended that "the question turns more on what can be done with the New York Society than on any other single consideration". While merging this society into the Continental was a possibility, he

concluded that "perhaps better still the American Chemical Society could be made the basis of the Continental." Support of the ACS also came from William McMurtrie (New York Tartar Co.) who could "scarcely see the necessity for another and independent organization until absolute or at least practical failure of this [ACS] society has been demonstrated." The ACS "is an active operation, is thoroughly organized, has a good membership of men of high standing, [and] is making an endeavor to extend its usefulness along exactly the lines proposed in this circular."

The longest defense of the ACS came from Charles A. Doremus (City College of New York) who elaborated on eight reasons why the ACS "shall be continued and that whatever may be deemed faulty in its constitution shall be modified to harmonize all existing views." Doremus hinted that "if in its [ACS] administration some, even of the most prominent chemists, may have felt aggrieved because perhaps their names may not have figured in high places, you may rest assured that the same difficulties ... will exist in any new organization. What guarantee is there of any new society being more successful? Are present prospects higher than those under which the ACS was born?" Doremus trusted "that at the American Association meeting there will be no evidence of sectionalizing or petty jealousy, but that a truly liberal spirit will prevail all deliberations on this subject." The eight-page letter dated 1 July evoked an Independence Day reply from Clarke. Six days later Doremus responded again, this time constructing a detailed scenario for implementing Clarke's scheme by "starting with the charter of the ACS." In rebuttal of Clarke's specific comments, Doremus pointed out that "men such as Dr. Bolton never entered the ACS because of dissension at the time of its founding," and "the meetings of the ACS have been held in N.Y. ... owing to the general apathy of the profession." Sensitive to individual interactions, Doremus wrote "strongly, ... for I should much regret to have our foreign professional brothers witness any inharmonious factions arising out of this movement."

It was Charles F. Chandler, however, serving as ACS President for 1889, who described the situation succinctly and candidly. He considered (9):

... it a very grave mistake to organize another chemical society of a general character. We already have an American Chemical Society, and we have recently modified the constitution so as to meet all the requirements, and I think it would be a very serious mistake to create another one. The present American Chemical Society will not be discontinued under any circumstances, and it seems a great pity for the chemists of the country to scatter their fire. The term "American Chemical Society" is just as continental as a matter of fact. What you say in your circular about a society similar to the British Society of Chemical Industry is met completely by what has been recently done in changing the constitution of the American Chemical Society. ... I would say, therefore, that in my opinion, it would be a fatal mistake

to establish the society that is proposed.

Chandler's caustic comments were not meant to be taken casually. He had watched the organization he founded in 1876 decline in membership as financial difficulties and journal publication problems increased (10). In addition, the growing dissatisfaction of chemists outside of New York resulted in the formation of other societies such as the CSW, founded by Clarke and Wiley in 1884. In spite of a dismal situation there was no real public concern about the ACS future until news of the AAAS meeting in Toronto during August of 1888 reached New York. "At the [7] December [1888] meeting the subject was discussed in the light of some vague reports, ... and there was a certain irritation expressed at the possibility of a new organization being formed without consultation with this Society, to assure the name which it has born." As a result, Chandler "was appointed ... Chairman of a Committee in considering any further developments of the subject" (11).

In early 1889 Prescott had begun gathering information for the AAAS on the feasibility of organizing a national chemical society (12). Among those responding to his inquiries were A. R. Leeds, J. P. Cooke, J. W. Mallet, W. A. Noyes, and Ira Remsen (13). While objecting to the proviso "that appointees of the AAAS remain a majority of the voting members" on a formal organizational committee, A. A. Breneman (editor of *JACS*) was willing "to make any sacrifice that would secure a better organization of American Chemists" (11). Worrying about how "to respect the organization and the vested rights of the ACS", he was optimistic "that your own name [e.g. Prescott] at the head of the committee is a guarantee of caution and judgment in the direction of the new movement, and for that reason wish it every success."

Writing to Prescott on 20 May 1889, Chandler saw "no difficulty in the way of accomplishing this most desirable end, provided everybody concerned is desirous of seeing it accomplished". Not surprisingly, Chandler thought "the best plan would be for all the chemists in the country to join the ACS" and proceed to reorganize the ACS with an annual meeting, local sections, and a journal. He was very specific that the journal issue was the most important matter of all. Suggesting that the *Journal of the American Chemical Society*, Hart's *Journal of Analytical Chemistry*, and Remsen's *American Chemical Journal* should be combined, Chandler was willing to give Remsen every concession to become editor. Using an unusual descriptor, Chandler assured Prescott "that the members of the New York Chemical Society (the American Chemical Society) would be willing to do everything that is reasonable in order to bring about so desirable a result" (3,14).

Almost immediately Prescott broached the idea with Remsen, who "carefully considered the subject" but "reached an unfavorable conclusion" (15). In a lengthy exposition of his reasons, Remsen made it clear that in his opinion the new organization and journal would not "advance the interests of

chemistry in this country." On 17 August 1889, with apparently little positive information to report on, Prescott solicited help from Clarke and Wiley in Washington. "I beg" Prescott wrote, "that you and your associates in Washington will make propositions, as to this report of the AAAS Committee, and as to action, if any, at Toronto... May I ask you to lay this letter before Professor Wiley and ask that he will kindly consider this letter addressed, also, to himself" (16,17).

Shortly thereafter, the final report was delivered at Toronto by Prescott and included a summary of individual and group comments, concluding with a brief order of procedure for formation of the new society (6,18). Prescott's fear that "unless the project is pretty clearly defined, to satisfaction of council, this body may refuse to confirm" (16) was subsequently justified, for no formal action was taken on the Committee report. On 23 September 1889, Prescott turned over his "meagre bundle" of correspondence to Clarke with his "good wishes for further effort" (19).

Alarmed by the AAAS committee suggestion that "the ACS holding meetings in New York ... submit its constitution and operative laws for the use of the committee, and to yield its name to the new Society," (16) and confronted with a formal proposal from C. E. Munroe on reorganization, the ACS began serious deliberations at the monthly meeting on 1 November 1889 (20). Discussion continued well into 1890 (21), culminating with a revised constitution adopted on 6 June 1890 (22). At the same time, Clarke and Wiley were gathering their support for the August 1890 AAAS meeting at Indianapolis. With a new constitution in effect, the ACS Board of Directors decided to meet Clarke and Wiley head on. On 22 July, a call was issued for the first general meeting of the ACS at Newport, RI. The dates were set as 6 and 7 August, just two weeks before the AAAS meeting. Munroe, who had been agitating for ACS reform, was surprised to be designated "chairman of the local committee with power." His apprehension about the lack of time and a poor location was dispelled by the arrival of the "genial, efficient, confidence-inspiring Dr. Charles F. Chandler, a host by himself." By special invitation, nonmember Frank W. Clarke was also in attendance (23).

Chandler chaired all of the sessions of the meeting, and on the second day "announced an informal discussion regarding the more general organization of chemists in America" and invited Clarke to present a "brief history of the movement" (23). There is no formal transcription of the resulting discussion. The effects, however, "were immediately noticeable" and "an era of good feeling and confidence were established" (23). When Clarke presented his report to the AAAS in Indianapolis just a few weeks later, he concluded with conciliatory remarks about the ACS. Particularly, he agreed that in "a consolidated national society the name of the American Chemical Society might well be retained by the enlarged organization ... which also would have rank of seniority above the other branches." It would be several years before the final consoli-

ation would come to fruition, but the die was cast and the future of the organization that "would rather swallow than be swallowed" was secured (24).

#### References and Notes

1. Presented in part at the 187th National Meeting of the American Chemical Society, St. Louis, MO, 9 April 1984; Abstract 013. The generous assistance of David M. Kiefer (*Chemical and Engineering News*) and Barbara A. Gallagher (American Chemical Society) is gratefully acknowledged. Their persistence was instrumental in uncovering the documentation reported in this paper. Jeffrey L. Sturchio (Merck & Co.) gladly provided important source material and shared his extensive expertise on the early ACS history. His continued interest and support is invaluable.
2. This Dickensian remark compared the ACS to the Club which was featured in *The Pickwick Papers*, published by Charles Dickens in 1837. See P. Harvey, *The Oxford Companion to English Literature*, Oxford University Press, London, 1938; p. 615.
3. From the collection of the American Chemical Society, Records Department, 1155 Sixteenth St., N.W., Washington, D.C. 20036.
4. Unless otherwise noted, this and all subsequent letters were written by the individual noted to Frank W. Clarke in June, July, or August, 1890. See Note 3.
5. J. J. Bohning, "Opposition to the Formation of the American Chemical Society", 184th National Meeting of the American Chemical Society, Kansas City, MO, September 1982, Abstract HIST 008.
6. "Report of the Committee of Conference on the Organization of a National Chemical Society", *Proc. Am. Assoc. Adv. Sci.* **1889**, 38, 35-38.
7. While the circular itself is undated, the earliest reply was written on 16 June 1890. There is no indication of the size of the original mailing list, although the responses seem to indicate that the 1889 membership rosters of the ACS, AOAC, and AAAS Section C were used.
8. In 1890 Frank Wigglesworth Clarke, age 43, was Chief Chemist of the United States Geological Survey. For more information on Clarke, see C. C. Gillespie, *Dictionary of American Biography*, Vol. 3, Scribner's, New York, NY, 1970-1979, pp. 292-294; C. E. Munroe, "Frank Wigglesworth Clarke", *J. Am. Chem. Soc.*, **1935**, 57, 20-30, and L. M. Dennis, "Frank Wigglesworth Clarke", *Bio. Mem. Natl. Acad. Sci.*, **1934**, 15, 146-165.
9. In 1890 Charles F. Chandler, age 54, was Professor of Chemistry at the Columbia College School of Mines. For more information on Chandler, see M. T. Bogert, "Charles Frederick Chandler", *Bio. Mem. Natl. Acad. Sci.*, **1932**, 14, 127-181, and R. D. Billinger, "The Chandler Influence on American Chemistry", *J. Chem. Educ.*, **1939**, 16, 253-257.
10. A detailed account of this period in ACS history is given by C. A. Browne and M. E. Weeks, *A History of the American Chemical Society: Seventy-Five Eventful Years*, American Chemical Society, Washington, D.C., 1952, pp. 26-40 and references therein.

11. A. A. Breneman to A. P. Prescott, 23 January 1889 (Note 3). Given the state of affairs of the ACS in December, 1888, Chandler was most likely the entire "Committee" described by Breneman.

12. Prescott had served as the ACS President in 1886 and AAAS Section C Vice-President in 1887. His committee appointed at the 1888 annual meeting of AAAS by Vice-President C. E. Munroe also had Alfred Springer (Cincinnati) and Edward Hart (Lafayette College) as members. For more on Prescott, see Anon., "Albert Benjamin Prescott", *J. Am. Chem. Soc.* **1905**, 27, Proceedings p. 76-78. The official records of the meeting give no Committee report, but simply a listing of the Committee members. ("Special Committees", *Proc. Am. Assoc. Adv. Sci.*, **1888**, 37, xviii). See also M. T. Bogert, "American Chemical Societies", *J. Am. Chem. Soc.* **1908**, 30, 171.

13. J. W. Mallet to A. B. Prescott, 2 February 1889 (Note 3): "[I am not] sanguine as to the likelihood of a vigorous Chemical Society being established". "In reply to your question as to chemists who might be written to in the South", Mallet gave 18 names and addresses. Albert R. Leeds to A. B. Prescott, 6 February 1889 (Note 3): "The difficulty in Northumberland in 1874 was that the leading chemists would not cooperate. They will not do so ... until a man of the stamp of Agassiz, or Hofmann, or of the personal magnetism (if not imminence) of Sir Henry Roscoe bids them fall into line in his rear. The American Chemical Society was originally organized with sections, the presiding officer of the Philadelphia Section being Dr. Genth, but these were abandoned from lack of general support, so excellent a chemist as Genth preferring to publish in other channels." Josiah P. Cooke to A. B. Prescott, 10 February 1889 (Note 3): "I have little faith in the usefulness of National Societies of any kind in this country." Ira Remsen to A. B. Prescott, 12 February 1889 (Note 3): "With every desire to cooperate with you, I do not feel that much can be accomplished."

14. C. F. Chandler to A. B. Prescott, 20 May 1889 (Note 3). This letter indicates that Chandler was willing, even anxious, to institute reform within the ACS before the 1889 AAAS meeting. From the limited available sample it is the only positive and encouraging response Prescott received. Apparently Prescott was not enthused about Chandler's proposal, but preferred to keep the AAAS as the dominant group in the reorganization.

15. Ira Remsen to A. B. Prescott, 10 June 1889 (Note 3). A copy of this letter was also sent to Clarke on 13 June 1889.

16. A. B. Prescott to F. W. Clarke, 17 August 1889 (Note 3).

17. There is reason to believe that Prescott did not have much to report in terms of a consensus of American chemists (see Note 13). In fact, Prescott asked Clarke about representatives on an organizational committee ("What others? Chem. Section of Franklin Inst? The Soc. of Agr. Chemists?") His letter to Clarke and Wiley was a desperate plea for help.

18. Prescott's report appears to have been greatly exaggerated in the claim that there had been consultation with committees of conference appointed by AOAC, ACS, CSW, and the Franklin Institute. The ACS Committee was Chandler, while official approval of the AOAC and CSW came after this meeting (see Notes 11, 14).

19. A. B. Prescott to F. W. Clarke, 23 September 1889 (Note 3).

Browne and Weeks (Note 10) have given full credit for the CCS movement to Clarke, and do not even mention Prescott in their description of these events.

20. "Minutes of Monthly ACS Meetings, November", *J. Am. Chem. Soc.*, **1889**, 11, 139-140.

21. "Minutes of Monthly ACS Meetings, December", *J. Am. Chem. Soc.*, **1889**, 11, 153-154; *Ibid.*, "January", **1890**, 12, 1-6; "February", 25; "May", 127; "June", 183.

22. "Constitution of the ACS", *J. Am. Chem. Soc.*, **1890**, 12, 184-188.

23. See Munroe's personal account in C. A. Browne, "A Half-Century of Chemistry in America"; American Chemical Society, Philadelphia, PA, 1926, Chapter IV. The official record is given in "The Newport Meeting", *J. Am. Chem. Soc.*, **1890**, 12, 247-255.

24. Edward Hart to Frank W. Clarke, 8 April 1891 (Note 3), commenting about the progress of the consolidation process.

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*James J. Bohning is Assistant Director of Oral History at the Beckman Center for the History of Chemistry and Professor Emeritus of Chemistry at Wilkes College, Wilkes-Barre, PA 18766. He is particularly interested in the history of the American Chemical Society.*

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## DIVERSIONS AND DIGRESSIONS

### The Tie That Blinds

*James J. Doheny, Chicago IL*

This is a tale of the "Terrible Thirties", when (as now), chemists were expected to be serious and single-minded, and even a bit sub-cultured. It involves George L. Parkhurst, a retired Vice-President of Standard Oil of California, and the late Robert E. Wilson, one-time Chairman and CEO of Standard Oil Company (Indiana). George was a recent graduate of the Armour (now Illinois) Institute of Technology, and Bob was then Director of Research, ca. 1930-31, and had picked George as one of his up-and-coming young men. In later years, Wilson was on the first U.S. Atomic Energy Commission, and was always active professionally and scientifically, and in business and civic affairs.

One year at Christmas, Wilson received a particularly horrendous example of a "Christmas necktie" which he could not persuade Parkhurst to accept. We can only surmise that somehow the two agreed that the only possible solution to the problem would be to present it to someone formally. They decided on Dr. Ward V. Evans, Professor of Chemistry at Northwestern University, esteemed teacher, *bon vivant*, and a *raconteur par excellence*. Obviously there were some conditions attached to the gift, as there is a somewhat cryptic reference in the Chicago Section A.C.S. publication, the

*Chemical Bulletin*, to a presentation being made by Evans to Erich von Gebauer-Fullneg. Gebauer was a brilliant young researcher in catalysis with Marbon in Gary, Indiana, who had some connections with Northwestern University. Not long afterward he died in a tragic accident involving the ingestion of liquid HCl which he was pipetting.

Gebauer presented the tie to Dr. Charles D. Hurd, of Northwestern University, who was also a neighbor of his in Evanston, Illinois. Hurd passed it on to Fred Sullivan of Standard Oil Company (Indiana), who was then Director of Research. Dr. Hurd recalls receiving and passing the tie onward and concurs that this sequence is most probably correct. Dr. Sullivan passed the tie on to Harrison E. Howe, who was then editor of the "News Edition" of *Industrial and Engineering Chemistry*. The News Edition was a small leaflet addition to the journal, and it soon became *Chemical & Engineering News*.

On 22 December 1933, Howe presented the tie to Dr. Edward Lyons, who was active in the Speaker's Tour Service and in local section activities of the ACS. He accompanied it with a copy of "The Tie that B(l)inds" by John Tarnowsky, with the suggestion that "each recipient pen an appropriate piece to accompany it ... thus completing the tradition". Actually this "started" the tradition. Tarnowsky's poem read:

Some may long for the soothing touch  
of lavender, cream, and mauve,  
but the ties I wear must possess a glare  
of a red hot kitchen stove.  
The books I read and the life I lead  
are sensible, sane, and mild.  
I like calm hats and I don't wear spats,  
but I want my neckties wild.

Give me a wild tie, brother,  
One with a cosmic urge  
A tie that will swear  
And rip and tear  
When it sees my old blue serge.

O, some may say that a gent's cravat  
should only be seen, not heard,  
But I want a tie that will make men cry  
and render their vision blurred.  
I yearn, I long, for a tie so strong  
it will take two men to tie it.  
If such there be, just show it to me -  
whatever the price, I'll buy it.

Give me a wild tie, brother,  
One with a lot of sins,  
A tie that will blaze  
In a hectic haze

Down where the vest begins!

Dr. Lyons rose to the occasion and firmed up the tradition by sending the tie to Professor Roger Adams at the University of Illinois with:

Some ties are short, some ties are long,  
Few ties, however, rate a song.  
But here is a tie, not red or blue,  
It's the tie that blinds I send to you.  
Wear it once - then pass it along  
And speed its journey with a song.

Roger Adams, for whom a biographical note would probably be unnecessary as well as impossibly long, sent the tie to Thomas Midgley, Jr., of the Ethyl Corporation. He is popularly remembered as the man responsible for the development of lead tetraethyl as an additive for gasoline, and freon, but had a distinguished career in related industrial fields. Adams' poem is almost a sonnet:

Hi! Ho! the tie that blinds  
That ever sacred bow;  
From this man to that man  
Is always doomed to go;  
To tall men, to short men,  
To prof and engineer,  
To fat men, to lean men,  
To men both far and near.

And now with Midg' it safely rests  
As bright and wild as ever,  
To help him in his daily task -  
More molecules to tether.

Midgley sent the tie to Professor Wilder D. Bancroft of Cornell University. Midgley's poem is one of the more carefully crafted ones:

"This chemical creation" said the Doctor to his class,  
"Is a simple preparation, made from tar and isinglass.  
Add some treacle, then a blotter and some dessicated toads,  
Heat it hot and still hotter till the entire mass explodes.  
Note quite well the fiery pattern as the detonation fades,  
Like the saffron rings of Saturn in a hundred million shades;  
Like some cosmic evolution to more scientific minds;  
Like the Cross to Rosicrucians: a symbolic tie that blinds."

Bancroft was known to the students of that period as Mr. Colloid. He also considered various geriatric problems, and suggested at one time that possibly the aging phenomenon might be considered to be a cause or effect of the brain becoming a colloid. He thus cleverly parodied his own work

in sending the tie to Gilbert N. Lewis of California, on 17 April 1935:

I am an old prof from Cornell  
And my brain is beginning to jell;  
I prefer other kinds  
To the weird tie that blinds,  
It gives me a pain where I swell.

(Could the last word have been misread or miswritten as "smell"?)

G. N. Lewis was, in turn, Mr. Thermodynamics. When one recalls Professor Kahlenberg of the University of Wisconsin stating in his thick, guttural accent "Thermodynamics is a gut thing for steam engines, but a chemist is afraid of an integral sign," one might suspect that Lewis thought the design was a mass of integrals. Charles Hurd suggests that "Maybe Lewis liked the tie!" It apparently sank into a thermodynamic morass. Of course, the Lewis papers are now in the Bancroft Library on the Berkeley campus of the University of California, and Dr. Robin Rider spoke about preliminary explorations of them at the recent meeting in Los Angeles. Possibly a warning sign could be erected with the hope that some future explorer may find the tie, write a poem, and start it off again on its journey. Many of us would have appropriate nominees for the honor.

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*James J. Doheny, 3625 McCormick Avenue, Brookfield, IL 60513, is a 64-year member of the American Chemical Society, largely because he was forced to join as an undergraduate in order to pass a course in physical chemistry taught by Ben Freud at the Armour (Illinois) Institute of Technology many years ago.*

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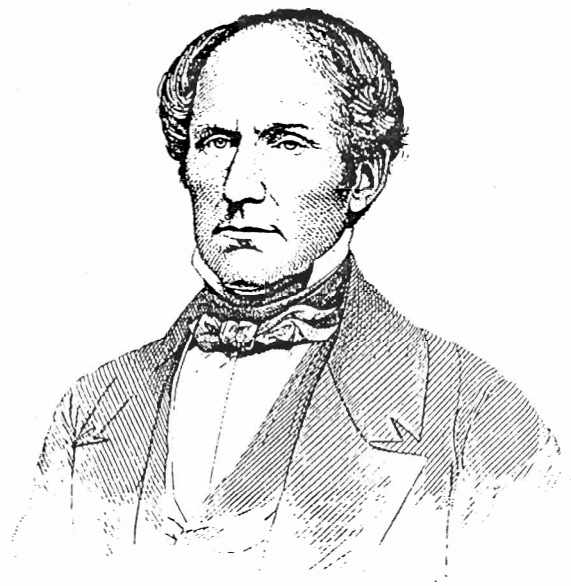
## OLD CHEMISTRIES

### John Johnston's *Manual of Chemistry*

*William D. Williams, Harding University*

John Johnston's *Manual of Chemistry* was an extremely popular college chemistry text during the middle of the 19th century (1). It had eight editions from 1840 to 1874, with several editions having over a dozen publishers distributed throughout the eastern half of the United States. It was equally popular west of the Mississippi (2) and continued to be used as late as 1879 (3). Although designed for colleges, it was used in some secondary schools.

Popularly known by its spine label, *Johnston's Turner's Chemistry*, this book was one of several American texts based upon the British work, *Elements of Chemistry*, by Edward Turner (4). Claiming little originality, Johnston referred to



John Johnston (16)

himself as a "compiler" and listed dozens of other works used in the preparation of the text. A comparison of the two works reveals that well over half of Johnston's text was a word for word abstract of Turner. He kept Turner's topic arrangement, but omitted tedious details and complicated theoretical material. He emphasized fundamental facts, principles and practical applications. Wherever possible, he replaced British names and applications with American. Johnston greatly enhanced the overview of the material by adding key words at the beginning of paragraphs and by presenting headings in more of an outline form. He added figures and 28 pages of study questions, which were not present in Turner. In short, Johnston altered the dry, tedious Turner in exactly the way needed to make it more palatable. The success of the volume verified his judgement.

The contents of Johnston's *Manual* were typical of most texts of the era:

- \* Part I. Imponderable Substances (chapters on Heat, Light, Electricity).
- \* Part II. Inorganic Chemistry (chapters on Chemical Combination, Non-metallic Elements, Metallic Elements, Salts).
- \* Part III. Organic Chemistry (chapters on General Principles and Constitution, Vegetable Chemistry, Animal Chemistry).
- \* Part IV. Analytical Chemistry.

Johnston also inserted several interesting American topics, including Morse's telegraph; Robert Hare's hydrogen-oxygen torch (with a comment that John Webster of Harvard had recently had an explosion using such an apparatus); Silliman's method of preparing fulminate of mercury; and a description of

A MANUAL  
OF  
CHEMISTRY,  
ON THE BASIS OF  
DR. TURNER'S ELEMENTS OF CHEMISTRY;  
CONTAINING, IN A CONDENSED FORM,  
ALL THE MOST IMPORTANT FACTS AND PRINCIPLES  
OF THE SCIENCE.  
DESIGNED FOR A TEXT BOOK  
IN COLLEGES AND OTHER SEMINARIES OF LEARNING  
A NEW EDITION.  
BY JOHN JOHNSTON, A. M.  
PROFESSOR OF NATURAL SCIENCE IN THE WESLEYAN UNIVERSITY.  
PHILADELPHIA:  
THOMAS, COWPERTHWAIT & CO.  
1846.

L. C. Beck and Joseph Henry's slide rule scale of equivalents (which used hydrogen as the standard rather than oxygen as Wollaston had done in England) (5). Hare was frequently quoted as if considered the most authoritative American.

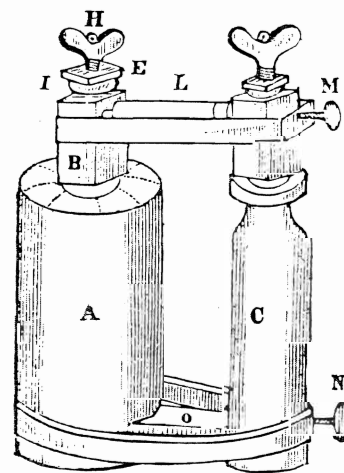
The only item of personal experience in Johnston's text was a description of his apparatus for producing solid "carbonic acid", as carbon dioxide was then known. Around 1840, he experimented with Faraday's techniques of liquifying a gas by generating it in a cooled, sealed tube which was strong enough to withstand the high pressure. Then he constructed a metal apparatus and duplicated the work of Thilorier, who had recently obtained solid carbon dioxide. Johnston's apparatus, which used cheap parts so that schools could afford to do the experiment, was described in Silliman's *American Journal of Science and Arts* (6). This article was reprinted in the section on heat of Johnston's *Manual* (7). The gas was liquified in a stopcocked metal collection tank that could be separated from the gas generation tank. When the liquid was rapidly released from this tank, solid carbon dioxide was formed. He constructed such an apparatus for Edward Hitchcock at Amherst and demonstrated one for John Torrey's classes at the College of Physicians and Surgeons in New York. Torrey stated that Johnston's apparatus produced twice as much solid as that of

John K. Mitchell, who was the first American to produce solid carbon dioxide (8).

John Johnston was born on 23 August 1806 in Bristol, Maine (9). Details of his early life and education are not known. At the age of 22, he entered Bowdoin College, Brunswick, Maine, and studied science under Parker Cleaveland. Following his graduation in 1832, he taught in a seminary in Cazenovia, New York. In 1835 he was appointed Lecturer in Natural Science and Assistant Professor of Mathematics at Wesleyan University, Middletown, Connecticut. Four years later he advanced to Professor of Natural Science and remained in that position until his retirement in 1873. He was awarded honorary A.M. degrees in 1835 from both Bowdoin and Wesleyan and the LL.D. degree in 1850 from McKendree College, Lebanon, Illinois. In 1854 he was an unsuccessful candidate for Professor of Natural Science and Chemistry at Columbia College, New York City (10).

Johnston had a high regard for his former professor, Parker Cleaveland. He not only dedicated his *Manual* to Cleaveland, but he conducted his lectures at Wesleyan in the same manner that Cleaveland had used at Bowdoin (11). One reference in the *Manual* referred to Johnston's "manuscript notes of Professor Cleaveland's lectures at Bowdoin College, in the year 1832" (12). Johnston probably wrote the anonymous pamphlet, *A Vocabulary, containing a concise explanation of certain terms used in Chemistry; more particularly those which relate to Chemical Nomenclature*. This glossary, which was published in Cazenovia in 1835 while Johnston taught there, was a revision and enlargement of an 1826 work by Cleaveland (13). The two men corresponded and, during his experimentation with liquifying gases, Johnston sent Cleaveland sealed tubes of liquid carbon dioxide (14).

Johnston's experience with gas liquification became a part of a family business. His four sons operated a dental supply



Johnston's apparatus for solidifying carbon dioxide.



business, Johnston and Browne, 1260 Broadway, New York City. They had a factory on Staten Island where they manufactured dental chairs and bottles of liquid nitrous oxide, the "laughing gas" used as an anesthetic for tooth extractions (15).

Johnston authored other widely used texts in chemistry and physics. His secondary level text, *Elements of Chemistry*, which was an abridgement of his *Manual*, was popular from 1850 to 1867. He also wrote the secondary level *Manual of Natural Philosophy* and the elementary *Primary Natural Philosophy*. He contributed articles to several scientific and popular publications. His pamphlet, *The Science of Agriculture* (1846), an address to the county agricultural society, emphasized the chemistry involved in plant growth. He was an authority on Maine history and wrote *A History of the Towns of Bristol and Bremen* (1873). He was an original member of the American Association for the Advancement of Science (1848) and was elected to the American Philosophical Society (1876).

Johnston married Maria Hamilton in 1835. The couple had five sons, with one dying in infancy. In his latter years, he lived with a son at Clifton, Staten Island, New York, where he died on 1 December 1879.

#### References and Notes

1. J. Johnston, *A Manual of Chemistry, on the basis of Dr. Turner's Elements of Chemistry; containing, in a condensed form, all the most important facts and principles of the science. Designed for a textbook in colleges and other seminaries of learning*, Barnes & Saxe, Middletown, [CT], 1840.
2. H. Hale, "Early Chemical Laboratories West of the Mississippi", *J. Chem. Educ.*, 1937, 14, 62-64.
3. F. W. Clarke, "A Report on the Teaching of Chemistry and Physics in the United States," No. 6-1880, *Circulars of Information on the Bureau of Education*, Government Printing Office, Washington, D.C., 1881, p. 161.
4. E. Turner, *Elements of Chemistry, including the recent discoveries and doctrines of the science*, 4th American from 3rd London ed., Grigg & Elliot, Philadelphia, 1832. Turner had seven British editions from 1827 to 1842. Franklin Bache edited six official American editions from 1828 to 1840. These were copies of the British work with frequent footnotes by Bache. A seventh American edition in 1846 was edited by James and Robert Rogers. Other American texts using Turner as a basis were: J. Green, *A Text Book of Chemical Philosophy* (one edition, 1828); J. Comstock, *Element of Chemistry* (over 100 editions from 1831 to 1859); A. Gray, *Elements of Chemistry* (over 40 editions from 1840 to 1858); L. Beck, *A Manual of Chemistry* (four editions from 1831 to 1844).
5. J. Johnston, *Manual of Chemistry...*, Thomas Cowperthwait & Co., Philadelphia, 1846, pp. 111, 153, 376, 133.
6. J. Johnston, "Description of an Economical Apparatus for Solidifying Carbonic Acid, recently constructed at the Wesleyan University, Middletown, Conn.", *Am. J. Sci. Arts*, 1840, 38, 297-301 and "Remarks and Suggestions with Regard to the Proper Construction and Use of Apparatus for Solidifying Carbonic Acid", *Am. J. Sci. Arts*, 1842, 42, 203-206.
7. Reference 5, p. 52.
8. ALS, J. Johnston to Parker Cleaveland, 4 November 1841, Bowdoin College Library Archives.
9. Biographical information on Johnston may be found in: "Obituary", *New York Times*, 3 December 1879, p. 2, c. 6; Johnston letters in the Bowdoin College Library Archives; N. Cleaveland and A. S. Packard, *History of Bowdoin College*, Ripley, Osgood & Company, Boston, 1882, p. 433; *General Catalogue of Bowdoin College and the Medical School of Maine; A Biographical Record of Alumni and Officers*, no publisher, Brunswick, Maine, 1950, p. 64; C. A. Elliott, *Biographical Dictionary of American Science*, Greenwood Press, Westport, Connecticut, 1979, p. 140; and *Appleton's Cyclopaedia of American Biography*, D. Appleton & Co., New York, Vol. 3, 1888, p. 485.

Table 1. Editions of Johnston's *Manual of Chemistry*

Edition	Date	Pages	Publisher	Place
[1st]	1840	453	Barnes & Saxe	Middletown, CT
[2nd]	1842	452	Thomas, Cowperthwait & Co.	Philadelphia
[3rd]	1843-48	480	Thomas, Cowperthwait & Co.	Philadelphia
4th	1848	480	Thomas, Cowperthwait & Co.	Philadelphia and ten other cities
5th	1852	480	Thomas, Cowperthwait & Co.	Philadelphia and ten other cities
5th	1854	480	Cowperthwait, Desilver & Butler	Philadelphia and 11 other cities
6th	1856, 59	528	Charles Desilver; Keen & Lee	Philadelphia; Chicago
6th	1861, 63	528	Charles Desilver; Cushings & Bailey	Philadelphia; Baltimore
7th	1869	528	Charles Desilver	Philadelphia
8th	1872, 74	542	Desilver; Claxton & Haffelfinger; J. B. Lippincott	Philadelphia and 13 other cities

10. ALS, Johnston to Nehemiah Cleaveland, 2 March 1854, Bowdoin College Library Archives.

11. ALS, Johnston to Parker Cleaveland, 4 November 1840, Bowdoin College Library Archives.

12. Reference 5, p. 34.

13. [J. Johnston], *A Vocabulary, Containing a Concise Explanation of Certain Terms Used in Chemistry...*, J. F. Fairchild & Son, Cazenovia, N.Y., 1835. This work was a revision and enlargement of *Vocabulary, Containing an Explanation of Certain Chemical Terms...*, published anonymously at Brunswick, Maine in 1826 and attributed to Parker Cleaveland. Johnston's pamphlet used most of the terms in Cleaveland's, revising about 15%. He added new terms to raise the number of entries from Cleaveland's 159 to a total of 290. As in Cleaveland, the last two pages were devoted to a list of "ancient" chemical names with their corresponding "systematic" names. There were 28 names in Cleaveland versus 45 in Johnston. (Cleaveland's *Vocabulary* was first published in the rear of his 1816 *Elementary Treatise on Mineralogy and Geology*. That shorter version of only 66 items contained many entries identical or quite similar to the 1826 edition. The Bowdoin College archives contain two undated variants of Cleaveland's *Vocabulary*. One is a manuscript copy which was apparently a precursor to the 1816 *Treatise*. The other is a printed edition, without title page, which predated the 1826 edition. It has five fewer entries and a few definitions that have been revised in the 1826 edition.)

14. ALS, Johnston to Parker Cleaveland, 7 November 1840, Bowdoin College Library Archives.

15. ALS, Johnston to A. S. Packard, 10 May 1878, Bowdoin College Library Archives.

16. The author would like to thank Wesleyan University for its help in obtaining a copy of Johnston's portrait.

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*William D. Williams is Professor of Chemistry at Harding University, Searcy, AR 72143. He collects and studies early American chemistry texts.*

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## WHATEVER HAPPENED TO THE NASCENT STATE?

*William B. Jensen, University of Cincinnati*

In 1884 the British chemist, M. Pattison Muir, published a textbook on theoretical chemistry entitled *A Treatise on the Principles of Chemistry* in which he attempted to summarize many of the recent results "on the subjects of dissociation, chemical change and equilibrium, and the relations between chemical action and the distribution of the energy of the changing system" - in short, most of the topics which would, within the next ten years, come to become identified with the new and rising field of physical chemistry and the work of

Ostwald, Arrhenius and van't Hoff (1). Though Muir himself did not succeed in establishing a British school of physical chemistry and did not make any significant experimental contributions to the new field, he did play a role in disseminating its early results through his review of Ostwald's work on the measurement of affinity coefficients (2), the writing of a monograph on thermochemistry (3), the editing of an influential dictionary of chemistry (4) and, of course, through his textbook.

Muir, who was later to write an important history of chemistry (5), also had an unusual appreciation of the history of his subject and in his textbook attempted to use the new views on chemical equilibrium and kinetics to unravel some long-standing paradoxes of chemical affinity that had been known since the end of the 18th century. Among these were the problems of predisposing affinity, contact actions, and the so-called *status nascens* or nascent state. The first of these topics has long since disappeared from the textbooks, whereas the second, under the rubric of heterogeneous catalysis, has survived. In many ways, however, it is the third topic that is the most fascinating, as not only the explanation of the nascent state, but the very question of whether it really exists, are still unresolved problems. A history of the various attempts to explain this phenomenon provides one with an interesting cross section of 19th and 20th century chemical theory and, though an explicit treatment of this subject has been missing from textbook literature since the 1940's, it is of interest to note that the term is still to be encountered, albeit in passing and without explanation, in more recent textbooks (6).

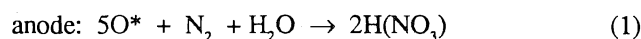
A knowledge that freshly prepared gases, when generated *in situ* within a reaction system, are frequently more reactive than when added already prepared from an external source seems to date from the late 18th century. This enhanced, but short-lived, reactivity appeared to be associated with the gases only at the moment of their "chemical birth", so to speak, and the resulting metaphor became enshrined within the chemist's lexicon in the phrase "nascent state", though the chemical poet who first coined the term is not known with certainty.

The first explicit use that I have been able to locate occurs in the 1790 edition of Joseph Priestley's work, *Experiments and Observations on Different Kinds of Airs* (7). Having incorrectly postulated that both fixed air (carbon dioxide) and nitrous acid (nitric acid) were compounds of inflammable air (hydrogen) and dephlogisticated air (oxygen), Priestley attempted to rationalize the different products as a function of differences in the reaction conditions, arguing that:

... when either inflammable or dephlogisticated air is extracted from any substance in contact with the other kind of air, so that one of them is made to unite with the other in what may be called its *nascent state*, the result will be *fixed* air; but that if both of them be completely formed before the union, the result will be *nitrous* acid.

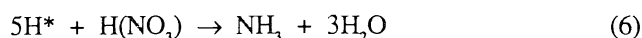
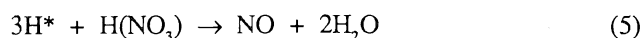
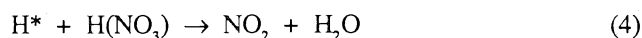
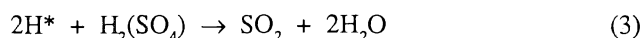
A year earlier, William Higgins, in his book, *A Comparative View of the Phlogistic and Antiphlogistic Theories*, also invoked the unusual reactivity of hydrogen "at the very instant of its liberation" to rationalize the necessity of water for the oxidation of iron (8). Though he did not describe the hydrogen as nascent, he did use the term when paraphrasing his original statement in 1814 (9).

In the examples cited by both Priestley and Higgins, the nascent gases were generated chemically. In 1807 Humphry Davy reported that a similar enhanced reactivity was also present in the case of electrochemically generated gases (10). Upon electrolyzing water, he observed that some nitric acid was invariably formed at the anode and a trace of ammonia at the cathode. These he attributed to the "combination of nascent oxygen and hydrogen respectively with the nitrogen of the common air dissolved in water", since he knew that these reactions did not occur at room temperature when the fully formed gases were allowed to interact. In modern terms, the observed secondary electrode reactions can be represented as:



where we have used the starred symbol to represent the nascent state of the element in question in order to avoid the explicit representation of any hypothesis concerning its molecularity or structure.

An examination of 19th century chemical literature shows that the nascent state soon became the accepted rationale for a number of otherwise puzzling chemical reactions. Among these were the varying products observed when metals lying above hydrogen on the activity series reacted with either sulfuric (11, 77) or nitric acid (12). At low acid concentrations dihydrogen gas is the major product in both cases, whereas at higher acid concentrations sulfur dioxide, nitrogen oxides and, in some cases, even ammonia are obtained instead. To explain this concentration dependency it was assumed that nascent hydrogen was initially generated in both systems. At low acid concentrations this reverted to normal dihydrogen gas, whereas as at higher acid concentrations it was entirely consumed in reducing the respective acids and dihydrogen gas was no longer observed among the reaction products:

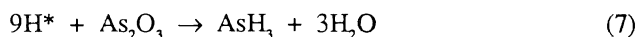


Other classic examples include the use of nascent hydrogen



Joseph Priestley (1733-1804) was one of the first to use the term nascent state in his chemical writings.

to generate arsine in the famous Marsh test for arsenic (13):

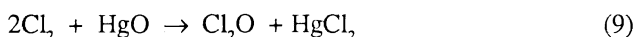


in the reduction of nitrobenzene to aniline:



in the Clemmensen reduction of carbonyls, and in the Jones reduction column. In each case the hydrogen is generated *in situ* by means of zinc and hydrochloric acid and the reactions are not observed under the same conditions if externally generated dihydrogen gas is used instead.

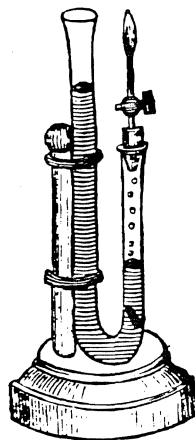
In all of these examples the actual generation of dihydrogen can be observed as a competing reaction. However, the nascent state concept was soon extended to systems in which the gas corresponding to the nascent intermediate is never observed under the conditions of the experiment and which apparently involve a single overall net reaction rather than two competing reactions. For example, nascent oxygen was used to explain the production of dichlorine oxide in the reaction (14):



via the mechanism:



Marsh's apparatus for testing for the presence of arsenic. The unknown is placed in the tube along with hydrochloric acid and a piece of arsenic-free zinc. The nascent hydrogen reacts with any arsenic in the sample (usually in the form of the oxide) to generate arsine. The hydrogen-arsine mixture issuing from the end of the tube is lit. The arsine decomposes in the flame and leaves a mirror of elemental arsenic on a cool surface held above the flame.



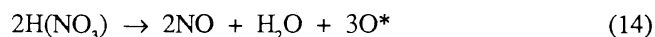
where again it was not possible to produce the highly endoenergetic dichlorine oxide by direct reaction between molecular dioxygen and dichlorine, though in step 11 it is presumed to occur with nascent oxygen.

Likewise, nascent oxygen was frequently invoked to explain the bleaching (or oxidizing) ability of *eau de Javelle* or hypochlorite solutions (15), via the initial step:

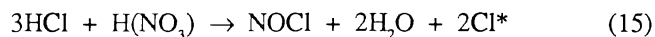


a supposition apparently supported by the fact that their water solutions eventually decompose, on standing, to give the corresponding metal chloride and dioxygen gas.

Many of the oxidation reactions of nitric acid with the less active metals and with the nonmetals were also rationalized in the same fashion (16):



and the ability of *aqua regia*, in contrast to either dichlorine or hydrogen chloride gas, to chlorinate gold was likewise attributed to the initial production of nascent chlorine (16):



As will be seen, the use of the nascent state in these cases became the basis of much of the criticism later directed at the concept.

The first, and by far the most historically significant, attempt to rationalize the nascent state was made by the French chemist, Auguste Laurent, in 1846. Laurent and his collaborator, Charles Gerhardt, were early proponents of the concept that the elements in their standard states were composed of

polyatomic molecules rather than isolated atoms. Contrary to the usual accounts found in most histories of chemistry, the supporting evidence for this proposition was based not just on Gay-Lussac's law of combining volumes, but on considerations related to the energetics of chemical reactions as well.

Rephrasing the original arguments of Laurent and Gerhardt in terms of modern bonding terminology, the implied assumption was that chemical bonds could be broken in the course of a spontaneous reaction only if they were replaced by stronger bonds. Given this premise, and the assumption that the elements were monoatomic, it was difficult to understand why some compounds spontaneously dissociated into their elements and, conversely, why it was not possible to synthesize certain compounds directly from the elements themselves, though they could be made indirectly by means of displacement reactions. However, once one accepted the existence of elemental polyatomic molecules, these paradoxes vanished, since in both cases the breaking or making of the bonds in the compounds was competing with the making or breaking of bonds within the elemental molecules, or as Gerhardt put it, with the synthesis or decomposition of a "hydride of hydrogen", a "chloride of chlorine", etc (17).

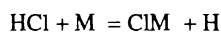
Applying this concept to the nascent state, Laurent wrote (18):

A binary association of atoms might allow us also to account to a certain extent for the affinity possessed by substances in the nascent state. If two free molecules of bromine and of hydrogen,  $\text{BB}'$  and  $\text{HH}'$ , are brought together, the affinity of B for B' and of H for H' may suffice to prevent the combination of B and B' with H and H'. But if the substances present are H and B, these two, which have no affinity



Auguste Laurent (1807-1853) was the first to propose that nascent gases contained free atoms.

to overcome, will be able to combine readily. This is what will occur when hydrogen is in the nascent state. That is, whenever it is evolved from hydrochloric acid by the action of a metal, we shall have the equation:



and there will be a tendency to a reconstruction of a binary molecule either by combination with bromine or with another atom of hydrogen.

Similar arguments were also put forward by the English chemist, Benjamin Brodie, in the early 1850's (19-21).

The value of this interpretation of the nascent state as a minor, but useful, piece of evidence in favor of elemental polyatomic molecules was fully appreciated by the 19th century chemist, and it is in this context, rather than as a topic in chemical kinetics, that the subject found a place in most chemistry textbooks after 1860. Adolphe Wurtz devoted three pages to the subject in his classic historical study of the development of the atomic theory in 1881 (17) and Pattison Muir was quite explicit about its pedagogical value in his 1884 text (1):

A study of the reactions in which nascent substances play important parts appears to me to keep before the student that all important distinction between the atom and the molecule which is so vital in chemical considerations, and also to draw attention in a marked way to the complexity of all chemical changes.

Though several 19th century chemists (11, 22-23) felt that their experimental investigations of the nascent state were not inconsistent with the free-atom interpretation, there was in fact very little direct experimental evidence for the theory until the second and third decades of the 20th century. This came from studies by Irving Langmuir (24), Karl Bonhoeffer (25-26), Hugh Taylor (27) and others of the gas-phase chemical reactions of atomic hydrogen that had been generated either by thermal dissociation on a heated wire or by means of high voltage electrical discharges. But even here the proof was really indirect and was based on the similarity between the reactions observed in the gaseous phase for atomic hydrogen and those observed in the liquid phase for chemically or electrochemically generated nascent gases. Attempts to bubble gaseous atomic hydrogen into solutions gave negative results due to rapid recombination into dihydrogen gas, whereas attempts to detect the existence of a soluble, diffusible, nascent species in the liquid-phase chemical and electrochemical systems were inconclusive at best (28, 29). Indeed, the results seemed to indicate that in these systems the nascent activity was confined to the surface of either the chemically reacting metal or the active electrode.

Not all 19th century chemists were enamoured of the free-

atom theory of the nascent state and in many ways the opposing experimental evidence was more convincing than the supporting evidence. Much of this was the result of the efforts of the Italian electrochemist, Donato Tommasi (30-33). He pointed out that, if nascent hydrogen was really atomic in nature, its properties should be independent of the metal - acid system used to generate it. But in actual fact, as his own experiments showed, its reducing ability was highly dependent on the nature of the metal. Thus nascent hydrogen generated by means of zinc and hydrochloric acid was able to reduce chlorates to chlorides, whereas that generated either electrolytically or by means of sodium amalgam produced no reaction. Tommasi further noted that all of the reductions observed for nascent hydrogen at room temperature could also be observed for fully formed dihydrogen gas at higher temperatures. He therefore postulated that nascent hydrogen was nothing more than thermally hot dihydrogen that had not yet had time to thermally equilibrate with the surrounding solvent molecules and that its reducing abilities should directly parallel the enthalpy of the reaction used to generate it. Thus he wrote in 1879 (31):

And if this gas in the nascent state possesses greater affinity than in the natural state, it is solely due to the fact that *the hydrogen the moment it issues from a combination is found to be accompanied by the whole quantity of the heat produced during the setting free of the hydrogen. Consequently, nascent hydrogen is nothing else than ordinary hydrogen in [different] thermic conditions or, speaking generally, in different physical conditions.* To my mind, the expression nascent hydrogen is synonymous with hydrogen + calories. In fact, all the reactions produced by nascent hydrogen can be obtained quite as well with ordinary hydrogen and a high temperature; and the differences observed between the hydrogen resulting from different chemical reactions are simply due to the fact that these reactions do not develop the same quantity of calories.

Aside from a correction of some of his thermodynamic data by Julius Thomsen (34), Tommasi's theory was generally well received and was often quoted by the authors of advanced textbooks and chemical dictionaries after 1880 (1, 4, 35). Muir expressed a somewhat more sophisticated version of it in his 1884 text, writing (1):

In a reaction wherein [a] given compound is produced there must be a moment of time when this compound can only be said to exist potentially, when the atoms which constitute its molecules have not settled down into stable configurations; at this moment the compound may be said to exist in the nascent state. If the atomic vibrations and interactions are allowed to run what might be called their normal course, the compound molecules are certainly produced, but if these interactions are interfered with, a new set of molecules may be formed, which molecules bear a more or less simple genetic relation to those produced in the normal process of chemical change.

This view of the nascent state has recently been revived among chemists working in the field of molecular reaction dynamics (36), where it is applied to the vibrationally and/or electronically excited species which are the initial products observed in molecular cross-beam studies of gas-phase reactions. Indeed, in 1976, Simon Bauer of Cornell University wrote a short note in which he argued that (37):

It is now time to restore the term "nascent" to legitimacy. Much evidence has accumulated, both experimental and theoretical, which demonstrates that in many reactions the partition of nascent products among their characteristic rotational, vibrational, and electronic states differs substantially from that expected were these generated in statistical equilibrium. *Nascent* contrasts with *state relaxed*, i.e. a system for which a *single* temperature and corresponding thermodynamic functions can be defined.

Though Bauer cited several examples of how the reactivity of these nascent products differed from that of the corresponding state-relaxed products, it is open to question, as in the earlier case of the gas-phase atomic hydrogen experiments, whether this explanation can be extended to the classical liquid-phase chemical and electrochemical systems which gave rise to the concept of the nascent state in the first place.

Yet a third theory of the nascent state was proposed by the German chemist, Gottfried Wilhelm Osann, in a series of more than a dozen papers published between 1852 and 1864 (38). By electrolysis of a dilute solution of freshly distilled Nordhäusen sulfuric acid, Osann obtained a form of activated hydrogen which was capable of reducing solutions of silver salts to metallic silver and mixtures of potassium hexacyanoferrate(III) and iron(III) chloride to Berlin blue. Platinum and porous carbon cathodes were capable of storing the active hydrogen and could be removed from the electrolysis apparatus and placed in other solutions, where they produced reactions not normally observed for dihydrogen gas under similar conditions.

Heavily influenced by Christian Schönbein's discovery of ozone, Osann decided he had discovered a similar allotrope of hydrogen and since, like ozone, it was much more chemically reactive than the normal form of the element, he gave it the name of *ozon-wasserstoff* (39). In later work he reported that the atomic weight of his new gas was 0.66 or about two thirds that of normal hydrogen (40).

Similar ideas were expressed by T. L Phipson in 1858 (41):

What is called the "nascent state" is, I think, nothing more or less than the allotropic state of bodies entering into combination ... it seems incontestible that *every time oxygen enters into or leaves compounds, it is in the state of ozone*. When we reflect further on the results already obtained with hydrogen, chlorine, bromine, sulphur, and phosphorus, we are inclined to believe that all simple bodies should behave in the same manner; that is to say, that all bodies may have an allotropic state

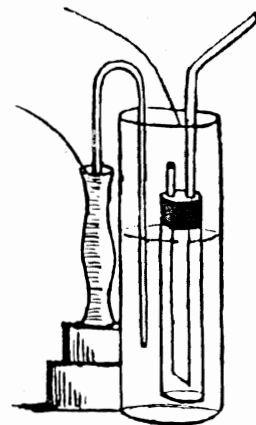
analogous to ozone, and that they are in this state at the moment of entering into combination, or when they are in the *nascent state*.

Osann's results were criticized by J. Löwenthal in 1858, who suggested that the enhanced reducing properties of the gas were due to the presence of sulfur dioxide from the Nordhäusen acid used in its preparation (42). This criticism was disproved by Osann (43), but more serious problems were uncovered by Gustav Magnus (44), who was unable to duplicate many of Osann's results. He concluded that Osann's materials were contaminated with iron and that this was actually responsible for the observed reductions. Though Hermann Fehling summarized Osann's work in his 1864 *Handbuch* (45), he was critical and, by 1895, Albert Ladenburg felt that Osann's hypothesis had been "aus der Welt geschafft" (35).

Actually Ladenburg's funeral oration proved to be premature as, in a series of papers published between 1920 and 1922, Gerald Wendt and Robert Landauer of the University of Chicago revived a form of Osann's hypothesis (46-47). By means of  $\alpha$ -radiation, electrical discharge under reduced pressures, and high potential coronas at atmospheric pressure, they generated an activated form of hydrogen which showed a reactivity different from both normal dihydrogen gas and atomic hydrogen. This they equated with the  $H_3$  molecule postulated by Joseph J. Thomson in 1913 (now known to be the  $H_3^+$  ion). Because of both its increased reactivity relative to dihydrogen and the analogy between its formula and that for  $O_3$ , they proposed that the new species be called *hyzone*.

As with the work on atomic hydrogen, these results could not be extended to the traditional liquid-phase chemical and electrochemical methods of generating nascent hydrogen (47), though A. C. Grubb, in a short note published in 1923, claimed to have prepared traces of the species by dropping acid on pieces of suspended metal so as to avoid emersion of the metal in liquid (48).

The phenomenon of allotropism was a difficult problem for the 19th century chemist. Rationales based on variations in either the degree of molecular complexity (i.e. polymeriza-



Osann's apparatus for the preparation of ozon-wasserstoff via the electrolysis of a water solution of freshly distilled Nordhäusen acid.

tion) or variations in molecular structure (i. e., isomerism) had to await the work of Brodie in the 1850's on the concept of elemental polyatomic molecules (49). Indeed, it was because of this very problem that Berzelius had introduced the distinctions between isomerism and polymerism, on the one hand, and allotropism, on the other (50), and he himself favored the idea that allotropes corresponded to different electrical states of an element's atoms.

This concept was widely used to rationalize allotropism in the first half of the 19th century and was applied in great detail by Schöbein in his attempts to explain the behavior of ozone (thought to be negative) and its relationships to normal oxygen (thought to be neutral) and the antozones (i.e., peroxides, thought to be positive) (51). Not surprisingly, it also made its appearance in discussions of the nascent state via the allotropic theory. Thus Phipson contended that the transient allotropic modification of an element which appeared at the moment of its formation was equivalent to having rendered the element "infinitely more electropositive or electronegative" than its normal state (52).

Like many of the other rationales of the nascent state, this so-called "polar" theory experienced a brief revival in the early 20th century, this time within the context of early electronic theories of the chemical bond. This modernized version was apparently suggested by Harry Shipley Fry of the University of Cincinnati, who was an early proponent of a polar theory of organic chemistry based on the electron transfer model of chemical bonding first proposed by J. J. Thomson (53). Consistent with the fact that this theory viewed all bonding as ionic, diatomic molecules were written as  $H^+H^-$ ,  $Cl^+Cl^-$ , etc. This formulation meant that in the generation of dihydrogen gas by reduction of aqueous  $H^+$  solutions, the reduction of the  $H^+$  ion had to proceed all the way to  $H^-$ . This then combined with another  $H^+$  ion from the solution to generate the dihydrogen gas. Likewise, generation of dichlorine via oxidation of aqueous chloride solutions required the generation of  $Cl^+$ , which then combined with  $Cl^-$  to generate the final product. Thus Fry equated the nascent state with the generation of transient free ions having unusual and highly reactive oxidation states. This view was also supported by A. Pinkus (54), but fell out of favor with the eclipse of the polar bonding model by the electron-pair covalent model of G. N. Lewis.

Even these permutations fail to exhaust the speculations of the 19th century chemist relative to the nature of the nascent state. Yet a fifth model - the so-called occlusion model - was developed by John Gladstone and Alfred Tribe in the late 1870's as a result of their work on the copper-zinc couple (55-58). Made by placing a strip of zinc foil in a copper sulfate solution for a few minutes in order to plate out a small quantity of copper on the zinc, Gladstone and Tribe found that in water solutions the resulting couple was able to perform many of the reductions observed for nascent hydrogen, whether generated electrochemically or via metal-acid reactions. Among these

were the reduction of chlorate to chloride, nitrate to nitrite and ammonia, hexacyanoferrate(III) to hexacyanoferrate(II), nitrobenzene to aniline, sulfurous acid to sulfur, and diarsenic trioxide to arsine (57).

As a result of their experiments, they became convinced that the zinc portion of the couple reduced the water, that the resulting hydrogen became occluded in the copper, and that this occluded hydrogen was responsible for the observed reductions (56). In support of this model they compared the reducing abilities of the couple with those of hydrogen occluded in platinum, palladium, copper and porous carbon, concluding that (57):

... the increased power of the hydrogen is due to its condensed condition [upon occlusion in the metal], while the observed differences between the action of the different combinations result from variations in this respect, and perhaps also from the more or less firm hold which the metal has upon the gas.

The above chemical changes are effected more or less perfectly by nascent hydrogen. But this hydrogen in every case is set free in contact with, or in very close proximity to a metal, which in virtue of the power known to be possessed by such solids very probably condenses and fixes some of this gas. It may therefore be conceived that the activity of the hydrogen under these circumstances is but the consequence of its intimate association with the metals, or, in other words, of its being in the occluded condition.

This hypothesis is perhaps the most satisfactory when it comes to explaining the known facts about chemically and electrochemically generated nascent hydrogen in the liquid phase. It explains the dependency of the reducing power of the hydrogen on the method of its preparation (variations in the ability of the metals to adsorb or occlude hydrogen), the absence of a soluble, diffusible, active intermediate in the solutions (it is really a surface reaction at the metal or electrode), and the problems with reproducibility and sensitivity to contamination (both highly characteristic of surface reactions).

Of course, Gladstone and Tribe were necessarily vague about the exact nature of the chemical species corresponding to the occluded hydrogen, though at one point they did draw an analogy with the reducing ability of copper hydride. Indeed, this question is still a subject of debate among chemists (59).

In 1927 Joseph Mellor drew attention to the close relationship between this subject and the problem of the hydrogen overpotential in electrochemistry (60). In general, metals with low overpotentials tend to strongly occlude hydrogen, and to act as effective catalysts for the recombination of hydrogen atoms into dihydrogen gas, the hydrogenation of alkenes, etc. (61).

A close examination of these five theories - the free-atom, the nonequilibrium, the allotropic, the polar, and the occlusion - shows that, in spite of their apparent diversity, they actually

have some fundamental points in common. In all five cases, one is postulating that the generation of a gas, X, either chemically, via the reaction of A and B, or electrochemically, proceeds through a short-lived intermediate, I, which, upon further reaction with more B, gives the final product:



If this intermediate is generated in the presence of another chemical species, D, capable of reacting with it to form an alternative product Z



then one has an example of nascent reactivity. These common features might be characterized as belonging to a generalized "active intermediate" model of the nascent state.

Since the intermediate, I, is presumably more endoenergetic than the product gas, X, this means that reaction 18 will be thermodynamically more favorable than the corresponding reaction between X and D:



and will necessarily entail a change in its kinetics as well. The only disagreement among the five theories is over the precise nature of the intermediate.

This model further implies that nascent activity can be manipulated by controlling the relative rates of reactions 16-18. Increasing the rate of reaction 17 will increase the rate of gas evolution and decrease the amount of product Z, whereas decreasing its rate and/or increasing the rate of reaction 18 will increase amount of product Z. This fact was explicitly recognized by several investigators in the 19th century.

Thus, Gladstone and Tribe tested this hypothesis in their study of the reduction of nitric acid during electrolysis, postulating that the failure to observe the production of dihydrogen at the cathode in the case of concentrated acid solutions was due to the fact that reaction 18 consumed all of the intermediate (occluded hydrogen in their theory) in the production of NO and NO<sub>2</sub>, leaving none for the generation of dihydrogen gas via reaction 17 (58). By increasing the rate of reaction 16 for a given acid concentration, via an increase in the rate of electrolysis, they were able to provide sufficient intermediate for both reactions 17 and 18 to be observed simultaneously.

Likewise, Thomas E. Thorpe, in his 1882 study of the reduction of ferric salts with nascent hydrogen generated by various acid-metal systems, wrote (23, 62):

Any condition which increases the rapidity of the evolution of the hydrogen, without to an equal degree increasing the chance of contact

of the hydrogen-atom [i.e., the nascent intermediate, Thorpe accepted the free-atom model] with the ferric sulphate, diminishes the proportion of hydrogen which does work as a reducing agent. By increasing the amount of free acid, we increase the rate at which the hydrogen is evolved, without to an equal degree increasing the molecular movement of the ferric sulphate, and hence an increased amount of hydrogen escapes ... by keeping the amount of acid constant and heating the liquid, we increase the chances of contact between the ferric sulphate and hydrogen-atom, and accordingly obtain an increased reduction ... By diluting the ferric sulfate solution the chances of contact between the hydrogen [atom] and ferric sulphate are of course diminished, and hence more of the hydrogen escapes in the free state.

Given that the active intermediate model requires an alteration in both the kinetics and thermodynamics of the reactions, the modern chemist cannot resist asking which of these factors is the most important in explaining the enhanced reactivity of the nascent state. There were in fact proponents of both the thermodynamic and kinetic points of view during the 19th century, though both necessarily rejected the active intermediate models, which imply the simultaneous alteration of both.

In a series of papers and books published in the late 1860's and 1870's, the French thermochemist, Pierre Eugène Marcelin Berthelot, argued that the nascent state did not exist and that the phenomena usually attributed to its operation could be explained in terms of thermochemistry alone (14, 63). All of his examples involved the use of the nascent state to explain the indirect synthesis, by means of displacement reactions, of endoenergetic compounds that could not be directly synthesized from the elements themselves. A case in point is the synthesis of dichlorine oxide outlined earlier in equations 9-12. The difference between the direct synthesis and indirect synthesis, Berthelot argued, had nothing to do with nascent intermediates but was due to the fact that in the latter case the production of the unstable endothermic product (Cl<sub>2</sub>O) was accompanied by the simultaneous production of a highly exothermic by-product (HgCl<sub>2</sub>) and this made the overall process exothermic and hence thermodynamically favorable.

In 1918 Alexander Smith pushed Berthelot's observations on the imaginary role of the nascent state in indirect versus direct syntheses a step further, sarcastically remarking that (74):

... Since hydrogen and chlorine do not unite in the cold, when sulfuric acid and common salt give hydrogen chloride, to be consistent we must suppose that nascent hydrogen and nascent chlorine were formed and combine. In other words, every union of two elements, other than direct union, must be explained by nascent action, although in double decomposition this logical necessity is uniformly overlooked.

Nevertheless, not everyone was satisfied by Berthelot's ap-



approach. Adolphe Wurtz, for example, characterized Berthelot's arguments as "useless", largely because they were not mechanistic in nature and therefore failed to provide a "natural" explanation (17). More telling were the comments of Tommasi in 1880 (64):

M. Berthelot ... not long ago pretended that he was the first to give a rational theory of the nascent state ... But if you open these two volumes, you will not be able to find a single sentence which makes one even suspect why nascent hydrogen is more active than ordinary hydrogen.

In other words, though Berthelot had given a proper thermodynamic rationale of the indirect synthesis of endothermic compounds, these cases had represented questionable extensions of the nascent state concept in the first place, and he had completely ignored the competitive gas-generating reactions which had originally given rise to the concept.

Indeed, the inappropriateness of Berthelot's rationale in the case of these competitive systems becomes apparent if one evaluates the thermodynamics of most of the classic reduction reactions involving nascent hydrogen, for one quickly discovers that the corresponding reductions with normal dihydrogen gas are all thermodynamically feasible at room temperature. This is true, for example, of the reduction of chlorate to chloride, nitrate to ammonia, diarsenic trioxide to arsine, nitrobenzene to aniline, iron(III) to iron(II), permanganate to manganese(II), etc. Thus our failure to observe these reactions must be kinetic rather than thermodynamic in nature, and a favorable change in the reaction kinetics is both necessary and sufficient to produce an observed reaction at room temperature. Though a change in mechanism may also alter the reaction thermodynamics, this is not a requirement.

The concept of a purely kinetic rationale of the nascent state brings us finally to our last model. This was proposed by Wilhelm Ostwald in 1902, almost as a passing thought, in his

famous *Lehrbuch* (65). He pointed out that, when initially formed, small submicroscopic bubbles must be under enormous pressure due to the high surface tension of the surrounding water. At molecular dimensions, a newly formed bubble with a diameter of  $10^{-7}$  cm would contain gas under a pressure of approximately 15,000 atmospheres, and this increased pressure, via the law of mass action, might well account for the enhanced reactivity of so-called nascent hydrogen. Today we would also emphasize the importance of the increased surface area between the smaller bubbles and the surrounding liquid.

This latter aspect was further pursued by the Greek chemist, Constantine Zenghelis, in a series of papers published between 1920 and 1921 (66-68). He created superfine gas bubbles by

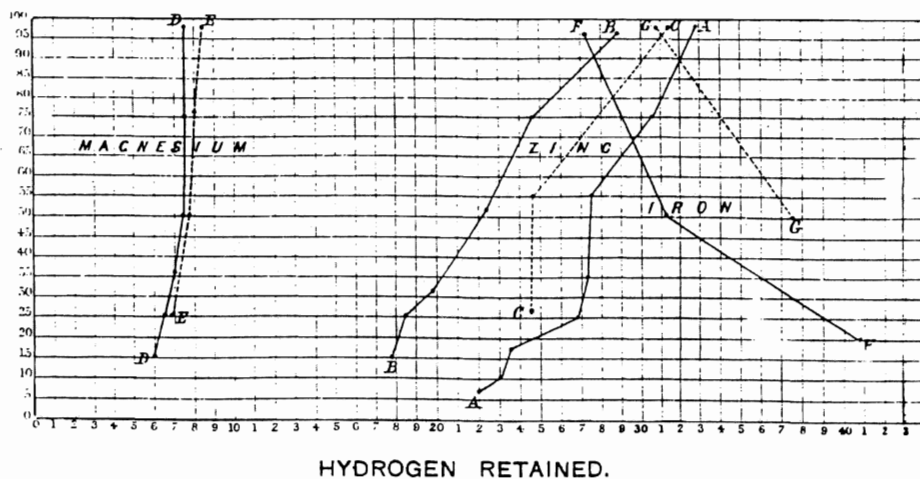
forcing the gases through fine filter paper or through dialysis membranes. When this was done in solutions of various reactants, he obtained many of the reactions usually attributed to the nascent state. In the case of dihydrogen, these included the reduction of chlorate to chloride, nitrate to nitrite, and mercury(II) to mercury(I).

In the case of mixtures of dihydrogen and dinitrogen, he obtained ammonia, and in the case of carbon dioxide, a variety of reduction products, including formaldehyde. Zenghelis concluded that (68):

We believe that on the basis of the preceding results we must reject the generally received hypothesis which attributes the considerable activity of certain gases in the nascent state to the excess energy of free atoms. On the contrary, we believe we have demonstrated that this activity is due to the extreme subdivision of the active masses in contact.

Using this theory to account for the dependency of the activity of the nascent hydrogen on the nature of the metal-acid system used to generate it, would require that different metals nucleate different size bubbles, a factor that should, in turn, depend as much on the physical roughness of the surface as on the

CURVES SHOWING  
REDUCING POWERS OF DIFFERENT METALS.



T. E. Thorpe's curves of hydrogen evolution versus reduction of iron(III) using nascent hydrogen generated by various acid-metal systems

chemical nature of the metal itself.

Some anticipation of the kinetic rationale was already present in the occlusion theory. The enormous decrease in volume accompanying the occlusion of hydrogen in palladium had been noted by Thomas Graham as early as 1868 (69-71). Indeed, he calculated that the density of the hydrogen went from a value of 0.0895 g/mL in the gas phase to a value of 0.733 g/mL in the metal. Chemists had long speculated that hydrogen was an analog of the alkali metals and, under this extreme condensation, Graham felt that it had in fact become a metal, for which he proposed the name *hydrogenium*. He also felt that the proper view of occluded hydrogen in palladium was that it was a metallic alloy of platinum and hydrogenium.

The idea that the increased concentration of occluded hydrogen versus gaseous hydrogen was partly responsible for its increased activity was already hinted at in the statement from Gladstone and Tribe quoted earlier and a similar idea was suggested by Edward Willm in 1873 (72). This is, of course, a purely kinetic effect only if one assumes that the occluded hydrogen is still diatomic and does not chemically interact with the metal lattice - fine points on which the 19th century chemist was understandably vague.

Having come to the end of our historical survey, what can be said about the fate of the term nascent state? Despite its apparent renaissance among chemists in the field of chemical reaction dynamics, it is unlikely that the term will return to the introductory textbook. With the demise of the free-atom model, it no longer has value as a minor piece of evidence for the polyatomic nature of elemental molecules and, though one might argue that historically it represents one of the first sustained attempts to deal with reaction mechanisms, its status as a well-defined concept in chemical kinetics is even more tenuous.

Our survey points to the fact that nascent activity is primarily a kinetic phenomenon and that it basically subsumes any change in reaction conditions that will favorably alter the kinetics of an otherwise thermodynamically allowed reaction. The precise nature of this alteration may vary from one system to the next. Thus the occlusion or gas adsorption model seems best suited to describe nascent activity in the case of gases generated electrochemically or via heterogeneous reactions in the liquid phase; the nonequilibrium model seems best suited to the vibrationally excited products formed in molecular cross-beam studies, and the gas bubble model for activation of absorbed gases in porous carbon. In short, there is no single explanation of nascent activity and hence no well-defined nascent state. Even if one accepts the generalized active intermediate model, the fact remains that similar active gases can be stored in platinum and porous carbon or produced by means of superfine bubbles and that the literal meaning of the term nascent no longer applies under these conditions.

It is of interest to compare these conclusions with those of earlier writers. In a memoir written in 1870, Henri Sainte-

Claire Deville objected to the use of the term "state" in conjunction with a transient species which, by its very definition, could not be isolated and assigned definite properties (73). Muir, on the other hand, attempted to weigh both the pros and cons of the concept in his 1884 text (1):

The term "nascent action" has probably been at once helpful and harmful to the progress of chemistry. By classing under a common name many phenomena that might otherwise have been lost in the vast mass of fact with which the science has to deal, the expression has, I think, done good service; but in so far as its use has tended to prevent investigation - for it is always easier to say of any unusual reactions, "these are of nascent action" than to examine carefully into their course and conditions ... the use of the expression has, I think, been unfavorable to the best interests of chemical science.

Indeed, examples of its use as a chemical scapegoat are not hard to find (78-79).

But it was Smith, in the 1918 edition of his popular *Introduction to Inorganic Chemistry*, who came closest to our own conclusion when he wrote (74):

The term nascent hydrogen is used in different senses, in a very confusing way. (1) It may mean nascent, literally, that is newly born or liberated. (2) It is also used to mean different-from-ordinary, or, in fact, an allotropic form of hydrogen. (3) It is often used to mean one particular allotrope, namely, atomic hydrogen. (4) It is used ... to mean hydrogen activated by contact with a metal. (5) Finally, its activity is explained as being due to the larger amount of free energy contained in zinc plus acid plus reducing agent, as compared with the free energy contained in free hydrogen plus reducing agent ... The word nascent is, of course, a misnomer, excepting in connection with [usage] 1.

Finally, in closing, we might briefly note a positive example of the use of the nascent state concept. In 1882 the German chemist, Moritz Traube, successfully used it to unravel the structure of hydrogen peroxide (75-76). Previous to his work, chemists had viewed this compound as oxygenated water. In contrast, Traube insisted that it "was not an oxidation product of water ... but rather a compound of an undamaged molecule of oxygen with two added hydrogen atoms". This he proved by comparing the products obtained by passing dihydrogen gas over nascent oxygen generated at the anode, via the electrolysis of water, with those obtained by passing dioxygen gas over nascent hydrogen generated at the cathode. Assuming the free-atom interpretation of the nascent state, Traube reasoned that, if the traditional view of hydrogen peroxide was correct, then it should be produced at the anode via the reaction:



whereas if his interpretation was correct, it should be produced at the cathode via the reaction:



In fact, only water was produced at the anode, whereas a small amount of hydrogen peroxide was formed at the cathode, thus both verifying Traube's hypothesis and providing a sobering reminder of how incorrect assumptions (i.e., the free-atom interpretation of the nascent state) can sometimes lead to correct results.

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*William B. Jensen holds the Oesper Position in Chemical Education and the History of Chemistry at the University of Cincinnati, Cincinnati OH, 45221. He is especially interested in the history of 19th and early 20th century inorganic and physical chemistry.*

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#### BOOK NOTES

*Life of a Scientist*, Robert S. Mulliken, Springer-Verlag, New York, NY, 1989. xv + 256 pp. Hard Cover (Typeset), \$49.50.

Edited by B. J. Ransil, this posthumously published autobiography covers Mulliken's life up to 1983 and is supplemented by a complete bibliography of his publications, a chronological summary of his life, and a good selection of high quality photographs. The book's subtitle, "An Autobiographical Account of the Development of Molecular Orbital Theory", incorrectly suggests that it is largely a technical account of the development of Mulliken's scientific work, when in fact the volume is full of human interest, though the brevity of Mulliken's comments often leave the reader wishing for more detail. In this respect, the book forms a sharp contrast with the "scientific autobiography" written several years ago by Mulliken's contemporary, John Slater, whose account was virtually devoid of personal information and was in many ways a final plea for the superiority of his X $\alpha$ -SCF method for the computation of molecular properties,

Short, but tantalizing remarks on Mulliken's philosophical

and aesthetic views appear, almost without warning, in the middle of discussions of more mundane matters. Typical examples include his passing comments on his mental state shortly after graduation from MIT in 1917 with a B.S in chemical engineering:

I decided that life rationally considered seems pointless and futile, but it is still interesting in a variety of ways, including the study of science. So why not carry on, following the path of scientific hedonism? Besides, I did not have the courage for the more rational procedure of suicide ...

or his inquiry about a Swedish spectroscopist named Heurlinger during his first trip to Europe in 1925:

... whose university dissertation in 1919 showed great promise for an understanding of molecular spectra, but [I] learned that he had unfortunately become insane. I was extremely sorry to learn this, but thought I could understand why it had happened, because even before the new flow of insight offered by quantum mechanics, there was already such an overwhelming vista of exciting possibilities of new research and understanding using the old quantum theory.

Nevertheless, in keeping with the subtitle, the main thrust of the narrative is the evolution of Mulliken's scientific thought. This is presented as a logical sequence stemming from his doctoral work, done under William D. Harkins at the University of Chicago, on the separation of isotopes by distillation. This led, in turn, to an interest in isotope effects on the electronic spectra of diatomic molecules, to an interest in understanding molecular band spectra in general, to the concept of molecular orbitals and, finally, to the interpretation of chemical bonding in terms of these orbitals.

Since Mulliken's role as the cofounder, along with Friedrich Hund, of the method of molecular orbitals is well known, it is hardly necessary to emphasize the relevance of this autobiography to all chemists having an interest in the development of 20th century chemical thought. It is, however, worth noting that Mulliken, like Slater, inhabited a borderline world which in many ways overlapped more strongly with the physics community than with the chemical community. As a consequence, upon reading this book, the average chemist, unfamiliar with the world of spectroscopy, will encounter a succession of names largely foreign to the conventional chemical pantheon of heroes. Given the present impact of MO theory on the daily thinking of synthetic organic chemists, this fact is a valuable historical example of how strongly one field of scientific activity can impact on another - however unrelated they might at first appear to be.

*Aleksandr Porfir'evich Borodin, A Chemist's Biography*, Springer-Verlag, New York, NY, 1988. xii + 171 pp. Hard Cover (Typeset), \$79.50.

This is an English translation by Charlene Steinberg and George B. Kauffman of a Russian biography of Borodin written in 1950. Although it is nice to have this material available in English, there are some definite problems with the decision to simply translate, rather than revise, the original Russian edition. Among these is the difference in the intended markets for the two versions. The original Russian edition was apparently targeted at the general reader and, while emphasizing Borodin's chemical career, rather than his musical career, did not go into great detail concerning the chemistry involved, as only a few formulas appear in the body of the text. However, both the printing of the English translation by a major publisher of technical and scientific books, and its pricing, suggest that the intended targets for the English version are professional chemists and chemical libraries. The translators have attempted to compensate for this lack of technical detail through the addition of extensive notes at the end of each chapter, but this necessitates a continuous thumbing back and forth between the text and the notes in order to unravel the chemistry being discussed, and it would have been an improvement if much of this material had been directly incorporated into the body of the text.

True to its original publication date, the book also reflects Russian political concerns of the 1950's. The usual obligatory reference to the infinite historical and political wisdom of Lenin is present, as well as a preoccupation with German plots to rob Russian scientists of their just due. This is particularly apparent in the discussion of Butlerov's contributions to organic structure theory, and here the translators have failed to balance the situation in their notes. Though several articles dealing with the question of Butlerov's contributions have appeared since the publication of the original Russian edition, none of them are referenced by the translators. Instead, they reinforce the German plot scenario by citing the current nonsense literature on Kekulé's supposedly fabricated dream-accounts of the origins of his own ideas on structure. Of course, it can be argued that by not revising the text so as to remove these anachronisms, the translators have reproduced an interesting "period piece".

The physical production of the book is excellent and is up to the usual standards one has come to expect of Springer which, in the reviewer's opinion, produces some of the most attractively printed technical literature on the market today. The text is supplemented by a complete list of Borodin's chemical papers, his musical compositions, an appendix of translated documents, and a bibliography of other biographical writings dealing with his life. The book is also extensively illustrated.

*Electrochemistry, Past and Present*, John T. Stock and Mary Virginia Orna, Editors, American Chemical Society, Washington, DC, 1989. ix + 606 pp. Hard Cover (Camera-Ready Copy), \$89.95.

This book is based on a three-day symposium organized by the volume's editors at the 195th National Meeting of the American Chemical Society in Toronto, Canada, on 5-11 June 1988. The 39 papers in the volume are subdivided into those dealing with the foundations of electrochemistry (12 papers), those dealing with organic and biochemical electrochemistry (four papers), those dealing with electroanalytical chemistry (16 papers), and those dealing with industrial electrochemistry (seven papers). Given the fact that most academic electrochemists now view themselves as analytical chemists, the section on the history of the more recent developments in electroanalytical chemistry is perhaps the most valuable part of this book and nicely supplements the earlier symposium volume on the history of electrochemistry, edited by George Dubpernell et al. and published by the Electrochemical Society in 1978, which dealt largely with the historical foundations of basic electrochemistry and the history of industrial electrochemistry.

As with all volumes of this sort, there is a certain unevenness in both the quality and intent of the papers. Several are really current technical reviews, rather than historical retrospects, and at least one is a plea for an alternative interpretation of electrochemical mechanisms. Nevertheless, it should prove valuable to future historians as a starting point for characterizing the state of academic electrochemistry in the 1960's and 1970's.

## FROM THE CHAIR

The dawning of a new decade seems to inspire both individuals and institutions to reassess themselves. Where are they now? Where have they been? Where are they going? HIST is no exception and the evaluation is a rewarding one. In 1981, according to the ACS office records, our Division had 303 members. Currently, the total for all HIST members is at or over 800. In 1981, we ranked 30th of the (then) 31 divisions in size. Currently we are 28th of 32, three steps higher. All of this is most satisfying, but I believe we are still getting our act together and an even more glorious future awaits. HIST has a rather unique position among the Divisions - it is hard to imagine any chemist as not having an interest in at least some aspect of the Division's territory. (Probably only CHAS and PRFR can also make this claim). So why aren't we the single largest Division with, say, 10,000 members? Partly, I believe, because we have an image problem. We are viewed as grey-bearded types occupied with decaying volumes in musty rooms, as nattering on about "obsolete" matters, or as "liberal arts"-oriented types who really don't think like chemists, even to the cluttering up of the teaching of chemistry with non-essential background. In all such cases, we are perceived as having no relevance to the practice of modern chemistry. Obviously, we have our work cut out for us. It is not enough



Jack Stocker  
1990 Chair

to quote the adage that those not knowing their history are doomed to repeat its mistakes. Some don't believe this and, mostly, nobody is listening. So obviously, our first goal is to get their attention. Now here's my plan...

I propose we work on two fronts: 1) Activities likely to gain wide attention, i.e., ones that will provoke conversation and advertise our existence, and 2) Projects that have personal utility to a large segment of our colleagues (probably most notably in the education area). More specifically, for the "activity", the Division might undertake to prepare a utilitarian, stripped-down handbook that could be entitled "Genealogy for Chemists", permitting the bulk of ACS members to trace their chemistry roots. This softcover handbook would be published as a Division undertaking and sold as inexpensively as possible. An informal evaluation suggests a 300-page compendium, possibly 3,000 names, the listing running to the time of WWII, and (probably limited to) those Ph.D. academicians who have trained chemists. Such a handbook would certainly provide hours of entertainment for our colleagues, would certainly lead to write-ups/reviews in a number of places, and could even provide useful data for seeking new and interesting correlations.

For projects of "personal utility", I would like to propose the following. In each of the more traditional areas of training, (i.e., general, organic, analytical, physical, inorganic and, possibly, bio-) the Division would undertake the preparation and publication of inexpensive, softcover, modest-sized (i.e., 200 pages or less) reference books providing brief background details for the significant names in that particular area of chemistry (possibly one per page). In addition to all the usual items of importance, i.e., date of birth, country, where educated, significant professional positions, major contributions, each entry would feature up to half-a-dozen brief items of

interest from that chemist's/scientist's background, preferably of a personal nature, that would make him/her more real and/or more interesting. For example: he had nine children, or was married four times, or fought in a particular war, or believed in spiritualism, or spent five years in jail, etc., etc.

The collection for general chemistry would appear first; subsequent work might depend on the reception of this first effort. Later books might be promoted through some sort of tie-in with the appropriate divisions, i.e., the Organic Division could market the "organic" one to its members at some profit to the Division per copy sold. The project would permit every chemistry teacher across the country to enliven his/her lectures by the simple investment of a very few minutes before class times. The service would be noted as coming to the teacher courtesy of HIST.

Obviously, all of the above makes certain assumptions. Do we want to have a significantly larger Division? Is it appropriate or desirable to devote the Division's energies and resources to what are, at best, marginally scholarly activities. I suggest "yes" to both questions - greater size and recognition can lead to greater influence and I believe all Division members share at least some measure of fervor in bringing to/reminding our colleagues of the value of HIST's activities. And it should be noted in defense that the proposed activities are clearly service-oriented.

So I should like very much to hear from HIST's members about their reactions to the above. Support? Hostility? Apathy? (i.e., your not writing could be so interpreted!) Are you interested enough to consider working on any of the several projects mentioned? Please do write. In any case, stay tuned for further developments.

*Jack Stocker, University of New Orleans*

## AWARDS

### The Dexter Award

The 1990 Dexter Award for outstanding accomplishment in the history of chemistry has been awarded to Dr. Colin A. Russell of the Open University. The award, which consists of a cash prize of \$2000 and an engraved plaque, will be presented at the Fall National ACS Meeting in Washington, D.C.

Born in 1928, Dr. Russell received his B.Sc. degree in chemistry in 1949 and a postgraduate degree in education in 1950. These were followed by a M.Sc. in the history and philosophy of science in 1958 and by Ph.D. and D.Sc. degrees in 1962 and 1978, respectively. After a 20-year career teaching chemistry, Dr. Russell became Senior Lecturer in the History of Science and Technology at the Open University in 1970 and Professor in 1981. Author or coauthor of ten books (including two chemistry texts) and more than 50 articles on chemistry



Dr. Colin Russell

and the history of science, Dr. Russell is perhaps best known for his classic study, *The History of Valency*, which appeared in 1971. He is currently in the process of completing the second volume of a biography of the 19th century English chemist, Edward Frankland.

The Division would at this time also like to solicit nominations for the 1991 Dexter award. Nominations should include a complete vita for the nominee, consisting of biographical data, educational background, awards and honors, publications, and presentations and other services to the profession; a nominating letter summarizing the nominee's achievements in the field of the history of chemistry and citing unique contributions which merit a major award; and at least two seconding letters. Copies of no more than three publications may also be included, if available. All nominations should be sent to Dr. John Heitmann, Secretary, The Division of the History of Chemistry - ACS, Department of History, The University of Dayton, Dayton, OH 45469 by 1 January 1991. It should be emphasized that the award is international in scope and that nominations are welcomed from all quarters. Previous winners have included historians and chemists from Germany, France, Holland, Hungary, and Great Britain.

### The Outstanding Paper Award

The 1990 Outstanding Paper Award has been given to Dr. James J. Bohning of the Beckman Center for the History of Chemistry for his paper, "The 1893 World's Congress of Chemists: A Center of Crystallization in a Molecular Mélange", which appeared in the Spring 1989 issue of the *Bulletin* (1989, 3, 16-21). The award, which consists of \$100, a certificate, and \$150 worth of books from Reidel, will be presented to Dr. Bohning at the Fall National ACS Meeting in Washington, D.C.

### The 1990-1991 Edelstein Fellowship

The 1990-1991 Edelstein Fellowship in the History of Chemical Sciences and Technology has been awarded to Dr. William H. Brock, Director of the Victorian Studies Center of the University of Leicester. Dr. Brock will divide his fellowship year between the Beckman Center for the History of Chemistry in Philadelphia and the Edelstein Center for History and Philosophy of Science, Technology and Medicine in Jerusalem.

### EVENTS OF INTEREST

- \* The Division notes the death of William J. Wiswesser on 16 December 1989 at the age of 75. Internationally known for the Wiswesser Line Notation and for his other activities in the field of chemical information, Wiswesser served as Chair of the Division in 1979. His most recent interests in the history of chemistry focused on the contributions of the Austrian physicist, Johann J. Loschmidt (1825-1895), to valence notation.
- \* The Division has received a \$1000 bequest from the estate of the late Florence Wall, who passed away on 2 October 1988 at the age of 93. Beginning her chemical career in 1913 as a high school chemistry teacher, Wall became a well-known expert on the chemistry of cosmetics. She was active in the Division for many years and published numerous papers on the history of chemistry, with subjects ranging from the career of Wilhelm Ostwald to the role of women in science.
- \* The Society for the History of Alchemy and Chemistry has announced that its Partington Prize of 100 pounds will be awarded on 30 April 1991 for the best original and unpublished essay on any aspect of the history of alchemy or chemistry. The competition is open to anyone with an interest in the history of chemistry who has not reached the age of 30 by the closing date of 31 December 1990. Essays must be submitted in English and be typewritten, double spaced, and single sided. They should not exceed 5000 words and should be fully documented following the conventions used in recent issues of *Ambix*. Though the prize-winning essay will be considered for publication in *Ambix*, publication cannot be guaranteed. Entries should be sent to Dr. A. K. Newmark, Hon. Secretary, The Society for the History of Alchemy and Chemistry, The Science Museum, South Kensington, London, SW7 2DD, England, with the words "Partington Prize" clearly written on the envelope. Each entry should contain a separate title page giving the author's name, institutional address and date of birth. This will not be given to the judges. The decision of the judges will be final and the Society reserves the right to divide the prize between competitors of equal merit or not to award a prize should none of the entries be deemed of proper quality. Essays will be returned after the announcement of the winner.
- \* Dover books has recently issued quality paperback reprints of three classics in the history of chemistry: the 1939 Macmillan edition of James Partington's *A Short History of Chemistry*; the 1942 English translation of Vannoccio Biringuccio's 1540 classic on metals and metallurgy, the *Pirotechnia*; and the 1957 Penquin edition E. J. Holmyard's *Alchemy*.
- \* The Société d'étude de l'histoire de l'alchimie, 45 rue Saint-Maur, 75001 Paris, has begun a new journal entitled *Chrysopoeia*, which is devoted to the study of alchemy in both its "spiritual" and "empirical" forms. The journal is now in its third volume and can be obtained from Gutenberg Reprints, 30 rue de Savoie, 75006 Paris.
- \* A half-day symposium on the history of the chemistry set was held at the 11th Biennial Conference on Chemical Education at the Georgia Institute of Technology in Atlanta, Georgia, on Thursday afternoon, 9 August 1990. The speakers included George Gilbert of Denison University, William Jensen of the University of Cincinnati, David Katz of the Community College Philadelphia, and David Rhees of the American Philosophical Society. A collection of old chemistry sets was also on display during the session.
- \* The Oesper Collection in the History of Chemistry of the University of Cincinnati is sponsoring an exhibit entitled "From Colorimetry to Spectrophotometry: 1890-1940". The display, which will run from June 1990 to February 1991, is based on portions of the extensive spectrophotometer collection recently donated to the Oesper Collection by Dr. M. G. Mellon of Purdue University. A description of Dr. Mellon's collection will be featured in the "Chemical Artifacts" column in the Winter issue of the *Bulletin*. For additional information, contact Dr. William B. Jensen, Oesper Collection in the History of Chemistry, Department of Chemistry, ML 172, University of Cincinnati, Cincinnati, OH, 45221, (513) 556-9308.
- \* Dr. Aaron J. Ihde's long-awaited history of the Department of Chemistry of the University of Wisconsin, *Chemistry As Viewed from Bascom Hill*, has just been published and can be obtained for the price of \$25.00 by writing to the Department of Chemistry, University of Wisconsin, Madison, WI 53706. A detailed review will appear in the Fall issue of the *Bulletin*.
- \* Travel grants are available from the Beckman Center for the History of Chemistry to enable interested individuals to visit Philadelphia to make use of the Othmer Library, the Edgar Fahs Smith Collection, and other associated facilities. The grants, which may be used for travel, subsistence, and copying costs, will not normally exceed \$500. Applications should include a vita, a one-paragraph statement on the research proposed, a budget, and the addresses and telephone numbers

of two references. Deadlines are 1 February for grants covering the period April-June; 1 August for the period October-November, and 1 November for the period January-March. Send applications to Dr. Mary Ellen Bowden, Assistant Director of Programs, Beckman Center for the History of Chemistry, 3401 Walnut Street, Philadelphia, PA 19104-6228, (215) 898-4896.

\* The Oesper Collection in the History of Chemistry of the University of Cincinnati is looking for donations of old chemistry texts, photographs, prints and chemical apparatus to add to its collections. Interested parties should contact Dr. William B. Jensen, The Oesper Collection in the History of Chemistry, Department of Chemistry, ML 172, University of Cincinnati, Cincinnati, OH 45221, (513) 556-9308.

\* Applications are now invited for the 1991-1992 Edelstein International Fellowship in the History of Chemical Sciences and Technology. For further information on how to apply, contact the Beckman Center for the History of Chemistry, 3401 Walnut Street, Philadelphia, PA, 19104-6228, (215) 898-4896. The deadline for applications is 31 October 1990.

## FUTURE MEETINGS

### Washington .... 26-31 August 1990

Five copies of 150-word abstract (original on ACS Abstract Form) by 15 April 1990. Title of paper by 1 April 1990.

\* *General Papers*. Contact J. L. Sturchio Corporate Archives, Merck & Co., Inc., P.O. Box 2000, Rahway, NJ, 07065-0900, (201) 594-3981.

\* *History of the Development, Use and Testing of Food Additives* (Cosponsored by CHAL). Contact H. T. McKone, Department of Chemistry, St. Joseph College, W. Hartford, CT 06117, (203) 232-4571.

\* *Chemistry and Crime II*. Contact R. O. Allen, Department of Chemistry, University of Virginia, Charlottesville, VA 22901, (804) 924-3622.

\* *History of Environmental Pollution and Protection in Relation to Federal Regulations*. Contact R. Sarmiento, U.S. Environmental Protection Agency, Analytical Chemistry Section, Bldg. 306, BARC-East, Beltsville, MD 20705, (301) 344-2266.

\* *50th Anniversary of the Discovery of the Transuranium Elements*. (Cosponsored by INOR and NUCL).

\* *True Stories of Small Chemical Businesses*. (Cosponsored by SChB).

\* *Chemistry on Stamps*. (Cosponsored by CHED).

### Atlanta .... 14-19 April 1991

Five copies of 150-word abstract (original on ACS Abstract Form) by 1 December 1990. Title of paper by 1 November 1990.

\* *General Papers*. Contact J. L. Sturchio (see address above).

\* *Michael Faraday - Chemist* (Cosponsored by CHED). Contact Derek Davenport, Department of Chemistry, Purdue University, West Lafayette, IN 47907, (317) 494-5465.

\* *Chemistry in Science Fiction*. Contact J. H. Stocker, Department of Chemistry, University of New Orleans, New Orleans, LA 70148, (504) 286-6852.

\* *History of Synthetic Fibers*. Contact R. B. Seymour, Department of Polymer Science, University of Southern Mississippi, Southern Station, Box 10076, Hattiesburg, MS, 39406, (601) 266-4868.

\* *Emil Fischer: One Hundred Years of Carbohydrate Chemistry* (Cosponsored by CARB).

\* *True Stories of Small Chemical Businesses* (Cosponsored by SChB).

### New York .... 25-30 August 1991

Five copies of 150-word abstract (original on ACS Abstract Form) by 15 April 1991. Title of paper by 1 April 1991.

\* *General Papers*. Contact J. L. Sturchio (see address above).

\* *History of Steroid Synthesis*. Contact L. Gortler, Department of Chemistry, Brooklyn College, Brooklyn, NY 11210, (718) 780-5746 or J. L. Sturchio (see address above).

\* *A Century of Chemistry in New York (Commemorating the Local Section Centennial)*. Contact J. Sharkey, Department of Chemistry, Pace University, Pace Plaza, New York, NY 10038, (212) 488-1502.

\* *Chemistry and Crime III - Forensic Methods: Past, Present and Future*. Contact S. M. Gerber, Color Consultants, 70 Hillcrest Road, Martinsville, NJ 08836, Phone (201) 356-4721; or R. Saferstein, New Jersey Forensic Laboratory, P.O. Box 7068, West Trenton, NJ 08825, (609) 882-2000, Ext. 2692.

\* *Social Responsibilities of Scientists*. (Cosponsored by CHED).

\* *True Stories of Small Chemical Businesses* (Cosponsored by SChB).

### San Francisco .... 5-10 April 1992

Five copies of 150-word abstract (original on ACS Abstract Form) by 1 December 1991. Title of paper by 1 November 1991.

\* *General Papers*. Contact J. L. Sturchio (see address

above).

\* *Chemical Genealogy*. Contact P. R. Jones, Department of Chemistry, University of New Hampshire, Durham, NH 03824, (603) 862-1550.

\* *True Stories of Small Chemical Businesses* (Cosponsored by SChB).

Geneva .... 21-22 April 1992 (Tentative)

\* *100th Anniversary of the Geneva Conference*. Organized by J. G. Traynham, Department of Chemistry, Louisiana State University, Baton Rouge, LA 70803, (504) 388-3459.

Chicago .... 22-27 August 1992

Five copies of 150-word abstract (original on ACS Abstract Form) by 15 April 1992. Title of paper by 1 April 1992.

\* *C. K. Ingold and his Influence on the Development of Chemistry in America*. Contact M. D. Saltzman, Department of Chemistry, Providence College, Providence, RI 02918, (401) 865-2298.

Note: The cosponsored symposia will have their primary sponsorships by the divisions so named and the programs will appear under their respective divisional headings.

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\* *Member-at-Large*: Ben B. Chastain, Department of Chemistry, Samford University, Birmingham, AL 35229, (205) 870-2725.

### Coming in Future Issues:

#### *Issue 7 (Fall 1990)*

- \* "The 1990 Oesper Lecture" by Jeffrey L. Sturchio
- \* "The Genesis of Electrogravimetry" by John T. Stock
- \* "The Reception of Hydrogen Bonding by the Chemical Community: 1920 - 1937" by Denis Quane
- \* "J. W. Mellor (1869-1938)" by Fathi Habashi
- \* "Between Two Stools: Kopp, Kolbe and the History of Chemistry" by Alan Rocke
- \* "Harry Jones Meets the Famous" by William B. Jensen

#### *Issue 8 (Winter 1990)*

- \* "The Great Laughing Gas Craze" by William B. Jensen
- \* "Jean-Baptiste Dumas: The Victor Hugo of Chemistry" by Ben B. Chastain
- \* "History of Chemistry and the Education of Teachers" by Aaron J. Ihde
- \* "The Origins of the Chainomatic Balance" by John T. Stock
- \* "The Bicentennial of America's First Chemistry Book" by William D. Williams and Wyndam Miles
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William B. Jensen, Editor  
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University of Cincinnati, M. L. 172  
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