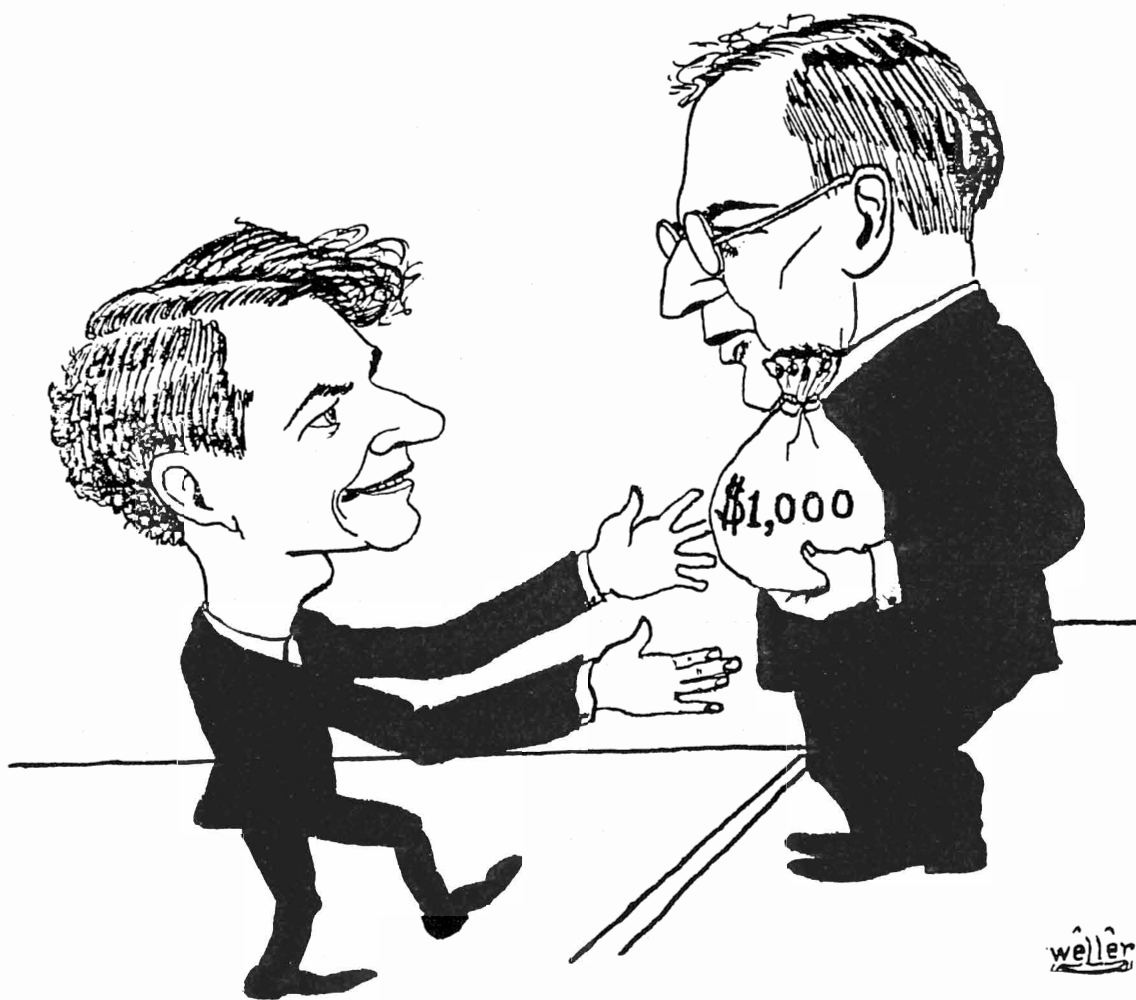


BULLETIN FOR THE HISTORY OF CHEMISTRY

Division of the History of Chemistry of the American Chemical Society

NUMBER 7

FALL 1990



Linus Pauling Receives the 1931 Langmuir Award from Moses Gomberg

BULLETIN FOR THE HISTORY OF CHEMISTRY, NO. 7, 1990

Editor William B. Jensen
 Associate Editor James J. Bohning
 Editorial Assistant I Kathy Orchin
 Editorial Assistant II Elaine Seliskar

The BULLETIN FOR THE HISTORY OF CHEMISTRY (ISSN 1053-4385) is published by the Division of the History of Chemistry of the American Chemical Society in collaboration with the Oesper Collection in the History of Chemistry of the University of Cincinnati and with assistance from the Beckman Center for the History of Chemistry. All changes of address should be sent to the current Treasurer of the Division.

EDITORIAL BOARD

Dr. Jeffrey L. Sturchio
 Merck & Co., Inc.
Dr. Leonard W. Fine
 Department of Chemistry, Columbia University
Dr. O. Theodor Benfey
 The Beckman Center for the History of Chemistry

CORRESPONDING EDITORS

Dr. Peter J. T. Morris
 The Open University, Milton Keynes, England
Dr. Yasu Furukawa
 Tokyo Denki University, Tokyo, Japan

The Cover...

This issue shows a caricature from the 2 September 1931 edition of the *Chemical Bond, Jr.* (Western New York Section of the ACS) of Linus Pauling receiving the 1931 Langmuir Award from Moses Gomberg. Pauling is discussed in this issue by Denis Quane in his article on the reception of the hydrogen bonding concept by the chemical community.

The deadline for the next issue (Winter 1990) is 30 December 1990. An Author's Guide appears on page 37 of this issue.

CONTENTS

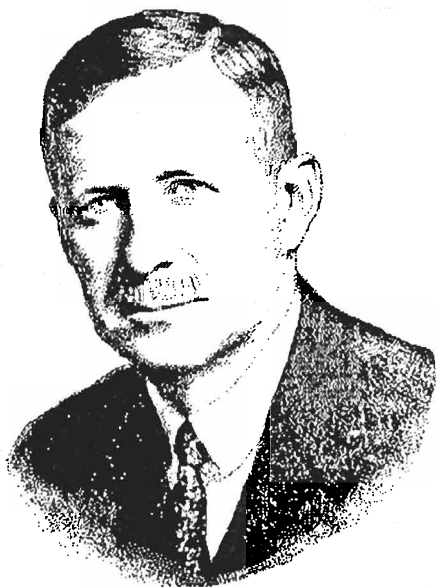
The Reception of Hydrogen Bonding by <i>Denis Quane</i>	3
The response of the chemical community in the period 1920-1937	
Joseph W. Mellor by <i>Fathi Habashi</i>	13
A tribute to the master textbook author of all times	
The Genesis of Electrogravimetry by <i>John T. Stock</i>	17
On the development of the first important application of electrochemistry to chemical analysis	
Between Two Stools by <i>Alan J. Rocke</i>	19
Kopp, Kolbe and the history of chemistry	
Chemical Artifacts by <i>John H. Wotiz</i>	24
The Butlerov Museum at the University of Kazan	
Diversions and Digressions by <i>William B. Jensen</i>	26
Kekulé was vain and van't Hoff was high strung - Harry Jones meets the famous and comments	
Book Notes	33
A history of the petroleum industry, notes on the history of metallurgy, a collection of papers on Joseph Priestley, and a major departmental history	
Divisional News	
Events of Interest	35
Future Meetings	35
From the Editor's Desk	36
Author's Guide	37
Parting Shots by <i>William B. Jensen</i>	38
Reinventing the Hofmann Sodium Spoon	
Future Issues	39
A preview of what's to come	

THE RECEPTION OF HYDROGEN BONDING BY THE CHEMICAL COMMUNITY: 1920-1937

Denis Quane, East Texas State University

It is well known that hydrogen bonding as a generalized concept was first proposed in the literature by Wendell Latimer and Worth Rodebush in 1920 (1). A footnote in their paper credits "Mr. Huggins of this laboratory [who], in some work as yet unpublished, has used the idea of a hydrogen kernel held between two atoms as a theory in regard to certain organic compounds". At the time Wendell Latimer was a lecturer in G. N. Lewis' chemistry department at the University of California at Berkeley, Worth Rodebush, a postdoctoral fellow, and Maurice Huggins, a first-year graduate student, working on his masters degree. The "unpublished work" referred to was actually done a year earlier when Huggins was an undergraduate. He has given several accounts of this work; the most complete and definitive of which appeared in 1980 (2). He begins by describing his reaction to advanced courses in organic and inorganic chemistry taught by Professors T. Dale Stewart and William C. Bray. In these courses the students were introduced to the Lewis theory of chemical bonding (2):

Prof. Stewart and Prof. Bray also discussed some of the *unsolved* problems of chemistry. These problems intrigued me. Could some of them be solved by the application of the Lewis theory, perhaps with modifications? I thought a lot and made crude notes about problem solutions that seemed reasonable to me ... In early May of 1919, as graduation approached, I became worried ... I had not written the term



Worth H. Rodebush



Wendell M. Latimer

paper that Prof. Bray required for his course and time was getting short ... I went to Prof. Bray and asked him if he would accept my crude notes, with a title and my name added. He did so.

Dr. Huggins, in 1978, was kind enough to send me a photocopy of parts of these notes (3). The copy sent me consists of pages numbered 1-7 and 15-17, with page 17 obviously not the end of the document. The clearest indication of what would be called hydrogen bonding today is a diagram on page 17 showing a hydrogen fluoride dimer, with the four atoms arranged in a square, each hydrogen bonded to both fluorines by electron pair bonds. On page 6 an explanation of the keto-enol tautomerism of acetoacetic ester postulates a transition state in which hydrogen is bonded simultaneously by electron pair bonds to a carbon atom and an oxygen atom. There is no indication given that the concept is generally applicable nor is any name given to the idea.

For that matter, Latimer and Rodebush themselves do not actually use the terms "hydrogen bond" or "hydrogen bonding" in their paper. The closest they come is to describe it as "the hydrogen nucleus held between two octets constitutes a weak 'bond'." The first use of the term in the literature was in Lewis' book, *Valence and the Structure of Atoms and Molecules*, in which the section headed "Bivalent Hydrogen" begins (4):

It seems to me that the most important addition to my theory of valence lies in the suggestion of what has become known as the hydrogen bond.

It is hardly surprising that the idea of hydrogen bonding should have occurred independently to several young chemists

at Berkeley at that time. Lewis had been, since 1912, building a strong department and, in doing so, attracted scores of bright young chemists as faculty members and doctoral students (Rodebush and Latimer had obtained doctorates from Berkeley in 1917 and 1919 respectively; Huggins would receive his in 1922). His new ideas on bonding must have been exciting to these young men. The idea of hydrogen bonding would appear to have been a natural outgrowth of the Lewis theory. In hindsight it is somewhat surprising that the idea of the hydrogen bond met with resistance from the older faculty whose teaching had inspired it. Huggins recounts how Prof. Bray commented on his paper (2):

Huggins, there are several interesting ideas in this paper, but there is one you'll never get chemists to believe: the idea that a hydrogen atom can be bonded to two other atoms at the same time.

Latimer has stated that Lewis' first response to the Latimer-Rodebush paper was to suggest that the section on hydrogen bonding be deleted (5).

Accounts of the history of hydrogen bonding in monographs and textbooks leave the impression that the concept was immediately embraced by the chemical community. A statement of George Pimentel and Aubrey McClellan is typical (6):

From these early beginnings welled a rising number of studies of H-bonding. As the common occurrence and importance of this weak bond has become apparent, the volume of work using the concept of a 'shared' proton has grown tremendously and at an increasing rate.

Actually the Latimer-Rodebush paper was almost completely ignored for ten years.

Before discussing the complex history of how the concept came finally to be accepted, some preliminary distinctions must be made. Latimer and Rodebush introduced the concept of hydrogen bonding to explain certain phenomena which were already well known: the anomalous boiling point of water (7), the vapor phase density of hydrogen fluoride (8), the anomalous freezing points (9) and vapor density curves (10) of various liquid mixtures, the basicity constants of ammonia and the substituted amines (11). Certain other phenomena now associated with hydrogen bonding were discovered soon after: the crystal structures of ice (12) and the HF_2^- ion (13,14); and the change in stretching frequencies of the -OH bond (15).

Chemists tended to adopt four attitudes toward these "anomalies": some just determined, as precisely as possible, the physical constants or structural parameters and did not worry about their meaning; some attributed these anomalies to "association", without speculating on the nature of the forces causing the association; some proposed theories about the nature of these forces different from the Latimer-Rodebush concept of hydrogen bonding; and finally, some accepted the Latimer-Rodebush picture, often at second or third hand,



Maurice L. Huggins as a student at Berkeley during the First World War.

although there was little unanimity as to the name to be given the concept. The number of chemists adopting this last attitude was small during the 1920's but became greater during the early and middle 1930's. By 1937 the concept was so widely accepted that it could be mentioned without giving a citation, and the term "hydrogen bond" had also become by far the most common name. In the remainder of this paper I hope to trace the web of influences by which this general acceptance was achieved, using biographical data, citation analysis, and preferred terminology as tools.

The first citation of the Latimer-Rodebush paper in connection with hydrogen bonding was by Irving Langmuir of the General Electric Company in 1921 (16). Langmuir had been instrumental in calling the attention of the chemical community to Lewis' ideas about chemical bonding (17). Rather than accepting the Latimer-Rodebush proposal, Langmuir put forth a competing idea (16):

In double molecules such as H_4O_2 (in ice), H_2F_2 , and in compounds such as KHF_2 , etc., it seems that the hydrogen nuclei instead of forming duplets with electrons in the same atom, form duplets in which the two electrons are in different atoms. The hydrogen nucleus itself thus acts as a bond in such a case. Latimer and Rodebush have made a somewhat similar suggestion in regard to hydrogen nuclei acting as bonds. They consider that the hydrogen nucleus acts on two pairs of electrons: one pair in each of the two atoms... Since the first layer of electrons in all atoms contains only two electrons, it seems probable that the hydrogen in this case also holds only two electrons and that these form the definite stable group which we have termed the duplet.

This idea is similar to the "*liaison monoelectronique*" pro-

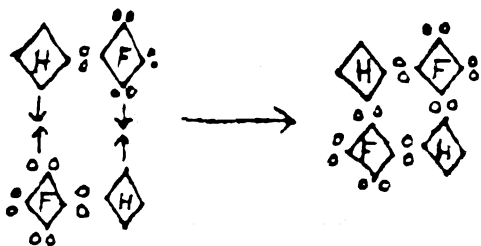
posed, probably independently, by Jean Perrin in 1927 (18) and maintained by some French spectroscopists till 1937.

Maurice Huggins published two papers in 1922 (19) in which he expanded on the ideas presented in his 1919 term paper. Reference to the Latimer-Rodebush paper is made in the second paper, but more in the context of their applications of Lewis theory than specifically to hydrogen bonding. One diagram, the same in both papers, shows a hydrogen atom bonded to two other atoms (in NH_4OH). In both papers this form of bonding by hydrogen is not given any special name but is included, along with examples taken from boron and transition metal chemistry, as an example of what we would now call the coordinate-covalent bond. In the second paper, Huggins gave his first indication that he was proposing this form of bonding by hydrogen as a general concept (19):

It is also this kind of reaction which often produces polymerization and the formation of molecular aggregates (e.g., of H_2O).

Only two research groups, both English, made use of the concept of hydrogen bonding in the early 1920's. The first, that of Thomas Lowry of Cambridge University, began publishing on this subject in 1923 (20). The initial paper from this group is a survey of the evidence for hydrogen bonding. It is much more extensive than that given in the Latimer-Rodebush paper, which is cited, along with Lewis' then forthcoming book. Lowry had corresponded with Lewis in connection with an invitation for the latter to speak at a Faraday Society Symposium in June 1923 on "The Electronic Theory of Valence" (17) and presumably was shown the page proofs on this occasion. The term used to describe this concept (as will be true of future papers by this group during the 1920's) is "coordinated hydrogen".

The second group, that of Nevil Sidgwick at Oxford, published their first paper in 1924 (21). In this paper the "coordination of hydrogen" is used to explain the "abnormal" solubilities and boiling points of certain benzene derivatives. The difference in properties between groups capable of being hydrogen bonded ortho to each other, rather than meta or para, is explained in terms of "chelation". This is what would today be called intramolecular hydrogen bonding. Reference is



The hand-drawn figure from Huggins' student notes showing a hydrogen-bonded hydrogen fluoride dimer (3).

given to the Latimer-Rodebush paper, Lewis' book, and the earlier paper by Lowry.

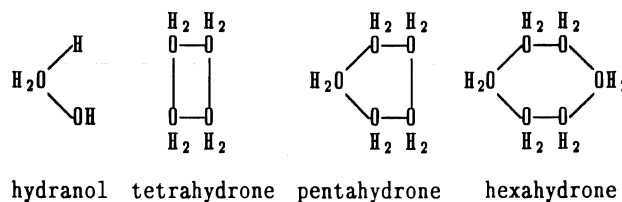
In the meantime, the X-ray determination of the crystal structures of ice (12), NaHF_2 (13), and KHF_2 (14) had been published. Huggins has claimed that (3):

Further evidence [for hydrogen bonding] came from the structure of ice. I predicted that hydrogen bridges would force ice to have a structure in which each oxygen is surrounded tetrahedrally by four others, with the hydrogens on the oxygen-oxygen center lines. In 1922, William H. Bragg showed that oxygen atoms are indeed in such an arrangement.

No such prediction appears in any of Huggins' early published papers, and if it appeared in his term paper, it had to be in a section I have not seen. Bragg himself considered that his work demonstrated that ice was an ionic structure with O^{2-} surrounded tetrahedrally by H^+ ions. Friedrich Rinne and coworkers and Richard Bozorth established that the HF_2^- ion in NaHF_2 and KHF_2 was linear with the hydrogen atom between two fluorine atoms, but no speculation was attempted as to why these atoms adopted such a peculiar arrangement.

The most commonly accepted theory of the structure of water during the 1920's was that of Henry Edward Armstrong. An account of the current version of his theory can be seen in one of his papers from 1923 (22):

Water is a complex mixture, in proportions which vary with the temperature, of the fundamental molecule, *hydrone* (OH_2), with molecules of various polymorphs, perhaps:



As can be seen from the diagrams, the association between molecules is assumed to be by way of O-O bonds.

Armstrong was born in 1848 and retired from teaching at Imperial College of Science and Technology in London in 1911. From 1890 till a few years before his death in 1937, he was considered one of the leading lights of British chemistry. He was noted for his love of controversy and ridiculed the theories of Arrhenius, van't Hoff and Ostwald for what he felt was their lack of consideration for the uniqueness of water (23). Naturally, hydrogen bonding did not escape his scorn. In a letter to *Nature* in 1926 Armstrong wrote (24):

I notice that in the lecture ... which Prof. Lowry gave recently in Paris ... he brought forward certain freak formulae for tartaric acid, in which

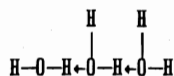
hydrogen figures as a bigamist... I may say he but follows the loose example set by certain Usaenians, especially one E. N. Lewis [sic] a Californian thermodynamiter, who has chosen to disregard the fundamental canons of chemistry - for no obvious reason other than that of indulging in premature speculation upon electrons as the cause of valency.

What may have made Armstrong so vehement was that Lowry had been one of his own students (17).

In 1927, Harris Chadwell, in his review of the molecular structure of water (7), under the heading of "Recent Theories", discussed various modifications of Armstrong's theory. The Latimer-Rodebush theory is mentioned, incongruously, under the heading "X-ray Analysis of Liquid Water". Incongruously, since when Latimer and Rodebush wrote, there were no X-ray data, and Chadwell does not discuss any X-ray evidence for their approach. The paragraph appears to be an afterthought, inserted at the last minute, possibly to meet a referee's comments.

The lecture by Lowry to which Armstrong referred had been recently published in *Nature* (25). The work described had also appeared in an earlier preliminary communication (26). In this work Lowry attributes anomalies in the optical rotary dispersion of tartaric acid to what would now be called intramolecular hydrogen bonding. The citation is to Lewis' book and attributes to Lewis the term "bivalent hydrogen" for the phenomenon. Intramolecular hydrogen bonding is also invoked about the same time by Sidgwick (27), under the name "chelation" to account for anomalous solubility data for β -keto-esters and β -diketones.

In 1927, Sidgwick published his influential book, *The Electronic Theory of Valency* (28), which did perhaps more than even Lewis' book to spread the new ideas about bonding to the chemical community. In it, Sidgwick presented what is essentially Lewis' theory, but developed more systematically and with a greater wealth of examples. He also had the advantage, as an experimental organic chemist, of being able to speak more directly to that audience than did Lewis. In this book, Sidgwick used the term "coordinated hydrogen" for what is now called intermolecular hydrogen bonding and "chelation" for intramolecular hydrogen bonding. The following shows how he represented coordinated hydrogen in water:



As a result of this work, Sidgwick's book (or papers from his group) became the most likely citation given for hydrogen bonding, and the most used term for the phenomenon became "coordinated hydrogen". This is particularly true of British chemists, but also, to a lesser degree, for Americans. For example, John Williams, of the University of Wisconsin, in a 1928 discussion of the relationship between molecular polari-

zation and association, contrasted the "chemical" theory of Sidgwick, in which association consists of the formation of coordinate linkages, with the "physical" theory of Debye, in which association is due to the mutual interaction of dipoles. Williams concluded that the "physical" approach, while promising, is not able to account satisfactorily for all the data (29).

In 1932, James Speakman, of Sheffield University (30), cited a paper by Sidgwick's group in pointing out that the parachor of liquid hydrogen fluoride supports the assumption that it is a liquid in which association results from the coordination of hydrogen. In 1934, H. M. Glass, W. M. Madgin, and F. Hunter, of the University of Durham, cited Sidgwick's book in arguing that the "coordination of hydrogen" accounts for the heat of dissociation of quinoline o-chlorophenoxide in benzene solution (31).

Despite the influence of Sidgwick's book, explicit references to hydrogen bonding, whether under the name of coordinated hydrogen or any other name, are rather infrequent during the late 1920's and the early 1930's. Citing all the papers during this period in which the expected mention of the hydrogen bonding concept is absent, would expand the list of references to unmanageable size. Most of the papers previous to 1934, and a good proportion even for the period 1934-7 listed in the bibliography in Pimentel and McClellan's book (32) are of this character. It will suffice to mention the redetermination of the crystal structure of ice by William Barnes (33), in which he came to the conclusion that "The structure for ice proposed in this paper is practically identical with those of Dennison and Sir William Bragg."

In 1925, Sterling Hendricks and Linus Pauling determined the structures of NaN_3 , KN_3 , and KNCO by X-ray diffraction (34). In the discussion they pointed out that the linear structure of the trinitride and cyanate ions is the same as that of the acid fluoride ion, HF_2^- . For this ion they proposed the structure: $[\text{F} \cdots \text{H} \cdots \text{F}]^-$. No reference is made to the Latimer-Rodebush paper in connection with this structure, nor to Lewis' book, but the structure clearly indicates hydrogen bonding.

Pauling had received his Ph.D. degree from the California Institute of Technology (Caltech) in 1925, and when this paper was written he was a National Research fellow. According to Pauling (35):

I consider Sterling Hendricks to have been my first graduate student. He started his graduate work with Roscoe Dickinson, who then in 1924 went to Europe, leaving Sterling Hendricks in my care. We wrote some interesting papers together.

Not long after Pauling himself went to Europe, where he studied with Sommerfeld and learned about the new developments in quantum theory (36).

In 1928, Pauling wrote the first paper (36) in a series that would lead eventually to his book, *The Nature of the Chemical Bond*. In this paper we see the first use in the literature of the

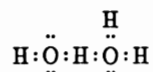
term "hydrogen bond" since it was used in Lewis' book. Pauling, however, had changed his views about the nature of the hydrogen bond since the appearance of the paper with Hendricks, apparently as a consequence of his exposure to quantum theory:

Calculated and observed values of the hydrogen-halogen distances in the hydrogen halides are in agreement only for HF, from which it can be concluded that HF is a polar compound formed from H^+ and F^- and that, as London had previously stated, HCl, HBr, and HI are probably non-polar. The conclusion regarding HF is further supported by the existence of the hydrogen bond. The structure $[:\ddot{F}:H:\ddot{F}:]$ for the acid fluoride ion and a similar one for H_6F_6 are ruled out by Pauli's principle, if the shared pairs are of London's type. The ionic structure $:\ddot{F}:H^+:\ddot{F}:$, in which the proton holds the two fluoride ions together by electrostatic forces (including polarization) is, of course, allowed. This conception of the hydrogen bond explains the observation that only atoms of high electron affinity (fluorine, oxygen, and nitrogen) form such bonds.

No reference is given to any earlier workers for the concept of the hydrogen bond.

An alternative candidate for the first mention in print of the term hydrogen bond since Lewis' book is a volume by Arthur Buswell of the University of Illinois entitled *The Chemistry of Water and Sewage Treatment* (37):

Latimer and Rodebush in an extension of the Lewis theory of valence have made a plausible suggestion as to the way in which these molecular aggregates may be built up. They cite several instances in which a hydrogen nucleus act as a valence bond by sharing two pairs of electrons with different atoms. Their suggestion would be to consider dihydrol as formed thus:



If we consider the four electron pairs of the oxygen shell disposed toward the corners of a tetrahedron, it would appear that a double or triple 'hydrogen bond' might exist... It is hoped that this theory of the constitution of water will be subjected to experimental investigation.

Buswell had obtained a Ph.D. from Columbia University in 1917 and since 1920 held joint appointments as Professor of Sanitary Chemistry at the University of Illinois and as Chief of the State of Illinois Water Survey. At Illinois he was a colleague of Worth Rodebush, with whom he would later collaborate on some papers on hydrogen bonding. It is doubtful that Buswell's book exerted much influence on academic chemists; I have found only one citation to it with reference to hydrogen bonding. The citation is from a review article by Rodebush.

A more complete presentation of Pauling's views on the

nature of the chemical bond was published in 1931 (38). The discussion of the hydrogen bond was essentially the same as in the 1928 paper, with the added point that the presence of OHO groups in many crystal structures indicates hydrogen bonding involving $[O^{\ominus}H^{\oplus}O^{\ominus}]$. This strictly electrostatic view of the hydrogen bond was held by Pauling for several years and does not appear to have been definitely repudiated but gradually abandoned. In this paper, Pauling credited the discovery of the hydrogen bond to Huggins, and to Latimer and Rodebush.

Jack Sherman of Caltech, at the time one of Pauling's graduate students, and previously an undergraduate at Berkeley, wrote a review of the thermochemistry of ionic compounds in 1932. In it he states (39):

The high value for the proton affinity from ammonium fluoride is surprising, but may be explained in the following way. Pauling has pointed out in his lectures that the unexpected occurrence of wurtzite structure for ammonium fluoride is probably due to the formation of hydrogen bonds.

Pauling covered hydrogen bonding in lectures on the nature of the chemical bond from 1927-1928 at Caltech and in lectures on the applications of quantum mechanics at Berkeley beginning in the spring of 1929. The view of hydrogen bonding in these lectures, at least from the period 1929-1931, is the same as that presented in his 1928 and 1931 papers (35,40).

In 1933, Pauling analyzed the X-ray diffraction data on NH_4HF_2 (41):

In predicting a structure for NH_4HF_2 we make the following assumptions:

1. We expect linear HF_2 groups as in $NaHF_2$ and KHF_2 with the H-F distance equal to $1.2 \pm 0.1 \text{ \AA}$.
2. We further expect hydrogen bonds between nitrogen and four surrounding fluorine ions. The fluorine ions should be approximately tetrahedrally arranged about the nitrogen ion, at a distance of 2.63 \AA , as in NH_4F .

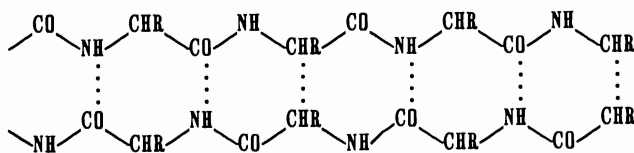
The hydrogen bond in this compound is assumed to be completely ionic.

Pauling and L. O. Brockway determined the structure of formic acid by X-ray diffraction in 1934 (42). Formic acid and other carboxylic acids had long been known to form dimers, and Latimer and Rodebush had suggested in 1920 that this was due to hydrogen bonding. The results of this determination confirmed this suggestion. In this paper Pauling retreated slightly from the purely ionic picture of the hydrogen bond, indicating that resonance between ionic and covalent forms of the carboxylic acid group may contribute to the stability of the hydrogen bond.

In 1935, Pauling published a paper on the structure and entropy of ice (43). In it he pointed out that the residual entropy of ice can be accounted for by a structure in which each oxygen

atom is attached to two hydrogen atoms in the same molecule, and to two other hydrogen atoms in other water molecules by hydrogen bonds, such that the hydrogens in the O-H-O linkage are not symmetrically located between the two oxygens. Since the discussion implies discrete water molecules, rather than ionic bonding between H^+ and O^- , it would appear that Pauling had quietly abandoned his insistence on the purely ionic nature of the hydrogen bond.

In 1936, A. E. Mirsky and Pauling (44) proposed that protein molecules are held in definite configurations by hydrogen bonding between amino and carboxylic side chains; the process of denaturation would be one in which these hydrogen bonds are broken. William Astbury and H. J. Woods (45) had previously proposed that bonding between protein chains is due to the attraction between $=NH$ and $=CO$ groups. The following diagram shows their proposed structure:



The dotted lines between the CO and NH groups might indicate that they had hydrogen bonding in mind, even though they do not use any of the names then current for the concept. The fact that the same dotted lines are used between the alpha-hydrogens in CHR groups, leaves some doubt that they intended anything so specific.

Several of the pioneers of hydrogen bonding returned to the concept in the 1930's, after a decade or more of pursuing other research interests. In 1931, Huggins proposed a mechanism for the high specific conductances of the hydroxide and hydrogen ions in liquid water involving the transfer of protons from one water molecule to another through the formation of temporary hydrogen bonds. This paper is the first use by Huggins of the term hydrogen bond (46). In 1936 he wrote reviews of hydrogen bridges in ice and liquid water (47) and in organic compounds (48). In the second paper he repudiates the accepted name for the concept (48):

The writer prefers the term "hydrogen bridge" to the expression "hydrogen bond" introduced by Latimer and Rodebush, believing it best to use the word 'bond' (in chemistry) only for a system consisting of one or more (nearly always two) electrons holding together two atoms. 'Hydrogen bond' is also confusing since it suggests the electron pair bond between two atoms in the H_2 molecule...

In this Huggins was fighting a lost cause, as by that year almost everyone else who was making use of the concept had adopted the term "hydrogen bond".

Lewis, by 1934, had turned his attention to the preparation

of deuterated compounds. Differences between the vapor pressures of deuterioacetic acid (49), deuteriochloric acid (50), and deuteriocyanic acid (51), and the vapor pressures of the corresponding "light" compound were attributed to stronger hydrogen bonding in the deuterio compound. Thus, there is a 5% difference in the vapor pressure of the acetic acids, for which hydrogen bonding is important, and little difference for HCl and HCN, where hydrogen bonding is much weaker.

Rodebush, in 1936, published a review on the subject of hydrogen bonding and coordination (52), pointing out the similarities and differences (mainly that the hydrogen bond is much weaker than the coordination of metal ions) between the two concepts. He pointed out that it was only then, 15 years after the introduction of the concept, that definite proof of the hydrogen bond has appeared, pointing specifically to the X-ray studies of Pauling and the disappearance of the O-H vibrational frequency in the IR in hydrogen bonding situations (of this more later).

William Claussen and Joel Hildebrand (53) reported on the vapor pressures of hydrogen and deuterium fluoride in 1934, citing the above-mentioned papers (49-51) of Lewis:

Since the hydrogen bond has been regarded by Lewis as largely responsible for the differences in vapor pressure between the corresponding light and heavy compounds, it occurred to us that a comparison of the two hydrogen fluorides, in which this bond is particularly strong, would be interesting.

The results of this investigation supported Lewis' contention. Four years earlier, G. H. Cady and Hildebrand (54) had studied the freezing points of water-hydrogen fluoride mixtures and explained the results in terms of molecular compounds between H_2O and HF without reference to hydrogen bonding.

Hildebrand had been a faculty member at Berkeley since 1913. He was one of the first of the young faculty members brought in by Lewis to build up the department, and had collaborated with Latimer on the *Reference Book of Inorganic Chemistry*. Consequently it is surprising to find that it took Lewis' renewed interest in hydrogen bonding for him to accept the concept in his own work. Considering that it took 14 years for the hydrogen bonding concept to cross the hall (figuratively) at Berkeley, it is perhaps understandable that it took as long as it did to win acceptance from the chemical community at large.

In 1933, John D. Bernal and K. H. Fowler discussed the theory of water and its ionic solutions. Bernal, a crystallographer at Cambridge University (in 1937 he would move to Birbeck College, University of London) is probably best known for his work on the structures of proteins. Their explanation for the high mobility of the H_3O^+ and OH^- ions was essentially the same as that proposed by Huggins in 1931, but no citation of Huggins' article was given. In comparing the association of water with that of other liquids, they stated that:

"In HF the cohesion is better regarded as due not to dipoles but to 'the H-bond'." In concluding they state (55):

It is clear that the ideas here developed, if they are at all correct, will lend themselves to a number of further applications particularly in physical and chemical applications of the hydrogen bond.

The term "hydrogen bond" does not otherwise occur in the main discussion of the structure of water and ice, but it is clear that this is what they have in mind. No citation of previous workers is given for the concept, but Lewis' work with deuterium compounds is mentioned in a discussion of the mobility of D^+ in heavy water.

In 1935, Bernal and H. D. Megaw published a major paper on the function of hydrogen in intermolecular forces (56). In it they attributed the "so called hydrogen bond" to Huggins and Pauling. The major thrust of the paper was a distinction between ordinary hydrogen bonding and bonding between two hydroxyl groups, which they consider different enough to give a separate name: "the hydroxyl bond". Bernal's papers influenced many British and Continental chemists during the middle 1930's, as judged by the number of citations. The conception of the hydroxyl bond was much commented on and debated. Rodebush (52) remarked that the "hydroxyl bond" did not appear to differ in any significant way from the hydrogen bond, and after the initial impression caused by Bernal's papers died down, this view came to be generally accepted.

Other crystallographers were, in the meantime, following Pauling's lead, finding hydrogen bonding in various crystal structures. As early as 1930, J. West, of the University at Manchester (57), suggested that the X-ray analysis of potassium dihydrogen phosphate could best be fit by placing the hydrogen atoms on a line joining two oxygen atoms of adjacent PO_4^{3-} groups, giving each hydrogen a coordination of two. There is, however, no suggestion in this paper that this conclusion can be generalized to other structures.

In 1933, William H. Zachariasen of the Physical Laboratory of the University of Chicago came to the same conclusion with regard to the position of the hydrogen atoms in sodium bicarbonate (58). In this paper Zachariasen suggested that this arrangement is due to the hydrogen bond and credits Pauling with the idea. In 1935, Zachariasen reported the X-ray analysis of liquid methyl alcohol (59):

Our analysis thus strongly indicates the presence of hydrogen bonds between oxygen atoms of neighboring molecules... Every hydrogen atom is thus linked to two oxygen atoms; undoubtedly it is linked more strongly to one of the oxygen atoms than to the other, so it would still be justifiable to talk about hydroxyl groups. Naturally if we wish to characterize the nature of these hydrogen bonds, we should employ the term dipole bonding.

This is the first clear indication of the current view that the

hydrogen bond is due primarily to unusually strong dipole interactions (60).

The current view also allows for some contribution from covalency in stronger instances of hydrogen bonding (such as HF_2^-). These days this is generally described in terms of three-center orbitals. The equivalent for the chemist of the 1930's would be "resonance". We have already seen that Pauling invoked resonance as contributing to the stability of the formic acid dimer (42). Sidgwick, in 1934, on quantum mechanical grounds, abandoned the theory that the hydrogen atom in the hydrogen bond has four shared electrons (61):

The solution of this difficulty is provided by the theory of resonance, the hydrogen atom being covalently attached to one and another of the two atoms in the two structures.

Bernal and Megaw (56) suggested that oscillation of the hydrogen atoms between positions in which it is bonded to each oxygen in turn might be equivalent to electronic exchange. They proposed this speculation rather tentatively, however, and concluded that the hydrogen bond is primarily due to electrostatic attraction.

In 1937, Albert Sherman (62), a Berkeley graduate and the twin brother of Jack Sherman who had worked with Pauling, considered that the hydrogen bond is stabilized by resonance structures in which the hydrogen atom is bonded to either oxygen, and that this stabilization energy should show up in thermochemical data for compounds in which it occurs. He pointed out that such stability is not found in thermochemical data for nitro derivatives of benzene and argued that hydrogen bonding is not present in these compounds, despite the indication from IR spectra that it is present. This analysis is flawed by the fact that he did not allow for the possibility of intermolecular as well as intramolecular hydrogen bonding, but the value (about 2 kcal/mole) that he predicted for the effect is of the right order of magnitude. IR and Raman spectra of water, the alcohols, amines, and related compounds were studied intensively in the 1920's and early 1930's. It was recognized that the characteristic stretching frequency of the OH or NH group appeared in the spectra of these compounds in the vapor phase, but was absent, or displaced, in the liquid phase or in solutions. In the condensed phase it had long been recognized that the position and intensity of this band varied with the temperature and the solution concentration. These effects in water were often interpreted in terms of Armstrong's model for water, in which changes in temperature or solute concentration were attributed to the changing proportions of the various polymers of "hydrone". For the alcohols and amines the equivalent explanation in terms of different degrees of association was offered. Some workers, noting the fact that changes were seen in the OH or NH vibrational frequencies, drew the conclusion that association was through these bonds, but before 1935, no connection was made with the Latimer-

Rodebush concept of hydrogen bonding (63).

The first clear recognition that these changes were due to hydrogen bonding appeared in 1935. A paper (64) published in *Nature* on 26 January 1935 by a research group in the Bureau of Chemistry and Soils of the Department of Agriculture in Washington, D.C., attributed the lack of the characteristic OH absorption in such molecules as salicylaldehyde, o-nitrophenol, and 2,6-dinitrophenol to the presence of "chelated" hydrogen. Reference is given to Sidgwick's book. It is noteworthy that one of the members of the group, Sterling Hendricks, had previously worked with Pauling on the crystal structure of the HF_2^- ion (34). Independently, in a paper presented to the Academy of Sciences in Paris on 18 February 1935, Jacques Errera and Pol Mollet of the University of Brussels (65) came to the same conclusion with regard to the IR spectra of salicylaldehyde and o-chlorophenol. They also cited Sidgwick's book for the concept, calling it "covalence de coordination".

At about the same time (paper submitted 20 March 1935), Hendricks reported the results of an X-ray diffraction determination of the structure of oxalic acid. He concluded (66):

The separation O-O... = 2.55 Å... is probably associated with a "hydrogen bond" between two oxygen atoms.

The following year the group at the Bureau of Soils (67) adopted the term "hydrogen bond" for the concept, citing Huggins, Latimer and Rodebush, Sidgwick, and Pauling for the previous history of the concept. The absence of the characteristic OH stretching frequency is proposed as a definite test for the presence of hydrogen bonding. A paper (68) from the same group later that year, extending the application of the criterion to phenols, states that "The authors are much indebted to Professor Linus Pauling for discussion of this type of spectra and helpful advice as to its interpretation." It is tempting to speculate that it was the presence in the group of Hendricks and Oscar Wulf, both of whom had worked with Pauling, which led them to look at hydrogen bonding as a possible explanation of the anomalies.

By 1936, Errera (69) was also using the term "hydrogen bond", though he was still citing Sidgwick's book for the concept. Before the papers of Errera and the group at the Bureau of Soils, papers using the concept of hydrogen bonding had largely been confined to crystallographers, British physical organic chemists of the Sidgwick school, and a few chemists with associations with Berkeley. During 1936 and 1937 an increasing number of spectroscopists, both chemists and physicists, began publishing papers making use of the concept.

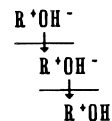
Another significant event leading to general acceptance of the concept was a Meeting of the Faraday Society on the Structure and Molecular Forces in Liquids and Solutions held at Edinburgh on 24-26 September 1936 (70). Hildebrand gave the keynote lecture for the section on the structure of solutions, in the course of which he stressed the importance of hydrogen

bonding (71):

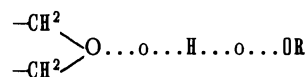
It is becoming evident, again, that the term "association" under which we have lumped all departures from normal behavior, must be subdivided into association arising from the interaction of dipoles, and that due to the formation of definite chemical bonds. Of these, perhaps the most interesting are the hydrogen bonds or 'bridges' between oxygen, nitrogen, or fluorine atoms, a species of chemical interaction.

Among the scientists who also gave papers at this meeting were Bernal (72) and Errera (73). The record of this meeting, appearing in print early in 1937, was much cited by chemists and physicists during the remainder of the year. Interestingly, the citations are as often to the "General Discussions" following the papers as to the papers themselves (74).

As an example of the shift of opinion during 1937, we might point to two French spectroscopists, René and Marie Freymann. They had been working on the IR spectra of alcohols, amines, and related compounds since 1931, and René Freymann had written his doctoral thesis (75) on the subject. In one of René's earlier papers (76), he studied the spectra of alcohols in non-polar solvents and attributed the shift in the OH stretching frequency on changing concentration or temperature to "les associations moléculaires", envisioned as in the following diagram:



In a paper presented to the Academy of Sciences in Paris by René on 28 December 1936 (77), he considers association as involving "des liaisons monoelectroniques" of Perrin:



Another paper, by Marie, presented 25 January 1937 (78), also proposes "liaison monoelectronique" to account for shifts in IR spectra of amines. However, in a joint paper submitted on 25 February 1937 (79), they review the evidence for "liaison hydrogène". Although the papers presented at the Faraday Society meeting are not cited as references, the timing of this paper and the abrupt shift in their view of the nature of association makes it likely that they were influenced by reports from other French scientists who had attended the meeting.

The Freymanns were not the first French scientists to use the term "hydrogen bond": it appeared (untranslated from English) in a paper submitted a month earlier (30 January 1937) by Ch. Sannié and V. Poremski (80) on the Raman spectra of organic acids.

By 1937, the term "hydrogen bond" was also used by Japa-

nese chemists. The earliest use of the term I have been able to find is in a paper by S. Mizushima, Y. Uehara, and Y. Morino (81) of the Imperial University of Tokyo. The first instance I have found of the occurrence of "Wasserstoffbrücke", the German equivalent of "hydrogen bond", is in a book published in 1938 (82). However, at the Edinburgh meeting, K. L. Wolf of the University of Würzburg wrote (83):

... it follows that the work required to separate completely the -OH bindings (due essentially to the dipole forces) from the state of pure alcohol to complete dissociation is ca. 8 kcal per mol. alcohol.

Since many participants at the meeting (including Hildebrand) were drawing distinctions between hydrogen bonding and dipole forces, it cannot be claimed that Wolf would have agreed that he was talking about hydrogen bonding. But his estimate of the heat of dissociation shows that he recognized that the forces between -OH dipoles are greater in magnitude than ordinary dipole forces.

Why did the acceptance of the hydrogen bonding concept by the chemical community take as long as it did? Rodebush gave one answer in his 1936 review (52): that it was only after 15 years that definite evidence for hydrogen bonding was forthcoming. The group at the Bureau of Soils proposed another explanation (67):

Bonding of hydrogen to two atoms has been recognized and described under such various terms as partial valence, secondary forces, hydrogen bond, chelate ring, coordination, Dimroth ring, association, intermolecular association, and the "ortho effect". This very multiplicity of naming reflects the amorphous character of the concept and its partially uncorrelated appearance in the literature during the past twenty-five years.

These explanations may be partially true; still, most of the textbook examples for the existence of hydrogen bonding were already known when Latimer and Rodebush wrote their paper, or were discovered soon afterwards. And most chemists who used the terms "association" or "intermolecular association" or "secondary valence" do not appear to have anything as definite as the hydrogen bond in mind.

It is more likely the case that the true explanation lies in two factors: first, despite the general acceptance of the Lewis theory by the late 1920's, most chemists were not yet accustomed to think in terms of electronic effects; they had learned early in their chemical education that "association" explained the properties now explained by hydrogen bonding, so that they did not question whether the concept of association had any real content. This is similar to the unthinking acceptance of the octet rule by chemists in the 1950's, so that the synthesis of xenon compounds came as a shock.

A second reason is similar to that proposed by Robert Kohler (17) to explain the delay in the acceptance of the Lewis

theory: namely that Lewis, Latimer, Rodebush, and Huggins did not have any program of experimentation which made use of hydrogen bonding and consequently did not influence the greater number of chemists who were experimentalists rather than theorists. We see this influence of an experimental program in spreading acceptance of hydrogen bonding repeatedly during the 17 year history described in this paper. Lowry, and particularly Sidgwick, primarily influenced the new British school of physical organic chemists; Pauling influenced crystallographers; Lewis, when he began research on deuterium, where he could apply the concept of hydrogen bonding to his own work, influenced Hildebrand. Hendricks, a crystallographer and Pauling's student, probably influenced the spectroscopists at the Bureau of Soils, who in turn, influenced other spectroscopists. Finally, the Edinburgh meeting of the Faraday Society, bringing together theorists and experimental scientists using different techniques, diffused the "new" ideas throughout the community of chemists and chemical physicists.

References and Notes

Acknowledgements: My thanks first, to Tom Chasteen, whose intention of writing an honors thesis on the beginnings of hydrogen bonding (unfortunately never finished) led him to write the initial letters to Huggins in 1978; then to Diana Nekuyi, who helped me with the final stages of the literature survey. I would also like to thank Dr. Linus Pauling, who answered my requests for information with several gracious letters, and to Clifford Mead, of the William Jasper Kerr Library at Oregon State University, who helpfully sent me photocopies of materials from the Pauling Archives. Finally, to Scott Downing and his staff of inter-library loan librarians at East Texas State University, who worked tirelessly to get me photocopies of those journals not in our collection.

1. W. M. Latimer and W. H. Rodebush, "Polarity and Ionization from the Standpoint of the Lewis Theory of Valence", *J. Am. Chem. Soc.*, **1920**, *42*, 1419-1433.
2. M. L. Huggins, "The Hydrogen Bond and Other Reminiscences", *Chemtech*, **1980**, 422-429.
3. M. L. Huggins, "Term Paper, Chem. 120, May 1919. Some Speculations Regarding Molecular Structure". Unpublished paper.
4. G. N. Lewis, *Valence and the Structure of Atoms and Molecules*, Dover, New York, 1966, p. 109 (Reprint of 1923 edition).
5. J. H. Hildebrand, "Wendell Mitchell Latimer", *Biogr. Mem. Nat. Acad. Sci.*, **1958**, *32*, 224-237.
6. G. C. Pimentel and A. L. McClellan, *The Hydrogen Bond*, Freeman, San Francisco, 1960, p. 4.
7. H. M. Chadwell, "The Molecular Structure of Water", *Chem. Rev.*, **1927**, *4*, 375-398.
8. J. H. Simons, "Hydrogen Fluoride and Its Solutions", *Chem. Rev.*, **1931**, *8*, 213-234.
9. E. Beckmann, "Bestimmung von Molekulargewichten nach der Siedemethode", *Z. Phys. Chem.*, **1890**, *6*, 437-473.

10. W. Nernst, "Verteilung eines Stoffes Zwischen zwei Lösungsmitteln und zwischen Lösungsmittel und Dampfraum", *Z. Phys. Chem.*, **1891**, 8, 110-139.
11. T. S. Moore and T. F. Winmill, "The State of Amines in Aqueous Solution", *J. Chem. Soc.*, **1912**, 101, 1635-1676.
12. W. H. Bragg, "The Crystal Structure of Ice", *Proc. Phys. Soc. London*, **1922**, 34, 98-103.
13. F. Rinne, "Bemerkungen über Raumformeln", *Z. Phys. Chem.*, **1922**, 100, 408; F. Rinne, H. Hentschel, and J. Leonhardt, "Über feinbauliche Versuche zur Konstruktion des Natriumhydrofluorids unter Verwendung der Atombereiche und über die röntgenographische Erforschung dieser Verbindung", *Z. Kristallogr.*, **1923**, 58, 629-640.
14. R. M. Bozorth, "The Crystal Structure of Potassium Hydrogen Fluoride", *J. Am. Chem. Soc.*, **1923**, 45, 2128-2132.
15. J. R. Collins, "Change in the Infra-red Absorption Spectrum of Water with Temperature", *Phys. Rev.*, **1925**, 26, 771-779.
16. I. Langmuir, "Types of Valence", *Science*, **1921**, 54, 59-67.
17. R. E. Kohler, Jr., "Irving Langmuir and the Octet Theory of Valence", *Hist. Stud. Phys. Sci.*, **1972**, 4, 39-67; "The Lewis-Langmuir Theory of Valence and the Chemical Community, 1920-1928"; *Hist. Stud. Phys. Sci.*, **1975**, 6, 431-468.
18. J. Perrin, "Valence et composés d'addition", *Compt. Rend.*, **1927**, 185, 557-561.
19. M. L. Huggins, "Atomic Structure", *Science*, **1922**, 55, 459-460; "Electronic Structures of Atoms", *J. Phys. Chem.*, **1922**, 26, 601-625.
20. T. M. Lowry and H. Burgess, "Studies of Electrovalency. Part II. Coordinated Hydrogen", *J. Chem. Soc.*, **1923**, 123, 2111-2124.
21. N. V. Sidgwick and R. K. Callow, "Abnormal Benzene Derivatives", *J. Chem. Soc.*, **1924**, 125, 527-538.
22. H. E. Armstrong, "The Origin of Osmotic Effects. IV. Hydrodynamic Change in Aqueous Solutions", *Proc. Roy. Soc. London, A*, **1923**, 103, 610-618.
23. C. E. Ronneberg, "Armstrong, Henry Edward", in C. C. Gillispie, ed., *Dictionary of Scientific Biography*, Vol. I, Scribners, New York, 1970, p. 288-289.
24. H. E. Armstrong, "Bigamous Hydrogen - A Protest", *Nature*, **1926**, 117, 553-554.
25. T. M. Lowry, "Optical Rotatory Dispersion", *Nature*, **1926**, 117, 271-275.
26. T. M. Lowry and P. C. Austin, "The Rotatory Dispersion of Tartaric Acid", *Nature*, **1924**, 114, 430-431.
27. N. V. Sidgwick, "The Structure of the Enolic forms of β -Keto-esters and β -Diketones", *J. Chem. Soc.*, **1925**, 127, 907-909.
28. N. V. Sidgwick, *The Electronic Theory of Valency*, Oxford University Press, London, 1927.
29. J. W. Williams, "The Relation Between Polarization and Association", *Proc. Nat. Acad. Sci.*, **1928**, 14, 932-936.
30. J. C. Speakman, "Coordination of Hydrogen in Associated Liquids", *Nature*, **1932**, 129, 244.
31. H. M. Glass, W. M. Madgin, and F. Hunter, "Equilibrium Constants in Terms of Activities (Cyroscopic) Part III. The Dissociation of Quinoline o-Chlorophenoxide in Benzene and p-Dichlorobenzene", *J. Chem. Soc.*, **1934**, 260-263.
32. Reference (6), pp. 387-447.
33. W. H. Barnes, "The Crystal Structure of Ice Between 0°C and -183°C", *Proc. Roy. Soc. London, A*, **1929**, 125, 670-693.
34. S. B. Hendricks and L. Pauling, "The Crystal Structures of Sodium and Potassium Trinitrides and Potassium Cyanate and the Nature of the Trinitride Group", *J. Am. Chem. Soc.*, **1925**, 47, 2904-2919.
35. L. Pauling, personal communication, 17 May 1989.
36. L. Pauling, "The Shared-Electron Chemical Bond", *Proc. Nat. Acad. Sci.*, **1928**, 14, 359-362.
37. A. M. Buswell, *The Chemistry of Water and Sewage Treatment*, Chemical Catalog Co., New York, 1928, pp. 60-62.
38. L. Pauling, "The Nature of the Chemical Bond. Application of Results Obtained from the Quantum Mechanics and from a Theory of Paramagnetic Susceptibility to the Structure of Molecules", *J. Am. Chem. Soc.*, **1931**, 53, 1367-1400.
39. J. Sherman, "Crystal Energies of Ionic Compounds and Thermochemical Applications", *Chem. Rev.*, **1932**, 11, 93-170.
40. Pauling Archives, Oregon State University: Box 90b; Advanced Inorganic Chemistry Lecture Notes, California Institute of Technology, 1930-31; Box 201, Berkeley Lectures on Applications of Quantum Mechanics, 1929-1933.
41. L. Pauling, "The Crystal Structure of Ammonium Hydrogen Fluoride, NH_4HF_2 ", *Z. Kristallogr.*, **1933**, 85, 380-391.
42. L. Pauling and L. O. Brockway, "The Structure of the Carboxyl Group. I. The Investigation of Formic Acid by the Diffraction of Electrons", *Proc. Nat. Acad. Sci.*, **1934**, 20, 336-340.
43. L. Pauling, "The Structure and Entropy of Ice and of Other Crystals With Some Randomness of Atomic Arrangement", *J. Am. Chem. Soc.*, **1935**, 57, 2680-2684.
44. A. E. Mirsky and L. Pauling, "On the Structure of Native, Denatured, and Coagulated Proteins", *Proc. Nat. Acad. Sci.*, **1936**, 22, 439-447.
45. W. T. Astbury and H. J. Woods, "X-Ray Studies of the Structure of Hair, Wool, and Related Fibres. II. The Molecular Structure and Elastic Properties of Hair Keratin", *Phil. Trans. Roy. Soc. London, A*, **1933**, A232, 333-394.
46. M. L. Huggins, "The Role of Hydrogen Bonds in Conduction by Hydrogen and Hydroxyl Ions", *J. Am. Chem. Soc.*, **1931**, 53, 3190-3191.
47. M. L. Huggins, "Hydrogen Bridges in Ice and Liquid Water", *J. Phys. Chem.*, **1936**, 40, 723-731.
48. M. L. Huggins, "Hydrogen Bridges in Organic Compounds", *J. Org. Chem.*, **1936**, 1, 407-456.
49. G. N. Lewis and P. W. Schutz, "Some Properties of Pure Deutacetic Acid", *J. Am. Chem. Soc.*, **1934**, 56, 493-495.
50. G. N. Lewis, R. T. MacDonald, and P. W. Schutz, "The Vapor Pressure of Liquid and Solid Deutochloric Acid", *J. Am. Chem. Soc.*, **1934**, 56, 494-495.
51. G. N. Lewis and P. W. Schutz, "The Vapor Pressure of Liquid

and Solid Deutocyanic Acid", *J. Am. Chem. Soc.*, **1934**, *56*, 1002.

52. W. H. Rodebush, "The Hydrogen Bond and Coordination", *Chem. Rev.*, **1936**, *19*, 59-65.

53. W. H. Claussen and J. H. Hildebrand, "The Vapor Pressures of Hydrogen and Deuterium Fluorides", *J. Am. Chem. Soc.*, **1934**, *56*, 1820.

54. G. H. Cady and J. H. Hildebrand, "Freezing Points of the System Water-Hydrogen Fluoride", *J. Am. Chem. Soc.*, **1930**, *52*, 3843-3846.

55. J. D. Bernal and R. H. Fowler, "A Theory of Water and Ionic Solutions, with Particular Reference to Hydrogen and Hydroxyl Ions", *J. Chem. Phys.*, **1933**, *1*, 515-548.

56. J. D. Bernal and H. D. Megaw, "The Function of Hydrogen in Intermolecular Forces", *Proc. Roy. Soc. London, A*, **1935**, *151*, 384-420.

57. J. West, "A Quantitative X-ray Analysis of the Structure of Potassium Dihydrogen Phosphate (KH_2PO_4)", *Z. Kristallogr.*, **1930**, *74*, 306-332.

58. W. H. Zachariasen, "The Crystal Lattice of Sodium Bicarbonate, NaHCO_3 ", *J. Chem. Phys.*, **1933**, *1*, 634-639.

59. W. H. Zachariasen, "The Liquid Structure of Methyl Alcohol", *J. Chem. Phys.*, **1935**, *3*, 158-161.

60. See, for example, F. A. Cotton and G. Wilkinson, *Advanced Inorganic Chemistry*, 5th Ed., Wiley, New York, 1988, p. 94.

61. N. V. Sidgwick, "The Theory of Resonance and the Coordination of Hydrogen", *Ann. Rep. Progr. Chem.*, **1934**, *31*, 40.

62. A. Sherman, "The Nature of the Hydrogen Bond. II. The Role of Resonance. General Considerations", *J. Phys. Chem.*, **1937**, *41*, 117-122.

63. See references 64-65, 67-69, and 75-82 for references to the earlier work.

64. G. E. Hilbert, O. R. Wulf, S. B. Hendricks, and U. Liddel, "A Spectroscopic Method for Detecting Some Forms of Chelation", *Nature*, **1935**, *135*, 147-148.

65. J. Errera and P. Mollet, "Isoméries intramoléculaires et spectres d'absorption infrarouges", *Compt. Rend.*, **1935**, *200*, 814-817.

66. S. B. Hendricks, "The Orientation of the Oxalate Group in Oxalic Acid and Some of Its Salts", *Z. Kristallogr.*, **1935**, *91*, 48-64.

67. G. E. Hilbert, O. R. Wulf, S. B. Hendricks, and U. Liddel, "The Hydrogen Bond Between Oxygen Atoms in Some Organic Compounds", *J. Am. Chem. Soc.*, **1936**, *58*, 548-555.

68. O. R. Wulf, U. Liddel, and S. B. Hendricks, "The Effect of Ortho Substitution on the Absorption of the OH Group of Phenol in the Infrared", *J. Am. Chem. Soc.*, **1936**, *58*, 2287-2243.

69. J. Errera and P. Mollet, "Intermolecular Force and O-H Absorption Bands in Alcohols at 3m", *Nature*, **1936**, *138*, 882.

70. Symposium on "Structure and Molecular Forces in Pure Liquids and Solutions", *Trans. Faraday Soc.*, **1937**, *33*, 1-279.

71. J. H. Hildebrand, "Intermolecular Forces in Solutions", *Trans. Faraday Soc.*, **1937**, *33*, 144-151.

72. J. D. Bernal, "An Attempt at a Molecular Theory of Liquid Structures", *Trans. Faraday Soc.*, **1937**, *33*, 27-40.

73. J. Errera, "Structure of Liquids Studied in the Infrared", *Trans. Faraday Soc.*, **1937**, *33*, 120-129.

74. "General Discussion", *Trans. Faraday Soc.*, **1937**, *33*, 141-3, 206-214.

75. R. Freymann, "Recherches sur le proche infrarouge", *Ann. Phys. (Paris)*, **1933**, *20*, 243-343.

76. R. Freymann, "Effet de la dilution et de la température sur les bandes d'absorption infrarouges: associations moléculaires", *Compt. Rend.*, **1932**, *195*, 39-41.

77. R. Freymann, "Spectres d'absorption dans le proche infrarouge de solutions d'alcools dans l'éther ou le dioxane: formation d'oxoniums", *Compt. Rend.*, **1937**, *204*, 41-43.

78. M. Freymann, "Spectres d'absorption dans le proche infrarouge de mélanges d'amines et d'alcools. Formation de composés ammoniums", *Compt. Rend.*, **1937**, *204*, 261-263.

79. M. Freymann and R. Freymann, "Interprétation de diverses propriétés chimiques par l'hypothèse de la liaison hydrogène: spectres d'absorption infrarouges", *Bull. Soc. Chim.*, **1937**, *4*, 944-950.

80. C. Sannié and V. Poremski, "Recherches sur la constitution des peptides. 1. Sur la structure des acides organiques. Bandes Raman de la fonction acide dans les acides et leurs dérivés", *Bull. Soc. Chim.*, **1937**, *4*, 880-893.

81. S. Mizushima, Y. Uehara, and Y. Morino, "The OH Vibration Spectrum in the Photographic Infrared", *Bull. Chem. Soc. Japan*, **1937**, *12*, 132-135.

82. F. Kohlrausch, *Der Smekal-Raman-effect; Ergänzungsband 1931-37*, J. Springer, Berlin, 1938, p. 117.

83. K. L. Wolf, "On Association, Heat of Mixing and Miscibility Gaps", *Trans Faraday Soc.*, **1937**, *33*, 179-190.

The Editor regrets to report that Dr. Denis Quane passed away on 21 September 1990 shortly after correcting the final proofs for this article. Dr. Quane was an Associate Professor of Chemistry at East Texas State University, where he had been teaching a course in the history of science since 1982.

Joseph William Mellor (1869-1938)

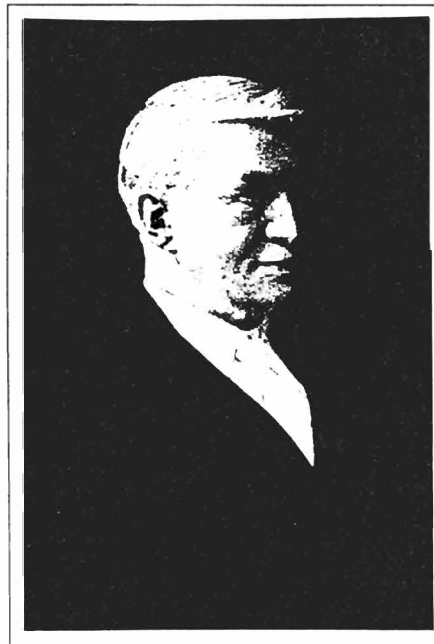
Fathi Habashi, Université Laval

Today most students of chemistry have probably never heard of Joseph William Mellor, though his monumental 16-volume *Comprehensive Treatise on Theoretical and Inorganic Chemistry* is still to be found on the shelves of most chemistry libraries and he was, without a doubt, one of the most prolific and influential textbook authors of his time. His biography in the *Obituary Notices of the Fellows of the Royal Society* appears to have been accidentally overlooked by the standard biographical indices (1) and he does not appear in any of the standard biographical dictionaries of prominent scientists (2,

3). Consequently the author, though long interested in Mellor, could find nothing about him until a recent biographical article by Mike Rose, in the *New Zealand Potter*, was brought to his attention (4).

Mellor was born in Huddersfield, England in 1869. When he was ten years old, the family emigrated to New Zealand where his father found work in the woolen mills of Kaiapoi and later at Dunedin on the South Island. His working class background ruled out any thoughts of higher education, and at the age of 13 he left school to take employment in a boot factory. In the evenings, however, by light of a kerosene lamp, he read secondhand or borrowed books and performed simple chemical experiments.

The young man's remarkable efforts at self-education eventually came to the attention of G. M. Thomson, the Director of the local technical school, who arranged for him to attend evening classes, and who, following Mellor's matriculation in 1892, also assisted him in becoming a part-time student at Otago University. Mellor rewarded Thomson's faith in him by graduating in 1898 with first class honors. Following graduation, he took a teaching post as a science lecturer at Lincoln Agricultural College and married Emma Bakes, the organist at Mornington Church. However, a few months later, he received an 1851 Exhibition Scholarship which enabled him



Joseph William Mellor

to resign his post and to sail to England for advanced study at Owens College in Manchester. Four years earlier, in 1894, another young graduate of Otago University by the name of Ernest Rutherford had also received an 1851 Exhibition Scholarship and had departed New Zealand to study physics at Cambridge University.

At Owens College, Mellor first did research in organic chemistry with William H. Perkin Jr. but, under the influence of Harold B. Dixon, soon switched to the field of physical chemistry and the study of chemical kinetics. This led to the publication of several papers on the influence of moisture on the kinetics of gas reactions and to the writing of his first two books: *Higher Mathematics for Students of Chemistry and Physics* (1902), and *Chemical Statics and Dynamics* (1904). Mellor later described the *Chemical Statics* as having been his favorite work. As for the *Higher Mathematics*, it was, like the later American text on the same subject by Farrington Daniels, the product of the author's efforts to make up for his own lack of formal mathematical training and the insights which the resulting program of self-education had given him into the mathematical difficulties experienced by other inadequately prepared students of chemistry,

After three years of research at Owens, Mellor accepted a teaching position as Science Master at the local high school at Newcastle-under-Lyme in the Midlands - a region famous for its "Potteries". These consisted of six towns that were federated into the city of Stoke-on-Trent a few years after Mellor's arrival. The region was rich in coal and clays, the necessary elements of pottery manufacture, and the resulting dominance



A self-caricature of Mellor depicting a stay in hospital (8).

of the pottery industry soon attracted Mellor's interest.

In 1904, the industry started a Pottery School which soon became part of the North Staffordshire Technical College. Mellor lectured there and later became the Principal. He also became Secretary of the newly-formed Ceramic Society. During World War I, he directed a modest research program on refractories that became the first stage in the formation of the British Ceramic Research Association and, in 1921, the British Refractories Research Association was formed with Mellor as Director.

A natural consequence of these activities was that Mellor's interests gravitated towards the fields of inorganic and industrial chemistry and, as had been the case with his earlier study of physical chemistry, the result of his thorough program of self-education in these new areas was a veritable deluge of research papers (more than 100 on the chemistry of ceramics) and books (Table 1) on these subjects, including his 16-volume *magnum opus* (Table 2). Indeed, the output from Mellor's 11 books alone (not counting his 116 published research papers and six patents) would eventually total more than 20,000 printed pages! A characteristic of all of these volumes are the

Table 1. Books by J. W. Mellor in chronological order.

Year	Title	Pages
1902	Higher Mathematics for Students of Chemistry and Physics	543
1904	Chemical Statics and Dynamics	528
1905	The Crystallization of Iron and Steel. An Introduction to the Study of Metallography	114
1912	Modern Inorganic Chemistry	871
1913	A Treatise on Quantitative Inorganic Analysis	778
1914	Clay and Pottery Industries	411
1915	Introduction to Modern Inorganic Chemistry	684
1920	Higher Inorganic and Theoretical Chemistry	na
1922-37	Comprehensive Treatise on Theoretical and Inorganic Chemistry	15,320
1930	Elementary Inorganic Chemistry	229
1930	Intermediate Inorganic Chemistry	690

Table 2. An Outline of Mellor's Comprehensive Treatise

Volume	Contents	Pages	Year
1	H, O*	1065	1922
2	F, Cl, Br, I, Li, Na, K, Rb, Cs	894	1922
3	Cu, Ag, Au, Ca, Sr, Ba	927	1923
4	Ra and Ac Families, Be, Mg, Zn, Cd, Hg**	1074	1923
5	B, Al, Ga, In, Tl, Sc, Ce, and Rare Earths, C (Part I)	1004	1924
6	C (Part II), Si, Silicates	1024	1925
7	Ti, Zr, Hf, Th, Ge, Sn, Pb, Inert Gases	977	1927
8	N, P	1110	1928
9	As, Sb, Bi, V, Nb, Ta	967	1929
10	S, Se	958	1930
11	Te, Cr, Mo, W	909	1931
12	U, Mn, Ma***, Re, Fe (Part I)	944	1932
13	Fe (Part II)	948	1934
14	Fe (Part III), Co	892	1935
15	Ni, Rn, Rh, Pd, Os, Ir	816	1936
16	Pt, General Index	811	1937
Total number of pages		15,320	

*Includes chapters on History of Chemistry, Thermodynamics, Crystals and Crystallization, Solution, Kinetic Theory, Electrolysis, and Electrical Energy.

**Includes chapters on the Structure of Matter, Radioactivity, and the Architecture of the Atom.

***Ma stands for masurium - an element discovered by Ida Tacke, Walter Noddack and Otto Berg in 1925. Thought to occupy the position held today by technetium, its confirmation was controversial.

numerous references and quotations and the thorough presentation of the history of each topic, a fact which today makes them an invaluable resource for historians of chemistry (5).

The Mellors had no children, but he had four sisters in New Zealand who provided several nieces and nephews. He used to write them amusing letters, illustrated with cartoons, some of which were collected together by friends and published in 1934 under the title of *Uncle Joe's Nonsense* (6). In addition, he described his trip to the United States in 1929, when the Ceramic Society travelled to New York on board the Cunard liner, "Laconia", in a 36-page booklet illustrated by witty cartoons and comments, three of which have been reproduced in the *Journal of Chemical Education*. (7), along with two self-caricatures (8).

Mellor retired as Principal of the North Staffordshire Technical College in 1934 and as Director of the British Refractories Research Association in 1937. He died the next year at the age of 69. After his death, some pieces from his pottery collection and other archival material were donated by his wife to the University of Otago. In 1949, the New Zealand Institute of Chemistry instituted an annual Mellor Lecture in his honor.

References and Notes

1. A. T. Green, "Joseph William Mellor, 1860-1938" *Obit. Not. Fellows Roy. Soc.*, **1936-1938**, 2, 573-576. This was reprinted in *J. Chem. Soc.*, **1943**, 341-343. The author is grateful to Dr. William B. Jensen, Department of Chemistry, University of Cincinnati, for bringing this reference and references 7 and 8 to his attention.

2. P. A. Pelletier, *Prominent Scientists, An Index to Collective Biographies*, 2nd ed., Neal-Schuman, New York, NY, 1985.

3. Thus Mellor isn't listed in any of the dictionaries indexed in reference 2 nor in C. C. Gillespie, ed., *Dictionary of Scientific Biography*, Scribners, New York, NY., 1970 or W. R. Pötsch, A. Fischer and W Müller, *Lexikon bedeutender Chemiker*, Verlag Harri Deutsch, Frankfurt, 1989.

4. M. Rose, "Joseph Mellor, Otago's Brilliant Chemist Who Took the British Ceramics Industry into the Twentieth Century", *New Zealand Potter*, **1985**, 1, 8-10. The author is grateful to Dr. D. W. F. James, Chief Executive of the British Ceramic Research Limited, Stoke-on-Trent, England, for bringing this reference to his attention

5. Since Mellor's richest legacy to the chemical community is in the form of his books, a brief commentary on some of the more important items in Table 1 is not without interest:

Higher Mathematics for Students of Chemistry and Physics - This book starts with differential calculus, then discusses analytical geometry, integral calculus, infinite series, probability, determinants, differential equations, and Fourier's theorem. The book went through four editions, was reprinted in a cheap edition by Dover in 1955, and was translated into German. The book seems to have been the first to make use of practical problems in chemistry and physics to teach mathematics to students of physical science. Farrington Daniels published *Mathematical Preparation for Physical Chemistry* in 1928

- the American counterpart to Mellor's *Higher Mathematics*. The book contains a wealth of information on chemistry and physics that is now of a great historical value.

Chemical Statics and Dynamics - This book carries the subtitle *Including the Theories of Chemical Change, Catalysis, and Explosions*. On the title page, the author quotes the phrase "The first law of nature is order." The book is one in the series "Textbooks of Physical Chemistry", edited by Sir William Ramsay. In this book, Mellor brings together the origins of the theories of chemical kinetics, the mathematics of the different orders of reactions, heterogeneous reactions, equilibrium and dissociation, the effect of temperature and pressure on chemical reactions, electrolytic dissociation, catalysis, fermentation, and explosions. More than three thousand references to the original literature are included.

Modern Inorganic Chemistry - First published in 1912, this volume went through eight editions because of the widespread popularity it achieved. The last edition was prepared by G. D. Parkes of Keble College, Oxford, one year after Mellor's death. This edition of the book was still being reprinted as late as 1951. The first one-fourth of the book is devoted to general principles of physical chemistry. This is followed by an equal number of pages devoted to nonmetals, and the remainder of the book deals with the metals and metalloids.

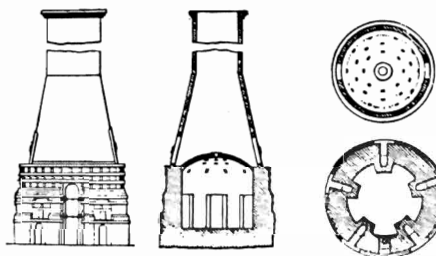
The Comprehensive Treatise - This is Mellor's masterpiece, a 16-volume reference book on inorganic chemistry complete with extensive references to the original literature (Table 2). It is difficult to believe that this was the product of a single person working alone with only a young lady to do the typing (electric typewriters and word processors were, of course, not known at the time). When one examines the numerous references, which also included the foreign literature, one is amazed by the magnitude of the task. Mellor dedicated it to "The privates in the great army of workers in chemistry. Their names have been forgotten, their work remains."

6. J. W. Mellor, *Uncle Joe's Nonsense for Young and Old, A Medley of Fun and Philosophy Reported by J. W. Mellor*, Longmans, Green, London, 1934.

7. A. Silverman, "Mellor's Nonsense", *J. Chem. Educ.*, **1952**, 29, 187.

8. Editor's Note, *J. Chem. Educ.*, **1954**, 31, 17.

Dr. Fathi Habashi is Professor of Extractive Metallurgy at Laval University, Quebec City, Canada, G1K 7P4 and has written numerous articles on the history of metallurgy and industrial chemistry.



Exterior and cross-sectional views of an industrial pottery kiln, circa 1880.

THE GENESIS OF ELECTROGRAVIMETRY

John T. Stock, University of Connecticut

In his Wolcott Gibbs Memorial Lecture, Frank W. Clarke, who had studied under Gibbs from 1865 to 1869, commented on the advances in analytical chemistry made by Gibbs and his students at the Lawrence Scientific School (1):

But the most important of all was the electrolytic determination of copper, now universally used, which was first published from Gibbs' laboratory. It is true that a German chemist, Luckow, claimed to have used the method much earlier, but so far as I can discover, he failed to publish it. Gibbs, therefore, is entitled to full credit for a process which was the progenitor of many others.

As discussed later, C. Luckow published this claim soon after the appearance of Gibbs' account. A paper that marks the centenary of Gibbs' work on electrogravimetry does not mention Luckow, who may have originated the technique (2).

Oliver Wolcott Gibbs (1822-1908) - he dropped the first name, Oliver, early in his career - was a scientist of wide interests (1). Apart from his contributions to analytical chemistry, his work on the ammonia-cobalt compounds and on phosphotungstic and other complex inorganic acids occupied much of his career. He was very much a person-to-person teacher, keeping in close touch with his comparatively few students.

Results obtained by E. V. M'Candless, presumably one of Gibbs' students, form the basis of the 1864 announcement of the technique that later became known as electrogravimetry. Actually, the very brief announcement, "On the Electrolytic Precipitation of Copper and Nickel as a Method of Analysis", is the sixth and final section (pp. 334-36) of Gibbs' paper, which carries the general title "Contributions to Chemistry from the Laboratory of the Lawrence Scientific School" (3). The other sections deal with purely chemical separations, such as of chromium, manganese, cobalt, and uranium from various other metals.

The deposition of copper from solutions of the sulfate was carried out in a small platinum capsule connected to the negative pole of one or two Bunsen cells. The positive electrode was a stout platinum wire that dipped centrally into the solution. Completion of deposition, taking one to three hours, was checked by testing a drop of the liquid with hydrogen sulfide water. After washing and vacuum-drying over sulfuric acid, the copper-carrying capsule was reweighed. Six results with an average close to the theoretical value and a standard deviation of about 0.3% are quoted.

M'Candless then determined copper in copper-nickel coinage alloy. Four of his results were within 0.05% of the specified 87.50% of copper. Some abnormally high results



Oliver Wolcott Gibbs

were attributed to over-rapid deposition, resulting in a spongy deposit. This is difficult to wash free from impurities and also oxidizes easily.

Two points made by Gibbs were that, after removal of copper, the solution contained any other constituents of the sample and that it was at least probable that nickel might be determined by electrolysis of an ammoniacal solution of its sulfate. In two determinations of nickel in a commercial sample, M'Candless obtained results of 91.36% and 91.60%. The metal deposit was bright and coherent, thus upholding Gibbs' prediction. What a pity that no nickel determinations were reported for the copper-free liquid from the coinage alloy experiments! Then we should have had the first example of an overall electrogravimetric analysis of a sample.

In 1865, C. Luckow, a chemist working for the Cologne-Mindener Railway, claimed that he had been determining copper and silver by electrolysis since 1860 (4,5). In view of subsequent events, there is no reason to doubt his claim. He entered his methodology in a competition organized by the Mansfeld Ober-berg und Hütten Direction in Eisleben. This company needed a rapid and reasonably accurate method for the determination of copper in ores, etc. The prize went to a Dr. A. Steinbeck for a method that involved titration in ammoniacal medium with potassium cyanide as a final step. However, Luckow also received an award. The details of both methods were published by the company in 1869 (6). Originally Luckow, like Gibbs, had used the rather slow deposition from sulfate medium; otherwise he might have won the competition. Progress by Luckow and by others soon increased the speed and versatility of electrogravimetry.

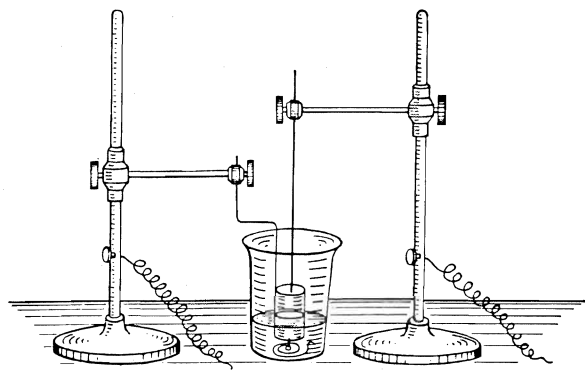
Two major advances made by Luckow were his discovery

that deposition of copper from dilute nitric acid medium was advantageous and his introduction of a separate cathode, i.e., one that was not also the solution container. He used a platinum foil that was about 2-1/2 in. long and 1-1/4 in. wide. This was bent into a cylinder and a stout platinum wire was attached. The anode, a flat platinum wire spiral of diameter to fit into the bottom electrolysis beaker, had a vertical extension to carry a binding post, and a stand with an arm carrying a screw connector held the cathode with its lower edge close to the anode.

Luckow also examined the electrolytic behavior of a number of other metals, especially those likely to accompany copper in technical samples (6). By suitable adjustment of the medium, he achieved simultaneous deposition of copper on the cathode and lead, as lead dioxide, on the anode.

Three years after the first communication from the Eisleben laboratories, another described some developments, including an improved platinum electrode system (7). The cathode, now of conical form, was slotted, so that oxygen arising from the anode could pass to the exterior of the cathode. In a sense, this was the ancestor of the gauze-type electrodes that permit free circulation of the solution. The actual aim was, however, to overcome a problem that occurred in the analysis of copper samples that contained much iron. With a simple cylindrical cathode a dark coloration, caused by reduction of iron along with the copper, appeared in the oxygen-starved region around the outside of the cathode. By the summer of 1869, the laboratory was able to determine copper in all samples that were free from antimony, arsenic and bismuth, which precipitate on the copper deposit and blacken it.

In 1880, Luckow wrote a partially-reminiscent paper concerning the use of the electric current in analytical chemistry (8). He recalled the accounts that he had published in 1865 (4) and pointed out the advantages of electrodeposition. One of these was that the process can run unattended, e.g., overnight. Following a survey of the electrochemical behavior of solutions of various acids and salts, Luckow referred to some of the then recent investigations by others. Examples included the



Luckow's electrode arrangement for electrodeposition

determination of nickel (9-12), copper (9,10,12), cobalt (9,10,12), lead, zinc and manganese (11), and mercury (13). This last determination was described by Frank W. Clarke, the author of the Gibbs Memorial Lecture (1).

Some of the later developments that extended the scope and speed of electrogravimetry have recently been reviewed (14,15). One of these was the mercury cathode, developed by Edgar Fahs Smith and his students (14). The publication of Smith's book in 1890 (16) evoked a short note from Gibbs (17). This concerns a paper that Gibbs had read before the National Academy of Sciences in 1885. The note states that the experiments that he made on metal deposition on a mercury cathode were purely qualitative, and that Luckow subsequently applied the same process to the estimation of zinc (18).

References and Notes

1. F. W. Clarke, "Wolcott Gibbs Memorial Lecture", *Trans. Chem. Soc.*, **1909**, *95*, 1299-1312.
2. F. Szabadváry, "Wolcott Gibbs and the Centenary of Electrogravimetry", *J. Chem. Educ.*, **1964**, *41*, 666-667.
3. W. Gibbs, "Beiträge zur Chemie aus dem Laboratorium der Lawrence Scientific School", *Z. Anal. Chem.*, **1864**, *3*, 327-336; *Amer. J. Sci. Arts*, **1865**, *89*, 58-65.
4. C. Luckow, "Ueber Elektro-Metall-Analyse", *Dinglers Polytech. J.*, **1865**, *177*, 231-235, 296-302.
5. F. Szabadváry, *History of Analytical Chemistry*; Pergamon: New York, 1966, p. 314.
6. Mansfeld'schen Ober-berg und Hütten- Direction in Eisleben, "Ueber Bestimmung des Kupfergehaltes aus Schiefen nach prämierten Methoden", *Z. Anal. Chem.*, **1869**, *8*, 1-43; the electrogravimetric section begins on p. 23.
7. Mansfeld'schen Ober-berg und Hütten- Direction in Eisleben, "Ueber die Bestimmung des Kupfers und einiger anderer Metalle auf electrolytischem Wege", *Z. Anal. Chem.*, **1872**, *11*, 1-16.
8. C. Luckow, "Ueber die Anwendung des elektrischen Stromes in der analytischen Chemie", *Z. Anal. Chem.*, **1880**, *19*, 1-19.
9. F. Wrightson, "Beiträge zur quantitativen Bestimmung der Metalle auf Electrolytischem Wege", *Z. Anal. Chem.*, **1876**, *15*, 297-306.
10. G. P. Schweder, "Zur electrolytischen Bestimmung des Nickels und Kobalts", *Berg-u.-Hüttenmänn. Z.*, **1877**, *36*, 5, 11, 31; cited in H. Fresenius, *Z. Anal. Chem.*, **1877**, *16*, 344-350.
11. A. Riche, "Zur electrolytischen Bestimmung des Mangans, Nickels, Zinks und Bleis", *Compt. Rend.*, **1877**, *85*, 226-229.
12. W. Ohl, "Die electrolytische Bestimmung von Kobalt, Nickel, Kupfer und deren Vortheile in der analytischen Chemie", *Z. Anal. Chem.*, **1879**, *18*, 523-531.
13. F. W. Clarke, "Zur electrolytische Bestimmung des Quecksilbers und des Cadmiums", *Amer. J. Sci. Arts*, **1878**, *16*, 200-201.
14. L. M. Robinson, "Borrowing from Industry: Edgar Fahs Smith's Rotating Anode and Double-Cup Mercury Cathode", in J. T. Stock and M. V. Orna, eds., *Electrochemistry, Past and Present*,

American Chemical Society: Washington, D.C., 1989, pp. 458-468.

15. J. T. Stock, "Henry J. S. Sand (1873-1944), A Well-Remembered Tutor", in J. T. Stock and M. V. Orna, eds., *Electrochemistry, Past and Present*, American Chemical Society: Washington, D.C., 1989, pp. 469-476.

16. E. F. Smith, *Electrochemical Analysis*, Philadelphia, 1890.

17. W. Gibbs, "Note on the Electrolytic Determination of Metals as Amalgams", *Amer. Chem. J.*, **1892**, *13*, 570-571.

18. C. Luckow, *Chem. Zeit.* **1885**, *9*, 338; cited in E. Hintz, *Z. Anal. Chem.*, **1886**, *25*, 113-114.

John T. Stock is Professor Emeritus in the Department of Chemistry of the University of Connecticut, Storrs, CT 06269 and is especially interested in the history of chemical instrumentation and the history of electrochemistry.

"BETWEEN TWO STOOLS": KOPP, KOLBE AND THE HISTORY OF CHEMISTRY

Alan J. Rocke, Case Western Reserve University

Hermann Kopp (1817-1892) and Hermann Kolbe (1818-1884) were two outstanding German chemists during the period in which German chemistry rose to a position of prominence in Europe (1). Although I know of only five surviving letters from Kopp to Kolbe and only one letter draft from Kolbe to Kopp - a document which we reproduce below - they must have been well acquainted for four decades. They may have first gotten to know each other when Kolbe was working as a newly minted Ph.D. with Robert Bunsen (1811-1899) at Marburg, and Kopp was Privatdozent and then Ausserordentlicher Professor at nearby Giessen, during the years 1842-1845. After Kolbe became Bunsen's successor in 1851 (Bunsen having been called to Heidelberg), he maintained relations with all of the Giessen chemists and visited them not infrequently. Upon Justus Liebig's transfer to Munich in 1852, Kopp and Heinrich Will became Liebig's joint successors; the following year they divided up their duties, Will taking experimental chemistry and the directorship of the laboratory, with Kopp becoming professor of theoretical chemistry. In 1863 Kopp was called to Heidelberg, becoming a colleague of Bunsen; he remained there for the rest of his life.

Kopp's life work was investigating the relationships between physical and chemical properties of chemical compounds; he has rightly been regarded as one of the founders of the discipline of physical chemistry. But he was also active in a literary sense right from the beginning of his career - indeed, his first love as a student had been philology. His classic four-volume *Geschichte der Chemie* was complete by his 30th birthday. The first edition was quickly sold out, and he began



Hermann Kolbe

immediately to work on revisions for a second edition; he died 45 years later, the revision still incomplete. When Liebig left Giessen, new literary duties were added - principal editor of Liebig's *Annalen der Chemie*, and, with Will, managing editor of the annual *Jahresbericht der Chemie*. He continued these duties even after his transfer to Heidelberg.

Shortly after his arrival in Heidelberg he was asked by the Bavarian Academy of Sciences to write a history of modern chemistry in Germany, as part of a project to commission two dozen disciplinary histories in a series entitled *Geschichte der Wissenschaften in Deutschland*. The initiator of this project was Leopold von Ranke (1795-1886), one of the founders of modern critical historiography, whose goal was to write history "wie es eigentlich gewesen ist", that is, without thematic, didactic, or rhetorical coloration. Kopp had been strongly influenced by this German objectivist historiographical movement as early as the 1840s (2).

The result of this contract emerged in the early 1870s as *Die Entwicklung der Chemie in der neueren Zeit*. (3). Kopp did not, however, succeed in making this a history of German chemistry, despite (as he wrote Liebig in January 1871) numerous attempts to follow Ranke's national program (4). In his preface, dated April 1873, he took the offensive; he averred that science, being international by nature, can only be written from an internationalist perspective (5). The work was indeed aggressively international. The first two-thirds of the long crucial final chapter, covering the development of theories of molecular constitution during the most recent period (1840-1860), scarcely mentioned a German name - until he introduced the development of structure theory by August Kekulé (6). In effect, Kopp found Ranke's critical historiography

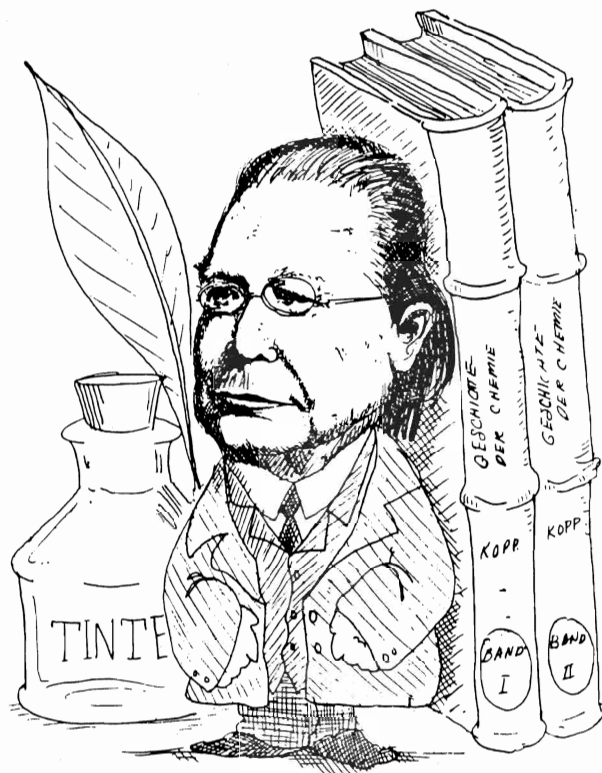
impossible to preserve if one attempted to maintain a national perspective, so the methodological standard necessarily dictated a broader approach to the material. Kopp's stance was all the more remarkable, as the book was written during the emotion-laden Franco-Prussian War, and shortly after the appearance of Adolphe Wurtz's history of chemistry, which was much attacked for its French chauvinism.

Kolbe's career followed a very different path. First at Marburg, then at Leipzig, Kolbe pursued organic chemical theory, developing his own idiosyncratic approach toward the elucidation of molecular constitution (he always avoided the term "structure"). In particular, Kolbe thought the chaining together of atoms was philosophically absurd and could not occur. Rather, implicitly following the dictates of the older radical theories, he thought that the atoms in a molecule were arrayed in a hierarchical manner, where one atom is always more central than its neighbors. He once used a military metaphor to describe this, speaking for instance of methyl as a "commando" unit with a carbon "corporal" and three hydrogen "privates". He even applied this approach to the benzene molecule, in contrast to Kekulé's cyclical chain theory (7).

As odd as it may sound, this approach proved very fruitful in the late 1850s and 1860s, making Kolbe one of the most esteemed masters of his field; but his productivity declined markedly from about 1870, when he devoted himself largely to bitter polemics against all of his purported enemies, foreign and domestic. He became pathologically chauvinistic, Francophobic and antisemitic from the time of the Franco-Prussian War. Increasingly, his ferocious and crude published attacks alienated him from the collegial community. His bombastic denunciation of van't Hoff's stereochemistry (1877) is perhaps best known; but he also regularly blasted the work of Kekulé, Adolf Baeyer, Johannes Wislicenus, Wurtz, and J. B. Dumas (8).

Strains appeared in the relationship between Kopp and Kolbe as early as 1854, when Kopp's physico-chemical studies led him to tilt in the direction of the newer type theory of Charles Gerhardt and Alexander Williamson (9). This theory was further developed by such workers as Wurtz, Kekulé, A. S. Couper, and A. M. Butlerov into the theory of chemical structure, a theory Kolbe violently and vocally opposed as excessively speculative. Kopp, more a physical than an organic chemist, never unequivocally signed on to the movement, but the structuralists felt, with reason, that his sympathies were with them. Kolbe always regarded Kopp's historical work as conscientious, but flawed by his refusal to be "critical" (by which Kolbe really meant tendentious) (10). He also did not have much regard for the emerging field of physical chemistry (11).

The brilliant German chemist August Wilhelm Hofmann, an exact contemporary of Kolbe and Kopp and a close friend of both, was also a part of these developments. He had spent 20 years as professor at the Royal College of Chemistry in



Hermann Kopp caricatured as a bookend for the volumes of his famous *Geschichte der Chemie*

London before following a call in 1865 to the University of Berlin. His most famous research, carried out in 1849 and 1850, formed a principal bulwark of the newer type theory. In 1867, he and four colleagues in Berlin formed the nucleus of a new German Chemical Society. Although a fair number of German chemists outside Prussia resented the implicit imperialism of a group of Prussians appropriating the name "German" (rather than more modestly calling themselves the Chemical Society "of Berlin"), Hofmann's timing was impeccable. Bismarck and his sovereign succeeded in forming the German Empire, centralized in Berlin, by the beginning of 1871, and many analogous pan-German organizations were formed around this time. Within a few years it was hard to argue with the resounding success of the new Society, both in terms of numbers of members and the size of the Society's *Berichte*.

In the fall of 1876, for his own amusement, Kopp wrote a comic fantasy describing a personified world of atoms and molecules, *Aus der Molekularwelt*. It lay in his desk for a few years, until he decided to revise and print it in honor of Bunsen's 71st birthday (31 March 1882) (12). By this time, Bunsen and Kopp had been intimate friends for many years. When Bunsen received a call to the University of Berlin in 1863 - the chair subsequently offered to Hofmann - he declined, but extorted from his administration as a condition of

his remaining not a raise in salary, but rather a promise that Kopp be called from Giessen as a second Ordinarius in chemistry. Their close association in Heidelberg extended from this year until Kopp's death almost 30 years later (13).

Since Bunsen was visiting Naples at the time of his 1882 birthday, Kopp introduced his fantasy by referring to the Naples aquarium, then suggesting that he and Bunsen pay an (imaginary) visit to an "aerarium" where they could watch molecules at play. The piece is filled not only with amusing conceits, but also with a variety of theoretical views of the nature of gases, atoms, valences, and molecular structures.

On 7 March 1882, while this work was in press, Kolbe visited Kopp in Heidelberg on a journey from Leipzig to his customary resort destination of Gersau, on the Lake of Lucerne in Switzerland. Two or three days later, having arrived in Gersau, he began a draft of a letter to Kopp, presumably completing it on the 12th; it was found among Kolbe's papers and is preserved in the library of the Deutsches Museum in Munich (14). I reproduce it here in my translation. My transcription of the original German is given in the notes (23), with all archaisms, and even a few obvious hasty errors typical of a preliminary draft, preserved *literatim*; cancellations are ignored, and interlineated revisions silently substituted. We cannot say for certain, though it is a reasonable assumption,

that a fair copy of the letter was actually made and sent to Kopp, for such does not seem to have survived.

Dear Kopp!

Gersau, 12 March 82

Your mysterious intimations to me on Tuesday regarding an upcoming publication by you lead me to conjecture that you have committed a transgression against me. I am all the more curious about its content, since my pen has always defended *you*.

I can image that the sharpness or form of my attacks, or, more properly, defense, against Kekulé does not please you, and that you also do not like my judgment of Baeyer's and Wislicenus' work. But in case you feel called upon to take Kekulé under your protection, I suggest you consider that you would be espousing the cause of a dishonest character, who does not shrink from thievery and falsifying the facts in order to gratify his ambition and satisfy his hatred.

If I understand him rightly, Kekulé is too cowardly to defy me publicly, now that his disgraceful actions have been revealed.

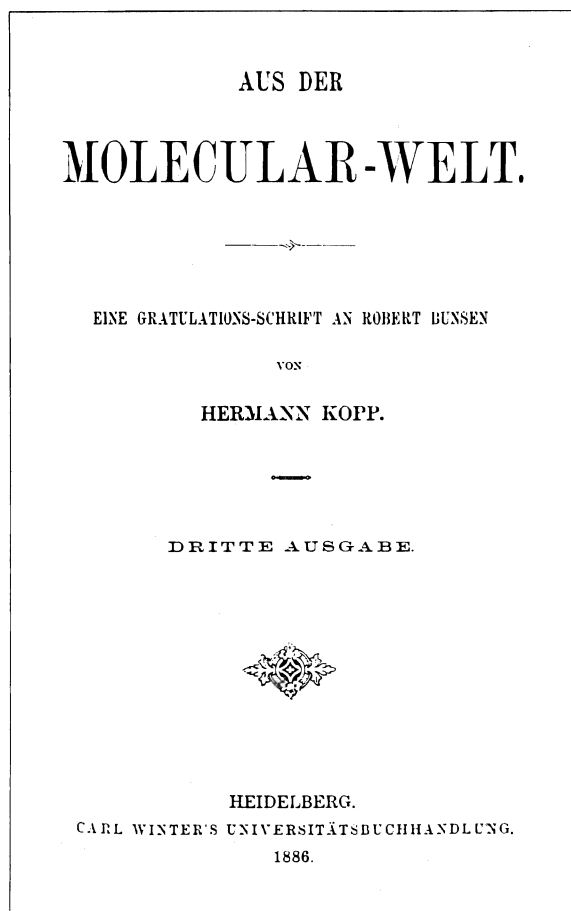
I should be just as sorry to see you try to pull Hofmann's chestnuts out of the fire. As deeply as I despise Kekulé, just as highly do I regard Hofmann, and not only as a chemist but also as a man, *as far as* his boundless vanity - his greatest enemy - does not mislead him. I am personally obliged to Hofmann from an earlier time by bonds of the greatest gratitude, and will never forget what he did for me; (15) but I cannot make myself his slave. Beyond a certain point, gratitude must yield to the duty that I believe I have toward our science.

Since Hofmann, accustomed in England to the highest favor, returned to Germany, he has discovered a rival in me, and withdrawn his earlier friendship; as often as I visited him in Berlin in hopes of a common but independent collaboration in chemistry, he has never reacted to my overtures, never graced me with a visit in Leipzig. He cannot stand it that I, quite contrary to my intentions, successfully compete with him in Germany.

In order to rule among chemists in Germany he founded the Chemical Society in Berlin for himself, and called it, not unintentionally, the 'German' [Chemical Society]. From this position - Hofmann is the Chemical Society, the others are his obedient lackeys and contrary Jew-boys - wherever there was an opportunity he has for years mistreated me, since I do not kowtow to him; in consideration of the past, I have held my tongue in these matters as far as possible. If only he had challenged me openly! But he is fond of setting others in motion, and remaining behind the curtains.

Don't take it amiss, if I openly say to you that I fear he has commandeered you as well, and is sending you into the encounter against me. I would be *very* happy were I mistaken in this, just as I should be just as sorry if, in case I am not in error, our good personal relationship of many years should thereby be troubled.

For, as I already told you orally, I am no longer the patient Kolbe, who, in blind faith (16) that others will act for me, allows everything to happen to me. I will decisively and powerfully repel every encroachment, every unjustified and uncalled-for attack, every misrepresentation of the truth in chemistry. I hope I *never* find myself in the position of defending myself against you.



Greet your dear wife and Bunsen, and thank you for the friendly reception at your home on Tuesday. Here the weather is heavenly, a southern climate. I feel like a new man.

Yours,
H. Kolbe

Unfortunately, this letter is representative of Kolbe's unbuttoned style of personal correspondence in his later years. His delusion of a Hofmann-led conspiracy against him suggests symptoms of paranoia; the extreme self-confidence of Kolbe's position, the assertion that Hofmann's enmity had arisen from Kolbe's independence and competition as a chemist, and the implicit threat in the penultimate paragraph, suggest megalomania. In fact, Hofmann had become repelled by Kolbe's language and attacks, essentially breaking his relationship with Kolbe after February 1873. Like Kopp, Hofmann was broad-minded and liberal - even if ambitious and inclined toward vanity, as Kolbe thought - and he found Kolbe's outspoken prejudices increasingly intolerable. The tragedy was that Kolbe had destroyed his own career by these actions. At the time of his call to Leipzig, in 1865, few European chemists could compare to him in productivity and significance of research. Within a few years his reputation had declined markedly. By the time of his death, in 1884, most obituarists felt called upon to comment on these events, and to offer exculpatory considerations - especially Kolbe's ardent love of the science of chemistry, and his fearless outspokenness.

As it turned out, Kolbe had less to worry about Kopp's "transgression" than he assumed. In the *Molekularwelt*, Kopp described valence theory in anthropomorphic terms: carbon atoms are "four-handed," oxygens "two-handed," and so on, with all hands "gripped" by another atom. This is a lovable characteristic of atoms, Kopp commented, like a child who needs to go to sleep with one hand in her mother's, the other thumb in her mouth. One-handed hydrogens link together and dance a "respectable but reckless Laendler;" other, more complex molecules perform chassés, allemandes, line dances, and other figures (17).

A long and important passage compares Kekulé's "democratic" notion of a chain of carbon atoms, where each atom is chemically equally important, with Kolbe's more hierarchical radical-based conception - though Kopp did not mention either chemist's name (18). Kopp obviously tried to be scrupulously tactful and fair here: both views have advantages, they are in fact quite similar in all important respects, and neither can ever hope for absolute validation. But a close reading leaves little doubt where Kopp's preference lay, namely with Kekulé; he even declared the incipient field of stereochemistry to be justified and respectable.

When he came to aromatic compounds, the carbon atoms were dancing "ring around the rosy," that is, in Kekulé's

benzene ring, of which Kopp confessed being exceedingly fond. "But in the evening of my life," he added, "I often find it a bitter thought, that I came to this world with the unfortunate characteristic of constantly seeking my place between two stools." So here, too, he confessed that he found the alternative theory of benzene appealing as well, and could not make a definite decision between them (19). Perhaps slyly, he did not name the alternative, but Kolbe's theory was one of the possible options to which he may have been referring. In any case, there is no evidence that Kolbe ever took serious offense or complained about Kopp's *Molekularwelt*.

In his preface, Kopp commented that he had edited the essay before publication, in order to eliminate some "harmless fun" that might have been taken too personally by some (20). There is no reason to doubt that in this masterly fantasy Kopp was indeed trying to be tactful with Kolbe and others who stood aloof from structure theory, by then the reigning orthodoxy among organic chemists, while subtly giving it his support. One of Bunsen's students later conjectured that Kopp was gently trying to teach his friend Bunsen about the modern theories, to which he was so indifferent, by means of this piece; another historian has suggested that Kopp wrote with more diffidence toward the theories than he really felt, since he was writing for the staunch empiricist Bunsen (21). But there is also every indication that in the final analysis Kopp counted himself among the fence-straddlers, as he forthrightly stated. This was not an uncommon attitude among the older generation in the last third of the century: not only Kopp and Bunsen, but also Liebig, Wöhler, and Dumas all fit this pattern after around 1840, as well as Liebig's successor and Kopp's former colleague in Giessen, Heinrich Will. Even Hofmann, closely associated with the new chemistry, was remarkably cautious in theoretical matters throughout his life. The essential difference between Kopp and Bunsen in this regard was that Kopp was fascinated by theories, even if ultimately noncommittal; Bunsen, on the other hand, was indifferent to the point of hostility to all hypothesis and theory in science.

It is likely that Kopp's inveterate indecisiveness towards chemical theory, which may ultimately have worked to limit his scientific reputation, contributed positively to his sterling qualities as a historian. His conventionalist philosophy and circumspect attitude toward the controversies of his day, along with the influence of Ranke's critical historiography, helped to produce those conscientious and judicious qualities of his historical work that were and are so greatly admired, in his day as in ours. To see the difference, one only need compare any of Kopp's works to the tendentious historical writings of his contemporaries Hoefer, Wurtz, Kekulé or Kolbe. Kopp sincerely and habitually - and largely successfully - tried to depict "wie es eigentlich gewesen ist;" Hoefer, Wurtz, Kekulé, and Kolbe all had important subtexts. It is only a shame that Kopp's occasionally nearly impenetrable Germanic style has severely limited the number of non-German chemists and historians

who have read his admirable books.

References and Notes

Acknowledgments: The research for this essay was generously supported by the National Endowment for the Humanities. I thank the staff of the Deutsches Museum, and especially Drs. Otto Krätz and Elisabeth Vaupel, for their many kindnesses to me on my visits to Munich, for helpful correspondence, and for permission to publish this letter draft. I also thank Vieweg Verlag, Wiesbaden, for use of their archive of 524 Kolbe letters.

1. There are no satisfactory biographies of either Kopp or Kolbe, though the articles and references in the *Dictionary of Scientific Biography* are useful. On Kopp, see also M. Speter, "'Vater Kopp': Bio-, Biblio- und Psychographisches von und über Hermann Kopp," *Osiris*, **1938**, *5*, 392-460, and E. Thorpe, *Essays in Historical Chemistry*, Macmillan, London, 1911, Chapter 12. I am currently engaged on a full-scale biography of Kolbe.

2. J. Weyer, *Chemiegeschichtsschreibung von Wiegand (1790) bis Partington (1970)*, Gerstenberg, Hildesheim, 1974, pp. 83-84 and 90-92; W. Strube, "Zur Geschichtsauffassung J. B. Trommsdorffs und H. Kopp's," *Wiss. Z. TH Chemie Leune-Merseburg*, **1963**, *5*, 377-378.

3. H. Kopp, *Entwicklung der Chemie in der neueren Zeit*, Oldenbourg, Munich, 1873. The work was published in three parts, beginning in 1871.

4. Kopp to Liebig, 16 January 1871, Bayerische Staatsbibliothek, Liebigiana IIB, cited in Speter, p. 421.

5. Reference 3, pp. viii-ix.

6. "Heranbildung der neueren Lehren über die chemische Constitution der Körper," *ibid.*, pp. 707-839.

7. H. Kolbe, "Ueber einige Abkömmlinge des Cyanamids," *J. Prakt. Chem.*, **1870**, *109*, 288-306, on 292-294.

8. See, for example, H. Kolbe, "Zeichen der Zeit: II," *J. Prakt. Chem.*, **1877**, *124*, 473-477 (on van't Hoff and Wislicenus); "Kritik der Rectoratsrede von Aug. Kekulé," *ibid.*, **1878**, *125*, 139-156; "Die chemische Synthese, ein chemischer Traum," *ibid.*, **1878**, *126*, 432-455 (on Baeyer); and "Meine Betheiligung an der Entwicklung der theoretischen Chemie," *ibid.*, **1881**, *131*, 305-323 etc. (an historical article on himself, Frankland, Kekulé, and all of his enemies).

9. H. Kopp, "Ueber die specifischen Volume flüssiger Verbindungen," *Ann. Chem.*, **1854**, *92*, 1-32, on 24-25, 28-29. Kolbe noted Kopp's apparent conversion in his letter to Eduard Vieweg of 23 October 1854 (in the archive of Vieweg Verlag, Wiesbaden, 311K, Kolbe letter no. 87).

10. Kolbe to Heinrich Vieweg, 15 June 1879 and 6 November 1882, Vieweg archive, Kolbe letters no. 443 and 482.

11. Kolbe to Heinrich Vieweg, no date but ca. January 1882, Vieweg archive, Kolbe letter no. 476.

12. H. Kopp, *Aus der Molekularwelt*, 3rd ed., Winter, Heidelberg, 1886. The preface, which contains a reference to the original date of composition (p. viii), is dated March 1882. The original press run must have been very small, as existing copies I have encountered

are from subsequent printings.

13. H. E. Roscoe, "Bunsen Memorial Lecture," *J. Chem. Soc.*, **1900**, *77*, 513-554, on 552, citing a letter from Kopp to Roscoe.

14. Sondersammlungen, Library of the Deutsches Museum, document no. 3633. The date of Kolbe's visit to Kopp and the date the draft was begun may be inferred from the circumstance that in the first line Kolbe wrote "vorgestern," then subsequently crossed it out and wrote "Dienstag." The nearest Tuesday to the date of the letter was 7 March 1882. It appears that he first dated the letter 10 March, later changing it to 12 March. Presuming the date of the visit was indeed Tuesday 7 March, he may have begun the letter on the 10th, initially under-counted the days back to the visit (hence having to change "vorgestern" to "Dienstag"), then finished it two days later and changed the date to the 12th, when the putative fair copy was made and posted.

15. I conjecture this has to do with Kolbe's call to Marburg in 1851. Hofmann was actually the first choice of the Marburg faculty, but he declined. In a letter to Liebig of 18 May 1851, Hofmann cited his personal relationship with Kolbe, whom he knew was next in line for consideration, as a factor in his decision: W. H. Brock, ed., *Justus von Liebig und August Wilhelm Hofmann in ihren Briefen (1841-1873)*, Verlag Chemie, Weinheim, 1984, p. 113.

16. These two words are canceled. I reproduce them here, as I cannot make out the interlineated revision.

17. Reference 12, pp. 2-3, 10-16, 30, 42-45. It is interesting to note how much of the satire *Berichte der Durstigen Chemischen Gesellschaft* (Berlin, 1886) derives from Kopp. Kopp even mentions apes who, counting their prehensile tails, are "five-handed" (p. 6).

18. *Ibid.*, pp. 17-29.

19. *Ibid.*, pp. 33-34.

20. *Ibid.*, p. viii.

21. H. Goldschmidt, "Erinnerungen an Robert Wilhelm Bunsen," *Z. angew. Chem.*, **1911**, *24*, 2137-2140, on 2140; G. Lockemann, *Robert Wilhelm Bunsen*, Wissenschaftliche Verlagsgesellschaft, Stuttgart, 1949, p. 195.

22. The familiar form of address indicates an intimate, and probably old, friendship.

23. The original German transcription is as follows:

Lieber Kopp!

Gersau, den 12. März 82

Die mysteriösen Andeutungen, welche Du (22) mir am Dienstag über eine von Dir zu erwartende Publikation machtest, lassen mich ein Vergehen Deiner gegen mich vermuthen. Ich bin auf den Inhalt derselben begierig, um so mehr, als meine Feder Dich stets verschont hat.

Ich kann mir denken, dass die Derbheit überhaupt die Form meiner Angriffe oder richtiger Abwehr gegen Kekulé Dir nicht behagt, dass Dir auch meine Beurtheilung von Beyers [sic] und Wislicenus' Arbeit nicht gefällt. Aber ich gebe, falls Du Dich berufen fühlen solltest, Kekulé in Deinen Schutz zu nehmen, Dir zu bedenken, dass Du Dich eines unehrenhaften Charakters annehmen würdest, welcher Fälschung der Thatsachen und Diebstahl nicht scheut, um

seine Ehrsucht zu befriedigen und um seinem Hasse Genüge zu leisten.

Kekulé ist, wenn ich ihn recht kapire, zu feige, um mir jetzt nach dem Aufdecken seines schmähhlichen Treibens, offen die Spitze zu bieten.

Eben so leid sollte es mir sein, wenn Du für Hofmann die Kastanien aus dem Feuer solltest holen wollen. Wie ich Kekulé tief verachte, so hoch schätze ich Hofmann und zwar nicht bloss als Chemiker sondern auch als Mensch, *so weit* seine unbändige Eitelkeit - sein grösster Feind - ihn nicht missleitet. Ich bin Hofmann aus früherer Zeit persönlich zu grossem Dank verpflichtet, und werde nie vergessen, was er für mich gethan hat (15) aber ich kann mich nicht zu seinem Sklaven machen. Ueber eine gewisse Grenze hinaus muss die Dankbarkeit der Pflicht nachstehen, welche ich gegen unsere Wissenschaft zu haben glaube.

Seit Hofmann, in England durch höchste und hohe Gunst verwöhnt, nach Deutschland zurückgekehrt ist, hat er in mir einen Rivalen entdeckt und mir seine frühere Freundschaft entzogen; so oft ich ihn in Berlin besuchte, in der Hoffnung auf ein gemeinschaftliches aber unabhängiges Zusammenwirken in der Chemie, hat *er* auf mein Entgegenkommen nie mehr reagirt, nie mich in Leipzig mit seinem Besuche erfreut. Er kann es nicht vertragen, dass ich, ganz gegen meine Intention, mit ihm in Deutschland erfolgreich concurrirte.

Um in Deutschland unter den Chemikern zu herrschen, hat er sich die chemische Gesellschaft in Berlin gegründet, und sie nicht ohne Absicht "die Deutsche" genannt. Von dieser Stelle aus - Hofmann *ist* die chemische Gesellschaft, die andern sind seine gehorsamen Trabanten und widrige Judenjungen - hat er mich da ich mich ihm nicht unterdrücke seit Jahren wo sich Gelegenheit bot misshandelt; ich habe mit Rücksicht auf die Vergangenheit, so weit es anging, dazu geschwiegen. Hätte er nur offenes Visir gezeigt! Aber er liebt es, Andere in Bewegung zu setzen, und sich selbst hinter den Coulissen zu halten.

Nimm es mir nicht übel, wenn ich hier Dir offen sage, dass ich fürchte, er hat auch Dich gekapert, und schickt Dich gegen mich ins Treffen. Ich würde mich *sehr* freuen, wenn ich darin irrte, eben so wie es mir sehr leid sein sollte, wenn, falls ich nicht irre, unser jahrelanges gutes persönliches Verhältniss dadurch getrübt werden sollte.

Denn, wie ich Dir mündlich schon sagte, ich bin nicht mehr der geduldige Kolbe, welcher, im blinden Vertrauen, (16) dass Andre meine Sache führen werden, Alles über sich ergehen lässt. Jeden Uebergriff, jeden unberechtigten und unberufenen Angriff, jede Einstellung der Wahrheit in der Chemie, weise ich mit Entschiedenheit kräftig zurück. - Möchte ich *nie* in die Lage kommen, gegen Dich Abwehr üben zu müssen.

Grüsse Deine liebe Frau und Bunsen, und hab Dank für die freundliche Aufnahme bei Euch am Dienstag. Wir haben hier himmlisches Wetter, südliches Klima. Ich fühle ich [sic] mich ein ganz anderer Mensch.

Der Deinige,
H. Kolbe

Alan J. Rocke is Director of the History of Science and Technology Program at Case Western Reserve University, Cleveland, OH 44106. He is author of the book "Chemical Atomism in the Nineteenth Century" and is currently working on a biography of Hermann Kolbe.

CHEMICAL ARTIFACTS

The Butlerov Museum at the University of Kazan

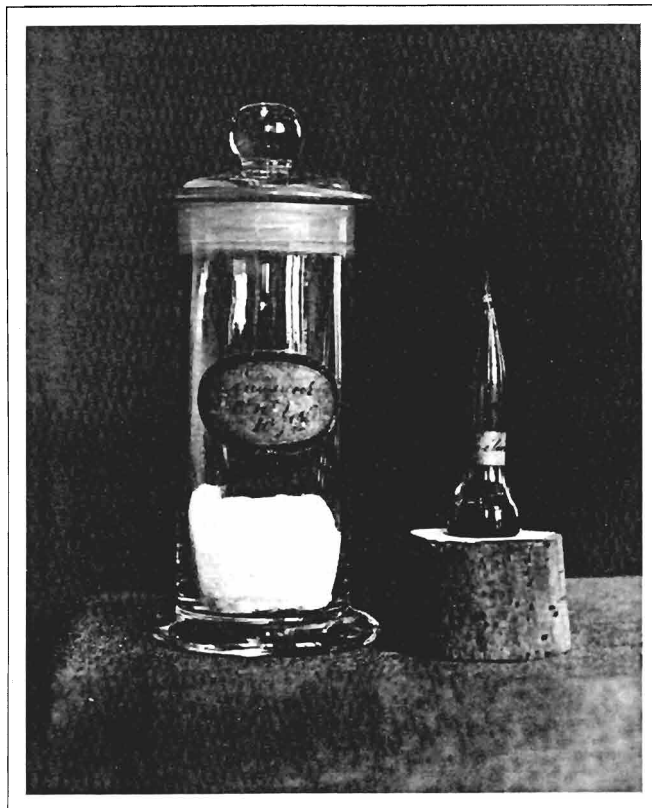
John H. Wotiz, Southern Illinois University at Carbondale

Kazan, the capital of the Tartar Soviet Republic, is located about 1000 kilometers east of Moscow near the Volga River (1). The University of Kazan was founded in 1804 and remained for a long time Russia's eastern most outpost for science and higher education. Its cultural influence reached into the Volga region, the Urals, Western Siberia, the Caucasus, as well as into Kazakstan and Central Asia. One of its early graduates was Nikolai Lobachevsky (1793-1856), the founder of non-Euclidian geometry. As Rector and Chairman of the Building Committee (1827-1846), he was responsible for the construction of many of the university buildings, including those for chemistry.

The Butlerov Museum houses artifacts relating to the work and careers of several famous Russian chemists connected with the University of Kazan. Though chemistry was first taught at Kazan in 1805 by German faculty largely imported from the Baltic region (2), it did not achieve an international



Aleksandr Milhailovich Butlerov



The sample of aniline prepared by N. N. Zinin in 1848.

reputation until the work of Nikolai Zinin (1812-1880) on the reduction of nitrobenzene to aniline. This was first accomplished in 1842, and a sample prepared by him in 1848 is among the items on display in the museum. Zinin was born in Azerbaijan and studied chemistry at Kazan and later with Liebig at Giessen. He was Professor of Chemistry at Kazan from 1841-1847, when he left to assume a position at St. Petersburg.

Zinin overlapped at Kazan with Karl Klaus (1796-1864). Born in Dorpat, Estonia, Klaus earned a doctoral degree in pharmacy at Kazan. In 1844 he isolated the element ruthenium, which he named in honor of Russia, and his original preparation is also among the items on display.

But perhaps the most famous chemist at Kazan was Aleksandr Milhailovich Butlerov (1828-1886), for whom the museum is named. A native of the Kazan region, he studied under Klaus and Zinin, and also worked in Wurtz's laboratory in Paris. In 1851 he became Professor of Chemistry at Kazan, where he remained until his departure for St. Petersburg in 1868. During his tenure at Kazan, he also served twice as Rector of the University. Best known for his introduction of the concept of chemical structure, Butlerov's contributions, both theoretical and experimental, made Kazan "Russia's Chemical Mecca". Among the talented students who came to

work with him were Vladimir Markovnikov (1838-1904), Aleksandr Zaitsev (1841-1910), Sergei Reformatski (1860-1934), and Flavian Flavitskii (1848-1917). All became famous in their own right and all held, in turn, appointments at Kazan in the period 1868-1911. Artifacts relating to their activities, mostly in the form of various compounds synthesized by them, are also on display.

The Butlerov Museum is located in the University's Chemical Institute, which is also named in Butlerov's honor. Some of his original furniture is still used in the office of its present director, Academician Boris A. Arbuzov. Together with the other items on display, they provide the visitor with a good introduction to the development of 19th century chemistry at the University of Kazan. Using the scale developed in my article on "Chemistry Museums of Europe", I would rate the Butlerov Museum as a "3", that is, it contains "original items and reconstructions worth seeing" (3).

The University of Kazan is also home to the Arbuzov



The office of Academician B. A. Arbuzov, current Director of the Butlerov Institute of Chemistry at the University of Kazan. Some of the furnishings in the office belonged to Butlerov.



Chemicals prepared by Zinin, Butlerov, Klaus, Markovnikov and Zaitsev on exhibit at the Butlerov Museum

Museum, which commemorates the life and achievements of the Kazan organophosphorus chemist, A. E. Arbuzov (1877-1968). This is housed in the residence in which he lived for more than half a century, along with his original furniture, his musical instruments, and his many honors, prizes, and awards. However, most of the chemical artifacts relating to his career are located in the Butlerov Museum, so there is little of direct chemical interest to be seen in the Arbuzov Museum itself.

References and Notes

1. Based on a paper presented at the 198th National Meeting of the American Chemical Society in Miami, FL, 10-15 September 1989. I would like to acknowledge the generous assistance I received while in Kazan from Academician Boris A. Arbuzov and Professor Rauza P. Arshinova of the Butlerov Chemical Institute which aided in the preparation of this article.

2. For a comprehensive review of early chemists at Kazan University, see N. Brooks, *The Formation of A Community of Chemists in Russia, 1700-1870*, Ph.D. Thesis, Columbia University, 1989. Dr. Brooks' assistance is gratefully acknowledged.

3. J. H. Wotiz, "Chemistry Museums of Europe", *Chemtech*, 1982, 12, 221-228.

Dr. John H. Wotiz is Professor Emeritus in the Department of Chemistry and Biochemistry at Southern Illinois University, Carbondale, IL 62901 and is best known for his History of Chemistry Tour of Europe.

HARRY JONES MEETS THE FAMOUS

William B. Jensen, University of Cincinnati

The attitude of historians and biographers toward the use of anecdotes has been, to say the least, ambiguous (1). One was summarily dismissed them as "yesterday's gossip grown stale". However, William Ellery Channing was definitely of the opposite opinion when he declared that:

One anecdote of a man is worth a volume of biography

and Isaac D'Israeli concurred when he wrote:

Some people exclaim, "Give me no anecdotes of an author, but give

me his works"; and yet I have often found that the anecdotes are more interesting than the works.

R. A. Willmott was even more emphatic in praising their use, going so far as to compare the potential of anecdote in the hands of a skilled biographer to the legendary ability of Cuvier to construct an entire fossil skeleton from a single bone:

Occasionally a single anecdote opens a character. Biography has its comparative anatomy, and a saying or sentiment enables the skillful hand to construct the skeleton.

In short, though anecdotes may well be "the thistledown of biography", to use Clifton Fadiman's felicitous expression, the majority of biographers have been more than happy to use them to leaven their subject and have eagerly combed the diaries, letters, and biographical memoirs of their subject's contemporaries in pursuit of appropriate examples.

Though chemists are not particularly noted for either the volume or literary quality of their autobiographical utterances (2), the appeal of anecdotes is still very strong and has actually resulted in the publication of several collections of "Chemical Anecdotes" (3). Interestingly, an important source of such anecdotes relating to several well-known late 19th century chemists seems to have been almost universally overlooked by chemical biographers, most likely because they were not recorded in an explicitly biographical document in the first place. In fact, the document in question is actually a book-length, semi-popular account of the origins and revolutionary impact of the then new discipline of physical chemistry, and the anecdotes were discreetly tucked away at the back of the book in an appendix. Published in 1913, the volume was entitled *A New Era of Chemistry* and was written by a professor of physical chemistry at Johns Hopkins University by the name of Harry Clary Jones (4).

Jones was born in New London, Maryland, in 1865 and received both his undergraduate and graduate chemical training at Johns Hopkins, taking his Ph.D. under Harmon N. Morse (1848-1920) in 1892. This was followed by two years (Summer of 1892 - Spring of 1894) of postdoctoral study in the laboratories of Wilhelm Ostwald at Leipzig, Svante Arrhenius at Stockholm and Jacobus van't Hoff at Amsterdam. Most of the impressions and anecdotes recounted by Jones were a result of this trip. Upon his return, he was appointed first as an honorary fellow at Johns Hopkins and then, in 1895, as an Instructor. In 1898 he became an Associate and in 1900 an Associate Professor, followed by promotion to full Professor in 1903. Inspired by his experiences in Europe, Jones immediately launched a vigorous research program in the physical chemistry of solutions which, by the time of his death in 1916, had generated 158 research papers and a dozen books, of which the *New Era* was his 11th and the last to be published during his lifetime (5).



Harry Clary Jones

Jones' motives for writing the *New Era* are complex and will be dealt with in more detail later. Suffice it to say that the most uncharitable interpretation would be that much of it was a self-serving attempt to justify his own career by historically legitimizing his research program on the theory of solutions as the culmination of the classic work of his mentors: Ostwald, Arrhenius and van't Hoff. In keeping with this view, an entire chapter of the volume was devoted to a description of his own work, which was characterized as having resolved all of the difficulties present in the original theory of ionic dissociation, and in the introduction, Jones made it quite clear that he viewed himself as having lived through and participated in a series of great historical events (6):

My apology for adding another book to the literature of chemistry is that I have lived through the "New Era", have well known most of the men who have been instrumental in bringing it about, and have been a student of the three leaders in this movement - van't Hoff, Arrhenius and Ostwald.

Given this motive and the semi-popular propagandistic nature of the volume, it goes without saying that the last thing Jones would do is record publicly any negative impressions he may have had of the famous chemists he had encountered during his stay in Europe. In other words, in this respect the volume is less than candid (7). Nevertheless, Jones' comments and impressions are still worth noting.

In the cases of Dmitri Mendeleev (1834-1907) and August Kekulé (1829-1896), the first two chemists mentioned by Jones, we have only first impressions, since Jones was not personally acquainted with either of them and, by the time he encountered them, the first via a brief introduction and the second from a distance at a scientific meeting, they had already



Dmitri Mendeleev: "Shaggy gray hair and an enormous cranium"

become legends and proper objects of adoration for a young, freshly minted Ph.D:

I met Mendeleev in the Spring of 1894. His was a most impressive personality; of medium height and stocky build, his long, shaggy gray hair and enormous cranium gave him an unusual appearance. His intense interest in science in general, and in the nature of solution in particular, his disregard of the ordinary social forms, his unkempt appearance, all pointed to a man of genius, whatever that may mean.

... Kekulé was the exact opposite of Mendeleev. He was as handsome as a picture, and evidently solicitous about his personal



August Kekulé: "Solicitous about his personal appearance"

appearance. I heard him lecture in the Summer of 1892. His German lacked the guttural so often heard, and was really musical. This was probably due in part to the fact that he had been so long in Belgium, and had spoken so much French, and in part also to his inheritance.

His lecture was on hydrogen peroxide and ozone. It was unusually clear, and delivered with an elegance of manner that made a deep impression. The most memorable feature of the lecture was that he interpreted all of the facts in terms of the constant valency of the atoms present, and then spoke at some length on this subject. This was almost a necessary outcome of his views on chemical constitution. Personally, he was the most genial of men, and at that time was especially interested in pyridine, upon which he had just finished an elaborate investigation.

The fact that in later life, Mendeleev would only submit to a haircut once a year is well known (8), and Jones' observation that Kekulé was still defending the doctrine of constant valence



Josiah Willard Gibbs; "Overly modest"

in 1892 confirms Russell's statement that Kekulé never abandoned the doctrine during his lifetime, though by this date he was virtually alone in defending it (9).

The next incident, involving the American physicist, Josiah Willard Gibbs (1839-1903), is the only one not based on Jones' postdoctoral experiences:

The modesty of Willard Gibbs has already been referred to. This was strikingly illustrated in an experience which the writer had with him a few years before his death. It was formerly the custom of Ostwald to publish in the closing volume of his journal, the *Zeitschrift für physikalische Chemie*, the portrait of some illustrious man of science. In 1895 Ostwald desired to obtain a good photograph of Willard Gibbs, and as I had recently returned to this country from Ostwald's



Sir William Ramsay: "The most skillful pair of hands that I had ever seen"

laboratory, he wrote me to secure for him the desired photograph.

I wrote Gibbs and extended to him Ostwald's request. Gibbs replied that he would gladly send the photograph which I desired to forward to Ostwald, but he was sure there must be an error somewhere. There could be no reason why Ostwald should want to publish his portrait in the *Zeitschrift*.

The photograph came, but with it a letter stating that he still could not understand the request of Ostwald, and that he reluctantly sent the picture with the understanding that I was to take all responsibility in the matter. I replied that this I would cheerfully do. Such a characteristic is quite common in really great men. They are nearly all perfectly natural. They can afford to be.

This incident is certainly in keeping with what is known of Gibbs' personality, though it is not mentioned in the official biography of Gibbs by Wheeler (10).

On his return trip from Europe, Jones also had the opportunity to meet Sir William Ramsay (1852-1916). This meeting was probably suggested by Ostwald, since he and Ramsay had been close friends since their first encounter at the 1890 meeting of the British Association in Leeds (11):

When returning from my studies of two years on the continent of Europe, I spent three weeks in London in the Spring of 1894. During this time I saw much of Ramsay both in the laboratory and in his home. The genial, attractive, and hospitable characteristics of the man were just such as to draw to him a young man. He impressed me then as having the most skillful pair of hands that I had ever seen at work in the laboratory. His glass-blowing, his manipulation in general, were unique

One incident is really of historical interest in connection with the

discovery of argon. The evening before I sailed for home I was invited to dine with Ramsay at his home. It being in May his family had already gone to Scotland. After dinner, over the cigar, he told the story of Rayleigh's discovery that atmospheric nitrogen was heavier than chemically pure nitrogen. He said Rayleigh had asked him to cooperate in isolating this heavier constituent in the nitrogen of the atmosphere. He then outlined the program which he had marked out for solving this problem. He was going to remove the oxygen from the air with hot copper. The nitrogen was to be taken out with hot magnesium; the ordinary constituents, carbon dioxide and ammonia, having been removed by the usual methods. In this way, said Ramsay, the heavier constituent in atmospheric nitrogen will be left behind, and we can then study it.

Any one who has followed the discovery of argon, recognizes at once that the above program was subsequently carried out to the letter. Indeed, Ramsay could have written, that evening, his paper on the discovery of argon, and simply waited for the predicted facts before publishing it. This incident shows the way in which Ramsay's mind worked. He had an insight into phenomena, and a foresight that has proved of incalculable value to him.

Jones' comment on Ramsay's skill at glassblowing is confirmed by both of the standard biographies of Ramsay (11, 12). As for the incident regarding the isolation of argon, here either Jones misunderstood the tense used by Ramsay or Ramsay wasn't being completely forthright with him, since we know that by May of 1894 Ramsay wasn't just planning the experiments but had already been conducting them for several weeks (13). His statement that Rayleigh had asked Ramsay to collaborate is also questionable since, from Travers' detailed study of the discovery and isolation of the rare gases, it is



Jacobus van't Hoff: "Of a decidedly nervous temperament"

apparent that it was Ramsay who approached Rayleigh, rather than the other way around (14).

Of his three mentors in physical chemistry: Svante Arrhenius (1859-1927), Jacobus van't Hoff (1852-1911), and Wilhelm Ostwald (1853-1932), Jones' comments on van't Hoff are perhaps the most enlightening (15):

I worked in the laboratory of van't Hoff in Amsterdam for a short while in the early Spring of 1894. My object was to study his method of investigating and his habits of thought. I found him a man of small stature and of a decidedly nervous temperament. The latter came no doubt in part from the extreme tension and concentration under which he worked.

He experimented all day in the laboratory, and it was the Spring vacation of the university. It is sometimes said that van't Hoff did not do much experimental work, or at least had not published the results of many investigations. The latter statement is true, but the former, from my own observations, I greatly doubt.

Van't Hoff looked upon experimental work, as he looked upon many other matters, in a different way from the average man. He did not carry out experiments and publish the results simply for their own sake. He looked upon experiments as means of testing generalizations; he regarded experimental work in a deductive rather than in an inductive light. I think it safe to say that many of the results obtained by van't Hoff were never published because he did not see any special object in publishing them. This is probably the condition which chemistry as a whole will reach in the next half-century

Another incident which occurred in van't Hoff's laboratory will illustrate his mental habit. Just before that time Baeyer had described a terpene derivative which was optically active, and which he thought did not contain an asymmetrical carbon atom. I asked van't Hoff what he thought of it. He replied, "We must have patience, it will come out all right", and it did. When the constitution of the compound in question was finally worked out, it was found to contain an asymmetric carbon atom.

Unfortunately, Jones' remarks on Arrhenius tell us little beyond reinforcing an image of him as the quintessentially jolly fat man (16):

I worked in the laboratory of Arrhenius in Stockholm in the Summer of 1893, and thus began a friendship which has grown with time. Arrhenius was at that period interested in the old Mendeleev theory of hydrates, and we worked on a problem bearing upon that theory. The results of the work were to show that this theory was fundamentally wrong

Personally, Svante Arrhenius is one of the most genial and jovial of men. His friends are almost as numerous as his acquaintances. When a few years ago it was proposed to publish a "Jubelband" to him in the series of the *Zeitschrift für physikalische Chemie*, to celebrate the 25th anniversary of the announcement of the theory of electrolytic dissociation, it was found to be necessary to publish two volumes, so many were those who desired to contribute.



Svante Arrhenius: "The most genial and jovial of men"

Jones' comments on Ostwald are even more disappointing, since the entire passage is devoted to a description of Ostwald's work and tells us virtually nothing about either his personality or physical appearance - at least nothing that is worth quoting (17).

As noted earlier, Jones died in 1916 at the premature age of 50. The 12th and last of his books, *The Nature of Solution*, was published posthumously and contained a biographical tribute to Jones by E. Emmet Reid, one of his colleagues at Johns Hopkins. Reid was vague about the exact cause of Jones' death but did drop hints that stress and overwork had played a role (5):

Work was his vocation, his vacation, his duty, his dissipation, his life, his death ... He worked long hours at his laboratory and went home to read proof. In summer he would go away for a vacation, but would spend it writing a book; when a bright Saturday afternoon came, he would get away to the country, but spend the hours riding over his three farms telling his farmers how to raise more corn and wheat on his fertile fields ... His unremitting work and an inherited tendency to nervousness brought on insomnia and melancholia which made his last months almost unbearable and led to his untimely death ... He learned many things but never learned to rest.

In his autobiography, written 55 years later, Reid, who was 100 years old at the time, was more candid about what had happened and confessed that Jones had actually committed suicide. Jones, wrote Reid, had become (18):

... obsessed with the fear of impending disaster. He could not trust himself or anyone else. If he wrote a check he would take it around

several times, asking persons whether it was possible for it to be "kited" so as to wipe out all the money he had.

One day Professor Morse went to him and suggested that he take a little vacation, telling him that the rest of us would care for his students until he returned. This set him wild. "It was a plot to get him out of the city so that his chair could be declared vacant."

... He would spend an hour in my office going over and over again his troubles, and then he would be back within the hour. On the average he must have spent half of each working day in my office. Then Saturday afternoons and Sunday he would telephone me to come out to his house for more of the same ... To have refused to listen to his troubles would have aggravated his fears. This went on for months, until he finally took the cyanide that he had long carried in his pocket.

Interestingly, the behavior patterns which ultimately led to this tragic end were already apparent during Jones' stay in Europe and were commented upon by Arrhenius in a letter written to Ostwald in 1893 (19):

... Jones was a very energetic worker ... But he was like other American and Englishmen are for the most part. He took the whole thing as "business", almost like a competition, where one uses physical strength, but he was completely lacking in imagination and time for reflection ...

Given these opinions, one can only imagine what Arrhenius would think of the current state of American science, where this sort of behavior has now reached, to put it mildly, epidemic proportions.

However, the story of Jones' death doesn't end here. In 1976, in a talk at the Fall National ACS Meeting in San Francisco, reprinted in *Chemical and Engineering News*, another eventual centenarian, Joel Hildebrand (1881-1983), of the University of California - Berkeley, recounted the story of (20):

A certain American professor [who, misapplying the Raoult - van't Hoff equation] measured freezing points of concentrated solutions of calcium chloride and used them to distinguish solvent water from water of hydration and published the results. When their absurdity was revealed, the poor man killed himself.

Knowing of Jones' suicide and that this was a description of his work on the theory of solutions, the author wrote to Hildebrand in 1978 and asked if he was in fact referring to Jones and, if so, whether there was any evidence that Jones' suicide was linked to an adverse response to his research rather than to the financial problems emphasized by Reid. Though Hildebrand did not directly answer all of the questions, he did verify that he was indeed referring to Jones (21):

Harry C. Jones was not well qualified as a defender of the ionic theory. He had published a "Color Demon of the Dissociating Action of

Water". I wrote a criticism of it (*J. Am. Chem. Soc.*, 1908, 30, 1672) but before submitting it to the journal, sent a copy to Jones. He came from Baltimore to Philadelphia to see the evidence and had to be convinced. I was polite.

He measured the freezing points of concentrated solutions of calcium chloride and used the van't Hoff equation, valid only at high dilution, as van't Hoff had pointed out, to calculate the amounts of water of hydration and solution. It was Washburn, I think, who pointed out that his calculated water of hydration exceeded the total water in the apparatus. He had talked arrogantly as an authority on physical chemistry, so he had made no friends. It is easy to guess why he committed suicide.

Elements of Hildebrand's story are plausible. As mentioned in his account of his work with Arrhenius, quoted above, Jones had started his career as a critic of Mendeleev's hydrate theory of solutions and had naively assumed, like many early proponents of the ionic theory of dissociation, that the solvent played no role in the process of solution other than that of a chemically inert dielectric filler between the ions. However, in the course of a study of the freezing points of complex salt solutions, he and his students observed that the magnitude of the freezing point depression not only increased upon dilution, as predicted by the ionic theory, but, above a certain critical concentration, also began to increase, rather than decrease, with an increase in concentration. In other words, a plot of concentration versus freezing point depression showed a characteristic inflection point.

Assuming the validity of the simple equation relating freezing point depression and concentration, derived by Raoult and van't Hoff, Jones explained this effect by postulating that in the concentrated solutions part of the water became bound to the solute as water of hydration and no longer counted as solvent. That is, the solutions were effectively more concentrated than calculated on the basis of the total water used in making up the solution in the first place. As the solutions decreased in concentration, the fraction of the water bound as water of hydration decreased and the behavior gradually approached the values predicted by the simple theory of ionic dissociation. Comparison of the depressions calculated on the basis of the Raoult-van't Hoff relation (using the degree of dissociation obtained from conductivity measurements) with those measured experimentally allowed Jones to estimate the degree of hydration. Jones called his approach the "new hydrate" theory of solutions and later, after extending the work to nonaqueous systems, he employed the term solvate theory (22).

Critics were quick to point out that Jones' use of the Raoult-van't Hoff equation was highly questionable in the case of concentrated solutions, and that some of his data on the variation in the degree of hydration with concentration appeared to be incompatible with the law of mass action (23-25). Though not among the critics of Jones' theory mentioned by

Servos (26), Edward Wright Washburn (1881-1934) is certainly a likely candidate, since he was an early pioneer in the use of transference numbers to determine the relative hydration of ions. This procedure, in contrast to that of Jones, which predicted as much as 100 moles of water of hydration per mole of electrolyte, gave much smaller hydration values (27).

There are, however, some problems with Hildebrand's story. No paper with the title "A Color Demon of the Disso- ciating Action of Water" is to be found among Jones' publica- tions and the paper which Hildebrand cites as his supposed rebuttal of Jones has nothing whatsoever do with the theory of solutions and makes no mention of Jones. Likewise, the basic flaws in Jones' work were all pointed out as early as 1905 and apparently did not change or intensify in the period before his suicide. Finally, though Washburn's 1915 textbook of phys- ical chemistry (28), in sharp contrast to the 1913 textbook by Jones' student and collaborator, Frederick H. Getman (1877- 1941) (29), pointedly ignored Jones' work, Washburn himself actually employed Jones' procedure in his text to determine the hydration of sugar in water solutions (30). Of course, the incorrect citations by Hildebrand may simply be the under- standable result of a century-old memory and a detailed study of both his and Washburn's publications well may confirm at least part of the account. But, questions relating to Jones' death aside, there is little doubt that the complete story of the rise and fall of his solvate theory is yet to be told, since it appears to have been totally overlooked in most published accounts of the historical development of solution theory (31).

References and Notes

1. For an interesting discussion of the history and nature of anecdote, see C. Fadiman, *The Little, Brown Book of Anecdotes*, Little, Brown, Boston, MA, 1985, pp. xiii-xxii.
2. M. Millar and I. T. Millar, "Chemists as Autobiographers", *J. Chem. Educ.*, **1988**, *65*, 847-853.
3. See, for example, J. Hausen, *Was nicht in den Annalen steht*, Verlag Chemie, Weinheim, 1969, and R. E. Oesper, *The Human Side of Scientists*, University of Cincinnati, Cincinnati, OH, 1975.
4. H. C. Jones, *A New Era of Chemistry*, Van Nostrand, New York, 1913, pp. 299-311.
5. A biography of Jones by E. Emmet Reid and a complete bibliography of his publications appear in the posthumously published volume, H. C. Jones, *The Nature of Solution*, Van Nostrand, New York, 1917, pp. vii-xi and pp. 359-370.
6. See reference 4, Chapter 9 and p. iv. This book shows signs of hasty composition and, in this and other quotes from reference 4, I have sometimes had to correct Jones' spelling and, in at least one case, his grammar.
7. More candid comments may be present in a letter in the Edgar Fahs Smith Collection in which Jones wrote about his impressions during at return trip to Europe in 1904. This has been brought to my attention by Dr. Jeffrey L. Sturchio. Unfortunately, neither the Smith Collection nor the current staff of the Beckman Center seem able to locate it.
8. See Oesper, reference 3, p. 128 and O. N. Piszarsky, *Dmitri Ivanovich Mendeleev*, Foreign Language Publishing House, Moscow, 1954.
9. C. A. Russell, *The History of Valency*, Leicester University Press, 1971, Chapter 10.
10. L. P. Wheeler, *Josiah Willard Gibbs, The History of a Great Mind*, Yale, New Haven, CT, 1951.
11. W. Tilden, *Sir William Ramsay, Memorials of His Life*, Macmillan, London, 1918, pp. 117-118.
12. M. W. Travers, *A Life of Sir William Ramsay*, Arnold, London, 1956, p. 27.
13. *Ibid.*, pp. 107-109.
14. *Ibid.*, reproduction of letter on p. 104.
15. E. Cohen, *Jacobus Henricus van't Hoff: Sein Leben und Wirken*, Akademische Verlagsgesellschaft, Leipzig, 1912.
16. E. Riesenfeld, *Svante Arrhenius*, Akademische Verlagsgesellschaft, Leipzig, 1931.
17. N. I. Rodnyj and J. I. Solowjew, *Wilhelm Ostwald*, Teubner, Leipzig, 1977.
18. E. E. Reid, *My First One Hundred Years*, Chemical Publishing Co., New York, NY, 1972, pp. 118-119.
19. H. G. Körber, ed., *Aus den Wissenschaftlichen Briefwechsel Wilhelm Ostwalds*, Vol. 2, Akademie Verlag, Berlin, 1969, p. 123. Quoted in Servos (26).
20. J. H. Hildebrand, "From Then to Now", *Chem. Eng. News*, **1976**, *54*(Sept. 13), 26-30.
21. Letter of 11 July 1978 to the author from J. H. Hildebrand.
22. The best summaries are given in references 4 and 5.
23. W. Böttger, Review of three papers by H. C. Jones and coworkers, *Rev. Am. Chem. Res.*, **1905**, *11*, 67-69.
24. L. Kahlenberg, Review of H. C. Jones et. al., "Hydrates in Aqueous Solution", *Science*, **1907**, *25*, 962-964.
25. J. J. van Laar, *Sechs Vorträge über das thermodynamisches Potential*, Vieweg, Braunschweig, 1906, pp. 6-8.
26. J. W. Servos, *Physical Chemistry in America, 1890-1933: Origins, Growth and Definition*, Ph.D. Thesis. Johns Hopkins University, Baltimore, MD, 1979, pp. 129-131.
27. Compare reference 5, p. 312 with reference 28, p. 232.
28. E. W. Washburn, *An Introduction to the Principles of Physical Chemistry*, McGraw-Hill, New York, NY, 1915.
29. F. H. Getman, *Outlines of Physical Chemistry*, Wiley, New York, NY, 1913, pp. 211-216. The solvate theory continued to be mentioned in the Getman text well into the 1930's. See, for example, the 5th edition, 1931, coauthored by Farrington Daniels, pp. 202-205.
30. Reference 28, pp. 152-153.
31. Thus there is no mention of Jones' work in either R. G. A. Dolby, "Debates Over the Theory of Solutions: A Study of Dissent in Physical Chemistry in the English-Speaking World in the late Nineteenth and Twentieth Centuries", *Hist. Stud. Phys. Sci.*, **1976**, *7*, 297-404 or in J. H. Wolfenden, "The Anomaly of Strong Electrolytes", *Ambix*, **1972**, *19*, 175-196.

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

BOOK NOTES

All That Glitters. Readings in Historical Metallurgy, Michael L. Wayman (Editor), The Metallurgical Society of the Canadian Institute of Mining and Metallurgy, Montreal, 1989. x + 197 pp. Cloth (Typeset). \$40.00 for members of the Institute, \$50.00 for nonmembers.

This book is a collection of 43 articles published on the occasion of the tenth anniversary of the founding of the Historical Metallurgy Committee within the Metallurgical Society of the Canadian Institute of Mining and Metallurgy in Montreal. To promote historical studies, the Committee sponsored a regular monthly feature, entitled "Historical Metallurgy Notes", in the *Bulletin* of the Institute. These "Notes" received wide acclaim and were read by many people with great interest.

The articles in this volume cover the entire spectrum of metallurgy from ancient times to the present. The book is divided into two nearly equal sections: general articles, collected under the heading "The Development of Metallurgy" (17 articles), and specific Canadian articles, under the heading "Canadian Metallurgical History" (26 articles). Among the general articles one finds topics such as: native copper; Roman lead plumbing; old iron nails; metallurgy in prehistoric Japan; the origins of zinc and brass; the Catalan furnace; cast iron in Medieval Europe; smelting in Swansea; the iron works of Richmond, Virginia; manganese in the 19th century; the Bayer Process for alumina production; and the cyanidation process. Topics in Canadian metallurgical history include: the Forges du Saint-Maurice, Québec (the first iron-making operation in Canada) and other Canadian iron-making works; metallurgical operations at Deloro, Ontario (arsenic, cobalt, and silver); the history of gold, copper, nickel, lead, zinc, and aluminum production; and finally, the history of the Sherritt ammonia pressure leaching process - a milestone in Canadian metallurgical history.

The book is generally well produced, with numerous photographs and high quality paper. However, it is missing an index. The price is very reasonable because the Institute subsidized the project. In a way, this book is a first as, to the best of my knowledge, no other such collection of historical articles on metallurgy exists. It is comparable to the volume, *Readings in the History of Chemistry*, published some years ago by the *Journal of Chemical Education*.

The book should appeal not only to metallurgists, but to chemists, chemical engineers and, of course, historians. The editor and the Institute are to be congratulated for this magnificent effort, and I look forward to the publication of a second volume, probably some time in 1999. - *Fathi Habashi, Department of Mining and Metallurgy, Laval University, Québec City, Canada G1K 7P4*

Petrochemicals: The Rise of an Industry, Peter H. Spitz, John Wiley & Sons, New York, 1988. Cloth (Typeset). xxvi + 588 pp. \$29.95.

Even though the rapid growth of the petrochemical industry is a major part of the history of 20th century technology, previously there has not been a systematic history of this development. Peter Spitz has done an excellent job of rectifying this oversight. His description of this complex process not only clarifies what happened and why it happened, but also includes many illustrative examples describing selected companies, new production methods, products, and personal experiences that combine to produce a fascinating narrative.

At the beginning of the century, chemical manufacture of synthetic organic products used either coal or agricultural products, like molasses, as starting materials, and German companies were the leaders. By the 1920s some American companies recognized that the extensive petroleum and natural gas deposits in this country provided a cheap and convenient feedstock, but most foreign chemical companies didn't convert to petroleum-based operations until after World War II. Oil and gas were less readily available overseas, and cartels or agreements to limit production discouraged international competition.

Following World War II, U.S. petrochemical companies almost totally dominated the field. The war had destroyed many of the chemical plants in the rest of the world and swept away agreements that limited production. In the U.S. wartime efforts had made essential technical information widely available and greatly expanded plant capacity. Soon the market was crowded with American companies competing to produce chemicals that had formerly been controlled by a few corporations.

Competition further escalated in the 1970s as both U.S. and foreign companies greatly expanded production. Although disruptions of the oil supply in 1973 and 1978 raised profits briefly, the ultimate result was even greater rivalry and decreased profits. Finally, many companies were forced to decrease or eliminate their petrochemical operations. The worst of this retrenchment may now be over, but the outlook for renewed growth is unclear. After reviewing the current situation, the author argues that a solid basis now exists for further development and the future looks promising for petrochemicals.

Mr. Spitz has skillfully combined his own considerable

professional experience with extensive research and personal communications from industrial leaders to create an informative and enjoyable history. His book should be interesting both to historians of science as well as chemists who wish to better understand the development of the chemical industry. - *Harry E. Pence, Chemistry Department, SUNY-Oneonta, Oneonta, NY 13820*

Motion Toward Perfection: The Achievement of Joseph Priestley, A. Truman Schwartz and John G. McEvoy (Editors), Skinner House Books, Boston, MA, 1990. xxvi + 277 pp. Paper (Typeset). \$15.95.

This collection of ten papers and an introductory essay is based largely on a 1983 symposium celebrating the 250th anniversary of Priestley's birth. Though five of the essays have been previously published, most of them in the unlikely volume, *Oxygen and the Conversion of Future Feedstocks*, it is nice to have them accessible in a single location and in a uniform format. Priestley had many interests: scientific, theological, philosophical and political, and the papers in this volume attempt to present a balanced picture of this multifaceted man.

The introductory essay by the editors provides a brief biographical overview of Priestley. A discussion of the basis of his attitudes towards the work of Lavoisier and the French chemists is given in the essay by John McEvoy (Joseph Priestley and the Chemical Revolution); an overview of his non-chemical activities by Robert Schofield (The Professional Work of an Amateur Chemist); his theological views by John Brooke ("A Sower Went Forth": Joseph Priestley and the Ministry of Reform) and George Williams (Joseph Priestley: The Minister, Citizen and Church Historian); his political activities by Issac Kramnick (Eighteenth Century Science and Radical Social Theory: The Case of Joseph Priestley's Scientific Liberalism) and Martin Fitzpatrick (Priestley Caricatured); his philosophical interests by A. Truman Schwartz (Priestley's Materialism: The Consistent Connection); his activities in America by Derek Davenport (Joseph Priestley in America: 1794-1804) and Donald D'Elia (Joseph Priestley and his American Contemporaries) and his family life by H. John McLachlan (Mary Priestley: A Woman of Character).

It must be confessed that this brief characterization of each essay is slightly misleading as each of them clearly shows how all of Priestley's activities were logically interrelated by his world view and how artificial it is to separate out just his chemical work. Curiously, about the only item missing from this volume is a good treatment of his laboratory discoveries in pneumatic chemistry, perhaps because it was assumed that this topic has already been worked to death in standard histories of chemistry.

The physical appearance of the volume is generally attractive, though the reproduction of the caricatures in Fitzpatrick's essay is rather poor, with many of them appearing to be slightly

out of focus. Overall, however, this is an excellent introduction to Priestley and is highly recommended to chemists and historians alike (not to mention philosophers, theologians, political scientists and connoisseurs of 18th century political caricatures).

Chemistry As Viewed From Bascom Hill. A History of the Chemistry Department at the University of Wisconsin in Madison, Aaron J. Ihde, Department of Chemistry, University of Wisconsin, Madison, WI, 1990. xvi + 688 pp. Cloth (Typeset). \$25.00.

For many decades one of the unique features of the Department of Chemistry of the University of Wisconsin - Madison has been the presence of an internationally known historian of chemistry on its faculty. Among the many benefits of this unusual arrangement is the volume under review, which is, without a doubt, one of the most thorough and well written departmental histories ever produced. In the course of nearly 700 pages, Aaron Ihde documents not only the origin and rise of an important Midwestern chemistry department, but much of the history of an important Midwestern university as well. For one of the more apparent lessons of this book is how strongly the fortunes of the chemistry department, whether they concern the acquisition of new facilities or the hiring and firing of faculty, were tied to the administrative policies of the university as a whole and how these, in turn, were tied to the policies of the state legislature.

Not only does Ihde place the chemistry department within this larger political context, he also attempts to place it within the context of the overall development of science teaching and research at Wisconsin and, in so doing, provides valuable information on the history of biology, geology, physics, agriculture and engineering at the university. Finally, and most importantly, Ihde also deals with the research of key figures within the department, a feature which is missing, to the best of my knowledge, from all previously published departmental histories - a somewhat ironic fact, since most chemistry departments would probably insist that research, to paraphrase a once popular advertising slogan, was their most important product. And, of course, it goes without saying that Ihde's expertise as an historian of chemistry allows him to place this research within the larger context of the development of chemistry as a whole.

In spite of these larger themes, Ihde has also managed to fulfill successfully the most important, though more mundane, obligation of a departmental historian - namely, to chronicle accurately and thoroughly the succession of faculty, buildings and students that compose the outer manifestations of every department's history. Ihde takes his detailed history of the department only up to 1952, probably, in part, because it becomes increasingly difficult beyond this date to accurately assess the course of events and the careers of faculty, most of

whom are still active, and, in part, because both the cast of participants and number of events become almost overwhelming. Nevertheless, a 39-page epilogue and several appendices bring at least the chronological data up to 1987.

There is no doubt that this book is a labor of love on the part of the author. Unfortunately, professional historians have become less and less willing to deal with regional themes of this type and may even feel that the book is too much about too little. Likewise, departmental alumni may feel overwhelmed by its size, since their usual idea of a departmental history is a quick read of the coffee table variety which is long on photographs and anecdotal nostalgia and short on historical detail and insight. If so, I feel that both groups are mistaken and that the true value of this wonderful gift that Ihde has given to his department will become more and more apparent with the passage of time.

The book itself is attractively typeset and is well illustrated with photographs. Copies can be ordered by writing directly to the Department of Chemistry, University of Wisconsin, Madison, WI 53708. - William B. Jensen, University of Cincinnati, Cincinnati, OH 45221

EVENTS OF INTEREST

* ACS Books has begun publication of its new series *Profiles, Pathways and Dreams*. Edited by Jeffrey I. Seeman, the series will consist of 22 autobiographical volumes by famous contemporary organic chemists, most of them lavishly illustrated with photographs. A review of the first three accounts, by John D. Roberts, Ernest L. Eliel and Donald J. Cram, will appear in the next issue (No. 8, Winter 1990) of the *Bulletin*.

* The Bruccoli Clark Layman Publishing Company is planning to publish two volumes on the history of the American chemical industry as part of their larger series, *The Encyclopedia of American Business History and Biography*. The volumes on the chemical industry will be edited by John K. Smith of Lehigh University who is currently looking for contributors to help write short biographies for about 250 industrial chemists, entrepreneurs and executives, short histories of about 100 chemical and pharmaceutical companies, and about 35 general entries related to the chemical industry and government legislation. Interested parties should contact Dr. John Smith, Department of History, Lehigh University, Bethlehem, PA 18015 or telephone him at (215) 758-3360.

* Volume 51 of *Kagakushi Kenkyu* (Studies of the History of Chemistry), the official organ of the Japanese Society for the History of Chemistry, was published this summer. Founded in 1973, the society has a membership of about 450 historians, chemists, chemical engineers and high school chemistry teachers. In addition to its annual meeting, the society occasionally sponsors special symposia, often in conjunction with the annual meetings of the Japanese Chemical Society.

The society's journal carries the same phonetic title as the journal of the Japanese History of Science Society as the Japanese words for science and chemistry are phonetically the same, though represented by different written characters. To avoid confusion, the History of Chemistry Society often uses the abbreviated title of *Kagakushi* when referring to its journal. The journal's current editor is Dr. Hajime Kasiwaga, who is also the current president of the society. The journal is published quarterly and carries articles, notes, book reviews, notices of recent publications, and news of interest. The articles carry English summaries. The current annual subscription rate, including handling and shipping, is \$90.00. Orders should be sent to the Export Department, Maruzen Co. Ltd., P.O. Box 5050, Tokyo International 100-31, Japan (Telex, J-26517).

* 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.

FUTURE MEETINGS

Atlanta 14-19 April 1991

Five copies of 150-word abstract (original on ACS Abstract Form) by 1 January 1991. Title of paper by 1 November 1990.

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

* *Michael Faraday - Chemist and Popular Lecturer* (Co-sponsored by CHED). Contact Derek Davenport, Department of Chemistry, Purdue University, West Lafayette, IN 47907, (317) 494-5465.

* *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).

New York 25-30 August 1991

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

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

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

* *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 08625, (609) 882-2000.

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

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.

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

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.

Washington DC 23-28 August 1992

Five copies of 150-word abstract (original on ACS Abstract

Form) by 15 April 1992. Title of paper by 1 April 1992.

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

Denver 28 March - 2 April 1993

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

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

Chicago 22-27 August 1993

Five copies of 150-word abstract (original on ACS Abstract Form) by 15 April 1993. Title of paper by 1 April 1993.

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

* *C. K. Ingold, 1893-1970: Master and Mandarin of Physical Organic Chemistry*. Contact M. D. Saltzman, Department of Chemistry, Providence College, Providence, RI 02918, (401) 865-2298 or Derek Davenport, Department of Chemistry, Purdue University, West Lafayette, IN 47907, (317) 494-5465.

Tentative Future Symposia

(Please contact J. L. Sturchio if you are interested in organizing or participating in the following.)

* *Chemistry and Communications*.

* *History of Chemical Processes in Industry*.

* *Case Histories of Drug Discovery and Development*

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

FROM THE EDITOR'S DESK

The *Bulletin* continues to expand and I thought that the members of the Division might like a brief report on current happenings and future plans. As indicated on the inside of the front cover, we are in the process of adding an international board of Corresponding Editors. Usually associated with the history of chemistry organizations in their respective countries, these editors will assist us in reporting the activities of their societies and in preparing our annual bibliography. They

will also recommend papers from their members for possible publication in the *Bulletin*. To date Dr. Yasu Furukawa, Assistant Editor of *Kagakushi*, the journal of the Japanese Society for the History of Chemistry, and Dr. Peter J. T. Morris, Secretary of the Historical Group of the Royal Society of Chemistry, have agreed to serve.

A careful examination of the inside of the front cover of this issue will also reveal that the *Bulletin* now has an ISSN number which will assist us in attracting library subscriptions. Though we have not conducted an advertising campaign directed at libraries, our subscriptions have shown a steady growth and now include several libraries in England, France, Germany and Italy, as well as those in the United States. Again, we encourage the membership to request that their school libraries subscribe. Our articles are now routinely covered by both the annual *Isis* bibliography and by the occasional bibliographies published in *BCHOC News*. In addition, authors can now purchase reprints with covers at a rate of \$15.00 for 20 copies.

As for the future, encouraged by the success of the special Lavoisier issue last winter, we will devote the entire 1991 Winter Issue to the papers presented at the special Faraday symposium being organized by Derek Davenport for the Spring ACS Meeting in Atlanta. We would also like to expand our book review section and encourage members to submit reviews of recent items which they may have read. To aid authors in preparing reviews and manuscripts we also plan to print an *Author's Guide*, reproduced below, at least once a year in either the Fall or Winter Issue.

William B. Jensen, University of Cincinnati

AUTHOR'S GUIDE FOR THE BULLETIN FOR THE HISTORY OF CHEMISTRY

Manuscripts

All manuscripts should be typed using double-spacing and standard one inch margins and should be no more than 20 pages in length, unless arrangements have been made with the editor. Both footnotes and references should be indicated using consecutive numbers enclosed in parentheses and located within the body of the text. Consult recent issues of the *Bulletin* for specific examples. Unless otherwise indicated, manuscripts should be submitted in duplicate.

References and Footnotes

Formal references, clarifying footnotes, acknowledgments, etc. should be grouped together in the numerical order of their appearance at the end of the text under the heading *References and Notes*. Journal abbreviations should be in keeping with the recommendations of *Chemical Abstracts*. Examples include:

Papers:

1. C. J. Furio Mas, J. H. Perez, and H. H. Harris, "Parallels Between Adolescents' Conceptions of Gases and the History of Chemistry", *J. Chem. Educ.*, 1987, 64, 616-620.

Books:

2. T. W. Reid, *Memoirs and Correspondence of Lyon Playfair*, Cassell, London, 1899, pp. 56-57.

Edited Volumes:

3. A. J. Rocke, "Convention Versus Ontology in Nineteenth-Century Organic Chemistry" in J. G. Traynham, ed., *Essays on the History of Organic Chemistry*, Louisiana State University Press, Baton Rouge, LA, 1987, pp. 1-20.

If the author doesn't have access to italic and bold fonts, italic should be indicated by underlining with a straight line and bold by underlining with a wavy line.

Illustrations

Extensive use of illustrations and other graphic materials is encouraged. In many cases additional illustrations may be added by the Editor. Both black and white line drawings and half tone photographs may be submitted in the form of high quality photocopies. The latter materials will be converted into stippled drawings for publication.

Captions

The captions for non-original illustrations should be footnoted in a manner identical to that used in the body of the text and the sources incorporated into the *References and Notes* section, generally as the items at the end of the list. Consult recent issues of the *Bulletin* for specific examples.

Quotations

Longer quotations should be set off as separate paragraphs using smaller print and without quotation marks. All non-English quotations, except for short characteristic descriptive phrases and titles of publications, must be translated into English. The original non-English version may be reproduced in the notes if the author so wishes.

Biographical Data

The author should include an address for correspondence and a short description of current areas of interest in the history of chemistry for use in a short biographical sketch at the end of the article.

PARTING SHOTS

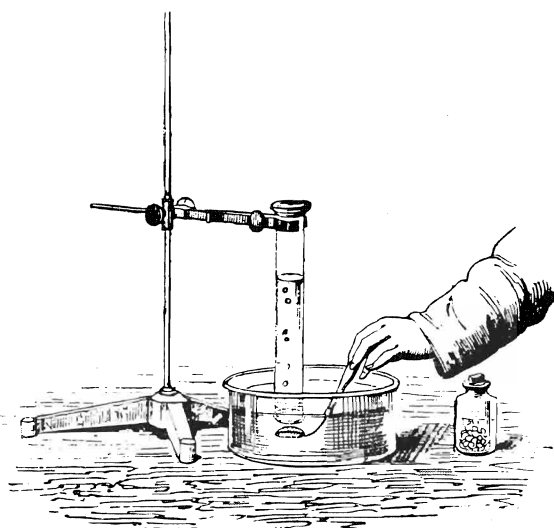
Reinventing the Hofmann Sodium Spoon

Several years ago the local high school chemistry teachers in Cincinnati invited Henry Bent to speak at a one-day conference on the teaching of introductory chemistry. As always, Henry gave a superb talk on the interactive use of lecture demonstrations, emphasizing a three-fold reinforcement of each concept: the direct observation of the chemical phenomena itself, its concrete representation by means of molecular models, and its abstract representation by means of chemical symbols and balanced equations.

At dinner afterward, the conversation turned to one of the demonstrations that Henry had used - the reaction of sodium metal with water done in a large crystallization dish on top of an overhead projector. The problem, Henry noted, was that although one could easily see how vigorous the reaction was and even demonstrate the production of NaOH by putting a little phenolphthalein in the water, it was almost impossible to collect and test for the dihydrogen gas which was also produced.

He recalled that he had once tried following the madly racing piece of sodium around the dish with an empty inverted test tube, hoping to collect enough dihydrogen so that the resulting mixture with the air in the tube would give a characteristic "pop" with a burning splint, but with no luck. One of the other teachers confessed that he had tried wrapping the sodium in a wad of paper in order to immobilize it long enough to get an inverted test tube full of water over it, and yet another had tried impaling the sodium on the tip of a spatula and holding it under the tube, but in each case the reaction with the water was so violent that the sodium immediately escaped.

Musing on this dilemma over dessert, the party finally



The Hofmann sodium spoon in action.



August Wilhelm Hofmann

agreed that the ideal solution would be a small wire mesh cup on a handle, similar to a small kitchen strainer, that could be held over the piece of sodium to confine it and also allow one to press it under the surface of water in order to position it beneath the opening of the inverted collection tube - the mesh allowing the dihydrogen gas to escape into the tube. Elaborating on this kitchen metaphor, one teacher even suggested that a device similar to a tea ball would work even better.

About a year later, while working on an article on the 19th century German chemist, August Wilhelm Hofmann (1818-1892), and an obscure atomic weight unit called the microcrith (1), I had occasion to examine carefully the first German edition (1866) of Hofmann's book, *Einleitung in die Moderne Chemie* (2). This consisted of 12 lectures on introductory chemical theory that Hofmann had given as part of his course on chemistry at the Royal College of Chemistry in London, where he had taught for 20 years before returning to Germany in 1865 to accept a position at the University of Berlin. This book is beautifully illustrated with original woodcuts showing most of the demonstrations used by Hofmann in his lectures and there, to my astonishment, in figures 3 and 4, both of which showed the reaction of sodium with water, was the very device that the high school chemistry teachers had wistfully concocted at dinner a year earlier!

A quick perusal of several of the laboratory supply catalogs in the Oesper Collection further showed that Hofmann's device had once been commercially manufactured. There it was on page 259 of the 1914 catalog for the E. H. Sargent Company of Chicago (2): "Item 3531, Sodium Spoon, 40¢." Examination of the 1929 catalog for the same company showed that by this date the original spoon had been further elaborated, was now called a sodium basket, now cost 85¢, and consisted of two

wire mesh hemispheres on the end of a handle with a metal slide to open and close them. When closed, one had a wire mesh version of the tea ball - the second idea suggested by the high school teachers. The 1937 catalog still listed the sodium basket, though it now cost 90¢, but by the time the 1967 catalog appeared, it had vanished.



The Sodium Basket

The sodium spoon wasn't the only demonstration device to come out of Hofmann's lectures. The Hofmann (universally misspelled as Hoffman) apparatus for the electrolysis of water, found in virtually every chemistry department, was also based on one of the figures in the book (4). Indeed, the same 1914 catalog of the E. H. Sargent Company listed no less than 12 items under the heading of "Hoffman Lecture Apparatus", all of them based on the devices described in Hofmann's original volume. By 1929 this list had shrunk to nine items, by 1937 to four items, and by 1967 just the standard electrolysis apparatus used today was listed. Probably no other chemist originated so many pieces of commercially manufactured lecture apparatus.

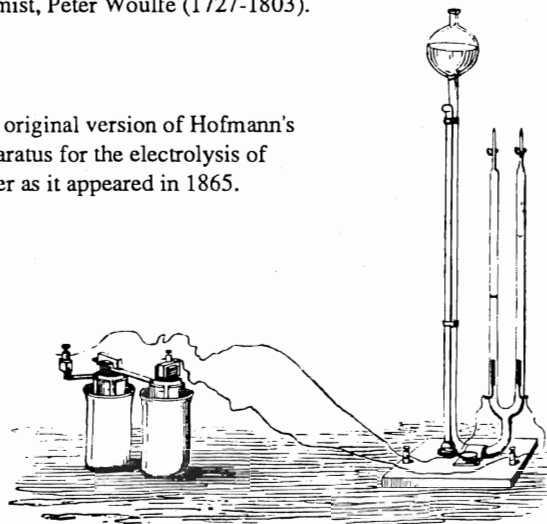
All of this has caused me to ruminate on the sad decline of the lecture demonstration as a teaching device in chemistry, though I will spare you the painful details, and to timidly suggest that the study of the history of chemistry may well have some very practical consequences for the teacher, in addition to the usual humanizing qualities which have been tradition-

ally invoked in order to justify its study.

References and Notes

1. W. B. Jensen, "Whatever Happened to the Microcrith?", *Bull. Hist. Chem.*, **1988**, *2*, 16-19.
2. A. W. Hofmann, *Einleitung in die Moderne Chemie*, 2nd ed., Vieweg, Braunschweig, 1866. The 1st edition (1865) was in English.
3. *Scientific Laboratory Apparatus*, E. H. Sargent Co, Chicago, IL, 1914 (No. 20), pp. 201-202, 259; *Ibid.*, 1929 (No. 35), pp. 308, 511; *Ibid.*, 1937 (No. 50), pp. 387, 448; *Ibid.*, 1967 (No. 115), p. 406.
4. A second example of misspelling apparently propagated by laboratory supply catalogs is the name "Woulff bottle" for the multiple necked bottles named in honor of the 18th century English chemist, Peter Woulfe (1727-1803).

The original version of Hofmann's apparatus for the electrolysis of water as it appeared in 1865.



COMING IN FUTURE ISSUES

- * "Jean-Baptiste Dumas: The Victor Hugo of Chemistry"
by Ben B. Chastain
- * "August Horstmann and the Origins of Chemical Thermodynamics"
by William B. Jensen
- * "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 Wyndham Miles
- * "The M. G. Mellon Spectrophotometer Collection"
by William B. Jensen
- * "The 1990 Dexter Address"
by Colin Russell
- * "A New Alchemy Catalog"
by Andrea Scotti
- * "The Museum of Early Philosophical Apparatus at Transylvania University"
by George Bodner
- * "The Rise and Decline of the British Dyestuffs Industry: An Object Lesson for American Industry"
by Martin D. Saltzman and Alan L. Kessler
- * "A Century of Chemistry at Clark University"
by Paul R. Jones
- * "A 'Lost' Silliman Chemistry Text"
by William D. Williams
- * "Laura Alberta Linton (1853-1915)"
by Mary R. S. Creese and Thomas M. Creese

BULLETIN FOR THE HISTORY OF CHEMISTRY

Subscription and Membership Application

- _____ ACS Member at \$10.00/year (includes full divisional membership)
_____ Non-ACS Member at \$12.00/year (includes affiliate divisional membership)
_____ Library Subscription (\$15.00/year)

Begin subscription with the year 19 _____

Name _____

Address _____

Signature _____

Return to Dr. William B. Jensen, Editor, *Bulletin for the History of Chemistry*, Department of Chemistry, ML 172, University of Cincinnati, Cincinnati, OH 45221. Checks should be made out to the Division of the History of Chemistry, ACS.

University of Cincinnati
BULLETIN FOR THE HISTORY OF CHEMISTRY
William B. Jensen, Editor
Department of Chemistry
University of Cincinnati, M. L. 172
Cincinnati, OH 45221

Non-Profit Org.
U. S. Postage
PAID
Cincinnati, OH
Permit No. 133

093530RY 12/90 ZC 515 1
EVAN J. MELHARD
UNIV OF ILL - SCH OF CHEM SCI
505 S MATHERS - BOX 3 NOYES LAB
URBANA IL 61801-3617