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.

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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 wag 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).

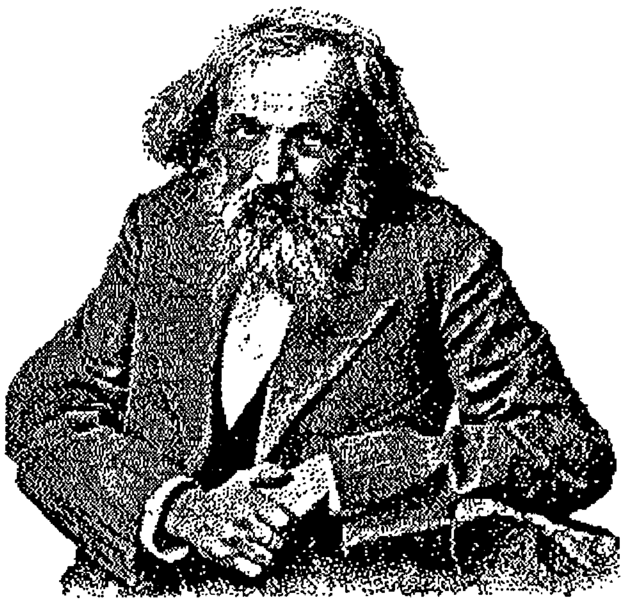
Harry Clary Jones

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.

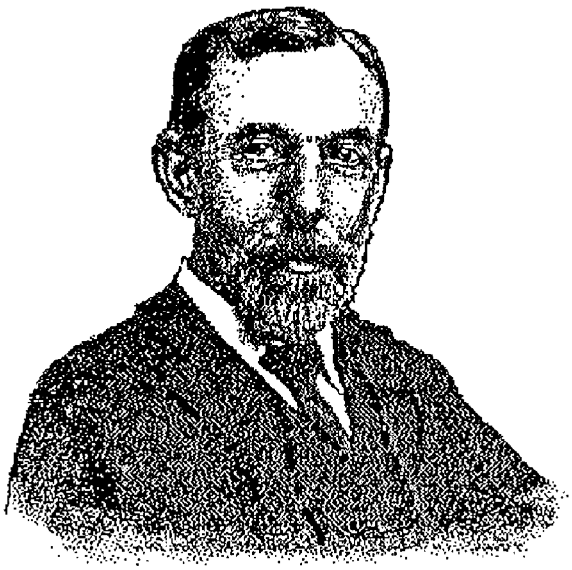
The fact that in later life, Mendeleev would only submit to a haircut once a year is well known (8), and Jones' observation that Kekulé was still defending the doctrine of constant valence

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 distinquish 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 Dissociating Action of Water" is to be found among Jones' publications 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 physical 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 understandable 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

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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. Pisarzhevsky, *Dmitri Ivanovich Mendeleyev*, Foreign Language Publishing House, Moscow, 1954.

9. C. A. Russell, *The History of Valency*, Leicester University Press, 1971, Chapter 10.

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12. M. W. Travers, *A Life of Sir William Ramsay*, Arnold, London, 1956, p. 27.

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14. *Ibid.*, reproduction of letter on p. 104.

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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 Soluton", *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.

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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