The Mystery of G. N. Lewis's Missing Nobel Prize

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"I call your attention to the curious incident of the Nobel prizes awarded to G. N. Lewis and Henry Eyring."

"But they were not awarded Nobel prizes," replied Watson.

"That was the curious incident," remarked Sherlock Holmes.

The Curious Incident of the Nobel Prizes (1)

Discovering G. N. Lewis

Ever since I was an undergraduate chemistry major at the University of Wisconsin I have wondered why Gilbert Newton Lewis (figure 1), or G. N. Lewis as he is universally known, was never awarded a Nobel prize. His work and name seemed to permeate virtually every aspect of my course work in chemistry, from the dot structures and electronic acid-base definitions of Freshman chemistry to the concepts of activity, fugacity and ionic strength taught in my course on physical chemistry. My senior year I purchased Dover reprints of both his book on valence (2) and the monograph by Luder and Zuffanti on the Lewis acid-base definitions (3) and avidly read both during the summer break following graduation.

In graduate school my acquaintance with Lewis continued to grow. Via my graduate course in thermodynamics, I became aware of both his classic monograph on this subject (4) and the fact that he and his collaborators were largely responsible for establishing our current data banks of free energy and entropy values. This latter knowledge was further reinforced by my research advisor, Edwin Larsen, who considered the monograph on oxidation potentials by Lewis's former student and colleague, Wendall Latimer, to be unsurpassed as a concise and convenient summary of useful thermodynamic data collected by what may be appropriately termed the "Berkeley School" of thermodynamics (5).

I also became aware of Lewis's pioneering work on the isolation of deuterium and his work on phosphorescence and the triplet state. Lastly, inspired by the monograph by Luder and Zuffanti and by the then recent attempts in the chemical literature to quantify the

Figure 1. G. N. Lewis
(1875-1946)

Lewis acid-base definitions, I took time out from my graduate work (much to the distress of my advisor) to write both a major review article (6) and a monograph (7) updating their original book, as well as to accept an invitation to participate in a major symposium on Lewis organized in 1982 by Derek Davenport at the 183rd National ACS Meeting in Las Vegas (8).

The Universal Question

After graduate school, I began to extensively read the literature dealing with the history of chemistry, including many biographies and autobiographies of famous chemists, and discovered to my delight that many authors, far more qualified than myself, were equally puzzled by the absence of a Nobel prize for Lewis.

Perhaps the first person to raise this issue in print was Arthur Lachman in a popular biography of Lewis published in 1955 (9). William Jolly, in his 1987 history of the chemistry department at Berkeley (10), would likewise devote an entire chapter to this question, and it would be repeated once more in a 1995 review article by Keith Laidler (1) and in the 1998 biography of Lewis by his son Edward (11). The most recent and most thorough discussion of this issue occurs in the 2008 monograph by Patrick Coffey on early 20th-century American physical chemists (12). Since both this author and Jolly have summarized any documents touching on this question that are to be found in either the archives of the University of California-Berkeley or in those of the Nobel Institute in Stockholm, much of my work has been done for me and it only remains for me to summarize their findings and conclusions.

A List of Possibilities

One or more of the above authors have suggested that Lewis should have received a Nobel prize for any one of the following five achievements:

1. His quantification of chemical thermodynamics.
2. His recognition of the electron-pair bond.
3. His isolation of deuterium.
4. His formulation of the electronic theory of acids and bases.
5. His work on phosphorescence and the triplet state.

According to Coffey (12), Lewis was nominated for the prize virtually every year between 1922 and 1944 and Jolly has provided a list of nominators from 1922 through 1935 (10). Though these include several former and future Nobel prize winners, such as Theodore Richards (1914 prize for chemistry), Karl Landsteiner (1930 prize for medicine), Irving Langmuir (1932 prize for chemistry), Otto Stern (1943 prize for physics), Fritz Haber (1918 prize for chemistry), and Max Planck (1918 prize for physics), their efforts were in vain. However, at least seven times (1924, 1926, 1932, 1933, 1934, 1940, and 1944) sufficient nominations had accumulated for the Nobel Chemistry Committee to commission one or more of its members to write summary reports and recommendations, all of which have been described by Coffey and which provide some interesting insights as to why Lewis was never awarded the prize.

The Quantification of Chemical Thermodynamics

It was Lewis's work on the quantification of thermodynamics that was the primary consideration in most of the reports issued between 1924 and 1934. In the years between 1899 and 1921, not only had Lewis and his various collaborators succeeded in generating the first reliable tables of free-energy and entropy values (4, 13), Lewis had also succeeded in extending the traditional approximate equations describing ideal gases and the colligative properties of ideal dilute solutions via his introduction of the concepts of activity and fugacity and, most importantly, he had made the first significant attempt to deal with the persistent problem of the anomalous behavior of strong electrolytes via his introduction of the empirical concept of ionic strength (14).

As revealed in a letter written in 1928 to James Partington, in response to the latter's request to nominate him for a Nobel prize, it was this work on thermodynamics that Lewis was most proud of (10):

While I have flirted with many problems, I was for many years pretty loyal to the main task which I had set for myself, namely, to weave together the abstract equations of thermodynamics and the concrete data of chemistry into a single science. This is the part of my work in which I feel the greatest pride, partly because of its utility, and partly because it required a considerable degree of experimental skill ... All of my papers dealing with potential measurements and the calculation of free energy from equilibrium measurements are included in the fifty papers which are listed on page 612 and following of our book on thermodynamics. If I have any claim to recognition, I think it would be based chiefly on these papers...

The first report to evaluate these claims was written by Svante Arrhenius (figure 2) in 1924. Though praising Lewis's work on thermodynamics as "careful and systematic," he felt that it did not involve any "new discovery or invention" and had simply applied principles long known to workers in the field and so did not merit a Nobel prize. In particular he criticized Lewis for his failure to come up with "some simple laws for concentrated solutions analogous to van der Waals' equation for strongly compressed gases." As Coffey has noted, these criticisms are highly questionable. Many Nobel prizes have been given to individuals who perfected and applied techniques originated by others, such as Theodore Richards' work on atomic weights or Manne Siegbahn's work on X-ray spectra, and, of course, Lewis had done, via his concepts of activity, fugacity and ionic strength, exactly what Ar-

Figure 2. Svante Arrhenius
(1859-1927)

rhenius had accused him of not doing. Arrhenius was 65 when he wrote this report and Coffey feels that it is obvious that he was no longer familiar with the current chemical literature.

The second report to evaluate these claims was written by Theodor Svedberg in 1926. Unlike Arrhenius, Svedberg fully acknowledged the great importance of Lewis's concept of ionic strength and also singled out his role in clarifying the meaning of Nernst's third law of thermodynamics. Nevertheless, while stating that "Lewis's work on chemical affinities is of such great importance that it would deserve to be honored with a Nobel prize in chemistry," he felt that there were still some unanswered, albeit unspecified, questions that needed to be clarified by future work and therefore concluded that "it was advisable to postpone the award of the prize for a few years." Of course this future work was never done since Lewis had ceased working in the field of thermodynamics with the publication of his 1923 monograph. In addition, one must note that Svedberg was competing with Lewis for the Nobel prize in chemistry, which he was given for his work on colloid chemistry the same year as he issued his report recommending postponement of an award for Lewis.

The reports for 1932, 1933 and 1934 were written by a relatively unknown Swedish electrochemist by the name of Wilhelm Palmaer, who basically did a hatchet job on Lewis's work on thermodynamics in a not so subtle attempt to explicitly deny him a Nobel prize. So obvious were his efforts in this direction that Coffey became convinced that this was done on purpose to appease Palmaer's close friend, Walther Nernst (figure 3), who had received the prize in 1920 for his formulation of the third law of thermodynamics. Lewis and Nernst had developed a mutual dislike that dated back to 1901 and Lewis's postdoctoral stay in Nernst's laboratory at Göttingen. In addition, in the succeeding years, when he was actively working in the field of thermodynamics, Lewis had repeatedly drawn attention to errors and ambiguities in Nernst's own work in the field (which may account for Svedberg's claim that Lewis had clarified Nernst's work), and Coffey feels that Palmaer's efforts to deny Lewis the prize for his work in thermodynamics were basically payback for these perceived insults.

The Electron-Pair Bond

During the above time frame (i.e., 1924-1934) Lewis's work on the electron-pair bond also came under consideration for a possible Nobel prize. This concept dated back to a paper written by Lewis in 1916 (15) and was elaborated in much greater detail in his monograph of 1923 (2). Basically, by postulating that the chemical bond was due to the sharing of electron pairs between atoms, Lewis succeeded in providing organic chemists with an electronic version of the chemical bond far more appropriate for describing the chemistry of the hydrocarbons and their derivatives than was the

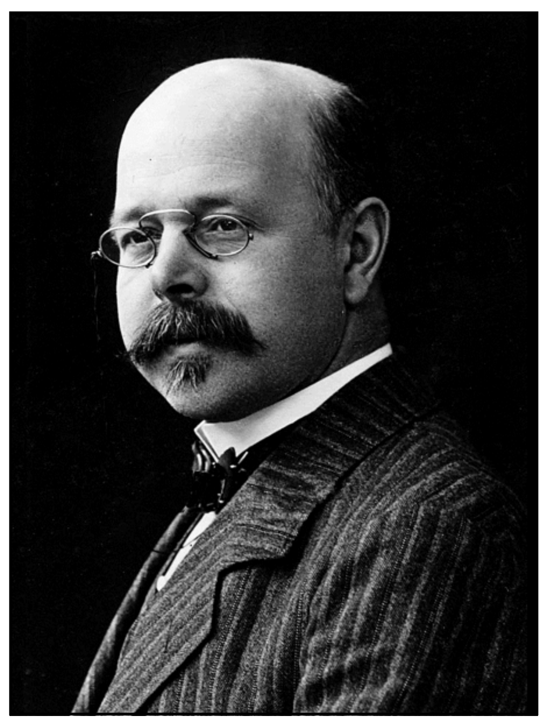

Figure 3. Walther Nernst
(1864-1941)

Figure 4. Irving Langmuir
(1881-1957)

highly polar, nondirectional, ionic bond of the inorganic chemist, though he was also able to show that the ionic bond was the end result of a series of progressively ever more polar covalent bonds between atoms due to an increasingly unequal sharing of the electron pairs. He was also able to show that the coupling of the electrons into pairs had significant consequences for both the reactivity and magnetic properties of molecules.

Lewis's emphasis on electron pairing, which was formulated nearly a decade before the enunciation of the Pauli exclusion principle (1925), would be retained as a central feature of all later quantum mechanical models of the chemical bond, including both the valence bond approach of Heitler, London, and Pauling, and the molecular orbital approach of Hund and Mulliken, and it is Lewis's work in this area, more than any of his other accomplishments, that has since become the focus of much attention on the part of chemical historians (8, 16-23).

In his 1924 report to the Nobel Committee, Arrhenius dismissed Lewis's electron-pair bonding theory in a single sentence, noting that it "is rather insignificant; and moreover the major part was done by Langmuir, and it is in opposition to the theory of Bohr, which is probably correct" (12). As Coffey has commented, Arrhenius was wrong on all accounts. Though Langmuir (figure 4) wrote extensively on Lewis's theory in the period 1919-1921, he was always careful to credit the basic concepts to Lewis. Unhappily others were not so careful and many began referring to it as the Lewis-Langmuir theory and, in England especially, even as the Langmuir theory alone. In addition, much of its popular vocabulary, such as "covalent", "octet theory", etc., had been coined by Langmuir rather than Lewis. Lewis had been deflected from immediately elaborating his theory by his service in the army during the First World War and there is ample evidence that he was less than happy about Langmuir's intrusion into what he considered as his personal bailiwick (18, 19). As for Bohr's atomic theory, however useful as a model for spectra, it would prove to be virtually worthless as a model of the chemical bond.

In 1932 the Nobel Committee requested that Theodor Svedberg (figure 5) prepare a report and recommendation dealing solely with Lewis's bonding theory. Though not repeating the errors and misinterpretations of Arrhenius's earlier comments, he nevertheless concluded that "Lewis's theory of valence neither has been nor can become of such importance for chemistry that an award of a Nobel prize should be motivated." Instead Svedberg felt that the future would lie with more quantitative concepts derived from the fields of spectroscopy and quantum mechanics.

From an historical perspective, all of this again is rather ironic, since by 1932 Lewis's model was on the

Figure 5. Theodor Svedberg
(1884-1971)

cusp of becoming the center piece of a newly reformulated electronic theory of organic chemistry in the hands of such British chemists as Thomas Lowry, Arthur Lapworth, Christopher Ingold, and Robert Robinson, where, in conjunction with qualitative resonance theory, it would hold sway until at least the early 1960s, when simplified quantum mechanical models, such as Hückel MO theory, would begin to have a gradual impact. But then again, we need to remind ourselves that Ingold never received a Nobel prize and, though Robinson did receive one in 1947, it was for his work on natural products synthesis rather than for his work on the electronic theory of organic chemistry.

In 1940 yet a third and final report on Lewis's bonding model was commissioned, written this time by a member of the committee by the name of Ludwig Ramberg, who was also a Professor of Organic Chemistry at the University of Uppsala. Despite being an organic chemist, Ramberg was well known for his dislike of "the so-called electronic theory of organic chemistry." Basing his comments largely on Svedberg's earlier report, he once again repeated the arguments that the future belonged with quantum mechanics and that Lewis's theory was simply too qualitative and elementary. He then concluded with the rather ambiguous assessment that, "From a pedagogical point of view, Lewis's theory undeniably holds quite a few advantages, perhaps mostly on an elementary level."

The Isolation of Deuterium

Though the concept of isotopes was a byproduct of the formulation of the radioactive decay laws and had also been experimentally established for the nonradioactive elements by Aston near the end of World War I via his work on mass spectroscopy, it was not until 1932 that Harold Urey (figure 6), a former student of Lewis, announced the discovery of an isotope of hydrogen having a mass of 2, known initially as heavy hydrogen and later as deuterium. This he and his coworkers had detected spectroscopically in samples of liquid hydrogen that had been isotopically enriched via fractional evaporation. A few months later Lewis initiated an experimental program designed to prepare macro samples of the new isotope via the fractional electrolysis of water, an approach independently suggested by Edward Washburn of the National Bureau of Standards, and over the next 16 months he would publish 26 communications describing the chemical, physical and biological properties of the new isotope (24). He also generously gave samples of the new isotope to other researchers.

Almost overnight, these actions made Lewis the world authority on heavy hydrogen and when, in 1934,

Figure 6. Harold Urey
(1893-1981)

the Nobel Committee in Chemistry commissioned Theodor Svedberg to write a report and recommendation on a possible prize for the discovery of deuterium, he immediately recommended that it should be shared between Urey and Lewis. However, a few months later Svedberg reversed himself. By then other laboratories had begun to isolate significant quantities of the new isotope, including Urey's laboratory at Columbia, and Lewis's achievement had begun to look more like something that was based on speed of publication rather than on uniqueness of technique (12):

One gets the impression that the research by Lewis in some measure has the character of a speed record. It is not improbable that workers in Urey's own laboratory, as well as those at Princeton, could have achieved the same results if they had only used Washburn's suggestions [i.e fractional electrolysis] unconnected with Lewis's work regarding heavy hydrogen.

Once again this is a curious evaluation since the same could be said of almost any experimental work in chemistry – if A hadn't done the work today then B would have eventually done it tomorrow. Nevertheless, based on this reevaluation, the 1934 Nobel Prize in Chemistry was awarded to Urey alone for "his discovery of heavy hydrogen."

There was a rumor that Lewis had jumped on the deuterium bandwagon with the explicit intent of winning a Nobel prize. Regardless of whether this is or is not true, what is known for certain is that once the prize was given to Urey alone, he immediately ceased work in the field and became relatively cool with regard to his personal relations with his former student.

The Lewis Acid-Base Definitions

Lewis had briefly stated his well-known electronic definitions of acids in bases in his 1923 monograph on valence (2, 6-7), but did nothing further with them until 1938, when he published a popular lecture on this subject in the *Journal of the Franklin Institute* (25) – a publication seldom read by your average chemist. Though this was succeeded by three followup papers in the *Journal of the American Chemical Society*, coauthored with Glenn Seaborg (26), it was not until the mid 1940s that the definitions began to gain traction in the chemical community, largely as a result of the popular articles, reviews, and monograph written by the team of William Fay Luder and Saverio Zuffanti (3). Since the latter book was not published until the year of Lewis's death, it is hardly surprising that this contribution was singled out only once by a nominator in 1944, and was never considered by the Nobel Committee to be worthy of a special report and recommendation.

In addition, as Jolly has pointed out (10), by the 1940s concepts equivalent to Lewis's definitions were already an established part of the new electronic theory of organic chemistry under the guise of Ingold's electrophilic and nucleophilic reagents, and in the field of coordination chemistry under the guise of Sidgwick's donors and acceptors, thus diminishing the uniqueness of Lewis's own claims.

Phosphorescence and the Triplet State

Lewis's final research interest dealt with the origins of color in organic compounds and especially with the nature of phosphorescence and the triplet state. In 1944 this became the subject of a special report and recommendation written by a member of the Nobel Committee by the name of Arne Fredga, who, like Ramberg, was a Professor of Organic Chemistry at the University of Uppsala. Though Fredga thought that Lewis's use of rigid glasses to trap reaction intermediates and excited states was very ingenious, he nevertheless felt that "decisive results do not seem ... to be won yet," leading the committee to conclude that it "wishes to wait for further development in this area and does not consider itself ready to award the prize to Lewis" (12).

This evaluation was perhaps fair at the time. As revealed by Lewis's last graduate student, Michael Kasha, who collaborated on this work, Lewis's interpretation of the role of the triplet state in phosphorescence was initially opposed by several prominent physicists, including James Franck and Edward Teller, and was not fully confirmed by ESR work until 1958, or well over a decade after Lewis's death (27).

Possible Defects in the Selection Process

In addition to the above personal reasons for the failure of Lewis to receive a Nobel prize, several authors have voiced the opinion that the fault might instead lie with the Nobel selection process itself. Thus Lachman, writing in 1955, observed that (9):

Due to human fallibility and human gullibility, this Nobel award has come to stand in the public mind for the highest possible distinction that can be awarded any individual. Unfortunately, this is far from true. After all, the [Nobel] committee itself is not composed of men of the highest distinction, and they are bound to make occasional false judgments. Many of the awards, to be sure, have been bestowed upon men who thoroughly deserved the recognition thus given them. However ... quite a few of the awards have gone to men who are not distinguished and whose selection was temporary and ill-advised. When two men of the outstanding eminence of Gilbert Lewis and Jacques Loeb are not included in the list of awardees, there is obviously something wrong with the system as a whole by which these men are selected.

Given what has been revealed by Coffey's analysis of many of the summary reports and their authors, there is a certain ring of truth to some of Lachman's accusations.

Interestingly, Lachman's views on the rather mediocre nature of some of the prize's recipients were also held by others, as revealed in 1929 in a letter to Lewis from the British chemist, F. G. Donnan (10):

Certain recent recipients of the Prize, though no doubt very worthy and excellent persons, do not strike one as particularly brilliant solutions of the yearly puzzle set by Nobel.

Perhaps more diplomatic are the concluding comments made by Laidler in 1995 when musing on the failure of both Lewis and Henry Eyring to receive a Nobel prize (1):

Not to win a Nobel prize puts one in excellent company. Neither Dmitri Mendeleev (1934-1907) nor Ludwig Boltzmann (1844-1906) won a Prize, but their *failure is easily explained by the fact that both had done their great work a good many years before the awards were first made in 1901. Less easy to understand is that prizes were never awarded to Lise Meitner (1878-1968), Christopher Kelk Ingold (1893-1970), and Friedrich Hund (born 1896). When Robert Sanderson Mulliken (1895-1986) was awarded his Prize for chemistry in 1966, he expressed regret that he had not shared it with Hund, and this would indeed have been appropriate.*

A Final Tragic Twist

Unhappily it is questionable whether Lewis would have found comfort in being a member of Laidler's "excellent company." Though he was the recipient of many honors, there is evidence that he was haunted by his failure to win a Nobel prize and was to a certain degree envious of the fact that his old nemesis, Irving Langmuir, had succeeded where he himself had failed, even though Langmuir had received the prize for his work on surface chemistry and not for his elaboration of Lewis's electron-pair bond and, after winning it, had also nominated Lewis for the prize.

Moreover, there is suggestive evidence that these insecurities may have been a factor in Lewis's death. This occurred on the afternoon of 23 March 1946 when his student, Michael Kasha, found his body on the floor of a laboratory flooded with hydrogen cyanide gas. Rumors quickly spread in the chemistry department at Berkeley that Lewis had committed suicide. However, the postmortem revealed that he had died of a heart attack and showed no signs of cyanide inhalation. Lewis had been working at a vacuum line with liquid hydrogen cyanide with the intent of investigating the effects of its high dielectric constant on the absorption spectra of organic dyes. Kasha believes that he suffered a fatal heart attack just after removing the cooling Dewar from the tube of liquid HCN he was working with and that, as his lifeless body lay on the floor, the unattended liquid HCN vaporized and the resulting pressure buildup blew the containment tube off the vacuum line, thus flooding the laboratory with cyanide gas (27).

Lewis had begun his lab work that morning, but had interrupted what he was doing in order to attend a special luncheon with an important guest of the department. Whereas in the morning he had been optimistic and brimming with ideas about future research possibilities, after returning from the luncheon that afternoon, he appeared to be withdrawn and morose. Only much later did Kasha recall that the special luncheon guest that day had been none other than Irving Langmuir and he now believes that this encounter may well have set Lewis to brooding over his imaginary failures and that the resulting stress may, in turn, have contributed to his fatal heart attack (12).

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