Presented at the Robert A. Welch Foundation Conference on Chemical Research, XX. American Chemistry--Bicentennial, Houston, TX, November 8 - 10, 1976

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GILBERT NEWTON LEWIS

Melvin Calvin

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GILBERT NEWTON LEWIS

Date of Birth: October 23, 1875, Weymouth, Massachusetts

Education:
1896 Harvard College (B.A.)
1898 Harvard College (M.A.)
1899 Harvard College (Ph.D. Chemistry
Thesis Advisor: T. W. Richards)

Married: 1912 Mary Sheldon
Two Sons: Richard, Edward
Daughter: Margery (now Margery Selby)

Professional Career:

1899-1900	Harvard College--Instructor
1900	Postdoctoral Traveling Scholarship University of Leipzig (Wilhelm Ostwald) University of Göttingen (Walther Nernst)
1901-1903	Harvard College--Instructor
1904-195	Manila, The Philippines--Superintendent of Weights and Measures; Chemist
1905-1912	Massachusetts Institute of Technology Assistant Professor - Professor
1912-1941	University of California, Berkeley Dean, College of Chemistry and Chairman, Department of Chemistry 1917-1918 Chemical Warfare Service, U.S. Army
1941-1946	University of California, Berkeley Professor of Chemistry

Honorary Degrees: Universities of Liverpool, Wisconsin, Chicago

Memberships:
The Royal Society
Royal Institution of Great Britain
Chemical Society of London
Royal Swedish Academy of Sciences
Indian Academy of Sciences
Franklin Institute of Philadelphia

Awards:
The Davy Medal (The Royal Society)
Arrhenius Medal (Royal Swedish Academy of Sciences)
Nichols, Gibbs, Richards Medals (American Chemical Society)
Distinguished Service Medal
Chevalier, Legion of Honor

INTRODUCTION

It has been a long time since I have been so concerned with a public appearance. I feel as though I am being watched, and I know that some of you will understand what I mean. It is an enormous responsibility to try and present to you, to the United States and to the world, Gilbert Lewis. I feel a little bit as though I were coming up for my first oral prelim examination, and, in fact, it is, because I have never tried to speak publicly about Gilbert Lewis before, and it is a very difficult and a very emotional task for me.

You have heard in the last two days a little bit of the scientific confusion of the latter part of the 19th and early part of the 20th centuries. You have heard a little bit of the two men (T. W. Richards and A. A. Noyes) who played such an important role in Gilbert's beginnings, and having heard about those men, I understand a little bit more of Lewis' teaching methods and the introduction of those teaching methods to Berkeley and a little more of his view of an experiment how you do an experiment. But Lewis was unique. He took from his teachers what he could, but he went alone in all of the things that he did.

I have prepared a summary of G. N. Lewis' biographical history and major scientific contributions, and have tried to catalogue the things Lewis did in science. I have listed five items more or less separate (but, in actual fact, they are not separate), any one of which would have been an entire life's work for a good chemist. I have put these together for you to peruse. What manner of man was this that could have done what he did? I will try to encapsulate as much as I can the formal history of Lewis' life. Instead of trying to discuss all five areas of his major contributions I will spend what time I have on the last two, with which I personally was involved.

<u>Biographical Information</u>: Lewis was born near Boston on October 23, 1875, just about ten years after T. W. Richards. At the age of nine, his father (a lawyer) and mother moved to Lincol nebraska. His formal education up to the age of 13 was entirely in the hands of his parents. It sounds reminiscent of what we heard about T. W. Richards. At the age of 13, Gilbert entered a University of Nebraska prep school, and from there, at 16, entered the University of Nebraska where he spent a couple of years. From there, at the age of 18, he went on to Harvard. where he received his B.A. in 1896. From 1896-1897 he was a master at the Phillips Academy, Andover, Massachusetts. He then returned to Harvard to finish his education, receiving his Ph.D. degree in 1899 at the age of 24. He did his thesis with T. W. Richards on the electrochemistry and thermochemistry (or thermochemical relations) of some zinc and cadmium amalgams. You can already see that he was into the area of thermodynamics from the very beginning. Lewis then became an instructor at Harvard from 1901-1903.

One of the interesting items in the chronology of Lewis' profesional career is that he went to Manila in The Philippines in 1904-1905 as the Superintendent of Weights and Measures. After that experience, he returned to Cambridge to join A. A. Noyes at the Massachusetts Institute of Technology, where he remained (passing through the ranks from assistant

MAJOR SCIENTIFIC CONTRIBUTIONS--G. N. LEWIS

CHEMICAL BONDING	
Electron Pair Bond	1913, 1916
Culminating in publication of VALENCE AND THE STRUCTURE OF ATOMS AND MOLECULES (1923)	1923
THERMODYNAMICS	
Culminating in publication of the classic text on THERMODYNAMICS AND THE FREE ENERGY OF CHEMICAL SUBSTANCES (1923)	1923
DEUTERIUM	
Isolation of Pure Deuterium	1933
Biological Effects of Deuterium	1933, 1934
Instability of Deuterons	1933
LEWIS ACIDS	1938, 1939
COLOR	1906, 1921, 1923
Theory of Color	1939
Photochemistry in Rigid Medium	1941
Fluorescence and Phosphorescence	1941
Paramagnetism	
Of Oxygen	1924
Of Triplet State	1944

professor to professor). In 1912 he was called to Berkeley and the University of California where he became Dean of the College of Chemistry and Chairman of the Department.

In this same year he married Mary Sheldon whom he brought to California with him and who survives him to this day.

Lewis retired from the "official titles" of Dean and Chairman in 1941, but he never retired from the laboratory. He actually died in the laboratory in 1946 in the course of performing an experiment.

LEWIS' RESEARCH CHARACTERISTICS

I have tried to summarize Lewis' peculiar characteristics as a scientist and these can be outlined as:

(1) Spirit of research and inquiry which he exhibited himself and engendered in all those around him. The external manifestation of this characteristic was in the form of the research conferences, and perhaps even in the hallways of Gilman Hall.

(2) As a result of Lewis' way of conducting research, American physical chemistry was dominated by his students and former associates from the early twenties onward.

(3) G. N. Lewis' general approach to science was intuitive, followed by a rigorous experimental testing of the intuition. The experimental approach was always direct and with a minimum of obfuscation.

(4) Lewis always used the very simplest type of apparatus (preferably homemade) for his experimental demonstrations, and took great pride in this simplicity. He always used visual demonstration of the effects which he was trying to explore.

(5) When Lewis would speak, he would always seek to have an experimental demonstration, no matter how large or small the audience, and that experiment was always something that everyone could appreciate (color change, magnetic moment, etc.).

(6) His curiosity and breadth of interest knew no bounds. He had ideas on the theory of money, archaeology, and the thermodynamics of glaciation (see Appendix II, list of Lewis' publication).

(7) Lewis had great intuitiveness in his approach to science, coupled with rigorousness of experimental observations, simplicity of equipment, freedom and range of his thinking. Lewis was not inhibited by current dogma, or boundaries of expertise. He had a tremendous breadth of scientific and nonscientific interests and contributions.

MAJOR SCIENTIFIC CONTRIBUTIONS

Gilbert and Mary Lewis came to Berkeley in 1912, already having experienced the influence of A. A. Noyes and T. W. Richards. As I listened to the discussions earlier in this conference, I heard for the first time -- because I knew neither of these two men who were his teachers -- some of the background of some of Lewis' idiosyncracies in his teaching methods as well as his research interests and methods. These came from his teachers, which is, of course, as it should be. When Lewis came to Berkeley he already had a free hand in bringing people, and Figure 1 shows some of the members of the Department of Chemistry in Berkeley in 1916. William Bray, Axel Olson, Merle Randall, Dorothy Parks, as well as Gilbert himself, are all in this picture.

FIGURE 1. Department of Chemistry, Berkeley, ca. 1916

Back row (L to R): Bray, Argo, Lewis, Gray, Borgstrom, Parks, Randall, Bisson, ?, Holmstrup, Cummings

Front row (L to R): Olson, Brighton, Cushman, ?

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From that time on, all the rest of the members of the department of chemistry were home-made (Berkeley graduates) and were at least in part responsible for the "clutter" you saw earlier in this conference in the time line developed by E. Bright Wilson.

Thermodynamics: At Berkeley, Lewis brought thermodynamics into its third and most generally fruitful area. Thermodynamics was invented in England by Rumford and Carnot, Joule and Kelvin, to help in understanding the mechanisms of steam engines. Thermodynamics was transformed into a general physical theory by Gibbs and van Hoff. The subject was brought to the useful stage of chemical thermodynamics beginning with Haber and Nernst, Brunsted and A. A. Noyes. Lewis' book of 1923 Thermodynamics and the Free Energy of Chemical Substances was the point at which the subject was brought into the hands of the chemists and chemical engineers of the world in a useful way. It may seem an anomaly that Lewis, who very carefully all his life shunned, as a personal activity, what he called "applied and engineering chemistry" should have produced this work of thermodynamics, upon which the entire chemical industry of the world is dependent, especially the petrochemical industry. It does not really give you the "flavor" of G. N. Lewis to say that.

But the true "flavor" of Lewis went beyond the simple and the practical. He was also a philosopher.

"There are ancient cathedrals which, apart from their consecrated purpose, inspire solemnity and awe. Even the curious visitor speaks of serious things, with hushed voice, and as each whisper reverberates through the vaulted nave, the returning echo seems to bear a message of mystery. The labor of generations of architects and artisans has been forgotten, the scaffolding erected for their toil has long since been removed, their mistakes have been erased, or have become hidden by the dust of centuries. Seeing only the perfection of the completed whole, we are impressed as by some superhuman agency. But sometimes we enter such an edifice that is still partly under construction; then the sound of hammers, the reek of tobacco, the trivial jests bandied from workman to workman, enable us to realize that these great structures are but the result of giving to ordinary human effort a direction and purpose.

Science has its cathedrals, built by the efforts of a few architects and of many workers."

(From: <u>Thermodynamics and the Free Energy of Substances</u>, G. N. Lewis and Merle Randall, 1923)

Lewis was <u>both</u> an <u>architect</u> and a <u>worker</u> in the cathedral of science, in a very real sense.

To me, thermodynamics was something I had to learn as a senior at college, and I learned it, using the Lewis and Randall book. Incidentally, I only recently learned, in a conversation with one of Lewis' earlier students who was in Berkeley at about the time the book was written, how it was done. Apparently Lewis wrote the book by pacing up and down and dictating, and Randall wrote it down and did all the calculations. When I learned of this <u>modus operandi</u> last fall (from Albert Loomis), it struck a very familiar note which I am sure a number of those in the audience will recognize. Incidentally, I failed to mention at the beginning of the talk that part of the reason I felt nervous about this presentation was that there would be present in the audience at least half a dozen people who knew Gilbert Lewis, but there must also be present several times that many who were taught by students who were taught by Lewis.

Chemical Bonding: Perhaps the most far-reaching and important step for chemistry that was taken by Lewis was the invention of the electron pair bond in the form he envisaged it. I will not try to give you an historical account of the origin of that idea, because a very excellent description of this has been written, entitled "The Origin of G. N. Lewis' Theory of the Shared Electron Pair Bond" by Robert Kohler, a professional historian of science, published in 1971. I do, however, want to say a word or two about Lewis' concept of the electron pair bond, because you have heard it referred to as being awaited in order to understand many of the chemical phenomena which were under investigation in the last part of the 19th and early part of the 20th centuries.

I came into this late, and can only remember Lewis' statement that there is only _one_ chemical bond, a shared electron pair, and everything else is electrostatic and is not a chemical bond at all. I came in pretty late to hear that, and it's only now that I am beginning to understand how he arrived at this conclusion. I taught the theory of the electron pair bond (shared pair) for many years, and I think it can still be taught that way(in a beginning course at least). That's what Lewis actually did: He brought the concept of the electron pair bond into being in a way that was understood readily by all chemists, including beginning students. He straightened out the organic chemists and the inorganic chemists, and he also straightened out the confusion that you heard described earlier in this conference. The electron pair bond concept which Lewis promulgated clarified and integrated these divergent views.

How did Lewis originate this concept of the electron pair bond? He didn't begin with first principles, as Gibbs did, and come out with the end result. He "guessed", and then he tested to see if it were so. That method of doing science went on all his life (I participated in some of the "guessing" myself and some of the testing of these guesses). I can just see how Gilbert must have done this: He saw the periodic table, with its rows of eight, and since molecules were real things, he asked what geometrical shape is characteristic of eight; it would be the eight corners of a cube. So, Lewis made the assumption that the atom was a cube and terminated every one of those corners with an electron. Thompson, in 1896, had discovered the electron. Lewis knew about this and sketched out his theory of the atom in 1902, but it wasn't until 1913 that Lewis discussed his ideas publicly in depth. (While Lewis was at MIT he did discuss his theory of the atom and electron pair bond from 1905-1912.) However, Lewis realized that there was something wrong with his initial concept, and gradually he brought the electrons together into a tetrahedral form, into four pairs at the corner of a tetrahedron; this concept was fully developed by 1916. The complete theory of the electron pair bond was published by Lewis in 1923 in a book entitled Valence and the Structure of Atoms and Molecules.

In doing some preparatory reading for this occasion I was struck by the fact that both of Lewis' major books were published in the same year, 1923. These were Valence and the Structure of Atoms and Molecules and Thermodynamics and the Free Energy of Chemical Substances. This must be a unique event in the history of science.

After Lewis brought the two electrons together at the corners of a tetrahedron, the rest of the theory of the electron pair bond followed rather straightforwardly. There were various kinds of inputs into this theory, but I can see Lewis visualizing that atom as a cube, just because of the rule of eight, Thompson's discovery of the electron a few years earlier.

In the book on chemical bonding Lewis had formulated the germs of many other basic scientific concepts; the paramagnetic susceptibility of oxygen, the paramagneticsm of free radicals and the exact definition of acids and bases. Lewis defined an acid as any atom, or group of atoms, which has a vacancy for an electron pair in the tetrahedron, and a base as being an atom, or group of atoms, which has an unshared electron pair which could occupy that vacancy; he expressed this idea very succinctly in 1923.

In a sense, the discussion of Lewis acids and the discussion of color really were born in Lewis' mind in 1923. Most of the ideas which were explored later in his publication on the theory of color, photochemistry in regid medium, fluorescence and phosphorescence, were expressed in those earlier papers of 1916-1923.

Deuterium: When Gilbert Lewis came to Berkeley, one of the conditions of his coming was that he should have a personal research assistant. Ronald McDonald, then Dan Luten, then Glenn Seaborg were involved in this progression. In chasing down Lewis' work on the deuterium purification, I called Dan Luten to find out what he knew about how the work began. He told me that Gilbert had been working on isotope separation, particularly of lithium, for some years. The moment that it became clear that a heavy isotope of hydrogen existed, Lewis realized that this would be the easiest one to separate, and, furthermore, that he probably already had some in the laboratory in a fairly pure form in the electrolytic cells that Giauque (Professor William F. Giauque) had been using for many years to make liquid hydrogen. Lewis began the work by using 20 liters of water from the electrolytic cells which Giauque had been using on the first floor of Gilman Hall and got 0.5 ml of water which was about 70% deuterium; he did this within the matter of about a year. The year after that Lewis had several milliliters of over 95% D_2O.

It was characteristic of Lewis to explore all the possibilities of his material, and he did both types of experiments at each end of the scale (physical and biological) with the material he had available to him.

About that time there was some suspicion about the stability of the deuteron (what it was composed of), and Lewis and Ernest O. Lawrence arranged to do some experiments in the cyclotron with deuterium gas. When the (any) target was bombarded with deuterons, neutrons emerged in great profusion. This work led to a paper with E. O. Lawrence on the instability of the deuteron.

On the other hand, Lewis also did experiments on the opposite end of the scientific spectrum. He took some of his original 0.5 ml of D_2O, put it on a petri dish and put some seeds in the petri dish. He also had a control petri dish with ordinary water and the same type of seeds. He watched the development of these seeds. The seeds in the ordinary water sprouted and grew, while the seeds in the heavy water did not; only a few of the seeds sprouted and grew poorly. Lewis recognized at that time (1933-1934) that the deuterium did not kill the seeds but, rather, inhibited their growth. When the seeds were removed from D_2O and placed in ordinary water and the usual nutrient, they would grow again. This observation was very important, but Lewis himself did not follow it up.

Lewis then fed some of his 95% D_2O to a mouse. The mouse apparently didn't like it at all, refused food and water for sometime thereaft but did survive. I think the reason the mouse survived was that Gilbert ran out of D_2O because the mouse wouldn't have lived if he had been able to increase the dosage over a longer period of time.

About twenty years after Lewis' experiment, we repeated basically the same thing in our laboratory af er D_2O had become readily available. I can tell you that the mouse cannot survive when the higher concentrations of D_2O are administered. When the concentration of D_2O reaches over 21% in the body, the mouse is sick, by 28% in the body, the mouse expires.

Another effect of the heavy water which Lewis didn't know (and we found out about much later) is that when the mouse has about 21% D_2O in his body there is temporary sterility. Just as the "seed" will not sprout with 21% D_2O, so the mouse will not "sprout". What I mean is that a male mouse becomes sterile if the water level in the body reaches more than 21% heavy water; if you remove the heavy water, the sterility is reversed. We actually performed that experiment for another reason. We had expected that virus replication involved hydrogen bonding and separation of the double helix, and thought that it (replication) should be effected by replacing the hydrogen in the medium with deuterium.

At that time, I was trying to cure the mouse of a virus disease when I found out as well that the mouse became sterile; the deuterium affected not only the virus replication but, additionally, the mouse "replication".

Lewis worked very vigorously in the deuterium business for a couple of years, and published several important papers. He presented the most important results of his work at the meeting of the International Union of Pure and Applied Chemistry in Madrid, Spain in 1934. He never touched the deuterium problem again after that.

Color: The next phase of Lewis' work is the one that I know most about. As I said earlier, most of the germs of what he did in his later life were contained in the book on valence published in 1923 and the earlier papers such as the definition of an acid and base.

When I came to Berkeley in 1937, Glenn Seaborg was Lewis' personal research assistant and they were working on acids and bases. I can remember that they used trinitrotiphenylmethane as an indicator in the laboratory on the first floor of Gilman Hall.

I wish to digress, here, into some personal reminiscences because you might be interested to know how I got into this business with Lewis at all. When Lewis came to Berkeley in 1912, he brought with him William Bray (from MIT), Joel Hildebrand (University of Pennsylvania), Wendell Latimer (University of Kansas), but the rest of the staff of the Department of Chemistry were "home grown" Berkeley graduates, until I came to Berkeley in 1937. This event was such a shock to him -- to bring in someone from the outside -- and this comment is being made in retrospect because at the time I did not realize what he was doing, that Lewis was a little bit nervous about my presence, because he did not know me personally. When Lewis and I began to discuss the color of the trinitrotriphenylmethane, which was being used in the acid-base experiments, Lewis realized that I had an interest in the color problem. So, Lewis said "let's do a paper on the theory of color". He didn't quite say it that way, but that's how it came out.

Within a few months I was involved with Gilbert Lewis, reading all that was available on the color of organic molecules and dyestuffs. This work began in 1937 and the review paper, Color of Organic Substances, was published in 1939 in Chemical Reviews. The literature searches went on for some time, to bring Lewis up to date from the period of 1923 to 1937 (1939), and we collected all kinds of literature, color indices, etc. We scattered this material all over the seminar room in Gilman Hall. We would discuss this literature for hours at a time. Lewis already had (and he transmitted this to me) an idea of what the paper would contain: He was going to try to introduce an idea of elementary linear harmonic quantum mechanics into the theory of color of organic substances.

After many months of discussion, Lewis decided it was time to write, and he started to dictate. The seminar room was in one end of Gilman Hall, and it was longer than it was wide, with the door at one end and the blackboard next to it. The room contained a long table, and about two dozen chairs around the table. The literature and books were on top of the table. Lewis would walk round and round the table, dictating to me, while I wrote, and I sat in a special place at the table. (I never sat at the forward right-hand corner; that was Lewis' place.) He would start to dictate, I would write and he would walk. He dictated sentence by sentence. One time I did not write the sentence when he dictated it. He inquired why I did not write it down, and I said it was already written down. He looked, and sure enough, the exact words he had just spoken to me were already on the paper. That wasn't very hard to do, because we had been talking for the past three months about these very things, and the same phrases were coming out all the time. By the time Lewis decided to dictate, it was all constructed in his head and I had heard most of it many times. So, it wasn't any great trick to do what I had just done. But after that, the situation changed a little bit, and Lewis would dictate more slowly, and his phrases weren't so coherent, so I couldn't "anticipate" him.

This bit of work gave rise to a whole sequence of scientific thinking which took place in Gilbert's mind. The basic notions of what was to come out in the color paper had already evolved in Lewis' thinking -- electron oscillation, nature of the excited state, simple linear oscillator quantum theory to understand these phenomena. In the course of our searches in this area, we learned that many dyestuffs were reported with broad absorption bands, whose color was not reported in the color index in terms of wavelength (only as blue, green, yellow or some other color), and so it was necessary to do some spectroscopic experiments. To do spectroscopy on these organic dyes, Lewis conceived the notion that he might put them in a glass, cool them down, and get low temperature spectra; in other words, to sharpen up the peaks. How do you put an organic dyestuff in a glass? Lewis invented and created the particular type of glass for these experiments, by taking boric acid in a large beaker with a Bunsen burner underneath, and using a thermometer to stir the molten boric acid. When the liquid came to the right temperature, he would remove the Bunsen burner, put the dyestuff in, and let th molten boric acid (containing the dyestuff) cool. It cooled to a glass. What Lewis had done was to dehydrate the boric acid just enough so that when it cooled it did not crystallize but solidifed to a glass -- in other words, it was a rigid medium, contaning a dye. Lewis always did these experiments, with the boric acid, himself, because of the critical temperature, which he never told me about! I'm not joking -- that's exactly what happened.

As we went through these experiments, he noticed that most of the dyes in the glass would fluoresce. He then asked himself why doesn't everything fluoresce? Whey is there thermal degradation? Why doesn't everything drop back to the ground state? He conceived the important idea that, in general, the molecules which failed to fluoresce had in them structures in which the vibrational mode interacted closely with the electronic excitation and the electronic excitation would then go over to vibrational excitation. To produce fluorescence in a rigid medium this process was inhibited. In a rigid medium, therefore, it is possible to increase the degree of fluorescence, which Lewis proved by his experiments.

In the course of this work, another idea evolved which came to be even more important. On some occasions, when Lewis turned off the light, the boric acid glass continued to glow (phosphoresce) for as much as 10 to 20 seconds. Why did some samples have phosphorescence and others not? The phosphorescence was not the same color as the fluorescence -- it was usually redder in color. Lewis then had another question to ask: Why do some of these materials phosphoresce instead of fluoresce, and why was the color different? As you undoubtedly know, Lewis was aware of the basic concepts of quantum mechanics and some selection rules for spectroscopic transition. He understood quite well angular momentum restraints, so he theorized that the dye molecule in its upper state must go over to some state by thermal motion which is prohibited from falling to the ground state with light emission. What would that prohibition turn out to be? What was thy condition of the state that prevented it from falling down (to the ground state) fast? In other words, this was the evolution of the theory of the triplet state. There should be a non-anti-parallel electron pair present which could be found as paramagnetic material.

To prove this theory, it was necessary to measure the displacement of a magnetic material in a magnetic field. Lewis therefore decided to try this type of an experiment, and he thought about it for a week or two. He went down into Gilman Hall and looked at Giauque's magnet, which was relatively small (10,000 to 12,000 gauss) and the pole faces were about 6 to 8 inches in diameter. There was a balance on the top of the magnet, and the first experiments were done with the samples hanging from the balance, with the light and magnet turned on; it was not possible to measure the magnetism. This turned out to be not sensitive enough for what Lewis wanted to know. He then constructed a balance in which the sensitivity was greater by another factor of 100 (or perhaps 1000). The construction of that balance turned out to be a most interesting thing. It was dependent upon horizontal displacement of the sample in the magnetic field, with light striking half the sample in the magnetic field. A given horizontal displacement would correspond to a given number of micrograms of force (by calculation). It was necessary to construct the sensitive balance in Gilman Hall since it wasn't possible to purchase equipment which was sensitive enough, and, anyway, we didn't have the money!

For the balance, we needed two quartz fibers, about 2 meters long from which to suspend the sample. Lewis wouldn't let me turn the quartz rods in the glassblowing torch. This is similar to the experience when he wouldn't let me help with the liquid boric acid experiments. Lewis got the glassblowing torch at one end of the 50-yard long hall on the top floor of Gilman Hall. He took the quartz rod about 4 mm in diameter, and stood over the torch, turning the rod. When the rod was just the proper temperature, he handed me one end of the fiber, and I had to run down the hall with it, while he held the other end. We kept repeating this performance until we got a couple of good fibers, which were then hung up inside glass tubes about 1 inch in diameter, with ground joints at either end. The sample was suspended from these fibers and the tip of the sample carrying rod was watched through a microscope.

The next step in the experiment was to turn on the magnetic field and then turn on the light. My job was to tell him which way and how much the sample moved. I said that it "jerked" the right way, but it came to rest going the wrong way. This went on for several days. We were illuminating the sample at one end of thy magnetic field, and the illuminated part of the sample should have come into the field, but instead it jerked and then went back the other way. Lewis didn't believe these observations. He then let me turn on the light and watched himself; the results were the same -- it went the wrong way.

As you can imagine, this was a rather nervous time for me, as the results were going the wrong way, and Lewis knew that they shouldn't behave in that fashion. He could not understand the results. Late one afternoon we were down on the lower floor of Gilman Hall in the magnet room and the door was open. You should know that the basement of Gilman Hall was Giauque's, including the magnet that Lewis was using. Giauque came by and asked what the matter was (he could feel the tension), and Lewis explained what was happening. Giauque asked what gas we had in the glass tubes, and Lewis replied that there was air in the tubes. Giauque said that you shouldn't have air in the tubes as oxygen is paramagnetic. When we changed the air in the tubes to nitrogen, all the experimental results were proper, and as expected. The sample became heated by the light, which warmed the oxygen, and the volume magnetic susceptibility becomes less, so that the oxygen moves out of the field. The result was acutally an oxygen wind blowing the sample in the wrong direction.

So, Lewis said if that's the case, the same result should ccur if I put a microscope slide with soot on it in the place of the sample. Everything did go properly. The next question was: How much does it move in the right direction (how many millimeters). We could calculate the cosine of the suspension angle and determine the answer. It turned out that the triplet state was indeed magnetic, and we closed up shop on that particular experiment in 1944.

Lewis had predicted that if the excited state of a long (extended) molecule was dipolar, then the energy level of the excited state should fall in a medium of higher dielectric constant. He should then be able to plot (if the ground state was not polar and the excited state was polar) the difference which would be due to a shift in the energy of the excited state. Therefore, the color of the molecule (the wavelength of the absorption) should have some relation to the dielectric constant of the solvent in which the measurement is made. In those cases in which the data existed, it was that way. However, Lewis wasn't satisfied with that and he wanted to check it out with a wider range of dielectric constant materials, which led to the next step in these experiments. Lewis then selected a group of molecules (I think they were carotenoids and diphenylpolyenes), simple, relatively long molecules, in which the dipole of the excited state should be noticeably larger than the ground state. He then sought to find a series of solvents which would dissolve these molecules (low dielectrics like hydrocarbon solvents and high dielectrics beyond water). In characteristic fashion, nothing stopped him, and he did what he had to do. He would acquire the necessary data, or necessary technology, even in areas with which he was not fully familiar. He would do what had to be done to test his scientific thoughts. The highest dielectric medium he could lay his hands on was liquid HCN, and so he went to work to measure the absorption spectra of various carotenoids, etc. in a variety of solvents, including liquid HCN which has a very high dielectric constant.

CONCLUDING PERSONAL REMARKS

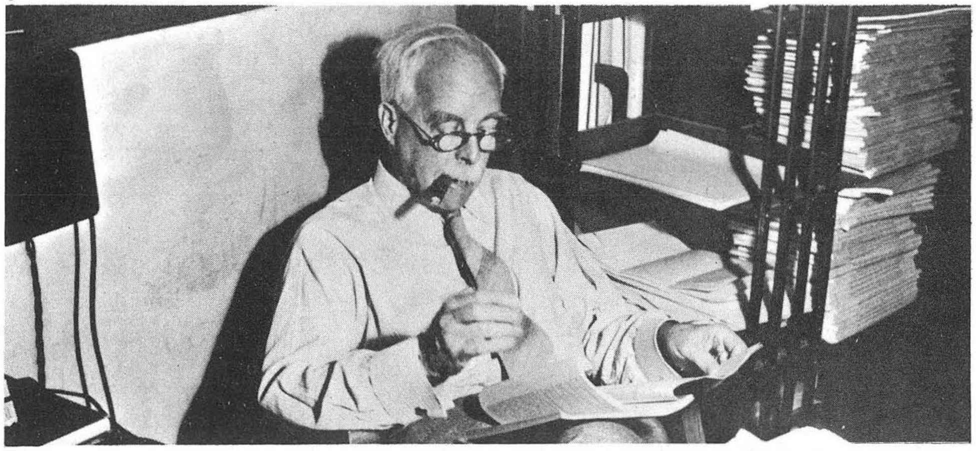
It was during the course of the work described immediately above that Gilbert N. Lewis died in March, 1946. He died on the third floor of Gilman Hall, in a laboratory across the hall from my laboratory (310 Gilman Hall) on a Saturday morning, with no one around. There has been some conjecture about the sequence of events, and it is, of course, a conjecture. But I would be very surprised if he dropped the test tube of HCN and didn't leave the laboratory. He wouldn't have done it any other way. That was the way he came to his end, doing an experiment, on a theory of his own, by himself, in the laboratory.

Not many of us will have the good fortune to live and to die as he did, to be as creative as he was. I have mentioned in this discussion only Lewis' scientific contributions, but that isn't all there was to Gilbert Lewis, by a long way.

Earlier in this discussion there was a photograph of themembers of the department of Chemistry at Berkeley in 1916. Gilbert Lewis in his "deaning" office is shown in Figure 2. The square bookcase, full of books, behind him was very characteristic, and his desk wasn't visible usually, nor were the guest chairs. He had two wicker guest chairs and they were always covered up with papers! (Incidentally, those wicker guest chairs are now part of the furniture of my own office, and area treated with great respect by all who use them.) Lewis is shown in this photograph in typical surroundings and in his typical appearance. The cigar was in his mouth at the time the photo was taken, but most of the time the cigar was in his hand; that's why he had to have a personal assistant, like Seaborg or myself!

Mary Lewis and Gilbert's Family Life: It was during this early period that there took place at the Lewis home on Piedmont Avenue near the campus a number of gatherings arranged by Gilbert' wife, Mary, for the integration of the younger members of the staff into the department. These affairs were attended only by the younger members and their wives, or girl friends, and constituted a genuine opportunity for these younger people not only to meet each other but to see Gilbert and his family in quite a different environment than we

FIGURE 2. GILBERT NEWTON LEWIS IN HIS GILMAN HALL OFFICE



GILBERT NEWTON LEWIS (1875-1946)
Chemistry in California's History: An Exhibit
Lawrence Hall of Science, August 31 to December 31, 1976

accustomed to seeing him in the laboratory. It was at such an event as this that I met Gilbert's daughter, Margery, and the younger son, Ted, and over the years came to know Mary Lewis herself and to understand a little better the role she played in making possible the dedication that Gilbert had to science.

The other opportunity to see both Gilbert and Mary Lewis in quite a different environment occurred somewhat later when my wife and I discovered that the Lewises were also very active and popular members of the Drama Section of the U.S. Faculty (as we ourselves became). They both participated in the production of plays once a month over a period of many years, some times as actors and some times as producers or directors. Their zest in this activity was an inspiration to all of the younger faculty in broadening the base of their lives. (Incidentally, Mary Lewis still participates in these gatherings.)

Gilbert's Cigars: I mentioned earlier the importance of the year he spent in Manila as Superintendent of Weights and Measures. He acquired a taste for Philippine cigars there, very early in 1904-1905, and there was a great collection of cigar boxes around the department. Glenn used some, I had one or two, for all sorts of things. We had learned that after Lewis returned to the United States and came to Berkeley he would buy his cigars from the Philippines. He continued to smoke those particular cigars. Eventually, the manufacturer of these particular cigars wrote to him and asked him how many he was going to use. It turned out that the company exported cigars to only one place in the United States, namely, Berkeley, and one shop in Berkeley, and that shop had one customer for these cigars, Gilbert Lewis. When he found that out, he arranged to get his cigars directly. I don't know how many cigars Lewis smoked each day, but it must have been ten to fifteen. He always had one in his hand, as shown in Figure 3.

Another characteristic activity which was associated with the cigar smoking was Gilbert's bridge playing at the Faculty Club. Just as he always had a cigar in his hand throughout the day, his lunch at the Faculty Club consisted of a two-hour bridge game. Fortunately, I don't think I ever played in one of those games, but I did watch them frequently. They were real "spectator" sports as well as a participatory sport. They were spectator sports not only for the technical aspects of bridge playing (at which Gilbert was superb), but also for the comments and asides of the players themselves.

The Seminar: I would now like to describe and discuss the departmental seminar, which was part of Gilbert's way of conducting research in Berkeley. The most important thing, as far as the department was concerned, was to talk about what you were doing, somewhere, sometime. The seminar room in Gilman Hall was the place in which most of this "talking" was done, usually every Thursday. Lewis always sat in the same chair at the table, the first chair on the right hand side facing the speaker and blackboard. Everybody else would sit in the ampitheater of two rises and the faculty sat around the table.

In the early days, I am told, the faculty gathered in the seminar room and there was no formal program. Lewis would look around and see if he could find someone whom he could "put the finger on", which he did. He would ask "What are you doing?", and the person would have to get up and tell Lewis what he was doing, just like that! It was that way everywhere in the department. The door to Lewis' office on the first floor of Gilman Hall was always open, and there was a blackboard on the

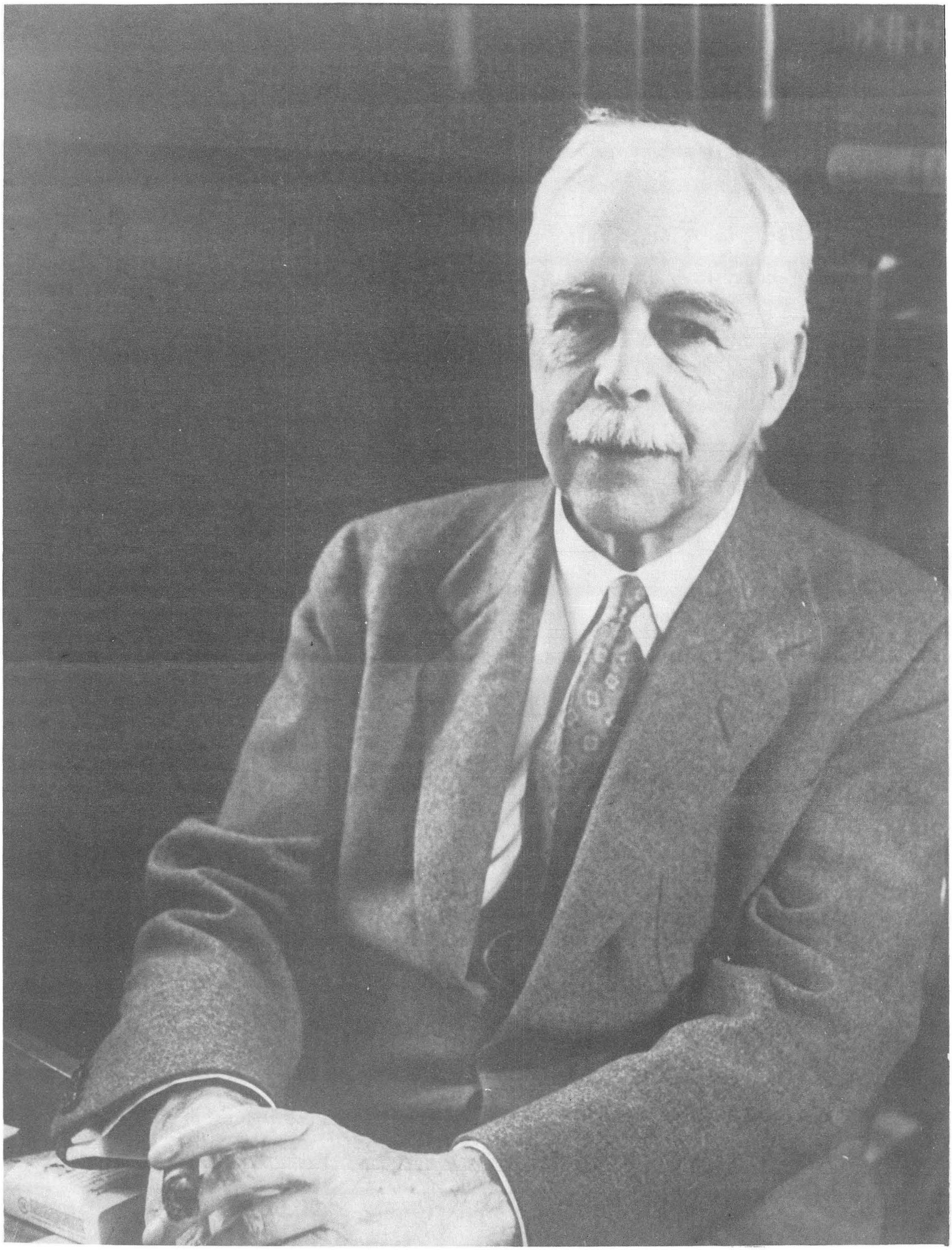


Fig. 3 Gilbert Newton Lewis

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door. You couln't go by that office without him seeing you. He would then catch you right there and ask you "What are you doing?" That went for everyone, not just the newer faculty and students but everybody -- Gibson, Branch, Giauque, Eastman, and the other senior faculty members. There was always something going on in the hallways of Gilman Hall immediately nearGilbert's office.

Incidentally, the first time I went to the seminar, I sat in the rear (in the chairs on the risers), and after attending this meeting a couple of times, I made a comment. Lewis, sitting at the table, looked around to see if he could see me in the back, because it wasn't a common occurrence for a faculty member to sit in the back. It was that comment, partly, which led him to try and discover what he had in the back there, to be sure that there wasn't something going wrong! That's how I got into this business with Lewis -- into the theory of color, the photochemistry of rigid medium, fluorescence and phosphorescence of the triplet state.

Lewis' Influence on Physical Chemistry. You have heard many others say that through Lewis they had their influence on American physical chemistry. I had the impression in the thirties, and perhaps in the forties as well, that American physical chemistry was in the hands of Lewis' students or grandstudents in one way or another. A list of those students who took their degree in Berkeley from the time Lewis arrived in 1912 until he retired in 1941 is given Appendix III.

The most important tribute that I can actually give to Gilbert Newton Lewis which is not in the formal record is found in that list of the students who received their degrees at Berkeley while he was there, thus providing thy continuity in American physical chemistry which still exists today.

Glenn Seaborg's View of G. N. Lewis: I can vividly recall Melvin (Calvin) taking G. N. Lewis' dictation down in the seminar room in Gilman Hall, and I myself had a similar experience. Actually, I was Lewis' personal research assistant nominally, but Melvin was serving as research assistant as well, as he has clearly indicated. I remember how I became Lewis' research assistant: The time for my Ph.D. research had come to an end in 1937 and it was time for me to go out and get a job someplace. Lewis didn't recommend me for a position anywhere, and you would think that was a bad sign. Actually, in this case, it was a good sign. That meant that I still had a chance to stay on at Berkeley which, of course, was my ambition. One day Lewis called me (Seaborg) into his office. Actually, the academic year had already started (it was past July 1, 1937), and he asked me if I would like to be his research assistant. I said, "Who, me?". I was completely flabbergasted and asked if he was sure I could actually fill this role. He indicated that if didn't think so, he would not have given me the opportunity. So, we did our work at the other end of Gilman Hall, Room 119 to be exact, the same room where the deuterium was isolated.

My first project with Gilbert Lewis was on acids and bases, and the first paper was on the generalized theory of acids and bases. When we finished that paper -- I was there on a Sunday afternoon taking his dictation as he walked back and forth along the laboratory bench -- I remember very well his attitude toward the Journal of the American Chemical Society. He would stop every now and then and say, "indicate that that goes into small print". I said, "Why?", and he would say, "That's one of our most important thoughts and they will put that in small print". He also had a disdain for the rules of the JACS because we didn't do any of our work in a thermostatically-controlled bath, where we knew the exact temperatures. Lewis was always investigating new phenomena (did they go or didn't they go) and he didn't care about the exact temperatures. He would say that "we'll probably have a great deal of trouble getting this paper published" because JACS only want things done in a carefully controlled environment, i.e., 20.00 $\pm$ 0.01°, or something like that!

I also remember when I got Lewis ready for this trip to Philadelphia to receive the Franklin Medal. We had worked out all of the demonstrations and experiments on generalized acids and bases, and he was going to bring the equipment, chemicals, dyes, etc. for his demonstration lecture in connection with his acceptance of the medal. He came in one day and put two moderate size suitcases on the lab bench and opened them up. I said, "this is great, it will give me plenty of room" for all the equipment and chemicals. Lewis said, "No, wait, I have to put my cigar boxes in there first". He filled one entire suitcase with cigars, and part of the other one.

I don't think Melvin remembered to tell the story that at a certain stage the Philippine cigar company had decided to go out of business, and the word came back that they would stay in business if Lewis would double his consumption!

The seminar that Lewis conducted was a fantastic operation. The graduate student would give his seminar on some paper from the literature for the first half of the period, then some faculty member, postdoctoral fellow or graduate student who had finished his research would talk on his research for the second half of the seminar. Everyone was waiting for Lewis' comments. In my case, when I finished my seminar as a graduate student, I had worked for weeks trying to think of every possible question that he could ask. When I finished my presentation, I turned to Lewis and he asked the question I finished my presentation, I turned to Lewis and he asked the question that I couldn't answer.

One of the famous incidents in the seminar, which perhaps many of you have heard, is that a graduate student made a comment (graduate students were almost on an equal footing with the faculty), and Lewis made the retort to the graduate student saying 'that was a very impertinent remark, but it was also very pertinent."

Also, Melvin reminded me of another story. Lewis used to spend a good deal of time at the Faculty Club conversing with members of other departments in the University. On one occasion, he was talking with faculty members from the department of education. An argument was raging as to what was the best way to teach the arithmetic operation of adding figures. Do you add the column from top to bottom, or do you add the column from bottom to top? Lewis said, "that's easy. I add the column from top to bottom and then I add it from bottom to top and I take the average."

When I finally completed my two-year term as Lewis' personal research assistant, I remember that he called me into his office. I was also doing a great deal of other work at the same time in the Radiation Laboratory. Lewis informed me that he was terminating my appointment as his research assistant and putting me on the faculty as an instructor, because he was taking "too much of my time". I was supposed to be working fulltime for Lewis!

CONCLUSION

Perhaps the best way to end the discussion on Gilbert Newton Lewis will be to quote from the last chapter of Valence and the Structure of Atoms and Molecules where he wrote, on the future of the quantum theory:

> Our show is now over. I trust that the majority of readers who had the patience to reach this point will now leave the tent. For what I am about to say is no longer chemistry, nor is it physics, nor perhaps is it sense. But, since we have been obliged here and there to take cognizance of the entering wedge of scientific Bolshevism, which we call quantum theory, or the theory of discontinuity in nature, I cannot refrain from attempting to forecast some of the logical consequences which must follow from the new facts which have been discovered and the interpretations they have been given.
>
> Later: Some of these abstractions may have to be abandoned as the conventional ether was abandoned after the acceptance of relativity. Others may have to be modified, and my chief purpose in writing the present section is not so much to predict just how these modifications are to occur, as it is to emphasize the necessity of maintaining an openness of mind so that when the solution of these problems, which now seem so baffling, is ultimately offered, its acceptance will not be retarded by the conventions and the inadequate mental abstractions of the past.

Personal Postscript: Gilbert Lewis and his methods of research, his methods of questioning the scientific facts his methods of intuitive approach coupled to experimental proof, still lives in many laboratories throughout the world. On the Berkeley Campus, my own Laboratory of Chemical Biodynamics is set up on these same principles, including, even, the seminar. The shape of the building (round) allows no long hallways with many doors to isolate people of various disciplines from each other. People can intermingle with ease, and there are blackboards everywhere where spontaneous discussions can and do occur. The round coffee table where the members of the lab gather each morning is another focus of free discussion which knows no boundaries, either in science or in nonscientific subjects. Even the seminar remains the same, to the point where there is one place (that of the director, myself) where no one else sits. Initially, when the group was small and just beginning, we used the same approach as Lewis' did -- a glance around the room, and a "finger" pointed to someone to stand up and talk about his work. Now that the group has grown to over 100 people, it is not so easy to do this, and the senior staff of the laboratory makes up a schedule for the weekly seminar speaker, allowing him (or her) some time for more formal preparation. The question and answer session, however, remains the same, in the tradition of Gilbert Newton Lewis.

APPENDIX I

BIOGRAPHICAL DATA -- GILBERT NEWTON LEWIS

1. In Honor of Gilbert Newton Lewis on His Seventieth Birthday. University of California Press, Berkeley (1945).

2. Gilbert Newton Lewis. By Joel H. Hildebrand. Obituary Notices of Fellows of the Royal Society, 5, 491 (1947).

3. Gilbert Newton Lewis. By Joel H. Hildebrand. Memoirs of the National Academy of Sciences, 31, 210 (1958).

4. The Origin of G. N. Lewis' Theory of the Shared Pair Electron Bond. By Robert E Kohler, Jr. Historical Studies in the Physical Sciences, 3, 343 (1971). University of Pennsylvania Press.

5. The Lewis-Langmuir Theory of Valence and the Chemical Community, 1920-1928. By Robert E. Kohler, Jr. Historical Studies in the Physical Sciences, 6, 431 (1975). University of Pennsylvania Press.

6. Borderland of the Unknown: The Life Story of Gilbert Newton Lewis (One of the World's Great Scientists). Arthur Lachman. Pageant Press, New York (1955).

BIBLIOGRAPHY

1898–1899. (With T. W. Richards.) Some electrochemical and thermochemical relations of zinc and cadmium amalgams. *Proc. Amer. Acad.* **34**, 87; *Z. phys. Chem.* **28**, 1.
1899–1900. The development and application of a general equation for free energy and physico-chemical equilibrium. *Proc. Amer. Acad.* **35**, 3; *Z. phys. Chem.* **32**, 364.
1900. A new conception of thermal pressure and a theory of solutions. *Proc. Amer. Acad.* **36**, 145; *Z. phys. Chem.* **35**, 343.
1901. The law of physico-chemical change. *Proc. Amer. Acad.* **37**, 49; *Z. phys. Chem.* **38**, 205.
1905. The autocatalytic decomposition of silver oxide. *Bull. Gov. Lab. Manila, P.I.* **30**; *Proc. Amer. Acad.* **40**, 719; *Z. phys. Chem.* **52**, 310.

1905. Hydration in solution. *Bull. Gov. Lab. Manila, P.I.* **30**; *Z. phys. Chem.* **52**, 224.
1906. Concerning silver oxide and silver suboxide. *J. Amer. Chem. Soc.* **28**, 139; *Philippine Jour. Sci.* **1**, 439; *Z. phys. Chem.* **55**, 449.
1906. The potential of the oxygen electrode. *J. Amer. Chem. Soc.* **28**, 158; *Z. phys. Chem.* **55**, 465.
1906. (With R. F. Jackson.) Galvanic polarization on a mercury cathode. *Proc. Amer. Acad.* **41**, 399; *Z. phys. Chem.* **56**, 193.
1906. (With P. Wheeler.) The electrical conductivity of solutions in liquid iodine. *Proc. Amer. Acad.* **41**, 419; *Z. phys. Chem.* **56**, 179.
1906. Über Komplexbildung. Hydratation und Farbe. *Z. phys. Chem.* **56**, 223.
1906. An elementary proof of the relation between the vapour pressures and the composition of a binary mixture. *J. Amer. Chem. Soc.* **28**, 569.
1906. On the applicability of Raoult's laws to molecular weight determinations in mixed solvents and in simple solvents whose vapour dissociates. *J. Amer. Chem. Soc.* **28**, 766.
1906. A review of recent progress in physical chemistry. *J. Amer. Chem. Soc.* **28**, 893.
1906. Equilibrium in the Deacon process. *J. Amer. Chem. Soc.* **28**, 1380.
1907. The specific heat of solids at constant volume and the law of Dulong and Petit. *J. Amer. Chem. Soc.* **29**, 1165; *Z. anorg. Chem.* **55**, 200.
1907. Outlines of a new system of thermodynamic chemistry. *Proc. Amer. Acad.* **43**, 259; *Z. phys. Chem.* **61**, 129.
1908. The osmotic pressure of concentrated solutions and the laws of the perfect solution. *J. Amer. Chem. Soc.* **30**, 668; *Chem. News*, **99**, 40.
1908. The determination of ionic hydration from electromotive force. *J. Amer. Chem. Soc.* **30**, 1355; *Z. Electrochem.* **14**, 509.
1908. A revision of the fundamental laws of matter and energy. *Technol. Quart.* **21**, 212; *Phil. Mag.* **16**, 705; *Ann. Naturphil.* **7**, 429.
1908. The ionic theory. *Sch. Sci. Math.* **8**, no. 6, 484.
1909. (With L. W. Sargent.) The potential of the ferro-ferricyanide electrode. *J. Amer. Chem. Soc.* **31**, 355.
1909. (With L. W. Sargent.) Potentials between liquids. *J. Amer. Chem. Soc.* **31**, 363.
1909. (With R. C. Tolman.) The principle of relativity and non-Newtonian mechanics. *Proc. Amer. Acad.* **44**, 711; *Phil. Mag.* **18**, 510.
1909. The fundamental laws of matter and energy. *Science*, **30**, 84.
1909. The use and abuse of the ionic theory. *Z. phys. Chem.* **70**, 212; *Science*, **30**, 1.
1910. (With C. L. von Ende.) The potential of the thallium electrode. *J. Amer. Chem. Soc.* **32**, 732.
1910. The theory of the determination of transference numbers by the method of moving boundaries. *J. Amer. Chem. Soc.* **32**, 862.
1910. On four dimensional-vector analysis and its application in electrical theory. *Proc. Amer. Acad.* **46**, 165; *Jb. Radioakt.* **7**, 329.
1910. (With C. A. Kraus.) The potential of the sodium electrode. *J. Amer. Chem. Soc.* **32**, 1459
1911. (With A. Edgar.) The equilibrium between nitric acid, nitrous acid and nitric oxide. *J. Amer. Chem. Soc.* **33**, 292.
1911. (With F. R. Rupert.) The potential of the chlorine electrode. *J. Amer. Chem. Soc.* **33**, 299.
1911. (With M. Randall.) The heat content of the various forms of sulphur. *J. Amer. Chem. Soc.* **33**, 476.
1912. (With F. G. Keyes.) The potential of the potassium electrode. *J. Amer. Chem. Soc.* **34**, 119.
1912. (With G. H. Burrows.) The equilibrium between ammonium carbonate and ammonium carbamate in aqueous solution at 25°. *J. Amer. Chem. Soc.* **34**, 993.
1912. (With M. Randall.) A summary of the specific heats of gases. *J. Amer. Chem. Soc.* **34**, 1128.

1912. (With G. H. BURROWS.) The free energy of organic compounds. I. The reversible synthesis of urea and of ammonium cyanate. *J. Amer. Chem. Soc.* **34,** 1515.

1912. (With E. B. WILSON.) The space-time manifold of relativity; The non-euclidean geometry of mechanics and electromagnetics. *Proc. Amer. Acad.* **48,** 389.

1912. The activity of the ions and the degree of dissociation of strong electrolytes. *J. Amer. Chem. Soc.* 34, 1631.

1913. The free energy of chemical substances. *J. Amer. Chem. Soc.* **35, 1.**

1913. (With F. G. KEYES.) The potential of the lithium electrode. *J. Amer. Chem. Soc.* **35,** 340.

1913. Valence and tautomerism. *J. Amer. Chem. Soc.* **35,** 1448.

1914. (With E. Q. ADAMS.) Notes on quantum theory: a theory of ultimate rational units; numerical relations between elementary charge, wirkungsquantum, constant of Stefan's law. *Phys. Rev.* 3, 92.

1914. (With W. N. LACEY.) The potential of the copper electrode and the activity of bi-valent ions. *J. Amer. Chem. Soc.* 36, 804.

1914. (With E. Q. ADAMS.) Notes on quantum theory. The distribution of thermal energy. *Phys. Rev.* **4,** 331.

1914. (With M. RANDALL.) The free energy of oxygen, hydrogen and the oxides of hydrogen. *J. Amer. Chem. Soc.* **36,** 1969.

1914. (With M. RANDALL.) The free energy of iodine compounds. *J. Amer. Chem. Soc.* **36,** 2259.

1914. (With M. RANDALL.) The free energy of the various forms of elementary sulphur. *J. Amer. Chem. Soc.* 36, 2468.

1915. (With M. RANDALL.) The free energy of some carbon compounds. *J. Amer. Chem. Soc.* **37,** 458.

1915. (With E. Q. ADAMS.) The Maxwell distribution law in Newtonian and non-Newtonian mechanics. *Phys. Rev.* **5,** 510.

1915. (With W. N. LACEY.) The equilibrium between carbon oxysulphide, carbon monoxide and sulphur. *J. Amer. Chem. Soc.* **37,** 1976.

1915. (With W. L. ARGO.) The potential of the rubidium electrode. *J. Amer. Chem. Soc.* **37,** 1983.

1915. (With E. Q. ADAMS.) The free energy of nitrogen compounds. *J. Amer. Chem. Soc.* **37,** 2308.

1915. (With E. Q. ADAMS and E. H. LANMAN.) Electrical transference in amalgams. *J. Amer. Chem. Soc.* 37, 2656.

1916. The atom and the molecule. *J. Amer. Chem. Soc.* 38, 762.

1916. Steric hindrance and the existence of odd molecules (free radicals). *Proc. Nat. Acad. Sci.* **2,** 586.

1916. (With M. RANDALL.) The free energy of bromine compounds. *J. Amer. Chem. Soc.* 38, 2348.

1916. (With T. B. HINE.) Electrical conduction in dilute amalgams. *Proc. Nat. Acad. Sci.* **2,** 634.

1917. (With T. B. BRIGHTON.) The potential of the lead electrode. *J. Amer. Chem. Soc.* **39,** 1906.

1917. The static atom. *Science,* **46,** 297.

1917. (With T. B. BRIGHTON and R. L. SEBASTIAN.) A study of hydrogen and calomel electrodes. *J. Amer. Chem. Soc.* 39, 2245.

1917. (With HYMAN STORCH.) The potential of the bromine electrode; the free energy of dilution of hydrogen bromide; the distribution of bromide between several phases. *J. Amer. Chem. Soc.* 39, 2544.

1917. (With G. E. GIBSON.) The entropy of the elements and the third law of thermodynamics. *J. Amer. Chem. Soc.* 39, 2554.

1918. (With M. RANDALL and F. R. v. BICHOWSKY.) A preliminary study of reversible reactions of sulphur compounds. *J. Amer. Chem. Soc.* **40,** 356.

1918. (With M. Randall.) Equilibrium in the reaction between water and sulphur at the boiling point of sulphur. *J. Amer. Chem. Soc.* 40, 362.

1918. (With E. D. Eastman and W. H. Rodebush.) The heat capacity of electropositive metals and the thermal energy of free electrons. *Proc. Nat. Acad. Sci.* **4**, 25.

1918. (With D. B. Keyes.) Equilibria involving cyanogen iodide; the free energy of formation of cyanogen. *J. Amer. Chem. Soc.* 40, 472.

1918. (With T. B. Brighton.) The oxidizing power of cyanates and the free energy of formation of cyanides. *J. Amer. Chem. Soc.* 40, 482.

1919. (With G. A. Linhart.) The degree of ionization of very dilute electrolytes. *J. Amer. Chem. Soc.* 41, 1951.

1920. (With G. E. Gibson.) The third law of thermodynamics and the entropy of solutions and of liquids. *J. Amer. Chem. Soc.* 42, 1529.

1921. (With M. Randall.) The thermodynamic treatment of concentrated solutions and applications to thallium amalgams. *J. Amer. Chem. Soc.* 43, 233.

1921. Colour and chemical constitution. *Chem. Metall. Engrg.* 24, 871.

1921. (With M. Randall.) The activity coefficient of strong electrolytes. *J. Amer. Chem. Soc.* 43, 1112.

1922. (With G. E. Gibson and W. M. Latimer.) A revision of the entropies of the elements. *J. Amer. Chem. Soc.* 44, 1008.

1922. The chemistry of the stars and the evolution of radioactive substances. *Publ. Astron. Soc. Pacific*, 34, 309.

1922–1923. Physical constants and the ultimate rational units. *Contrib. Jefferson Phys. Lab.* 15, no. 21; *Phil. Mag.* 45, 266.

1923. (With M. Randall.) *Thermodynamics and the free energy of chemical substances.* McGraw-Hill, New York.

1923. *Valence and the structure of atoms and molecules.* The Chemical Catalogue Company, New York.

1923. Valence and the electrons. *Trans. Faraday Soc.* 19, 452.

1923. Extremely dry liquids. *J. Amer. Chem. Soc.* 45, 2836.

1924. The magnetochemical theory. *Chem. Rev.* 1, 233.

1924. The magnetism of oxygen and the molecule O_4. *J. Amer. Chem. Soc.* 46, 2027.

1924. Europas Skudder och Mynfoten. *Finsk Tidskrift*, December.

1925. A new principle of equilibrium. *Proc. Nat. Acad. Sci.* 11, 179.

1925. A plan for stabilising prices. *The Econ. J.* 35, 40.

1925. Ultimate rational units and dimensional theory. *Phil. Mag.* 49, 739.

1925. (With D. F. Smith.) The theory of reaction rate. *J. Amer. Chem. Soc.* 47, 1508.

1925. The distribution of energy in thermal radiation and the law of entire equilibrium. *Proc. Nat. Acad. Sci.* 11, 422.

1925. (With N. W. Taylor.) The paramagnetism of 'odd molecules'. *Proc. Nat. Acad. Sci.* 11, 456.

1926. The nature of light. *Proc. Nat. Acad. Sci.* 12, 22.

1926. Light waves and light corpuscles. *Nature, Lond.* 117, 236.

1926. The path of light quanta in an interference field. *Proc. Nat. Acad. Sci.* 12, 439.

1926. *The anatomy of science.* Yale University Press, New Haven.

1926. The conservation of photons. *Nature, Lond.* 118, 874.

1927. The entropy of radiation. *Proc. Nat. Acad. Sci.* 13, 307.

1927. A new equation for the distribution of radiant energy. *Proc. Nat. Acad. Sci.* 13, 471.

1927. A disproof of the radiation theory of chemical activation. *Proc. Nat. Acad. Sci.* 13, 623.

1927. Entropy at infinite pressure and the equation of state of solids. *Z. phys. Chem.* (Cohen-Festband), 130, 532.

1928. (With A. R. Olson.) Natural radioactivity and the origin of species. *Nature, Lond.* 121, 673.

1928. (With J. E. Mayer.) Thermodynamics based on statistics. I. *Proc. Nat. Acad. Sci.* **14,** 569.
1928. (With J. E. Mayer.) Thermodynamics based on statistics. II. *Proc. Nat. Acad. Sci.* **14,** 575.
1929. (With J. E. Mayer.) The quantum laws and the uncertainty principle of Heisenberg. *Proc. Nat. Acad. Sci.* 15, 2.
1929. (With J. E. Mayer.) The thermodynamics of gases which show degeneracy (Entartung). *Proc. Nat. Acad. Sci.* 15, 208.
1930. The symmetry of time in physics. *Science,* **71,** 569.
1930. Quantum kinetics and the Planck equation. *Phys. Rev.* **35,** 1533.
1930. The principle of identity and the exclusion of quantum states. *Phys. Rev.* **36,** 1144.
1931. Generalized thermodynamics including the theory of fluctuations. *J. Amer. Chem Soc.* **53,** 2578.
1931. A more fundamental thermodynamics. *Phys. Rev.* **38,** 376.
1933. The chemical bond. *J. Chem. Phys.* **1,** 17.
1933. The isotope of hydrogen. *J. Amer. Chem. Soc.* **55,** 1297.
1933. (With R. T. Macdonald.) Concentration of H^2 isotope. *J. Chem. Phys.* **1,** 341.
1933. (With R. E. Cornish.) Separation of the isotopic forms of water by fractional distillation. *J. Amer. Chem. Soc.* **55,** 2616.
1933. (With M. F. Ashley.) Spin of hydrogen isotope. *Phys. Rev.* **43,** 837.
1933. (With F. H. Spedding.) A spectroscopic search for H^3 in concentrated H^2. *Phys. Rev.* **43,** 964.
1933. (With M. S. Livingston and E. O. Lawrence.) The emission of alpha-particles from various targets bombarded by deutons of high speed. *Phys. Rev.* **44,** 55.
1933. (With E. O. Lawrence and M. S. Livingston.) The emission of protons from various targets bombarded by deutons of high speed. *Phys. Rev.* **44,** 56.
1933. (With R. T. Macdonald.) Some properties of pure H^2H^2O. *J. Amer. Chem. Soc.* **55,** 3057.
1933. A simple type of isotopic reaction. *J. Amer. Chem. Soc.* **55,** 3502.
1933. The biochemistry of water containing hydrogen isotope. *J. Amer. Chem. Soc.* **55,** 3503.
1933. (With T. C. Doody.) The mobility of ions in H^2H^2O. *J. Amer. Chem. Soc.* **55,** 3504.
1933. (With R. T. Macdonald.) The viscosity of H^2H^2O. *J. Amer. Chem. Soc.* **55,** 4730.
1933. (With A. R. Olson and W. Maroney.) The dielectric constant of H^2H^2O. *J. Amer. Chem. Soc.* 55, 4731.
1933. (With D. B. Luten.) The refractive index of H_2O^{18}, and the complete isotopic analysis of water. *J. Amer. Chem. Soc.* **55,** 5061.
1934. The biology of heavy water. *Sci.* **79,** 151.
1934. (With M. S. Livingston, M. C. Henderson and E. O. Lawrence.) The disintegration of deutons by high speed protons and the instability of the deuton. *Phys. Rev.* **45,** 242.
1934. (With P. W. Schutz.) Some properties of pure deutacetic acid. *J. Amer. Chem. Soc.* **56,** 493.
1934. (With R. T. Macdonald and P. W. Schutz.) The vapour pressure of liquid and solid deutochloric acid. *J. Amer. Chem. Soc.* **56,** 494.
1934. (With W. T. Hanson.) The vapour pressure of mixtures of light and heavy hydrogen. *J. Amer. Chem. Soc.* **56,** 1000.
1934. (With W. T. Hanson.) The vapour pressure of solid and liquid heavy hydrogen. *J. Amer. Chem. Soc.* **56,** 1001.
1934. (With P. W. Schutz.) The vapour pressure of liquid and solid deutocyanic acid. *J. Amer. Chem. Soc.* **56,** 1002.
1934. (With P. W. Schutz.) The ionization constant of deutacetic acid. *J. Amer. Chem. Soc.* **56,** 1002.

1934. (With M. S. LIVINGSTON, M. C. HENDERSON and E. O. LAWRENCE.) On the hypothesis of the instability of the deuton. *Phys. Rev.* **45**, 497.

1934. Different kinds of water. *IX. Congr. Int. Quimica*, Madrid, April.

1934. (With W. T. HANSON.) The vapour pressure of solid and liquid deuterium and the heats of sublimation of fusion and of vaporization. *J. Amer. Chem. Soc.* **56**, 1687.

1934. (With P. W. SCHUTZ.) The ionization of some weak electrolytes in heavy water. *J. Amer. Chem. Soc.* **56**, 1913.

1934. The genesis of the elements. *Phys. Rev.* **46**, 897.

1936. A theory of orbital neutrons. *Phys. Rev.* **50**, 857.

1936. (With R. T. MACDONALD.) The separation of lithium isotopes. *J. Amer. Chem. Soc.* **58**, 2519.

1937. (With P. W. SCHUTZ.) Refraction of neutrons. *Phys. Rev.* **51**, 369.

1937. Neutron optics. *Phys. Rev.* **51**, 371.

1937. (With P. W. SCHUTZ.) Neutron refraction. *Phys. Rev.* **51**, 1105.

1937. Review of 'A commentary on the scientific writings of J. Willard Gibbs' (edited by F. G. Donnan and Arthur Haas). *J. Amer. Chem. Soc.* **59**, 2749.

1938. Acids and bases. *J. Franklin Inst.* **226**, 293.

1939. (With G. T. SEABORG.) Primary and secondary acids and bases. *J. Amer. Chem. Soc.* **61**, 1886.

1939. (With G. T. SEABORG.) Trinitrotriphenylmethide ion as a secondary and primary base. *J. Amer. Chem. Soc.* **61**, 1894.

1939. (With M. CALVIN.) The colour of organic substances. *Chem. Rev.* **25**, 273.

1940. (With G. T. SEABORG.) The acidity of aromatic nitro compounds toward amines. The effect of double chelation. *J. Amer. Chem. Soc.* **62**, 2122.

1940. (With T. T. MAGEL and D. LIPKIN.) The absorption and re-emission of light by *cis*- and *trans*-stilbenes and the efficiency of their photochemical isomerization. *J. Amer. Chem. Soc.* **62**, 2973.

1941. (With D. LIPKIN.) The dissociation of tetraphenylhydrazine and its derivatives. *J. Amer. Chem. Soc.* **63**, 3232.

1941. (With D. LIPKIN and T. T. MAGEL.) Reversible photochemical processes in rigid media. A study of the phosphorescent state. *J. Amer. Chem. Soc.* **63**, 3005.

1942. (With T. T. MAGEL and D. LIPKIN.) Isomers of crystal violet ion. Their absorption and re-emission of light. *J. Amer. Chem. Soc.* **64**, 1774.

1942. (With D. LIPKIN.) Reversible photochemical processes in rigid media: the dissociation of organic molecules into radicals and ions. *J. Amer. Chem. Soc.* **64**, 2801.

1942. (With J. BIGELEISEN.) The initial step in the action of acids on tetraarylhydrazines. *J. Amer. Chem. Soc.* **64**, 2808.

1943. (With J. BIGELEISEN.) The orientation of molecules produced photochemically in rigid solvents. *J. Amer. Chem. Soc.* **65**, 520.

1943. (With J. BIGELEISEN.) Methylene blue and other indicators in general acids. The acidity function. *J. Amer. Chem. Soc.* **65**, 1144.

1943. (With O. GOLDSCHMID, T. T. MAGEL and J. BIGELEISEN.) Dimeric and other forms of methylene blue: absorption and fluorescence of the pure monomer. *J. Amer. Chem. Soc.* **65**, 1150.

1943. (With J. BIGELEISEN.) The second order x bands in absorption spectra. *J. Amer. Chem. Soc.* **65**, 2107.

1943. (With J. BIGELEISEN.) The y bands in absorption spectra. *J. Amer. Chem. Soc.* **65**, 2102.

1943. (With J. BIGELEISEN.) Photochemical reactions of leuco dyes in rigid solvents. Quantum efficiency of photo-oxidation. *J. Amer. Chem. Soc.* **65**, 2419.

1943. (With J. BIGELEISEN.) Further photo-oxidation in rigid media. *J. Amer. Chem. Soc.* **65**, 2424.

1944. (With D. Lipkin and T. T. Magel.) The light absorption and fluorescence of triarylmethyl free radicals. J. Amer. Chem. Soc. 66, 1579.

1944. (With M. Kasha.) Phosphorescence and the triplet state. J. Amer. Chem. Soc. 66, 2100.

1945. Rules for the absorption spectra of dyes. J. Amer. Chem. Soc. 67, 770.

1945. (With M. Kasha.) Phosphorescence in fluid media and the reverse process of singlet-triplet absorption. J. Amer. Chem. Soc. 67, 994.

1945. (With M. Calvin.) Paramagnetism of the phosphorescent state. J. Amer. Chem. Soc. 67, 1232.

1946. Thermodynamics of an ice age: The cause and sequence of glaciation. Science, 104, 43 (1946).

1947. The beginning of civilization in America. Amer. Anthro. 49, 1.

APPENDIX III

DOCTORATES AWARDED BY THE DEPARTMENT OF CHEMISTRY, 1912 - 1945

UNIVERSITY OF CALIFORNIA, BERKELEY

1914

Adams, Elliot Quincy
Rosenstein, Ludwig

1915

Argo, William Lind
Branch, Gerald Eyre Kirkwood
Brauer, Oscar Leo
Lacey, William Noble
Porter, Charles Walter

1916

Bichowsky, Francis Russell von
Dowell, Carr Thomas
Hine, Thomas Buck
Sebastia, Reuben Louis
Stewart, Thomas Dale

1917

Brighton, Thomas Bow
Eastman, Ermon Dwight
Keyes, Donald Babcock
Rodebush, Worth Huff

1918

Horsch, William Grenville
Olson, Axel Ragnar
Scalione, Charles Caesar

1919

Bisson, Charles Stewart
Borgstrom, Parry
Latimer, Wendell Mitchell
Loomis, Albert Geyer

1920

McGee, John Merritt
Newton, Roy Frederick
Parks, George Sutton

1921

Bardwell, Dwight Cooley
Buehrer, Theophile Frederic
Hampton, William Henry
Hogness, Thorfin Rusten
Maeser, Sherwin

1922

Almquist, John Arthur
Cuy, Eustace J.
Danner, Philip Sheridan
Edlund, Karl Richard
Gerke, Roscoe Harlan
Giauque, William Francis
Huggins, Maurice Loyal
Millar, Russell Ward
Phipps, Thomas Erwin
Williams, Albert Morris

1923

Draper, Hal Daniel
Evans, Robert Montgomery
Glockler, George
Hoskins, William Muriece
Ihrig, Harry Karl
Ramage, Wallace Douglas
Rollefson, Gerhard Krohn
Storch, Henry Herman
Taylor, Nelson Woodsworth
Urey, Harold Clayton
Young, Thomas Fraser

1924

Buffington, Ralph Mulvaney
Frost, Grenville Barker
Ramsey, James Blaine
Simons, Joseph
Vanselow, Albert Percival

1925

Breckenridge, Gerald Frederick
Hoenshel, Howard Domer
Iddings, Carl
Livingston, Robert Stanley
Ramsperger, Herman Carl
Ruhle, George Cornelius
Shaffer, Sherman Simpson
Spencer, Hugh Miller
Westwater, Waldo

1926

Cornish, Robert Edwin
Gardiner, Elmslie William
Lunn, Edward Gowan
Morgen, Ralph Alexander
Poth, Edgar Jacob
Rice, Oscar Knefler
Robinson, Preston
Vietti, William Armand Victor
Wiebe, Richard

1927

Aston, John Geldart
Bent, Henry Edward
Chipman, John, Jr.
Failey, Crawford Fairbanks
Frandsen, Mikkel
Frantz, Harper Wine
Freed, Simon
Halford, Joseph Olney
Langford, Cecil Theodore
Robson, Homer Louis
Scott, Gordon Nickelsen
Smith, Francis Edward
Toubes, Benjamin

1928

Eyring, Henry
Frank, Henry Sorg
Greensfelder, Bernard Sutro
Harkness, Roy Wendell
Martin, Jerome Leander
Mayer, Joseph Edward
Rossini, Frederick Dominic
Sosnick, Benjamin

1929

Barnes, Charles Douglas
Hirst, Lester Larsen
Johnston, Herrick Lee
Kvalnes, Hamline Monroe
Milner, Reid Thompson
Mithoff, Robert Charles
Roseveare, William Earl
Smith, Donald Milton
Spedding, Frank Harold
Young, Leona Esther

1930

Cady, George Hamilton
Christensen, Carl Joseph
Korpi, Karl John
Lawrence, Robert Wilbur
Liebhafsky, Herman Alfred
Polissar, Milton Joseph
Salstrom, Edward John
Sermatti, John Peter
Stewart, Harold Robert
Wachter, Aaron

1931

Ahlberg, Jacob Elston
Allen Clyve Charles
Bradley, William Earle
Brown, Weldon Grant
Kasper, Charles
Melvin, Eugene Harold

1932

Berry, Norton Ellsworth
Clayton, James Oliver
Cone, Richard Morton
Hudson, Frederick Lyth

1932 (continued)

Macdonald, Ronald Tom
Makower, Benjamin
Meads, Philip Francis
Taylor, Marion Downey
Yabroff, David Louis
Younger, Herbert Alexander

1933

Almquist, Herman James
Bayliss, Noel Stanley
Bear, Richard Scott
Blue, Richard Whiting
Donnally, Laurance Henderson
Hicks, John Fred Gross, Jr.
Libby, Willard Frank
MacDougall, Duncan Peck
Mohammad, Ali
Montgomery, Charles Woodward

1934

Bettman, Bernard
Booher, John Earl
Byrns, Alva Cecil
Gorin, Manuel Harold
Hanson, Wesley Turnell
Krauskopf, Konrad Bates
Kung, Hin Pao
Luten, Daniel Benjamin, Jr.
Maroney, William
Nutting, George Cook
Reiber, Harold George
Schutz, Philip William
Vester, Harold Goule
Voge, Hervey Harper

1935

Archibald, Raymond Clare
Binder, John Lloyd
Givens , John Wilkinson
Hull, Donald Emerson
Joslyn, Maynard Alexander
Kumler, Warren Donald
Long, Franklin Asbury
Macfarlane, William Angus
Nixon, Alan Charles
Weidenbaum, Bernhardt

1936

Barnett, Benjamin
Brown, Oliver Leonard Inman
Claussen, William Hammerick
Denison, George Haigh, Jr.
Egan, Clark James
Kemp, Jacob David
McGavock, William Crews, Jr.
McGinnis, Richard Adams
Meehan, Edward Joseph
Negishi, George Ryoji
Parsons, George Shelby
Potts, John Calvin
Vold, Robert Donald
Wood, Scott Emerson
Young, Marjorie Jean
Young, Maxine Bardsley

1937

Blanc, Louis Alec
Faull, Richard Francis
Fletcher, Clarence John Molyneux
Grahame, David Caldwell
Kraus, Douglas Lawrence
Manov, George Gregory
Mueller, Manfred Ernst Adolf
Overstreet, Roy
Peterson, Merlin Dewey
Pitzer, Kenneth Sanborn
Seaborg, Glenn Theodore
Smith, Wendell Vandervort
Stephenson, Clark Conklin
Stout, John Willard, Jr.
West, Helen Louise

1938

Clear, Charles Gerald
Goyan, Frank Mayer
Halford, Ralph Stanley
Jehle, Leon Paul
Lee, Donald Douglas
Li, Choh-hao
Longtin, Bruce
Ruben, Samuel
Schultz, Thomas Henry
Stross, Fred Helmut

1939

Boyd, Robert Neilson
Doody, Thomas Clare
Etzler, Dorr Homer

1939 (continued)

Frederick, Kenneth Jacob
Lipkin, David
Miller, Robert John
Morrell, William Egbert
Powell, Thomas Mabrey
Ruehrwein, Robert Arthur
Wakeham, Helmut Richard Rea
Zimmerman, Herbert Wilhelm

1940

Coulter, Lowell Vernon
DeVault, Don Charles
Fontana, Beppino John
Forsythe, William Rowe
Goldschmid, Otto
Kennedy, Joseph William
Lingafelter, Edward Clay
Morse, Erwin Emerson
Slansky, Cyril M.
Stoughton, Raymond Woodford
Taube, Henry

1941

Anderson, Henry Wayne
Atwood, Kenton
Barieau, Robert Eugene
Goodrich, James Clinton
Heckel, Harry Lightner, Jr.
Holmes, Jerome Knowlton
Hyde, James Logan
McCready, Newton Willoughby
Magel, Theodore Tellmann
Mosesman, Max Abe
Perlman, Morris Leonard
Preston, Roger Guy
Roth, Walter Lester
royal, Joseph Kenneth
Rubin, Thor Richard
Tuttle, John Ephraim

1942

Bailes, Richard Hazel
Barter, Cyril
Connick, Robert Elwell
Friedlaender, Gerhart
Gwinn, William Dulaney

1942 (continued)

Hayes, Norman Vincent
Long, Robert William
Nahinsky, Paul
Norris, Thomas Hughes
Scott, Donald William
Sherwood, Donnelle Wayne
Tong, Lee Karl Jan
Wahl, Arthur Charles
Wilmarth, Wayne Keith
Young, Frank Evans

1943

Altman, David
Bigeleisen, Jacob
Brewer, Leo
Campbell, James Arthur
Duffield, Robert Brokaw
English, Spofford Grady
Ferguson, Lloyd Noel
Gofman, John William
Gordon, Joseph
Guttman, Lester
Harman, Denham
Hill, Terrell Leslie
Kent, James Woodward
Leitz, Fred John
Ruebsamen, William Charles

1944

Barkelew, Chandler Harrison
Hamaker, John Warren
Harle, Oliver Lathom
Miller, Warren Widmer
Rice, Charles Nelson
Westrum, Edgar Francis, Jr.

1945

Beckett, Charles William
Evans, Marjorie Woodard
Kasha, Michael
Kilpatrick, John Edgar
Kind, Edward Louis
Leininger, Robert Farnes
Reid, James Cutler
Tolbert, Bert Mills
Yankwich, Peter Ewald

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