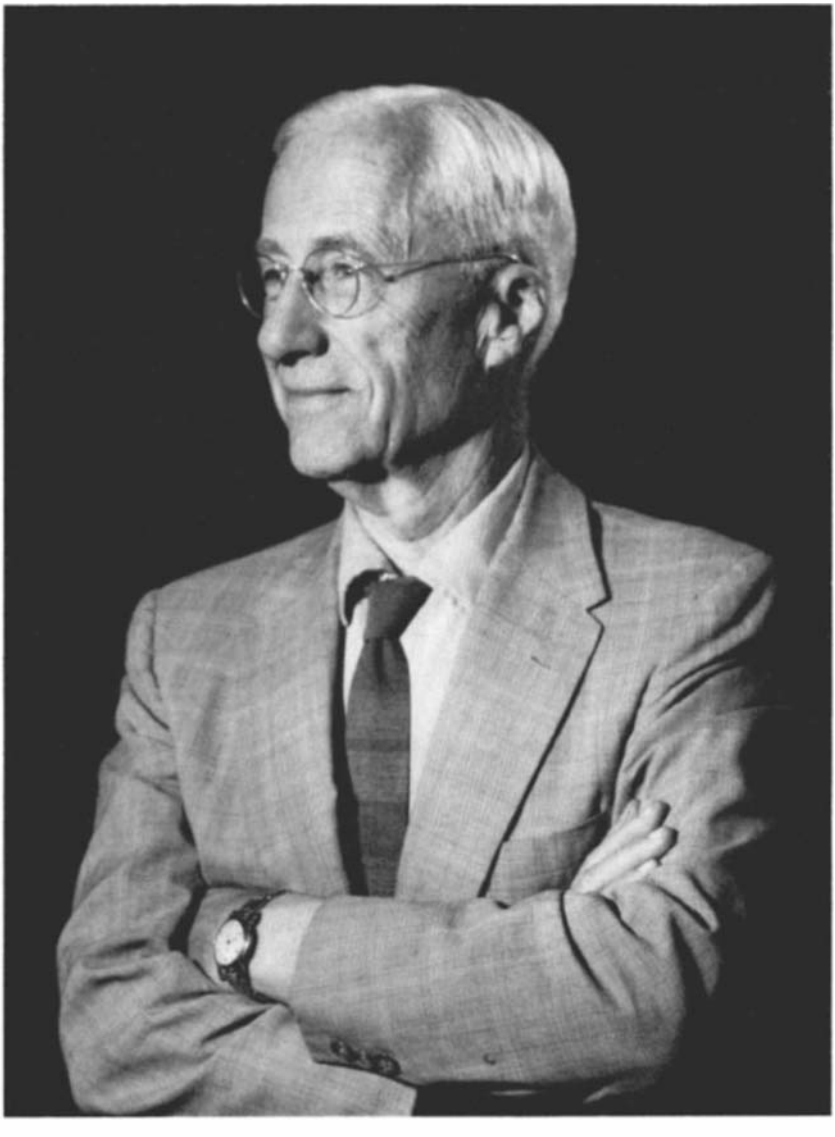

Joel H. Hildebrand

FIFTY YEARS OF PHYSICAL CHEMISTRY IN BERKELEY

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This is to be a reminiscence rather than a history. By the time it is published fifty years will have passed since I joined the little band of young iconoclasts in Berkeley that Gilbert Lewis took there with him in 1912. Much that might be said about my exciting half century has been published in the *Biographical Memoirs* that I wrote for the National Academy of Sciences on the careers of Gilbert N. Lewis, William C. Bray, and Wendell M. Latimer. A labor of love in each case. What follows will include items from these memoirs.

Lewis was uniquely qualified for the task that he undertook. He had had little schooling in his early years, enjoying an advantage which he mentioned in his later years as having occurred frequently in the careers of distinguished men, that of having "escaped some of the ordinary processes of formal education." After spending two years at the University of Nebraska, he entered Harvard College, from which he graduated in 1896, and from which in 1899 he received his Ph.D. degree. His thesis, published with Theodore William Richards as joint author, was on "Some electrochemical and thermochemical relations of zinc and cadmium amalgams."

There followed next a year at Harvard as instructor, and another at Leipzig and Göttingen. During this year he visited Albert Einstein and became deeply interested in relativity. After three more years at Harvard, he joined the notable group gathered by A. A. Noyes at the Massachusetts Institute of Technology. It included William C. Bray, Richard C. Tolman, and Edward W. Washburn.

In 1912, he was called to Berkeley as Dean of the College of Chemistry. He took with him to Berkeley Bray and Tolman and imported G. E. Gibson and G. E. K. Branch from Britain. The President, Benjamin Ide Wheeler, was laying the foundation, by a wise paternalism then appropriate to the juvenile status of the institution, of the great university it was to become. G. A. Hulett had declined the position, a fact that made easier the acceptance of the conditions that Lewis stipulated.

In March of 1913, I accepted an invitation from Lewis to deliver three lectures, in order for the new department and me to react to one another. I had at first little notion that I would care to move so far from the centers of learning—Berkeley seemed to be somewhere in the neighborhood of Tibet. Four days were required for the trip. But I could at least see California and the Grand Canyon at no expense to myself. However, there was little to hold me at the University of Pennsylvania where, although I had spent a postdoctoral year in Berlin with Nernst, I had been an instructor for six years at

a maximum salary of $1200. But I had been sawing wood, and in March was entertaining an offer from W. F. Hillebrand to join the Bureau of Standards at $3500, the equivalent of at least $14,000 today.

Lewis offered me $2000 and an assistant professorship. I sensed the intellectual stimulus and freedom among the little company of youngsters he had gathered; I wanted freedom to investigate problems of my own invention and I loved to teach. I proposed to Edgar F. Smith conditions that I knew he would not accept, especially the privilege of having graduate students for research. Herbert Harned had done a good piece with me, but all thesis work had to be carried out under the "head" of the department.

I eagerly joined the Berkeley group, where I soon came to feel as if I had escaped from a dungeon into open air. My teaching load shrank from eighteen to eight hours per week; new graduate students were sent to me, as to all staff members, to hear what ideas for research each had to offer; and I could mention an ion without encountering a cynical smile.

In one of the first "research conferences" that I attended, Lewis made one of the challenging remarks such as he was fond of, whereupon E. Q. Adams, then a graduate student, said, "No, that isn't so." I turned in some alarm. At Pennsylvania I had often had the same thought, but had not deemed it expedient to utter it. Lewis turned to him almost eagerly, asking, "No? Why not?" On another occasion, Lewis said to a graduate student who contradicted him, "That is an impertinent remark, but it is also pertinent." Latimer was especially forthright, seldom leaving any doubt about his opinions. Once, while still a graduate student, presenting something in the colloquium, Lewis constantly interrupted him, saying, "The trouble with you is that you don't take your audience into consideration." Latimer replied, "The trouble with me is that I can't keep my audience quiet long enough to say what I have to say."

The notable feature of all such interchanges was that we all loved them; no one took offense. There was a complete break with the European tradition of an institute with a single oracle at its head insisting that when its mouth opened no dog should bark.

Lewis seemed to take as much satisfaction in the productivity of his young colleagues as in his own. He protected us from excessive teaching schedules. He sent new graduate students to talk with the members of the staff, and left them free to choose that particular problem which appealed most strongly. He accepted rather less than his share of research students, in striking contrast to the practice of certain other German-trained department heads who had imported the theory that all junior members of an "institute" should work for its chief.

One of his first moves was to turn almost the entire staff loose upon the problem of starting the freshman in the way he should go by fostering a scientific habit of mind in every conceivable way. We met weekly to discuss the organization of the freshman course and the methods of presenting difficult topics. Although the lectures were given to five hundred students at a time

in the large chemistry auditorium, with great attention to lighting, projection, and realization of the full dramatic possibilities of the subject, the laboratory work and quizzing took place in sections of only twenty-five students, taught by a majority of the permanent staff with the help of numerous teaching assistants. The complaint that a freshman in a large university has no contact with professors has not applied in freshman chemistry at the University of California, for as many as eight full professors have in a single term taught freshman sections. The example thus set by senior professors has had a profound effect upon the apprentice teachers, making them take their teaching seriously and convincing them that talent for research is not demonstrated by indifferent teaching.

The laboratory manual for the freshman course, under the title, *A Course in General Chemistry*, was first published in 1915 under the authorship of W. C. Bray and L. Rosenstein. It was revised and published in 1921 and subsequently under the authorship of W. C. Bray and W. M. Latimer. The pioneering nature of the efforts of these authors can only be appreciated by comparing the experiments in this book with those found in the average laboratory manual a generation ago, which too often consisted merely in verifying descriptive statements. The very different aims of Bray and Latimer were stated, in part, in these words,

> This course in General Chemistry has been developed with the conviction that it is the duty of a university to train its students to meet new problems, and that it is more important to give the student a scientific training than it is to sort out for him those facts which may have a special bearing on the particular line of work that he is intending to follow.
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> In the Laboratory the effort is made constantly to throw the student upon his own responsibility, especially in observing accurately and in drawing conclusions from his experiments. He is often called upon to predict results of untried experiments. Numerous questions and problems are introduced to draw attention to essential points which the inexperienced or the careless student might pass over. The problem of "keeping the gifted student busy at his level of achievement" may be partly solved by allowing him to work slightly ahead of the rest of the class, for he welcomes the opportunity to overcome difficulties by his own efforts, even though this involves more work than if he had waited for the class discussion.

To state all the significant contributions to physical chemistry that came out of the department even during the first two decades of the half-century here considered would far exceed the space allotted to this review. Only a limited sample list can be given. It includes: the free energy of many chemical substances, precise electrode potentials, the octet and the electron-pair bond, the entropy of elements and the third law of thermodynamics, paramagnetism of "odd" molecules, separation of pure deuterium, catalytic burning of carbon monoxide, mechanisms of many inorganic reactions, systematic detection of the rarer elements, heat capacities of elements and compounds, entropies of aqueous ions, and the discovery of the hydrogen bond.

In the early days we were so far from the sources of supply of graduate students that we set out to produce them ourselves by making chemistry attractive to bright students in their freshman year. Soon, however, a rumor spread, even to the east, that out in Berkeley there was a department where chemistry was pretty exciting. Able students began to come from near and afar: W. F. Giauque from Niagara Falls; N. W. Taylor from Saskatchewan; J. E. Mayer from Pasadena; F. H. Spedding from Michigan; T. R. Hogness from Minnesota; H. A. Liebhafsky from Nebraska; W. M. Latimer, W. H. Rodebush and J. B. Ramsay from Kansas; H. Eyring from Arizona; H. C. Urey from Montana; and M. E. Huggins and T. F. Young from our own crop of undergraduates, and many others.

The contribution the department was soon making to the scientific strength of the nation was illustrated by a study made at another university of the chemists newly starred in the 1933 edition of *American Men of Science* (a practice since discontinued). Ten of these young men had received their Ph.D. degrees in our department; the largest number from any other department was four. The men we helped to educate are now widely spread, contributing to the productivity of their several institutions. Far from being jealous, I, as the lone survivor of the old guard, take a fatherly and now even a grandfatherly pride in their achievements. At a time when there were eight of our Ph.D.'s in the department of chemistry at the University of Chicago, Latimer remarked that it was "ingrowing with Californians."

Today, fifteen of the ninety-six members of the National Academy of Sciences won their doctoral degrees with us. They include three Nobel laureates: Giauque, Libby, and Seaborg. We lost Latimer and Rodebush by death.

One of my greatest personal satisfactions is the quality of the present department. We have seven members of the National Academy, three of whom are Nobel laureates, together with young men rising rapidly. In spite of great growth in the numbers of staff, graduate and undergraduate students, the department is as exciting as ever. My view is no long range one from a rocking chair at home; I am in the department all day, five plus days a week. I exchange questions and answers freely with my young colleagues. In 1952, I "retired"; now I am "pseudo-retired," and having the time of my life!

I trust that the main impression the reader will get from what I have written will not be one of statistics, but rather of a group of men engaged in the exciting, joint enterprise of discovering scientific truth and transmitting it in its beauty and power to the oncoming generation, an enterprise carried on in a spirit that has made the group far greater than the sum of its individual members.