NATIONAL ACADEMY OF SCIENCES

WILLIAM GOULD YOUNG

1902—1980

A Biographical Memoir by

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Biographical Memoir

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WASHINGTON D.C.

Wm G Young

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July 30, 1902—July 5, 1980

BY JOHN D. ROBERTS

WILLIAM G. YOUNG was born in Colorado Springs on July 30, 1902, and died July 5, 1980, in Laguna Hills, California. Bill Young was a physical organic chemist whose name is not now much of a household word among the current fraternity of workers in the field. There are two reasons for this. One is the fact that the major research he did was so basic to organic chemistry that it was subsumed into "what every organic chemist knows," an arena where it is not usual for individual contributors to be identified by name. In addition, much of the work that Bill Young initiated is often now associated more with his brilliant student and fellow member of the National Academy of Sciences, Saul Winstein.[1]

The second reason that Bill Young is not as well known as he might be is that he devoted enormous energy and steadfastness to making the University of California, Los Angeles, a first-rate institution in teaching and research. In this effort, another Young, Charles E. Young, UCLA's long-serving and energetic chancellor is generally given major credit, but as I shall relate, Bill Young provided critical impetus in the early days of UCLA's relatively brief history. Whether or not Bill Young would have wanted to see UCLA continue to expand to its present status as al-

most the epitome of the modern multifaceted megauniversity is of course unknowable, but it seems unlikely given his character, at least as I knew it.

Bill's father, Henry A. Young, was a self-educated man who was a jewelry salesman in Providence, Rhode Island. At age thirty he made a successful move to Colorado Springs to improve his health. There he first entered the insurance business and later became a stock and investment broker. Bill described his father as "a man of broad cultural and civic interests, who reared his children to appreciate good music, the theater and a well-chosen library. They were encouraged then to take advantage of the educational opportunities that he missed."[2] He supplied no information about his mother, Mary Ella (Salisbury), but he had brothers, Ralph, who followed his father as an investment broker, Ray, and Harry.

Bill went to grade and high school in Colorado Springs, graduating in 1920. Even at an early age, he was interested in all areas of science, but less so in other areas and had what he characterized as "a genuine dislike for foreign languages."[2] Bill enjoyed outdoor sports particularly baseball, basketball, trout fishing, and golf. The latter two sports he participated in until late in life, along with gardening and color photography. He was especially proficient at golf and was a Colorado state intercollegiate golf champion.

Bill entered Colorado College and obtained a B.A. in 1924 and an M.A. in chemistry in 1925. For reasons unclear to me, rather than going to graduate school, he then became a research assistant, working at the Carnegie Institution of Washington's plant physiology laboratory at Carmel, California, with H. A. Spoehr and James H. C. Smith. It must have been a productive period, because it resulted in five publications on four quite different subjects, probably the most significant being on the preparation and proper-

ties of carotene. This may have sparked Bill's lifelong interest in unsaturated compounds. During this period, he married Helen Graybeal, a woman of intelligence and vivacity with whom he was very compatible. They had no children.

In 1927 Bill entered the California Institute of Technology as an American Petroleum Institute fellow and began his doctoral research with Howard J. Lucas.[3] At this time, Howard Lucas was the only organic chemist on the Caltech faculty and was to become probably the only chemist to be elected to the National Academy of Sciences in the modern era without a Ph.D. degree. Howard's chemical career hardly seemed to qualify him for a professorship at Caltech in the era of such early giants of chemistry at that institution as A. A. Noyes, Linus Pauling, Richard Tolman, Richard Badger, and Don M. Yost. However, Lucas was hired as an instructor of Throop College somewhat before A. A. Noyes and George Ellery Hale began to exert their notable effects on Throop, which metamorphosed it into the California Institute of Technology. Lucas received his M.S. degree at Ohio State, then became associated with an agricultural station in Puerto Rico, where he published papers on the milk of Puerto Rican cows and the adulteration of peanut oil with, of all things, nitrobenzene. Despite this, by 1927 Howard Lucas had become a leader in the field of what in 1940 was to be labeled physical organic chemistry. He had a knack for picking significant research problems and he had the intelligence and persistence to learn qualitative structural theory from Linus Pauling.

With Lucas and his prior training at Carnegie, Bill Young flourished, and completed his Ph.D. with six publications in two years, each concerned with preparation and characterization of the isomeric butenes in pure form, surely projects of interest at that time to the Petroleum Research Institute. One should remember that this was many years

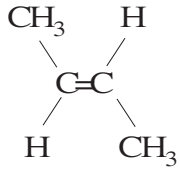
before the advent of chromatographic or spectroscopic means of establishing the composition of butene mixtures, and one of the projects Young worked on was a chemical procedure for analysis of such mixtures. It was a nontrivial task, involving first conversion of the butenes to dibromide, distillation, and then analysis of the three-component dibromide mixture by density, refractive index, and determination of the second-order rate constants with potassium iodide in acetone.

Young's promise as a researcher led to an award of a prestigious National Research Council postdoctoral fellowship at Stanford, where he again worked with H. A. Spoehr. From this came one publication on the preparation of glyceric aldehyde, the stereochemical prototype of the carbohydrates according to the convention of Emil Fischer.

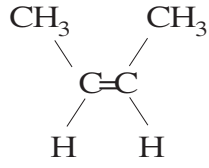
At the end of his Stanford period, he was appointed instructor at the University of California, Los Angeles. The year marked a turning point for this new university, which was founded to provide higher public education in the rapidly growing Los Angeles area. The program was set up under the thumb of the Berkeley administration and no competition was desired on the doctoral level. However, a number of excellent faculty members were acquired in the hope that the situation would ultimately change and also because a wonderful new campus of Romanesque buildings was being constructed in a bucolic setting in West Los Angeles. Even in its earliest days, UCLA had notable undergraduates, the most prominent being Glen Seaborg of transuranic-element fame and Saul Winstein, a world leader in physical organic chemistry, to be discussed further shortly. The chemistry department was a mix of young, bright instructors and assistant professors with a small older group inherited from a prior, essentially teachers college at a campus in west-central Los Angeles. Chairman William

Conger Morgan, who had done no chemical research since his Ph.D. thesis, was regarded as a curmudgeon by the students, but he had excellent taste in selecting new faculty members. A master's degree was offered in chemistry and, as is not uncommon for strong departments with no doctoral program, master's theses were usually the equivalent of Ph.D. theses at many other institutions. This was so at UCLA where Bill Young's students Saul Winstein, Stanley J. Cristol, and Jerome Vinograd, all future Academy members, had significant master's theses.

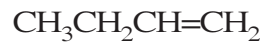
Young's major chemical achievements stemmed from his desire to be able to prepare pure *trans*-2-butene **1** in substantial laboratory quantities from of *cis*-2-butene **2** and 1-butene **3**.



trans-2-butene **1**

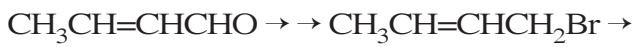


cis-2-butene **2**



1-butene **3**

The sequence was simple and seemed to offer no difficulties.



4 **5**

6 **1**

The starting material, 2-butenal **4**, was shown to be the all-*trans* isomer, but the succeeding steps would up giving an unexpectedly serious mixture of **1**, **2**, and **3**. Here Saul

Winstein produced a magnificent master's thesis showing that the procedures to make **5** were such as to produce mixtures of the isomers **5** and **7**.

$$\underset{\mathbf{5}}{CH_3CH{=}CHCH_2Br} \rightarrow \underset{\mathbf{7}}{CH_3CHBrCH{=}CH_2}$$

Furthermore, **5** and **7** were more or less unstable and rather easily converted to an equilibrium mixture. Winstein and Young unraveled the mechanisms for the rearrangement reactions (often known as allylic rearrangements) and showed how pure **5** and **7** could be prepared. Then to the surprise of all concerned, it was found that the above sequences **5** → **6** → **1** gave not **1** but the same mixture as from mixtures of **5** and **7** or from pure **7**. The resolution of this problem has never been completely achieved, but much more is known today. After completion of his master's thesis, Winstein did a pathbreaking Ph.D. at Caltech with Howard Lucas, went off on a National Research Council fellowship to Harvard, an instructorship at the Illinois Institute of Technology, and finally back to UCLA for an illustrious career until his untimely death in 1969.

I arrived on the UCLA scene as a freshman in 1936, when elementary chemistry was being taught by William Conger Morgan. I had my first exposure to Bill Young when he was brought in to the freshman class to demonstrate and explain electrical conductance of ionic solutions, a subject on which Professor Morgan apparently was not up to speed. I started research in 1938 in analytical chemistry and, intrigued by organic chemistry as taught by master teacher G. Ross Robertson, I asked Robertson if I could do research with him in my senior year. He said no and that I should be working with someone at the forefront and recommended Bill Young. When I talked with Bill, he suggested that I

work on the allylic rearrangements occurring in the conversion of **5** → **7**. But before I could get started, Saul Winstein came to visit fresh from his postdoctoral at Harvard. He convinced Bill that I should work on the mechanism of a different type of allylic rearrangement, which was of substantial theoretical interest during that period

At about this time (1940) Professor Morgan died and Bill Young was the popular choice to succeed him as department chairman. So, when he moved upstairs to the chairman's office, he installed me, a senior undergraduate, in his private office-laboratory downstairs. I shared this with another senior, William G. McMillan, later to be a professor of physical chemistry at UCLA. Besides research, we did most of our physical chemistry laboratory experiments in Young's laboratory. The allylic-rearrangement research went well, and Bill Young, with characteristic generosity and knowing I was to start graduate work at Penn State, invited me to present the results at the September 1941 meeting of the American Chemical Society in Atlantic City.

After the attack on Pearl Harbor, I was quite uncertain of the future, and Bill arranged for me to come to work on a war project starting at UCLA. Bill and T. A. Geissman (a new faculty member from Illinois) were the principal investigators on the project, which was concerned with extracting oxygen from the air with a cobalt-containing organic chelate called salcomine.[4] It was an ostensibly efficient process intended to be used in the field or on aircraft in flight. The brown chelate absorbed oxygen rather quickly from the air and turned black. When heated to 100°C, the oxygen was released. The cycle could then be repeated. The problems were to synthesize new chelates to find ones that were more rapid in combining with oxygen, had higher oxygen capacities, or would degrade more slowly when re-

cycled. For me it was a marvelous learning experience in all three areas.

For a brief period the peons of the project were also involved in assembly work for the famous bat-incendiary bomb project.[5,6] Bill Young was also in the testing phase, and this proved quite effective when by accident the testing group burned down a small military airport in New Mexico. Bill very much enjoyed telling about this ill-fated venture, which has been marvelously documented by L. F. Fieser.[5]

During Bill Young's chairmanship, a powerful group of young faculty were brought in; besides T. A. Geissman and Saul Winstein, there were T. L. Jacobs (Cornell via Harvard), C. D. Coryell (Caltech), and D. J. Cram (Harvard). Coryell was lost to MIT via Oak Ridge, but the department flourished and soon became one of the strongest in the country, especially after a Ph.D. program was started in the early forties. Bill was able to expedite the process by serving several critical years on the university budget committee. In 1948 Bill became dean of physical sciences and in 1957 vice-chancellor for planning, a position he held until he retired in 1970. Here he played an important role in bringing about the Center for Health Sciences, now one of UCLA's crown jewels.

My own research connection with Bill Young, besides the undergraduate research done in collaboration with Saul Winstein, had to do with the nature of the butenylmagnesium bromide **6** and was started immediately after the war project was closed down. In seeking a solution to this difficult problem in the absence of such techniques as nuclear magnetic resonance spectroscopy, Bill was for me an ideal doctoral supervisor.[7] He was willing to spend a substantial sum for a state-of-the-art fractionating column, encouraged my creativity, allowed me to participate extensively in teach-

ing, and developed a close and warm friendship with my wife and me. The department was generally close-knit, with a monthly dance group and other social activities. Bill often invited me to lunch with other faculty and administrative officials to understand better how universities work. Along with his continued commitment to understanding allylic rearrangements, Young extended his early interest in polyenes by embarking on a program to devise a practical synthesis of vitamin A. Although the effort led to eight publications, it was unsuccessful; but, it did bring back one of Bill's strongest students, Stanley J. Cristol, an earlier, successful M.S. candidate for a UCLA Ph.D. under Young's supervision.

Bill Young was active in the American Chemical Society (ACS), first, in the Southern California Section, where he served as chairman for the 1940-41 term and nine years as national councilor. He was on the Executive Committee of the Organic Division for five years and chairman from 1948 to 1949. From 1958 to 1960 he was a member of the ACS Board of Directors. However, his most important work for the ACS was on the Committee on Professional Training, where he served for seventeen years (1943-60) and as chairman (1948-58). This committee established an influential accreditation system to certify institutions based on the quality of their undergraduate chemistry programs and to certify students who met the minimum requirements for a chemistry degree. The program had its controversial aspects. Many universities and colleges resented having another accreditation program piled on those already in place and particularly for a scientific rather than a professional major. The penalty, hardly severe, for noncertified graduates was exclusion from full ACS membership for two years. Nonetheless, lack of accreditation was used by department chairmen as a club on their administrations to

strengthen their course offerings, facilities, and faculties often with signal success. Of course, the chemistry program of the major research universities had little difficulty in becoming accredited, even if, in some cases, the menu of their chemistry courses differed from the standards. A certain amount of experimentation was not only allowed but was encouraged.

Certification of graduates was a different matter. Many institutions, including Harvard, had premedical students that were chemistry majors, but these students did not take the full measure of courses required by the Committee of Professional Training, and the schools usually reported large discrepancies between their total number of chemistry graduates and their certified chemistry graduates.

Suggestions for accreditation of graduate programs were considered, but they were never implemented. Instead, the committee developed and kept updated a very useful directory of U.S. and Canadian graduate programs in chemistry, biochemistry, and chemical engineering. This directory contains brief vitae of faculties, lists of their publications, and an indication of which publications came about from the work of graduate students and which from postdoctoral fellows. Bill Young sacrificed much of his research effort as the result of his resolve to make this and other parts of the accreditation program successful.

Bill Young received many honors for his scientific and administrative achievements. He was the first of the UCLA faculty to be elected to the National Academy of Sciences (1951). UCLA honored him as faculty research lecturer (1947), accorded him its Distinguished Service medal (1964) and an honorary LL.D. (1972), and named the chemistry building Young Hall (1970). He also received D.Sc. degrees from Colorado College (1962) and the University of Colorado (1975). He was selected for a Distinguished Alum-

nus Award from Caltech (1968). The American Chemical Society honored him with the first Richard C. Tolman Medal (1961), the Chemical Education Award (1962), and the society's highest award, the Priestley Medal (1968).

William G. Young was an exceptionally modest and gentle man, who focused his attention far more on helping others than advancing his own interests. Always fun to be with, he worked out difficult situations with dogged patience and always with his eyes on the future. He was enormously effective in teaching, research, and administration, and he was a person you liked to deal with. He brought enormous credit to his profession.

NOTES

1. W. G. Young and D. J. Cram. Saul Winstein. In *Biographical Memoirs*, vol. 43, pp. 320-53. New York: Columbia University Press for the National Academy of Sciences, 1973.

2. W. G. Young. Autobiographical statement, p. 1. National Academy of Sciences archives. Washington, D.C., 1950.

3. W. G. Young and S. Winstein. Howard Johnson Lucas. In *Biographical Memoirs*, vol. 43, pp. 163-76. New York: Columbia University Press for the National Academy of Sciences, 1973.

4. T. A. Geissman, M. J. Schlatter, I. D. Webb, and J. D. Roberts. The synthesis of some intermediates for use in the preparation of analogs of salicylaldehyde ethylenediamine cobalt ("Salcomine"). *J. Org. Chem.* 11(1946):741-50.

5. L. F. Fieser. *The Scientific Method. A Personal Account of Unusual Projects in War and Peace*, pp. 121-34. New York, Reinhold, 1964.

6. S. M. Hersh. *Chemical and Biological Warfare*, pp. 62-63. New York: Bobbs-Merrill, 1968.

7. J. D. Roberts. *The Right Place at the Right Time*, pp. 29-44. Washington, D.C.: American Chemical Society, 1990.

SELECTED BIBLIOGRAPHY

(The selection of references given below is based on the author's judgment and may not reflect in more than a qualitative way the range or importance of William G. Young's research.)

1927

With J. H. C. Smith and H. A. Spoehr. The preparation and properties of leaf pigments. *Carnegie Inst. Washington, Yearb.* 26:156-57.

1929

With R. T. Dillon and H. J. Lucas. The synthesis of the isomeric 2-butenes. *J. Am. Chem. Soc.* 51:2528-34.

1930

With H. J. Lucas. The composition of butene mixtures resulting from the catalytic decomposition of the normal butyl alcohols. *J. Am. Chem. Soc.* 52:1964-70.

With R. T. Dillon and H. J. Lucas. The reaction rates of potassium iodide with 1,2- and 2,3-dibromobutane. The analysis of mixtures of the normal butenes. *J. Am. Chem. Soc.* 52:1953-64.

1932

With A. N. Prater. The allylic rearrangement of crotyl bromide and crotylmagnesium bromide. *J. Am. Chem. Soc.* 54:404.

Investigations on the stereoisomerism of unsaturated compounds. I. The composition of crotonaldehyde. *J. Am. Chem. Soc.* 54:2498-2503.

1936

With S. Winstein. Allylic rearrangements. I. Crotyl and methylvinylcarbinyl bromides. *J. Am. Chem. Soc.* 58:104-107.

With S. Winstein and A. N. Prater. Allylic rearrangements. II. Crotyl and methylvinylcarbinylmagnesium bromides. *J. Am. Chem. Soc.* 58:289-91.

1937

With F. E. Blacet and J. G. Roof. Studies of absorption spectra. I. Crotonaldehyde and acrolein. *J. Am. Chem. Soc.* 59:608-14.

1938

With J. F. Lane. Allylic rearrangements. V. The mechanism of the reaction of crotyl alcohol and methylvinylcarbinol with solutions of hydrogen bromide. *J. Am. Chem. Soc.* 60:847-53.

1939

With G. Ballou and K. Nozaki. Allylic rearrangements. VIII. The action of magnesium on cinnamyl chloride. *J. Am. Chem. Soc.* 61:12-15.

1942

With J. D. Roberts and S. Winstein. Allylic rearrangements. XIII. Kinetics and mechanisms of the conversion of crotyl and methylvinylcarbinyl chlorides to acetates and ethyl ethers. *J. Am. Chem. Soc.* 64:2157-64.

1944

With L. J. Andrews and S. J. Cristol. Polyenes. I. The synthesis and absorption of the ionylideneacetones and related compounds. *J. Am. Chem. Soc.* 66:520-24.

With J. F. Lane and J. D. Roberts. Allylic rearrangements. XV. The carbonation of butenylmagnesium bromide. *J. Am. Chem. Soc.* 66:543-45.

With J. D. Roberts. Highly-branched compounds. The preparation of triisopropylcarbinol and diisopropyl-s-butylcarbinol. *J. Am. Chem. Soc.* 66:1444-45.

With J. D. Roberts. The reaction of butenylmagnesium bromide with acetomesitylene. *J. Am. Chem. Soc.* 66:2131.

1946

With A. C. McKinnis, I. D. Webb, and J. D. Roberts. Allylic rearrangements. XIX. Studies of the ozonization of allylic systems. *J. Am. Chem. Soc.* 68:293-96.

With J. D. Roberts. Allylic rearrangements. XX. Some addition reactions of butenylmagnesium bromide. *J. Am. Chem. Soc.* 68:649-52.

With J. D. Roberts. Allylic rearrangements. XXI. Further studies related to the nature of the butenyl Grignard reagent. *J. Am. Chem. Soc.* 68:1472-75.

1947

With T. W. Campbell. Allylic rearrangements. XXIII. The reaction of the sodium derivative of allylbenzene with carbonyl compounds. *J. Am. Chem. Soc.* 69:3066-68.

With R. L. Meier, J. Vinograd, H. Bollinger, L. Kaplan, and S. Linden. Investigations on the stereoisomerism of unsaturated compounds. VIII. The catalytic hydrogenation of butadiene. *J. Am. Chem. Soc.* 69:2046-50.

1949

With R. E. Kepner and S. Winstein. Allylic rearrangements. XXIV. Abnormal bimolecular substitution. The condensation of butenyl and pentenyl chlorides with sodium malonic ester. *J. Am. Chem. Soc.* 71:115-18.

1950

With K. W. Wilson and J. D. Roberts. Allylic rearrangements. XXVIII. The reactions of butenylmagnesium bromide with hindered ketones. *J. Am. Chem. Soc.* 72:218-19.

1951

With I. D. Webb and H. L. Goering. The reaction of butenyl chlorides with diethylamine and triethylamine. *J. Am. Chem. Soc.* 73:1076-83.

With S. Winstein and H. L. Goering. Allylic rearrangements. XXXII. The solvolysis and intramolecular rearrangement of α,α-dimethylallyl chloride. *J. Am. Chem. Soc.* 73:1958-63.

1957

With R. H. DeWolfe, D. E. Johnson, and R. T. Wagner. Allylic rearrangements. XXXVIII. The reactions of cinnamyl Grignard reagents and alkali metal derivatives of allylbenzene with acetophenone. *J. Am. Chem. Soc.* 79:4798-4802.

1958

With S. H. Sharman, F. F. Caserio, R. F. Nystrom, and J. C. Leak. Allylic rearrangements. XLIII. Some studies of the thermal decomposition of allyl chlorosulfinate-1-C^{14}. *J. Am. Chem. Soc.* 80:5965-71.

1959

With R. E. Ireland, T. I. Wrigley, C. W. Shoppee, B. D. Agashe, and G. H. R. Summers. Allylic rearrangements. XLII. Solvolytic reactions of 3β-chlorocholest-4-ene and 3α-chlorocholest-4-ene. *J. Am. Chem. Soc.* 81:1452-54.

1966

With J. S. Franklin. The acid-catalyzed isomerization of α- and *cis*- and *trans*-γ-methylallyl alcohols. *J. Am. Chem. Soc.* 88:785-90.

1969

With R. A. Benkeser, W. E. Broxterman, D. A. Jones, Jr., and S. J. Piaseczynski. The prevalence of *cis*-addition products in the reaction of the butenyl Grignard with sterically hindered ketones. *J. Am. Chem. Soc.* 91:132-37.

1971

With H. E. Green and A. F. Diaz. The acid-catalyzed isomerization of the butenyl acetates. *J. Am. Chem. Soc.* 93:4782-87.

1972

With Z. Rappaport and S. Winstein. Allylic oxidation of olefins by mercuric acetate. *J. Am. Chem. Soc.* 94:2320-29.