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ROBERT BURNS WOODWARD
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A Biographical Memoir by

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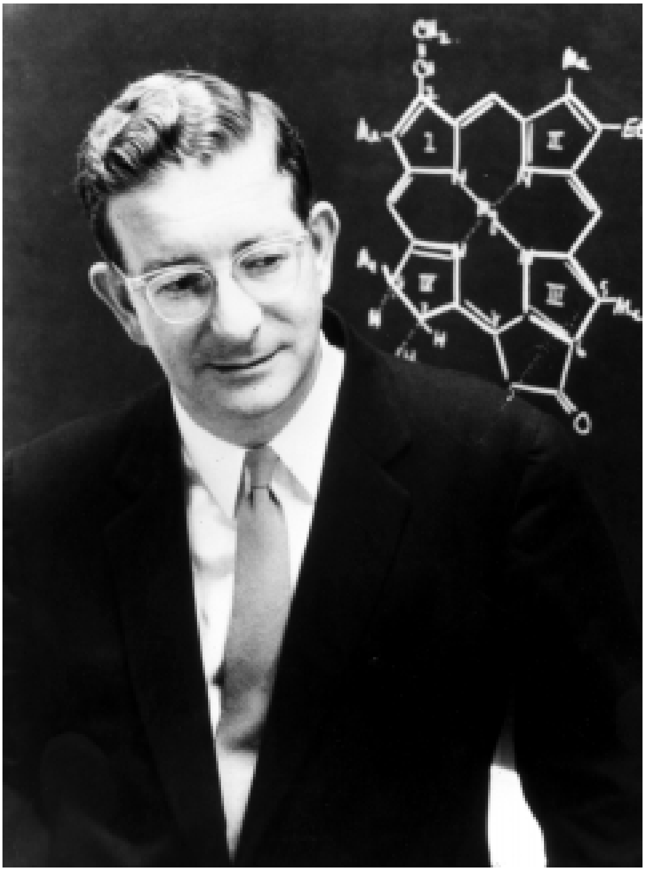

ROBERT BURNS WOODWARD

April 10, 1917–July 8, 1979

BY ELKAN BLOUT

ROBERT BURNS WOODWARD was the preeminent organic chemist of the twentieth century. This opinion is shared by his colleagues, students, and by other distinguished chemists. Bob Woodward was born in Boston, Massachusetts, and was an only child. His father died when Bob was less than two years old, and his mother had to work hard to support her son. His early education was in the Quincy, Massachusetts, public schools. During this period he was allowed to skip three years, thus enabling him to finish grammar and high schools in nine years. In 1933 at the age of 16, Bob Woodward enrolled in the Massachusetts Institute of Technology to study chemistry, although he also had interests at that time in mathematics, literature, and architecture. His unusual talents were soon apparent to the MIT faculty, and his needs for individual study and intensive effort were met and encouraged.

Bob did not disappoint his MIT teachers. He received his B.S. degree in 1936 and completed his doctorate in the spring of 1937, at which time he was only 20 years of age. Immediately following his graduation Bob taught summer school at the University of Illinois, but then returned to Harvard's Department of Chemistry to start a productive period with an assistantship under Professor E. P. Kohler.

He remained at Harvard until his death in 1979. Although Bob remained formally at Harvard for his entire professional life, he traveled extensively in order to lecture, receive awards, and sometimes to relax for a day or two with friends.

During the period 1945 through 1978 he received 24 honorary degrees. He was also the recipient of 26 medals and awards, which included the most prestigious in the world of chemistry. He was elected to the National Academy of Sciences in 1953 at the notably young age of 36. In addition, Bob was elected to membership in numerous academies and learned societies. A more complete and personal biographical summary, including a listing of his honors and awards, can be found in the Royal Society memoir by Lord Todd and Sir John Cornforth.[1]

What kind of a person was Bob, and how do we remember him? He was a genius and a very sensitive individual with a prodigious memory. He also had a drive to solve difficult problems and liked teaching in the broadest sense of the word. His lectures were models of clarity, originality, and insight. He enjoyed starting at the upper left-hand corner of a very large blackboard and finishing at the lower right-hand corner with precise formulation of his ideas and thoughts and a total package that was characteristically Woodwardian. He really relished lecturing to students and to colleagues, but he did not enjoy formal courses. Fortunately, in the middle of his Harvard career, Bob was given an endowed professorship in 1962 that eliminated the need for him to teach regular courses. However, he maintained, "I teach all the time so that I don't have to teach formal courses." He taught in the laboratory, in seminars, and via lectures, so here was no question that he was an expert in this kind of teaching.

He accomplished much in several diverse areas (e.g., the correlation of various physical methods with organic structures, determination of the structures of complex compounds, the syntheses of many naturally occurring biological compounds, and devising beautiful and concise synthetic pathways for complicated molecules). Last but not least, he had a deep appreciation of the wonders of nature and often attempted to understand many of the relations between the biological world, the chemical world, and the synthetic chemistry world in which he practiced so adroitly. Among the achievements of Woodward in the field of organic synthesis are the total syntheses of quinine, steroids, strychnine, reserpine, chlorophyll, and, finally, one of the most important and complicated naturally occurring molecules, vitamin B_{12}.

During his lifetime he authored or coauthored 196 publications, of which 85 are full papers, the remainder comprising preliminary communications, the text of lectures, and reviews. The pace of his scientific activity soon outstripped his capacity to publish all experimental details, and much of the work he participated in is even now being published by his colleagues and coworkers.

In preparing this review I was assisted by written communications relating to Bob's work by Derek Barton, Albert Eschenmoser, and Roald Hoffmann. Many of his students and postdoctoral fellows wrote to describe their feelings, attitudes, and assessments of Bob Woodward as a mentor, as a teacher, and as a friend. We regret that space does not allow us to include many of these statements, but we will do our best to present the feelings of some of them in the next sections.

EARLY SCIENCE
(WITH INPUT FROM DEREK BARTON)

In 1948, when he was 31, Woodward gave a brilliant lecture at Imperial College, London, on the structure of santonic acid. He spoke without notes or slides and covered the blackboard with beautifully drawn formulae. He argued that santonic acid, a compound obtained from santonin and hitherto without a structure, was formed by the base-catalyzed opening of the lactone ring with alpha-beta double bond shift to give a keto-acid, the anion of which cyclized to give santonic acid. He showed that the latter must be a derivative of cis-decalin.

Every scientist must be judged by the standards of his time. In 1948 we had never heard anyone pose, and then resolve, a problem in such a clear and logical manner. Woodward was the first to show that problems in chemistry could be solved by thinking about them. The scientific world first heard of Woodward between 1940 and 1942 because of his publications on the correlation of ultraviolet spectra with structure (1941,1942). He next became famous for the formal synthesis of quinine (1944), in association with W. von E. Doering.[2] This was Woodward's first multistep synthesis. For this synthesis he immediately gained the respect of the older generations of organic chemists.

Woodward was exceptionally gifted in deducing structures. At a time when physical methods were not yet perfected he could integrate an enormous number of facts, both clear and misleading, into a coherent whole better than any chemist who had ever lived. During World War II he started with a reasoned argument (1944) for the beta-lactam formula for penicillin, in contrast to the incorrect oxazoline formula advocated by Sir Robert Robinson and others.

The problem of the structure of strychnine had been a challenge to organic chemists for more than a century. Robinson had worked hard and well on this subject for many years, and immediately after the war he made it his major project. It was a perfect challenge for Woodward. There was an enormous body of fact that, with the aid of a minimum of concise experimentation, led him to deduce the correct formula (1954).

In 1949 Barton was invited to spend a year at Harvard to replace Woodward, who was on sabbatical leave. It was no surprise to anyone also that Woodward stayed exactly where he was. At that time his evening seminars were marvelous, with Gilbert Stork also participating in a stimulating way. In theory a speaker would be recruited to talk about his work starting at 8:00 p.m., but in fact at approximately 8:30 p.m. The speaker would be closely questioned by all. At about 10:00 p.m. Woodward would pose a problem from the literature. Guests and students would then spend up to an hour trying to solve this problem. When they had all failed Woodward would give his solution, which was always correct. He would then call for anyone else to pose a problem. This being done, it was usually Woodward who proposed the correct answer first. However, this was somewhat unfair, because as midnight approached Woodward got better and better; at least it seemed like that, while others tired.

The most brilliant analysis ever done on a structural puzzle was surely the solution (1953) of the terramycin problem. It was a problem of great industrial importance, and hence many able chemists had performed an enormous amount of work trying to determine the structure. There seemed to be too many data to resolve the problem, because a significant number of observations, although experimentally correct, were very misleading. Woodward took a large piece of cardboard, wrote on it all the facts and, by thought alone, deduced the correct structure for terramycin. Nobody else could have done that at the time.

The first major synthesis by Woodward after quinine was that of the steroid nucleus, including cholesterol (1951) and cortisone (1951). Woodward went to the United Kingdom in 1951 to deliver a Centenary Lecture. He spoke about his total synthesis of steroids, which was brilliant, and all in the audience were impressed when he showed the formula and said that this was known as chrismasterol because it was first synthesized on Christmas day in 1950.

The next important target chosen was strychnine (1954). Strychnine had five asymmetric centers, and therefore would seem to be a difficult objective. However, Woodward realized that the constitution of the molecule was such that it defined its own configurations and that a total synthesis should be relatively easy. An elegant synthesis was planned and executed with highly talented collaborators, again in a short time.

During the same *époque* Woodward and Barton collaborated on another total synthesis (by relay). The determination of the structure (1954) of lanosterol suggested to Woodward that this biosynthetically important compound should be synthesizable by the addition of three methyl groups to cholesterol, whose total synthesis he had just accomplished more easily than expected.

Woodward now began what may be the most beautiful synthesis of his life, the synthesis (1956) of the medicinally important alkaloid reserpine. This synthesis was meticulously planned and executed in less than two years by a highly gifted group of coworkers. It was a challenging stereochemical problem, and it was a pleasure to see how skillfully he used the now mature theory of conformational analysis.

Now we come to the funding of the Woodward Institute in Basel by the Ciba Company. Ciba research was reorganized with Dr. Heusler, who had participated in the Woodward steroid synthesis, joining the new Woodward Institute, directed by Woodward. Their first project was the synthesis of cephalosporin C starting with L-cysteine, and he produced an elegant and sophisticated synthesis that was completed just in time (1966) for the Nobel Prize ceremony in

1965. The Woodward Institute continued to do good work until his death.

This account of Woodward's earlier scientific life reflects his unique intellectual superiority in his own generation. In the 1940s he was already a mature and apparently self-assured young man who knew more organic chemistry than anyone else did, and who could instantly integrate the remembered facts to face a new challenge. He pretended to be lazy and to be without ambition; he was just the opposite. In the 1950s his work in several areas reached a level of brilliance that may never be equaled.

INTERMEDIATE SCIENCE
(WITH INPUT FROM ROALD HOFFMANN)

In 1983 Woodward gave a lecture at an American Chemical Society meeting in which he described his pleasure at seeing the original publication by Diels and Alder describing the discovery of the reaction that bears their name and his lifetime preoccupation with that reaction. He made use of the Diels-Alder reaction in a marginally commendable (in his own words) approach to the synthesis of oestrone during the middle 1930s in the course of his work for his Ph.D. degree. Much later he used the Diels-Alder reaction with greater effect in the syntheses of cholesterol, cortisone, and reserpine. And beginning in 1939 he remembered that he pursued a number of investigations explicitly concerned with the detailed course and mechanism of Diels-Alder reactions.

Theoretically, interesting molecules always intrigued Woodward. The paper that marks the beginning of modern organometallic chemistry, the assignment of the correct sandwich structure of ferrocene, bears not only the name of one of the future leaders of the field, Geoffrey Wilkinson, but also of Woodward and two of his coworkers, M. Rosenblum and M. C. Whiting.

It is likely that it was his colleague William Moffitt who sharpened Woodward's perception of modern theoretical chemistry and introduced him to molecular orbital theory. Though the remarkable generalization that is the octant rule was initially empirically formulated by Woodward, Klyne, and Djerassi, the theoretical support it received from Moffitt and Moscowitz was essential (1961).

Woodward then began to think in orbital ways. One interesting piece of evidence of this was a comment he made after a lecture by Rolf Huisgen at a Welch conference in 1961, where he drew quite explicitly the orbitals of a vinyl carbene and asked, "Do we have specific orbital requirements for this reaction and would they in this case preclude the operation of the mechanism, which is so general in many of the cases?"

It is clear that Woodward was exceedingly well prepared for what ensued. In the course of the synthesis of vitamin B_{12} he raised and considered many theoretical points in trying out some reactions that would create several asymmetric compounds in one step. He concluded that electronic effects had to be at work to explain the observed results. The reaction was the cyclization of a hexatriene to a cyclohexadiene. It is clear that one of Woodward's main interests in his latter years was the rationalization of organic synthesis through the use of orbital symmetry in predictions of conformations. In 1964, after a brief discussion with E. J. Corey and others of his thoughts and his chemical results, Woodward was able to formulate his important ideas in this area. Some of them were published in the book *The Conservation of Orbital Symmetry* (1970) with his collaborator, Roald Hoffmann. Woodward turned for theoretical support to a promising young theoretician, then a junior fellow at Harvard (as Woodward had been 20 years earlier). This was Roald Hoffmann, then 26 years old. Hoffmann began by corroborating

Woodward's simple orbital idea. Quickly a true collaboration developed that led to five communications (1965) that changed the nature of the interaction theory with experimental organic chemistry.

Woodward and Hoffmann in a remarkable blue-green paper in *Angewandte Chemie* (1969) and book showed that frontier orbital arguments, the simple nodal structure of orbitals, enforced by their symmetry could explain the concertedness or lack thereof and many important stereochemical selectivities in every reaction. They extended their consideration from electrocyclic reactions to cycloaddition and other concerted reactions. Here recall Woodward's nearly 40-year-long interest in the Diels-Alder reaction and to sigmatropic reactions, which included the Cope rearrangement and various bond shift reactions. The predictions made by Woodward and Hoffmann were easily accessible to modern organic chemists already introduced to orbitals by important books on this subject. The predictions were readily verifiable, and there was a large community of physical organic chemists ready and able to test them. Within two years, in a flood of beautiful experimental work, the fantastic insight into the workings of nature achieved by the orbital symmetry rules lay clear. Quantum mechanics and organic chemistry were drawn much closer to each other.[3]

SCIENCE IN THE LAST YEARS (WITH INPUT FROM ALBERT ESCHENMOSER)

It is clear that two of the major synthetic achievements of Woodward are those that occurred in the 20 years prior to his death, namely, the synthesis of chlorophyll and the synthesis of vitamin B_{12}. Woodward's analysis of the complex structure of chlorophyll and the transformational aspects of what he called "a chemical fairyland" rests securely among the great chapters of interpretative natural products chemistry. The result of this analysis was a visionary plan for the synthesis of chlorophyll. The essence of his approach and his synthetic plan derived from his stereochemical analysis that a porphyrin substituted in the γ position can be expected to be convertible into a chlorin with the correct structure.

Woodward's conclusion that he would be able to use the porphyrin to chlorin transformation was the key to his synthesis but was not supported on the basis of any known change from a suitably constructed porphyrin molecule. He had the faith of "an impassioned mountaineer" who would negotiate the critical phase of an ascent on the basis of analysis, experience, and strength. This synthesis bears witness to Woodward's extraordinary ability to explore and also to deal with discoveries made in the course of the synthesis.

It was a source of some satisfaction to Woodward that a photochemical reaction was incorporated into his chlorophyll synthesis as an important step. It has been stated that perhaps the most important element in his synthesis was when he could pit his intellect against the puzzles provided by unforeseen observations, and perhaps his greatest strength in synthesis was his capacity of overcoming the experimental difficulties that could interfere with his original plan. He had a deeply rooted conviction, nurtured by the history of natural products chemistry, that the chemist is offered an opportunity to explore new phenomena as a consequence of the elucidation of unanticipated findings in synthesis.

With the completion of this synthesis, Woodward stood at the summit of recognition: He had mastered chlorophyll, the material that gives the continents their primary color and the heart of photosynthesis in all green plants on Earth. It is a substance deeply anchored in the consciousness of humanity as a material upon which ultimately our entire existence depends. Finally, it could be argued that natural product synthesis had been invoked at an important time in the exercise of one of its original functions, namely, to provide chemical proof of the constitutional formula of a natural product.

As Woodward began to tackle the synthesis of more and more complicated biologically important natural products, he ultimately chose to attack vitamin B_{12}.[4] At Harvard this work started in 1961 and gradually evolved into a unique collaboration between Woodward's group and the group of Eschenmoser at ETH. This work culminated in the announcement by Woodward of the total synthesis of cobyric acid (1973).

The two major challenges posed by the vitamin B_{12} structure were the novelty of the ligand chromophore and the stereochemical complexity of the ligand's periphery. Woodward's main focus was the latter. This led him to create a great synthesis of the so-called "Harvard component"—the part of the B_{12} molecule that is the most complex and contains rings A and D. This synthesis, both in design and execution, appears today as the apotheosis of all that constituted the Woodwardian art and science in natural products total synthesis. Forever in the history of chemistry it will also remain connected with that creative insight of Woodward that eventually grew into the message of the Woodward-Hoffmann rules, changing the way organic chemists think about the reactivity of organic molecules.

REMINISCENCES ABOUT A SPECIAL MAN

I first met Bob Woodward a week or so after I arrived at the Chemistry Department of Harvard as a National Research Council fellow. In this initial meeting he was as advertised by his senior colleagues—intense, committed, and brilliant. As a result of this meeting he and I worked together experimentally for well over a month, and this resulted in the publication of a paper in the *Journal of the American Chemical Society*. Perhaps this was the last time that I knew he did experiments in the laboratory.

As a result of this initial experience he invited me to join an embryonic poker game that met in the basement of Harvard's Converse Laboratory on some late evenings each month. This poker game still exists now, in a slightly altered form, some 50 years later. The following are some not-so-random thoughts about Bob and many of his qualities. His intensity as a scientist is well known (vida supra), but he was just as intense in the nonscientific areas of his life. When he wanted to be, he was quite a social person. I remember some of the parties at his Belmont, Massachusetts, home, where puzzles and games were played at his behest and with his participation. He loved such challenges, and as an example, I should tell that he loved doing *The New York Times* crossword puzzle every day, but of course, only in ink. It wasn't necessary for him to erase. He loved and appreciated good food and also good drink. In fact, there was a period in his life where he was drinking a lot and enjoyed having contests with a select few visiting scientists whom he liked socially. He loved to be able to drink more than his colleagues, and he loved trying to prove it (successfully) on many occasions.

Was he into sports? I would say not really, but although on occasion his competitive instincts got the better of him, and he played baseball with some of his younger colleagues. Did Bob have any strong likes except for research and science? Well, as we know, he was married to his first wife Irja Pullman at a very early age, and with Irja he fathered two daughters, Siiri and Jean. Following his separation from Irja, he tried to play the field with women he knew, but he was too straightforward a person to do it with abandon. After this period, he did undertake a second marriage with Eudoxia Muller, and this marriage had very good times as well as many rough spots. As a result of this marriage he fathered two additional children: a daughter who Bob and Doxie named Crystal and a son named Eric. When this marriage broke apart in 1966, he became a not-so-willing bachelor. He still enjoyed female companionship and several, including his secretary, were close to him during this period.

One relationship (not female) that was important to him was with Edwin ("Din") Land, the founder and chief executive officer of Polaroid Corporation. He became a consultant to Din in 1940, and it was a relationship that was maintained despite varying interests by each party through a long period of years. As a result of Bob's input, when I left Harvard temporarily to join Polaroid as its chief chemist in 1960, Bob and I spent a lot of time together while he was a consultant to the company during the critical phases of the development of the Polaroid instant photographic color process. He was a wonderful consultant; he was able to be as intense and thoughtful in his criticism and imagination in fields relating to color chemistry as he was in his own active fields of chemistry (i.e., natural products). During this period he gave us at Polaroid many suggestions and much criticism that helped in the final development of the color photographic process. It was also during this period that Polaroid issued options to its most important personnel (e.g., the vice-presidents of the company). Knowing Bob's contribution to Polaroid, I talked to Din Land about doing an unusual thing: Give this unusual scientist, who was not a Polaroid employee, an option on Polaroid stock. Land agreed after some discussion, and Bob received this option; it played a very positive part in the financial aspects of his later life. In some ways it allowed him to be as independent financially as he was scientifically.

He loved to smoke and smoked two or three packs a day from 1942 until his death in 1979. Many of us tried to get him to give up this habit, but we were not successful.

As we know from some of his scientific activities, symmetry played a large part in his thinking and, in fact, it played a part in his personal life. He had a very symmetrical license plate, and he tried to have symmetrical relations with his children, although that was not always successful. He had very strong likes as a person. Some of these were his love of the color blue, which he showed by only using blue neckties and only wearing dark blue suits. His parking place at Harvard was painted blue (by a couple of students) so that no one would use it inadvertently. In addition, in his later years he had a well-loved blue Mercedes sedan that occupied this parking space during the days and nights when he was doing science in Converse.

I can testify that he also liked adventure in areas other than science. I remember well when I bought a new twin-engine fishing boat in 1960, and we tried it out one day by going from Cuttyhunk to Doxie's homeport of Bridgehampton on Long Island. The day was very foggy, and we didn't have any instruments aboard except a compass and a depth meter. Did Bob want to try running the boat? "Of course," he said. He loved it, and actually very much enjoyed piloting the boat for several hours without incident.

In his later years, although he was most interested in science, I observed that he was much more aware of the importance of one-to-one relationships; some he had with his children and some he had professionally. I can testify that when he had a personal relationship he spared no effort. It was as intense as the way he did science. He came through for me in many critical times in my life, and I hope I did the same for him. In personal relationships he knew what was important; he tried to further them with thoughtfulness and intense understanding. I, along with many of his friends, students, and colleagues, can testify how he came through for us in critical areas and times in our lives. He was an emotional man, although he didn't show it often. This emotion led him to be very supportive in close relationships like we had. After the breakup of his second marriage, Bob showed many interests in addition to science. For example, it was clear that he had become a strong Anglophile, and he spent a lot of time in London in its multiple galleries and hotels. In addition, an interest in antiques was awakened, and he began to collect English furniture as intensively as he did science. I regret that many of the beautiful pieces he collected and had in his apartment have now been sold.

In his last years he became much more social, not in a global sense, but in his relationships. When he was in Cambridge we had dinner at least once a week. Even though he loved having a home-cooked meal practically every week with my wife, Gail, and me, we varied the routine by occasionally going to local restaurants. The night before he died we had a wonderful dinner together at the Stockyard and left him in a very good mood when we separated at about 11:00 p.m. During the next hours he suffered a fatal heart attack, and I never saw him again. Although in this period he had many symptoms of a cardiac condition, he ignored them as if they weren't important, and maybe such symptoms were not important to him.

SOME PERSONAL RELATIONSHIPS

In addition to the personal relationships indicated above, Bob had at least two corporate relationships that he specially valued. The first was to a small company called CHON Corporation and the second one was to a very large company Ciba-Geigy. CHON Corporation was set up in 1974 by a group of scientists with the idea of encouraging academic research that might have practical value. One project undertaken by CHON was the development of a high-temperature superconducting polymer. Bob spent several days and weeks trying to think of suitable approaches of what would be new substances. The writings on his ideas are still in the CHON files.

The second corporate relationship that was important to Bob Woodward was his involvement in a research institute in Basel, Switzerland. It started in 1963 and was appropriately named the Woodward Research Institute. This institute, which was his own domain, allowed him to do original research with a practical flavor. J. Gostelli was his senior colleague there. It was well supported originally by Ciba Company and eventually by Ciba-Geigy. His involvement with Ciba-Geigy increased over time, especially after he was elected to its Board of Directors in 1970. In spite of his other interests I am told that his directorship at Ciba-Geigy allowed him to be involved in the real world of the chemical industry in practice, and he found this exciting. Perhaps it appealed to another aspect of Bob Woodward's character.

When it became clear that we were going to write a memoir for the National Academy of Sciences, many of his former colleagues and students wrote to give us some of their thoughts on this remarkable man. A selection of some of these personal feelings follows.

Obviously, a life like Woodward's is not lived without working long hours. Much of his peace and quiet used to come in the night hours after everybody else had left the laboratory. As his noon-to-three-a.m. hours became proverbial, and his eager disciples acquired the habit in increasing droves, so the midnight solitude was no more.

He had the courage to work on a series of more and more complex and difficult natural products during his career, and the intelligence, imagination, energy, and skill to succeed. He inspired the revolution in synthetic organic chemistry, which continues until this day. His synthetic routes are often described as elegant. They incorporate features, which are surprising and would not be expected to occur to other chemists. Often some aspects of the synthesis were inspired by Woodward's ideas concerning the biogenesis of these molecules.

We arrived at the night club and Armstrong was already playing. I introduced Bob to my friend saying, "This is Bob Woodward." My friend turned around impatiently, shook his hand, and returned his attention to the music. I said, "Look, Bill, Bob Woodward is to organic chemistry what Louis is to the trumpet!" At that my friend turned around slowly, looked Bob in the eye, and said, "Man, you must be one hell of a chemist!" Bob said he thought that was the most sincere compliment he ever got.

Bob was very interested in architecture, and at least on one occasion said that he might have liked to become an architect. . . . His habit to start a new pack of cigarettes when the former one was half-empty and to order two daiquiris, which might in both cases have been a sign of continuity of keeping a chain uninterrupted.

I know that he was very interested in mathematical problems. I exchanged problems with him for a while. I once heard him say that, when he was young, he thought of becoming a mathematician. The reason that he did not do so was that in mathematics any new original idea you had was okay. He contrasted this with chemistry where one always had to test any new idea against experimental results in the real world. He considered that this challenge made chemistry a much more demanding and therefore more attractive field to work in.

I owe a lot to R. B. Woodward. He showed me that one could attack difficult problems without a clear idea of their outcome, but with confidence that intelligence and effort would solve them. He showed me the beauty of modern organic chemistry, and the relevance to the field of detailed careful reasoning. He showed me that one does not need to specialize. Woodward made great contributions to the strategy of synthesis, to the deduction of difficult structures, to the invention of new chemistry, and to theoretical aspects as well. He taught his students by example the satisfaction that comes from total immersion in our science. I treasure the memory of my association with this remarkable chemist, and deeply regret his premature death.[5]

Originally, this manuscript was to be the responsibility of Konrad Bloch, Frank Westheimer, and Elkan Blout. Unfortunately, Konrad Bloch became ill before he could contribute to the writing. I am indebted to Derek Barton, Roald Hoffmann, and Albert Eschenmoser for comprehensive pieces about Bob's scientific work. I am also indebted to the several former students, postdoctoral fellows, and colleagues who contributed anecdotes and quotations about Bob's life and scientific achievements. Finally, I want to acknowledge the continuing and thoughtful advice on this memoir by a wonderful colleague who should have been a coauthor, Frank Westheimer.

NOTES

1. Lord Todd and Sir John Cornforth. *Robert Burns Woodward Biographical Memoir*, pp. 629-95. London: The Royal Society, 1982.

2. It should be recorded that at least part of the stimulus for Bob Woodward to do a quinine synthesis in the 1940s was due to the need of quinine by the Polaroid Corporation for a synthetic polarizer invented by Edwin H. Land. Although the synthesis was accomplished by Woodward and Doering, it was never used industrially, because new polarizers with superb qualities had been invented by that time. Gilbert Stork has pointed out recently that Woodward and Doering's synthesis was not a complete synthesis of quinine and raises questions about the stereospecificity of the synthesis, while not detracting from its originality (J. Am. Chem. Soc., 123, 3239 (2001)).

3. In 1981 Roald Hoffmann and Kenichi Fukui received the Nobel Prize in chemistry for discovering the rules governing the course of chemical reaction. Many scientists feel that, if Bob had lived, he would surely have shared in this award.

4. A. Eschenmoser. RBW, vitamin B_{12}, and the Harvard-ETH collaboration. In *Robert Burns Woodward: Architect and Artist in the World of Molecules*. American Chemical Society-Chemical Heritage Foundation, in press.

5. When Konrad Bloch and I were raising money for a Woodward professorship at Harvard, many of the organizations cited above indicated their strong and positive feelings about Bob by contributing generously to this professorship.

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