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R. B. Woodward the teacher, as remembered by his students

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In August 1981, at the New York meeting of the American Chemical Society, a special symposium was organized to honor the memory of R. B. Woodward. Among the participants at this symposium were five of Woodward's former coworkers holding academic positions in the United States.[†] On invitation from the organizers of the meeting, we took advantage of this opportunity to take part in an informal panel discussion of Woodward, the teacher. Our remarks, as meaningful today as they were at that time, are based on our separate experiences as his graduate students or postdoctoral assistants covering a period of about 25 years. The panel members included Jerome Berson (JB) (Yale University), James Hendrickson (JH) (Brandeis University), Daniel Kemp (DK) (Massachusetts Institute of Technology), Ernest Wenkert (EW) (University of California–San Diego), and Harry Wasserman (HW) (Yale University), moderator. The following discussion, transcribed from a videotape taken at that time, has been edited by the participants.

HW: I began graduate work at Harvard in 1941. Woodward had just been appointed an instructor in the Chemistry Department and during the summer and fall of that year, his first group of graduate students began to work in the laboratory. After one year I left to go into the military service, returning in 1945 to take part in the penicillin project. In 1946 I reentered the graduate school, completing my Ph.D. work in 1948.

EW: I was an undergraduate at the University of Washington in Seattle and was advised to do my graduate work at Harvard by a young assistant professor, Hyp Dauben, who had recently arrived on the scene from Cambridge. I took his advice. When I began my studies at Harvard, I chose to work with the young, magnetic R. B. Woodward. At that time his group included a number of older World War II veterans along with younger students like myself who were still wet behind the ears.

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[†] A biography of Woodward and the participants of the panel discussion follows this transcript.

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JB: I had finished my graduate work in 1949 at Columbia where I worked with Bill Doering, and it's interesting to note how the special quality that Woodward had was already manifest in certain circles at that time. Woodward was only 32 years old and yet Doering was absolutely adamant about his instructions to me to do postdoctoral work with Woodward. He worked very hard at arranging this, and as matters turned out, it was possible to do that, but in order to accept a postdoctoral position with Woodward, I had to turn down an offer of an academic job. This sounds crazy in retrospect. Perhaps it isn't done very often these days, particularly not by someone going to work with a 32-year-old associate professor, which is what Woodward was at that time. Nevertheless, I've never regretted it, and I think Doering's insistence was absolutely correct.

HW: Along the same lines, I would add that when I first came to Harvard as a fresh MIT graduate, I had planned to work with one of the senior members of the staff, Paul Bartlett or Louis Fieser. But Fieser was doing war work, and Bartlett had just taken on a rather large group of students. Bartlett advised me to consider working with a new young faculty member who I found down the hall, supervising the undergraduate organic laboratory. He was lean, intense, rolled his own cigarettes and made organic chemistry sound like a completely new science. In the space of a few hours, he outlined syntheses of cantharidin, quinine and estrone. I immediately decided that he was the person I wanted to work for despite the protestations of my friends at MIT who advised me that I'd never get a job working with an unknown 'genius.'

JH: I had gone out to Caltech as an undergraduate, and had Linus Pauling for a freshman chemistry teacher and I thought he was a very fine teacher, but after that I didn't like many of my teachers. I'd gone to Caltech because I'd seen something about Caltech in *Life* magazine. When I grew up in Toledo, Ohio, that's about all I knew about the outer world. And I'd also seen R. B. Woodward in *Life* magazine because he'd synthesized quinine. I was interested in organic chemistry in high school, but I went four years to Caltech and there wasn't any real organic chemistry at all. I kept thinking, well, where is this quinine? I don't understand this. So then I applied to Harvard for graduate school and went the first fall to Woodward's lectures, and there it was! And I suddenly remembered, it was his picture in *Life* magazine some years ago. So I worked with him as a graduate student from 1950 to 1954. I left Harvard for a year to do a postdoctoral fellowship with Derek Barton in London and came back and did two postdoctoral years with Woodward. So I was actually on three different projects with him over the span of 1951–1957 before going to UCLA to teach.

DK: I arrived at Harvard as a graduate student in 1958. Postdocs at that time carried out Woodward's major research, and his group was very large and dispersed over several floors and buildings. R.B. often discouraged new graduate students, but a number of us persisted in trying to see him. After some months he met with three of us and discussed chemistry for two entire evenings, outlining 12–15 new research problems, and each night giving us nearly four hours of his time. Potential thesis topics ranged from classical natural product studies to a proposal for making amide bonds by orienting amino acids or peptides on electrode surfaces. At the end of the first evening around midnight all of us were unimaginably tired and could barely remember how the session began, but Woodward was perfectly alert and ready to continue. I well remember emerging with the feeling that we had talked about the entire world of chemistry I had known, as well as much that was totally new. It was enormously exhilarating. All of us who witnessed those evenings signed up as new Woodward students.

HW: The sessions that are taking place during this meeting have recognized the many facets of Woodward's contribution to organic chemistry. In this panel, we would like to focus on the impact that Woodward had as a teacher. I am calling on Dan Kemp to start the discussion.

DK: I think we have to start with a paradox. To the irritation of many of his colleagues, Woodward was not interested in classroom teaching, he had no contact with undergraduates, and he taught essentially no formal classes, even at the graduate level. Yet, for many of his associates, he ranked as their greatest teacher. Teaching can move at two levels, and as teachers, most of us are primarily concerned just with information transfer. Woodward largely taught principles and values. He showed us by example and precept that if anything is worth doing, it should be done intelligently, intensely, passionately.

Woodward set very high personal standards for his lectures, and I think his abhorrence for classroom teaching sprang in part from its frequency, which warred with his striving for perfection. All who were fortunate enough to attend one of his infrequent lectures witnessed a masterpiece. Organic chemistry is an architectural subject; it hinges on structure. When he lectured, Woodward hated slides, preferring to draw all of his structures by hand in front of us using a simple blackboard and chalk. Many who first observed him painstakingly drawing complex molecules saw affectation in his patience and care. But as you sat there and watched his structures gradually appear before your eyes, he would often pause with comments that directed your attention to special structural features that were emerging within his drawing. You began to realize how important structures are. How they are the center of our subject of organic chemistry. How they must be taken more seriously than any other concept we deal with. You saw their beauty.

I also remember arguments at his group meetings that hinged on a complicated weighting of many potentially significant effects. One of the messages Woodward underlined was that you start by listing these effects completely. Then you examine each one for its relevance, and assign its weight. If you need two or three hours, fine. You take the time that is needed. If the analysis is worth doing, you cannot be casual, you have to be serious and painstaking.

Intensity was a key part of Woodward's message.

JH: You could feel that intensity while he drew those structures. I think the idea of this being affectation just wasn't it. When he put that chalk on the blackboard, the electricity would come out from it and you could feel every part of that structure. You could feel the relation between the parts, and that just drew you right in. If you were excited about the subject to begin with, it made it more important.

DK: Graduate students and postdocs would practice for hours, learning to draw structures sufficiently carefully to be able to make their debut at Woodward's evening seminar — to go to the blackboard in front of the audience and in front of Woodward himself, to pick up the chalk and to draw a proper structure as an answer to one of the problems he had posed.

HW: I am certain that the beauty of his presentation on the blackboard was due in no small measure to the careful attention to detail which he brought to the presentation. Before he gave a lecture, he took great pains to come very well prepared, even to the point of carrying with him his own kit: colored chalk and white cloth for use as an eraser. It was very deceptive, because when he gave a lecture, it seemed as though the talk evolved as he spoke. In fact, he usually planned every detail of the talk. This of course was representative of the way that he approached almost every problem, leaving no stone unturned.

JB: I would make an observation that perhaps hasn't been stressed that concerns the ability of Woodward to teach in the course of writing a scientific paper. I think if you read the Woodward papers, particularly if you read the footnotes, there's a whole course in organic chemistry in those footnotes. There's one example that I still use in teaching my courses. It concerns the synthesis of quinine, in which one of the last steps, in fact the crucial step, is the introduction of a vinyl group. The nitrite cleavage of that bicyclic compound leads to a precursor of homomeroquinene. The introduction of a vinyl group is accomplished by an elimination reaction. And Woodward goes to great pains to tell you why the whole synthesis was constructed in such a way that the group could be eliminated while it's on the end carbon rather than on the center carbon, because if it had been on the center carbon, the chances were the double bond would have gone the other way around. And that was all in one jewel-like footnote. You just read that and your eyes opened up for the first time. It's a marvelous quality. He did that repeatedly.

DK: For me, wonderful examples of this quality are found in the paper published by Woodward and the Pfizer group that assigns a structure to the antibiotic terramycin. The assignment involves a complex series of deductions, based on chemical degradation evidence and spectroscopic model studies, and the paper is written so that you experience each step of the analysis.

But I also remember an evening seminar in which Woodward assigned a natural product structure that later proved to be incorrect. In this case his structural reasoning was based on preliminary evidence from ongoing chemical degradation studies. Many of his critics felt that Woodward had too much pride to disclose anything but seamless reasoning based on impeccable evidence. In this case he put the learning experience of the audience ahead of perfection. He wanted to show us how he reasoned at an early stage in the deduction process, and he called a special group seminar immediately after one of his work sessions. There was insufficient evidence to assign a local structure to all of the atoms in the molecule, and he dealt with this problem boldly, by postulating that biogenetically the structure was a dimer and thus showed corresponding structural symmetry. Chemical evidence suggested the presence of a urea, but the infrared carbonyl stretching frequency did not support this assignment. However, Woodward noted that the natural site for the urea in his dimeric structure would not allow it to be planar. The night before, he found one of my graduate classmates in lab at 3 am and persuaded him to drop what he was doing and immediately attempt a one-step synthesis of a model for the proposed strained urea. As Woodward told us in his talk, the model spectrum was sitting on his desk at 9 am that morning, and within experimental error it matched the spectrum of the natural product. To do science by Woodward's standards, you had to seize the moment, relying on experimental data drawn from realistic structural models.

HW: On this occasion where there is such a great emphasis on Woodward's contributions to synthetic organic chemistry, one should recognize his enormous impact on the art of structural elucidation. More than almost any other organic chemist in the field, he was able to bring the full power of his intellect to the solution of the most complex structural problems. His students learned chemistry by observing the way his mind worked. He neglected no data, whether it was derived from chemical degradation, or from whatever spectroscopic information was available; everything had to fit. He was willing to throw out conventional models to propose new concepts in order to reconcile the structure with the available facts.

JH: You know, it's a curious thing. There seems to have been a fairly short period in his career in which he actually did classroom teaching. It happens to have been the time I was there, he mostly was doing that, and giving his course on natural products. But the course in the chemistry of natural products as he gave it was always called 'Selected Topics,' and it was always just one subject. He would start with one subject and all of chemistry grew and flowed from that subject. You really learned all of chemistry, not really just the chemistry of quinine or patulin. But there were always exercises in the deduction of structure. Most chemists nowadays know that a structure is not 'deduced' anymore. It's simply produced for you by an X-ray machine, and this whole exercise in the logic of organic chemistry largely dated from him. One of the most exciting parts of organic chemistry in those days, which is no longer available to us, was this building of the logic, the clarity and the brilliance and the excitement of creating a logic of structure deduction just by mental application to some experimental facts; he practically invented that exercise. I think that Robinson used to be very good at it but he guessed, you know, and Robinson guessed two or three times, and when he was right, he'd forget the other two or three times and pick the time when he was right! Woodward did it once and for all, because he went through the entire logic very carefully and made his presentation, and that was in fact the right answer.

DK: Woodward's evening seminar was his classroom, where members of his audience offered solutions to problems he posed. When an incorrect solution was offered, he took great pains to show both its virtues and exactly where it was flawed.

HW: He encouraged participation in the problem-solving. Individuals felt free to go to the board to present their answers. There was no penalty for trying out ideas that might have flaws because he was able to use these exercises as teaching aids.

JH: Woodward made it as comfortable for the presenter to make a mistake as anyone else I've ever known.

DK: He was always extremely fair and courteous, but at the same time, he also always made sure the didactic message came across. Most of our current University teaching contexts follow the mid-19th century German lecture model, but this was a throwback to an older model, the Renaissance workshop. You learned by witnessing a direct, immediate, trenchant analysis of the strengths and weaknesses of solutions that you and your compeers had just proposed.

EW: In fact, one of the rather fascinating things about the seminars was not only the marvelous pedagogic appeal, because there you did your deductive thinking and delineated thoughts, even when you ran the risk of making a fool of yourself. Woodward had a humane touch, the touch which at a later time, certainly served me well in appreciating the scientific world. He made it very, very clear that what he was attacking, or when he was tearing something apart, it was a something, not the someone.

And one of the fascinating things about all of this was that this was a period of time when much of the scientific world had not yet realized that there's a difference between the two objects of any critique. He made it very clear indeed that he was focusing on the chemistry and not on the chemist. And as soon as you appreciated this, which invariably took at least a year or two years of graduate study, you felt more comfortable in making your presentation, and you realized that even though you made mistakes, you did not in fact make a fool of yourself. You learned and the rest of the group learned.

DK: He also had all of the priorities in place. I well remember having dinner with him after I became a Professor myself. I complained about an MIT committee's evaluation earlier that day of an oral exam performance of one of our graduate students. This student proposed an exceptionally original research idea, but foundered miserably on details. R.B.'s characteristic comment was, "But didn't you consider the underlying quality? After all, anyone can make those sorts of superficial mistakes."

HW: The other special aspect of Woodward's work on the structure of organic compounds was the way that he brought artistry into the exercise. When a structure was completed and he wrote it on the blackboard, he was creating a beautiful work of art. As Dan pointed out, he would take great pains to draw the structure carefully, to delineate the three-dimensional quality — as though he were a sculptor. I think that in some way, all of his students were influenced by his appreciation of the aesthetics in organic chemistry.

JB: I think that the business that Jim raised about structure determination being a lost art is certainly true at the professional level. I mean, one doesn't find that most serious organic chemists spend a lot of time now determining structure. But the didactic value of this art is great. In my own group seminars, for example, we still have problems of that kind and we handle them very much in the Woodward spirit.

EW: Well in fact, you know, it's rather amusing that in the field of synthetic organic chemistry, where Woodward's real strength lay, very much the same type of thinking went on and still goes on. He taught us to appreciate the fact that when you have a highly polyfunctionalized molecule and you try to carry out a unique reaction at one spot, the predictability of success would be fairly low. The consequence of this would be that you would get surprises. The identification of any 'undesired' product would call for the same careful analysis.

DK: There was this unique moment in the history of organic chemistry. The mining and rational analysis of a treasure of classically derived structural information for the important natural products that had accumulated in the literature over many decades. The data were clarified by a long series of natural product chemists, and particularly by Robert Robinson in England. But this clarification culminated in Woodward's spectacular structural assignments based on mechanistic reasoning, often with the help of new spectroscopic evidence.

JB: It lasted about, probably from the late 1890s, I would say, from the time of Wagner and the terpene chemists, until about nineteen, oh, fifty-nine or the end of the 1950s. From there on, the subject just became fossilized.

DK: Intellectually and temperamentally, Woodward was a perfect match for the last act of this era. That's what continually amazes me.

JB: The thing I think, Dan, is that there's one of the things that contributed to that, the interest in that subject was the intrinsic interest of the molecules themselves because people recognized that they were significant beyond their chemical structure. They were physiologically active, many of them played a role as examples of the operation of postulated mechanisms of how natural molecules are formed. There was a glamour associated with them that was beyond their mere chemistry. But, you know, the residue of that field now is what is called biosynthetic chemistry, or biosynthesis of natural products, where a lot of the work is now really at the enzymatic level — a really important scientific contribution.

DK: As you well know Jerry, that same period had another glamour. Part of the excitement of the time was generated by the final assignments of the classic natural product structures we have been talking about. But theoretical chemistry was also predicting whole new classes of potentially stable molecules and reactive intermediates, and Woodward was strongly involved with these.

This was the period of the tropylium ion, the boron hydrides, ferrocene, homoaromaticity. There was a structural romanticism in the air. Theoreticians predicted totally unsuspected types of organic molecules, and organic chemists could test the predictions through mechanistic studies and of course through synthesis.

I recall a discussion of the non-classical carbonium ion problem in which R.B. called a number of us into his office and told us: "I have finally seen what the structure of the [2,2,1]7-cycloheptadienyl cation must be." He had averaged all the major classical resonance structures and obtained a benzene that was π-complexed to a methine cation. He was proved wrong of course, but his analysis was revealing. Underlying the structural organic chemistry of the time was a heady dream of bold new developments. We have lost that now.

JH: It's interesting, what we're all probing in a way, is Woodward's teaching, what he conveyed to all of us, or at least turned on what was latent in all of us — a sense of the enormous excitement and artistic satisfaction that chemistry could provide if it were only done in the right way. Somebody said the other day that in the old days, nobody used to think. They'd just go to the bench and work and get results, and then here came Woodward telling you about chemistry, that it was one thing that you did in the laboratory, but it was very important how you thought about all these details and how logic held them all together and how, when you solved it, it was such a beautiful structure that it even took on an aesthetic pleasure which then was reflected in the beautiful drawings on the blackboard. He would start drawing at the upper left-hand corner, put all the structures on the board, and finish at the lower right-hand corner just at the end of the hour. This of course means that he had thought about the communication of all of it very, very carefully in advance.

HW: Along with this, of course, came his intense preoccupation with precision, his unbelievable ability to accommodate all of the details. He constantly amazed his students with his knowledge of the literature and had a memory for all of these facts which he was able to use very effectively.

EW: And one of the most extraordinary things that left a deep impression on me is that through his reading, through his own innate curiosity about things, he drew both upon the theory as well as the experimental practices of very different disciplines. Mechanistic chemistry was not a separate science as it was in many European universities of the day. To him, thermodynamics was a living thing, and kinetics was equally a living thing, even though we might not have used the true *k* rate and might never have run a full blown experiment to get a rate. We knew very well it was hidden in the type of statements that you made and if you were, in fact, not partaking of that type of thought, you would make all types of faux pas. He was a pioneer in using everything we knew about chemistry to construct these complex molecules. He taught us that for a young scientist to become a truly outstanding organic chemist, he or she had to be knowledgeable in all the areas of theoretical and physical organic chemistry — in addition to proficiency in the art of synthesis.

Let me tell you of one experience which illustrates this discussion. During a seminar in which the six electron cyclic transition state was discussed, somebody had written the arrows in "the wrong direction". We spent about half an hour asking, "well, does it matter?" It was a fascinating experience because there was no question that the audience absolutely was biased in the sense that it did matter. And only through that discussion did we begin to understand the formalism of it all and the much more serious significance of what electrons are all about, what bonds are all about. It clearly showed that as a teacher, he was using pedagogic tools, the crutches and the techniques, in order to force the young student into an appreciation of the significance of these concepts.

HW: But as Dan points out, along with his facility in the mechanistic and theoretical aspects of organic chemistry, he never underestimated the importance of the experiment. Over and over again, he pointed out that organic chemistry is an experimental science. He would even go so far as to say, perhaps a little facetiously, "there are no general reactions, every reaction has to be tried on its own."

DK: Woodward's passion for impeccable experimental evidence could lead him to rank it higher than anything else. I asked him in my first graduate year, should I obtain a combustion elemental analysis for all the new compounds I had been making? (At that time I was of course hoping he might say no.) He looked at me characteristically sternly and said, "If you make a new chemical compound, do it the courtesy of full characterization."

Woodward's seminars usually focused on mechanistic questions, not on synthesis, yet he clearly prized synthesis above all other studies in chemistry. A classmate of mine who frequently talked with Woodward once explored this point with him, saying, "I think I now understand why you prefer synthesis. When you take a mixture melting point of two crystalline materials, one natural and one you have made in the laboratory, and it is undepressed, or you find that the two complex spectra are identical, you know you have made it. The theories and the structure could change, but your synthesis stands." Woodward replied, "Exactly. However valuable the theories, time may prove them wrong." He brought to his mechanistic studies his characteristic intellectual rigor and passion for chemistry, but there was always that element of reservation.

JB: There is in every mechanistic study, and yet, that's characteristic of mechanistic study and, take it from a veteran, that no matter how carefully you think you have proven something, there's always somebody with another opinion.

DK: Absolutely.

JB: The mixed melting point doesn't argue. If you've got it, you've got it. I can understand that absolutely, that sense of satisfaction that he must have had about closing the circle. No argument. Yes, that's true. But I think, you know, we can make lists about the qualities that he had and try to identify what the special features were about the way that he thought. But there was something else that made him what he was and made him unique. I remember a particular case that came up in Woodward's seminar in which the speaker had presented impressive evidence on the structure of a new natural product. Well, the seminar was clearly presented and very convincing to almost everyone in the audience. But at the end, Woodward continued to sit in the front row, lost in thought. Finally, he stood up and placed on the blackboard an alternative structure, which was compatible with the evidence that had just been presented, and moreover, was in better accord with biogenetic ideas of the time. I tell you this story not for any trivial purpose. Woodward's structure was in fact correct, as was shown by subsequent experimentation, but the point I'm trying to make here is, you see, that that intensity of application, that feeling that what was going on at that seminar was truly important and merited one's deepest emotional concern...

JH: Rather than just listening, saying "yes,"...

JB: Saying yes, it sounds so very plausible. That is something that was unique to Woodward. That, and something that's even less definable and that is, the feeling that he must have had inside, and I assume that this was not just a stage operation, that he did not, in fact, had not worked it all out beforehand. I assume it was all on the up and up. It doesn't really matter whether he did or not because, even if he had worked it out in advance, perhaps we did learn from the drama of the presentation itself. But nevertheless, at some point he must have said, there's something not quite right. Something that doesn't fit and he pursued that.

JH: And it's important to get it right. If you can't do it well, then don't do it at all.

DK: Let me even comment on that because I can vividly remember that. I was in my first graduate year. In terms of educational content per second, that moment surely had reached some sort of maximum, because what this said to us was, here is this easy route. You follow what the speakers tell you. And that isn't all that easy for a first year graduate student to follow the logic of a talk, but here is a much rockier path, where you have to be thinking, at each point, of alternatives. And that's what the real game is. And sharing the evidence.

HW: The impact of his extensive communication with other scientists may not be evident in his published papers and symposia. Woodward was constantly consulted. He was so interested in organic chemistry that it was easy to discuss one's research with him. And everyone of us probably knows examples of contributions that Woodward made: to structure, to synthesis, to theory — based on informal exchanges of information which he readily proffered — contributions which never received formal acknowledgment.

EW: Actually, he had a typical behavior pattern which, if misunderstood, could be intimidating and which, some have considered to be ego-squelching. And that had to do with the fact that nearly all of his career, he had huge research groups, and the probability was always exceedingly high that no matter what topic you might bring up, he had already done some bit of work in that area. So I remember on many an occasion when I came through Cambridge, he would ask what I was doing at the time, and invariably, there would be an hour chatting back and forth. He would say, "Ernie, wait a moment." And he would go to his outer office where he had huge filing cabinets and he would go through them, and after a while he would produce a file and before long I discovered that 10 years earlier, he already had completed what I had planned. But, what was very interesting about this was that you could usually learn quite a bit about your own chemistry in this way.

DK: I think we can't leave the subject of his seminar without mentioning the other seminar — where it was happening at about the same time, also a tremendous didactic experience of a quite different sort. Saul Winstein had the reputation for being able to memorize the speaker's numbers as they were being presented and to think about them as they were being offered and then to feed them back to the speaker at the end of the talk in the form of questions, and woe to the person who was not sufficiently acquainted with his or her own data to be able to defend themselves. I well remember being told by several people of Winstein saying to a rather distinguished chemist when he asked him a question and the man said, I don't know, he said, well, why don't you know? Those two things together had similar flavors in the sense of the intensity of thought and absolute integrity in terms of pushing the limits of thought and perspective right to the hilt, and they were certainly, I think, very close in terms of their own regard for each other.

HW: I think it's true. Now that you mention Winstein, it should be noted that Woodward was very fond of games and jokes. He loved to tease colleagues like Saul Winstein and Bill Johnson. And, at least in the early days, he was not above teasing his students; he made bets with his coworkers on the outcomes of reactions — he rarely lost these bets. He made games out of chemical problems. His style in writing papers carried this even one step further. Achievements in synthesis were battles to be won.

EW: But all great fun.

HW: Well, they were challenges. His Swiss postdoctorals were "a gallant band of Swiss hurled at the barricades" at a critical time in a complex indole alkaloid synthesis.

EW: That would be the ultimate fun.

HW: He infused his research with a spirit of excitement. He had a wonderful appreciation for the dramatic quality of his presentations. Not only was there a high level of artistry and flair, but there was timing and drama, such that you were always completely engaged listening to his lectures. Along with this we will remember his contributions as a teacher by acknowledging our appreciation of the rigor of his thinking. He had an amazing ability to sort through experimental facts, even misleading facts, to arrive at the correct answer. Some concluding remarks, Jerry?

JB: I would just summarize by saying that he taught by example, and his example was unparalleled. There was no second. That was how things should be done, and we all try the best we can to reach that level.

Biographical notes

July 1999 marks the 20th Anniversary of the death of **R. B. Woodward**, considered by many to be the greatest organic chemist of modern times. The following biography is based on an obituary published in *Tetrahedron* in 1979 by Harry Wasserman and Derek Barton at the time of Woodward's death.

R. B. Woodward was the father of a major school of organic chemistry in which the theory of reaction mechanisms and the latest physical methods were utilized to their fullest capability as guides through the complex stages of natural products synthesis. Few scientists have had such widespread influence in developing the careers of so many investigators. More than 400 graduate and postdoctoral students were associated with him at Harvard and at the Woodward Research Institute in Basel. These chemists, many of whom occupy important positions throughout the world, include some of the most distinguished investigators in the field of organic chemistry.

Among his accomplishments in total synthesis are quinine (1944); patulin (1950); cholesterol (1951); cortisone (1951); lanosterol (1954); strychnine (1954); reserpine (1956); chlorophyll (1960); colchicine (1963); cephalosporin C (1965); vitamin B_{12} (1972, with A. E. Eschenmoser) and prostaglandin F_{2a} (1973).

At a time when X-ray crystallography was not yet fully developed, Woodward performed some masterly feats of structural deduction. He was the first, during the wartime program on penicillin, to argue with logic and clarity for the correct β-lactam structure. His derivation of the correct constitution for the tetracycline antibiotics from a mass of conflicting evidence was an amazing feat of integration of correct (and of misleading!) facts.

Woodward contributed heavily to the field of theoretical organic chemistry, as for example, in the postulation of the Woodward–Hoffmann rules for the conservation of orbital symmetry — certainly one of the most important advances in the entire history of organic chemistry.

He was born 10 April 1917 in Boston, the son of Arthur C. and Margaret Burns Woodward. As a very young man he became interested in organic chemistry and amused himself by devising syntheses for natural products. He liked to say that his synthesis of quinine was planned when he was 12 years old. By the time he graduated from high school, it is said that he possessed a knowledge of chemical reactions that easily surpassed that of college graduates majoring in organic chemistry and indeed of many professors.

At the Massachusetts Institute of Technology, the faculty recognized that he was a unique student and they bent the rules by arranging a special program for him whereby, at age 20, he received both Bachelor's and Doctorate degrees. After an instructorship at the University of Illinois for the summer of 1937, he returned to Cambridge as research assistant to E. P. Kohler just before the latter's death. One year later he became a junior fellow of the Society of Fellows at Harvard, and in January 1941 became an instructor in Harvard's Chemistry Department. This was followed by an assistant professorship in 1944, an associate professorship in 1946 and then full professorship in 1950. In 1953 he became Morris Loeb Professor of Chemistry and, in 1960, Donner Professor of Science.

Besides the Nobel Prize (1965), Woodward was awarded the National Medal of Science in 1964 by President Johnson. He also received a long list of awards and honorary doctorates from universities all over the world. In 1958, with Sir Robert Robinson, he helped in the founding of *Tetrahedron* and *Tetrahedron Letters*, and he served as Co-Chairman of the Editorial Advisory Boards until his death.

Those of us who were privileged to know this great man personally will testify that Woodward was indeed unique. He had a mind like a computer which retained an enormous amount of factual information stored in an instantly integratable form. It was this, and his impeccable capacity for logical thought, that led him first to deductions of structure and then to brilliant and meticulously planned, complex syntheses of natural products.

Harry H. Wasserman

Harry Wasserman[‡] was born in Boston, MA, and received his B.S. degree from MIT in 1941. His graduate study at Harvard under R. B. Woodward was interrupted in 1942 for service in the United States Army Air Force. He returned to Harvard in 1945 to work on the O.S.R.D. penicillin project and resumed graduate work in 1946. In 1948 he accepted an instructorship at Yale University where he has spent his academic career. He is currently Eugene Higgins Professor Emeritus of Chemistry and Senior Research Associate. His research has concentrated on developing synthetic methodology from reactive intermediates such as cyclopropanes, β-lactams, singlet oxygen and, most recently, vicinal polycarbonyls and cyano analogues. The new chemistry discovered in this work has been applied to the synthesis of bioactive natural products.

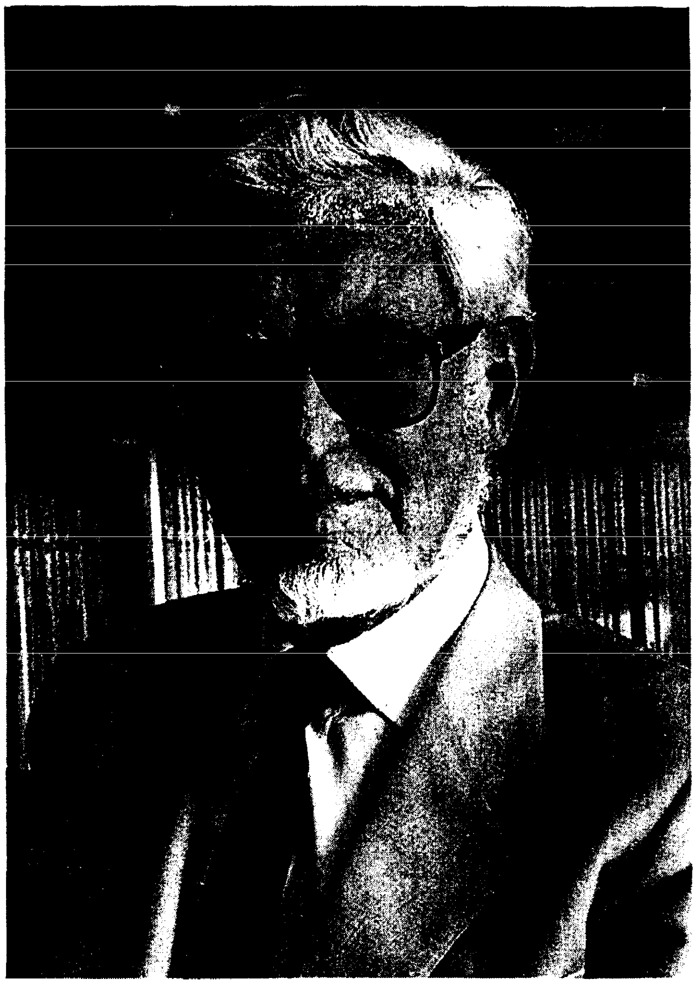
‡ Photograph courtesy of Michael Marsland, Yale University Office of Public Affairs.

Jerome A. Berson

Jerome A. Berson§ was born in Sanford, FL. He was educated in the public schools of Florida and New York, received the degree of B.S. in chemistry from the City College of New York in 1944, and worked briefly at Hoffmann–LaRoche, Inc., in Nutley, NJ. After service in the Army of the United States (1944–1946, China–Burma–India Theater), he returned to the study of chemistry at Columbia University, where he received M.A. (1947) and Ph.D. (1949) degrees, working with W. von E. Doering. Following a postdoctoral year (1949–1950) at Harvard University with R. B. Woodward, Berson held faculty positions successively at the University of Southern California (1950–1963) and the University of Wisconsin (1963–1969). Since 1969, he has been on the faculty of Yale University, where he is now Sterling Professor Emeritus of Chemistry.

His research group has concentrated on the elucidation of the mechanisms of organic reactions and the synthesis of molecules designed to test theoretical concepts. Recently, he has been studying the origins of chemical ideas.

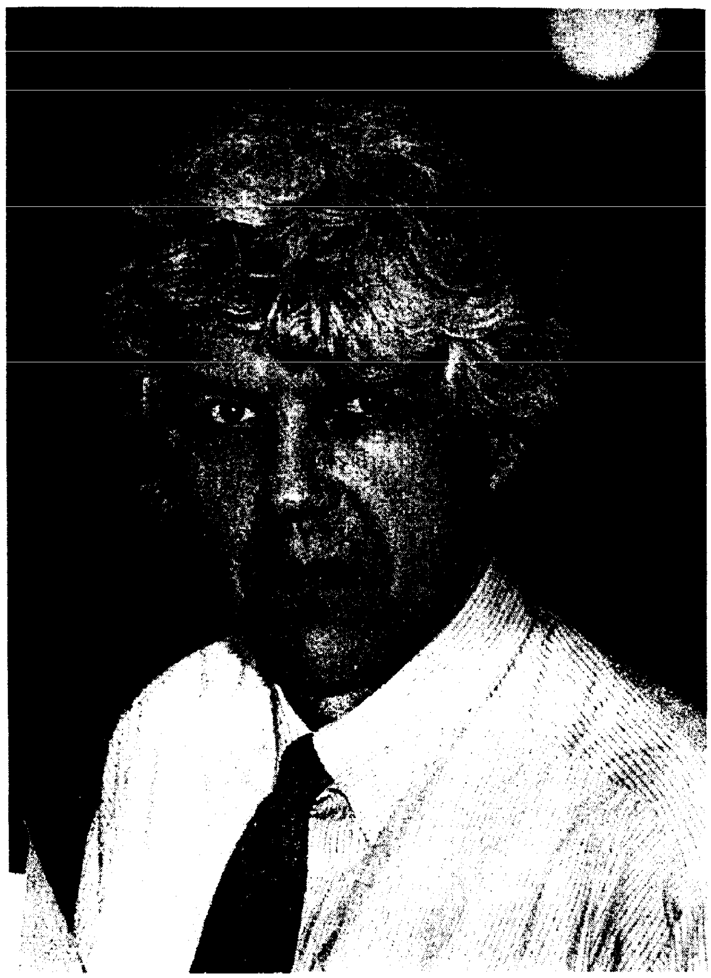
§ Photograph courtesy of Michael Marsland, Yale University Office of Public Affairs.



James B. Hendrickson

James B. Hendrickson[¶] received his B.S. degree from the California Institute of Technology in 1950. He did his graduate work with R. B. Woodward at Harvard University, receiving M.A. (1951) and Ph.D. (1955) degrees. Following a postdoctoral year with D. H. R. Barton at Birbeck College in the University of London, he returned to Harvard for further postdoctoral research with R. B. Woodward. He has held faculty positions at the University of California at Los Angeles (1957–1963) and Brandeis University (1963–present), where he is now the Henry F. Fischbach Professor of Chemistry. He was the first to use the computer for molecular mechanics, and his research has more recently been divided between laboratory syntheses and computer programs for synthesis (mainly SYNGEN and COGNOS).

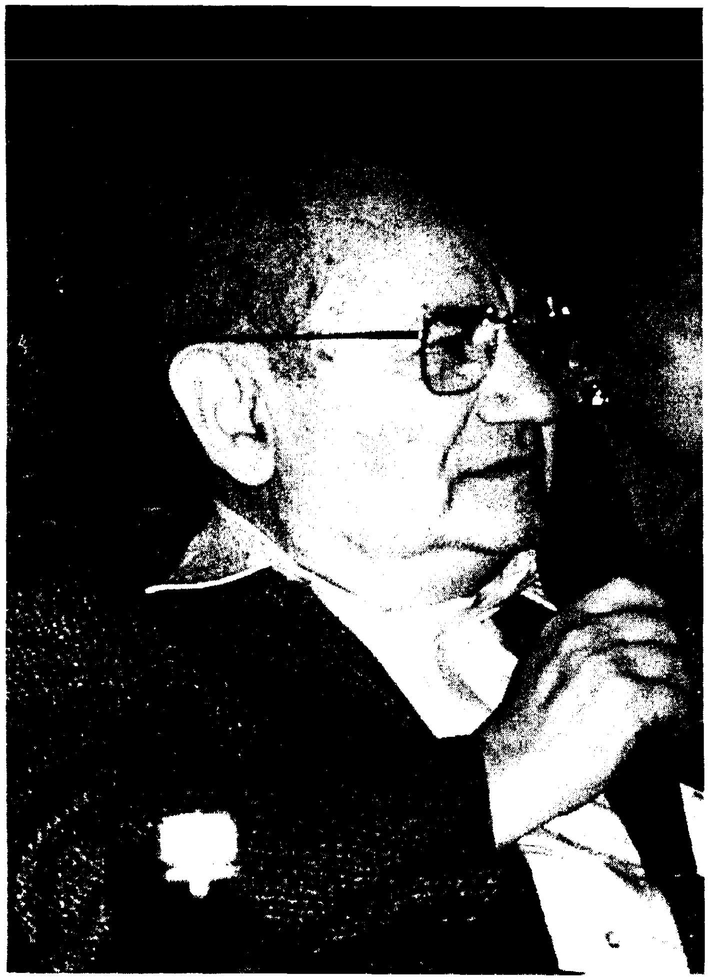
[¶] Photograph courtesy of Brandeis University Photography Department.



Daniel S. Kemp

Daniel S. Kemp was born in Portland. He was educated in public schools in Missoula and Montana. In 1958 he received the B.A. degree in Chemistry from Reed College, Portland, OR, and the Ph.D. degree in Organic Chemistry in 1964 from Harvard University under the supervision of R. B. Woodward. In 1961–1964, he was a member of the Harvard Society of Fellows. In 1964 he joined the faculty of the Massachusetts Institute of Technology, where he is currently a Professor and where he has developed innovative approaches to the teaching of chemistry at the college level.

His scientific research has contributed to the understanding of catalysis and to the design of biomimetic molecules. During the past decade his research group has concentrated on the problem of controlling polypeptide conformation in solution, primarily by developing and applying novel tools for initiating, quantitating, and understanding the formation of the helical structures of polypeptides.



Ernest Wenkert

Ernest Wenkert was born in Vienna, Austria and immigrated to the USA at age 15. He graduated from Garfield High School (Seattle, WA) in 1943 and thereafter attended the University of Washington, receiving B.S. in Chemistry (1945) and M.S. (1947) degrees. After a stint as Instructor of Chemistry (1947–1948) at the Lower Columbia Jr. College (Longview, WA) he joined the R. B. Woodward research group at Harvard University (1948–1951) receiving his Ph.D. degree in 1951.

E.W. held faculty positions at Iowa State University (1951–1961), Indiana University (1961–1969; Herman T. Briscoe Professor, 1969–1973), Rice University (E. D. Butcher Professor, 1974–1980; chairman, 1976–1980) and University of California–San Diego (1980–1994; chairman, 1990–1993). He is now Professor Emeritus at UCSD (1994–present). His research interests include the total synthesis of organic natural products (especially alkaloids and terpenes) and the development of new, general methods of organochemical synthesis.