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A View from the Far Side. Memorable Characters and Interesting Places

David A. Evans

Department of Chemistry & Chemical Biology, Harvard University, Cambridge, MA 02138, USA

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Introduction

It is extremely difficult to predict one's career path as a scientist and educator. I guess it is the self-preservation quality of human nature to unconsciously err on the conservative side in these projections. The projection of the last four decades of my life from Oberlin to Harvard was truly impossible for me to imagine. During this period, I have been associated with some wonderful people who have served as mentors, as teachers, as friends, and as students. I have also been associated with a marvelous collection of educational institutions, each with its own unique character. In reflecting on my own career, I have come to realize that each of us needs mentoring throughout our lives. In the following discussion I have identified some of the important individuals in my professional life who have strongly influenced me in my career. I have also included some stories that will hopefully reveal some of their human qualities. Finally, to the younger members of our profession, you will notice that academic life, particularly hiring practices, have changed significantly over the last three decades.

My Academic Travels

Oberlin (1959-1963). I was fortunate to have the opportunity to go to Oberlin College. My father had died in 1957 while I was a sophomore in high school, and money was scarce in the Evans household. Fortunately Oberlin awarded me a need-based scholarship. As an undergraduate, I had little contact with modern organic synthesis. This was due in large measure to the research strengths of the Chemistry Department at that time which were focused in the general area of experimental physical chemistry. In spite of the fact that the sophomore organic course taught by Professors Peter Hawkins and William Renfrow was terrifically stimulating, I quickly gravitated to the laboratory of Professor Norman Craig whose dynamic personality and exciting research in vibrational spectroscopy provided me with the introduction to research that I had been seeking.

There is an interesting story that is connected with the initiation of my research with Norm Craig. Before I could start my research project, a new vacuum line was needed. In preparation for the glassblowing, all of the components (stopcocks, McLeod gauge, Toepler and diffusion pumps, etc.) were ordered. Unfortunately, the components arrived during the final exam period and Craig was exceedingly busy. After my third visit to his office to see whether the job might be accelerated, he informed me that he would be unable to start work on the vacuum line on the coming Saturday because his wife, Ann, had chores for him at home that included grading the driveway. After leaving Craig's office dejected, I came up with "a deal." Since I was an expert at manual labor but didn't know the first thing about glassblowing, I proposed that I would take care of his driveway thus freeing him up to begin my vacuum line. With "the deal" arranged, I spent Saturday morning taking care of the driveway. Unfortunately, Ann Craig, returned home from her Saturday morning shopping to find what she perceived to be an exploited undergraduate doing her husband's chores with her husband nowhere in sight. Little did she know that I was the exploiter on this occasion! As a postscript, Norm did finish the vacuum line in record time.

My three-year collaboration in the Craig laboratory, that continued throughout the summers, formed the basis of my decision to pursue a career in academic chemistry. The summers of 1961 and 1962 were filled with experimental work and several memorable seminar series. During the summer of 1961 we worked through J. D. Roberts' informative monograph, *Spin Spin Splitting*, and the following summer we tackled Roberts' *Molecular Orbital Calculations*. The Oberlin experience was profound. I wanted to be just like Norm Craig, get a Ph. D. and return to a place like Oberlin to a career of teaching and undergraduate research.

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During my junior year (1962), while visiting the college bookstore, I came across Ernest Eliel's just published monograph, *Stereochemistry of Carbon Compounds*. I purchased and subsequently read "Eliel" in its entirety, and having reflected on its impact on me, it has been the most influential chemical text that I have read to date. This experience convinced me that I wanted to learn more about the organic-physical chemical interface, conformational analysis, reaction mechanisms and reactive intermediates. After some deliberation, I decided to apply to the University of Michigan for graduate school. I was particularly attracted to the research of Martin Stiles on the chemistry of benzyne.

Michigan (1963-1965). I arrived at the University of Michigan in the fall of 1963 and enrolled in the usual battery of courses designed to elevate the entering students to the next level of competence. While I took an intermediate-level course in quantum mechanics that was interesting, the course taught by Richard Lawton and Robert Ireland entitled *Advanced Organic Chemistry* (Chem 540-541) captivated me. Until that point, I did not realize that organic chemists already possessed the sophistication to rationally construct complex structures. It was also clear to me that I had harbored a rather narrow perspective on organic chemistry. A striking example of my naiveté was that I had never been exposed to the Wittig reaction.

Bob Ireland's superb course in *Organic Synthesis* cemented the transition to organic chemistry, and I decided to pursue my Ph. D. in his laboratory. At that time, I was less interested in complex total synthesis than I was in the general issue of reactivity and mechanism. This perspective, no doubt, was derived from my Oberlin experience (the Craig factor). I naively presumed that all good synthetic organic chemists must know the detailed mechanisms of the reactions with which they were working. Hence, in using these reactions, I would also acquire this knowledge through a study of the literature. I now look back and marvel at my lack of knowledge of the field. One of the pivotal Ireland graduate students who both attracted me to the Ireland group and who patiently answered my never-ending stream of questions was Fred Giarrusso. We remain close friends to this day.

Shortly after joining the Ireland research group, a postdoc from New Zealand arrived to share my modest two-person laboratory and three-foot fume hood. His name was Lewis Mander. Along with Norm Craig and Bob Ireland, Lew proved to be my third great "find" in the world of chemical personalities. Lew taught me many things during my three-year association with him both at Michigan and at Caltech. Aside from his enduring friendship, I valued the high standards that he applied to his chemistry. As a student, I hoped that someday I might be half as good a chemist as Mander.

There were several events that still stand out strongly during my time at Michigan. During my first year of graduate school (1963) I heard several memorable lectures presented by distinguished professors, two of which were remarkable demonstrations of the power and versatility of organic chemistry. The first lecture was presented by Professor William von Eggers Doering on the synthesis and properties of bullvalene. The second lecturer was Professor Albert Eschenmoser who, as that year's Bachmann Lecturer, was slated to give a series of seminars on approaches to the synthesis of vitamin B_{12}. Unfortunately, shortly after the first lecture on corrin synthesis, the assassination of John Kennedy terminated the series. Nevertheless, I was now convinced that I had found the right field of chemistry.

Professor Jim Marshall (U. Virginia), then a promising young assistant professor at Northwestern and former Ireland grad student, came down for the Eschenmoser lecture, and I had the opportunity to meet him. I found my discussions with him fascinating. While I could not relate to Bob Ireland and his achievements because of his advanced age (34 years), I could relate to Jim Marshall, who was just then getting his research program underway. Both Jim and I vividly remember our discussion, and to this day I view him as my academic older brother. A word to the young is in order at this point. At this time I was 22 years, 12 years younger than Bob Ireland who was now a full professor at the age of 34. As is invariably the case with young people, I was unable to extrapolate 12 years into the future. I could not conceive of ever accomplishing what Bob had achieved during his life in academia. As Mander observed in proofing this article, "students contemplating careers and life choices need to know that good professors didn't descend from outer space. Rather, they (professors) began where the student presently stands and slowly built their careers."

In my second year of graduate school, I had the good fortune to have a fellowship that also provided the funds to travel to a meeting of my choice. I chose the Organic Symposium that was being held that year in Tempe, Arizona (June 13-17, 1965). While the speakers list was truly impressive (Professors Büchi, Bunnett, Corey, Dauben, Muxfeldt, Roberts, Stiles, Stork, Wasserman, and Wiberg) I was equally impressed with the opportunity to take my first trip on an airplane. While in Tempe I met the late Professor Paul D. Bartlett and had a long discussion with him about Harvard and the possibility of a postdoctoral appointment with him at some point.

Caltech and a Ph. D. (1965-1967). In the early months of 1965 Bob Ireland received and accepted an offer to join the Caltech faculty. Needless to say, the whole group was excited to embark on this new adventure. Bob instructed us to apply to the Caltech program, and three of us students and several other postdocs, including Lew Mander, "made the cut." Bob was quite nurturing during that period. We were now at an institution that viewed the intellectual rigor associated with organic synthesis approximated that found in the fields of sociology or psychology. In short, if you were not measuring quantum yields, determining singlet-triplet gaps, or doing research in nuclear magnetic resonance, "you were out of it." Bob's attitude toward this culture was, "We'll show them!" He was right! Within the year the Hammond boys started to come around with questions like, " How can I make this molecule?" Nevertheless, organic synthesis was a hard sell. I still remember George Hammond's comment at my candidacy exam, "Dave, tell me why you really want to synthesize natural products. Why not synthesize unnatural products instead?". That was a good question.

The Caltech experience was remarkable and entirely complementary to my exposure to chemistry at Michigan. While Michigan had placed an emphasis on synthetic organic chemistry, the Caltech strengths were in physical and physical organic chemistry. I had the privilege of taking a course on *Molecular Orbital Theory* from Professor Jack Roberts, and I was a regular attendee at Professor George Hammond's group meetings where I learned a great deal of photochemistry. Research in the Ireland Group was tremendously stimulating. Bob's capacity for synthesis design was inspiring. He also applied extremely high standards to our experimental work and to our performance on the projects, and those who could not measure up enjoyed a short tenure in the group. Since we had been relative "outsiders", while he never stated as much, I think that he wanted us to out-perform the "Tech boys".

By this time, I had decided that I wanted to pursue a postdoctoral appointment at MIT with Professor George Büchi to learn how to deal with nitrogen as a architectural element. As a terpene chemist with Ireland, I felt that alkaloid synthesis, as practiced in the Büchi laboratory, was an exciting next step. Having set this course of action, I was unprepared for the following encounter with Don Cram.

In March, 1967 Professor Don Cram (UCLA) visited Caltech to give a seminar. While there and in accord with the hiring traditions of this period, Don had asked George Hammond whether there were any students or postdocs with an interest in organic synthesis who might be interested in an academic job at UCLA. Since George had been a member of my thesis committee, and I had been a regular attendee of his group meetings, we had gotten to know one another. In spite of the fact that George never did understand why one could be interested in natural products synthesis, he did pass my name along to Cram, who appeared in my laboratory in the midst of my efforts to work up a very large reaction which could not be ignored. Cram and I had a brief, pleasant conversation while I was wielding a two-liter separatory funnel. The meeting terminated with an invitation to visit UCLA that coming Saturday where I was to give an informal blackboard seminar to the organic faculty (Professors Cram, Winstein, Foote, Anet, and Haake). Since I had already accepted a postdoctoral position in the Büchi laboratory, it did not actually occur to me that this was a job interview. The Saturday seminar was held in Don Cram's office, and it went as well as could be expected considering the research interests of the audience! At the end of the day Cram informed me that it was their intention to offer me an assistant professorship. While I informed Cram that I would think about the proposition, at that moment I had no intention of accepting the offer. I wasn't ready! What about my postdoctoral appointment with Büchi? What about my thesis? After the panic subsided, my wife Sally pointed out that I would probably learn more during the next two years as an assistant professor than I could ever hope to learn as a postdoc anywhere. At that time I had no idea how prophetic that statement would be. One week later I called Don

Cram and accepted the position. During that conversation he did admit that they only really interviewed one person. In response, I told him that we were coherent in that respect since I had only (unknowingly) been interviewed at one place myself!

As a graduate student, one has the obvious mentors, your research directors. Norm Craig and Bob Ireland were, without question, the center of my academic universe. For me, there were also others whose influence was strongly felt through the chemical literature. I read essentially all of the published papers of people like George Büchi, Gilbert Stork, E. J. Corey, and Albert Eschenmoser. These chemists were my heroes and academic role models.

Academia at UCLA (1967-1974). I started writing my thesis on the 6th of June, 1967, and on September 10th, with my thesis accepted, Sally and I loaded up the rental van and moved across town to Westwood. The total cost of the move was $36.50 for which I was reimbursed $18.25. For the record, the UCLA policy was to pay for one half of the moving expenses of its junior faculty irrespective of the distance traveled. No Exceptions!

Laboratory Setup Expenses? As was the custom in those days, I had none! Shortly after arrival, I went to the Vice Chair, Robert Pecsok and nervously asked whether I might purchase an analytical balance and a rotary evaporator. With Pecsok's approval of these crucial items, I was off and running. I recruited four excellent students that year (Carl Bryan, Doug Bond, Tom Crawford, and Bill Scott). We were given an old teaching laboratory and we began our experimental work. On reflecting back on those times, at the age of twenty-six, I was not a very good research mentor. There are a number of humorous stories to recount during that first year. Here is one. I had equipped a small personal laboratory that was adjacent to my office where I carried out my own experiments. One Sunday afternoon about six months into the year, Carl Bryan was in need of a water aspirator since the aspirators in the student lab were corroded. Having decided to "borrow one" from my personal lab, he took a large pipe wrench in to extract the needed item. Now, Carl was a big, powerful fellow, and the results of the aspirator extraction were a disaster. He sheared off a pipe which proceeded to flood the benches, filling the wood drawers with water thus causing them to swell tightly shut. I was not able to open the drawers in that lab for at least a week!

Graduate students in my generation, or any generation for that matter, were not fully prepared for the experience of facing 350 sophomore premedical students and actually trying to teach them something. I did a mediocre job in that first experience and came away with a profound admiration for those senior faculty who could both teach and run a research group at the same time. Dave Lightner, the other junior faculty member in organic chemistry, patiently guided me through this humbling experience. Without Lightner, I do not know if I would have survived. He showed me the ropes! On the other hand, research was progressing along at an encouraging pace. By the end of the first year, my students had gained a measure of control over their hands and my glass breakage bills were starting to subside, I had landed an NIH and a PRF grant, and had at least one idea that was starting to bear fruit.

In 1968 the Vietnam War was escalating and no one that I knew was sympathetic to the cause. At that time I had six students in my group, and all but one, including myself, were in grave danger of being drafted. The lone loss from the Group to the military during that traumatic period was Tom Crawford, who enlisted in the Navy. The disruption in research during that period was catastrophic! It is noteworthy that Tom Crawford did return to graduate school completing his Ph. D. in 1974.

In 1969, Saul Winstein, the pillar of the UCLA Chemistry Department, died at the untimely age of 57. While the loss deeply affected all of us, Don Cram took Saul's passing particularly hard because of the close working relationship that the two had developed over the years. As Don said at one point, "Saul was the innovator and I was the expediter." He was unaccustomed to his new role as both innovator and expediter. This loss precipitated both senior and junior faculty searches. Cram, feeling pretty smug about the ease with which he hired me, instructed Chris Foote to "Go East" and hire some smart young faculty. Chris visited those bastions of chemical sophistication, MIT and Harvard, interviewed a number of prospective candidates, and reported back with his two selections to whom he extended offers. Foote selected Julius Rebek, a Kemp

graduate student from MIT, and Larry Scott, a Woodward graduate student from Harvard. Both individuals accepted without an interview and arrived in July, 1970.

Rebek and Scott were wonderful additions to the Department and my own world. There were now four assistant professors in the organic group. We were all trying to figure out how to launch our respective research programs. We had some wonderful discussions about science, scientists, and personal philosophies. We also had a pretty good faculty touch football team with a little help from graduate student Crawford, who also happened to be the best athlete of the bunch.

Don Cram proved to be very influential, both by personal interaction and by example. It was during this period that Don began to focus on the development of the crown ether field. In a matter of two years or less he redirected his whole research program toward this area. Don never did anything half-heartedly. He was on a mission! I had never seen anyone focus their attention so acutely on a scientific problem before. One piece of advice that he gave me at lunch one day was, "Dave, when you attack a problem, surround it!" In actual fact, one only needed to watch Don in action to come to this very conclusion. Cram assumed a very paternalistic role toward his young colleagues Lightner, Rebek, Scott, and myself. On one occasion, upon learning that we had all gone skiing together and ridden in the same car, he became visibly upset. "What if you had all been killed in an auto accident. The Department would have to hire four new junior faculty!" On another occasion at lunch with Rebek and Scott, he opened his hands (in a disrotatory motion according to Rebek) and informed them, "with these hands I published 14 papers during my first 3 years as an assistant professor." Without question, Don was a motivator! He was also a great friend to me during that period. In 1987 Don Cram won the Nobel Prize! Don began the crown ether work in around 1970 at the age of 51 and two decades later was rewarded.

The next story is one that remains with me to this day. In 1971 the Department initiated a search for a senior faculty replacement for Winstein. This process was begun with a series of Organic staffing meetings. It was clear from the start that Cram had one particular candidate in mind, a well-known organic chemist who was several years older than Don. To Rebek, Scott and myself, this individual, who at that point was more than 30 years our senior, was unacceptable. In the ensuing discussion which gradually escalated in intensity, I volunteered that this individual was "too old and was over the hill." Don took personal offense at this statement, looked me in the eye, and said, "just remember, Dave, old deadwood is better than young deadwood!" In the next breath someone said, "We need a termite inspection," which was followed by another insightful statement, "Hey, maybe we should appoint John Wayne." Rebek, Scott and I still debate who blurted out these memorable remarks. Needless to say, the meeting ended at this point. To this day, during staffing meetings, the "old deadwood is better than young deadwood" quote revisits me. That meeting had the same lasting impression on the others as well.

Concerning the issue of tenure, I adopted a rather simple philosophy. I felt that I knew how to identify quality science, and if I could live up to my own expectations, this hurdle would be passed. I frankly never worried about this issue since there were so many more important objectives such as learning how to run a research group, teach effectively, and think creatively. In 1971 Cram informed me that the department wanted to put me up for tenure. In my opinion, this was premature since I had a number of projects that were nearing completion. I felt that when the organic community sat down to deliberate my case, I did not want to have less than a strong case for promotion. My promotion came the following year.

In 1968 I met Barry Sharpless in his interview at UCLA. He was terribly impressive! His broad range of interests, enthusiasm for chemistry, and personal charm signaled to me that he would be a real winner. Unfortunately, Barry elected to go East to MIT to launch his career. Barry was the first of my "Class of '41" contemporaries that I met. I was soon to meet the others. Barry Trost, then at Wisconsin, Bob Stevens, then at Rice and now deceased, Edwin Vedejs, then at Wisconsin, and Martin Semmelhack at Princeton. A year behind us was John McMurry, then at U. C. Santa Cruz, and Bob Bergman, then at Caltech. This was a very exciting collection of young people and I struggled to measure up to them. In many ways I have viewed members of this group as contemporary role models over the years.

Back to Caltech (1974-1983). In 1974 I received and accepted an offer to join the Caltech Chemistry Department as part of an effort to increase the emphasis on synthetic organic chemistry in the Department. After my arrival I discovered that some of the physical chemistry faculty took a less enlightened view on the value of organic synthesis. The following quote was relayed to me by Bob Ireland from one of the physical chemists. "For God's sake Bob, we already have one synthetic organic chemist, why do we need another!" This is a view that was prevalent during the 60's when physical and physical organic chemistry dominated the department. This was also a view that continually frustrated Bob Ireland in his attempts to strengthen organic chemistry.

While the organic faculty was small relative to UCLA, the research programs of Professors Bergman, Roberts, and Ireland were both diverse and intellectually stimulating. In addition, the recent addition of Peter Dervan, whom UCLA had also strongly courted, simply added to the attractiveness of the move. The relative sizes of the two institutions was also a positive point for Caltech, whose undergraduate population numbered no more than 800 compared to 20,000 at UCLA. In short, it was more like Oberlin, both in size and style. As a student at both Oberlin and Caltech, I was quite accustomed to taking examinations under the honor system. I also felt more comfortable in a small private institution. Obviously, these are highly personal choices since two years later Bob Bergman was to make the opposite decision in electing to move to Berkeley. Sadly, while I was deliberating my move to Caltech, I jokingly asked Bergman to sign a blood oath that he would never leave Caltech. As they say, one needs to get these things in writing!

The move to Caltech in the summer of 1974 was conveniently made while I was in Europe, and my wife, Sally, did not let me forget that! On this occasion, *Caltech paid for all of our moving expenses*, including the relocation of my motorcycle. On this latter point Sally suggested that I should give up the beard and the motorcycle since I was starting to look like a Hell's Angel and was now the father of a three-year old daughter. Sally was not impressed with the combination. As always, she won out. Both the beard and the cycle went shortly thereafter.

My first teaching assignment upon arrival in Pasadena was an experimental course to a group of about 30 freshmen who had essentially had the equivalent of one year of college chemistry. My instructions were, "Don't worry Dave, you can't ruin them!" I was to teach structure, bonding, kinetics, and mechanism hung on an organic framework during the first two quarters, while Bob Stroud (chemical physics) was to introduce them to thermodynamics and quantum mechanics in the third term. The course was a great success! There were two memorable students in that class. The first was Mike Steigerwald who is now located at Lucent Technologies. The other was Peter Schultz, now somewhere between U. C. Berkeley and Scripps. In those days Pete was a very reserved fellow. Shortly thereafter, Pete inquired about the possibility of carrying out undergraduate research in my laboratory. Due to the fact that my lab was exceedingly crowded at that time, I suggested that he talk to Peter Dervan. The rest is history!

My interactions with Peter Dervan during that period were truly memorable. Like all young academicians, Peter, who was now in his second year at Caltech, was vigorously searching for his research identity, and I, at the ripe old age of 33, was his sounding board. One day Peter would come into my office and propose a terpene synthesis. Several days later he would come in with an idea on a new transformation involving silicon. The nucleic acid studies were initiated in his third year with the simple statement, "Dave, everyone has been missing the boat in the host-guest area. DNA is the ultimate host!" I should also interject at this point that Don Cram had also had a very strong influence on Peter at that time.

As young people, Peter and I had many conversations about our respective ten-year research plans. By this time I was convinced that I could control the absolute stereochemistry of carbon-carbon bond-forming reactions. I was going to demonstrate that many important classes of natural products, such as macrolides, could be constructed as one assembles a peptide using iterative chiral enolate bond constructions. Peter's ten-year plan was to design a small-molecule restriction enzyme mimic that would cleave DNA at predetermined base pair sequences. In looking back on these dreams, they both came true! In 1979 Dervan was promoted.

The loss of Bob Bergman to U. C. Berkeley in 1977 was tragic. Bob was the one of the principal reasons that I had come to Caltech. In my opinion, he was going to be the "young Winstein" of our generation. The reasons that precipitated Bergman's departure were rather complicated and should be left for Bob to elaborate upon at the proper time; however, one thing did constantly irritate him. The roof in his laboratory continually leaked and nobody would fix it. I remember Jack Roberts saying at one point, "Bob, you are making too much out of the problem, it only rains about three times a year!" Bergman's loss stimulated us to canvass for a replacement. The ensuing search resulted in the identification of Bob Grubbs who was, at that time, a member of the faculty at Michigan State. As the record now shows, this proved to be an outstanding appointment. I view Grubbs as one of the most creative chemists in my generation. It should be noted in passing that Grubbs inherited Bergman's lab with the leaky roof.

In December of 1982 I received a note from my secretary that a man by the name of Rosovsky from Harvard had called. At the time, I thought that it was probably someone calling about a postdoctoral appointment. When I returned the call the next day, I found that the Rosovsky call had been placed from the Dean's Office and that Rosovsky was in fact the Dean of the Faculty of Arts and Sciences at Harvard and not a graduate student! The courtship was extremely difficult because of my ties to Caltech. When I met with Murph Goldberger, then President of Caltech, he asked me what it would take to keep me. I proceeded to describe the plight of the laboratories that I and my colleagues inhabited. They were really deplorable! The labs were overcrowded, old, and the roof leaked (*vide supra*). I did not want my labs renovated unless, at the very least, the Grubbs Dervan, and Ireland labs would also be renovated. Murph didn't buy the proposal so I left. One month before I was to depart and three months after I had accepted the position at Harvard, the renovation a renovation plan very close to my proposal was approved. Thus was born the Arnold and Mabel Beckman Laboratory of Chemical Synthesis (minus one synthetic organic chemist). Leaving Caltech was very difficult for me, but Bob Grubbs' roof was finally fixed!

On to Harvard (Since 1983). We packed up the laboratory in late-July, 1983, and headed East. One of the most difficult moments in a chemist's life is seeing his whole laboratory reduced to a pile of boxes! A collection of 16 students and postdocs made the trip. The moving van pulled into the Converse Laboratory parking lot at 12 Oxford Street, where we unloaded the equipment, and moved into temporary labs recently vacated by the Corey Group in the basement of Converse. My sympathies go out to those generations of Corey students who lived there! We reassembled the lab and were doing chemistry by the third week of September thanks to people like Don Ciappenelli and Jerry Connors and a crew of hard-working students. One year later, we moved into our permanent facilities. The contrast between Harvard and Caltech was striking. Organic chemistry was now the center of the universe! My organic colleagues at that time (E. J Corey, Yoshi Kishi, Bill Doering, Frank Westheimer, Konrad Block, Jeremy Knowles, and George Whitesides) made me and Sally feel quite at home. I guess this will be my last move!

I have a myriad of memories from the last fifteen years. The first memory that reminded me why I came to Harvard was that day in 1984 when Jon Ellman (U. C. Berkeley) and Erick Carreira (Caltech→ETH) walked into my office together and announced that they would like to join my group. Naturally, they both looked like reasonable people to me so I accepted them. Shortly thereafter, Tom Britton and Steve Kaldor (now both at Eli Lilly) joined as well. What an amazing group of people! The following year, Greg Fu (MIT) joined the group as did postdocs, Scott Rychnovsky (U. C. Irvine) and Amir Hoveyda (Boston College). In the subsequent year and Keith Woerpel (U. C. Irvine) showed up. It has been like that ever since.

I have never witnessed a more dedicated collection of faculty. When I drive into the parking lot on Saturday mornings, Yoshi Kishi and Bill Lipscomb are already here! Recently, Bill informed me if he could have his way, he would retire promptly at the age of 85. Bill Doering, Frank Westheimer, and Konrad Bloch all come to work every day and remain engaged with departmental decision making. Last year, at the end of my three-year tour of duty as the Department Chair, these wonderful gentlemen and their wives took Sally and me out to dinner as a gesture of their appreciation. It has been a privilege to be included among them.

My Coworkers

Without question, my greatest resource as an academic chemist has been my graduate student and postdoctoral coworkers. They have also been my greatest pleasure. Nothing has been more satisfying than to witness a young person gain the skills necessary to succeed in the next step of his or her career. I have been associated with 200 co-workers, half of whom have been graduate students. I keep asking myself, am I doing as good a job with their training as Norm Craig would have done? I hope so. They are wonderful people. The privilege of my life has been my association with them.

My Life with Sally

Sally Welliver and I went to high school together. We were actually in the same physics class. I was introduced to her in 1957 by a mutual friend. Her world and mine barely intersected in high school. Her group of friends was very studious. She belonged to the literary magazine staff, sang in the choir, belonged to the science club and other activities of that sort. I basically was interested in football and track. In the Summer of 1959 I heard that she was also going to Oberlin College, so I asked her out. The strategy was to become acquainted with at least one woman when I arrived on campus. Well, that was that! We were married December, 27, 1962. In 1971 our daughter, Bethan, was born, and in 1998 we became grandparents. I have known Sally for 40 years and we have been married for nearly 37 of those years. She is also my best friend, confidant, and boating partner. She has been by my side through all of the moves and all of our adventures. This year, having taught high school mathematics, chemistry, physics, earth science, and physical science for 35 years she retired (quit!) and is now working with a number of local charitable organizations. As my most honest critic, she cautioned me not to submit this manuscript.

Epilogue

I have found that the integrated exercise of organic synthesis and new reaction development captivating. These activities are crucial to the continued development of our field. I have had a wonderful time as an academic chemist. It has been filled with inspiring individuals. My chemical life began with Norm Craig at Oberlin who I still view as the consummate educator. Through my association with him I acquired a life-long interest in mechanism and reaction development. With Bob Ireland I discovered that organic chemistry really is the center of the universe! I've met some wonderful young academic chemists (Lew Mander, Jules Rebek, Larry Scott, Chris Foote, Dave Lightner, Peter Dervan, Bob Bergman, Bob Grubbs) along the way who have influenced me greatly. I now have a collection of senior colleagues (Konrad Bloch, E. J. Corey, Bill Doering, Yoshi Kishi, George Whitesides, and Frank Westheimer) who inspire me to improve and who act as my role models. I also have a collection of younger ones who, hired in accord with one of Frank Westheimer's principles, are all smarter than I (Stuart Schreiber, Eric Jacobsen, Andy Myers, Greg Verdine). Harvard is not a bad place to find inspiring scientists! I look forward to the next decade with much enthusiasm. I have never enjoyed my chemistry more. This is a good time to be a synthetic organic chemist.

Publications of David A. Evans

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