Hanna Reisler: Autobiographical Notes

On July 13, 2017 I delivered the Herschbach Prize lecture at the Dynamics of Molecular Collisions Conference. The audience included colleagues and friends, and a large number of students and postdocs. Being the first woman to receive this Prize, for which I am immensely grateful, I decided to aim my talk at the young people in the audience, many of them women, and tell them about my long and sometimes tortuous journey in life and science. My goal was to illustrate that even scientists that they perceive as accomplished and self-confident had started just like them, sometimes stumbling along the way. To my surprise, not only the students but also some of my senior colleagues found my talk inspiring and asked me to write it down. The autobiographical notes below are based on that talk. For those interested, the original talk, which includes figures and photos, is posted as a link in my group website (http://chem.usc.edu/~reisler_group/assets/pdf/DMC17_lecture.pdf).

I realize that, like everybody else, I am a product of my time and place. I grew up in the then young state of Israel at a time when there was strong emphasis on education. The many holocaust survivors who arrived at that time realized that education was the only thing that could not be taken away from them. Both men and women were encouraged to get their education early and become self-reliant. The country was poor, everybody had to work, and working in a science field was not considered unusual for a woman. Why did I choose Chemistry? Frankly, I did not want to be a teacher, which would be my fate had I chosen a humanities or social sciences major. As a chemist I thought that I would simply work in a lab. Only later did I discover that teaching and mentoring were also tremendously rewarding.

In these notes I focus first on my long and winding road to a faculty position. I then describe my early research, which has launched some of the themes that I still find exciting. I write less about my current research because it is readily available in print and is presented at conferences. I will instead comment on being a woman in science, and describe the WiSE Program at USC, which is aimed at increasing the representation of women in STEM fields. I end with comments that I addressed to the junior members of the audience.

Education

I enrolled at the Hebrew University of Jerusalem as a Chemistry major after receiving excellent liberal arts education at my high school, which unfortunately did not include high-level science classes. Actually, I was supposed to complete my military service first (compulsory for women in Israel), but to my surprise, in spite of my poor science training, I was accepted to a competitive program in which science and engineering students could go to university before doing their military service. Thus, my plan of completing the science high-school curriculum during my military service was thwarted by the capricious nature of military decisions. Apparently, all I needed was to pass the admission test for officer training course.

At university I discovered how poorly prepared I was, and that no remedial classes in math and science were offered to students like me. My first year was indeed difficult, but fortunately my classmates helped me catch up. By the end of the academic year I managed to pass my final exams, and even complete successfully the obligatory officer-training course in the summer. I was in!

One of my classmates, Emil, was especially helpful during that time, and after a while we discovered that, in fact, we liked each other enough to want to spend our life together. Thus, dual career issues accompanied me from the start, and my life and science were always intertwined. While women were fully welcome to study and work in science, it was also self-evident at that time that the husband's career comes first. In fact, my women friends and I never even posed the question of whether this is how it should be.

After receiving my BS degree, I enrolled for MS in synthetic organic chemistry (The MS degree was mandatory before pursuing a Ph.D.). I chose organic chemistry because I was good at it as an undergraduate, and I did not yet feel confident about my math and physics skills. My MS work involved expansion of aromatic compounds by reactions with carbenes, and in order to understand better the reaction mechanisms, I audited a couple of physical chemistry courses. My advisor eventually recommended that I pursue a Ph.D. in physical chemistry, because I appeared more interested in how reactions transpire than in making molecules. This was important advice, and to this day I tell my students to “listen to their brain” when making career choices. Luckily, there are many different ways to pursue excellent science.

While I was eager to pursue a Ph.D., I first had to complete my military service. Now the dual-career issue became real. Emil had completed his military service before college and already started his Ph.D. studies at the Weizmann Institute, and I did not want to fall too much behind. I discovered that the Soreq Nuclear Research Center accepted science graduates for military service. I applied to Professor Michael Anbar, a physical chemist, and he was willing to accept me and even let me start my thesis research. This was fortunate, because I was already married, and Professor Anbar had a lab at the Weizmann Institute.

The topic of my project was the temperature dependence of reactions of solvated electrons and their activation energies. The work was published as a JACS Communication in 1967, and was my first paper. As it turned out (see below) this was also my last paper in this field, but a couple of years ago, while I was chairing a session at a Gordon Conference, Dave Bartels gave a talk on this topic. Much to my surprise, he started by paying homage to Prof. Anbar as a pioneer in the study of solvated electron reactions, highlighting our 1967 paper. At the end of the talk, I asked whether the results of this paper were still valid because they had been obtained by an indirect and now obsolete method. Happily, his answer was that most of the rates were correct as was the conclusion of the paper. Only then I revealed that Hanna Bregman-Reisler, the last author of the paper, was I before I dropped my maiden name. I pointed to the students in the audience another lesson: papers published before they were born could still be valuable!

I was happy with my work with Prof. Anbar and agreed to extend my military service by one year in order to pursue my Ph.D. degree under his guidance but, unfortunately, shortly afterward he moved to the U.S. and I was left at Soreq without an advisor. Tragically, my second supervisor was killed in the Six Day War in June, 1967 shortly after I was transferred to him. I was then transferred to the Soreq Nuclear Chemistry Department, headed by Prof. Saadia Amiel, who agreed to be my Ph.D. advisor and allowed me to enroll at the Feinberg Graduate School of the Weizmann Institute of Science.

Professor Amiel’s specialty was in what was then called “hot atom reactions”, an area pioneered by Richard Wolfgang in the US. However, Prof. Amiel informed me that Prof. Wolfgang had told him that the new way of studying the kinetic energy dependence of chemical reactions of ions was by crossed beam experiments with tandem mass spectrometers, like the EVA machine he had built at Yale. Prof. Amiel decided that my Ph.D. project would be to design and build such a machine. Luckily, my guidance committee at the Weizmann Institute did not approve of this ambitious project, because Soreq did not have even a single mass spectrometer at that time. I was told to come up with a thesis project that used existing equipment. What did I have? I had a 6” and a 4” diffusion pumps. I also had a couple of surplus high-voltage power supplies (made with vacuum tubes) brought from Brookhaven National Labs by a visitor who wanted to help the (then) poor state of Israel. He brought also the RF power supply of a quadruple mass spectrometer. Regrettably, the mass spectrometer itself had to stay at Brookhaven, but

I was assured that I could build one myself by buying the rods and machining them, which I eventually did.

I scoured the literature searching for a way to study ion-molecule reactions with the equipment that I had, and discovered the work of Zdeněk Herman, an outstanding scientist in Czechoslovakia, another country that was poor. By a clever use of electron impact ionization and ion optics, he was able to study charge transfer reactions of atomic ions with neutral molecules with a single mass spectrometer. This gave me an idea on how to configure the ion optics, and using my two diffusion pumps I was even able to implement differential pumping. I exploited the fact that negative electrons and positive ions go in different directions to create molecular ions in one chamber and collide them with neutral molecules in the other, and succeeded in measuring relative charge-transfer cross sections as a function of ion kinetic energy. To make sure that the electron impact ion source produced only molecular ions, I “invented” slow electron impact ionization decades before Piero Casavecchia exploited this method in his molecular beam studies. Coming from a poor lab, however, I was embarrassed to admit that this was what I had to do to obtain results. Zdeněk collaborated with several American scientists, and many years later I met him at a party for Carl Lineberger and thanked him personally for his help in launching my Ph.D. research.

There was nobody else at Soreq working on ion-molecule reactions, but I was able to enlist the help of a staff chemical engineer who made sure that I did not electrocute myself with high voltage. I also met a group of accomplished senior women who served as my mentors and remained my life-long friends. They all combined careers and family and were excellent role models. Because I was isolated scientifically I tried to meet foreign scientists who visited Israel. One of them was Aaron Kupermann from Caltech whose course on molecular scattering at the Weizmann Institute I audited. I kept the notes from his excellent lectures for many years. The benefit of being isolated, though, was that I learned early on to be independent.

I worked diligently on my experiments and was able to graduate not too far behind Emil. At the end of my military service I accepted a permanent position at Soreq, and was allowed to take a two-year leave for postdoctoral training. Emil was offered a tenure-track position at the Weizmann Institute and got a postodctoral fellowship to the Johns Hopkins University. As an employee of the Israel Atomic Energy Commission, I was able to get a small fellowship from the International Atomic Energy Agency for scientists from underdeveloped countries. Considering the type of equipment I had at Soreq, I thought that I was eminently eligible for such a fellowship. With this fellowship in hand, Professor John Doering offered me a postdoc position at the Chemistry Department of the Johns Hopkins University. And so, in January 1972, right after I submitted my thesis, I arrived in Baltimore.

I had not met my postdoc advisor before, so when I arrived, I introduced myself to the person in the lab who looked the most distinguished; he wore a jacket and had grey hair. He was actually a postdoc in the group, whereas John Doering, who was only 34 years old (though already a full professor), looked very young indeed. I worked on electronic and vibrational excitation in ion beams collisions with neutral molecules, and I spent months analyzing the spectral data printed on the strip chart recorder. My only complaint was that John took the group every day at 10 am to the cafeteria to drink Dr. Pepper, which I did not like. He always caught me when I tried to cheat and order Pepsi.

Being a postdoc in the US was a wonderful experience. Emil and I both had good jobs to go back to in Israel at a time when most of our American colleagues had difficulty finding academic positions. I had a hard time at first understanding John’s Texas drawl, but I got used to different American accents by listening to the Watergate hearings, in particular the Southern accent of Senator Sam Ervin of North Carolina, and I learned American slang from “All in the Family” (e.g. dingbat, meathead, and more). We also traveled extensively both as tourists and also to visit labs that were doing sophisticated crossed-beam experiments. I enjoyed working with state-of-the-art equipment for the first time, and exploiting spectroscopy to decipher reaction mechanisms. John was a fantastic experimentalist, and I learned from him to always strive to get the best data possible; interpretations may change but the experimental results should hold! What I regretted was not being allowed to travel to Churchill, Canada, to observe the Aurora Borealis and be part of an electron spectrometer experiment in a rocket launch supported by NASA. John said that the guys drink too much while waiting for the aurora, and that was no place for a lady.

Early Career

When we came back to Israel in 1975, Soreq appointed me as group leader of a new chemical lasers project based on the Cl + HI reaction. However, the situation for Emil changed when tenure-track positions at the Weizmann Institute were abolished. When he got an excellent offer from the newly founded Molecular Biology Institute at UCLA, we decided, after much soul searching, to go back to the US. Los Angeles seemed like a good choice because it had more than one institution of higher learning, which increased my chances to get a position.

This was a difficult transition for me because our son was only 15 months old, and we had no family or other support system in LA. Also, the Soreq Nuclear Research Center was virtually unknown in the US, and it would have been hard for me to get a permanent job. I decided that my best option was to take another postdoc position and start afresh. This time I applied to a person that I did see: I saw Curt Wittig at the Quantum Electronics Conference in Amsterdam in 1976. What impressed me the most about him were his insightful comments and questions after each talk, delivered in his inimitable animated style. He was still an assistant professor but his research program was ambitious and exciting. To my delight he accepted me in spite of my humble beginnings, and so in April 1977 I started as a postdoc in his lab, which was then in the EE Department of USC. The lab was, indeed, an exciting place. The students and postdocs worked very hard; there was a night shift and a day shift because we had to share equipment, and I would typically arrive early in the morning and send the night shift home. There was an air of urgency and vibrancy, and new projects were initiated often. What I admired in Curt was his creativity and ability to think outside the box. He also taught me to always seek the physical meaning behind equations, while still following all the derivations.

My first project was E-V energy transfer from Br* to selected molecules, and after a year or so, Curt offered me a Research Assistant Professor position. The area of laser-initiated chemistry had just begun, and we soon switched to study bimolecular reactions of radicals such as C_2, CN and C_2H produced by IRMPD. I was happy because I always enjoyed working the lab; there is nothing comparable to the exhilaration of seeing the first signal after a long struggle! The hardest thing for me was leaving at a set time each day because of childcare constraints. I never got used to this. On the other hand, the time management and organizational skills that I developed then served me well later, e.g. when I was Department Chair.

The years I spent in Curt's group were exciting and inspiring, and they formed the foundation on which I built later on. At that time state-to-state studies of chemical reactions were at their infancy. We didn't even have a commercial tunable dye laser and built our own. Yet, it was the first time that we could perform state-specific measurements and test the underlying assumptions of theories. The experiments I participated in involved unimolecular reactions on the ground state as well as direct photodissociation on excited states. The field was advancing rapidly and there was a lot to learn. I was grateful to Curt for creating study groups to go over the theoretical foundations of our research. Below I describe briefly two representative examples.

To test the validity and limitations of statistical theories of unimolecular decomposition, such as Phase Space Theory (PST), we needed a molecule that could serve as a good test case. NCNO turned out to be such a molecule because we could detect both CN and NO by LIF. However, it was explosive, and we invited Joe Pfab from Herriot Watt University to teach us how to synthesize it without blowing ourselves up. He brought with him his own lab coat, which he believed would protect him from shards of broken glass in case of explosion. Luckily, this did not happen. We were fortunate to have Israel Nadler-Niv, Marcus Noble, and Charles Qian working on this project. We succeeded in detecting products right at threshold, and found that they were ultra-cold. We obtained excellent agreement with PST for the rotational distributions of the CN and NO products at relatively low excess energies. We also identified signatures of tightening of the transition state at higher energies, and Curt developed the Separate Statistical Ensemble (SSE) method to describe product vibrational distributions. Other groups focused on state-specific experimental and theoretical aspects of unimolecular reactions at the same time, e.g. Brad Moore and Bill Miller at Berkeley, Ahmed Zewail and Rudy Marcus at Caltech, and more. These were indeed exciting times!

At the other extreme, we studied the direct photodissociation of ICN on coupled potential energy surfaces. We used Doppler profiles to determine changes in the recoil anisotropy parameter as a function of CN(J) and deconvoluted the rotational distributions associated with the I and I* product channels. Other groups followed, e.g., Zare, Zewail, Houston, and finally Keiji Morokuma and his coworkers calculated the couplings between states required to explain the product state distributions.

Getting a Tenured Faculty Position

Time flew by, and in 1986 I realized that if I was ever going to make the transition to an independent tenure-track position this was my last chance. It was not my intention to apply to USC, because research universities simply did not hire their soft money faculty – not then and rarely even now. In fact, Emil and I planned to go on a national search for two positions. However, Curt encouraged me to apply also to USC, even though I thought that my chances were slim. You can read the detailed story of my appointment in Curt's excellent autobiography, which is posted on his website (https://www.curtwittig.com/wp-content/uploads/curt-wittig-autobio.pdf). In the end, the USC Chemistry Department voted to appoint me as a tenured Associate Professor. The Dean, however, was not as supportive; I got no setup funds, no space, and my salary was cut. Nevertheless owing to the generosity of Curt who gave me the equipment I worked with and other resources, the department that arranged space for me, and the existence of our ARO-funded Center for Fast Transient Processes (directed by Curt), I was able to launch my independent career in the fall of 1987. I thank Bob Shaw, our ARO program officer, for supporting us for 11 years, providing equipment money and continuity. I am also indebted to my Chemistry colleagues and have always tried to be a good citizen and contribute to my department and university.

I continued to work on state-to-state reaction dynamics, because this was the golden age of gas-phase dynamics studies, and there were many great advances in both experiment and theory. I defined my research at that time as the spectroscopy and dynamics of fast evolving states. Our goal was to identify properties that are sensitive to the shape of the potential energy surface and characterize them by using prototype molecules. This approach afforded textbook examples of statistical unimolecular reactions on the ground state (e.g. NCNO and NO_2), and photodissociation on excited states (e.g. FNO, ClNO). This also started my long-term and fruitful collaboration with theoreticians, first with Reinhard Schinke, Moshe Shapiro, and Bill Miller, and later with Anna Krylov, Joel Bowman, Stephen Klippenstein, David Yarkony, Hua Guo and others. On the experimental front, the photofragment imaging technique was a fundamental advance that changed qualitatively the way we study photodissociation dynamics (see below).

Representative examples of this work were the studies of the photodissociation dynamics of ClNO and FNO. In the dissociation of ClNO on the first triplet state, which exhibited diffuse structures identified as bending levels, we observed how wavefunctions in the excited state map onto NO product rotational distributions, exhibiting the nodes in the bending wavefunctions. My then postdoc, Charles Qian, was able to model this mapping semi-quantitatively by expanding bending wave functions of an anharmonic oscillator in angular momentum basis functions. Reinhard Schinke and his coworkers then calculated the wavepacket dynamics and reproduced well the absorption spectrum and the NO rotational state distributions. The photodissociation of FNO showed striking interference effects. It turned out that the electronic excitation accessed the transition state region and we observed interference between the directly outgoing wavepacket and the part that had brief recurrences into the bound region. The signature of such interference was irregular lineshapes known as Fano profiles (see the cover of this issue). These lineshapes were very sensitive to the shape of the potential energy surface, as was demonstrated in collaborative studies with Reinhard Schinke and Moshe Shapiro. These findings are highlighted in the monograph "Photodissociation Dynamics" by Reinhard Schinke. Surprisingly, they were relevant also to our studies of the unimolecular reaction of NO_2 on the ground electronic state.

Just when we thought that we understood statistical energy distributions in unimolecular decay, NO_2 came along to challenge us. It was supposed to serve as another excellent test case for statistical theories, because it exhibited complete IVR on the ground electronic state. Indeed, Curt Wittig measured the near-threshold unimolecular reaction rates and they agreed with the prediction of statistical theories. In our group, we measured the NO product state distributions, and here we had a completely unexpected result. Instead of the smooth rotational state distributions that we observed with NCNO, the distributions showed large state-to-state fluctuations, which increased when the initially excited state was selected in IR-UV double resonance experiments. In a complementary experiment, we observed random but reproducible fluctuations in the action spectra; each monitored rotational level led to a different excitation spectrum (see the cover of this issue). These observations perplexed us until my USC colleague, Howard Taylor, pointed out to us the existence of Erickson Fluctuations in nuclear reactions, which were interpreted as interferences due to overlapping resonances. We started reading books and articles on nuclear reactions, including papers by Niels Bohr, and we borrowed concepts of mapping and interference from our work on FNO and ClNO. I then spent a Sabbatical leave at Berkeley, and with Bill Miller and his then postdoc Uri Peskin (now a professor at the Technion in Israel), we developed a random matrix model, which combined mapping of overlapping transition state wavefunctions (resonances) with interference with random phases. This model could explain all of our observations. Indeed, the value of transporting concepts from one research area to another cannot be overstated.

I was very fortunate that Scott Reid and Andrei Sanov were in our group at that time. They used the random matrix model with a random number generator to select the phases, and were able to generate fluctuating NO rotational state distributions, very similar to the observed ones. Moreover, by summing several random distributions obtained for a specific excess energy, they recovered the smooth energy distributions predicted by PST. We concluded that the complete IVR assumption was still valid, but the random phase approximation did not apply because of the small size of the ensemble of transition state levels.

A major advance in our experimental capabilities occurred when David Chandler and Paul Houston developed the photofragment imaging technique in 1987. The imaging technique keeps us honest: we cannot ignore what we see – and we see a lot! I greatly appreciate the generosity of David Chandler in sharing his knowledge and creating a collaborative user community. I visited Sandia with Andrei Sanov to learn how to install this technique in our time-of-flight apparatus, and I came again, with Thierry Droz-Georget and Mikhail Zyrianov, to learn how to implement velocity map imaging. My way of paying forward was to make BASEX, the imaging reconstruction method that we developed, freely available to the scientific community. We first applied the imaging technique to photodissociation studies on coupled electronic surfaces, such as HNCO, which exhibits several dissociation pathways, and then proceeded to the more challenging studies of radicals and clusters. As is usual in science, questions answered lead to more questions, and we keep discovering intriguing manifestations of unimolecular dynamics on coupled potential energy surfaces.

Current Research

Our current research has evolved from previous studies and includes: (i) photodissociation dynamics of molecules and radicals in which coupled surfaces and conical intersections play a dominant role; (ii) predissociation of small clusters: dissociation energies and state-selectivity in product energy distributions; and (iii) collaborative studies with Curt Wittig on the reactivity and transport in amorphous solid water. These topics are covered in recent publications, and below I focus only on examples that highlight my valuable collaborations with theoreticians.

The photodissociation of free radicals was an area in which such collaborations were crucial. This is partly because these processes are often dominated by conical intersections involving couplings of Rydberg and valence states. My long-term and productive collaboration with Anna Krylov resulted in a 2009 review on this topic, published in the International Review of Physical Chemistry. I am grateful to the DOE combustion program for allowing me to devote so much effort to the study of the spectroscopy and multi-channel photoinitiated dissociation of the CH_2OH radical. This project serves as an excellent example of how, when scientists get obsessed with a problem, they return to it time and again whenever a new experimental or theoretical capability becomes available.

Our most recent return to CH_2OH was prompted by our success in 2010 in developing a version of time-sliced velocity map imaging that is optimized for detection of H atoms. This advance was introduced in our lab by Mikhail Ryazanov, a graduate student, who designed and built a 5-ns high-voltage pulser for this purpose along with a much improved ion-optics system. With this capability in hand we went back to study the overtone induced dissociation of CH_2OH to H + H_2CO. For the first time, we were able to identify specific signatures of pathways in which the radical dissociated directly or via prior isomerization to methoxy. In collaboration with Joel Bowman and Anna Kyrlov we described the relevant overtone excitation and O-H bond fission processes on the ground potential energy surface of the radical.

We also studied the UV photodissociation of CH_2OH leading to H_2CO and HCOH products following conical intersections from the excited 3s and 3p states. In these studies we were guided first by the insightful predictions of David Yarkony and coworkers, who pinpointed the important role of conical intersections and how the g-h vectors inform us on the ensuing dynamics. Several conical intersections were identified along the O-H and C-H coordinates. When the radical is excited to the 3s state, it dissociates mostly by breaking directly the O-H bond following a conical intersection with the ground state. On the other hand, to access efficiently the C-H bond fission channel, the excited $3p_x$ state must first couple to the 3s state, which in turn couples to the ground state along the C-H bond breaking coordinate. By determining the kinetic energy release of H-photofragments, we were able to identify both H_2CO and HCOH as nascent dissociation products and determine their rovibrational excitation. In their recent wavepacket calculations on the 3s state, David Yarkony, Hua Guo and their coworkers show how most of the dissociating flux proceeds directly along the O-H coordinate, giving rise to excitation in the CO stretch of the H_2CO product (see the cover of this issue). Happily, the calculations are in excellent agreement with our experiments. I await eagerly their new calculations on dissociation via the $3p_x$ state, which can lead to both H_2CO and HCOH, and how they compare with our newest experimental results.

I end the science part by highlighting briefly our ongoing studies on vibrational predissociation of hydrogen-bonded clusters (see the cover of this issue). This project led first to a fruitful collaboration with Tony McCaffery, who highlighted the importance of angular momentum conservation in product rotational state distributions. We then started a long-term and enjoyable collaboration with Joel Bowman and his students and postdocs. We succeeded in determining with great precision the bond dissociation energy of several dimers, including the water dimer, and in all cases there was excellent agreement with the high-level theoretical calculations of the Bowman group. Frankly, I was more interested in understanding the predissociation dynamics than in just thermochemistry, and with the complementary contributions of theory and experiment, we were able to describe many aspects of the predissociation of the water dimer. We then turned our attention to cyclic trimers, learned about energy transfer in and out of the ring, and obtained quantitative estimations of cooperativity. The most ambitious experimental and theoretical study in this series dealt with the predissociation of the $HCl(H_2O)_3$ tetramer, the largest $HCl(H_2O)_n$ cluster that dissociates to neutral HCl and H_2O fragments. These collaborative studies are summarized in a comprehensive review published in the special 2016 issue of Chemical Reviews on Noncovalent Interactions.

A Woman in Science

It has now been more than 50 years since my first paper, which gives me a half-century perspective on the advances of women in science. When I started my science education in Israel, I was fortunate enough to have women teachers as role models at the undergraduate and graduate levels. The senior women scientists at the Hebrew University, the Weizmann Institute, and the Soreq Nuclear Research Center all successfully combined career and family, and my women colleagues were happy to discuss with me their science and families with the same enthusiasm. All this changed when I came to the US in the early 1970s. There were no women faculty members and only few postdocs in the Chemistry Department at Johns Hopkins. When I came to USC in the late 1970s, the few women I met were, like me, employed in soft money positions. The situation began to change in the early 1980s when a few women were hired in Chemistry Departments. Many of these women, who were typically a decade younger than me, were either not married or did not have children. This was not the case, of course, for men in STEM fields. Finding childcare was difficult, and there were no academic accommodations for women with children. This was one of the main reasons why I decided to take a soft money position when my son was young.

A major change came after the report on the status of women at MIT was published in 1999. Professor Nancy Hopkins raised forcefully the issue of inequity for women faculty, and was able to win the support of the MIT leadership. This was crucial because it spearheaded initiatives at other research universities to increase the number of women faculty. As a result of the national attention to this issue, USC received a private donation to increase the representation of women in STEM fields at all stages of their career. This is when I became an activist. I succeeded in convincing the USC administration that we should establish a long-term program for women in science and engineering (WiSE), which would be run by women for women. This was not easy to achieve at USC with its top-down management style, especially because at that time there were only 15 women faculty in the School of Engineering and in the science departments of the College, and only few of them were tenured. However, with the support of our male colleagues we finally got permission from the Provost to create and manage a comprehensive WiSE program (see wise.usc.edu). This program provides resources, mentorship and networking opportunities to women faculty, and includes also specific programs and opportunities for women graduate and undergraduate students. When we started, most of the departments at USC had at most one or two women, and therefore we created a networking group that met for lunch once a month to exchange information and mentor junior colleagues. These networking lunches have been ongoing now for nearly 20 years. Through this networking group I met many outstanding women faculty and made new friendships. It was indeed wonderful to witness their career advancement, with some of them moving into leadership positions. Our motto is: though complaining is a time-honored occupation in academia, after each complaint there must be a proposed solution and an action item!

Although much progress has been achieved, I know that academic cultures are hard to change. In fact, a major change came only when men became more involved in raising their children and took advantage of family leave. This in turn made it more acceptable for women. We must recognize, however, that most women are still the primary caretakers of young children, and some of them experience career interruptions and nonlinear career trajectories. We therefore should find ways for them to return to academia after an interruption. Life expectancy is long, and these women can contribute for many years. For those women who stay the course, we need to provide affordable and accessible childcare, mentorship, networking, and additional accommodations and support when needed. Indeed, it has been found in numerous studies that support networks are helpful not only for careers but also for good health. We should encourage women to participate in workshops on career advancement and leadership, such as those offered by COACh (www.coach.uoregon.edu). For this, I am immensely grateful to Geri Richmond for founding and heading COACh since 1998. I am impressed that to date about 18,000 scientists around the world have taken the COACh career-building workshops. I know how much time and effort it takes to lead a sustainable program like that!

Being a woman in science will never be easy because of the challenges of balancing career and family. Can we have it all? No, in fact nobody can, but each one can set her priorities, follow her passion, find her own balance, and have a very satisfying life.

Closing Remarks

I ended my talk by addressing the students in the audience:

“Keep experimenting and you will discover things about yourselves: I discovered my aptitude for teaching and mentoring only later in life. If you practice what you believe in, you will also imprint your students.

Keep on studying and learning because what you learn informs and inspires your future research, and you might see unexpected connections.

Keep your enthusiasm and “getting high on science”, as was the motto of the meeting.

Don't forget to find an environment and/or activity where you can relax.

You don’t have to be “superwoman” to have a great career. Remember, scientific life expectancy is long and only a small fraction of it requires devoting most of the time to family. There is no reason to derail a woman’s career because of that, and certainly no reason to change career aspirations.

Having a career in science is a privilege. I consider myself lucky having this opportunity.

What kept me going? Excitement about science and discovery; optimism; keeping a sense of humor in times of adversity; the high quality of the people around me; my friendships and a sense of community; and last but certainly not least – my husband and son.”

I take this opportunity to thank all those who accompanied me in this journey: my graduate and undergraduate students and my postdocs who made being a university professor such an enriching experience and for keeping me young; NSF, DOE, AFOSR, ARO and PRF for keeping my research going; my colleagues in the Chemistry Department for always striving to become better; my WiSE friends who are committed to making USC a better place for all; my Molecular Dynamics colleagues who created a community that is supportive and encouraging of its young members; and my family and personal friends who gave me immeasurable support and believed, like me, that literature, art, music, theater, travel, and friendships greatly enrich our lives.