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Special Issue Preface

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Steven J. Sibener − A Brief Scientific Memoir

PRELUDE − REFLECTIONS ON GROWING UP IN BROOKLYN

My journey along the river of life and science began in Brooklyn, New York in 1954. My parents, Gerie and Daniel Sibener, raised me in the wonderful Flatbush section of Brooklyn, a location teeming with life and energy. To this day, I would not trade my early years spent there for any other location.

It was a neighborhood quite typical of the 1950s and 1960s, with many friends always around due to the high density of the apartment buildings on Linden Boulevard. The neighborhood at that time was already a melting pot, especially with ever-present populations of Jewish, Italian, Irish, Chinese, Hispanic, and African-American families operating many of the memorable stores: The mouth-watering bakeries Ebingers and Jeffreys, Doc's Pharmacy, Mel's Fruit and Produce, Michael's Meat Market, George's Five and Ten (purveyor of great Sunday morning Brooklyn bagels along with the requisite Daily News for the funnies and the New York Times for serious reading), and the many soda fountain stores on corners where one could buy a huge egg cream for a quarter. My favorite was vanilla! One particularly memorable man was Mr. Danziger. He was the TV repairman (a trade now replaced by Genius Bars and Geek Squads) and would appear at our house with a huge portable suitcase, trunk-size, filled with tube testing equipment and spare parts. He would always leave us as satisfied customers. If he were still around, I would in a heartbeat hire him to assist in our institute. He always gave me the still-working but worn tubes for my own electronic parts department; more on this below.

But now back to the social scene of growing up in Brooklyn. We had many strange customs that I now appreciate were memorable. First, whenever one wanted to go out and play or socialize with friends the primary method of summoning the tribe was to stand outside, regardless of the hour, whether in front of the building, in the side alleyway, or in the backyard, and scream at the top of your lungs the name of your friends. Heads would invariably pop out of the windows one-by-one from all floors, and presto, the street would be ready for action. Most notable were my best childhood friends Randy Spruchman and Jason Feld. The staples at that time were of course stickball, punchball, boxball, and the curious types of handball played in NYC, "Chinese" where you bounced the ball before hitting the wall and "American" where it was a straight shot to the wall. For more ambitious activities we hiked to the Parade Grounds for touch football. The default was always stoopball, where you threw a Spaulding (Spaldeen) or, if you had the extra nickel, a much livelier Pensy Pinky against the steps leading into the building and counted the bounces it had before being caught by a fielder unless it was a homerun. The inner courtyard at 305 Linden Boulevard was perfect for this, though we were frequently chased away for disturbing the peace and quiet of the lucky residents on the ground floor who could watch our cherished game at around dinner time or twilight. In the warm weather there were also the escapes to the beaches and cool waters of Coney Island, Manhattan Beach, Jones Beach, and Riis Park.

I also wish to note, and I will always remember, a number of Holocaust survivors who would line the streets in front of our apartment building every spring and summer day with folding chairs. They took great joy in seeing all of the kids running around with life and energy; one can only with the benefit of hindsight appreciate their joy in bearing witness to Life.

I spent a large amount of time at the Parade Grounds learning and playing tennis; the city courts at that time were clay (!) and were freshly watered and rolled each morning. The city pass for the entire season was only a few dollars. If you did not have a partner you just sat on the concrete steps until you were invited to either complete a doubles match or hit singles. My father would always tell me to wrap my arms around my body with my follow through, not the right technique, but it seemed to work at the time.

My special refuge was, without doubt, the then brand new Kate Wollman Ice Skating Rink in Prospect Park. Before this was built, we went to frozen playgrounds around the borough or, for special gatherings, went to the wilds of Queens for the city facility in Flushing Meadows Corona Park or after an endless journey to the Roosevelt Field rink on Long Island went to meet other members of the suburban Sibener skating group. I truly loved the ice, and still do. For some reason unknown to me the extended family members of Clan Sibener were speed skaters. I remember getting my first pair of used Planert speed skates from my beloved Aunt Vivi. After that, there was no turning back. The speed skating sessions were every Tuesday, Thursday, and Saturday in Prospect Park, and I rarely missed the opportunity to take a spin. There were an incredibly large number of kids with speed skates in those days, and each rink had its home team. I remember well the yearly Turkey Trot on Thanksgiving that would attract some elite skaters from around the country and especially the Brooklyn Blades competition, which had many heats leading up to the finals. Somehow, miraculously, I became a not too bad "indoor" sprinter (now short track). I will confess here that one year I won the championship! The rink was also a key focus of the evening social scene during my high school years, as was Jahn's ice cream parlor.

My hobby during childhood was amateur "ham" radio. WB2EZE was my call sign. This grew out of my father's fascination with electronics; he was a radio operator as a member of the Greatest Generation. The trips I would relish were to "Radio Row" in the lower west side of Manhattan. That area was the center of all things electronic. I would buy all sorts of used components to build my next creation. I was expert with vacuum tubes, souping up receivers and transmitters before the grand switch to solid state electronics, and, a personal specialty, antenna design. My first shortwave receiver was actually a Heathkit that my uncle Mel helped to assemble;

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he was a wizard with such things and helped to build, while working for Grumman, the lunar excursion modules of Apollo fame, so I knew that my first receiver construction project was in expert hands. But on one trip to Radio Row I hit the jackpot and purchased, after begging my father for the few extra dollars, a used and still famous command set radio ARC-5 receiver and transmitter pair from the Aircraft Radio Communications company that was the mainstay of military aircraft communications in WWII. I still have my ARC-5 receiver next to my desk at the University of Chicago to this day. I could tell if my mother had cleaned up my room as she would unplug the ARC-5 receiver for the vacuum cleaner. The archaic power supply did not have a grounded plug; put in one way the chassis was at ground, but reversed the chassis would be "hot" at 120 V. I always kept a light bulb with two wires to test the state of the chassis by touching one lead to it and the other to the radiator ground; this would determine if it were safe to turn on or if I had to flip the plug over to be safe. I had also built a rather large and high antenna array for 80 through 10 m wavelength operation on the roof on top of the elevator structure closest to my fifth floor bedroom window; as far as I know, neither the fire marshals nor a bolt of lightening ever struck this creation. This hobby served me memorably well while in summer camp in the Poconos in 1969. On the night of the Apollo 11 moonwalk our small black-and-white TV could not quite capture the signal. I assembled with the help of bunkmates a simple antenna out of the metallic slats of an individual springy bed and placed it in the building's rafters—we got an excellent, grainy view of Neil Armstrong's moonwalk once this was attached!

MY SCIENTIFIC VOYAGE

I attended public schools at all levels before going to college. A few blocks away, down New York Avenue, was P.S. 181. I remember being a great fan of the space missions of the time and decided to demonstrate rocketry to my fifth grade class. I brought in a pump-up plastic rocket that was propelled by pressurized water. I gave it only a few pumps to be safe, but when launched it smashed into the classroom ceiling. I suspect that the sizable dent still remains in what was then Mrs. Baker's classroom. After that it was Walt Whitman Junior High School, JHS 246. This was a stopping ground before high school. I faced my first big education decision at that time as I was offered admission to Stuyvesent High School, one of the city's three elite science high schools. My neighborhood school was Erasmus Hall High School. It had a famous tradition of excellence, though, unknown to me, was beginning its rapid decline right at that moment due to sweeping changes in the neighborhood. It was established in 1786 and was the first secondary school chartered in New York by the Board of Regents. Its many notable alumni include, for example, Barbra Streisand, Bobby Fischer, and a notable sports connection to my adopted home in Chicago, Sid Luckman (later of the Chicago Bears) and Jerry Reinsdorf (owner of the Chicago Bulls and White Sox). In the sciences there were many notable alumni, including Nobel Laureates Eric Kandel and Barbara McClintock. It also was home in prior years to my fellow physical chemist Bruce Berne from Columbia University. There was still a full suite of AP classes, and, unlike Stuyvesant, I could walk to school each day rather than taking the subway, and, ahem, it was coed, unlike Stuyvesant at that time. It also had sports teams, including the tennis team that I played varsity on for all years, grades 10−12. Looking back, I have no regrets on my decision to attend Erasmus. There were several notable teachers, but I would like to remember Wayne Mitty for his remarkable efforts and high standards that brought out the best in all of us who were so lucky to sit in his AP English class.

It was in Erasmus that I decided to focus on science. The math, chemistry, and physics faculty were excellent, and tough in the old-fashioned way. Unfortunately, the biology department was behind the times, with focus on memorization rather than the already occurring breakthroughs in what was then in high fashion, molecular biology. Teachers do matter, and my journey into the physical sciences was launched.

I was attracted by the renowned chemistry and physics departments at The University of Rochester that was at that time one of the wealthiest institutions of higher learning in the country. I will be forever grateful for receiving Rochester's Alumni Scholarship for New York City that helped me to convince my parents that we could make this work financially. It was one of the best decisions I have ever made. I found an amazing intellectual environment, lifelong friends, and clarity that my work would straddle the dotted line between chemistry and physics. Some of the professors were, to this date, the best that I have encountered. To name a select few, in chemistry there was John Huizinger (freshman chemistry and nuclear chemistry—telling us about the quest for the new island of nuclear stability), Marshall Gates (organic chemistry), and John Muenter, Thomas George, Jacob Bigeleisen, and Keiji Morokuma (physical chemistry). In physics my favorite professor was Adrian Melissinos. He had great joy in his science, and it was infectious with his enthusiasm; in addition to a lower-level introductory class I took his particle physics course with one other student and learned how scattering could be used to probe the wonders of nature. Little did I know that this would serve me well when I went off to graduate school. I was then trying to pick a research advisor and became fascinated with the statistical mechanics of liquids. I decided to work for two years with Frank Buff on the shape of liquid interfaces. I learned a great deal of mathematics and statistical mechanics from him and, being one of two members of his group at that time, had ready access to him, as long as I was prepared for a thorough grilling on my latest thoughts and work. I worked on analytic and numerical solutions for the Young−Laplace equation for various droplet geometries. I note that I have teased to this day, with great friendship, John Muenter for not introducing me to molecular beams while an undergraduate, but that would wait for Berkeley...

Rochester presented a whirlwind of opportunity. We worked hard and played hard. For several years my suitemates were Randy Essex, Bruce Tandy, and George Stanley, all in touch to this day. I had a double major in chemistry and physics. In my senior year I petitioned to trade an advanced synthesis course for a math course needed to complete my physics degree requirements. The undergraduate education committee, consisting of a few students and faculty, turned down my request; a leak informed me that all of the faculty had voted yes save one, while the student representatives voted no. I took the petition to the full chemistry faculty and offered in trade two math courses for the required course in question, and it was approved. I learned there that inflexible requirements are a very bad idea in the world of education and new ideas.

As part of the Rochester experience I spent two summers at the research laboratories of Eastman Kodak. These were my first exposures to the private sector. I had a great time and was well paid for the privilege. My first summer was in the area of

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photographic research, and I was assigned to examine lithographic printing plates to see if I could find out why some plates worked well and others did not. With a hint of what was to come, I became a fledgling surface scientist. I remember setting up in a little lab that they gave me with a contact angle apparatus and a Kelvin probe. After weeks of experimentation I produced some plots that suggested potential correlations and was summoned to the department head's office to explain what I had done. Unknown to me until that moment was that the correlations uncovered using advancing and receding contact angles with various oils and inks, including some rudimentary humidity control, along with the Kelvin probe measurements, corresponded in some ways to the Kodak plates' performance for a given production run and also indicated in some instances their relative performance versus some of the competitor's plate outcomes. When I completed my summer internship I was not allowed to have a copy of the technical report that I wrote. Another lesson in the differences between academic and industrial science.

I was invited back for the next summer and gladly accepted the opportunity. I probably had one of the most memorable internships, ever, as I was assigned to the germinating electronic photography group. The group leader was Richard Ahrenkiel, and I was assigned to growing anodic native oxides on GaAs and related materials for potential use in the then very primitive charged coupled array sensors. I am not sure that I broke any new ground with my work on oxide growth and electron charge transport, but I note that the upper management did not fully appreciate, as I picked up at lunch discussions, the tsunami that was coming given the incredible resolution achieved with film at the time versus the primitive, grainy low-resolution images that were suggested by these early attempts at making a digital camera. It was an amazing opportunity to be exposed to the beginnings of electronic photography and the coming end of the film age. To say that Kodak blew this call is one of the great understatements of the last century in the high-tech arena. The ubiquitous yellow film boxes of the late 1970s are effectively all but gone today.

My senior year at Rochester was occupied with planning for graduate school. I applied widely to programs in physical chemistry and chemical physics and was fortunate to be accepted and to have the opportunity to visit many of the leading schools in the country. Particularly memorable were my visits to Berkeley and Harvard, with those schools ultimately becoming my short list. I could have easily gone to Harvard as I was enthralled by the science and fantastic personalities of Bill Klemperer and Dudley Herschbach. I should also mention that I received several letters from Stuart Rice on behalf of the department at Chicago, the other great center of physical chemistry, but decided not to go there. Perhaps that was lucky, as I now understand that Chicago tends to send their best students away after receiving their doctorates rather than recruiting them locally to the faculty. I am not sure if this is the best policy, but it more or less remains in place in some form to this day. By that time I knew that I would return to my passion for experimental science. It was also my first opportunity to go west of the Mississippi. My visits to Berkeley and Stanford were memorable, but the scale of the Berkeley science enterprise, plus a very unusual and memorable meeting with Yuan T. Lee, proved decisive. At that time chemical dynamics was THE hot field of chemistry, with researchers uncovering the underlying principles of chemical reactivity and molecular interactions using molecular beams, high-resolution and time-resolved laser spectroscopy, and the ever improving dynamical workhorses of scattering theory, electronic structure, and molecular dynamics or Monte Carlo simulations.

At Berkeley I remember well my meetings with Bruce Mahan, Bill Miller, Charles Harris, Gabor Somorjai, and Dave Shirley. Then something magical happened that almost changed my entire career. It snowed in the Berkeley hills. I can still see Bruce Mahan trying to drive up the steep roadway, Cyclotron Road, to bring me to my last appointment with Yuan Lee—but we could not go up due to the slippery conditions and the road was closed. That night my room phone rang in the faculty club, and it was Yuan. He invited me for an early morning get together to see his new laboratories at Lawrence Berkeley Laboratory in Building 70A and discuss science. This meeting, his already unique laboratories, and his first generation of Berkeley students really impressed me. When done he took me on top of the Bevatron to view San Francisco Bay; needless to say I was hooked and matriculated at Berkeley in Fall, 1975. Upon arrival I did the requisite visits to meet the faculty in order to learn about their research opportunities for new graduate students—but it was clear that my heart was set on having the privilege to study under Yuan and utilize molecular beam scattering to probe the atomic-level details of molecular interactions and reactivity. I feel fortunate to have had the opportunity to join his group at that particular moment in time—special things were in store...

Once in the group I began my work on two projects, attempting to develop an atomic beam source for producing $O(^3P)$ and $O(^1D)$ atoms of sufficient intensity, reliability, and narrow velocity distribution to be useful in crossed-beam scattering studies. Drawing on my radio background, I went the route of a radio frequency plasma source. I went down to Burlingame, CA to pick up some high-powered transmitters and chose the 14 MHz regime as it could penetrate through a water-cooled jacket for a quartz nozzle as long as purified and deionized (very low-conductivity) water was used as the coolant. I spent quite some time in the Lawrence Berkeley Lab glass shop working with their then head, Dane Anderberg, to fabricate nozzles that would not melt under the high RF power conditions needed to operate at sufficient pressures to generate a high Mach number expansion. My job was to look at the nozzle's diameter with a microscope as he did the critical glass blowing on the lathe; by the successful end of this endeavor we could reliably fabricate nozzles with 0.001 thousandths of an inch precision. I viewed him then, and still do, as a supreme artist—none better that I have ever encountered. The hardest part proved to be the impedance matching requirements for different gases and especially different pressures. It turned out that a pi-network impedance matching circuit did the trick: It could accommodate the electronic changes in the plasma as we went from the low pressure needed to strike initially the plasma and then match the impedance as the pressure was increased to achieve the intensity and Mach number needed for scattering experiments. Moreover, the matching requirements for different carrier gas arrangements could also be met by changing the grounding arrangement on the inductive coupling coil plus retuning the matching network, allowing the production of beams of varying translational energies when using Ar, Ne, or He. The final breakthrough was designing an arrangement that crossed over from inductive to capacitive coupling at high pressures, leading to the high temperatures needed to achieve molecular oxygen dissociation. Yuan had suggested that I look into this technical detail. The first proof of success was

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scattering O atoms from He and seeing that the scattering fit the Newton Diagram predictions for the angular distribution expected for O rather than O_2. We then began our series of reactive scattering experiments, with the heavier carrier gases yielding access to ground state $O(^3P)$ chemistry, while the lighter gases gave access to both $O(^3P)$ and electronically excited $O(^1D)$. We then went on to perform a series of experiments with halogens, hydrogen, and small hydrocarbons. Probably the most memorable of these was the combustion of benzene where we showed under single-collision conditions that the then expected and accepted mechanistic chemistry needed major revision; the power of the crossed-beam method to elucidate the correct mechanistic processes, always known to Yuan, became readily apparent to all of us. We also carried out some early examples of excited electronic state crossed-beam reactive scattering studies, using the beam to produce beams of $O(^1D)$ under different carrier gas and power settings. I had several partners in these studies, but certainly my colleague, friend, and officemate Richard Buss, who was an absolute wizard with coding the CAMAC and Nova minicomputer data system, was absolutely essential in all ways—he was a truly brilliant experimentalist and scientist. Also, it was at that time that I met and worked with Piero Casavecchia, who visited from Perugia. He remains one of my great friends and scientific colleagues to this day.

My other project, one that would introduce me to the study of interfaces, involved the question of molecular sticking and condensation. At the time there were many approaches being examined to find more efficacious approaches to separating isotopes than gaseous diffusion. Using a velocity selector that Yuan and group had built for prior reactive scattering studies, I examined the condensation of CCl_4 and SF_6 as a function of molecular internal energy as well as gas−surface collision energy and surface temperature. The goal was to see if the combination of added thermal internal energy (rotations plus vibrations) could modify the probability for scattering vs condensation. It was truly wonderful when, under gentle collision conditions, we were able to observe very small but important differences in the reflection probability for the hot (internally energized) molecules vs those at room temperature. This was great fun for all involved.

I had some of the best four years of my life at Berkeley while in Yuan's group, and it was a privilege to be one of the many members of his team of apprentice dynamicists. Cheuk Ng was my first graduate student mentor, introducing me to the "real way" to use the A-Machine properly. He was a great teacher. Others in the group are too many to list in full, but, in addition to Rick Buss, Piero Casavecchia, and Cheuk Ng, I would note Scott Anderson, Chris Becker, Sandy Bustamente, Lee Carlson, Sylvia Ceyer, Mike Coggiola, Manfred Faubel, Dieter Gerlich, Ed Grant, Carl Hayden, Tomohiko Hirooka, Frances Houle, Carol Kahler, Marta Kowalczyk, Doug Krajnovich, Dan Neumark, Peter Schultz, Kosuke Shobatake, Randy Sparks, Peter Tiedemann, Dennis Trevor, Jim Valentini, and Matt Vernon, and many others who would go on to make their mark in science. Matt Vernon enriched our social lives in an unexpected manner. Before coming to graduate school he became trained as a gourmet French chef. He would prepare and serve the entire group remarkable gourmet meals, much appreciated by all! We also had several memorable group activities, including the end of day (nighttime) ritual of piling out in cars and road rallying to Fenton's ice cream parlor in Oakland, the perfect capstone to a day in the lab. There was also Yuan's group softball team, the Chem Pistols, that drew members from his research group as well as many other physical chemistry groups such as Mike Berman, Dave Castner, Fred Grieman, Steve Hansen, Carl Hayden, Tomohiko Hirooka, Floyd Hovis, Henry Luftman, Herb Nelson, Tony O'Keefe, Peter Schultz, Tom Turner, Jim Weisshaar, ... Yuan was our third baseman, and a good one at that!

In addition to the group of dynamics faculty, I note a special friendship with Kenneth Pitzer. I was assigned to be his TA in undergraduate physical chemistry. He would occasionally invite me to have lunch with him and share stories about science and politics from his prior roles as president of Rice and Stanford. He was a true gentleman and scholar from the old school of science. He is missed.

It is time to note another critical moment in my life that occurred during my second year at Berkeley. That is when the love of my life, Linda Young, arrived from MIT. We became fast friends, tennis partners, travelers, explorers in all ways along the river of life, and so much more during the Berkeley years. We have been together ever since.

In my fourth year I began to think about the next stage of my career. I was becoming fascinated with dynamics in the condensed state including surface science. Given this, I applied for postdoctoral positions at three places, Bell Laboratories, IBM T. J. Watson Research Center, and the National Bureau of Standards, the latter due to the presence of John T. Yates, Jr. After visiting all three places the magnetic attraction and excitement of Bell Laboratories at that time proved irresistible, so I made my choice.

In the middle of this process I was invited by Stuart Rice and colleagues to give a seminar and interview for a faculty position at the University of Chicago. I was honored to have this unexpected opportunity. It was my first exposure to the "Chicago School" of physical and nuclear chemistry with Ed Anders, Stephen Berry, Bob Clayton, Karl Freed, Robert Gomer, Clyde Hutchison, Ole Kleppa, Don Levy, John Light, Robert Mulliken, Norman Nachtrieb, Stuart Rice, Clemens Roothaan, Willard Stout, Nathan Sugarman, Tony Turkevich, and the young Jeremy Burdett and David Oxtoby. I had a fantastic visit and enjoyed my first trip to the city of Chicago. Months went by, and I was actually choosing between a permanent or postdoctoral position at Bell Laboratories. I called Robert Gomer, the Director of the James Franck Institute, and he told me to hold on just a bit longer. I was shortly thereafter offered an Assistant Professorship at Chicago. I accepted and then went to Bell Laboratories for a wonderful postdoctoral experience with Mark Cardillo to learn the ins and outs of surface science and gas−surface interactions.

At Bell Laboratories, I really came to appreciate the efficiencies of a few good people working together with a stable and sufficient funding stream. Mark's group while I was there consisted of four of us: Mark, his technical assistant Gordon Becker, visiting professor David Miller from San Diego, and myself. There was also Jack Rowe and Homer Hagstrum in surface physics and the designer/machinist Ed Chaban whom I quickly learned was essential to bringing the conceptual innovations into lab reality in many groups that worked with him—he was an essential cog in the Bell Laboratories research engine. I learned about the common UHV surface analysis tools and was introduced to gas−surface interactions with work on the surface structure of GaAs(110). It was an exciting time with dynamics discussions with John Tully and others who were then there. I also had a great postdoctoral cohort consisting of

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Paul Barbara, Trevor Sears, and Peter Armentrout. Cherry Murray was also then a new staff member, while my office mate, Wilson Ho, was deciding to leave Bell Laboratories for a faculty position at Cornell. One could not ask for a more interactive and interesting group of peers. I note that the young Bob Laughlin, a good friend of Mark's, was also then at Bell Laboratories and derived useful expressions for how to relate interfacial surface charge density to the interaction potential experienced by a helium atom scattering from an interface at a given collision energy, a relationship derived at more or less the same time by Esbjerg and Nørskov. Another memory from Bell was the weekly literature review seminar where someone presented an interesting paper and then discussed it, subject to questions from the audience—a very hard crowd to please. There were also the daily lunchroom discussions where I picked up on the latest science at the laboratories. Lois and Mark Cardillo and John Tully became my lifelong friends during that time in NJ. A funny remembrance was that for a few months I had Maurice Rice's office after he left for ETH Zurich. In addition to his theoretical brilliance, he was also a manager so had a rug on his office floor, an honor reserved for staff in administrative positions. A few people were surprised when they came to chat that I had a rug and were clearly perplexed as to how this lowly postdoc had become a manager so quickly! Also critical were the late night coffee and chocolate cake runs to our favorite local NJ dinner. It was there that we planned for the next experiments, and Mark shared his bountiful comments on all aspects of science and life. He was a wonderful teacher, mentor, and lasting friend.

After a year it was time to assume my position at The University of Chicago. I was appointed in the Department of Chemistry and the interdisciplinary James Franck Institute where my office and laboratories were located. One marvels at the sophistication of today's assistant professors. At the time I had much to learn about the science enterprise and getting onto the right path to tenure. It was my great fortune that Graham Fleming, who had arrived earlier, was my comrade as we navigated this phase of our careers together. Graham and his wife Jean, Linda, and I loved the city and restaurants, and that period remains fresh in all of our minds as enduring friendships were made. It became clear that we had chairs, directors, deans, and above in the administrative chain but that Stuart Rice was the scientific and political godfather of the place. Stuart shared his wisdom (and shelves of electronics) with great pleasure. Stephen Berry was the real surprise, a gem of a person with great scientific insights and many ideas. Collaborations were quickly struck with John Light and his student Birgitta Whaley for quantum scattering calculations needed to interpret our first scattering results on gas−surface interactions, as well as with Ugo Fano and his student Peter Knipp for surface phonon calculations. Bob Gomer, one of the leading surface scientists of his generation, was not only Director of the JFI but also sounding board for many ideas as my group moved into many aspects of surface chemistry and physics.

I reserve a few lines alone for Don Levy, fellow experimentalist and master of supersonic beam technology and spectroscopy. Together we have had, and still do to this day, endless discussions on science, life, politics, and family, many with his wonderful wife Sue and Linda. I feel very lucky to have had my offices and laboratories next to his in two buildings and to have had in earlier times a joint softball team that played in the departmental league. He played a wicked first base. Perhaps most importantly, he provided level-headed and very practical advice on all matters scientific and personal. To be honest, on many occasions I felt that he was the older brother that I had never had.

The physics part of the JFI was special too. The clear leader in all ways was Leo Kadanoff. On my first day at Chicago he put some recursive equation on my blackboard and said I should play with it. Having no idea what he was talking about, I then fiddled around with it and learned from him about the new science of chaotic systems. The soft school of condensed matter that he helped spawn has been ever present since then, with Sid Nagel, Heinrich Jaeger, briefly Albert Libchaber and Steve Shenker, and Tom Witten being the core group for a generation. Tom Rosenbaum was since the first days of his arrival at Chicago another dear friend and colleague. He was one of those rare individuals in science whose administrative talents matched his scientific prowess; he has recently become President of Caltech.

One of the great joys of being at an elite university is the quality of the students and postdocs that pass through its halls over the years. I have been blessed to have many of them in my research group spanning high school students through visiting professors. They are listed on the accompanying collaborators page of this Festschrift issue. In the following brief discussion of science at Chicago I will not be able to cite each by name, nor cite all of their experiments, but know that each of them left their firm mark on the group's science and also with me personally. This includes the over 30 undergraduates who have done either undergraduate research for their honors dissertations or had summer experiences in our laboratories. Science groups are really like extended families, where one can go long periods of time without communication and then suddenly hear from someone about a new job or family member after years of quiet. I also note the many superb colleagues that have joined the faculty after my arrival at Chicago. It gives one pause to realize a consequence of being hired as a faculty member at an unusually young age; by seniority, I am now the ranking member of the active departmental faculty since the retirement of Karl Freed, and I have participated in the decision to hire all of its current members.

Our initial work at Chicago focused on molecule−surface interaction potentials where we used neutral molecule elastic and rotationally inelastic scattering, including bound-state adsorption resonances, to extract quantitative information on molecule−surface physisorptive interaction potentials. Experiments by Chien-fan Yu and Jim Cowin (one of the last graduate students of Lennard Wharton) with H_2, D_2, and HD (including rotationally state-selected beams of para-H_2 and ortho-D_2), when combined with quantum scattering calculations, done by Birgitta Whaley and John Light, led to one of the most accurate determinations of a molecule−surface physisorption potential including spatially anisotropic terms up until that time.

We then began a series of innovative experiments where we demonstrated that the use of precision energy- and momentum-resolved inelastic single-phonon helium scattering could provide rich insights into the properties of atomically thin films. This beautiful work, the basis of Kevin Gibson's doctoral dissertation, revealed how the surface vibrational band structure of an ordered atomically thin film evolves, on a layer-by-layer basis, toward that characteristic of a thick crystal. Lattice dynamics calculations (with Doug Mills and John Black) allowed determination of the interatomic forces present in films of Ar, Kr, and Xe as a function of film thickness. We also

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utilized elastic He diffraction to confirm layer-by-layer growth of rare-gas crystals on Ag(111) and combined diffractive and resonant scattering experiments with quantum scattering calculations (with John Light and postdoc Charlie Cerjan) to assess the 2-body and 3-body contributions to the gas−surface interaction potential. I remember telling Kevin that our budget at that time could only support a few dewars of liquid helium for cooling the manipulator; we ended up doing these experiments for well over a year. This opportunity to do forefront science clearly overruled our group's budgetary constraints; the trick, confessed here, was to roll over one year's bills to the next, giving us the financial buffer to complete these studies before larger grants started to arrive from Washington in support of our activities.

With the arrival of a new postdoc, Wesley Natzle, we took our first foray into interfacial photoprocesses. This work demonstrated that two competing mechanisms are responsible for the UV laser-induced desorption of NO from condensed films of this material. One channel was thermal in origin, while the other was a nonthermal, single photon process. These results were among the earliest to suggest that specific photochemical pathways may be found at surfaces with time scales that successfully compete with energy randomization. More recent photochemical studies were carried out by Amadou Cisse, Dan Killelea, Nataliya Yufa, Hanqiu Yuan, and visiting scientist Eitan Grossman, where we examined polymer photochemical decomposition and related structural changes.

The group then began its examination of reactive processes. At the time modulated molecular beam scattering had been developed by a few groups, and we were aware of the power of this method for extracting precise mechanistic information. Our extension involved the pioneering use of multiply modulated molecular beam scattering techniques for the elucidation of heterogeneous reaction mechanisms. Graduate student David Padowitz, a gifted experimentalist with mathematical skills to match, readily accepted the challenge of bringing these difficult measurements to fruition. Here we utilized three supersonic beams, providing a general approach to linearizing reaction kinetics as a function of adsorbate coverage and thereby extracting elementary reaction steps and rate constants from complex chemical environments. I recall clearly when he called me in my office and summoned me to the lab to see one of my dream outcomes: Oscillatory behavior was seen in a particular place within the coverage−composition−temperature phase diagram. David's work built upon Larry Brown's kinetic and dynamics studies of CO oxidation. Jennifer Colonell went on to measure dynamical properties of reaction products as a function of adsorbate density to elucidate interadsorbate effects on reactive potential energy surfaces, with extension to the supersonic molecular beam growth of SiC by Errol Sanchez, while Kevin Peterlinz and Tom Curtiss used helium reflectivity to monitor on-surface processes. Mark Viste joined this effort and probed the heterogeneous combustion of benzene. Other reaction studies included the use of kinetically activated reagents to enhance the reaction rate and selectivity of heterogeneous reactions, such as the Synthesis Gas reaction on rhodium by Kevin Gibson.

After our initial studies of rare gas surface phonon spectroscopy were underway, we realized that new, higher energy, and momentum-resolution instrumentation was needed to resolve the subtle but important details of surface phonon spectroscopy. We set out in two directions, the first involving the construction of our helium atom based surface phonon spectrometer and the second using angle-resolved inelastic electron scattering. They each had a complementary role to play in elucidating surface interactions and how they differed from bulk behavior. A heroic team of graduate students took on the job of designing and constructing the atom scattering apparatus, Yaw-Wen Yang, Barbara Gans, Suzanne King, and subsequently Dan Koleske. Afterward, Warren Menezes and Glenn Tisdale designed and assembled the electron scattering instrument, followed by Mike Stirniman and Jeong Sook Ha who added its supersonic molecular beamline. The university's central machine shop was essential in both of these large projects, with the best machinists put to work by us: Helmut Krebs and Paul Dolmer. I am grateful for their technical brilliance and many suggestions that helped these projects to become reliable workhorses in our exploration of surface dynamics. An illustrative success of helium scattering was the probing of the structure and dynamics of alloy surfaces. For $Cu_3Au(001)$, Barbara Gans, Suzanne King, Dan Koleske, and Peter Knipp from Ugo Fano's group examined many aspects of structural ordering and disordering and that the surface force field has significant departures from bulk behavior. Similarly, with the ability of inelastic electron scattering to probe higher-lying excitations, Warren, Glenn, and Peter were able to quantify the presence of surface stress for Ni(111).

I then became interested in examining imperfect surfaces that while still crystalline contained localized or extended surface defects. In order to do this we decided to use stepped (vicinal) metallic surfaces, important for understanding the structural and dynamical properties of clean and partially oxidized metallic interfaces while containing long-range extended step defects to serve as a model metallic one-dimensional system. A seminal advance occurred when our group was the first to probe the localized surface phonons that exist at extended surface structural step discontinuities using inelastic He scattering; these studies, pioneered by Licheng Niu, of step-localized phonons gave new information on bonding and, hence, the charge redistribution that occurred at extended surface defects. As part of the next stage of these studies, Dan Koleske examined several aspects of the initial stages of metallic oxidation, including the observation of step doubling/undoubling and relevant kinetics on metallic interfaces induced by oxygen adsorption before the onset of bulk oxidation. Dan Gasper extended the surface dynamics studies into the multiphonon regime. A postdoc, Aubrey Hanbicki, brought his great expertise in surface physics to the group and examined how the order−disorder transition for hydrogen adsorbed on Ni(111) was modified due to the presence of steps and terraces. This work was subsequently augmented and extended in many ways by Tom Pearl, who assembled our first ultrahigh vacuum STM. The motivation for constructing this STM was clear: We wanted to look at the local, spatial correlations that were important during surface oxidation and restructuring. Tom's time-lapse STM movies succeeded in visualizing the inter-row zippering dynamics that were the foundation of such events. Such details, while correctly inferred from reciprocal space studies, became unambiguously resolved using STM imaging. Yi Wang subsequently used this system to produce organized linear arrays of silicon on vicinal surfaces.

In addition to the aforementioned studies on the oxidation of imperfect surfaces, around the same time we also launched several projects to illuminate the initial stages of metallic oxidation under energetic conditions. Julie Slezak and Ben Zion examined oxidation using atomic oxygen in the HREELS lab,

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while Ben and Aubrey Hanbicki used translationally fast molecular oxygen for such studies. Wei Li and Mike Stirniman studied synergistic effects during metallic oxidation arising from incident and energy selected electrons. Jong-in Hahm took a different approach, using AFM to examine the influence that externally applied stress fields exert on electrochemical corrosion. Inelastic electron scattering was also utilized at this time to examine electronically resonant inelastic electron scattering processes. This allowed Jeong Sook Ha to study the ultrafast dynamics of transient negative ion states, e.g., the $^2\Sigma$ shape resonance for CO/Ni(111) and the librational dynamics of adsorbed CO and NO.

Around this time my interests began to evolve from our prior work on "hard" surfaces (metals and semiconductors) to focus on more complex "soft" molecular and polymeric interfaces. This required us to develop new tools and scattering methods. Seth Darling and Aaron Rosenbaum's work focused on the phonon structure of SAMs, collisional energy transfer, self-organization, and reactivity. Our studies of gas−surface collisional energy exchange have helped us to better understand the hierarchy of interfacial energy-exchange processes responsible for energy accommodation, sticking, and gaseous condensation. Scattering of heavy rare gases from SAMs, including quantitative comparisons with large-scale molecular dynamics simulations, led to improved understanding of energy and momentum exchange at complex molecular interfaces. This marked the beginning of many fruitful collaborative efforts with Bill Hase, whose large-scale and accurate MD simulations allowed us to fully leverage the information contained in the scattering results. This work took an unexpected turn when Bill Isa and Kevin Gibson demonstrated that high-energy gas−surface collisions with SAMs can lead directly to penetration into the film followed by directed re-emission.

Another new scattering regime that attracted our focus was hyperthermal studies of relevance for both materials processing and low-Earth-orbit materials chemistry. When in orbit, spacecraft and satellites are subjected to energetic collisions, primarily involving 5 eV gas−surface encounters with O(^{3}P) atoms. Tim Minton, from Montana State, spent a sabbatical with us, helped us to construct, and taught us how to utilize his magnificent laser-ignition beam source for producing fast neutral beams of rare gases and reactive species, most notably ground electronic state atomic oxygen. Hyperthermal studies of collisional energy transfer, neutral sputtering, and sticking under nonequilibrium conditions are ongoing. I very much value my long-term association and friendship with Tim. The topic of space chemistry seemed to be worth a major effort, and I successfully assembled a multidisciplinary university research team (MURI) that competed successfully for a national center on this topic. The key collaborators in this program included a wonderful group of dynamicists and materials chemists, including Tim, Barbara Garrison, Dennis Jacobs, Luping Yu, Bill Hase, George Schatz, John Tully, and our group in Chicago. The postdoc at the time, Ken Nicholson, provided the key link in our joint studies where materials were oxidized in Montana and imaged in Chicago.

In order to complement our scattering work, it became apparent that we needed to enter the age of scanning probe microscopy to visualize, in real-space with time-lapse (not yet fully real-time video) resolution the dynamics of complex interfaces. Atomic force microscopy was in particular needed to look at the evolution of nanoscale surface structure upon annealing above the glass transition temperature including both defect mobility and more global phase tranformations. Jong-in Hahm was a spectacular experimentalist, with great patience and persistence; brave too given that she was the first of now many generations of "polymer students" in the group. She used our first atomic force microscope to visualize defect evolution in thin films of phase-separated diblock copolymer films, successfully tracking the time evolution and annihilation of individual defects in the microdomain patterns of diblock copolymer thin films and giving us the ability to predict thermally activated structural changes. This work also demonstrated how self-organizing polymer domains can be spontaneously generated using local stress fields and flow. Jong-in also deserves special mention as my first student to work on biological interfaces, at the time focusing on the cell wall structural changes between drug-susceptible and drug-resistant *Staphylococus aureus* bacteria. This project was memorable for many reasons, including an impromptu visit to my office by many of my group members telling me that they were worried about working in proximity to "these bugs"—I assured them that they were dead and harmless. Being a smart group they knew that I really had no idea about this, so I summoned my collaborators Robert Daum and Susan Boyle-Vavra from the UC medical center, who supplied these clinically important isolates to tell them the details and that all was well. An uneasy skepticism remained in the AFM lab for the duration of this project!

Subsequent studies, by Nataliya Yufa, Deepak Sundrani, Qin Zheng, Qianqian Tong, Hyuang Ju Ryu, and Jonathan Raybin, went on to explore nanoscale domain alignment and perfection spanning micron length scales via graphoepitaxy, that is guiding the formation of desired nanostructures by using top-down lithographic templating. Qianqian took these studies to a new level with her remarkable studies of real-time/real-space visualization of how individual nanodomain dislocation and disclination defects approach and annihilate other defects leading to perfected materials and domains. She also succeeded at using oriented electric fields to break local spatial degeneracy and therefore could switch the connectivity diagrams for the massively parallel nanonetwork polymer junctions that were present in arrays of intersecting lithographic channels. This may find utility in nanofluidics and circuit fabrication. More complex molecular films also came under study at this time, with multicomponent films being examined by Miki Nakayama and chiral systems by Lieve Teugels and Gaby Avila-Bront, now continued by Jeffrey Sayler.

Accompanying the aforementioned nanoscale polymer studies were our experiments aimed at probing polymer chain dynamics using inelastic helium scattering, a probe well-suited for such studies of polymers given that it was both surface sensitive and nondestructive for organic systems. This turned out to be a most incisive approach with Miriam Freedman and Jim Becker's work revealing how polymer surface dynamics change due to nanoconfinement as film thickness approaches the radii of gyration of the polymer chains and how surface vibrational dynamics change when going from the amorphous to the crystalline phase. These surface measurements provide a precise window into polymer surface dynamics, complementing other (and not exclusively surface sensitive) spectroscopic, neutron, and X-ray scattering methods as they revealed with clarity a picture of surface dynamics isolated from bulk signatures. We also explored at this time polymer reactivity and photomodification in our scattering laboratories, efforts

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spearheaded by Amadou Cisse and Hanqiu Yuan with assistance by Irene Popova.

More recently, we have become interested in interfaces for energy conversion systems including photovoltaics, hydrocarbon decorated semiconductors, and quantum dots. One effort (with Seth Darling now a staff scientist at Argonne and postdoc Sanja Tepavcevic) in hybrid molecular/nanotube solar cells showed that nanostructures with *in situ* polymerized photoactive species have efficiency advantages over more conventional photovoltaics. In still ongoing work, elastic and inelastic He scattering from the electrochemically important CH_3−Si(111) and CH_3−Ge(111) interfaces has elucidated valuable surface structural information, as well as how methyl librations hybridize with the phonon structure of the underlying Si interface. Key graduate student contributors to this project include Jim Becker, Ryan Brown, Zack Hund, and Kevin Nihill. CH_3−Si(111), synthesized by Nate Lewis' group at Caltech, offers many advantages over conventional H-decorated interfaces for applications involving redox chemistry, e.g., advanced fuel cells. Our most recent work on electronic materials has used scanning tunneling spectroscopy at near liquid helium temperatures to probe charge transport and decay in InSb quantum dots; this done by Tuo Wang in collaboration with Dmitri Talapin's nanoscience group.

At the present time we have several new projects that point to the future. Foremost among these are our studies of the new phenomenon of energetic neutral embedding of atoms and molecules into ice. Dan Killelea, Kevin Gibson, Hanqiu Yuan, Wenxin Lin, and Grant Langlois, now joined by Becca Thompson, have recently demonstrated that atoms and molecules can be energetically embedded into ice, with ballistic embedding probabilities dependent upon the nature of the ice, crystalline vs amorphous, as well as the properties of the incident species such as incident kinetic energy, mass, and size. The dynamical phenomena associated with energetic embedding have been further elaborated using molecular dynamics simulations done by Bill Hase. Energetic embedding is of importance for a variety of areas of chemical research encompassing gas−surface interactions and energy transfer, astrochemistry, trace detection related to national defense, and environmental science associated with permafrost and clathrates. Other new directions include the studies by Natalie Kautz and Darren Veit involving niobium interfaces for superconducting radio frequency materials. Lance Cooley from Fermilab introduced us to this fascinating topic. It was observed that different baking conditions at relatively low temperature, done in the presence of air, could alter the important SRF properties of niobium leading to cavities that could, on occasion, support higher-field gradients for particle acceleration. It quickly became apparent that the influence of oxygen, nitrogen, carbon, and hydrogen, depending on preparation procedures, could significantly influence SRF materials performance. I enjoy the thought that the high-energy physics community is now partnering with surface and materials chemists to further enhance the performance of their next generation of accelerators.

Our newest instrument has just been completed. It is a combination of "both sides" of the group, our first fully integrated supersonic molecular beam−scanning tunneling microscopy apparatus. This effort was begun by Gaby Avila-Bront and Chris Fleming and has reached completion due to the ongoing efforts of Bryan Wiggins with the recent assistance of Ross Edel. This instrument will allow us to probe spatial-temporal correlations in interface reactivity including materials growth, erosion, and oxidation. Our initial studies of energy-dependent oxidation of Si(111)-(7 × 7) are just beginning but already look quite promising. I am hopeful that this instrument, along with our other scattering laboratories, will finally open the door to geometry (opacity)-restricted reactive scattering experiments, a topic that has intrigued me since my days at Berkeley. If these experiments pan out, they will reveal the details of molecular reactivity without the complication of rotational averaging, in essence to probe the reactivity of molecules at various points within their molecular framework including control over approach orientation.

My scientific life at Chicago has been greatly enriched by many wonderful colleagues at the university, too numerous to cite in this short memoir, as well as others from outside the university. Kevin Gibson, one of my first graduate students and now holding the rank of Senior Scientist, deserves special mention: Except for his time at Bell Laboratories, Kevin has been a brilliant researcher and experimentalist in my group. He has enriched our science in untold ways. In addition to Piero Casavecchia, Tim Minton, Bill Hase, Mark Cardillo, and John Tully, as discussed earlier, I would note my great friendships and personal interactions with Stephen Berry, Karl Freed, Ka Yee Lee, Don Levy (as elaborated previously), and Stuart Rice in our department and institute, Gil Nathanson from Wisconsin, and George Schatz from Northwestern. Our scientific exchanges through the years have enriched my science and thinking. Beyond the local scientific milieu, Ka Yee and her husband Thuong Van Ha have also been the center of social activity for numerous events large and small, inheriting and carrying on admirably this important mantle from Marion and Stuart Rice.

To conclude this brief memoir, I would also like to comment on some of the leadership roles I have played in the university as well as in the context of national science centers that I helped create. My first such role was as Director of the University of Chicago Materials Research Science and Engineering Center, otherwise known as the NSF-MRSEC. I took over its administration from Leo Kadanoff. I found it quite a responsibility and task to orchestrate its large faculty to operate as the required thematically organized science center that also included educational, industrial, and public outreach activities that extended beyond its scientific focus. I was pleased to help my colleagues in this manner. I was very happy when we learned that the MRSEC had been renewed competitively for another funding cycle. I then became Director of The James Franck Institute at a particularly crucial moment in time. The university had agreed that the famous Research Institutes Building dating back to Enrico Fermi and colleagues had reached the end of its functional life. A large number of faculty in the physical and biological sciences, especially Don Levy, Mike Hopkins, and I, toiled over the structure and function of what was to become the Gordon Center for Integrative Science, named after the family that owned Tootsie Roll Industries that was based in Chicago, IL (and that is why each morning a large bowl of Tootsie Roll products appears in the entryway to the building). All went seemingly well until it was time for final acceptance. The underground laboratories that were specified to meet nanoscience vibrational and acoustic specifications on two subterranean floors had failed the final tests. I remember all too well needing to explain that this was a critical flaw to then Dean Bob Fefferman and others in the upper administration. The recommendation was that the move-in be stopped in full

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until these shortcomings could be corrected. After nearly a year and considerable expense essentially all of the laboratories met spec and were ready for occupancy. In the end all fell into place nicely, and the building has now been occupied for just over nine years with microscopes capable of atomic resolution and femtosecond lasers all working quite well in their current home. During that time I was involved in the hiring of six new faculty spanning chemistry and physics, and am delighted that all in that superb cohort of young scholars are now tenured professors. I was also involved as Director in the aforementioned MURI Center on Materials Chemistry in the Space Environment, as well as a recent NSF Center for Chemical Innovation: The Center for Energetic and Nonequilibrium Chemistry at Interfaces, CENECI. I found these two group endeavors to be some of the most pleasurable and productive scientific interactions that have occurred during my years at Chicago. I had recruited many of the leading interfacial scientists, dynamicists, and materials chemists in the nation to participate in these centers. Such activities, which in essence gather together a relatively small group of researchers to focus their complementary skills and interests onto common problems, are a very powerful route to advancing science. I look forward to helping to create and participating in such stimulating and effective collaborative efforts in the future.

My most recent involvement in such matters was when President Robert Zimmer and Provost Tom Rosenbaum asked me to chair the two committees that ultimately led to the creation of the university's first engineering component, now called the Institute for Molecular Engineering (IME). This was a much-needed addition at Chicago as prior to this we only had departments of basic science. At the nano and atomic scales, for physical, biological, and medical studies, it was clear that the boundaries separating fundamental science from applied science and engineering were merging, indeed vanishing. The proposals were accepted by the faculty governing body, the Council of the Faculty Senate, and then by the Board of Trustees. This fall the new Eckhardt Center will open as the home of the IME. The IME is off and running, with Matt Tirrell as its founding Director and a goal to grow to 25 faculty over the coming years. Following the recommendations of our committee, it is organized thematically into research themes, and I have had the pleasure to serve as inaugural Director of the university's IME Water Research Initiative. I believe that the creation and management of precious fresh water resources will be a defining issue for the next generation and am glad to have the opportunity to help launch our activities in this critical area of endeavor.

I wish before concluding to extend my thanks and appreciation to Maria Jimenez and Tanya Hagerman for their many years of dedicated service as my group's administrators at the university. I also thank John Phillips for his years of dedication as building manager of the James Franck Institute in originally the Research Institute Building, and now the Gordon Center.

Although the prime focus of this short memoir has been on my scientific voyage, it would not be complete without citing the joy that has been brought into my life by my loving wife, Linda. We have also been blessed with our two wonderful daughters Leah and Leslie, who have—and continue to—enrich our lives daily and immeasurably.

I look forward to seeing this journey continue for many more years. It is after all the journey, not the destination, that matters most as we travel along the river of life. I cannot wait to see what tomorrow has in store.

Steven Jay Sibener

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