

# CHEMISTRY AT STONY BROOK

From SUCOLI to SBU

A Memoir

by

Francis T. Bonner

## Acknowledgement

This memoir has been constructed primarily from my own recollections of events during the formative years of our University and its Chemistry Department in the long ago days of Oyster Bay and Stony Brook. Sidney Gelber's book, *Politics and Public Higher Education in New York State - Stony Brook, a Case History*, has been an invaluable reference source, particularly in the reconstruction of time lines.

I am grateful to my wife Jane Carlberg, who as First Reader provided helpful criticism and advice every step of the way. I wish also to express deep appreciation to three friends and colleagues, William le Noble, Robert Schneider and Robert Kerber, who read the memoir chapter by chapter and gave me both corrections and valuable suggestions enriched by their own recollections of the early days of Stony Brook. Finally, many thanks to Kristen Nyitray and Jason Torre, of Special Collections & University Archives, for their helpfulness, in particular for unearthing several relevant historical photographs.

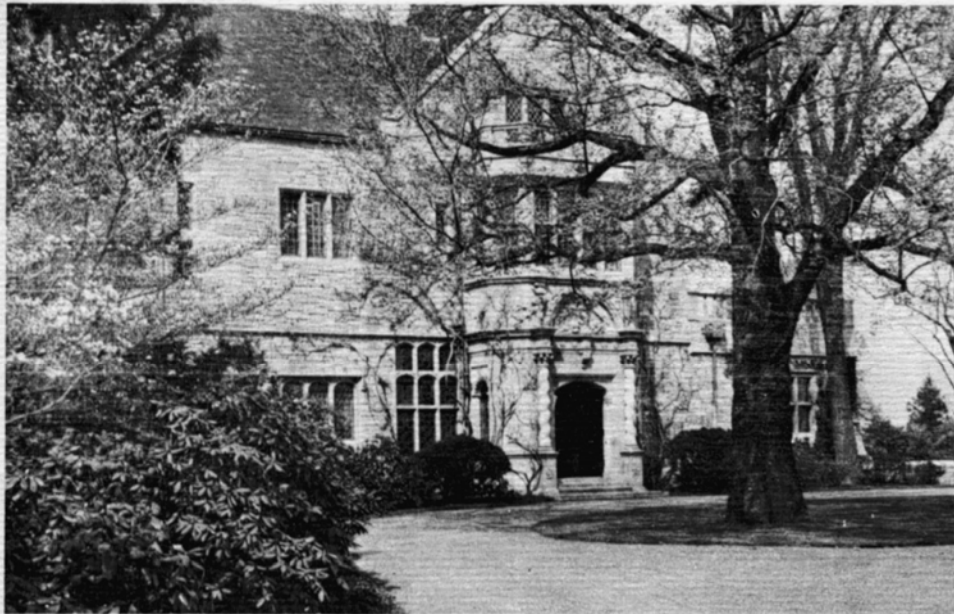
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# INDEX

Chapter	Page
I Prologue	5
II Planting Fields	15
III Trouble in Paradise	25
IV President Lee	31
V The Final Year at Oyster Bay	41
VI The New Campus at Last	51
VII Time Out	63
VIII Bright Future Assured	73
IX Campus Life in the Sixties	81
X End of the Decade	87
XI Epilogue	99



STATE UNIVERSITY OF NEW YORK  
announces  
the opening of a new college  
STATE UNIVERSITY COLLEGE ON LONG ISLAND  
at Oyster Bay  
in the fall of 1957



*Main entrance to the classroom and administration building.*

On September 17, 1957, State University College on Long Island officially opened its doors and began the fall 1957 semester.

## PROLOGUE

The institutional life of Stony Brook University began in 1957, under the name State University College on Long Island, at a temporary campus in Oyster Bay. I joined the SUCOLI faculty the following year, and served as founding Chair of the Chemistry Department during its first twelve years of existence, through the academic year 1969-'70. Our first four years of departmental life, at Oyster Bay, were followed by the University's move to Stony Brook in 1962. By 1970 we had grown from our original size, three faculty members with three undergraduate chemistry majors and one secretary (shared with Physics), to a size of thirty faculty, 70 graduate students, 25 postdoctoral research associates and 30 support staff members, with teaching responsibility for the needs of a substantial body of undergraduate chemistry majors and others in related programs, in addition to our own graduate students. I will attempt in this memoir to relate how this rapid and successful development took place. Since I led the Department through that formative twelve year period, telling about it is inevitably autobiographical. That being the case, I shall begin here by recalling some of my own personal history and experience during the years leading up to my arrival in Oyster Bay in 1958.

I graduated with a BA from the University of Utah in 1942, just a few months after Pearl Harbor, and had been accepted for graduate study at Yale, with the support of a teaching assistantship. After the war began Yale announced its implementation of an accelerated academic schedule, and for that reason I traveled from Salt Lake City to New Haven in June, since course work and teaching were set to begin in July. New Haven was a "far east" destination for me, since at that time in my life I had not gone beyond Evanston, Wyoming in that direction. I had been granted a draft deferment, because there would be naval officer trainees among the undergraduates I was scheduled to teach. In choosing Yale for graduate study, I had been drawn by the presence there of the well known physical chemist Herbert Harned, and it was soon determined that he would be my PhD research advisor. During my first weeks at Sterling Chemistry Laboratory I became fleetingly acquainted with Raymond Davis, a Harned student just then completing his PhD studies. A reserve officer, Davis was about to depart for military duty, and it was his laboratory that I would shortly "inherit."

During my second year at Yale, Professor Harned was urgently asked to carry out a feasibility study of uranium isotope separation by solvent extraction. The request came from Professor Harold Urey of Columbia University, where urgent and top secret Manhattan Project research was under way. A single mass spectrometric measurement at Columbia had suggested a small possibility for

separation of the fissionable form U-235 from the dominant isotope U-238 by this method. Under Harned's direction, assisted by Bernard Weinstock from Urey's group, I was one of a small group of Yale graduate students that carried out a rapid and intensive experimental program to test this possibility. The results were entirely negative, and it was concluded that the single positive observation at Columbia had been an artifact. "In those days", I heard it remarked maliciously, "Urey thought God separated isotopes." It was through this experience that I first learned about the Manhattan Project and the possibility of a nuclear weapon.

Under the wartime accelerated schedule, Yale was operating on a three semester academic year. With substantial course work, combined with TA duties in the physical chemistry lab course, it was a busy time, to say the least. After I completed the required courses and comprehensive exams, I was able to devote full attention, day and night, to my dissertation research on the thermodynamic properties of carbonic acid in aqueous solutions of sodium chloride. In the spring of 1944, when the experimental work was nearing completion, my draft board informed me that my deferment had been canceled, and summoned me for a pre-induction physical exam. The circumstance that my draft board was in Salt Lake City but I was in New Haven, and that all correspondence between us took place by slow surface mail, materially assisted my dissertation efforts. Directed to come to Salt Lake for the physical, I wrote back to request a change of location to New Haven. After I passed the delayed physical exam in New Haven I was again summoned to Salt Lake, this time to be inducted. I wrote again, and when at last I received notice from the draft board directing me to appear for induction at a certain location in New Haven on a certain Saturday morning, my dissertation was completed and ready for submission. But shortly before that certain Saturday arrived I received a cryptic call from Columbia University, asking me to come there for an interview. As I suspected might be the case, this turned out to be the Manhattan Project. I was interviewed by Willard F. Libby, who then asked me to report for work at Columbia on the following Monday morning. When I told him I had been ordered to report for military duty on the Saturday prior to that Monday, he replied: "Just don't show up. We'll take care of it." When I went to say goodbye to Lars Onsager, one of my favorite professors, I found him with his feet up, reading my dissertation.

The Columbia branch of the Manhattan Project, officially called S.A.M. Labs, for "Special Alloy Materials," was dedicated to research on the gaseous diffusion method for uranium isotope separation. The plant, then called K-25, was under construction in Oak Ridge, Tennessee, while major supporting research was carried out in New York. Much of the work in Libby's group was devoted to the development of metallic materials suitable for use as diffusion barriers for the volatile compound uranium hexafluoride ( $UF_6$ ), and investigation of the interactions of these materials with  $UF_6$  itself and with the related corrosive gases fluorine ( $F_2$ ) and hydrogen fluoride (HF). Located in the upper floors of the Pupin Physics building, we were a dedicated and hard working group. I learned a lot at the outset from two particular members of the group, John Casper and Walter Roth, and enjoyed close working and learning relationships with several

others, particularly including Leonard K. Nash, a recent Harvard PhD graduate who returned to a faculty position there after the war. In the spring of 1945 we moved from the Pupin building to new quarters in a former automotive center just north of the Columbia campus, called the Nash Building. In the late days of the project there, I worked closely with Jacob Bigeleisen and had my first experience with the use of radioactive tracers when we carried out some isotope exchange experiments employing fluorine-18 produced in the Columbia cyclotron.

Because the Manhattan Project was tightly compartmentalized I knew nothing about Los Alamos, or nuclear reactors, or plutonium. I was therefore greatly surprised on August 5, 1945, when I was on a home visit in Salt Lake City, it was announced that the first atomic bomb had been deployed over Hiroshima. While we knew at S.A.M. that the K-25 plant in Oak Ridge was in successful operation, it seemed not have been long enough to produce enough fuel for a bomb, and I had no idea that there were other options, e.g. the Calutron at Oak Ridge for U-235 separation, and the nuclear reactor at Hanford for plutonium-239 production.

After the war ended with the Japanese surrender, the project continued for several months at a greatly reduced pace. While devoting careful consideration to my next career move during this period, I also participated actively in the "atomic scientists" political movement that arose spontaneously at most branches of the Manhattan Project, whose immediate objective was to secure civilian control of atomic energy. We called our organization the Association of Manhattan Project Scientists, New York Area, "AMPS" for short. A parallel organization of non-Manhattan Project scientists soon formed, calling itself the Association of New York Scientists, or "ANYS." In the fall of 1945 I represented the AMPS at a landmark meeting in Washington that gave birth to the Federation of American Scientists. The physicist Melba Phillips was there also, representing the ANYS, and our long and close friendship dated from that occasion.

After prolonged pondering of the several options that became available to me, I accepted a research appointment at Clinton Laboratories, in Oak Ridge, Tennessee. Clinton Labs, the primary nuclear research center in wartime Oak Ridge, was renamed within a few years to become the Oak Ridge National Laboratory. Quite separate from gaseous diffusion research and the K-25 plant, its facilities included a graphite-moderated nuclear reactor. The move to Clinton Labs offered a learning opportunity, in what was for me a vast new realm of knowledge.

At the time of our relocation to the Nash Building in the spring of 1945, a fellow Manhattan Project worker named Evelyn Hershkowitz was transferred from an electron microscopy group in the basement of Pupin to an analytical laboratory near my own new work space. Separated by eleven floors in the Pupin building, neither of us had known the other existed, but in the Nash Building we became quickly acquainted, increasingly close, and inseparably so

within weeks. Evie and I were married on January 17, 1946, and departed for Oak Ridge a few weeks later.

The atmosphere at Clinton Labs was very education oriented, offering an abundant opportunity to advance my knowledge of nuclear science with lectures, seminars and classes. The lab was also very much in flux at that time, with numerous people leaving to pursue graduate studies or academic appointments elsewhere, while others were arriving. As a member of Ray Stoughton's research group, I overlapped with several of its departing members for just a few months, among them Daniel Koshland, later the long term editor of Science magazine, his talented wife Muriel (Bunny), John P. Hunt, later of Washington State, and Joseph Halpern. The Nobel laureate Eugene P. Wigner, who had come to Oak Ridge from Princeton to be Director of Clinton Labs, was a very open and accessible presence, and a source of great stimulation. One of Wigner's protégés, Leonard Eisenbud, who arrived with him, contributed to the educational atmosphere by offering a series of extraordinarily lucid lectures on quantum mechanics. My wife Evie and I soon became close friends with Leonard and his wife, Ruth-Jean. In the laboratory I learned a lot about handling radioactive materials as I pursued a number of radiochemical research topics concerning fission product nuclides, uranium and thorium decay chain products, and neutron capture cross section measurements employing the Oak Ridge reactor.

Evie enjoyed the life of a nonworking woman for several months, but before long became restless and applied for a job at the lab. One day I was summoned to the office of the Department Chair, Jerry Coe, who informed me in the presence of my supervisor Ray Stoughton that they wished to offer a position to my wife, but found upon evaluating her experience record that she was eligible for a salary higher than mine. "So if you have no objection," said Jerry, "we'd like to raise your salary." Those were the days. Neither of us objected, and Evie went to work in a pilot plant operation where she could apply her analytical skills. After some six months on that job a need arose for a person with technical training to work in the library. She was offered an opportunity to give that a try, and did so productively and enjoyably.

After some months more than a year in Oak Ridge, when the novelty of living in eastern Tennessee had begun to wear off, I learned there were plans afoot to establish a new national laboratory on Long Island, in the township of Brookhaven. From the beginning of my time there, the atmosphere at Clinton Labs had had an unsettled feeling about it, and scientific staff departures continued to be frequent. The laboratory was managed by the Monsanto Chemical Company, whose executives had not articulated a clear and attractive vision of the lab's long term future, and it became known that Dr. Wigner would not be staying on. When I learned that the Chairman of Chemistry at the new Brookhaven National Laboratory, Richard W. Dodson, would be attending a national meeting of the American Chemical Society in Atlantic City, I made arrangements to meet him there. In the interview, Dick Dodson projected a very



persuasive and attractive picture of the plans for BNL, and I was glad to accept his offer of a virtual ground floor appointment in the new Chemistry Department. At about the same time my friend Leonard Eisenbud accepted an offer to join the new BNL Physics Department, and another close friend and colleague, Edward Shapiro, a physical chemist who had been at Clinton Labs throughout the war years, also accepted an appointment.

Evie and I departed Oak Ridge in September, 1947 for Long Island, starting off with a touristic automobile trip to New Mexico and Arizona, north to Salt Lake City to visit my parents, then eastward, eventually arriving at Brookhaven in mid October. Dick Dodson had been very successful in his recruitment efforts: several new staff members were already there, and new arrivals seemed to occur nearly continuously after that. My friend Oliver Schaefer and I arrived virtually simultaneously. The Eisenbuds with their newborn son David, and Toni and Ed Shapiro with their three boys, were all there ahead of us, living in newly purchased houses in Patchogue. We found an attractive over-the-garage apartment in the village of Medford, at that time a quiet, rustic suburb of Patchogue. The Schaefer family settled nearby, and Oliver and I began carpooling to BNL via Yaphank. Work was underway at the lab to create a Chemistry Department facility out of an array of narrow, one story buildings, left over from the site's military days, as Camp Upton, and several of us went to work immediately on plans for the various components needed for this complex. Buck Rubinson and I worked together on plans for a radiochemistry facility that was in due course successfully created.

John Turkevich, a physical chemistry professor at Princeton, owned a vacation home in the north Brookhaven area, and had developed a relationship with the new BNL Chemistry Department during the previous summer. At Dick Dodson's suggestion, John approached me to propose a research problem that seemed so right for me that I lost no time to begin working on it. The project was an exploration of the mechanism of the gas-solid interface reaction between carbon dioxide and graphite to form carbon monoxide, employing CO<sub>2</sub> gas labeled with carbon-14 as a tracer. I had an enjoyable relationship with Turkevich, and an equally enjoyable time working on this project during the winter and spring months of that year (1947-8). Our thoroughgoing mechanistic study of this industrially relevant reaction has stood up well to the test of time.

While still at Oak Ridge, I had begun to hear disturbing accounts about lab employees encountering security clearance problems. We had all received the necessary clearances during the war, but now the FBI was around asking new questions, and it appeared that an entirely new round of clearance procedures was under way. Some employees were asked to respond to "interrogatories" about their political beliefs and associations, and I heard that in one such case the employee was informed that his landlady at a previous address had reported finding copies of the communist publication *New Masses* in the trash bin. I had not yet heard of Senator McCarthy, but it became clear in retrospect that these were early intimations of the McCarthyite times to come. Upon arrival at BNL I

was informed that I would require “Q” clearance, and made aware that my clearance worthiness would be up for reevaluation. I hadn’t been there very long when I was shocked to learn that Leonard Eisenbud’s “Q” clearance was in jeopardy: he had received an “interrogatory” asking him to respond to a broad range of questions about his and his wife’s political affiliations. It was no secret that Ruth-Jean’s mother and sister were members of the Communist Party, and the charges against Leonard began and wandered off from there. Indignant and dispirited, and believing the probability of his receiving clearance to be vanishingly small, Leonard chose not to pursue it to the stage of formal hearings and left to accept an appointment at the Bartol Research Foundation in Swarthmore, PA. Within a few weeks of Leonard’s receipt of charges, our friend Ed Shapiro was called up on the clearance carpet in a very similar manner. Some of the charges in Ed’s case were on the level of the landlady searching the trash basket, and all were outrageous and insulting. He elected to respond to the charges in a formal hearing, but had no desire to stay on in a job where that could happen, and began immediate arrangements to depart. In the end it appeared likely that his clearance would be restored, but Ed left anyway, also to the Bartol Foundation.

Within six months of our arrival at BNL, then, our close friends were gone, I was the only remaining member of our Oak Ridge triumvirate, and it seemed entirely possible that I might also encounter a clearance problem. With my sense of permanence at BNL compromised, I began increasingly to think about and keep an eye open for other possibilities. Among these, academic appointments were of greatest interest to me, and some time in the spring I was offered an assistant professorship at Brooklyn College. There were several reasons for us to welcome a period of living in New York City, and when the time came for me to make the decision I still had heard nothing about my “Q” clearance. I discussed the situation with Dick Dodson, who was supportive of either decision. It seemed clear that I would be able to maintain a close relationship with BNL if I did go to Brooklyn College, and I decided to go. More than one year later I was informed by BNL that my “Q” clearance had been approved.\*

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\*My concerns had not been irrational, however: two of my brothers were subsequently subjected to clearance nightmares. My brother David, after several years on the biology faculty at Yale, accepted a major research appointment at Oak Ridge National Laboratory, and was then confronted with security clearance charges. He fought the charges successfully, then went to UC San Diego instead of Oak Ridge. My brother Walter, after accepting an appointment at the Smithsonian Institution in Washington, received an interrogatory in which (among other things) he was asked to identify which of his “relatives and associates” were “fellow travelers affiliated with communist causes.” He soon left Washington for Cornell.

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By the time of my departure the BNL Chemistry Department had added a number of new members, among them Gerhart Friedlander, who had been at Los

Alamos during the war, and came to BNL following a disappointing period at the General Electric Company. In addition to being a brilliant nuclear chemist, Gert is an accomplished pianist. I had been a serious violin student from an early age, and we began to get together frequently to explore the violin-piano sonata literature, and quickly developed a close personal and musical friendship.

Morris Perlman, who had been at both Los Alamos and GE with Gert, arrived at BNL only a few months after I left. Simon Freed and Norman Elliot, both former colleagues at Oak Ridge, also joined the BNL department soon after that, as did Jacob Bigeleisen, who had gone to the University of Chicago after our time together at Columbia during the war, and Raymond Davis, whose laboratory I had taken over when I arrived at Yale for graduate study.

In the late summer of 1948 we found a basement apartment in the Park Slope district of Brooklyn, and Evie found a new job as a science writer for the Funk and Wagnall's Encyclopedia. I had virtually no teaching experience, and had a rude awakening to the reality of a Brooklyn College 16 hour per week teaching schedule. I taught physical chemistry, lecture and laboratory, plus general chemistry in its various modes. Brooklyn College did not have a graduate program beyond the MS level at that time, but it did have the saving virtue of a very high quality undergraduate student body. A number of the students I was privileged to teach during those years went on to stellar careers. I taught an evening graduate course in nuclear chemistry, using the then newly published textbook by Gerhart Friedlander and Joseph Kennedy. While I had a generous sized laboratory, and directed several MS thesis projects, I found it discouragingly difficult to carry out research, given the heavy teaching load. I maintained close communication with my friends at BNL, and returned there from time to time, most notably for a full summer period in 1951, when I carried out some research with Jake Bigeleisen employing nitrogen-15, a stable isotope that played an important role in much of my subsequent research.

One of my colleagues at Brooklyn College was the physicist Melba Phillips, whom I had known since 1945, and greatly respected and admired. She had earned her PhD at UC Berkeley with Robert Oppenheimer, and was well known as the coauthor of a theoretical paper interpreting deuteron polarization, a phenomenon that has been known ever since as the Oppenheimer-Phillips effect. In the very early '50's Melba and I, with other colleagues from both Physics and Chemistry, and one or two from Geology, set out to develop a new interdisciplinary course in physical science for non-science students. This project was proceeding very well, and was at a fairly advanced stage when, in the fall of '52, McCarthyism struck again. Melba was called before the US Senate's Internal Security Committee, chaired by Senator Pat McCarran of Nevada, where she invoked the fifth amendment repeatedly in non-response to questions about her own and her friends' and associates' political affiliations and beliefs. Since the New York City Charter contained a provision, originally intended to combat governmental corruption, that automatically terminated the employment of any City employee who invoked the fifth amendment, Melba suddenly found herself

unemployed. No one in the Brooklyn College administration or the NYC government said one public word about it at the time, and it was not until 1987 that the College at last made a public apology, and named a fellowship in her honor. It was the fact of Melba's stark unemployment in 1952 that led to our collaboration in writing the successful textbook *Principles of Physical Science*, published in 1957, and in a revised edition in 1971, by the Addison-Wesley Publishing Co.

Prior to Melba's firing I had been awarded tenure at Brooklyn College. I was still an assistant professor, however, since tenure was not coupled with promotion in the City University at the time. My promotion to tenure had occurred so easily that I warned myself not to allow it to become a life sentence. The trauma of Melba's firing brought added impetus to my growing desire to go elsewhere, and during the following year I was granted a one year Carnegie Foundation Fellowship at Harvard. In the summer of 1954 we closed out our apartment on Ocean Parkway in Brooklyn and moved to a rented house in Winchester, MA, with our son Michael, now two years old. One of the benefits of living in the Boston area was that we were reunited with our dear friends Ed and Toni Shapiro and their three sons, now living in Newton. Ed had gone to the Bartol Foundation from BNL, but was now working at the Tracerlab Company. A few years later, while we were still in the Boston area, Ed founded a company of his own, the New England Nuclear Corporation, which was a great success from its very first day, and was eventually bought by the Dupont Company.

My fellowship was related to the Carnegie Foundation's contribution to general education in science, and brought me into participation in a Harvard course then being taught by Leonard K. Nash, my friend and former Manhattan Project colleague. The students were all non-science majors meeting a science requirement, and the course dealt with a series of concepts of importance to modern chemistry, each receiving in-depth treatment in relation to its scientific content and historical and philosophical origins. I taught a regular section, gave some of the lectures, enjoyed working with Len Nash and his eager group of TAs, and it was a stimulating and profitable experience. I also enjoyed being in residence in the Harvard Chemistry Department, and getting to know several of the outstanding and stimulating scientists there, including Geoffrey Wilkinson, his graduate student Albert Cotton, and Eugene Rochow.

During the Harvard year I searched diligently for a new academic appointment for the following year. Several attractive opportunities became available to me, but unfortunately no one was able to meet my minimum salary demand of \$6000 per year. Academic salaries were notoriously low everywhere at the time, and I had been "spoiled" by the somewhat higher than average salary scale then prevailing in the New York City system. In addition, our needs were greater because we now had two children: our daughter Alisa arrived in March, 1955. On Gene Rochow's recommendation I decided to take a close look at the Arthur D. Little Company, and soon after interviewing there I was made an offer that I found very hard to refuse. In the spring of 1955 I resigned from Brooklyn

College, resisted their efforts to persuade me to return, and in the summer took up my new duties in A.D. Little's Acorn Park campus, on Route 2 in Cambridge, not far from Arlington.

The Arthur D. Little experience, carrying out industrial research under contract, was quite fascinating. It was like having a picture window to observe a wide segment of industry and its technical problems, without need to assume any particular corporate commitment. The research projects I worked on under contract at A. D. Little included such topics as color television screen production, high resolution photographic film development, operation of nickel-cadmium batteries in sealed condition, and continuous analysis for uranium in a proposed nuclear reactor design incorporating nuclear fuel in liquid metal solution. After one year, I considered my new professional life to be going so well that we bought the first house of our married life, a nice colonial in Lexington. Our third child was now on the way.

In the fall of 1957 I received a letter out of the blue, from a man named Leonard K. Olsen, who identified himself as the Dean of a new college in the State University of New York system. The college had just that fall opened its doors to a small entering class on a temporary campus in Oyster Bay, Long Island, and plans were under development for a permanent campus further east, in the village of Stony Brook. Dean Olsen wrote that he had recently visited Brookhaven National Laboratory to seek advice concerning a senior faculty appointment in chemistry, and my name had been strongly recommended to him by a man named Gerhart Friedman. While I did know a man named *Lew* Friedman in chemistry at BNL, I thought it likely that Olsen meant *Friedlander*, not Friedman, which indeed turned out to be the case. It was a busy time, and my first inclination was to forget about it. We hadn't greatly enjoyed living in Medford during our previous year on Long Island, and didn't feel any compelling desire to go back. It was true, however, that Oyster Bay and Stony Brook were located in an entirely different region of the Island that we knew little about. While still undecided about my response to Olsen's letter, I received a severe dressing down from an ADL vice president about a budgetary over-expenditure issue. I had indeed overspent one of my contract budgets, but in a manner and for reasons I considered to be essential. The incident had not cost the company one dime beyond its high overhead rate, and seemed almost certain to lead to a much larger follow-up contract (which it subsequently did). As my success level with the company had grown, I came under increasing pressure to keep new contracts coming in, which meant ever increasing travel commitments. Now I seemed to be receiving a message that technical matters should be considered secondary to financial concerns. After a few more days of rumination I accepted Dean Olsen's invitation to visit the new State University College on Long Island, in Oyster Bay.



## PLANTING FIELDS

The temporary campus in Oyster Bay, called Planting Fields, was the estate of the late William R. Coe, who had willed it to the State of New York. Oyster Bay includes a portion of the so-called “Gold Coast” of the Island’s North Shore, a region of splendid estates belonging to wealthy residents. Among these, Planting Fields had ranked among the best. Mr. Coe arrived in the US an impoverished immigrant from Scotland, and after acquiring great wealth he appears to have built the estate that he would have wanted to build back in the Old Country had he stayed there. The centerpiece, Coe Hall, is an authentic Tudor mansion, whose Great Hall includes a fireplace large enough, as one visitor presciently remarked, to roast a faculty member whole. The grounds are beautiful, including breathtaking greenhouse displays and a magnificent arboretum. The College held some classes in Coe Hall, others in a series of prefabricated Butler buildings, and later on in an array of geodesic dome structures. The Coe Estate’s extensive stables were converted from equine to student residential use on a temporary basis. On my first visit I was struck by the sheer beauty of the surroundings, and enjoyed being back in a setting in which young people, apparently happy young people, were in the majority.

Leonard Olsen turned out to be a fine looking, charming and articulate man. He came to New York from the University of Chicago, where he had taught in the general education program and pursued graduate study in philosophy. As an assistant to William S. Carlson, then President of the State University of New York, he had been assigned to oversee planning and development for a new SUNY College on Long Island. The location of the new campus had been virtually determined by a gift to the State of a large and desirable tract of land in Stony Brook, from the millionaire shoe magnate Ward Melville. The time table for the project had been advanced in response to the Soviet Union’s launching of its Sputnik satellite, stimulating action to increase and update science and engineering education capacity within the SUNY system. Olsen came from Albany to Long Island to start up the operation in person, and the first entering class of 140 students had been assembled on very short notice in 1957. The official name of the institution was the State University College on Long Island, and its stated mandate was to train science and mathematics teachers and engineers. There was a clear implication in Olsen’s presentation, however, that this was just a beginning, that significant studies were under way, and an expected report on the future of the SUNY system would recommend development of the Long Island campus for major university status. That made good sense to me: the State University of New York, then only recently created

(in 1948), consisted primarily of four year teacher's colleges and two-year Agricultural and Technical colleges, located almost entirely in the upstate region. There was just one SUNY campus on Long Island, an Ag and Tech College at Farmingdale. There seemed a clear need for the SUNY system to develop a comprehensive campus that would fully include graduate education and research, for which the Stony Brook location could be ideal. In New York's traditional upstate-downstate political division, that would probably mean two such campuses. There were indications, or at least rumors, that the then private University of Buffalo was likely to be acquired by the State, and if that happened Stony Brook would be a logical location for its downstate counterpart.

Olsen showed a high level of interest in me and my qualifications, and in turn I found myself intrigued by the possibilities at the new college. I met most of the seventeen faculty members who had come in Year One, whom I later fondly referred to as "the aboriginals." There was one chemist among them, Barry Gordon, who had come over from the Brookhaven National Laboratory Chemistry Department. There were two physicists, Richard Mould and Clifford Swartz. Cliff had also come over from BNL, and was continuing to maintain his relationship there in the Cosmotron Department. The lone Biology professor, Frank Erk, was also the designated Chair of the Division of Natural Science. In the organization then in effect there were three divisions, the other two being Humanities and Social Sciences. There was no departmental structure, but it was clear from the outset that if I were to come it would be in expectation that I would head and build a chemistry program, in a Chemistry Department.

Soon after this first visit, Dean Olsen offered me a full professorship, with the designation Acting Chairman of Chemistry, at a salary substantially equivalent to the level I was then earning at Arthur D. Little. He also offered to cover our moving expenses by the device of an early appointment: I was to go on the payroll May 1, 1958, and my official duties at Oyster Bay would begin the following September. After two more trips to Long Island and much deliberation, I accepted the offer.

Since we planned to move to Long Island in August, we had to put our Lexington house on the market and find a replacement for it down there. I had major commitments to my ongoing programs in Cambridge, and it became a very hectic period in our lives. I received frequent communications from Oyster Bay, and visited several more times. Certain aspects of the new college began to disturb me, in particular a penchant among some of the faculty for teaching subjects remote from their fields of specialization. I was subsequently surprised and disturbed to find that this particular "interdisciplinary" mode was promoted by Dean Olsen himself, when I received a communication from him suggesting that I might be asked to teach a mathematics course. Concluding that I had made a big mistake, I decided to resign the appointment at once, before it was too late. I communicated that decision to Dean Olsen, and he responded with an all-out effort to dissuade me. Cliff Swartz, with whom I had been in frequent communication and developed a good sense of rapport, asked me not to take any



irreversible action without first coming down to BNL to meet with George Collins. Collins, then Chairman of the BNL Cosmotron Department, was a member of the Council for the new college. (Every campus unit in SUNY has a Council of local citizens, appointed by the Governor and charged to look after the institution's well being.) As Cliff had asked, I visited and had a long conversation with George Collins. He appeared to be very well informed about and concerned for the college's future, and it also seemed clear that his hopes for that future coincided closely with my own. Given that impression, in combination with the fact that he had a voice in the situation and fully intended to use it, I felt sufficiently reassured to withdraw my resignation. Dean Olsen never again brought up the subject of cross disciplinary teaching.

As moving time approached we began house hunting in earnest. Evie managed to get down to LI by herself a number of times, and we went together whenever possible. We didn't know when the institution would move to the new campus in Stony Brook, but we did know that wouldn't happen for at least four years, and decided to focus our search in the region halfway between the two locations, which meant somewhere in Huntington Township. We had some surprising and sometimes disconcerting house hunting experiences. Evie had been taken by a real estate agent to a house in Huntington Beach that seemed promising, and asked me to come with her to take a look at it. The real estate agent set it up for us to meet with the big boss lady of the agency, who proceeded to zero in for the kill. Among her selling points, she told us that there was an especially attractive feature of the property called "deeded beach rights." I requested an explanation, and she responded by saying that if we owned the house we would not be allowed to sell it to "undesirables." After I asked her what *that* meant, she replied: "Well, should I say - heh, heh - the community is 100% Christian?" Evie and I stood up instantly, and I said: "Then that lets us out because my wife is Jewish." As we hastily departed, the agent called out to her: "It's alright if your husband is Christian!" That was not the only instance of anti-Semitism we encountered during that time, and I realized that it may have been at least part of the reason why so many Brookhaven Lab personnel had settled on the South Shore during its early days. Much of the North Shore, in addition to being generally wealthy, had a reputation for being traditionally anti-Semitic and anti-black, and many North Shore real estate contracts and property rights were set about with protocols and covenants, of which "deeded beach rights" was one example.

Later on during our house hunting, an agent that Evie had grown to like surprised her by remarking about a client couple that they were "pushy Jews, you know the kind." Evie said "Careful, darling, you're talking about my mother and father." The agent, chagrined, ashamed and appearing to be genuinely remorseful, did her utmost to find just the right house for us. Having found it, she even slashed her own fee to make it possible for us to buy it. "It" was an old farm house on Stratford Avenue in the village of Greenlawn, very nearly halfway between Oyster Bay and Stony Brook. Because the sellers allowed us an uncomfortably short period of time to secure financing, I called Dean Olsen and

asked whether he thought Ward Melville would be willing to help us. I knew that Melville, the multimillionaire donor of land for the Stony Brook campus, was now the Chairman of the College Council, and actively interested in the new College's development and affairs. Soon after Olsen placed a call to him, Melville personally visited and appraised the Greenlawn property, and our needed financial commitment was secured within 48 hours. A curious footnote to this event was that among the numerous payments we were required to make at the closing was an item identified as "Appraiser's Fee."

In addition to being comfortable and roomy, our new dwelling came with more than two acres of land, and turned out to be a very happy place to raise children. Michael, Alisa and Rachel were 6, 3 and 1 when we moved into the Greenlawn house, and we lived there for fourteen years before eventually moving to Setauket and its much more convenient proximity to the campus.

A few weeks after I accepted the appointment at Oyster Bay I received a letter from my friend Leonard Eisenbud, who was still at the Bartol Foundation. We had not been in touch for some time, and I was pleased to hear from him. I was even more pleased and excited by the content of his letter. He said he'd heard on the grapevine that I was going to join the faculty of some unheard-of new college on Long Island; would I please tell him about it? He might be interested in joining us there. I told him about it, then immediately told Dean Olsen about him, stressing my strong belief that Leonard would be a major asset to the College if he could be persuaded to come. Within weeks, Leonard was signed up to join the faculty as Professor of Physics and Acting Chairman of the Department of Physics. Leonard and Ruth-Jean were soon on Long Island looking for real estate, and the four of us were delighted to be reunited.

During my frequent visits to BNL in the early '50's I had come to know and admire a young man named Joseph Silverman. Joe was then a Columbia graduate student, doing his PhD research at BNL with Dick Dodson, where he had carried out some ground-breaking research on electron transfer exchange reactions. When I encountered him again in the spring of 1958 he was working for a small company on Long Island that was gearing up for industrial scale application of irradiation technology. Joe seemed quite intrigued when I told him about the new College on Long Island, and it struck me that he would be a fine addition to our faculty. Dean Olsen interviewed him, agreed with me, and when Joe was then offered and accepted an associate professorship I was delighted to know that we would be starting out in the fall with a three man Chemistry Department!

September came, and the fall semester at State University College on Long Island began. Both its faculty and student body were roughly doubled in size from the previous year. Among the new faculty arrivals were Sidney Gelber, from the Columbia Philosophy Department, where he had been both a student and a colleague of Justus Buchler, and William Lister, a mathematician arriving from a faculty position at Brown. Joe Silverman, Barry Gordon and I had our

offices, departmental headquarters and teaching laboratories in one of the several temporary buildings that had been rapidly put in place to get the College off to a quick start. These were the so-called Butler buildings, which can be assembled nearly as quickly as Quonset huts, but are rectilinear and much more substantial. A large number of Geodesic Dome structures were installed the following year. Most of our teaching that first year was related to a survey course, Natural Science I and II, intended to be a cornerstone of the general education curriculum. I gave many lectures in the Great Hall of Coe Hall, the Tudor mansion, and Barry and Joe taught sections. A laboratory program was initiated, and evolved as the academic year progressed.

I recall little about my teaching experiences during that first year, in contrast to two other activities that fully absorbed my attention. The first of these was faculty recruitment, and the other was planning for the new chemistry building due to be built at Stony Brook. President Carlson and the SUNY Trustees, anxious to get a fast start on the new campus, had engaged one of the largest architectural firms in the country, Voorhees, Walker, Smith, Smith and Haines, based in New York, to design the first round of buildings. Chemistry was designated to be part of this first round, and when I arrived in Oyster Bay I learned that plans were already advancing rapidly. I met Mike Golicki, the architect in charge of the Chemistry building plan, and the two of us soon embarked upon a productive and pleasant collaboration.

Concerning architectural aesthetics, and even basic design, there was little discussion to be had: Mr. Melville had requested that the campus buildings blend well with his beloved Revolutionary era Stony Brook Village, and the first buildings were to be rendered in a red brick Voorhees Walker version of Georgian Colonial. But there was a lot to discuss about interior layout and space allocation. Mike was very receptive to my suggestions and quickly caught on to the kind of building facility I thought we should have. The building was being designed as a teaching facility, with a large lecture hall, and generous faculty office and teaching laboratory space. Looking ahead to what I felt sure would be our future, I was anxious to get as much research laboratory space included as possible. However, the specifications given by the Department of Public Works (DPW), the State agency then responsible for all building facilities, contained no provision for research space. I was also anxious to have services and facilities adequate and appropriate for a research oriented department. This would include a central distilled water distribution system, for example. Mike and I met often at Oyster Bay to discuss the plans, and after each of our meetings he sent his latest sketches to Elwin Stevens, then representing the DPW, and later, after its creation in 1962, the State University Construction Fund. To my dismay and Mike Golicki's frustration, Mr. Stevens invariably responded by either changing or crossing out many, sometimes nearly all, of the items and features I had requested. Frustrated, Mike arranged to get all three of us together at a meeting in Albany, and at a crucial juncture asked: "Mr. Stevens, who's going to operate this building, you or Dr. Bonner?" That had the desired effect, and explains how we got central distilled water and many other essential features in our first

building. Unable to get as much faculty-designated research space as I thought would be necessary, however, I resorted to the subterfuge of spotting spaces vaguely designated "Preparation Room" throughout the building. This concept was accepted without question, and as it worked out we started life in the new building well housed for both teaching and research, and owing a big debt of gratitude to Mike Golicki for making that possible.

I believed that nothing could be more important in shaping the future of our new Department than the successful recruitment of well qualified faculty colleagues, an issue of significance even greater than that of building design. Dean Olsen allocated three new faculty lines for Chemistry, one of which could be filled at the full professor level. Since the first three members, Joe, Barry and I, were all physical chemists, I assigned first priority to recruitment of an organic chemist, of the highest possible stature. I pursued this objective in a variety of ways, by correspondence and telephone calls, following up personal lines of communication, and attending national ACS meetings. I was going to BNL on a near-weekly basis at the time, and on one such occasion I had a long conversation with Alfred P. Wolf. Al was a full member of the BNL staff, but had come from and remained closely connected to Columbia. He strongly recommended a man named Fausto Ramirez, who had just that year departed the Columbia faculty for Illinois Institute of Technology in Chicago. His publication record was very substantial, and he was well known in his field of synthetic organophosphorus chemistry. Al expressed high regard for him, and surprise that he had not been granted tenure at Columbia. After gathering as much further information about Ramirez as I could, I decided to go ahead with an approach and invited him to visit Oyster Bay. Fausto made a very strong impression on interview, his publication record was outstanding, his references were excellent, and it seemed clear that his appointment would do a lot to help us get the new Department off to a good start. I went all out to persuade him to accept a full professorship at our virtually unknown institution, and we were delighted when he agreed to do so. Joe Silverman voiced an opinion that this appointment would be "the making of the Department."

With the Ramirez appointment secured, our search for a second organic chemist, at the assistant professorship level, was greatly facilitated. We received quite a few applications and had an outstanding field to choose from. Of the five candidates we invited to visit, the one who made the strongest impression was William le Noble, who had earned his PhD in physical organic chemistry under George W. Wheland at Chicago, worked as a postdoc with Nathan Kornblum at Purdue, and was then employed at the Rohm and Haas Company in Philadelphia. Bill gave a very interesting talk about his plans for a research program in high pressure chemistry, to explore the effects of pressure on reaction rates, assess the potential importance of the then little applied parameter *volume of activation* in interpreting reaction mechanisms, and beyond. He was able to tell us exactly how he intended to begin this program and what he would need to get it under way. He made a very convincing case, both for the significance of the program and for his own capacity to pursue it. It was a great day for the

Department when Bill agreed to join us, and he built a very distinguished career by doing exactly what he said he was going to do.

I received a number of suggestions about possible physical chemistry candidates for the second assistant professorship appointment, and invited several to make interview visits to Oyster Bay. One of these was Ted Goldfarb, then in his final year of PhD research with George Pimentel at UC Berkeley. Pimentel and I had come to know each other during a period of collaborative editorial work, and I had high regard for him and for the technique of matrix isolation spectroscopy he had developed in his research program. I asked him to keep us in mind, and his answer was Ted, whom he recommended very highly. All of 24 years old when he came for interview, Ted was the only candidate that asked questions about retirement benefits, which was a bit embarrassing for me because I knew so little about them. He made an excellent impression, we liked his expertise in spectroscopy, especially in matrix isolation methodology, and were glad when he agreed to come aboard for the fall semester of 1959.

With our three new positions successfully filled, I thought the 1958-59 recruitment season was over, but little did I know. In April, at a rather advanced date in the second semester, Joe Silverman came to me with unexpected news: he had been offered an appointment in the Department of Chemical Engineering at the University of Maryland, and decided to accept it. Joe had been an excellent, compatible and productive colleague during the entire year, discharging his responsibilities with great energy and enthusiasm. I had come to rely increasingly on his good judgment and support, and this news came as a surprise, to say the least. He made it very clear that his desire to go to Maryland was not due to lack of confidence in the future of Stony Brook and its Chemistry Department. Rather, it reflected an increase in interest on his part in industrial applications of chemistry, resulting from his time in industry before coming to Oyster Bay. Joe Silverman and our fledgling Department parted ways amicably, and he has enjoyed a long, productive and successful career at Maryland.

Since it was now very late in the academic recruitment season, I felt dubious about our chances of appropriately filling Joe's vacated position in time for the 1959 fall semester. When I reported this development to Fausto, however, he brought up the name of a colleague at Illinois Institute of Technology, an inorganic chemist named Sei Sujishi, and suggested that I get in touch with him to explore his possible candidacy. I did so, found him interested, and arranged for an almost immediate visit to Oyster Bay. Sei, a native Californian, was among the many Japanese-American citizens that were relocated during World War II. In his case, relocation had resulted in his entering graduate studies at Wayne State University in Detroit, where his PhD mentor was the great Herbert C. Brown, later of Purdue University. In 1959 Sei had been at IIT long enough to become a tenured associate professor. He made an excellent impression during his interview visit, and from his CV, references and experience record he emerged as a very well qualified candidate indeed. We offered him an appointment at associate professor level. Sei accepted it without hesitation, and

we were happy to have our recruitment problem so satisfactorily resolved. We could now look forward to the doubling of our faculty, from 3 to 6, with four very promising new colleagues slated to arrive by September.

While I was busy seeking chemistry candidates, Leonard Eisenbud was of course equally busy recruiting physicists. The two who came in that very first year, David Fox and Herbert Muether, both became very long term, and very valuable faculty members. Leonard and I frequently participated in each other's interviews during those early years, and by circumstance I came to play an important role in the recruitment of David Fox. David, a victim of the early '50's loyalty oath fiasco at UC Berkeley, had been teaching at the Technion in Israel for several years. In 1959 he had been back in the US for just one year, at the Bausch and Lomb Company in Rochester. He made a very strong impression during his interview visit, and I enthusiastically endorsed his appointment. He was offered an appointment as associate professor. Shortly after receiving our offer he had been offered a full professorship, at substantially better terms, by nearby Adelphi University, and Adelphi was asking for a virtually immediate decision. Unable to reach Leonard, David called me about the situation. He made it quite clear that he would prefer to come to Oyster Bay, but since Adelphi's terms were so much more favorable he felt he could not afford to turn them down. Leonard was away and I couldn't reach him either, but since I had a strong feeling that bringing David to the College represented a major opportunity, I took it upon myself to get the terms of his offer improved. I was fortunately able to accomplish that, and Leonard and I were both very pleased when David agreed to come. His appointment was indeed a major opportunity for the College, and it turned out to be an important personal event for me as well, because within a very short period of time Evie and I counted David and Laura Fox among our closest friends.

The Chemistry and Physics departments shared one secretary, Margaret Smith, during that first year. An incredibly competent person, she was a valuable asset to both departments at the beginning. It was Chemistry's good fortune that Marge Smith became exclusively affiliated with us later on in Oyster Bay, and that she came out to the new campus with us in 1962. She contributed in a very major way to the Chemistry Department's success. Although she would have preferred to stay, in the long run we were unable to keep her, because her Civil Service rank level could not be raised as long as she worked for a mere department chairman. Eventually she left us, and enjoyed a long and successful subsequent career assisting the presidents of the new SUNY College at Old Westbury.

I hardly need emphasize how busy and occupied I was with administrative, policy and practical matters during the early days at Oyster Bay. When I arrived in the fall of 1958 there were no research facilities there, and with teaching added on I had little opportunity to establish a research program of my own, despite my determination to establish a strongly research oriented department. I had been warmly welcomed back to Long Island by my many friends at BNL, and it was

quite clear all around that Brookhaven Lab would be a major asset to us in the development of our programs. My friend Oliver Schaefer, who had arrived at BNL at the same time as I in 1947, invited me to join his research group as a part time participant, and in the fall when things had shaken down a bit I took him up on it. I did my best to set aside one day per week to spend at Brookhaven, and thoroughly enjoyed being there on the days when I could make it. Oliver had become deeply involved in geochemical research, and the project that we worked on together concerned the potential use of the long-lived, naturally occurring radioisotope chlorine-36 for dating applications, in lake waters of the Great Basin area. A publication resulting from this work appeared in the journal *Geochimica et Cosmochimica Acta* in 1961.

In addition to the pleasure of involvement in research at BNL during that early period, my frequent visits to BNL gave me needed relief from the pressures and sometime insanities of Oyster Bay. It was a pleasure to be there for scientific discussions with BNL friends and colleagues, and on occasion to seek information and advice. As I indicated earlier, the successful recruitment of Fausto Ramirez had its origin in a conversation with Al Wolf. I enjoyed keeping up with progress in the several other projects in geochemistry and cosmochemistry that were then ongoing at BNL, particularly including the very long term solar neutrino experiment being carried out by Ray Davis, whom I had fleetingly known upon my arrival at Yale, and taken over his laboratory after he left.





### III

## TROUBLE IN PARADISE

Soon after we moved into our new home in Greenlawn in the fall of 1958, we were pleased to discover that a colleague at the new College, Ralph Bowen, lived very nearby in the village of Northport.\* Ralph, a professor in the Social Sciences Division whose field of scholarship was European history, was an “aboriginal,” having joined the faculty in 1957. His degrees were all earned at Columbia, where he had also been a member of the History faculty for ten years and taught in its Contemporary Civilizations course (CC). Dean Olsen had recruited him from a tenured professorship at Elmira College, on the recommendation of Justus Buchler, a well known Columbia professor, and a key figure in its CC program. Ralph and his wife Sue were born and bred Vermonters, she the niece of the Republican Senator Ralph Flanders, and he a delightful curmudgeon in classical Vermont tradition. We quickly became good family friends, and Ralph and I regularly commuted together to Oyster Bay.

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\*Our Greenlawn house, on Stratford Avenue, was situated just south of Northport and very slightly to the west of East Northport, a location that I often identified as “Southwest East Northport.”

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During my first year at the College occasional faculty meetings were held in the Tudor mansion’s Great Hall, with Dean Olsen presiding. It became increasingly apparent that he had autocratic tendencies, and that his ideas about the future structure and nature of the College on Long Island were fixed and inflexible. In this vision, the general education curriculum was to extend through all four undergraduate years, in the form of required courses controlled and taught within the Divisions. The senior year was to include a capstone interdivisional seminar, in which the threads developed by divisional instruction would be brought together by interdisciplinary effort. Olsen was determined to retain the divisional administrative structure of the College, even though he had accepted the creation of departments in the sciences in view of the mandate to train science teachers and engineers. While I was not familiar at first hand with the general education program at the University of Chicago College, I did know that Olsen had been closely affiliated with it and with Robert Hutchins, its creator. It seemed clear indeed that his vision for the general education program, and for its supporting administrative structure at the new College on Long Island, was patterned closely after that of Chicago College in the Hutchins era.

Early in June, 1959 Dean Olsen informed me by telephone that I would be awarded tenure, effective July first, and formally appointed Chairman of the Department of Chemistry. While I was well aware that my initial appointment was for a three year term, I'd given little thought to or concern about tenure. While I was nevertheless pleased to have this question out of the way, the second item seemed more significant to me, because although initially designated Acting Chairman I had begun to feel some concern about the "acting" part, in view of Olsen's devotion to divisional organization. I soon learned from Leonard Eisenbud that he too had received a call from Olsen, awarding tenure and designating him Chairman of the Department of Physics. We were both pleased that our departments were now formally constituted.

Toward the end of July, 1959 I received shocking news from Ralph Bowen: he and two other faculty members, Emanuel Chill and Martin Fleisher, had received letters from Dean Olsen notifying them that their appointments would not be renewed upon their expiration in the spring of 1960. Soon after that all of the divisional and department chairs received a memo from Olsen informing us of this action. The memo went on to say that it had been taken "...because of special circumstances involved, and for reasons well known to all of you...", and "...with painstaking concern for justice and the welfare of this College." I knew nothing of the "special circumstances," or of the "well known" reasons requiring that they be fired. I knew only that the three named faculty members were bright, dedicated and articulate, that they cared deeply about educational issues, spoke up often during faculty meetings, and that all three had previously been faculty participants in Columbia's Contemporary Civilization program. While Bowen, the senior and most experienced of the three, had seemed particularly outspoken at times, the others were scarcely less so than he. It was my strong impression that all three, in their comments, reflected a sincere desire to identify the best possible ways to achieve a new and unique beginning for our brand new institution. Marty Fleisher and Manny Chill, both assistant professors, were advanced doctoral candidates at Columbia, Fleisher in Political Science and Chill in History. Both had substantial backgrounds in the Columbia CC program. Like Bowen, Chill and Fleisher had been engaged by Olsen upon the recommendation of Columbia's Justus Buchler, who characterized them as "front-line men" that had distinguished themselves at Columbia by excellence in teaching.

The three victims of this bizarre coup were not disposed to depart quietly, and word of the event soon became known far and wide, including to the press. The strong prevalence of Oyster Bay faculty members whose educational backgrounds and degrees had been earned at Chicago was duly noted, and the firing soon came to be depicted as a "purge" of a minority of scholars with Columbia backgrounds. The fact that the two deans at the time, Leonard Olsen and Dean of Students Allen Austill, were both from Chicago contributed to that impression, as did some cryptic references Olsen had made to "CC commitments," and "factions." There were no established channels for the three affected faculty to appeal the decision. The printed Policies of the SUNY Board of Trustees provided starkly that there could be no appeal in cases of non-

reappointment. The College faculty had recently adopted a set of bylaws that did contain some vague provisions for appeal, but these were never invoked. In an effort to appeal directly to the President and Trustees of SUNY, Bowen, *et al* prepared and submitted a lengthy and detailed report, which in time became widely distributed. The three never received a direct response from anyone in the SUNY administration. Interestingly, however, the Trustees passed a resolution four months later, in November, 1959, expressing a sense of “urgency about the appointment of a President” for the College on Long Island.

In the course of these events, Bowen, *et al* engaged the interest and support of the Workers Defense League, who took up their case and held a press conference in March, 1960. It was attended by three prominent Columbia Professors, Justus Buchler, Moses Hadas and Richard Hofstadter, who defended their former colleagues and spoke openly about a “doctrinaire” Chicago orientation at the College on Long Island as the cause of the dismissals. In the end, the three had no choice but to move on, to Stony Brook’s loss. In 1960 Bowen accepted a professorship at the University of Northern Illinois, in DeKalb, where he pursued a long and productive continuing career. Chill and Fleisher soon completed their Columbia doctorate studies, and both pursued long and significant academic careers within the City University of New York.

The new academic year 1959-60 began in September, and while the Bowen-Chill-Fleisher case was a constant, troubling cloud over the campus, we could not afford to let it impede progress in developing our own programs. Four new Chemistry faculty members were arriving and there was much to be done. Fausto Ramirez, his wife Joan, who was convalescing from surgery to remove a benign brain tumor, arrived in late August with their two children. The Ramirez family stayed with us in Greenlawn for two weeks while pursuing housing arrangements. Fausto had been able to transfer substantial grant support from IIT, and brought along two postdoctoral research associates. Thus the Ramirez group established the first sponsored research program at the new College on Long Island, in one of the Butler buildings. Fausto and Joan found a well located house in Stony Brook Village, and moved in immediately. While this committed Fausto to a long commute as long as the College remained in Oyster Bay, it promised good proximity to the Stony Brook campus in the future. The move was now scheduled to take place in 1962.

Sei Sujishi, his wife Mitsuko and their two children came to Long Island by car from Chicago. My favorite recollection about their arrival is that Sei had developed a fixed idea that the long axis of Long Island is oriented north-south rather than (roughly) east-west. As a result, he tried to make a left turn whenever he came to a sign reading “To Eastern Long Island.” While this got them hopelessly lost in Nassau County, they did eventually find Planting Fields and Oyster Bay, where they soon found housing not far from the temporary campus. With the arrival of Bill le Noble with his wife Helene, and Ted and Carol Goldfarb, the new six man Chemistry Department faculty was fully assembled and ready to greet the new academic year.

Among the crucial tasks demanding immediate attention was the development of our undergraduate chemistry major program. Since the academic year now beginning was the third of the college's existence, in principle we would now have students at freshman, sophomore and junior levels. The first class, admitted in great haste in 1957, had already become sharply attenuated. Due in part to inadequacy of admission standards, but perhaps in equal measure to the relatively primitive nature of classroom and living arrangements during that first year, only a small percentage of the 148 students originally enrolled would actually graduate in 1961. Of these, three had declared intent to major in chemistry: Patrick Crean, Jessie Nicholson, and Marie Collins. We had to make appropriate arrangements for their upper division course work, while at the same time offering appropriate courses for the students coming along in the earlier two class years. The quality of admissions had greatly improved since 1957, and we expected much larger numbers of potential chemistry students in those classes.

I was confident that it would serve our best interests well to seek accreditation by the American Chemical Society's Committee on Professional Training at the earliest possible moment. For that reason, we began at once to prepare a draft of our projected four year chemistry major program, providing a solid course structure in general, organic, inorganic and physical chemistry, with elective courses to be offered when enrollment would justify them, and with research participation to be encouraged at the upper division level. The principal point of novelty of our curriculum, in context of its time, was that it included no courses bearing the label "analytical chemistry." Since basic analytical methods underlie all of chemistry, and the pursuit of chemical research requires all of us to be analytical chemists, it has long been my view that analytical chemistry should not be regarded as a separate subdiscipline. In that spirit, analytical methods are best introduced in the context of particular chemical systems of interest, throughout the curriculum, and in the course of presentation of all the subdisciplinary branches.

While I doubt this view of analytical chemistry would be considered controversial today, I mention it here because it was a source of some difficulty for us in those early years. When we first became eligible to apply for ACS accreditation, during the academic year 1961-62, the Committee on Professional Training sent a well-known professor of analytical chemistry from MIT, David Hume, to inspect and evaluate us. A very kind and compatible person, despite his clear disagreement with that aspect of our curriculum he chose not to make a big issue about it. He was otherwise well impressed with us and what we were doing, and we became a fully ACS-accredited department at the earliest possible moment, even before our move to the new campus in Stony Brook. A few years later, with our graduate program well under way, we were evaluated by an external review committee that included another prominent analytical chemistry professor. That committee's report contained some strong language about what they considered a glaring deficiency in our program, and urged our administration to take corrective action. Fortunately nothing was made contingent upon that suggestion, and we suffered no penalty for ignoring it.

The six of us had much to do during the year 1959-60, with students and courses at three levels, new courses to both plan and teach, including laboratory as well as classroom instruction. My records show that I continued to contribute to the general education science course, and taught freshman chemistry as well. I no longer recall in detail what each of us did, but I do remember that we were all busy. Fausto, in addition to his teaching, got his research program quickly under way with the help of his several postdoctoral students. Bill le Noble applied for and obtained NSF support for his program, and before the year was out had begun construction of the high pressure equipment needed for his research. Sei Sujishi and Ted Goldfarb planned a joint program to apply matrix isolation spectroscopy in exploration of certain inorganic reaction systems, and submitted a proposal to the Department of Defense that would be funded during the following year. With whatever time I could muster, I continued my research collaboration with Oliver Schaeffer at BNL. As in the previous year, it was a continuing benefit, and a privilege to enjoy the relative calm that prevailed there.

We were authorized to fill two new faculty positions at junior level, and in the spring of 1960, after suitable recruitment efforts, assistant professorship offers were made to and accepted by two outstanding candidates. One of these was Robert Schneider, who had recently completed his PhD in physical chemistry under Ben Dailey at Columbia, and was at BNL on a postdoctoral research appointment. The other was Arthur Lepley, who had earned his PhD in physical organic chemistry with Weldon Brown at Chicago and carried out postdoctoral research with Michael Dewar at Texas. Both were young, eager and excited about prospects for the new College on Long Island, and we were pleased to have them coming aboard in September.

Busy as we were, and forward-looking as we had to be, it was not possible to ignore the impact that the Bowen-Chill-Fleisher firing had now brought to the campus atmosphere. Faculty meetings were infrequent, but when they did take place Dean Olsen went out of his way to suppress discussion of the non-reappointment issue, in even the most abstract possible manner. Knowing that the three were doing all they could to demand attention and secure review by the SUNY Central administration, and fully feeling the outrage and anomaly of their situation, I tried to be hopeful that common sense would somehow prevail. But as the academic year went on, hopefulness was hard to sustain. I had expressed my opinions freely at all stages of the situation, and found myself becoming increasingly outspoken as the academic year progressed. It was during this year that polarization began to appear and affect interpersonal relations within the faculty, a condition that grew and became uncomfortably widespread during our remaining two years at beautiful Planting Fields.

One day during the height of the Bowen-Chill-Fleisher crisis I received a call from Fausto Ramirez, to inform me that in his opinion I was damaging the Department by being outspoken. I wasn't surprised, because I had been aware for some time that he and Joan had established a cordial social relationship with the Olsens. I could only tell him that I had to do what I thought was right, and that in my view it was the entire College that was being damaged by the plight of the

three Columbians. The Olsens had also cultivated a social relationship with our friends the Eisenbuds. While Leonard and I never talked about it, I believe it was his view that Bowen-Chill-Fleisher had no bearing on the future of the Physics Department. Socializing had not been an issue for Evie and me, since my relationship with Olsen had become prickly before our arrival.

In an unrelated call from Fausto in the spring of 1960, he informed me that he had recently visited the SUNY College of Forestry in Syracuse, upon invitation by Professor Michael Swarc. In the course of the visit Swarc informed him that he held a special title, Distinguished Professor, and recommended to Fausto that he seek promotion to the same rank. Therefore, Fausto was asking me as his Chairman to make that recommendation to the Dean, whereupon the Dean could pass along to the SUNY Central administration for appropriate action. I was caught by surprise by this request, but responded without hesitation that I thought it much too early in the life of the Department to begin seeking special titles for any of its individual members. That while he was clearly the most distinguished member so far, we would surely be doing our best to recruit others of equal or greater distinction, and would be a mistake to create a precedent-setting benchmark at such an early stage. Fausto wasn't about to accept this argument, became very insistent, and the conversation developed a regrettably ugly tone before it was over. It was a request that Fausto would renew many times during the years to come. In combination with his disapproval of what he considered my impolitic outspokenness, this led to an unfortunate deterioration of our personal relationship.

In the spring of 1960 an important event occurred in Stony Brook: land was broken for the new campus, with Governor Nelson Rockefeller turning over the first shovel. His was a charming presence, and I enjoyed his wry expression of pleasure about being there to "inaugurate another Harriman project," referring to the fact that initial planning for Stony Brook had been carried out during the gubernatorial administration of his predecessor in the office, Averill Harriman.

During the summer of 1960 it came to light that Illinois Institute of Technology's chemistry chairman, Ralph Kirkpatrick, was engaging in a determined effort to reclaim our colleague Sei Sujishi. This was successfully met by countermeasures on our part, principally including Sei's promotion to the rank of full professor.

During the year 1959-60 we were a six member department, and there would be eight of us the following year. Of the eight members three, Fausto, Sei and I, constituted the senior faculty. We held frequent senior faculty discussions on Friday evenings, in Louie's Neptune's Cave, a cozy seaside restaurant in Cold Spring Harbor. Starting with martinis, and continuing with seafood delights of all kinds, interpersonal tensions were set aside as we talked light heartedly of the present, and seriously and enthusiastically of plans for the future. It was also during these earliest years that we began holding Departmental Colloquia on Friday afternoons, a tradition that has continued to this day.

## IV

### PRESIDENT LEE

One day in the early summer of 1960 Dean Olsen called me and said: “You no longer work for the College on Long Island.” In the light of all that had been happening I was momentarily startled, but relieved when he went on to say that I was now working for the *Long Island Center*. The name State University College on Long Island was cumbersome, and I wasn’t going to miss it, but it had had its humorous aspects. Our English professor colleague Ed Fiess had come up with a slogan: “You may loin fast, you may loin slowly; but you’ll loin it good at old SUCOLI!” Isaac Nemiroff had composed a whimsical school anthem incorporating the full title, and there was a student newspaper called the *Sucolian*. Because the acronym suggested a hybrid of succotash and broccoli, the imposing collegiate name “Old Broccotash” had also been proposed. The designation of our campus as a *University Center* suggested something much closer to our institutional aspirations than SUCOLI, and the new title was most welcome.

An event that proved critical to the development of SUNY was Nelson Rockefeller’s election to the governorship of New York in 1958. In step by step fashion, he emerged as the powerful proponent needed to create a real university system. In 1959 he created an external commission, headed by Henry Heald, then president of the Ford Foundation, to review the facilities for higher education in the state, and the steps necessary to assure educational opportunity for all citizens who qualified, at the undergraduate, graduate and professional school levels. The resulting Heald Committee report, issued in November, 1960, made sweeping recommendations for development and expansion of the entire SUNY system. Included among these recommendations was a call for immediate establishment of two graduate education centers, one at Stony Brook, and the other at an undesignated upstate location. In the language of the report, both were to be developed to “stand with the finest in the country.” This was the origin of the phrase “Berkeley of the East,” heard increasingly as an expression of aspiration for Stony Brook.

While the Heald report was in preparation, the SUNY Board of Trustees, chaired by Frank Moore, was busy preparing its own master plan for the ‘60’s. Moore had held elected, statewide office in New York, both as Comptroller and Lieutenant Governor; he was as thoroughly connected in state politics as anyone could possibly be. He had a close relationship to the Rockefellers, and served as President of the Rockefeller Government Affairs Foundation. The Trustees’ Master Plan for the ‘60’s, steered to adoption by Moore, called for the establishment of not two but *four* university centers, with degree programs

extending through the doctoral level. As in the Heald Report, one of these was designated for Stony Brook, and the recent change in the name of our campus to Long Island Center had clearly anticipated these Trustees' recommendations. The other three were to be developed by the conversion of existing campuses: the State Teacher's College at Albany; the University of Buffalo, then still a private institution; and Binghamton's Harpur College. While the location of Albany State College in New York's capital city could be said to justify its upgrading to the status of University Center, the inclusion of Binghamton in this category was puzzling. Formerly an outpost of Syracuse University, a private school, Harpur College had come to be well regarded in its day as the one and only true four year liberal arts college in the SUNY system. Its inclusion in the category of University Center, however, appeared to be more closely responsive to political than to educational needs.

One of the many aspects of the Olsen administration's performance that Bowen, Chill and Fleisher had been careful to expose in their public declarations during 1959-60 was the near total absence of engineering faculty and course offerings at the College on Long Island in the third year of its existence. While the mandate included engineering, only one member of the faculty, Dick Glasheen, held an earned engineering degree (MS). While Dick did his conscientious best to provide instruction at introductory level for prospective engineering students, he had neither time nor qualifications to prepare plans for higher level instruction or degree programs. With this spotlight shining, Dean Olsen was moved to seek a qualified person to head and develop an engineering program. As a result of this search he identified John Francis Lee, then Chairman of the Department of Mechanical Engineering at North Carolina State University in Raleigh, as a leading candidate for the position of Dean of Engineering, and sought Albany's approval of his appointment. In mid September 1960, however, we were suddenly surprised to learn that the Trustees had appointed the same John Francis Lee to the position of President of the Long Island Center, to become effective in January, 1961. As previously remarked here, in the midst of the Bowen-Chill-Fleisher crisis the Trustees had expressed urgent need for a President at Oyster Bay. Rumors that circulated following the switch of Lee's candidacy from Engineering Dean to President suggested that the Trustees may have turned the tables on Olsen following his strong endorsement of Lee. E.g.: "...well, if he's *that* good, perhaps he should be president - you really need one down there!"

John Lee was introduced to the Trustees as a candidate by a new President of SUNY, Thomas Hamilton. SUNY's first president, William Carlson, had been discharged by the Trustees in 1958, and Hamilton was appointed to the office early in 1960. John Slocum, who as an executive dean under Carlson had played a substantial role in launching the new Long Island campus and was the Central Administration executive most directly familiar with all developments at Oyster Bay, had now been designated Vice President under Hamilton. And the politically ubiquitous Frank Moore continued to occupy the powerful position of Chairman of the Board of Trustees.



John Lee visited both Oyster Bay and Stony Brook during the months prior to assuming office as President of Long Island Center. Then, in January, 1961, he took up residence fully charged with the excitement and challenge of his new assignment. He and his wife moved into the "Coe Cottage," an estate house secondary to the Coes' great Tudor mansion but nevertheless fitting, in both size and accommodations, to be a proper presidential residence. It soon became clear that Lee had fully internalized the words and implications of both the Heald report and the Trustees' 1960 master plan, and was ready to cast himself in the role of true visionary. In his first address to the faculty in February he expressed his view that development of the Long Island Center presented a unique challenge and opportunity, which he described as "...the creation of a new university in a rapidly changing society under the influence of a scientific revolution." To meet that challenge, he argued, it was necessary to recognize that "...a university has two equally important and inseparable responsibilities, namely teaching and research;" and that "...teaching which is not sustained by research to gain new understanding and insight into the nature of man and the natural world is bound to be sterile." The necessity for our new university to build and maintain a faculty fully active in research was an urgently recurrent theme during the Lee presidency. He also showed in that first address that he could be blunt, as he made sharply critical remarks about the then current status of the biological sciences, and questioned what he saw as lack of diversity in the Division of Social Science. Appearing to trample upon Olsen's cherished divisional administrative structure, he stated that "...academic policy should begin its foundations at the departmental level, flowing to the faculties of the several colleges and finally to the general faculty of the University." While he did not spell out the need for a College of Arts and Science just yet, he clearly implied it by unveiling his own detailed plans for a departmentally organized College of Engineering. His candidate to become founding Dean of that College was already on board: Thomas Irvine, Lee's colleague from North Carolina State, had come with him in January fill that role in our faculty.

The content of John Lee's address was most welcome to all of us in the Chemistry Department, as well as to the physicists and numerous others throughout the faculty. For the Division Chairs, particularly those of the Humanities and Social Sciences, and many of their closely affiliated faculty, it was a possible cause for anxiety and tension. An additional matter that Lee had touched upon in his address, and considered essential, was the creation of a designated Graduate Faculty. This became another possible source of unrest when he declared it possible that some current faculty members might not be found fully qualified for membership. Later in the semester, this was further aggravated when all faculty members were requested to provide information about their experience in graduate level instruction. Change, welcome to some, perhaps unwelcome to others, was on the way.

On June 4, 1961 the College's first Commencement ceremony was held, with all appropriate pomp and circumstance. President Lee presided, and Dean

Olsen awarded the degrees. The faculty marched in full regalia, with Ed Fiess carrying the Marshall's baton. Of the 140 members of the entering class of 1957, only 25 were present, ready to graduate, in cap and gown. The chorus, directed by Frank Erk, provided a prolonged, four movement rendition of "Pioneers O Pioneers!" After invocation and prayer, speeches were made, honors and distinctions recognized and, at last, degrees awarded. Nineteen of the 25 were science majors, and of those three were in chemistry. The composition of our first graduating class was politically exemplary: one woman, Marie Collins, and two men, of whom one (Patrick Crean) was white, and the other (Jessie Nicholson) was black. Both Pat and Jesse had participated in research under the supervision of Bill le Noble. Enabling and encouraging undergraduate students to participate in research is a practice that we considered important from our very earliest Departmental days, and has continued to the present day. After graduation Marie went to work for a small chemical company on Long Island. Jessie went directly to Brandeis for graduate study, and after earning his PhD there he joined the faculty of Howard University, where he has had a long and successful career, a major portion of it as Chair of the Chemistry Department. Pat Crean taught chemistry at Northport High School for a few years, then went on to Purdue, with Bill's encouragement, to earn a PhD degree. Pat has enjoyed a long career in chemical industry, at the DuPont Company.

Immediately following the first commencement, President Lee was ready to proceed with his plan for reorganization, calling for a College of Arts and Science in parallel with the already established College of Engineering, each to be headed by a Dean, and departmentally structured. In addition there would be a Graduate School, also headed by a Dean. Tom Irvine had already been appointed Dean of the College of Engineering. Lee asked Sidney Gelber to serve as Acting Dean of the College of Arts and Science. To my great surprise, he also asked me to serve as Acting Dean of the Graduate School. Pleased by this expression of presidential confidence, and recognizing it as an opportunity to contribute to the future of Stony Brook in a ground-breaking role, I agreed to accept the appointment.

Shortly after Lee submitted these nominations to the Central Administration in Albany I had to be away for several weeks to meet commitments to the Chemical Education Materials Study (ChemStudy, for short), a program sponsored by the National Science Foundation as part of a national effort to reform and revitalize secondary school education in science. The program was co-directed by George Pimentel of Berkeley and Arthur Campbell of Harvey Mudd College. They had asked me to serve as Director of a trial center for ChemStudy materials in the New York metropolitan area. In the summer of 1961 I attended meetings in Berkeley, followed by several weeks of summer session at Cornell for participating high school teachers from my center and others on the east coast. When I returned from Ithaca in late July, John Lee and Sidney Gelber contacted me to impart strange news: in response to Lee's request for approval of his two Acting Deanship nominations, a communication

had come from Albany approving the appointment of Sidney as Acting Dean of the College of Arts and Science, and Leonard Eisenbud as Acting Dean of the Graduate School, a position for which he had not been nominated. My name was not mentioned. I could only suppose that my outspoken support of Bowen, Chill and Fleisher, and by now perhaps of Lee as well, had motivated this mysterious action on the part of our remote administrators. John Lee made it clear there was no way he would accept the Eisenbud substitution. Since it didn't seem a productive path for him to make a *cause célèbre* over me, I suggested that he counter the Administration's action by nominating Arnold Feingold, a well qualified member of our Physics Department. Lee did so, the suggestion was accepted by Albany, and Arnie was appointed. I never learned whether Leonard even knew that he had been "appointed." Neither of us mentioned it to the other, and our friendship remained undamaged.

This strange event was symptomatic of a general deterioration in the levels of communication, clarity and trust that characterized relations between the Long Island Center, at Oyster Bay, and the new Central Administration in Albany. In addition to President Hamilton himself, the principal communicator on the Albany side was Vice President John Slocum, in continuation of the role he had played toward Oyster Bay from its beginning. He had a long standing relationship with Olsen, and since John Lee had inferentially announced the end of divisional organization it seemed likely that Slocum had been maintaining contact with one or more of the disaffected divisional chairs. In addition to Hamilton and Slocum, there was a new member of the top SUNY administration: Harry Porter, formerly President of the SUNY Teacher's College at Fredonia, had arrived in mid-summer to fill the newly created post of SUNY Provost.

John Lee's scheme for the administrative structure of the Long Island Center called for a principal academic administrator at a level above the deans, to bear the title of Dean of Faculty. Leonard Olsen's relationship to the new regime had become a major question mark, and Lee allowed a long delay to occur before acting on it. In the end, Lee offered Olsen the position of Dean of Faculty. After traveling to Albany to discuss it with President Hamilton and others, he informed Lee that he would leave Oyster Bay to return to his previous post in SUNY's Central Administration.

With his Arts and Science appointment in place and approved, and enjoying the full confidence and support of the President of Long Island Center, Sidney Gelber entered a period of high activism. One of his priority concerns was departmentalization of the Social Science and Humanities divisions, for which he set out to seek appropriate department heads. Several new appointments were made in this period. One of these was Richard Morse, a well established scholar of Latin American History then at the University of Puerto Rico, who came to chair the new Department of History. Martin Travis, a political science professor at Stanford who was spending the summer with relatives on Long Island, dropped in unannounced for a visit, and Sidney lost no time to sign him up. One action of Sidney's that created a stir in some sectors of the faculty

was his rejection of a recommendation for promotion and tenure for a young assistant professor of biological science, on the ground that his research record was not at a level that would qualify him for graduate faculty membership.

Concerned about the status of the mathematics program at Oyster Bay, and feeling a need for advice about its further development, Sidney asked me whether I could suggest someone that could be a helpful consultant. I gave him the name of a mathematician at the Arthur D. Little company, Leslie Peck, whose collaboration with me on one of my research projects there had resulted in a joint publication in the *Journal of Applied Physics*. Sidney invited Les Peck to come to the campus as an advisor/consultant. After he arrived, to my surprise, Sidney quickly raised the ante to offer him an appointment as Professor and Chairman of the Department of Mathematics. Sidney made the offer entirely on his own, and I felt at the time that it was made much too hastily. The offer was promptly accepted.

In addition to these and other appointments in the Arts and Science College, Tom Irvine was successfully recruiting new engineering faculty, and many new faces appeared on campus as the fall semester of 1961 approached. Among these was Edward Kosower, the only new member of the Chemistry Department to join us that year. Ed, a physical organic chemist, had completed doctoral studies under Saul Winstein at UCLA, and joined the faculty of the University of Wisconsin after a period of postdoctoral research in England. Despite an excellent publication record he had been turned down for promotion and tenure at Wisconsin. His references were outstanding, and this seemed to me a fortuitous combination of loss for Wisconsin and good fortune for us. We offered him an appointment as Associate Professor of Chemistry, and were pleased by his acceptance.

The pace of change occurring on the Oyster Bay campus following Lee's arrival as President was breathtakingly rapid. Although exhilarating to some, it had quite the opposite effect on others. For those dedicated to Olsen's Chicago College vision and its accompanying administrative and curricular structures the new developments were a source of major disappointment and, for some, even bitterness. Not surprisingly, an atmosphere of tension and conflict developed on the campus. The Hamilton administration in Albany had been inconsistent and unpredictable all along, and it was not at all clear that they shared the vision for Stony Brook expressed in the Heald Report and the 1960 Trustee's Master Plan. Lee pressed hard for budget increases commensurate with that vision, but with little success. Meanwhile, major financial commitments were being made to the University of Buffalo in support of its presumably imminent entry to the SUNY system. In addition, it became increasingly evident that Central Administration's support for the rapid changes Lee was bringing about at Oyster Bay was at best shaky. It seemed clear that John Slocum, who had been the principal Central Administrative shepherd of Oyster Bay development until Lee arrived, and now had Olsen at his side in Albany, was the focal point of negativity in Albany. Lee made an effort to modify this influence by formally asking Hamilton to shift

responsibility for liaison with Oyster Bay from Slocum to Harry Porter. Hamilton rejected the request, on the ground that Slocum possessed greater knowledge and “experience” of the Long Island Center and its history, which was of course exactly why Lee wished to have him replaced.

Tensions on campus were not limited to the faculty, and unrest began to develop in the student body. This began to boil over when President Lee made clear his intention to replace the Dean of Students, Allen Austill, who was the one remaining administrator from Olsen’s administration. A student strike was organized in support of Austill, demanding his retention in that post. While Lee met with the students, and persuaded them to call off the strike, he was (not surprisingly) unable to give them an explanation for Austill’s removal that they considered satisfactory. A petition bearing some 400 student signatures was sent to Hamilton, Moore and Lee, and it was this event that triggered what turned out to be the final days of John F. Lee’s brief presidency.

Again, I was away from the campus when dramatic events were about to unfold. I had been asked by the Rockefeller Foundation to join a commission to the Universidad del Valle, in Cali, Colombia to evaluate its academic status and potential, and recommend actions the Foundation might take to strengthen and assist its development. The visit took place in late October, and on the way home from Colombia the commission stopped off in Montego Bay, Jamaica to prepare a draft of our report. The time spent in Cali had been both hectic and interesting, and that spent in Montego Bay both calming and peaceful. When I returned to Oyster Bay in early November I found it hard to focus on the unbelievable events that I found unfolding there.

Upon receipt of the students’ Austill petition, Hamilton summoned Lee to Albany and demanded that he sign a letter of resignation. John Lee adamantly refused the request, and upon his return to Oyster Bay prepared an open letter to Governor Rockefeller, with copies to Moore and Hamilton. He released the letter at a press conference on November 8. Its opening paragraph read as follows:

“I was appointed less than one year ago by the Trustees of the State University of New York to help create a major university campus on Long Island. The terms of this challenging assignment followed the recommendations of the Heald Commission Report of last year as embodied in the Master Plan of the State University. It has been shocking to me, as an educator and administrator, gradually to discover the incompetence, corruption and cynical politics that were fostered on this campus by the previous administration. Fiscal irresponsibility and academic and administrative incompetence have no place in any institution of learning. Certainly they cannot be tolerated by the good citizens of this State who have so long been awaiting the development of the Long Island Center as a university second to none.”

Following that attention-getting start, the letter went on to accuse the SUNY Central Administration of obstructing his program of reform, acting to protect members of the previous administration, and coercing him into silence. Invoking Rockefeller's known interest in higher education, he called on the Governor to investigate the conduct and practices of the previous Long Island Center Administration, and of the Central Administration, to the end that appropriate actions may be taken, so that "...we may then move forward on a plane of truth and integrity to build a vital public institution of higher learning."

The response from Albany - from SUNY, that is - was prompt. On the very next day, November 9, 1961, the Trustees of SUNY voted unanimously to remove John Francis Lee from the office of President of the Long Island Center, "effective immediately." In a companion resolution they designated President Hamilton himself to "...exercise the powers and perform the duties of President of the Long Island Center until such time as a new incumbent of such office shall have been appointed."

While press coverage of Lee's appeal to the governor had been subdued, his firing turned out to be a major attention-getter. It was reported on the front page of the New York Times, and inaugurated a period of rather close examination and scrutiny of ongoing events both at Oyster Bay and in Albany. The NY Times's education editor, Fred Hechinger, after conducting a number of interviews, reported on November 11 that Lee had enjoyed widespread support on the campus. Hechinger maintained his interest in the situation and reported on it frequently during the coming months. Lee held a well-attended follow-up press conference on November 11, in which he was more explicit about some of his charges than he had been previously, and spoke bluntly about what he termed the "flooding" of the campus with faculty from Chicago. The New York Times commented, in an editorial on November 15, on what it called the "controversy" over Lee's firing, and suggested that there was a need for close examination of progress in SUNY's development.

The impact of these events on the campus was dramatic. On November 9, as word of Lee's firing was on its way down from Albany, many of Lee's supporters gathered at the Coe Cottage to express their continuing support for him, and their concern for the future of our fledgling institution. One memorable detail of the day's happenings was the arrival at Coe Cottage of Harry Kalish, who had recently been designated Chair of Psychology. Informed that Lee's supporters were gathering, but apparently not knowing what had happened, he came to the door and inquired without entering. Upon being told that John Lee had been fired, he turned around and walked away. This incident illustrates the degree of faculty polarization that had developed during Lee's brief presidency: there were now two camps. If one side were to win, the other would surely lose. Anyone not allied with either camp would have to be careful not to give the impression of being on what might turn out to be the wrong side.

The Governor did not rise in indignation to demand an investigation, and/or the return of John Lee to his presidency. Nor had Lee expected any such dramatic rescue. In speaking out as he did, he acted courageously to do whatever he could to save the vision for Stony Brook's future that he had articulated so well during his brief term of office. By shining so bright a light on the situation he did indeed greatly assist its intended cause, the preservation of a bright future for Stony Brook. And it was helpful to Lee's own future as well. After moving out of Coe Cottage, John Lee went to Washington for a brief period at the National Science Foundation, and was then appointed Executive Vice President of International Development Services, Inc. Within two years he became its president, a post that he held successfully for ten years. Since IDS is a foundation with close ties to the Rockefeller family, it seems safe to say that the governor was paying attention when John Lee spoke out.

In retrospect, I feel quite sure that development of the new university at Stony Brook would not have taken place as rapidly as it did, or as successfully, if the brief presidency of John Francis Lee hadn't happened. When I hear John Toll referred to as Stony Brook's first president, which is just about always, I feel a twinge of regret on John Lee's behalf. While it can be said beyond doubt that we would never have come as far or as fast as we did without John Toll, it's also the case that without the nearly sacrificial contribution made by our actual first president, John Francis Lee, we may well not have reached the level needed to attract a John Toll by the time his presidency began in 1965.





## V

### THE FINAL YEAR AT OYSTER BAY

With our first and only president snuffed out by the seemingly capricious powers that be, there seemed little reason for optimism about the future for Stony Brook. But before drawing conclusions we had to find out what our new president pro tem, Thomas Hamilton, had in store for us. We didn't have to wait long: he came to spend a full day on campus on November 20. In his first act, he met with Sidney Gelber and asked him to resign his position as Acting Dean of Arts and Science. Sidney immediately complied. Hamilton then met with Arnold Feingold, and asked him to stay on in the office of Acting Dean of the Graduate School. At first Arnie agreed to do so. But when Hamilton next informed him that Sid Gelber had resigned at his request, irate that Hamilton had taken such a major step without consulting him, Arnie instantly resigned. He announced his resignation at a faculty meeting held later that day, where he spoke openly and caustically about what he characterized as Central Administration's reckless disregard for the welfare of our campus, reflected in the sudden removal of both our President and our Arts and Science Dean. Hamilton seemed taken quite by surprise to learn that this point of view was shared by more than a tiny minority of the faculty, doubtless in contradiction to the impression held by his colleagues in Albany. The Trustees had created a hot seat for him, and we inferred he was not comfortable sitting in it. While he continued to hold the title Acting President, Hamilton soon appointed Harry Porter to the position of Acting Dean of Arts and Science, and in the coming months we saw much more of Porter than of Hamilton.

In the culminating event of Hamilton's first day on our campus, Sidney Gelber managed to run his car off one of the back roads near Planting Fields. No one found this surprising, but all were relieved to learn that Sidney himself had emerged undamaged even though his car had been badly banged up.

Gelber's removal from office, as the first sign of Albany's intentions, could easily be read as a declaration of intent to revert to our previous administrative structure. A second sign, soon received, was news that the Lee administration's negative tenure decision for a member of the Biological Science faculty had been quietly reversed in Albany. While the many friends and colleagues of this valuable and well liked faculty member were pleased, and welcomed his good fortune, this action was troubling as an apparent indication of intent to lower the standards Lee had set for graduate faculty participation. Indeed, the entire question of Stony Brook's future as a graduate research and education center, which we thought had been settled and assured, was suddenly clouded and up in the air. My colleagues and I had prepared a detailed proposal for the PhD program in chemistry that we planned to implement in the fall of 1962 on the new

campus in Stony Brook. Several weeks prior to Lee's firing, it had been submitted to Central Administration for evaluation and approval, and we knew that it would require further approvals from the Board of Regents and the State Education Department. But we had received no word about it since we sent it to Albany.

Antagonisms on campus that had previously been more or less privately contained were now becoming more frequently and openly expressed. Some members of the mathematics department, in particular one A .J. "Spike" Martin, conducted a vicious campaign against their chairman, Leslie Peck, who had been appointed by Sidney Gelber without consulting them. They hated Peck, and held me responsible for his presence on campus because I had referred him to Sidney as a consultant. I was subjected to a considerable volume of verbal abuse about this on more than one occasion, to an extent that I began to feel there might be fisticuffs in the offing. The Chemistry Department was represented in one of Spike Martin's hate messages as a gang of unthinking automatons in lockstep with an unsavory leader. Peck's detractors claimed to have detected serious exaggerations in his self-presented record of graduate education experience. Peck had unwaveringly denied them at the time, and I believed then and still believe that the indiscretion was minor if at all. But the attack was continued without letup throughout the academic year 1961-2, and in the end, Peck's resignation was forced by Harry Porter.

I need use just one word to describe my own personal reaction to the sudden, potentially overwhelming change of fortune that had overtaken our cherished infant university: I was overwhelmed. It was hard to retain perspective in a situation in which the controlling forces seemed so remote, with intentions so retrograde. For the first two weeks after November 11, and perhaps even longer, I felt quite overcome by despair, convinced we were on our way down the tubes. I felt a heavy responsibility for the fine colleagues I had recruited, whose bright expectations for our future I had stimulated by vivid projections of my own. At one point I even entertained a fantasy about locating another campus somewhere - perhaps in California, a state known to support and treasure public higher education - where our fledgling Chemistry Department could reassemble *en masse*. At one point Tom Irvine chided me about what he considered my aura of pessimism and gloom, saying that I was undermining the morale of my young colleagues. I responded that I didn't know what there was to be cheerful about, but promised I would try to keep my despair more to myself.

Then suddenly I came out of it. Impulsively, I picked up the phone and tried to reach the State Commissioner of Education, James Allen, who had shown a strong interest in our campus and its development during a visit earlier on in that fall. I was away at the time and had not met him, but I knew enough about him to have a strong feeling that, in his person, there was at least one Albany administrator concerned for the advancement of public higher education in the State of New York. To my surprise my call went through directly to Commissioner Allen, who came on the phone. After identifying myself I told him we had a campus down here in a tight spot that it might not survive, and I thought

he should know about it. Should he be willing to receive me I would gladly come to Albany and tell him about our situation. Allen extended an immediate invitation, and we quickly agreed on a day and hour in the coming week that suited us both well. Having accomplished this with such ease, I began to ponder the steps I might have taken beforehand to make my mission as effective as possible. The principal thought that came to mind was that a delegation of two might very likely be more effective than one, and I sought Arnie Feingold to tell him what had happened and discuss it with him. Delighted that the interview appointment had been made, and agreeing that two might be better than one, Arnie volunteered to go to Albany with me. Instead of making another call to Allen, we would go together, and Arnie would wait outside while I sought Allen's permission for him to participate. If Allen said no, which we thought unlikely, I would at least have had pleasurable company for the ride up and back.

When the day came we drove to Albany in my little two-cycle Saab. I entered the Commissioner's office alone, secured Allen's immediate agreement to Arnie's participation, and invited him to join us. We found James Allen a very impressive man, and the atmosphere he created for the interview was so encouraging that we spoke out fully and frankly, without hesitation. He knew the factual outline of the event - Lee's firing and Hamilton's designation as President *pro tem* - but this was the first time he was hearing about it from the inside. He wanted to know as much as we could tell him, and we were only too glad to provide. We told him about the firing itself, how it was felt on the campus, in combination with the rapid-response removal of the Arts and Science Dean, and that all signs since then seemed to indicate we were being maneuvered in a retrograde direction. We spoke to him about the many highly qualified faculty members in all divisions, and more particularly in our two departments, whose morale was being severely damaged by uncertainty, lack of information, drift, neglect and heavy-handedness on the part of the Central Administration. We went over the question of graduate degree programs and the apparent reluctance on the part of SUNY to seek approval for them. While Allen seemed eager to hear and absorb all that we wanted to tell him, and listened with manifest sympathy and concern, he refrained for the most part from making specifically detailed comments. He did remark that Frank Moore, the chairman of the SUNY Board of Trustees, had ubiquitous and tight political connections up and down the state, and that these in turn gave him the power to put in place any action he might wish, even reckless actions like the firing of Lee and manipulation of Hamilton. While we asked nothing of him, and he didn't offer to do anything specific for the Oyster Bay campus, we felt afterward that it had been a very satisfying meeting. We had spoken as openly as we could and said nearly all that we wanted to say, while he had clearly listened carefully and taken it all in with sympathy and concern. Given his high office and its responsibilities, and knowing that he had access to the ear of a Governor interested in and concerned for advancement of the State University of New York, we felt quite confident we had been effective.

Although I never saw or spoke with him again, James Allen's personality left a strong impression with me following this single encounter. More than a decade later I experienced a sense of almost personal loss upon seeing his name, with that of his wife, on the casualty list of passengers who perished in a flight that crashed on takeoff from the JFK airport.

The press, mainly Newsday and the New York Times, had maintained its interest in the Oyster Bay drama, and newspaper articles continued to appear from time to time bearing quotations from faculty members and others. Fred Hechinger, the Times' education editor, was keenly interested and doing his best to stay on top of whatever information might be available. He called me several times for background information, which I provided if and when I could, with a tacit understanding that it was not for attribution. One day in December, near the end of the semester, with the Christmas break coming up, Hechinger called to engage me in what turned out to be a long conversation. Among the matters that I sounded off about was my concern that we had no idea whether we would receive authorization to begin graduate degree programs the following fall semester, on the new campus at Stony Brook. I told him that my Department had prepared and submitted a PhD program plan to Central Administration months ago, but had received no response. If we were to stand any chance to get the program off the ground in September, we had to send out a brochure to describe it, and to invite applications from prospective graduate students, as soon as possible. Indeed, since it was then already December, it might soon be too late to inaugurate the program in September. This was clearly a new wrinkle for Hechinger, and he asked whether he could attribute the remark to me in his Times article. After pondering for at least five seconds, I said yes. A few days later, on a Sunday, a nice long Fred Hechinger piece appeared in the Times. One had to search the high numbered back pages to find it, because as always at Christmas time the paper was overflowing with advertising. My remark about the need for action now if PhD programs were to be initiated in September, duly attributed to me, was prominently featured in the article.

The end of semester Christmas break could not have been more welcome after such a tense and eventful fall season. Our Latin American historian friend Dick Morse still owned a house near San Juan, Puerto Rico, where he had been a faculty member before coming to Oyster Bay. He and his stunning Haitian dancer wife Emmy proposed to swap houses with us during the Christmas-New Year's week, and we jumped at the opportunity. We packed up our three children, now 4, 6 and 9, and flew down together on a bargain fare airline for a delightful, restful and diverting Puerto Rican vacation. We were able to fit in some sight seeing around the island, and spent a couple of days on a ranch in the hilly interior. When we went to the airport for the evening flight home we learned that the one and only airplane owned by our bargain fare airline was under repair in New York, and not expected back in San Juan until the following morning. Somehow we made it through the night at the airport, and were glad to get back to Greenlawn the next day. It then turned out that due to a change in plans the Morses had not yet stayed at our house. When they did so later for a

few days, conditions were crowded but a good time was had by all. Regrettably for Stony Brook, Dick Morse moved on to Yale after just one year as a member of our faculty. I had the pleasure of traveling with him in South America on several occasions, however, when we served together as Ford Foundation advisors at the Universidad de Antioquia, in Medellin, Colombia.

In the early part of January, 1962 Thomas Hamilton paid another of his rare visits to Oyster Bay, and held a meeting with a faculty-administrative group that I attended as a member. About midway through the meeting, Hamilton said that he had been reviewing Dr. Bonner's proposal for a PhD program in chemistry, found it excellent, and was recommending its immediate approval for implementation in September, 1962. It was a great moment for me, I could hardly wait to get to my colleagues to tell them, and that was the manner in which ours became the first PhD program approved for inauguration at Stony Brook. Two additional graduate programs, in physics and in mechanical engineering, were approved soon after that for initiation in September. It was a great relief, and a strong source of motivation, to know that this most major and crippling uncertainty had at long last been relieved.

With the graduate program now secured, our focus necessarily turned toward the new campus and the many preparations that had to be made in order to begin operations out there. There had been a ground-breaking ceremony in 1960, at which Governor Rockefeller wielded the first shovel; soon after that, construction work came rapidly under way. The very first act consisted of a merciless orgy of tree removal, carried out to facilitate a rapid pace of multiple building construction, but unfortunately imparting a bleak cast over what had been a lovely wooded area. While it would take decades to recover, that's the way things were done in those days. At least we were getting a campus, and very quickly indeed. The chemistry building was one of the first scheduled for occupancy in the fall of 1962, the others being the humanities building, a dormitory and dining hall, and several service buildings. Just as I had paid close attention during the design stages, I now made regular visits to the new building itself, to monitor construction progress, and confer on occasion about deviations from the plan. This first phase of construction was being carried out under the direct and rigid control of the State Department of Public Works. The relatively more flexible management style of the State University Construction Fund, a State agency created by Governor Rockefeller to facilitate and expedite the rapid growth and development of SUNY, was not yet fully in place.

The DPW understood that every new building had to be equipped properly to enable its function, and we were asked to prepare a list of equipment that we considered essential. Seeing this as a matter of utmost urgency and importance, I called a department meeting and asked everyone to pitch in to create an imaginative and comprehensive list. Having been given a very short deadline, everyone worked hard. Setting out to cover the electromagnetic spectrum as widely as possible in our range of spectroscopic equipment - "from gamma rays

to sixty cycles,” I liked to say - we tried to anticipate the needs of the future as well as present faculty. Nuclear magnetic resonance spectroscopy was relatively new, for example, and while we did not yet have an NMR specialist in the Department there was no doubt that we needed one or more, and that at least one would be joining us in the near future. The final list included a Varian HR-60 NMR instrument. When all the suggestions were in and a master list assembled, its projected cost came to more than \$1.25M. While that may not sound like very much today, it loomed large at the time, particularly at the State University of New York. My request, submitted through local channels, found its way to the Central Administration, then located in Albany at Thurlow Terrace. After a few weeks went by, I was informed that our equipment request would be the subject of a hearing at the Bureau of the Budget, which I was invited to attend and defend the request. On the appointed morning I flew to Albany, where Harry Porter met me at the airport. While Porter’s administrative relationship to the Long Island Center had begun in the previous fall with his appointment as Acting Dean of Arts and Science. But by this time (March, 1962), Hamilton had stopped coming to Long Island altogether, and appointed Porter to replace him in the position of President *pro tem*.

Before we arrived at the State Capitol for the hearing, Porter made cautionary comments to me about the size of our request, implying that we were unlikely to get approval for more than a fraction of it. He also said that it was going to be very bad luck for us if we should draw a certain Mr. Roberts in the role of budget examiner, depicting him as an unreasonable and uncompromising examiner with whom SUNY had had only bad experience in the past. Arriving at the appointed hearing room, a man came in who looked vaguely familiar to me, and I detected a disappointed expression on Harry Porter’s face. I soon realized that this was indeed the dreaded Mr. Roberts. I made a brief presentation about our program, the breadth of chemical research we expected it to encompass, and the wide range of instrumentation required for its implementation. Harry Porter interjected remarks from time to time, seemingly intended to narrow or contain the scope of my presentation. When we got into specifics he persisted with his interjections, and I got an increasing impression that he was letting Roberts know he did not fully support our request. Then came a magic moment: during one of Porter’s interruptions Mr. Roberts sat back and said to him: “I’ve worked with Dr. Bonner before, and I have great confidence in him.” I couldn’t recall the exact occasion he was referring to, but he did look familiar to me, and at this point I couldn’t be happier about being his old acquaintance. The rest of the hearing went very well, no serious objections were raised, and I left feeling optimistic. Harry and I went back to Thurlow Terrace to wait for a promised call from the Bureau of the Budget. I spent most of the next two hours with Milton Lewis, the Central Administration’s top man for equipment and supplies matters at the time. Milton took the call when it came, and it was Roberts himself, calling to inform us that all but about \$20,000 of the equipment request had been approved. This seems to have been a major milestone in SUNY history. Milton Lewis and Harry Porter were both speechless, and sent me off in a spirit of high congratulation. When I got back to Long Island it was late afternoon, but I went directly to Oyster

Bay to tell anyone I could find there about what had happened. The news was received very well indeed, and it wouldn't have surprised me if my colleagues had carried me on their shoulders in triumph.

Faculty recruitment efforts were resumed during the spring semester when the future had begun to look bright again. After the extensive negative publicity brought on us during the previous fall with the firing of our first president, candidates tended to be wary about the future of Stony Brook. This was understandable, but at least we could show concrete evidence that we would have a new campus and a new Chemistry building to move to, and tours of the building became an important part of the routine. One candidate that we did our utmost to bring on board at that time was Benjamin Chu, who had completed PhD research with Richard Diamond at Cornell, and was now completing a two year postdoctoral appointment there with the great Peter Debye. Ben seemed very interested in Stony Brook, and since we liked him a lot and his credentials were superb we did our best to convince him to come. Since we had begun to feel optimistic about our chances, it was a severe disappointment when he informed us that he would be going to the University of Kansas instead. He did go to Kansas, but that was not the end of the story: six years later, in 1968, Ben Chu *did* come to Stony Brook, at full professor rank.

Recruitment efforts during academic year 1961-'62 resulted in just one new appointment in the fall of that year. Richard Solo, a Berkeley physical chemistry PhD student, had visited Oyster Bay during the previous year. His PhD mentor was Bruce Mahan, whom I knew and regarded highly, and Dick made a strong impression as a young man with gifts for both teaching and experimental research. The previous negotiation had been left open, and in the spring of 1962 we offered him an assistant professorship. With his acceptance, we were set to begin departmental life in our new headquarters at Stony Brook with a total of ten full time faculty members.

It was during the summer months of 1962 that I first heard about Paul C. Lauterbur and his possible availability to become a faculty candidate. An employee of the Mellon Institute in Pittsburgh, Paul was carrying out research in the field of nuclear magnetic resonance spectroscopy (NMR). He was already known for pioneering studies he had carried out on NMR spectroscopy based on the stable isotope carbon-13, demonstrating the great potential of this method for application to a broad range of problems in organic and biochemistry. During his years at Mellon he was enrolled as a part-time graduate student at the University of Pittsburgh, and in 1962 had just completed requirements for the PhD degree. He paid his first visit to us when we were still at Oyster Bay, and I had no doubt from the outset that we should hire him. The appointment was strongly supported in the Department, despite some hesitation about my proposed terms. While on paper he appeared to be a freshly minted PhD, he was already in his thirties, had a substantial and innovative record of publication, and I considered him fully qualified for appointment at the rank of associate professor. Indeed, I thought it would be self-defeating to offer him less. Once my colleagues were comfortable

with the idea I had to convince the higher administrative echelons, and that took some time. Eventually, in September, the associate professor offer was made, and we were delighted by Paul Lauterbur's agreement to join us on the new Stony Brook campus in January, 1963. Almost immediately after we moved into our new building it became necessary for us to request our first space rehab. Our equipment list specified a Varian HR-60 NMR instrument, but at Paul's request we now had to change that to the more advanced and higher resolution model HR-100, which posed substantially greater and more complex service requirements.

It was in the early '70's that Paul Lauterbur conceived the idea of employing nuclear magnetic resonance signals to create images, and his earliest demonstrations of the concept were carried out on the much humbler Varian A-60 instrument. This instrument is now on display in the Chemistry Building lobby, in commemoration of Paul's award of the Nobel Prize in Medicine or Physiology, awarded in 2003 for his discovery of MRI.

In addition to the faculty, we had a small supporting staff preparing to move with us. Its key member was Marge Smith, a phenomenally good secretary and an energetic woman of all around general competence, and an indispensable, not-so-secret source of our success. Grossly understaffed as we were, it is surely no exaggeration to say we would not have made it through the early years without her. Not long after the move we were able to add an additional, much-needed junior secretarial position. Up to this time support staff available to look after stockroom needs, supplies, and equipment were common to the three science departments. The two principal employees in this category, Woody Leahy and Stuart Cohen, were too excellent to choose between, but in some manner a choice was made: Stu came with chemistry, and Woody with biology.

Considering the roller coaster nature of the year 1961-'62, it may not seem surprising that I've said nothing so far about the Department's principal responsibilities, teaching and research. But we did indeed teach, and now with course offerings at all undergraduate curriculum levels. My records show that I taught lecture and laboratory sessions in our introductory course during both the fall and spring semesters. I recall a young woman student coming to me during a freshman chemistry lab session to complain that her experiment wasn't working. "I've been stirring it for a long, long time but nothing's happening," she said to which I replied: "Shut up, and keep stirring!" The advice turned out to be right and somehow the remark got out, went around, and developed semi-legendary status in the Department.

Research activity continued to develop in our improvised Butler building laboratories. Fausto Ramirez's research in organophosphorus chemistry, employing several postdoctoral associates, was the most active and extensive faculty program. Ed Kosower was getting his research quickly under way, and Bill le Noble was making strides in his high pressure reaction studies, under NSF



support, and with several undergraduate students working with him and gaining valuable experience. In the collaborative program of Ted Goldfarb and Sei Sujishi, supported by the Department of Defense, spectroscopic measurements were being carried out at the extreme low temperature range made possible by the use of liquid hydrogen as coolant. Needless to say, all essential safeguards and precautions were observed, and the liquid hydrogen used in this program did not add to the already explosive character of the atmosphere elsewhere on campus.

Fausto recruited a postdoc from Sweden in the fall of 1961, who brought with him his small Saab sedan, powered by a two cycle engine, expecting to impress us. But to his surprise, he found several identical little Saabs already in the parking lot: I had bought one, followed by Bill le Noble, then Bob Schneider. Several of our friends and colleagues at BNL, including Jake Bigeleisen, were also driving them. The car was lively, had the favorable but then unusual feature of front wheel drive, and its exceptionally high gas mileage outweighed the inconvenience and humiliation of having to add oil to the gas tank as though it were a lawn mower. It was a popular model for some time, until the EPA eventually got wise and labeled it the air polluter that it was.

As the spring season deepened and departure drew closer I found myself ever more aware of the beauties of Planting Fields, particularly so in contrast to the hectic and muddy Stony Brook construction site we were now seeing with increasing frequency, and was about to become our daily environmental ration. A memory of playing chamber music in one of the Planting Fields sheltered garden areas comes to mind. Sidney Gelber, an accomplished pianist, was one participant, and Isaac Nemiroff, a composer and violinist, then a new member of the Department of Fine Arts, was another. I was a third, with my violin, and there was also a student participant whose name I no longer recall. Nor can I recall what music was played, because the strongest part of this nostalgic reminiscence is the beauty and tranquility of the Oyster Bay setting. It would be a long time before anything at all comparable could be experienced at Stony Brook.



## VI

### THE NEW CAMPUS AT LAST

Putting the finishing touches on our new building was a long drawn out procedure, and it was not until quite late in August, 1962 that it became feasible for us to begin the long awaited move. The new freshman class, along with large numbers of returning students, would soon be arriving, and teaching laboratories had to be fitted out and supplied as quickly as possible. Everyone had to pitch in and make it happen, and in many ways it was a chaotic scene. I recall that our young colleague Art Lepley played a particularly near-heroic role, putting in long hours of effective hard work and ceaseless intercampus travel. I also have a strong recollection of Marge Smith playing a commanding role in the effort, barking orders at Lepley and others, and occasionally responding “because I say so” when asked “why?” Miraculously, it got done. The building felt welcoming and comfortable from the beginning, and the Department got settled in and functioning in a remarkably brief period of time. After working in cramped quarters in Oyster Bay for so long, it was a welcome relief to be able to expand into our relatively spacious new setting. But still, Stony Brook was a construction site, and would be one for a long time to come. Muddy walkways, detours made necessary by the presence of scaffoldings above and excavations below, all made even more perilous at night by faulty or nonexistent outside lighting, became our new way of life.

The new building included a spacious executive office suite at ground level, but since it wasn't available for the Chemistry Department's use at the outset, my staff and I were housed in a makeshift but comfortable suite on the second floor. Dean of Engineering Tom Irvine was the first occupant of the ground level suite while the first of several engineering buildings was under construction. The suite was subsequently assigned to David Fox, then Acting Dean of the Graduate School, and it was nearly three years before it at last became official headquarters of the Chemistry Department. Conversion of the numerous “preparation room” spaces for their intended function as research laboratories was begun at once, and equipment items from our extensive order were arriving with great frequency. Particular attention was given to the newly designed basement NMR facility in anticipation of Paul Lauterbur's arrival in the spring. The University's entire library was initially housed in a space of approximately 1000 square feet in our building, designed for the future Chemistry Library.

In addition to the faculty and staff members from Oyster Bay, we now had our first class of graduate students. When our PhD program was approved we had immediately created a graduate brochure, distributed it widely, and begun

efforts to recruit qualified students. By the time of the move, thirteen were signed up. The majority were supported on graduate assistantship stipends, which remained the principal source of graduate student support for many years. Other stipend sources available from the beginning included research grants, a source soon essential and widely applied. Walter Correa, one of the thirteen, was a member of the faculty of the Universidad del Valle in Cali, Colombia, where I had interviewed and recommended him to the Rockefeller Foundation. He was supported under the Foundation's development assistance program for Cali through his entire period of doctoral study at Stony Brook, which he successfully completed under the direction of Ed Kosower. Raymond Mackay, who would become Stony Brook's very first PhD graduate in 1966, arrived at midyear to join our first graduate student class.

In addition to inaugurating the new graduate program, we began our first year at Stony Brook with a substantially increased undergraduate teaching load. The introductory chemistry courses were now serving a broad base of student need, from engineering, physics, and biology major programs in addition to our own. We also experienced a substantial and rising need for course work in organic chemistry from biology majors and premedical students. And for our own major students we were now offering required and elective courses at all four undergraduate levels. My records show that in 1962-'63, I taught the freshman course in the fall and the graduate chemical thermodynamics course in the spring. In the following year I taught the first semester of our junior level physical chemistry course in the fall, and graduate thermodynamics again in the spring. It was in these early years on our new campus that we inaugurated a course assignment rotation policy, in which faculty members would be assigned to teach undergraduate courses for three consecutive years, and graduate course offerings for two. This policy became a departmental tradition.

Major changes in the relation of SUNY Central Administration to our campus were now underway. Not long after President Hamilton turned over his position as President *pro tem* of Long Island Center to Harry Porter, it became known that Hamilton himself would be leaving SUNY altogether at the end of 1962, to become President of the University of Hawaii. The search for a new leader of the SUNY system was begun at once, but did not come to fruition until 1964. In that year Samuel Gould was appointed to the office of President of SUNY, and in the following year the title of the office itself was changed from President to Chancellor.

In September, 1962 Karl Hartzell, who had been a Dean for several years at Bucknell University, accepted appointment to an office in SUNY's Central Administration, carrying the title Executive Dean. Due to Hamilton's imminent departure, however, Harry Porter's statewide responsibilities had now become so greatly expanded that it was impossible for him to give Stony Brook the close attention it needed, and in consequence Karl Hartzell was asked to come directly to Stony Brook. While by title of record he was still Executive Dean for all of SUNY, he was actually in residence at Stony Brook, where his initial local title

was Acting Dean of Arts and Science. He soon found there was much more to be done than could be handled by a single lone administrator, and sought to appoint an Acting Dean of Arts and Science from the faculty ranks. When that was accomplished his own local title was changed to Administrative Officer. Under that nondescript handle, Karl Hartzell became and remained the *de facto* leader of our campus, from 1962 until John Toll arrived to become its President in 1965.

With wide faculty support, Hartzell appointed Stanley Ross to serve as our Acting Dean of Arts and Science. Stan Ross, an historian of Latin America, was the new Chairman of History. He had been recruited at the recommendation of his predecessor Richard Morse, who had now left us to accept an appointment at Yale. Academically experienced and holding good values, Stan was a level headed, fair minded and practical administrator, and he and Karl Hartzell made a good team.

Administrative Officer Hartzell did not hold a mandate to charter new directions for Stony Brook's future, but he was in a strong position to serve as caretaker and conservator of that which had already been achieved. Karl did his utmost to hold the institution together and encourage its further development. Events of the previous year had placed graduate education and research clearly at the forefront of the agenda. He was fully supportive of our efforts in Chemistry, and thanks to him we were able to make steady progress in faculty and graduate program development through this entire interregnum period. It was fortunate for Stony Brook that Karl was right here looking after the institution during that formative time, and we owe him a great debt of gratitude. I was pleased to be able to tell him so in writing on the occasion of his 100<sup>th</sup> birthday in August, 2005. We were fortunate also in Karl's choice of Stanley Ross, who contributed well to our development during that time, but departed Stony Brook for the University of Texas in the '70's. Because Stan's longevity level was not the equal of Hartzell's, it is unfortunately now well beyond too late to express our gratitude to him.

The polarization of the faculty that had developed during and in the aftermath of the John Lee firing crisis persisted up to and beyond the time of the move to Stony Brook. In new quarters, with new challenges, and with everyone busy as everyone had to be, it appeared gradually to phase itself out. Peaceful coexistence was strongly encouraged by Karl Hartzell, with his calm approach and calming manner, and striking improvement occurred during the first year. Still, some degree of rift between the faculties of the natural sciences and those of humanities and social science, whose origins were in the pro- and anti-Lee battles of Oyster Bay, persisted for a number of years.

Another important new actor now on the scene was T. Alexander Pond, who had come from Washington University to succeed Leonard Eisenbud as Chair of the Physics Department. Leonard had informed his Department that he wished to be relieved of the chairmanship, and it was Ed Lambe, himself only recently arrived from Washington University, who had the bright idea to recruit

Alec. It turned out to be a shining example of finding just the right person at the right moment. A man of brilliance, determination and exquisite activist instincts, Alec became a major force in the development and welfare of Stony Brook in a number of ways that included building a superb Physics Department and playing a crucially important part in bringing John Toll to our campus as President.

In the fall of 1962 there was no crystal ball to tell us what kind of person would become our President, or when it was going to happen. But at least there was a search committee, chaired by George Collins of the BNL Cosmotron Department, in his capacity as a member of the Stony Brook Council. Alec Pond and I both served as faculty representatives. At an early stage of the search Loren Eiseley, a well known professor of evolutionary biology at the University of Pennsylvania, was brought to the campus as a candidate. While he had excellent academic credentials, and for a short time appeared to be a promising possibility, he never became a serious candidate. After that we were presented with unsuitable candidates at regular intervals, largely on the initiative of SUNY Central. One of these was Harry Porter's successor to the presidency of the College at Fredonia, who had before that been a long term Dean at the State Teacher's College at Albany. While able and affable, his all-SUNY experience record did not speak well to the need at Stony Brook. The composition of the search committee was such that unsuitable candidacies seldom advanced very far, and I don't recall any determined effort on the part of Central Administration to impose the appointment of one of its own candidates. It became clear before very long that the local search at Stony Brook was unlikely to come to a successful conclusion in advance of the parallel search for new leadership in Albany. That didn't discourage our own continuing effort to identify potential candidates, however, and it was Alec Pond that came up with one of the most promising-looking candidates of all: John Sampson Toll, then Chairman of the Physics Department at the University of Maryland.

Recruitment of new faculty was an ever ongoing, high priority activity that could not await the outcome of the presidential search, and our first new appointment in 1962 was one that I'd had in mind for a long time. At a Gordon Conference on Nuclear Chemistry in New Hampshire in the 1950's, I met an MIT graduate student named John Alexander, then engaged in PhD research under Charles Coryell. In addition to hearing John talk at the conference, I got to know him in the casual atmosphere that prevails during most Gordon Conference afternoons. Coryell, whom I knew, considered him a major talent. I liked his scientific smarts and personality, and it seemed to me natural, and perhaps imperative, for a major Chemistry Department located near BNL to include nuclear chemistry among its research dimensions. I resolved to recruit him when the right time came, and followed the trajectory from his completion of PhD studies at MIT to postdoctoral research at Berkeley, and on one occasion even visited him and his wife Betty in San Francisco. Now in Stony Brook at last, it was clear that the right time had come. John was invited to visit, gave a seminar, met with faculty members in the Physics and Chemistry Departments at Stony Brook, and at BNL as well. Everyone including John was enthusiastic, Hartzell and

Ross were helpful and cooperative, and all essential conditions and arrangements were soon in place for him to join us at the rank of Associate Professor. Arriving in the fall of 1963, John brought well funded research support from the Department of Energy and two postdocs, Joseph Natowitz and Paul Croft.

Our Department now included strongly qualified senior faculty representing the three broad fields inorganic, organic, and physical (including nuclear) chemistry. Since as a graduate student at Yale I had experienced an atmosphere sharply divided by antagonism between the Department's organic and physical sectors, a not uncommon phenomenon of the times, I was determined not to let it happen at Stony Brook. Fausto's early presence in our Department had greatly benefited its development, and helped to spread the word about our institutional intentions. My personal relationship with Fausto had unfortunately become strained early on, but not so my continuingly high scientific and professional regard for him. To my surprise and dismay, my relationship with our second senior organic professor, Ed Kosower, also became quite strained, virtually from the beginning of his appointment. At first I thought it meant that Ed simply had no *need* for a chairman, a known faculty syndrome. No matter what the initial cause, however, his antagonism was soon confirmed and inflamed by events. Without consultation, Ed set out to establish a "Kosower Seminar," in emulation of the famous "Winstein Seminar" conducted by his mentor at UCLA. While it was a fine idea to inaugurate a topical seminar, I thought it quite inappropriate to personalize it in that way, and had to tell him it couldn't be allowed. He was bitter. The organic seminars continued, but now under Fausto's auspices, contributing to the hostility then developing between them. Next, Ed proposed to establish an Institute for Molecular Medicine, under his direction, and sought my support. Consistent with my earlier reaction to Fausto's request, I told Ed that I thought it much too soon for us to create subunits within the Department, and that we needed first to focus on building the all around, broad-based, top quality Department of Chemistry we all wanted. Ed's thorough resentment of my refusal sharply elevated the already high level of his hostility toward me.\*

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\*At the time of this writing, in 2006, Ed Kosower's concept has been realized at Stony Brook by the recent establishment of a research unit of the School of Medicine called Institute for Molecular Medicine. A unit called Institute for Chemical Biology and Drug Design has also been launched in our own Department of Chemistry, directed by Professor Iwao Ojima.

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The difficulty of my personal relationships with our two most senior and visible organic chemists was a source of tension and disagreement whenever personnel matters came under discussion, and particularly so when they concerned the organic faculty. The atmosphere of mutual hostility that developed between the two of them, however, was a more disruptive factor than their separate interactions with me. Indeed, opposition to me seemed to be the only

topic that could bring them into agreement with each other. The organic seminar became a source of such friction that outside speakers began to decline invitations. Fausto made no effort to conceal his distaste for the field of physical organic chemistry, and Ed openly referred to Fausto as a "potboiler." In my zeal to promote good relations between physical and organic chemists, I had inadvertently created a den of antagonism within the organic sector by bringing together two needy professors, both of them fresh from humiliating experiences elsewhere, whose separate desires for recognition and prima donna status brought them into irresolvable conflict. This situation created what has been described as a "minefield" for the other organic chemists affiliated with the Department during the '60's, and had a distinctly negative effect on our development in that area.

There were two instances in which valuable recruitment opportunities were sabotaged by the Ramirez-Kosower conflict. In the first of these, a very well known organic chemist called to alert me that one of his students, whom he considered stellar and recommended in glowing terms, was applying for a faculty appointment at Stony Brook. It sounded to me like a major recruitment opportunity, and since I knew the candidate was interviewing at several top universities, I lost no time in inviting him to visit. But because I had personally arranged the visit, Ed pronounced the procedure improper. Neither he nor Fausto would participate fully in the interview, and in the end it became impossible to offer an appointment. The candidate is well known today at one of the best chemistry departments in the country. On a subsequent occasion, when another famous organic chemist strongly recommended a candidate, neither Ed nor Fausto participated in the interview, nor did Bill le Noble, who was often absent at that time because of his wife's fatal illness. By default, the interview was principally conducted by Bob Kerber and George Emerson, both young and untenured assistant professors. The candidate in this case also quickly established an outstanding research career elsewhere.

Non-tenured faculty in the State University of New York are normally appointed for three year terms. Those initially appointed at assistant professor rank are in most cases reappointed to a second term, then evaluated for promotion and tenure during the second year of that term, i.e. their fifth year on the faculty. When we moved to the Stony Brook campus in 1962 there were four assistant professors in the Department, one of whom, Dick Solo, had just arrived. The others were Barry Gordon, Bill le Noble and Ted Goldfarb. Barry's fifth year, 1961-62, was our final year in Oyster Bay, and his was our first tenure evaluation case. Following careful examination of the record, and discussion by the entire senior faculty, there was general agreement that because his case was not strong he should be encouraged to look for a position elsewhere. Barry was universally well liked, it was not easy to break this news to him, and he was understandably dismayed when I did so. Fortunately, he succeeded in locating an appropriately challenging and satisfying position, in the nuclear engineering area of BNL, and returned there at the end of 1963.



For le Noble and Goldfarb, tenure evaluation became mandatory in 1963-'64, and their tenure files were prepared and reviewed by the senior faculty in preparation for discussion and deliberation. The case for Bill le Noble was straightforward: his performance had been excellent in all categories of faculty responsibility, particularly and remarkably so in research. Despite the transient and relatively primitive conditions of Oyster Bay, Bill had succeeded in getting a first class research program well under way. He had also played and was continuing to play a crucial role in the development of our departmental library. The senior faculty unanimously favored Bill's promotion to the rank of Associate Professor with tenure, and our recommendation was transmitted to and approved by the administration. Bill had clearly navigated the organic "minefields" of the time with consummate skill, since the action was approved by both Kosower and Ramirez.

In contrast, the case for Ted Goldfarb was not straightforward. He had proven himself a dedicated and effective teacher, had contributed well to the life and development of the Department, and responded well to the many demands that accompanied our growing pains of the time. But while he had been active and made headway in research, he had not yet succeeded in establishing a distinctive research direction. He continued to be the bright and promising young man we hired in 1959, but had come directly from graduate school, without postdoctoral experience, into an unformed and developing institution, inevitably encountering multiple distractions and demands that he would not have had to deal with in a more established setting. I believed strongly that it would be unfair, perhaps even unethical, not to take those circumstances seriously into account in making our evaluation, and felt confident that in the long range Ted's continuing membership would work out well and prove advantageous for the Department. In summary, I argued that although we should not regard it as a precedent setting case, with these special circumstances taken appropriately into account the record would justify a positive recommendation for tenure and promotion. To the best of my recollection, however, only one member of the group declared support for this evaluation. After doing my very best to assess the issue calmly, despite the acrimony it had generated, I prepared a positive recommendation for promotion and tenure, over the objection of a majority of the senior faculty. The Administration's decision, delivered by Stan Ross after listening to both sides, was of the plague-on-both-your-houses variety: tenure, but no promotion. This action, in addition to being unsatisfactory for everyone, came as a severe blow and morale buster for Ted. It also posed a lingering problem for the Department by creating the anomalous faculty category of tenured assistant professor. While that problem was resolved two years later with Ted's promotion to associate professor, this time with the support of a majority of the senior faculty, it is safe to say that his morale had been permanently affected.

Expansion of our departmental responsibilities and activities, in combination with occupation of our new quarters, found us severely understaffed in the early Stony Brook days. My own immediate staff still consisted of my one incomparable secretary, Marge Smith, who held the highest Civil Service rank a

mere department chair was permitted, with occasional part time help. Not until we moved to Stony Brook were we authorized to hire a second full time junior secretary. The promising young woman that we interviewed told me she was seventeen, but admitted years later that she had actually been only sixteen, and was not legally eligible when I hired her. This was Donna Barrington, who became a mainstay of the Department in the course of her long career with us, making innumerable, important contributions right up to the time of her retirement in 2004.

Maintaining the stockroom, and providing supplies and services to both teaching and research laboratories was an area of strong demand, and at the outset our faithful and overburdened Stu Cohen had to cope with it nearly single handed. Our first stockroom employee, a stores clerk, was a woman named Borka Kern. My signature had to go on every requisition during that first year, and once a week Stu would bring me a staggering pile of documents to wade through. On one occasion he and Borka, deciding it was time to test my vigilance, inserted a phony requisition for one hamburger plus French fries in the middle of the stack. I passed the test.

Seeking and filling positions for support personnel was frustrating and difficult in the early Stony Brook days because most if not all nonacademic employees came under the purview of the State's Civil Service regulations, with typically rigid employment categories, job descriptions and pay scales. It was hard to see how we could develop an adequate structure and level of support for teaching and research under these restraints. We were experiencing increasing need for glass blowing, machine shop, and electronics shop services, for example, but knew that the level of professional expertise we wanted in these areas was not to be found at Civil Service rates. I was relieved to learn that help might be on the way: the Rockefeller administration had recognized the need for an entire new category of professional positions in SUNY. Clutching at that straw, I began to prepare some position descriptions.

During my mid '50's year at Harvard, I had witnessed and admired the Harvard Chemistry Department's way of coping with the day-by-day, nonacademic aspects of running a chemistry department. The key to it was the position of Director of Laboratories, an experienced professional with a PhD in chemistry, reporting to the Chairman, who was placed in charge of all aspects of nonacademic administration, including space, equipment, operations and shops. The Director of Laboratories was a long term officer of the Department, while the chairmanship itself was regularly rotated among the most senior members of the faculty. Ronald Vanelli, the Director of Laboratories I had known when I was at Harvard, was still in office there, and I engaged him to come to Stony Brook as a consultant. He gave us a very complete briefing on both the broad scope and the details of his responsibilities, which was extremely helpful to me as I set out to prepare a description of the position we needed at Stony Brook. My request for the Director of Laboratories position lingered in the decision deferral pipeline for

something well over a year, but our need for it was never seriously questioned, and in the end it was approved unaltered.

When at long last we could fill the position of Director of Laboratories, we found the ideal candidate right here at home. Paul Croft, who had come to Stony Brook from Berkeley as a postdoc in John Alexander's group, had expressed interest in the job, seemed ideally qualified for it, and we hired him. This turned out to be an excellent match, as was amply proven by his excellent performance as our first DOL. Paul's need for secretarial help was undeniable, and I felt obligated to grant his first request, which was to steal Donna Barrington from my office. In the course of time Donna was promoted to professional rank as Assistant Director of Laboratories. After doing a superb job of organizing and administering the office of Director of Laboratories for six years, Paul moved on to a high level administrative post at a branch campus of the University of Michigan, and went on from there to a long and productive administrative career at the University of California at San Diego.

The creation and successful filling of the Director of Laboratories position brought major and most welcome change to my life, making it possible for me to come up for breath once in a while. An additional welcome change came in response to another new position request I had made, for a personal administrative assistant. While I tried to describe the position of Assistant to the Chairman in the broadest possible terms, Stan Ross insisted that training and credentials in chemistry had to be explicitly required. The Albany administrators were said to be anxious to keep us from promoting our present Civil Service employees into the new professional level positions, which is of course what I would have tried to do in order to hold on to Marge Smith. The first occupant of our position called Assistant to the Chairman was Ann Carvalho, whose credentials included an MS in chemistry from the University of Texas. She did a good job, and we were fortunate to have her on board when Marge's inevitable departure happened at last. Marge returned to Planting Fields, where a new SUNY College, to be located at Old Westbury, was then in its formative stages. When the College at Old Westbury moved to its permanent home she was a key member of its President's staff, and had been promoted out of Civil Service to be his Special Assistant.

Faculty recruitment was a continuing, non-stop activity throughout the decade of the '60's. Several junior level appointments that were made in the earlier part of that period included Robert Boikess, a physical organic chemist from UCLA; Ivan Bernal, an inorganic chemist and crystallographer, who moved over to BNL after two years, then went eventually from there to the University of Houston; and William Kern, a physical chemist and theorist, who unfortunately left in a state of impatience about his promotion. After leaving Stony Brook Bill Kern served as a long term program officer at NSF, and was at Northwestern University when I last had news of him.

In the late summer of 1961 I took my two older children on a camping trip at Napeague Harbor, on the north shore of eastern Long Island's South Fork. A seagull spent so much time hanging out around our camp that we came to think of him as a family member, and called him Sam, short for Sam Seagull. Very soon after returning from that camping trip I went to a national American Chemical Society meeting in St. Louis. At a mixer event on the first night I encountered Sam Weissman of Washington University, accompanied by an acquaintance. He began his introductions by asking: "Francis Bonner, do you know Sam Siegel?" Sam Siegel said: "Francis Bonner.....you know, your name is very familiar to me." And I couldn't resist responding: "You know.....your name is also very familiar to me." While neither of us explained his familiarity with the other's name, the encounter led to an important consequence for Stony Brook. Sam Weissman was interested in Stony Brook, and asked me how things were coming along there. I said we were doing well and were always on the lookout for outstanding new faculty. Sam then told me about an extraordinary graduate student from Japan that had come to St. Louis to work with him, and suggested that we keep him in mind for a future appointment. Noboru Hirota, also known as Joe, had come to St. Louis for training in the relatively new field of electron spin resonance spectroscopy (ESR), because Sam Weissman was a world leader in ESR research and there was no one of his stature in the field in Japan. It was a field in which we had every reason to seek representation, and on the strength of Sam's recommendation I resolved to bring Joe Hirota to Stony Brook if at all possible, and tracked his progress from then on. After completing his PhD at Washington University he went to Chicago to do postdoctoral research with Clyde Hutchinson. After two years there he was ready to consider the next step, and we were among the first to invite him for interview. Joe was interested and favorably inclined toward Stony Brook from the beginning, although I believe he would have returned to Japan had there been a comparable opportunity for him there. In the environment prevailing in Japanese academia at the time, obtaining his advanced training abroad had essentially taken him out of the running for appointment at a top level Japanese university, and he accepted our offer.

Joe Hirota turned out to be a splendid colleague in all respects, scientifically and personally, and so productive of first class research that within five years we had promoted him to a full professorship. His rapidly rising reputation brought international recognition, and it inevitably came to be realized in Japan, in due course, that a mistake had been made. At first Joe resisted all efforts to lure him back, but eventually there came an offer he couldn't refuse, from Kyoto University, where he had received his undergraduate education. He returned to Japan in 1978, after ten years on our faculty. It was difficult for us to see him go, and it wasn't easy for Joe either, because by that time he had become thoroughly Americanized, and he and his wife had two American born sons.

Our tradition of Friday afternoon colloquia, begun in the earliest days at Oyster Bay and carried on under cramped conditions, now flourished in our new 240 seat lecture hall. Among the distinguished colloquium speakers to visit Stony

Brook during the 1964-65 season was Henry Taube of Stanford University. In the course of a private conversation in my office I asked Henry whether he could suggest anyone who might be interested in joining us here at senior faculty level. In response he told me, in strict confidence, that he had reason to believe Harold Friedman could be persuaded to leave his then current position at IBM Research in Yorktown Heights. Henry Taube's own first PhD graduate from Chicago, Harold had been a member of the University of Southern California faculty for ten years before joining IBM. While he had begun his research career as a Taube-style mechanistic inorganic experimentalist, his interests had shifted increasingly over the years toward theoretical physical chemistry. Employing the methodology of statistical mechanics, he had brought greatly improved understanding to aqueous electrolyte systems, which was very impressive to me because I knew only too well how severe were the limitations to electrolyte theory at the time I was doing my own doctoral research. Harold had also spent some time in Brussels in collaboration with Ilya Prigogine, then the foremost proponent of non-equilibrium thermodynamics. While still able and interested in experimental research, Harold had become almost entirely a theoretician by the time I got to know him.

Harold sounded like a great addition to our faculty, and I was very excited about the possibility. Following Henry Taube's strictures of confidentiality I proceeded to make a discreet overture. In fact it was so discreet that several weeks went by before I even received a reply. After several more weeks Harold came out to visit Stony Brook for the first time. He made a strong impression on everyone, as both a splendid person and an outstanding scientist, and we were anxious to succeed in the recruitment effort. Harold was basically noncommittal at first, but stayed in touch with me and kept the door open to negotiations. After one or two more visits he brought his delightful wife Edith, first to the campus, and then to our home for an overnight, and at that point we began to sense the possibility of a great friendship. At a time now somewhere well beyond the midpoint of spring semester, communication virtually ceased, to my concern and surprise. In the continuing spirit of discretion, I didn't press for an answer, but it was clear by this time that if Harold were to come at all it could not be before the midpoint of the following academic year 1964-65. I was preparing to leave for a fifteen month sabbatical in France, and Sei Sujishi had consented to serve as Acting Chair during my absence, so it would be up to Sei to shepherd the appointment through to completion if Harold were to say yes. I remained hopeful, but at the same time I was uneasy that so much time had gone by without hearing from him.

Early in June we departed for France on the Italian liner *Aurelia*, from one of the major piers on the west side of Manhattan. After we'd seen to all the details concerning tickets and luggage, the five of us were about to ascend the boarding ramp, when suddenly Edith Friedman appeared, bearing a departure gift and a hearty wish of *bon voyage* for us all! After that lovely surprise I felt quite confident that Harold would be joining us at Stony Brook after all.



## VII

### TIME OUT

Academic year 1964-65 would be my seventh year on the Stony Brook faculty, and I would become eligible for a sabbatical leave, which I had long anticipated and considered well-earned. In addition to the prospect of relief from heavy administrative pressures, I was eager to launch a strong effort to resuscitate my research career. While I had participated fully in the Department's teaching program, it had been impossible for me to give priority to research, while simultaneously teaching and discharging the responsibilities of the chairmanship, during those first six years of administrative pressure, turmoil, and excitement. My collaborative arrangement with Oliver Schaefer and his group at BNL had been very helpful, and remained in place, but by now the time and energy I could devote to it had seriously fallen off.

To spend an entire year on sabbatical I needed financial support equivalent to half my annual salary, and for that I applied to the National Science Foundation's Senior Postdoctoral Fellowship program. For the application I had to identify an institution that would be ready to receive me, and approached Étienne Roth, head of the Service des Isotopes Stables at the Centre d'Études Nucléaires de Saclay, a laboratory of the Commissariat à l'Énergie Atomique (C.E.A., the French Atomic Energy Commission), at Gif-sur-Yvette, a short distance south of Paris. I had become acquainted with Étienne during a sabbatical period of his own at BNL, and we were coauthors with Oliver on an article concerning the long-lived radioactive isotope chlorine-36. Étienne extended a warm invitation for me to come to Saclay, and offered to provide financial support from the C.E.A. if it would be needed. Early in 1964 I was delighted to receive a favorable response from the NSF, informing me that I had been awarded a Senior Postdoctoral Fellowship. Planning a full fifteen month sabbatical period in France, we booked our departure for the first week of June.

Our passage on the Italian liner *Aurelia* was a crossing sponsored by the Institute for International Education (IEE), and many of the passengers were students on their way to study abroad opportunities. The *Aurelia's* dimensions turned out to be nearly perfect for the induction of seasickness, which began to take a heavy toll as soon as we reached open ocean, just beyond the Verrazano Bridge, then under construction. Since I have never been seasick I was not bothered by the ship's exaggeratedly rolling motion, but all four of my family members were among the afflicted. On the first morning at sea I was surprised to find myself one of just seven passengers showing up for breakfast, out of 1100 on board. The period of seasickness was mercifully short lived, however, and within 24 hours everyone was up and about and enjoying the leisurely crossing,

which was due to last for ten days. The weather was beautiful, we set our watches ahead just one half hour each day, and it was a truly restful time, disconnected from the world. I think of it with nostalgia every time I board an airplane for an all too quick intercontinental flight.

After a first landing at Southampton, we arrived at le Havre on June 14. Proceeding by train to Paris, we were met at the station by Étienne Roth and his wife Françoise, who took us on our first tour of the city. With help from Françoise, we had the good fortune to locate and move into an old (early 19<sup>th</sup> century) but comfortable, unattached house in the 14<sup>th</sup> Arrondissement, on the Boulevard St. Jacques, just two blocks from the busy intersection Place Denfert-Rochereau. From there I had an easy commute by automobile to Saclay, and we enjoyed ready access to all of Paris on the Metro. We enrolled the children in the bilingual school *École Bilingue*, on the right bank of the Seine. After classes began in September they were picked up by bus each weekday morning, and returned by Metro, on their own, at the end of the school day.

While living at 34 Boulevard St. Jacques we soon became aware that there was also a Rue St. Jacques not very far away, and even an apartment building at number 34. We tried to warn people about this when they were coming to visit us, but in one instance the warning was neglected. My friend Leo Yaffe of McGill University, then serving a term at the International Atomic Energy Agency in Vienna, was in Paris for a brief visit, and set out with his wife to call on us. They stopped at 34 Rue St. Jacques, and rang every doorbell in the building before realizing they had made a mistake.

My new colleagues at the Saclay laboratory were cordial and welcoming. Most of them spoke to me exclusively in French, and since I would occasionally hear English being spoken by someone who had never spoken a word of it to me, I soon realized that this was by prearranged policy. While difficult for me at first, I thought it quite appropriate, and it benefited my effort to achieve fluency as quickly as I could, and steadily improve it, in conjunction with my participation in evening class sessions at the *Alliance Française* in Paris.

At Saclay I was welcomed and soon settled into a laboratory suite headed by Eiichi Saito, who had previously spent some time at BNL. While a student in France in the '30's, Eiichi had been caught in place there by the outbreak of World War II in 1939. He returned to Japan after the war, but soon returned when he realized he had become incurably adjusted to the French lifestyle, and by now had even become a citizen of France. Eiichi had many talents, artistic as well as scientific, and in the latter category he was a very knowledgeable and skilled experimentalist. There were several women pursuing independent research as senior members of his group. I found it interesting to note that the proportion of women in science, not just at Saclay but everywhere in France, was at that time much higher than in the US. While I liked to think the difference could be attributed wholly to the Nobel laureate examples Madame Curie and her daughter Irène Joliot-Curie, the massive loss of French male lives that occurred during



World War I was surely a contributing factor. Another member of the group was Elie Hayon, an Englishman who had also been at BNL, and who worked closely with Eiichi. A certain Monsieur Ducheylard was assigned to work with me as a mass spectrometry technician. He was a very nice man, and we developed an interesting and friendly working relationship, although mutual use of first names proved to be beyond reach. After some time I realized that M. Ducheylard obtained his most reliable results in the morning work hours. Since wine is widely considered essential at lunch time in France, it was plentifully available at the Saclay canteen, in high quality and low prices subsidized by the state.

I devoted several weeks of the summer relearning, revising and improving experimental techniques needed for my intended research program, designed to explore the reactive chemistry of nitrogen compounds, particularly oxides, in aqueous solution systems, employing the stable isotopes nitrogen-15 and oxygen-18 as probes. In some of this work I would be following up on previous work I had carried out at BNL. The summer weeks went quickly by, with learning about and adjusting to the life and mores of a French laboratory, and construction, testing and adjustment of a vacuum system incorporating the numerous features essential for the experimental work yet to come.

Since nearly the entire month of August was then widely considered vacation time in France, we spent most of it away, first for a relaxing stay near Les Gets, an Alpine formation in the Haute Savoie region. An experience there gave us a striking impression of the French postal system. A colleague from the Stony Brook French Department misaddressed a letter to us by writing 34 Boulevard St. Germain instead of St. Jacques. A colleague from the Stony Brook French Department misaddressed a letter to us, writing 34 Boulevard St. Germain instead of St. Jacques. We had given the local post office in Paris our vacation address at Les Gets, and the letter reached us there after just one day of delay. Our stay in the Alps was followed by an extended trip in our Peugeot 404 sedan through central and southern France, into the Pyrenees, then briefly to Spain and Andorra before returning to Paris at the end of August.

Back in Saclay, my experimental program began in earnest. Essentially, through the entire time it could be said I was serving as my own postdoc, since aside from assistance with mass spectrometry, occasional glass blowing and vacuum system maintenance, it was basically hands on for me. I determined all experimental conditions, carried out the corresponding experiments, did the calculations and evaluated the results. In addition to the Saclay staff personnel I got to know several graduate students who were doing dissertation research at Saclay, and some of their supervisor professors from campuses of the University of Paris. There were interesting lectures to attend, both at Saclay and the nearby Orsay Laboratory.

One day in January, Professor Moise Haissinsky, Director of the Institut Curie in Paris, reached me by phone to invite me to give a lecture there. I had come to know him during the previous year, when I assisted publication of an

English translation of his nuclear and radiochemistry text. He visited me in Stony Brook during the previous spring while returning to Paris from a stay at the Argonne Laboratory near Chicago. I accepted the invitation, and we agreed on a title, date and time for my talk. Neither of us brought up the question of language, and I was taken by surprise when a notice quickly appeared throughout the Paris region to announce the date and title for my lecture, specifying that it would be presented *en français*. I should have expected that, because when I saw Haissinsky in Stony Brook he related to me with some pride that he had just spent two weeks in Chicago speaking French exclusively, in retaliation for a previous two week visit at the Curie Institute by an Argonne visitor who spoke only English. Nervous about my talk, I drastically over-prepared, writing everything out in detail to be sure that vocabulary and syntax, as well as the science, were appropriate and correct. When the appointed day arrived I was surprised to find a near capacity audience gathering in the lecture hall of the Curie Institute's ancient Left Bank headquarters. Seats were occupied all the way to the top of the very steep hall, where I could see my wife Evie and, in a pleasant surprise, my friend Oliver Schaefer, newly arrived in Paris. I carefully arranged my extensive script on the well-lit lectern at the front of the room, and Professor Haissinsky introduced me. I began my talk, and quickly realized that I was depending so utterly on what I had written that I was basically reading instead of talking. Desperate to break out of that mode, it turned out I didn't have to wait very long. Following a brief introduction I called for the first slide, "premier cliché, s'il vous plait," whereupon the projectionist turned out all the lights in the hall, including the one on the lectern. I had no choice but to abandon my reading, relax, deliver the talk in a conversational mode, comment on the slides one by one, all in the most polished French I could summon. The talk went well from then on, and was followed by a lively question period, suggesting that it had been understood.

My communication with the Chemistry Department in Stony Brook, and my receipt of news about ongoing affairs there was limited throughout my time in Paris. While this happened in large part because I had said I wanted it that way, the primitive condition of the telephone network in Paris at the time was also a contributing factor. Our phone number at 34 Boulevard St. Jacques came with the house, appeared to have been unchanged for many years, and there was no way it could be found under our name in a phone book. When we first moved in we received quite a few calls for Madame Célestin, about whom we knew nothing. Most of the callers were men, some of whom conveyed a suggestive tone when Evie answered. After asking if she were American, one of them requested permission to visit. The house contained an unusually large number of bidet installations, an additional clue to the nature of Mme Célestin's business.

The first communication I received from Stony Brook was from Sei Sujishi, conveying the happy and important news that Harold Friedman had accepted our offer, and would be joining the Department in the fall of 1965. It was exhilarating to have that confirmed, and know that the Department's strength in physical

chemistry would be greatly enhanced by Harold's presence, his scientific stature and his fine personal qualities.

Sei's next communication was to tell me that in response to a request I had submitted in the previous year, a non-Civil Service position for a professional glassblower had been approved in Albany, and that Paul Lauterbur had identified an outstanding candidate, a glassblower named Rudolph Schlott, who had made a strong impression on him at the Mellon Institute in Pittsburgh. I asked Sei to follow up on this lead, and when I returned to Stony Brook in September, 1965 Rudy Schlott was in place there with a fully equipped glass shop. Rudy was an outstanding master craftsman, and because of my own extensive and continuous need for glass shop services in my research program his appointment meant a great deal to me personally. He was a great asset indeed to the entire Department, and to several other units of the University as well, during a long and productive career that came sadly to an end with his untimely death from cancer in 2005.

Alec Pond, another important and regular communication channel from Stony Brook, sent me occasional reports about the Presidential search. Before I left, we had been doing all we could to stimulate and encourage the candidacy of John Toll, and to prepare the way for a positive response to him in Albany. A key event in 1964 was the appointment of Samuel Gould as the chief officer of SUNY. He arrived in the fall, and all available information suggested that this would be a favorable development for Stony Brook: Gould's record as Chancellor of UC Santa Barbara was strong, he supported the concept of University Centers characterized by excellence, and had quickly developed a strong relationship with Governor Rockefeller. It seemed clear that if John Toll were to become serious, there would now be someone there that he could engage in substantive negotiations. And before long it became clear that Toll *was* serious. He prepared a visionary, impressively detailed document, spelling out in detail the commitments from Central Administration and the State that he believed to be essential for Stony Brook's development over the coming decade ahead, and it was this vision that constituted the heart of his negotiations with Albany. Early in 1965 I received an ecstatic communication from Alec, informing me that John Toll had accepted an offer of the presidency and would be coming on board in the fall. I too was ecstatic: the Stony Brook future that we had hoped and dreamed about, worked so hard for, now seemed assured.

In 1964, the State Legislature and Board of Regents created two special, State-sponsored professorships for New York's public and private universities. There were to be two categories, one in the sciences, named for Albert Einstein, and the other in the humanities, named for Albert Schweitzer. These became known as the "Alberts." Subsequent to his news about John Toll, Alec informed me that he and his colleagues in Physics had succeeded in persuading the State agencies to offer an Einstein Professorship to Chen Ning ("Frank") Yang, a Nobel laureate physicist then at the Institute for Advanced Study in Princeton, and that

Yang had accepted it and would be coming to Stony Brook. This too was exhilarating news. In the coming years I was all too frequently asked the question "When will the Chemistry Department be getting *its* Nobel laureate?" I invariably responded by saying "we're growing them *in situ*." It took a long time, but I was happy, excited, and felt exonerated when that response was at last corroborated in 2003, when Paul Lauterbur was awarded the Nobel Prize for Medicine or Physiology for his discovery of magnetic resonance imaging (MRI).

News of a less welcome variety came next from Sei, when he called to inform me that the senior faculty of the Chemistry Department had reviewed the tenure and promotion files of Arthur Lepley and Robert Schneider, our two assistant professors then in their fifth year of appointment, had turned them both down, forwarded both negative recommendations to the administration, and so informed the candidates. It was quite clear that the intent of Sei's call was to inform me about this as a *fait accompli*, and not to discuss it or invite my participation. While consistent with the letter of what we had agreed to before I left, I found it disappointing, to say the least. If asked, I would have recommended an additional one year appointment in each case, which was permissible under SUNY Trustees' regulations, would have given the candidates some vital extra time, and assured my full participation in the decision. There appeared to be little recourse, and even if I had hopped on a plane and come over it seemed unlikely I could change the outcome. Both of these cases presented issues similar to those that had concerned me in the Goldfarb case, to which there was an obvious relation. Both Lepley and Schneider had come at what turned out to be a turbulent and distracting time, and contributed well to all aspects of the Department's life and programs, conspicuously so in the case of the intercampus move. They had worked hard to get research programs underway that in my opinion showed good promise. Again, as in the Goldfarb case, I thought Lepley and Schneider deserved an at least minimal level of special consideration, taking into account the unusual circumstances that had prevailed during their early years of appointment.

Not trying to argue with Sei because I knew him better than that, and seeing no way to engage myself in the situation effectively without actually going over, I felt constrained to let it go, but promised myself to try to do something remedial about both cases when I got back. Before I got back, however, Art Lepley, hurt and disappointed, left for an unpromising appointment at Marshall University in West Virginia. He got in touch with me several years later, when he was devoting a sabbatical year to research with Cheves Walling at the University of Utah, and hoping to secure a better appointment from there.

When I returned from Paris Bob Schneider was still at Stony Brook, in the final year of his assistant professorship appointment. I attempted to arrange a joint appointment for Bob with a new Continuing Education program then under development. The idea was that Bob would participate in the Continuing Education teaching program and coordinate it with ours, while retaining a close relation to and status in the Chemistry Department. But Continuing Education

was a new entity, and Bob's appointment was unfortunately subject to the shifting sands of administration. Although he continued in his research program for some time, his status as a fully participating member of the Department was never restored. In a few years' time he became Director of the Office of Grants Management, and contributed strongly to the entire University community in that capacity for many years. He has contributed to our teaching program through most of the long years of his affiliation with Stony Brook, and is still doing so, strongly, at the time of this writing (2006).\*

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\*Robert Schneider's contribution to Stony Brook's newly inaugurated PhD program should not go unremarked here. He was the dissertation research advisor for Raymond MacKay, who marched alone at Commencement in 1966 as the University's very first PhD graduate, and has since pursued a significant and successful academic career. Another of Bob's PhD students, James DiLorenzo, made a major contribution to the world of electronics as the father of gallium arsenide semiconductor, and continues to pursue an outstanding career at the top level of that industry. A third Schneider doctoral student, John Jost, plays an important international role in science information and policy as Executive Director of the International Union of Pure and Applied Chemistry.

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A further communication came from Stony Brook in the spring of 1964, again from Sei Sujishi, informing me that two new faculty appointments had been made in organic chemistry, both of whom sounded excellent. One was George Emerson, who had recently earned his PhD at the University of Texas, where his doctoral research had proven newsworthy: he had succeeded in creating the first known, stable cyclobutadiene complex. The other new appointee was Robert Kerber, who had earned his PhD at Purdue under the direction of our friend Nathan Kornblum. Both Emerson and Kerber were on board in the Department when I returned in September.

While communication with the Stony Brook home base was important it was not time consuming, and I remained well focused on research in progress at Saclay. An episode occurred in the spring of '65 that did briefly threaten that focus, however. I received a notice directing me to come to the central Paris Police headquarters to be interviewed by a certain inspector, whose name I no longer recall. The notice specified a date and an hour for the interview, but gave no reason for the request. Since the police headquarters are located on the Ile de la Cité near the Notre Dame Cathedral, and a midday time interval was specified, it was clear that this would consume an entire day. I discussed it with Étienne Roth, who was as puzzled as I about the meaning of it, but let me know that I had no choice but to comply. At Police headquarters at the appointed hour I was kept waiting several hours for Monsieur l'Inspecteur, only to be told that he would not be available to see me that day and the interview would have to be rescheduled. I staged a small tantrum about that, to no avail, but when I did return several days later it was clear that the inspector had heard about it. He was prompt, apologetic and polite, and explained that he had been required to meet with the American ambassador on the previous occasion. He then asked

me numerous questions about my work at Saclay, how I enjoyed living in France, where I had been and what I had seen. In the midst of this seemingly amiable chit-chat he asked if I had ever been to Pierrelatte. I recognized Pierrelatte as the site of a gaseous diffusion uranium enrichment plant then under construction by the French atomic energy agency, and responded at once: "No, of course not - Pierrelatte's top secret!" After ambling on a bit longer, the conversation came to a close with expressions of thanks and good wishes on the Inspector's part. I concluded at the end that I must have come under suspicion of spying for the US: I was residing in France under support of NSF, a US government agency, after all, and I had background experience in gaseous diffusion technology that would make me a logical choice to spy out the Pierrelatte plant. But I couldn't imagine why they would think the US would be motivated to spook out their gaseous diffusion plant. We had successful plants of our own, and I could see no reason for us to expect to find significant new knowledge in theirs. Several months later, therefore, I could scarcely believe my eyes when I read in the press that the French had caught the US red handed in an effort to carry out airborne espionage at Pierrelatte .

We managed to get around in France quite extensively, and enjoyably, during our time there, and learned a lot about famous French cathedrals and wine regions. Eiichi Saito's wife Alice was a member of the medical college faculty in Strasbourg, and on one occasion took us on a memorable tour of both that interesting city and the nearby vineyard country of Alsace. At the childrens' spring break we flew to Greece for an unforgettable vacation in Athens, the Peloponnisos, and the Island of Paros. Our Stony Brook friends Ed and Natalie Fiess were then on sabbatical in Strasbourg, and came to stay in our house at 34 Boulevard St. Jacques while we were in Greece.

I attended an international meeting on isotope applications in Spoleto, and Evie accompanied me there, for our first time ever in Italy. The meeting and the sightseeing were both excellent. I traveled to England for a seminar engagement in Manchester, and to an international book fair in Frankfurt, Germany in connection with my editorial work. My NSF fellowship provided travel funds, which I applied principally to a visit to the Weizmann Institute in Rehovot, Israel, where my host was Israel Dostrovsky, whom I had met previously at BNL, and who became President of the Institute just a few years later. I enjoyed interesting and informative discussions with him and several of his colleagues, and joined a field trip to the Dead Sea. This turned out to be the first of many visits I would make to the Weizmann Institute. I also visited and became acquainted with scientific colleagues at the Technion, in Haifa, and at Hebrew University, in Jerusalem.

By midsummer it was time for me to wrap up my experimental program and prepare for the return to Stony Brook. While I had not accomplished all I had hoped, I had definitively pinned down some central aspects of the research, and opened up promising new avenues for future investigation. I enjoyed the feeling of return to research with hands on, and the confidence and desire to continue,

that the year had brought me. I said my farewells to Saclay and my many friends there in a spirit of gratitude, particularly to my generous and helpful hosts and good friends Étienne Roth, Eiichi Saito, and their wives.

We booked return passage on the giant, recently built superliner *France* for a sailing date in late August. After the steamer trunks were all packed and picked up for delivery to le Havre we departed Paris. Our trip to the port city was leisurely, and included some sheer vacation time in Bretagne. In due course we delivered our trusty Peugeot 404 to the dock: it was going home with us. And in further due course, when we went on board and settled into our cabin quarters, we found them much more spacious and luxurious than had been the case on the *Aurelia*. The return trip, which didn't produce a single symptom of seasickness, took only five days. That seemed pretty fast in comparison with our previous experience, but it was a very smooth and enjoyable trip nevertheless. The first thing we noticed as we approached New York harbor was that the Verrazzano Bridge had been completed during our absence, and we could even see cars driving across it. Newly arrived on the west side of Manhattan, Sei Sujishi met us at dockside and ceremoniously presented me the key to the Chairman's office. I was back in the saddle.





## VIII

### BRIGHT FUTURE ASSURED

The July 30, 1965 issue of the AAAS journal *Science* contained an extended news article bearing the title: "Stony Brook: Young and Ambitious New York Institution is Beginning to Stir Notice in Academic World." Virtually proclaiming the emergence of a major new research university on Long Island, the article reported that the physicist John Samson Toll of the University of Maryland was about to become President of this small and hitherto little known institution, that the distinguished biological scientist H. Bentley Glass, of Johns Hopkins University, would join him there as Academic Vice President, and that the Nobel laureate Chen Ning Yang had also agreed to come to Stony Brook, as Einstein Professor of Physics. All this news was appropriately interpreted in the article as a sign that the State of New York was serious about building a top flight research university at Stony Brook. Additional evidence cited in the article included the news that Harold L Friedman would be leaving IBM Research to join the new university's Chemistry Department.

That was the situation that greeted me as I returned from my sabbatical year in France. It was what we had been striving for from the beginning, and achieving at a very slow pace, beset by uncertainty and anxiety every step of the way as we struggled to overcome major obstacles, setbacks and reversals. The appointment of Samuel Gould as the new Chancellor of SUNY was a crucially important link in the sequence of events that led to John Toll's agreement to become the President of Stony Brook. The most important factor of all, however, was the character of John Toll himself. As a conditional prelude to his acceptance of the position, he had spelled out his own vision for the University in tireless detail. That done and accepted, he came, bringing his apparently infinite capacity for hard work and unlimited powers of persuasion, both of which would be needed to bring the vision to reality.

An anecdote will illustrate Toll's tirelessness: in mid Spring semester of 1966 some members of his immediate staff, concerned to see him working around the clock without letup, attempted to draw him away from his desk for a leisurely restaurant dinner. Toll expressed gratitude for their concern, and for the invitation, but said without hesitation that he could not possibly find time to accept it "before the end of the semester."

Following the arrival of Bentley Glass to be our first Academic Vice President, other important posts in the Toll administration were soon filled. Sidney Gelber returned to the Department of Philosophy from the Mannes School of Music, where he had been serving as interim president. Before long he was

appointed to a new position carrying the title Vice President for Liberal Studies. Eventually, Sidney succeeded Bentley Glass as Academic Vice President upon the latter's retirement in 1971. John Toll's vision for Stony Brook called for immediate, concrete planning and development for a Health Science Center and University Hospital, and during his first presidential year he brought Edmund D. Pellegrino aboard to be our founding Vice President for Health Science and Dean of the College of Medicine. Alec Pond was a top advisor to John Toll from the very beginning, and his role in the administration was soon formalized with the title Executive Vice President. This position was officially described in later editions of the University's Blue Book as "alter ego of the President."

Concurrent with John Toll's recruitment, an entirely new Department of Earth and Space Science was established, and Oliver Schaefer came over from BNL to become its founding chairman. As I had played some part in his recruitment, I was delighted by this development. Though the first round of campus construction was still far from completion, its austere red brick architectural phase was now over, and construction of an excellent and handsome new building for Earth and Space, designed by the architectural firm Gruzen and Partners, was soon under way. Governor Rockefeller came down from Albany for the ground breaking ceremony. Assisted by the growing reputation of Stony Brook, and the generous resources that were made available to him, Oliver quickly developed an excellent Earth and Space Science Department. He had earned his PhD in physical chemistry at Harvard under the great George B. Kistiakowsky, and after coming to BNL had developed deep interest, expertise, and a fine record of scholarship in geochemistry and cosmochemistry. Ollie was uniquely qualified to put together the tightly knit team and environment that characterized this multidisciplinary Department. He played a prominent role in studying the moon rocks that were returned by the Apollo mission later in the decade. A congenital hunchback, Oliver had had much to overcome in attaining his high level of achievement. When his heart gave out, leading to his early death in 1980, the Earth and Space Science faculty unanimously requested that his memory be honored by naming their building for him. The request was denied.

Recognizing on arrival that the Department of Mathematics was not off to a promising start, John Toll gave high priority to the search for new leadership in that discipline. In one of his more stunning recruitment strikes, he brought in James Simons, a brilliant mathematician with an outstanding record of scholarship, to chair the Department. Provided with a major number of senior faculty positions and related resources, Jim Simons developed a Mathematics Department of stellar quality in a very short period of time. Having done so, he left the University after a decade to found a resoundingly successful company called Renaissance Technologies, dedicated to the application of higher mathematics to higher finance. Continuing to reside in the area, Jim has been a supportive good friend of the University, and of the Community as well, in numerous ways.

In the Chemistry Department we enjoyed excellent support and resources for our own faculty recruitment efforts, and they continued to be a priority activity to the end of the '60's. With an effective Departmental committee structure long since developed, and settled into a regular routine of faculty meetings, where most program and policy issues, including those related to faculty recruitment, were sorted out and discussed. Decisions to offer faculty appointment were addressed by the tenured senior faculty group, which had by now attained a size large enough to minimize frictions.

Harold Friedman learned from his mentor Henry Taube that Albert Haim, who was then at Penn State, might have an interest in coming to Stony Brook. Albert earned his PhD with Wayne Wilmarth at USC, carried out postdoctoral research with Taube at Stanford, and was now rapidly developing an excellent reputation in the field of inorganic reaction kinetics. After an interview and seminar visit we quickly offered him an associate professorship, and were glad when he accepted. Albert has contributed immensely to the University and Department over his long, outstanding career in both teaching and research, and to BNL as well, in his collaboration there with Norman Sutin. I learned soon after Albert's arrival that he didn't actually *need* a chairman, a category of faculty previously referred to in this memoir, and my own personal relationship with him was initially uneasy and difficult. This situation was greatly improved after 1970, when I left the chairmanship, and we became good friends.

George Emerson, a young and promising organic chemist from Texas, arrived in the fall of 1965. He was bright and personable, and seemed to us all to have excellent potential for a major career in his field. He got his research up and running quickly and well, following up on the newsworthy success of his graduate research. By all accounts his performance in teaching was excellent. Very early one morning in 1967, George's third year in the Department, I received a call at home from Paul Croft, bearing the grim news that George Emerson had been found dead at his desk. It was an unbelievable shock. George had apparently gone to the first floor stockroom during the night, secured a bottle of cyanide and returned with it to his third floor office, where he ingested a fatal dose. No suicide note was found, and the sad event remains an unexplained mystery, although colleagues that had been in closest contact with George realized in retrospect that he had shown signs of clinical depression. The Department memorialized his brief presence among us by creating the George Emerson Award, which is presented annually to the undergraduate chemistry major achieving the highest record of success in the difficult junior year of our curriculum.\*

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\*In a curious but sad footnote to the loss of George Emerson, I am reminded that the first two Stony Brook faculty deaths were suicides. During our last year at Oyster Bay James Raz, a dynamic, young and promising member of the Physics Department, took his own life by gunshot in his parked car.

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Crystal structure determination by X-ray diffraction, a method of broad utility and importance in many branches of chemical research, was an unavoidably tedious and long drawn out procedure before the age of computers, and it wasn't until the early '60's that the possibility of computer-assisted X-ray crystallography came within view. Via Harold Friedman's previous relation to IBM Research, we learned with great interest that what was thought to be the world's first computer-controlled X-ray diffractometer had been created there by a man named Yoshi Okaya. We invited Okaya to visit, to discuss the possibility of his coming to Stony Brook. Before making him an offer, however, we had to make sure we would be able to acquire the computer equipment that would be essential to his program. In all of our experience to that date we had found New York State's capital equipment budget examiners unbelievably skeptical and negative about computers. We had not been allowed to purchase a single computer with State funds. When we proposed the concept of a central computer facility that could be applied to a wide range of research instrumentation within the Department, we found the official budget examiners still negative, but at least willing to allow us to rent such a system with an option to purchase. So we went ahead, first securing Okaya's agreement to join us in 1967, then leasing a recently developed IBM system that appeared well matched to the need. If my recollection serves correctly, the central processing capacity of the system was 16 kilobytes. One year later, when we opted to purchase the system at a cost of \$500,000, the CPU was upgraded to 32 kb.

A large air-conditioned space was dedicated to our new, central computing facility, and with the assistance of our able electronics shop, Yoshi soon had a computer-assisted X-ray diffractometer up and running. While pursuing his own crystallographic research, he then set out to demonstrate the capacity of the system to control several instruments simultaneously, employing his own time-share software. The next instrument to get hooked up was a mass spectrometer then in dedicated use for my own research program, and located in an area of the building entirely separate from the diffractometer. It was a joy to find that the simultaneous computer control method worked beautifully for both instruments. Siegfried Jordan, a postdoc in my laboratory from the University of Mainz, made extensive use of the newly computerized mass spectrometer in carrying out the multiple isotope ratio measurements at the heart of our exchange reaction studies. As other instruments were successfully added, one by one, it struck us that this method, to the best of our knowledge unique, might serve as a useful example for other research organizations. Yoshi and I prepared a proposal to NSF, seeking support for further elaboration of the concept, and to demonstrate its power and potential. The cost of computers at that time was rapidly falling, however, while their speed and capacity were equally rapidly rising. The day of the instrumentation-dedicated computer was much more nearly at hand than we had realized. Our proposal was politely received but went nowhere. The New York State budget examiners' stubborn resistance to computer purchasing persisted well into the '70's, when instruments began to appear that could neither be bought nor operated without computer control.

As our faculty size increased, the relationship between the BNL and Stony Brook Chemistry Departments, which had always been strong, became ever more so. During the last half of the '60's decade several BNL colleagues came over to spend varied periods in collaboration with members of our Department, and in some instances contributed to our teaching program as adjunct faculty. Marshall Newton, for example, contributed a great deal over many years through his research collaborations, with Harold Friedman, and later with Jerry Whitten and George Stell, and his occasional teaching. Other BNL colleagues that have either spent some time in residence, or participated in our programs, or both, include Norman Sutin, James Ibers, the late Stanley Seltzer and (also late) Walter Hamilton.

I had come to know Max Wolfsberg well as a BNL colleague, and also through my participation in the annual Gordon Conference on the Physics and Chemistry of Isotopes. When he told me one day that he was thinking about seeking an academic appointment but was hesitant to leave BNL, I suggested that he consider enjoying the advantage of both worlds by joining us at Stony Brook on a half time basis. The idea intrigued him, and as my colleagues were also enthusiastic I set out to obtain the administrative approvals needed to create such an unusual arrangement. When in due course the principle had been accepted at both BNL and Stony Brook the first BNL-Stony Brook joint appointment was created for and accepted by Max. While there were hitches that had to be ironed out, it worked generally well, and set the stage for the numerous joint appointments that have since been made. Max was an excellent colleague and contributed well to the Department while he was with us, but unfortunately that was not very long. The University of California was starting up a new campus at Irvine, and our friend and fellow isotope enthusiast (and future Nobel Laureate) F. Sherwood Rowland was engaged to build a new chemistry department there. Sherry targeted Max for early recruitment to Irvine, and succeeded in luring him away from the dual orbits of BNL and Stony Brook.

Not long after we lost Max Wolfsberg, it came to my attention that Jacob Bigeleisen, who had then been at BNL for twenty years, had decided to leave BNL and was looking at professorship offers. Jake and Maria Mayer had coauthored a seminal paper on the calculation of equilibrium constants for isotopic exchange reactions in 1947. Through his continuing pioneering contributions to fundamental understanding of both thermodynamic and kinetic isotope effects he had become a world leader in the field. When I told him that he would be most welcome to join our Department, he said it would be out of the question for him to accept an offer from Stony Brook, one reason being that he wanted to leave Long Island, as well as BNL. Of his several offers, Jake accepted and joined the University of Rochester, where he also served as Chairman of the Chemistry Department for several years. Some ten years later, however, he returned to Long Island after all, as Vice President for Research, with the additional title Professor of Chemistry, at Stony Brook. Upon leaving the Vice Presidency a few years later, Jake became a full time participating member of our Department until his retirement in 1989.

In 1966 we learned that Melvin Simpson was coming to Stony Brook to become the founding chair of a new Department of Biochemistry. That was welcome news, because development in the life sciences at Stony Brook up to this time, while strong, had been largely focused on the macroscopic, whole organism aspects of biology and was in my opinion seriously lacking in molecular underpinnings. Biochemistry was the first of an extended array of Life Science departments established during the Academic Vice Presidency of Bentley Glass, all of them essential to the development of the Health Science Center. The Biochemistry Department has the unique status of a joint academic unit: it belongs equally to the College of Arts and Science and the Health Science Center. An outstanding department was developed under Mel Simpson, and the Chemistry and Biochemistry Departments have enjoyed close collaborative relations from its earliest days.

Mel Simpson became available to Stony Brook because an ongoing crisis at the Dartmouth Medical School stimulated him to look elsewhere. This situation created a bonus opportunity for us, because Arnold Wishnia, then also at Dartmouth and similarly stimulated, became available and joined our Department. Arnold has carried out important research, and been a valued faculty member and colleague in the Department, since his arrival in Stony Brook in 1967.

The tenure case of Robert Boikess came up for review in 1967. A physical organic chemist who had earned his PhD at UCLA with Saul Winstein, Bob had come to us highly recommended. Extremely bright and personable, he proved to be a fine teacher, got along very well with the students, and we had high hopes for him in research. But for whatever reason, he never got a viable research program in motion at Stony Brook. In addition to the presence of multiple distractions in these times of student unrest, his morale may have been affected by the atmosphere of conflict that afflicted the organic chemistry sector. A negative decision was inevitable, and Bob wasn't a bit surprised when I informed him about it. He went on from here to an appointment at the Douglas College of Rutgers, where he later served as Department Chair, and wrote a successful freshman chemistry textbook.

Dick Solo also came up for tenure review in the late '60's. He had proven himself an excellent teacher, but despite clear diligence in research his record of accomplishment was not promising. A non-promotion decision was made after long and careful deliberations, and this action resulted in the first ever student protest in the Department, in 1968, led by Stu Novick and Al Porter, two of our undergraduate chemistry majors. The graduating class of 1968 also created an "Outstanding Professor" award, and presented it to Dick Solo. I believed Dick had a very good chance of securing appointment in a quality four year college, and offered to help to that end, but he elected to stay on at Stony Brook in the undergraduate studies and counseling administration. He became the founding Director of New Student Orientation, and served the campus well in that capacity for a couple of decades. In recent times he has returned to teaching in the

Chemistry Department, helping out in the introductory courses, where large enrollments have brought heavy teaching loads.

In 1967 I began to receive reprints of Ben Chu's research publications in the mail. I was glad to get word of him for the first time since he declined our 1962 offer and went to Kansas, and both the articles themselves and the frequency of their arrival were impressive. Realizing that Ben was trying to tell me something, I invited him to come for a visit. He had been following the development of Chemistry at Stony Brook with great interest all along, and I was delighted to learn that he would now be receptive to a renewal of our offer. Ben's career had advanced in a major way since 1962, and as he was now an associate professor at Kansas it was only appropriate to begin our negotiation at the full professor level. He also had substantial equipment needs, which caused the negotiation to extend over several months. It was a great help that by this time our second chemistry building was in early planning stages and we were able to make commitments against a new capital equipment budget. In the end we were able to satisfy his equipment needs quite handsomely, and it was a landmark event in departmental history when he agreed to join our faculty. Ben arrived in the fall of 1968, with an accompanying entourage that included Jeffrey Shook, who became a mainstay member of the Department's electronics shop.





## IX

### CAMPUS LIFE IN THE SIXTIES

Upon John Toll's arrival in 1965, the entire plan for Stony Brook's future, in enrollment and facilities, was suddenly and greatly enlarged. The first phase of construction, in the "Georgian colonial" red brick architectural style often referred to as "neo-penal," was still ongoing but nearing completion. But thanks to Governor Rockefeller's creation of the State University Construction Fund, new and bolder and better building plans were coming into the pipeline at an increasing pace. The campus quickly became a single giant construction site, stretching on into what appeared to be an indefinite future. The central red brick library, for example, which had been the focal point of the first phase and became the first home of the new Toll administration, was declared woefully inadequate even before it was completed. The major expansion it required was subsequently accomplished by construction on all four of its sides, enclosing the original within a single immensely large new building. Wherever one went on campus it was necessary to navigate with care around construction sites. In a believable and widely retold story of the time, Graduate School Dean Herbert Weisinger, while strolling on the campus with President Toll, remarked: "God, this place is a mess!" To which the President responded: "No it's not! Wait till you see it in five years!" The widespread concern among the students about this subject was expressed by lapel buttons bearing the message "Now, Not 1980!"

Among my numerous personal recollections of the time, here is a favorite: I observed a hazardous situation at the back exit to the red brick Physics Building, created by construction work in progress on the nearby Earth and Space Sciences Building. Alarmed that someone might get hurt, I hastened to my office to dictate a memo to construction headquarters, then in the Library. I received a detailed response within one hour, describing in detail the measures that had been taken in anticipation of this hazard, and were now in place at the site. But there was a problem: nothing at the actual site matched this description. I could only conclude that the memo answerer's "information" had been obtained by reading a blueprint, instead of stepping outside to check out reality.

Living and working on a campus that was itself undergoing continuous and rapid change was a circumstance that I found simultaneously disorienting and exhilarating. Necessitating an alert and flexible attitude, it became a new way of life. An additional feature was that gross errors and displays of incompetence occurred with such regularity and frequency that we came to expect them as a built-in aspect of the new way of life. It was no surprise, for example, when a bridge that was built to unite the Student Union Building with the Melville Library came up short of that connection.\* It became known as the infamous "Bridge to

Nowhere.” After several years it was phoned up with a left turn to the Fine Arts Center, providing it at last with a destination. By 2002 it had seriously deteriorated, and was removed altogether, to the relief of everyone.

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\*While architectural inadequacy was the principal reason for this, an additional factor was that the librarians at the time opposed the bridge entrance on the ground that it would create a thoroughfare through their reference room. As a result, the Melville Library has always lacked an appropriate main point of entrance.

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One of the hazards that had to be endured was the absence of adequate lighting for pedestrians on sidewalks and roadways at night. This had been anticipated, and an imposing row of street lights was very much in evidence along one of the most traveled pedestrian routes. For some reason, however, these lights simply wouldn't stay lit, especially at night when most needed. The community suffered with this problem for a long time before it was discovered that the electricians had wired the street lights in series rather than in parallel.

The streetlight fiasco was just one example of low technology items that never seemed to work at Stony Brook. Others were wall clocks and building doors. As this variety of unbelievable, self-inflicted calamity became the expected norm, the exclamation “Only at Stony Brook!” came into common parlance. When construction began at the Health Science Center and Hospital site, I can recall being asked several times, and asking others myself: “Would you want to be the first surgery patient in Stony Brook Hospital?” The expected and invariable answer, of course, was an emphatically shuddered “no!” It must be said at once, however, that Stony Brook University Hospital, having long since passed its debut with flying colors, has established itself as a thoroughly safe place to undergo medical procedures of all kinds.

The operational buildings and construction sites were interconnected by a network of steam tunnels, and since these were undergoing constant revision there were steam vent sites spotted about the campus. Commonly referred to as “hell holes,” these were well marked, and the presence of rising steam gave them full visibility. But to the best of my recollection they were not set about with “keep off” signs, or any warning that the steam vents were sites of potentially severe hazard. One of them resulted in an “Only at Stony Brook” event of tragic proportions, when a group of young male students engaged in a “jump over the hell holes” contest at a vent near the Earth and Space Science Building. One member of the group made a faulty jump, didn't make it across, and fell directly into the hole. With no means of immediate rescue available, the unfortunate young man was scalded to death on the steam pipes below. The ESS Department has kept his memory alive by awarding an annual prize in his name, for academic achievement.

The late sixties and early seventies were a nurturing time for close relations between faculty and both undergraduate and graduate students. One tradition that developed in the Chemistry Department was an annual picnic, including a student-faculty softball game, held at Sunken Meadow State Park. I treasure a photograph of myself in batting mode at the plate. While I cannot recall driving in any runs the photo convinces me that I must have done so. In addition to our traditional Friday Colloquium and afternoon divisional seminars, a relatively loose seminar structure developed, and in turn encouraged a greatly enhanced degree of student participation. Some of these occasions were called Symposions, and others became known as BIPO seminars. The term BIPO, standing for "biological, inorganic, physical and organic," indicates inclusion of the full range of chemical specializations: BIPO seminars were for everyone. They were held on Tuesday evenings, usually after dinner with wine at Shepherd's in Setauket, and often followed by joint faculty-graduate student beer consumption.

While we were still confined to the Oyster Bay campus, Ward Melville presented a priceless gift to the University: a property called Sunwood that had been his father Frank Melville's summer estate. Located on Mt. Grey Road in Old Field, it consisted of a very substantial Tudor mansion, complemented by very extensive and beautifully landscaped surroundings. The mansion commanded a dramatic view of Long Island Sound in the westerly direction toward Northport. A footpath from the mansion leads down to a long stretch of private beach below. Rather than open Sound, the beach is located on the relatively quiet estuarial waters of Smithtown Bay, leading to Stony Brook Harbor.

The Sunwood gift was presented as a general facility for faculty, students above undergraduate level, and visitors, with few restrictions on its use. Several bedrooms on an upper floor, commanding spectacular views, were made available to faculty members at low rates, and before the move to the Stony Brook campus Evie and I took advantage of them for an occasional weekend of R and R. These rooms became a major asset for the use of our visitors, many of whom became confirmed Sunwood devotees. One of these was my dear friend and colleague Martin Hughes of King's College, University of London, with whom I was engaged in a long term research collaboration supported by NATO. This brought him often to Stony Brook, and me, equally often, to London. Another visiting colleague who came and enjoyed staying in Sunwood was Geoffrey Stedman of the University College of Swansea, a campus of the University of Wales.

A large and beautifully furnished living room area at Sunwood provided an ideal space for gatherings and performances of all kinds. The Chemistry Department was able to reserve this space for receptions and other special occasions, which gave rise to a new tradition in the late sixties, the annual faculty-graduate student Christmas party. In addition to much good food, drink and merriment at these parties, the graduate students presented satirical skits, using their own made-up names for members of the faculty. Sei Sujishi, for

example, was called Sy Sojuicy, and Harold Friedman became Ichabod Fried Ham. A memorable moment occurred when Sei, Paul Croft and I arrived at Detroit airport for a visit to the architectural firm working on our new building. Paul very briefly detached himself, then rejoined us, and suddenly as we strolled the entire great terminal came alive with a paging message for “Dr. Sojuicy, Dr. Sy Sojuicy!”

A traditional event at the faculty-graduate student Christmas party was the appearance of a graduate student Santa Claus, bearing gifts for members of the faculty. On one occasion, for example, Fausto Ramirez was presented with a massive volume, identified as his *complete* publications, along with a very thin folder purporting to be his *significant* publications. On another occasion the students made Bill le Noble the recipient of 6 inch thick Manhattan telephone directory. They had carefully replaced the cover with heavy duty red paper bearing the title “Errata in ‘Highlights of Organic Chemistry - 10<sup>th</sup> Edition,’ by W. le Noble,” in reference to Bill’s successful textbook, which was then in its first edition. One year I was presented with a packet of Koolaid, in obvious reference to the mass suicide event in Jonestown, Guyana. I didn’t appreciate that very much, but the following year I was touched to receive the unusual gift of a green frog ashtray. During the previous summer when Evie and I were traveling in Cape Breton, we had been forced to seek help from the Tourist Police to find accommodations. They had helpfully arranged for us to spend a night in the lovely antique shop home of one Mrs. Sheila McInerney. After our departure in the morning a Royal Canadian Mounted Policeman tracked us down at a Laundromat, to inform us that Mrs. McInerney had reported the loss of a priceless green frog ashtray and was determined to get it back. The RCMP man declined our invitation to search our overloaded station wagon, and eventually implied that this may not have been the first time he’d heard such a charge from Mrs. McInerney. This story had circulated around the Department, and the graduate students decided to satisfy my yearning for a green frog ashtray. While the version they gave me may not be as “priceless” as Mrs. McInerney’s, for me it’s a one and only.

Sunwood was a tremendously valuable facility for the entire University, and it was a sad day for everyone when the mansion burned down in 1986. A sad night, actually, and a frustrating one. There were no hydrants on Mt. Grey Road, so the water to fight the fire had to be brought in by the fire trucks. Time after time the fire would appear to be nearly extinguished when the water ran out and the truck had to go fill up again. During each filling up period the fire proved able to rejuvenate itself. With President Marburger and others watching, this sequence repeated itself all night long. While all items of value were saved, including a fine Steinway piano, by morning the mansion itself was essentially destroyed. The fire was determined to have had its origin in electrical wiring in the attic. At that time Evie, serving as a Literacy Volunteer, had been teaching one of the University’s electricians to read. Her pupil told her that he had warned his superiors repeatedly, for more than six months, that there was a serious and potentially incendiary situation in the attic wiring.

While the Sunwood beach remained accessible to and enjoyed by the University community in summer months, the ruins of the Sunwood mansion lay sadly dormant for years, until our current President Shirley Strum Kenny managed to find the funding needed to rebuild it. While it was not feasible to rebuild it in Tudor style, the new version is tasteful, charming, and successful, and we're glad to have Sunwood back in full scale use as the President's residence.

There was a serious shortage of concert performance venues on campus during the sixties, while the University waited for completion of the Fine Arts Center. Somehow the Music Department proved itself adept at finding plausible spaces, and good listening opportunities were frequently made available. While I recall many of these performances with pleasure, I have only dim recollections about the actual venues in which they took place, with the exception of the auditorium in the Student Union Building. Musical events had to be scheduled with caution there, because there's a bowling alley located just one level below.

For major, large scale performance occasions the only possible venue was the Gymnasium, and two such occasions that took place during pre-Fine Arts Center times come to mind. The first of these was a concert appearance of the then well known group Pink Floyd. Evie and I attended the concert because a close friend of ours, a film editor in New York, had a film editor friend from London staying with her, who wanted to come out for the Stony Brook concert because her son was the group's principal guitarist. I managed to get good seats for the four of us. The Gym, prepared in advance with a fantastic surround sound installation, was absolutely packed. I tried not to get too high breathing the smoky atmosphere, and recall the entire occasion as highly enjoyable. One of my teacher evaluation forms at the end of that semester bore this comment: "A man who digs the Pink Floyd can't be all bad."

Another major event that was held in the Gym was a concert of Stravinsky's choral music, performed by an orchestral and choral group from New York City, trained and conducted by Stravinsky's well known protégé Robert Craft. While the music and the performances were excellent, the most memorable feature of the concert was that Igor Stravinsky himself was in the audience. We felt privileged, not only to hear his music but to do so in his presence. We were in fact more privileged than we knew at the time, because the occasion turned out to be Stravinsky's last public appearance.

When the Fine Arts Center was at last ready for inauguration, the possibilities were exciting, with its Main Stage, Recital Hall and two theaters. The University had the good fortune to engage an experienced concert manager to launch and direct the Center's programs during its earliest years. His name was Dante Negro, and he was living on Long Island in retirement from a faculty plus concert management career at Brooklyn College, where I had known him in the long ago of my own time there. A splendid person of excellent artistic taste and good management skills, Dante got the Fine Arts Center off to an excellent start.

Under its excellent subsequent management by Terence Netter, followed by his successor Alan Inkles, this facility, now named the Staller Center, has played a central and important role in the cultural life of the University.

## X

### END OF THE DECADE

The first Chemistry Building, which seemed so spacious and comfortable in 1962, rapidly became crowded and squeezed as the decade advanced, due to our virtually continuous expansion of faculty, students and staff. Plans for major new construction throughout the campus were set in motion soon after Toll's arrival in 1965, and several architectural firms were engaged. It was proposed to meet the future needs of Physics, Mathematics and Chemistry with a single, unified, extremely large project, for which planning was assigned to Gruzen and Partners, the architects that designed the successful Earth and Space Science building. Sei Sujishi agreed to chair a Building Committee to oversee the plans for Chemistry in this complex. Sei and Paul Croft worked closely with the architects, and kept me fully informed as the work progressed. The three of us met with the architects regularly, and joined them on occasional field trips. One of these took us to the new UC San Diego campus in La Jolla, where one of the buildings we visited was named in memory of my brother David Bonner, who had played a major role in the development of biological science at UCSD before his untimely death in 1964.

The planning process for this ambitious, three building project took a long time to come to fruition, but eventually a plan was created and ready for bids. The plan, which we thought magnificent, called for extensive use of poured concrete, at the high level that had been successfully deployed in the Earth and Space Science building. By the time the new project was ready for bidding, however, the cost of pouring concrete on Long Island had risen sharply within the local construction industry, and the bids came in at nearly twice the amount estimated and budgeted for the project. To deal with this situation, Alec Pond decided to separate the project into two portions, one for Physics and Mathematics, the other for Chemistry. Gruzen and Partners were asked to redesign the larger, two building sector, and the Chemistry building portion, budgeted at \$24M, was reassigned to the Detroit firm of Smith, Hinchman and Grills. All we knew about our new architectural firm was that it had a reputation for saving money by using modular construction methods, and our initial impression was that we were being treated as welfare clients. This impression was soon dispelled, however, because the new firm worked efficiently and well, and proved very attentive to our needs and desires. Fortunately, they were able to incorporate an excellent laboratory furniture design that had been elaborated for the previously scrapped Gruzen plan. The principal modular aspect of their plan was a regular array of stairwells, which made good sense from the beginning and has worked well in practice. A basic structural concept of our own, the alternation of laboratory suites and service chases, proved an ideal match for

the architects' approach. This same concept had been employed by the architect Marcel Breuer in his design for an earlier chemistry building at BNL. Jake Bigeleisen had played a prominent role in the design of that building, and liked to claim credit for its appearance in our building.

As the work of planning progressed, our confidence in the Smith, Hinchman and Grills team steadily increased, and in the end we were very pleased with the outcome. When bidding time came all the bids fell well within range of the budget target, meaning that construction could begin without further delay. Executing two planning stages instead of one had taken a long time, however, and brick by brick construction of the basic masonry new building was a slow process. It was not until the late summer of 1973, when I returned from a sabbatical period in Zürich, that I was at last able to move into my lovely new 5<sup>th</sup> floor office and laboratory suite. While the building is far from beautiful, it's not ugly either, and has proven itself thoroughly functional, serviceable and livable. In the end, far from being welfare clients, we were more fortunate than the Physics and Mathematics Departments, whose much larger building complex began to experience serious infrastructure problems from its earliest days.

Harold Friedman was primarily a theorist upon his arrival in 1965, although he continued to engage in and supervise some experimental research during his first several years here. He was essentially our sole theorist until Max Wolfsberg joined us on his part time, joint Stony Brook-BNL appointment. After Max left, the need for another theorist was strongly felt, and we had the good fortune to recruit Jerry Whitten, then a junior faculty member at Michigan State University. Jerry earned his doctorate at Georgia Tech, and had gone to Lansing after a period of postdoctoral research under Leland Allen at Princeton.

Richard Porter, also a theorist, was also recruited from another faculty appointment, in his case at the University of Arkansas. Dick had earned his PhD at Illinois with Frederick Wall, and carried out postdoctoral research with Martin Karplus at Harvard. I first met him at the 1968 Isotopes Gordon Conference in Issequa, Washington, where he had been invited to speak about isotopic exchange calculations he had carried out employing semiclassical methodology. I was very impressed, and felt certain he would be a fine asset to our Department. He came to Stony Brook for a visit and interview, my colleagues agreed with me, and all of us were delighted when he joined the Department in 1969.

The Chemistry faculty was strengthened during the waning years of the '60's by the arrival of several young assistant professors, then at the threshold of long and productive careers, who have stayed on to develop and pursue excellent careers right here at Stony Brook. One of them, Frank W. Fowler, universally known as Bill, came for interview from East Anglia University in England, where he and his wife Joanna had accepted postdoctoral appointments after completing PhDs at the University of Colorado. Bill made a very strong impression, and we had no hesitation in offering him an assistant professorship.



He came to Stony Brook in 1968, where his research program has flourished and he has long been a mainstay member of the Department.

After Bill Fowler had received and accepted our offer, Joanna Fowler explored the job possibilities at BNL, and happily landed one in Al Wolfe's group, where ground breaking research in positron emission tomography (PET scanning) was getting under way. Joanna has had a long and outstanding research career at BNL, where the major recognition and awards she has received include her election to the National Academy of Science in 2003. To our good fortune, she has also long been affiliated with Stony Brook as an adjunct faculty member, and has made strong contributions to our graduate program.\*

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\*It was typical for the time that when Bill applied for a job at Stony Brook, Joanna did not. How times have changed since then! Nepotism in academia had been illegal for many years, and while no longer so at the time of the Fowlers' first visit to Stony Brook, was still frowned upon. But now, in the year 2007, four married couples enjoy full faculty status in the Department.  
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Philip Johnson, a PhD student of Andy Albrecht at Cornell, was doing postdoctoral research with Stuart Rice at Chicago when we first learned about him. We were looking to strengthen our experimental physical chemistry sector, particularly in spectroscopy, and an Albrecht student seemed to be just what was needed. Phil has contributed in a major pioneering way to multiphoton ionization spectroscopy, and applied its methodology to numerous significant problems in molecular structure. With his strong contributions to teaching and other aspects of departmental life at Stony Brook, he has long been one of our most valued and respected colleagues.

David Hanson, another physical chemist who joined the department in the late '60's, was awarded a NATO fellowship for postdoctoral study in Europe after completing his PhD research with G. Wilse Robinson at Cal Tech. Before departing for Munich he came to Stony Brook for interview, and received and accepted an offer to join us the following year. It was a long wait, but one day a sleek Porsche convertible turned up in the parking lot and we knew Dave was back. He has made major contributions through his research in spectroscopy, his teaching, his service for two terms as Department Chair, and more recently in pioneering educational research.

Charles S. Springer was recruited as an assistant professor in the late '60's to broaden our presence in inorganic chemistry. A graduate of St. Louis University, Charlie had earned his PhD at Ohio State and came to Stony Brook following a postdoctoral period at the Aerospace Research Lab at Wright-Patterson Air Force Base. An enthusiastic research worker, a good teacher and fine colleague, Charlie's interests gradually shifted away from inorganic chemistry to magnetic resonance imaging. While he remained a member of our

Department for many years, his need for a major MRI research installation eventually caused him to move over to BNL. While there he retained a close connection to the Department and continued to supervise Stony Brook PhD research. More recently, however, he has departed from both BNL and Stony Brook, and is directing a major MRI research facility at Oregon Medical University in Portland, OR.

Three junior faculty members came to the organic sector of the Department in the '60's but did not stay long. One of these was Allan Krantz, who achieved the first direct detection of the elusive molecule cyclobutadiene, and studied its properties by the matrix isolation method, all here at Stony Brook. This newsworthy research bears an unintended but interesting relation, in departmental history, to the earlier work of our late colleague George Emerson. Allan Krantz departed Stony Brook to take up a position in industrial molecular biology. The other two young organic chemists appointed in the late '60's were Steven Murov and Raymond Jesaitis. While we enjoyed their relatively brief presence in the Department, both departed following tenure reviews, and successfully secured academic positions elsewhere.

There were three assistant professors of physical chemistry in the late '60's whose appointments were not renewed at tenure review time. George Kwei, one of the three, earned his PhD at Berkeley under Yuan Lee, and was a postdoctoral student of Dudley Hershbach at Harvard before coming to Stony Brook. He had played a prominent experimental role in the molecular beam research programs for which his two mentors later shared a Nobel Prize. A talented laboratory worker, George put together a fine molecular beam facility here. While we admired his talent, and enjoyed having him as a colleague, it was felt when review time arrived that he had not charted a satisfactorily clear course of independent research. George went to Los Alamos National Laboratory after Stony Brook, where he had a long and successful career in research and administration. We were sad to learn of his untimely death in 2005.

David Lloyd came to us directly from his PhD studies in physical biochemistry at Berkeley. An excellent teacher and stimulating colleague, it was a pleasure to have him as a colleague, but when evaluation time came he was not deemed to have developed a discernible and compelling direction for future research. It was unusual for us to make faculty appointments directly from graduate school, as we had done in David's case, and in retrospect I came to believe we had done him a disservice by doing so. The more usual sequence, including postdoctoral experience prior to faculty appointment, provides a candidate time to attain further maturity, and to chart future directions in research. David Lloyd has stayed on Long Island since he left Stony Brook, and is a valued member of the Hofstra University faculty.

Stephen Schwartz was a PhD student of Harold Johnston at Berkeley and himself an enthusiastic gas kineticist when he came to Stony Brook. An excellent all around member of the faculty, Steve got his research program in gear from the

beginning, was drawing good financial support for it and attracting graduate students to work with him. When tenure evaluation time came, the promise of his program was deemed unduly greater than its published record, and he was not promoted. The case did not seem so marginal to me, and I personally felt doubtful about the decision. Fortunately for Steve, and for the field of gas kinetics, he was welcomed to a well supported position in environmental research at BNL, where he has pursued a long and continuingly excellent research career.

Edward Stiefel, a star inorganic chemistry PhD student of Harry Gray at Columbia, came to us very strongly recommended by his mentor. To me, Ed bore the earmarks of a future academic star, and I followed his progress with great interest. At tenure evaluation time, while his publication record might well have appeared to be slim, it was clearly all first class, and indicative of a brilliant record to come. I knew also that Ed had had to contend with some difficult health problems. When the evaluation went against him and Ed had to leave, it was my opinion that a major mistake had been made, and I believe his subsequent contributions to inorganic and bioinorganic chemistry, made over many years at the Exxon Foundation and more recently at Princeton, have abundantly borne out that opinion. It came as a shock to learn, in September 2006, that Ed Stiefel had been swept away by pancreatic cancer.

The '60's was a time of student unrest on many campuses, and Stony Brook was no exception. I was in Martinique with my wife Evie, returning from a consulting trip to Cali, Colombia, when the famous Stony Brook drug raid occurred in January, 1968. I was astonished, to say the least, when I read about it in a newspaper picked up on an airplane. The only remotely beneficial thing that could be said about it was that it did put Stony Brook on the map: news about it was so widely disseminated that we no longer had to explain what Stony Brook is and where it is located. This deplorable incident, while basically a political manifestation, became a source of student unrest, and began a long and continuing sequence of incidents and demonstrations during the remainder of that decade and on into the next. Important contributing factors were felt nationally, e.g. the ongoing Vietnam war and the draft. Others were local, in particular the problem of crowded dormitory conditions created by a slowdown in construction that had occurred without parallel adjustment of admissions.

Early in the fall semester of 1968 the administration, in coordination with the student leadership, declared a three day moratorium on classes, October 22 to 24, to bring together the entire university community for a sustained exchange of information, opinion and ideas. This historic event, labeled the "Three Days," proved successful in improving communication among students, faculty and administration, and bringing about several aspects of institutional reform. A student-faculty group called "The Commission of Twelve," one of whose members was Chemistry's Ted Goldfarb, developed a set of concrete proposals based on the discussions that took place during "Three Days," concerning such matters as core requirements and interdisciplinary programs. Ted's life and career were transformed by the experience: he divorced his wife to marry a

student member of the Commission, publicly denounced the DOD agency that had previously supported his research, and embarked on a new phase of his life as an environmental activist.

Difficult times for campuses everywhere continued as the Vietnam War dragged on, and the invasion of Cambodia, followed by the National Guard killings at Kent State University in May, 1970, was an event carrying particularly explosive potential. John Toll was away and Alec Pond, as Acting President, dealt with it wisely, ordering campus flags lowered to half mast, and creating a faculty-student security watch system to look out for possible signs of violent action. Of particular concern for us in the Chemistry Department was an outbuilding we had built to accommodate our substantial supply of volatile and combustible solvents. Recognizing that it would be an inviting target for anyone contemplating arson, several members of the Department organized an around-the-clock watch, focused on the solvent shed but including the entire perimeter of the chemistry building. The wisdom of this action was corroborated during the long night of May 5-6 when an old barn near one of the residence halls was burned to the ground and an unoccupied faculty office in the Humanities building was firebombed.

John Toll's initial administrative structure for the University did not include a College of Arts and Science headed by a single Dean. Instead there were identifiable clusters of departments in Biological Science, Physical Science and Mathematics, Humanities and Fine Arts, and Social Science. Bentley Glass's title was Academic Vice President. There was no Provost, and President Toll proposed to use that word to designate the heads of the four departmental clusters. The word was changed from "Provost" to "Dean" at a later date, but the structure remained the same, and the concept of a single Deanship of Arts and Science was not reintroduced until the '80's. In 1967 President Toll informed us that he was planning to appoint Max Dresden to the position of Provost of Physical Science and Mathematics. Max was a distinguished physicist, a smart, fine man, and a friend whom I liked and admired. But since Alec Pond, a physicist, was now our Executive Vice President, it was clear that the Dresden appointment would create an all Physics-based channel of administrative communication concerning personnel, resources, and everything else, for the two non-physics physical science departments Chemistry and Earth and Space Science. Oliver Schaefer and I discussed it, and fully agreed that the appointment was not appropriate and we should resist it. We carried that conclusion directly to John Toll, who expressed utter astonishment, saying repeatedly that of the six key people concerned we were the only ones disapproving.\* We declined to

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\*Counting on my fingers, I could identify only five: the chairs of Chemistry, Earth and Space Science, Physics (Oakes Ames), Mathematics (Jim Simons), and the Director of the Institute for Theoretical Physics (Frank Yang).

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withdraw our objection, and thus began a standoff that lasted for months. I received emissaries on several occasions, particularly including Alec Pond and Sidney Gelber. Frank Yang also paid a call, and explained to me that physicists make the best administrators because they can see further than others. Oliver and I were in this together, and neither of us was about to yield. Chairmanships come in three year terms, and we were both up for renewal of our appointments that year. I was greatly surprised when I received notification that I was being reappointed to a one year 'probationary' term, and learned when I checked with Oliver that he had got the same message. We simply shrugged and tried to forget about it, and in the end Max Dresden's appointment as Provost never happened, and Oliver and I were not removed from our chairmanships. I hadn't seen or talked with Max while all this was going on, and was glad to have an opportunity to talk it over with him afterward. He spoke about it in an open and understanding manner, and it was good to know we were still friends.

In 1968 I learned that State funding might be available for the support of brief visits from distinguished scientists and other scholars, under sponsorship of an agency called the Science and Technology Foundation. I resolved at once to submit an application, and after consultation with my colleagues decided to propose three names: Linus Pauling of Cal Tech, Manfred Eigen of Goettingen, and Joshua Jortner of Tel Aviv. The application was submitted, and after a surprisingly brief interval we were delighted to learn that it had been accepted and fully funded. All three of our invitees accepted their invitations, and each came to visit for a period of two weeks, all within the academic year '68-'69. Eigen, the first visitor, was engaging and charming, and delivered an absorbing series of lectures about his own contributions to kinetics and their fundamental and widespread implications. An accomplished pianist, he joined me and musical friends one evening in reading the Mozart g minor piano quartet following a dinner party at our Greenlawn house.

We rented a house on route 25A in Stony Brook for the Paulings. They were quite comfortable there, and I had the privilege of doing regular chauffeur duty. The owner of the house was known to speak proudly in later years of the time when "Paulus Lining" had lived there. Pauling's former student and protégé Walter Hamilton, of BNL, joined me in greeting them when they arrived, and I enjoyed an interesting evening listening to the two of them chatting about recently solved three dimensional structures, which they verbalized without drawing a single diagram. Linus was quite indefatigable, and delivered a series of brilliant and diverse lectures, about peace and nonproliferation as well as science, including nuclear structure and vitamin C, meeting with student groups, and discussing research programs with many faculty members in the Department.

Jortner's visit was stimulating, informative and enjoyable. It also turned out to be productive for him, because not long after he left we learned that Ed Kosower had been persuaded to join the faculty at Tel Aviv University. Ed's Israeli wife, Nachama, was herself a scientist at the Hadassa Hospital and Medical Center. Tel Aviv was a strong university that would be further

strengthened by Ed's presence, he could continue his research program there, and the move would solve a severe geographic problem for both of them. We made arrangements for Ed to retain a long term formal affiliation with Stony Brook.

In the fall of 1966 Walter Hamilton came to talk about a Congress of the International Union of Crystallography that was due to occur in the US in 1969. The IUCr traditionally held a major international session once every three years, and this would be the eighth such occasion. The most recent previous one been held in Moscow, where the Russians did themselves proud, and this would be the first in the US in 20 years. It would attract a very large attendance from around the world, with many distinguished and well-known participants. Walter proposed that we invite the IUCr to hold its 1969 Congress at Stony Brook. Since this sounded like an interesting opportunity to showcase our young University, I agreed to take it up with the administration. After a full round of consultation, concerning practical matters such as the projected availability of housing and lecture hall space in 1969, not to mention the desirability of having the Congress here at all, it was agreed to go ahead. The general assembly and general meeting was scheduled to take place on our campus August 13-21, 1969, preceded by a topical meeting on the crystallography of biologically important substances at SUNY Buffalo, and followed by a topical meeting on neutron diffraction at BNL, August 22-23, and scientific laboratory tours in Washington, DC, August 25-27.

Walter Hamilton was the designated local chairman for the main IUCr meeting, and I began to see him at regular intervals in my office. On one of these occasions that spring he told me that the Congress would need a full time Executive Secretary to oversee the details of its organization, and asked whether I could recommend someone for the job. I went home pondering the possibilities, and brought it up with Evie at the supper table. After brief rumination she said "What about Natalie?" I knew at once she was right. Natalie Fiess's husband Ed, whose field of scholarship was American literature, had been a member of the faculty since 1957, and we had come to know them both well over the years. We knew that Natalie possessed superb planning and organizational talents, and was a person of great fiber and persistence. I passed the suggestion along to Walter, and before very long, in July 1967, Natalie was happily signed up and on the job. We assigned her a desk in the main office of the Chemistry Department, which served as her home base for some time. But the space available there became inadequate as the date of the Congress approached, and the IUCr headquarters were moved to a much more ample location in the Earth and Space Science building. Although Stony Brook was a young university by anyone's standards, its administrative arteries all too often appeared to be clogged. Natalie proved to be just the right person to stay the course with the myriad problems, large and small, that had to be solved, sooner rather than later, to make the IUCr Congress a success.

As the contours of the Congress unfolded, we learned that there would be some 1500 participants, coming from 35 countries. A major concern was the readiness of the Lecture Center,\* where the opening plenary session

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\*Later known as the Javits Center, in memory of Senator Jacob Javits.

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was scheduled to be held on August 13, with Linus Pauling as the keynote speaker. Seats were installed in the Lecture Center just one week in advance of that date, but when opening day came the hall was all put together and the occasion was a great success. The scientific program, which had been arranged by Walter Hamilton and his local committee, was well supported by all the essential campus facilities and services, and went very well. In addition to looking after all the logistics of programming and housing, Natalie arranged a large number and variety of local tours, museum visits and cultural events. One entire day was set aside for “down time” in the form of a clam bake held at Smith Point, an ocean beach on Fire Island. Natalie recalls that there were some problems with beach security officers, caused by European participants who considered it only normal to bathe in the raw. There was also at least one bad case of poison ivy. Since poison ivy doesn't exist in Europe, uninitiated Europeans tend to be unimpressed when we warn them about it, mistaking it for the much milder stinging nettle species that they do know.

Natalie stayed on her job with the IUCr, looking after the extensive post-Congress detail work that had to be done, well into 1971. Given her extensive experience gained from IUCr, she was ideally qualified for another Stony Brook administrative opportunity that soon came her way when a vacancy occurred in the position of Assistant to the Chair of the Chemistry Department. She served in that post with distinction, and was a major asset to the Department until her retirement in 1985.

By the time of the IUCr Congress, it was established that 1969-70 would be my last year in the chairmanship. I had been pondering this question for some time, and in 1968 tried to make it quite clear that I thought it was time, and I was ready to step aside. In recent years I had received several inquiries about my availability for opportunities elsewhere, and some offers. One of these, that I was deeply honored to receive and considered very seriously, was an offer to head the Chemistry Department at Purdue University. I visited West LaFayette twice, once alone and the second time with my wife Evie. The offer carried a full 12 month salary of \$35,000, which was very high on the academic salary scale at the time, and a great leap beyond what I was then being paid at Stony Brook.\* It was a complex decision, but in the end it was clear to me that my emotional attachment to Stony Brook, after all we'd been through together, was strong, and that what I really wanted most was to stay there, enjoy the life of a common faculty citizen, and get on with research. Another offer that I considered seriously came from the University of Missouri at St. Louis, then a new campus in the Missouri system, where I was invited to serve as Dean of Faculty, essentially the chief academic officer, number 2 to the President. It was clear that this would be

a point of no return if I accepted it, and I would have to think of it as a potential stepping stone to higher administration somewhere. Upon careful consideration realized that I didn't want to go that route. Six months later I was doubly glad I'd said no, when I learned that the president who tried so hard to recruit me to UMSL had left it himself, for another presidency elsewhere.

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\*John Toll sent me a letter presenting what was purported to be a matching counteroffer, in which the designated source for one third of the projected annual income was my own research grants.

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On the question of future leadership, I had come by 1968 to feel strongly that it would be in the best interests of the Department to establish a rotating chairmanship of the kind I had observed in action at Harvard. With such a policy the Department could ask its best known, strongest and most productive members to serve, without burdening them with long term administrative commitments. Leadership responsibility could be distributed equitably over time to faculty members representing the full range of the subdisciplines of chemistry. The Director of Laboratories position, which I considered key to making the rotation scheme function successfully, had been organized well, and was now ably occupied, by Paul Croft. In presenting and discussing this plan with my colleagues I first had to assure and convince many of them that it truly *was* time for me to go, and that I really wanted to do so. I had been in office for a long time, and some were used to and comfortable about my being there to look after the store. I also encountered understandable skepticism about rotation. Many thought it would be best to recruit a new chair from outside, and this was the prevailing view in the administration. One very strong potential candidate was brought to the campus for interview, and although he certainly seemed a good match for the job, the idea didn't evoke great enthusiasm and was dropped. By this time we were well along in the spring semester, and I was urgently requested by the administration and many colleagues to stay on through 1969-70. The request carried a firm commitment that that would be my final chairmanship year, and I consented.

Through the entire 1969-70 academic year I enjoyed my new status, that of a self-inflicted lame duck. I recall it as a year of great collegiality and harmony, with the entire Department pulling together toward common goals. One of those goals, an important one of course, concerned the question of continuity in the departmental leadership. As we continued to discuss it, both in private conversations and in Department meetings, the idea of a rotating chairmanship developed increasingly wide appeal, and in the spring of 1970 a resolution to that effect was agreed upon by the entire faculty and committed to writing. The next question was "who will go first?" My candidate was John Alexander, and many colleagues thought so too. John was understandably reluctant, but after several of us spent time leaning on him pretty hard he began to come around to the idea. Formal action was taken in the Department, and a recommendation was made to the administration. With its approval, John Alexander was appointed to the



position of Chairman of the Department of Chemistry, effective September 1, 1970, for a term of three years.\*

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\*Sidney Gelber's book about Stony Brook includes this comment on page 278 concerning the role of department chairs in determining the quality of programs and faculty in their units: "Those who up to 1982 served as chairs for a decade or more [represented] departments that were gaining academic distinction. These departments were Chemistry (Professor Francis T. Bonner and Benjamin Chu)..... [followed by eight others]..... The practice of rotation of chairs in departments was not encouraged." Our rotation plan was carried out entirely in the open, and I don't recall ever hearing an administrative objection to it. Indeed, I thought it had the Administration's full support, including that of the Academic Vice President. In Sidney's comment he appears unaware that between the chairmanship terms of Bonner and Chu the Chemistry chair had been occupied by three others, i.e. Alexander, Sujishi and Friedman.

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My son Michael graduated from high school in 1970, and would enter Harvard in September. For a graduation present I took him with me to an Isotopes Gordon Conference in Issequa, followed by two fine weeks of camping and hiking in British Columbia, Montana, Wyoming and Utah, and returning by air from Salt Lake City after visiting my sister and her family in my home town. In August of 1970 Evie and I enjoyed another splendid camping trip in Maine and New Brunswick with our two daughters. Immediately following our return, on the day after Labor Day, there was a great gathering of the Chemistry Department, which had become a very large community since its small-scale origin in Oyster Bay in 1958. The occasion was held to honor me and express gratitude for my twelve long years of service as the founding Chair. I was deeply moved to be so abundantly honored, celebrated, wished so well, and it was indeed a memorable occasion. On the following day my newfound, administration-free, civilian life began.



# XI

## EPILOGUE

The Chemistry Department's rotating chairmanship plan, begun in 1970, has now been in continuous operation for more than three decades. During and beyond the first two of those decades the chair was held by faculty members recruited to Stony Brook during the years of my chairmanship, in the following sequence: John Alexander, Sei Sujishi, Harold Friedman, Ben Chu, Jerry Whitten, David Hanson, and Bill le Noble. John Alexander, finding the burden more costly to his research program than he could afford, left the office after two years. Each of the others completed a full three year term, and two of them, Ben Chu and David Hanson, stayed on to complete a full second term. It was noted by everyone that on any day during Harold Friedman's chairmanship he could cite the exact number of weeks and days remaining in his term. It was also clear, however, that the burden of maintaining this mental clock did not compromise his conscientious performance, or dampen his perennial optimism, in any way.

In anticipation of each change in the chairmanship the Department created an *ad hoc* Committee, consisting of one tenured representative of each of the major subdisciplines, and one junior faculty member. The Committee members interviewed all the faculty members in their areas before meeting to deliberate and identify a candidate. I served on one such committee in which the junior faculty was represented by Jimmy Doll, a fine young theoretician then well on his way to promotion and tenure. In his report to the full Committee Jimmy said that the junior faculty did not want the new chairman to be a member of the "old guard." When I asked what that meant, he said "people that have been here since the year one." To that I couldn't refrain from responding: "But Jimmy, there's only one person in the Department that meets that description, and I'm not available!" Jimmy Doll was a great spirit and a fine asset to the Department, and we very much regretted our loss when he departed for Browner\* pastures a few years later.

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\* Jimmy departed Stony Brook for the superior rock climbing environment of Los Alamos, then returned to the east coast several years later, to join the faculty of Brown University.

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Following Bill le Noble's term, our ninth chairman was Iwao Ojima, who served two terms. Iwao had been recruited directly from Japan, and was scheduled to take up residence here in the fall semester of 1983. With that date approaching, his entry and residency visa was suddenly denied, due to an error in the application. In July of that year I had just begun a term in the position of

Dean for International Programs, and immigration problems were within the purview of that office. Because it was clear we would need legal assistance to get Ojima on campus by the beginning of the fall semester, I engaged an impressive young immigration lawyer, Allen Lee, to represent him. Allen succeeded in untying the impeding legal knots in record time, and made it possible for Iwao to take up residence and employment at the University on schedule. When Iwao succeeded Bill le Noble as our Department Chairman, eleven years later, it could be argued that I had brought all eight of my first successors to Stony Brook. More significantly, however, when I submitted the necessary paper work to pay Allen Lee's legal fee from my discretionary International Programs budget I was severely reprimanded from above: the right to engage lawyers was restricted exclusively to the SUNY Office of Legal Affairs in Albany, and I had done something unthinkable. I can't honestly say this surprised me, but I was not fired, and I knew for sure that my way was faster.

Since the completion of Ojima's second term in 2002 Michael White has been our chairman. Since Michael's appointment is joint between Stony Brook and BNL, this arrangement is unique. In its orderly way the Department has already, in January, 2007, selected its next chairman, to succeed Michael White in July. He will be Ben Hsiao, affectionately known as "Little Ben", to distinguish him from Ben Chu, who is sometimes called Big Ben. I believe the record abundantly shows that our rotating chairmanship plan has worked well and will continue to do so, even though we now know after reading Sidney Gelber's book that it had been officially frowned upon by the Administration.

During most of my own twelve chairmanship years I was able to count on good budgetary support from State funds, particularly in lines for faculty and staff positions, but also for supplies and, to a lesser extent, equipment. The rapid rate of growth and development we sustained during the last half of the sixties would not have been possible otherwise. This situation began to decline from the beginning of the seventies, and most of my successors were forced to deal with painful budget shortfalls. During Sei Sujishi's term we lost a significant number of precious teaching assistantship lines, so essential to both our graduate and teaching programs. Later on, the Department sustained serious faculty losses at times when whoever was chairman found himself unable to secure the financial means needed to prevent them. This was particularly the case during Bill le Noble's chairmanship, because President Kenny, who had discovered upon her then recent arrival that the campus was severely in debt, had declared a period of extreme fiscal restraint. The deleterious effect of this policy was magnified by the then Dean of Physical Science and Mathematics, whose refusal to allow salary adjustments to forestall departures contributed to our loss of several valued faculty members, among them Cynthia Burrows and Scott Anderson, who departed for the University of Utah.

One serious instance of faculty loss was brought about by direct administrative action. A valued colleague named Gerry Harbison had successfully completed his tenure review and was about to advance to the rank

of associate professor. The Provost at the time, who came here from Rutgers but will not be otherwise identified here, invited the entire campus-wide group in this category to his office for a little ceremony of celebration. In the course of his remarks on this occasion he made light whimsy of the fact that most of the promotions that year would be “dry,” i.e. unaccompanied by salary raises, and suggested to the new class of associate professors that to secure raises they should seek offers from elsewhere. Gerry was infuriated, and declared to the Dean that if Stony Brook wanted him to seek a job offer elsewhere he would do just that, but with the intent of accepting it rather than using it to negotiate a raise. He meant it, and that was how we lost Gerry Harbison to the University of Nebraska. Despite this occurrence, and additional painful faculty departures the Department has suffered, it has managed exceedingly well in maintaining and steadily increasing its strength, made possible by numerous top quality faculty appointments.

Rotation at a slower pace has occurred at the presidential level. After 14 years at Stony Brook’s helm John Toll accepted an offer to return to Maryland as Chancellor of the state’s entire system of higher education. Alec Pond, who had already had recent experience as Acting President, returned to that role while the Stony Brook Council, the body bearing responsibility for presidential nominations, began its search for a new leader. A search committee was appointed, consisting of Council members, faculty representatives, and one student representative (Chemistry graduate student David Skolnick). It was widely expected that an outside appointment would be made, and as the search progressed we began to hear exciting news about some outstanding candidates. The search committee was asked to present the names of three nominees for the post to the Council, in order of preference. After the committee had agreed on a slate of three top outside candidates, one of its members requested that Alec Pond’s name be added to the list, in fourth place, as a courtesy to him. What happened next sent out a major shock wave: the Council passed over the top three candidates and nominated Alec. While widely respected and liked, Alec had long since come to be regarded on campus as the alter ego of John Toll, as was even stated in the official description of the position of Executive Vice President. In that role he was perceived by many to be the President’s naysayer and hatchet man, a perception enhanced by a touch of authoritarian style retained from past service in the Navy. Added to these factors, more than a little envy of the Physics Department’s success was felt in the Humanities and Social Science departments. In sum, there was a widespread desire on the part of a majority of the faculty for a change in the style of administration on our campus, and few expected Alec to provide it. After the Council’s recommendation was conveyed to Albany, accompanied by loud dissent from the campus, it became clear that the Chancellor and Trustees felt the same way, and Alec’s nomination was rejected. By this time, enough publicity about the situation had leaked out to cause the three top outside candidates to withdraw, and we were back to square one. The rebuff was also understandably difficult for Alec, and within a year he left Stony Brook for a high level administrative post at Rutgers.

Faced with the need for a new search, the Chancellor and Trustees appointed Richard Schmidt, then president of the Upstate Medical Center at Syracuse, to come to Stony Brook for one year as Acting President. As a fellow member of the Board of Directors of the Research Foundation at that time, I was well acquainted with Dick, and thought it a good choice. He did a splendid job of keeping the lid on, holding the campus together and maintaining tranquility throughout that transitional year. By the end of the year another search brought us a new President at last, and John H. Marburger, whose field was applied physics, came to us from The University of Southern California, where he had most recently served as Dean of the College of Arts and Science. Jack Marburger's rather low key reign was characterized by numerous improvements, accomplished at a moderate pace. These included changes in degree programs and academic administrative structure, and rehabilitation of on campus student housing. He also launched several important long range beautification initiatives to improve the appearance of the campus. Jack Marburger left the presidency after ten years, but remained on campus as a faculty member in the College of Engineering and Applied Science.

A new search brought a new president, Shirley Strum Kenny, then president of Queen's College of CUNY. For the first time we had a non-physicist president, representing an academic discipline of the humanities rather than the sciences. Accelerating and adding to Jack Marburger's initiatives, President Kenny has presided over an era of close and activist attention to the appearance and aesthetic appeal of the campus. Stony Brook has become a much more attractive and livable place since she came. Her presidency has also been an era marked by extensive new construction on campus, and even territorial expansion as well: the creation of Stony Brook Manhattan, acquisition of the entire former Southampton College campus to become Stony Brook Southampton, and purchase by eminent domain of Flowerfield, a large nearby tract formerly owned by the Gyrodyne Corporation, for development of a technology park.

In a crucial event that occurred in the early years of Dr. Kenny's presidency, Stony Brook acquired an entirely new and managerial relationship to Brookhaven National Laboratory. The contractual agency that had held responsibility for the management of BNL from its earliest days, called Associated Universities, Inc. (AUI), was a consortium of distinguished institutions. The Department of Energy (DOE) became increasingly dissatisfied with AUI in the '90's, due to the Laboratory's inept handling of a thicket of difficult public relations problems. Essentially, the DOE fired AUI by refusing to negotiate a renewal of its contract. Correctly seeing this as an opportunity for Stony Brook, President Kenny sought and established a relationship with the Battelle Institute to create a new corporation, called Brookhaven Associates. This entity submitted a successful bid for the BNL management contract, which it has now held for several years. In my own experience the presence of BNL had been a crucially, indeed almost parentally important factor in the launching of our infant university, and it was at first difficult for me to get my head around this new situation. The

new relationship has clearly worked very well, and has brought SBU and BNL ever closer together, to the benefit of both. One of its earliest consequences was the appointment of Jack Marburger to the position of Director of BNL, where his public relations skills contributed well to the resolution and relief of the sticky problems that had accumulated there.

The Stony Brook Chemistry Department's output of first class, peer-reviewed research has been impressive and outstanding since its earliest days, and the decade of the '70's was no exception. The most exciting and newsworthy research in that decade, however, was carried out by Paul Lauterbur. Paul had come to us in 1963 well versed in the field of nuclear magnetic resonance, and as noted earlier had made pioneering contributions to NMR based on the important isotope carbon-13. In 1971 Raymond Damadian reported striking differences in proton ( $^1\text{H}$ ) NMR relaxation times measured in excised samples of normal and malignant tissue in rats. Paul had occasion to observe a series of measurements designed to confirm this observation for the case of a different rat malignancy. Finding the experience interesting but unpleasant, he began to speculate about the possibility of a more comprehensive and less invasive procedure, based on NMR. A period of focused and brilliant speculation led him to the concept of image formation by interactions between the field associated with NMR radiation and an imposed, static magnetic field gradient. After further elaboration of this concept, he devised and carried out his first experimental test. Employing a 60 MHz analytical NMR spectrometer in the Chemistry Department, he created an image of two 1 mm inside diameter capillaries of normal  $\text{H}_2\text{O}$  attached to the wall of a larger glass tube of deuterium oxide ( $\text{D}_2\text{O}$ ).

In October, 1972 Paul submitted a brief paper to the British journal *Nature*, describing his new concept of nuclear magnetic resonance imaging and presenting the evidence of his first images. The paper was quickly rejected by *Nature*, but Paul appealed the rejection and submitted a revised manuscript in which he suggested the possibility of applying the new imaging method to medical conditions such as cancer. This time the journal accepted the paper, and it appeared in *Nature's* volume 242, pp 190-191, on March 16, 1973.

Because the method could be regarded as the coupling of two fields, Paul proposed the name *zeugmatography* for the new imaging technique, based on the Greek word *zeugma*, "that which is used for joining." He introduced the word in his first publication in 1973 and continued to use it, and to refer to its images as *zeugmatograms*, in all of his publications well into the '80's. But the name never entered into general usage. As the NMR and medical communities gradually became aware that a new, powerful, noninvasive imaging method had arrived, it was at first most often referred to as NMR imaging. As it came into increasing usage in diagnostic applications the "N" for "nuclear" got quietly lost, sparing patients their possible fears about being nuked, and became known universally as MRI.

Paul Lauterbur has said that when the MRI concept came to him he dropped everything else he was doing to pursue it. Paul's strong focus on its development was clear to all of us in the Department, and we had the pleasure to observe and even participate in the excitement that he and his research group were generating. Paul kept us informed as the work progressed, and I can recall a number of Lauterbur seminar occasions at which his attending students and postdocs wore T shirts frontally adorned with great Z's. Paul also did a great deal of traveling to other academic research centers to inform others and stimulate contributions to the development of MRI. One such center that became the locus of much important MRI research was England's University of Nottingham.

The first MRI image of a live organism, a very small clam harvested at the beach by Paul's daughter Sharyn, was obtained soon after his first publication appeared in *Nature*. As the work progressed, images of the "first ever" variety came along at regular intervals, and interest and excitement about MRI imaging was felt throughout the Department. With the attention of Paul's laboratory inevitably focused on biological specimens, we began to hear about measurements carried out on the body parts of chickens and small mammals. I recall one MS thesis in which the degree candidate presented MRI images of her own knee at a sequence of depths. As it became increasingly clear that MRI would become a diagnostic tool of major importance in medicine, it was equally clear that Paul would need access to patients to continue with its development in an effective manner. It was during Harold Friedman's chairmanship that this issue was taken to the University and Health Science administrations, and President Marburger brokered an arrangement to provide a joint appointment for Paul, to be shared equally between the Departments of Chemistry and Radiology. There was as yet no commercial MRI device capable of accepting an entire human body within its magnet gap, but the method had been noticed by industry, and instruments were under development at the General Electric company, Phillips, Siemens and elsewhere. Unfortunately, the Chairman of Stony Brook's Radiology Department at the time did not share Paul's vision for the future of MRI, lacked patience for the long wait to get it up and running on a routine basis, and quite possibly didn't like Paul either. After a period of less than two years he summarily canceled the joint appointment arrangement, without notice, and removed Paul from the Radiology Department's payroll, complaining that Paul was not contributing to patient care. This turned out to be a major event along the bumpy road that led to our losing Paul to the University of Illinois at Champagne-Urbana in 1985.

Paul Lauterbur's publication in *Nature* in March, 1973 constituted public disclosure of the discovery of MRI. By US patent law this meant that if a patent application for MRI had not been submitted by March, 1974 the invention would lapse into public domain. Paul was well aware of this, and made an earnest effort to secure a patent. He was an employee of the University, his work was performed employing the University's facilities, and the invention was incontestably the property of SUNY. In keeping with instructions in SUNY's policy manual at the time, Paul submitted an invention disclosure to the Central



Administration in Albany. After considerable delay, the disclosure was sent to Research Corporation, the agency that SUNY Central was then relying upon to evaluate invention disclosures. After a further delay the Research Corporation provided its evaluation: SUNY was advised not to apply for a patent, because in their opinion MRI would not generate enough income to even cover the cost of application. Central Administration accepted this recommendation, and informed Paul of their decision. SUNY policy at the time provided that if the University decided not to file for a patent the inventor could request it to release ownership of the invention to him. It was specified that this request had to be submitted to SUNY in writing, and Paul did so. By this time the March 16, 1974 deadline was perilously near, Paul never heard from Albany, and the invention of MRI passed quietly into the public domain.

Although Paul had been unable to obtain a patent on MRI, a patent was issued to another investigator who claimed to have invented it. This was Raymond Damadian, who made the important observation in 1971 of differences in proton NMR relaxation times between normal and malignant living tissue. In the method he described in his patent application individual NMR measurements were to be made at each of several body locations, and the patient would have to be moved to accommodate each measurement. Declaring this to be a patent on magnetic resonance imaging, Damadian proceeded to bring patent infringement charges to industrial firms that were producing the earliest body scanning MRI instruments. His first suit was brought against a Japanese company, whose lawyers put up a knowledgeable and vigorous defense. The suit was thrown out of court by a discerning judge. But when Damadian sued the General Electric company, that great corporation's defense was surprisingly inattentive and inadequate. He was awarded a monetary settlement, huge for him but peanuts for GE, for the claimed "infringement," despite the reality that the method described in his patent was not capable of producing images. This result not only emboldened his claim to the invention of MRI, but also provided him an abundant financial resource to press that claim, and to depict himself to the medical community as the true discoverer of MRI.

By the time we lost Paul to Illinois in 1985 most of his colleagues at Stony Brook believed it quite certain that he would be awarded a Nobel Prize. Year after year I listened eagerly as the Nobel announcements came out of Sweden in October, only to come away disappointed each time as Paul's name failed to appear. Then one morning in October, 2003 my wife told me she'd heard something on the radio she thought I would like to know. "About what?" I asked. "A Nobel Prize" she answered. "Lauterbur, at last" I shrieked. It was now thirty years since Paul's seminal MRI publication appeared in Nature, and I had long since begun to think the Nobel wasn't going to happen. There was much excitement in the Department that day, and we held an impromptu public meeting that afternoon in the Chemistry lobby, near our "Discovery of MRI" display, which features a photograph of an exuberant Paul Lauterbur with the A60 NMR he had used to acquire his first images. This display had been installed several years previously, and was now due for a significant update. There was a big turnout of

faculty and students, and one of the speakers was Sharyn Lauterbur, who recalled her triumph over her older brother Danny when she harvested a clam just small enough for her dad to make it the first living organism to be imaged by MRI.

The 2003 Nobel Prize for Medicine or Physiology, awarded for MRI, was shared equally by Paul Lauterbur and Sir Peter Mansfield of the University of Nottingham, who had made important contributions to the development of MRI from its earliest days in the 1970's. We could only speculate about the reasons for the long delay, but we all knew that Damadian had conducted a long and well financed campaign to convince the medical community that he was the true inventor of MRI. It seemed clear that the Karolinska Academy, the body responsible for Medicine or Physiology Nobel nominations, had wisely chosen to wait until they could be sure they were getting it right, and it was generally agreed that they had done so. Dr. Damadian did not agree, however, and from the day of the Nobel announcement in October to the formal Nobel award ceremony on December 10, he ran full page ads in the New York Times, Washington Post, Los Angeles Times, a newspaper in Stockholm and others, protesting the injustice he believed had been done to him.

My wife, Jane Carlberg, and I were honored and excited to receive an invitation from Paul Lauterbur and his wife, Joan Dawson, to attend the Nobel award ceremonies in Stockholm. I had hired Paul in 1963, had encouraged and facilitated the development of his academic career, and was grateful for this personal recognition. But beyond the personal, Paul was recognizing the Stony Brook Chemistry Department, where his ground breaking research had been carried out in an encouraging atmosphere of lively and infectious intellectual curiosity. His recognition was felt and appreciated throughout the Department.

We flew to Stockholm on a stormy December night, and had a unique and enjoyable time through the entire Nobel award week, culminating on December 10, the anniversary of Alfred Nobel's death. We attended many prize-related events, including a press conference, Paul's and Sir Peter's Nobel addresses at the Karolinska, Literature laureate J.M Coetzee's public lecture, an intimate banquet hosted by the royal family for us and some 1300 other guests, and, on the anniversary day itself, the presentation of the Nobel awards to Paul and the other laureates by King Karl XVI Gustaf. It was good to be there, in interaction with Paul, his family and other members of his entourage, as well as with other laureates and their entourages. Despite its being such a busy week we managed to do some enjoyable sightseeing in Stockholm, and attended a performance at the Royal Ballet.

After witnessing the outrageous outcome of SUNY's patent policy in the case of Paul Lauterbur's MRI patent attempt, I promised myself I would do something to change it. The opportunity came in 1976, when I was asked by the Research Foundation of SUNY to chair a state-wide committee on another patent-related topic. I proposed from the beginning that we seek a broader

mandate, to examine and make recommendations concerning all aspects of SUNY's Patent Policy. The other members of the committee and the Foundation agreed, and after dealing with the first matter we became the *ad hoc* Committee on Patent Policy. We conducted a thorough review of the policy in effect at the time, and proposed a new and completely revised version. Our recommendations were welcomed and accepted at all levels, and within a remarkably short period of time became official SUNY policy, unamended. Among its many features, the revised policy gives SUNY six months from the date of an inventor's disclosure to determine whether it will pursue a patent, and mandates automatic transfer of ownership of the invention to the inventor at the end of that period. It also established greatly improved royalty incentives for entrepreneurial minded faculty members, with the result that SUNY's patent income, which is largely generated at Stony Brook, now ranks among the top 20 US universities in that category.

Paul Lauterbur's award was the second long delayed Nobel to come to someone I had known long and well. In 2002, the Physics Nobel Prize was awarded to Raymond Davis for his research on solar neutrinos. Ray Davis, the Harned PhD student whose laboratory I "inherited" at Yale in 1942, had been at BNL since the late '40's. I had followed his remarkable and difficult program for many years, and admired his formidable experimental skills and unbelievable staying power and patience. It was widely felt that Ray had been unfairly overlooked at the time of a previous prize for neutrino research, and everyone was glad to see him receive this richly deserved recognition at long last.

My own personal trajectory through the decade of the '80's included a brief return to administration, from 1983 to 1986, with my appointment as Dean for International Programs. A biology professor named Raymond Jones, my predecessor in this office, had developed a promising network of scholar exchange agreements with foreign universities, combined with programs and opportunities for our undergraduate students to study abroad. One day in 1982 Ray Jones tragically dropped dead on a tennis court. Hispanic Language Professor Roman de la Campa served for one year as interim director, and I agreed to take charge of the office in July 1983. Ray Jones had developed a strong Stony Brook presence in Poland, with US Education Department support. The program was in trouble, and became my first priority. Other program locations then ongoing included Paris and Avignon, France; Rome, Italy; Chonnam, South Korea; Beijing and Shanghai, China; Bogota and Medellin, Colombia; and Lima, Peru. In addition to looking after the well being of these programs, of the numerous international exchange scholars on our campus and Stony Brook undergraduates studying abroad, my dedicated staff of four and I held responsibility for student and scholar visa and residency issues of all kinds, and an Institute for English as a Second Language. One major initiative during my term as Dean was the development of a comprehensive exchange agreement with the University of Rome, to complement and extend our well established study abroad program there. Another was a comprehensive exchange agreement with the University of Chengdu, a major city in far western China.

International Programs was an exciting and enjoyable assignment, but after three years it became clear that its scope and pace were drawing me further from my interest in and concerns about nitrogen chemistry than I could afford. Major discoveries in the life sciences at that time had begun to reveal that nitric oxide (NO), a small toxic molecule whose reactions and properties I had studied comprehensively, plays a major role in mammalian physiology. Since it was important for me to participate in the excitement about NO that those studies were creating, I asked to be relieved of the deanship, and was back full time in the Chemistry Department in the fall of 1986. My successor at International Programs was Frank Myers, a political science professor, who did a good job of maintaining and advancing the office and programs for several years. When he wished to be relieved a search committee was appointed, one of whose members voluntarily kept me informed about its efforts. The search committee interviewed several candidates, one of whom was a faculty member that had served as a deputy to Sidney Gelber during his years as Academic Vice President and had previously applied for the International Programs position. In the end, the search committee recommended the names of two candidates for the Provost's consideration, and also recommended explicitly that that particular candidate *not* be appointed to the position. Whereupon the Provost, who had come to us from Rutgers but will not otherwise be identified here, promptly appointed the unrecommended candidate. A few days after the appointment became known I encountered Ted Goldfarb, who greeted me by saying: "I see the Administration has decided to destroy International Programs." While I was not in a position to follow the curve in detail, Ted's prediction of decline seems to have come close to the ensuing reality. Eventually study abroad and exchange programs were placed under the aegis of the Graduate School, where extensive and essential resuscitation efforts appear to have been successfully applied.

Finally, a few words about my own personal life during the decade of the '70's and beyond. On August 17, 1974 the lives of all in my family were forever changed when our talented, beautiful and beloved older daughter Alisa took her own life, at the age of 19. She had completed her freshman year at Oberlin College, and was soon due to return there. The questions about why this happened are still there, mostly unanswered, and each of us, in his or her own way, has managed to come to terms with the pain of the event and, without forgetting, move on.

My wife Evie retired from her position in the Biochemistry Department in 1986, and we managed to fit in a number of very interesting and enjoyable trips. The best of these was a trip to India that occupied the entire month of January, 1987. The itinerary had been worked out for us by an artist friend who had been living in Bombay for many years. The early part of the trip was focused on the many beautiful Hindu temples of south India. Venturing further north at the end of the trip, we also visited the Taj Majal, and wound up in New Delhi on Indian Independence Day. I just barely made it back from Bombay in time to give my first physical chemistry lecture of the spring semester.

In January, 1990, just a few weeks after Evie's 70<sup>th</sup> birthday, we spent some vacation weeks in Arizona, as we had done in a number of previous winters. We had a busy and active time, hiking in exotic places like Organ Pipe Cactus National Monument, and visiting friends. On January 17 we celebrated our 44<sup>th</sup> wedding anniversary in Scottsdale, with our dear friends Ed and Toni Shapiro. We came home invigorated, ready to begin the new semester and pursue our usual winter activities. In early February, after a concert in Alice Tully Hall, I waited for Evie at the top of a long flight of stairs, and fondly recall to this day how quick and light was her step as she came up to me. A few days after that she began to experience some dizziness, and went to see her doctor, who thought it was not to worry, but had her hospitalized for observation. At three AM the next morning I received a call from the hospital, informing me that "we think your wife has a brain tumor," and asking me to come in to authorize surgery. The brain tumor turned out to be a galloping, inoperative cancer at the brainstem. Evie passed away on March 11.

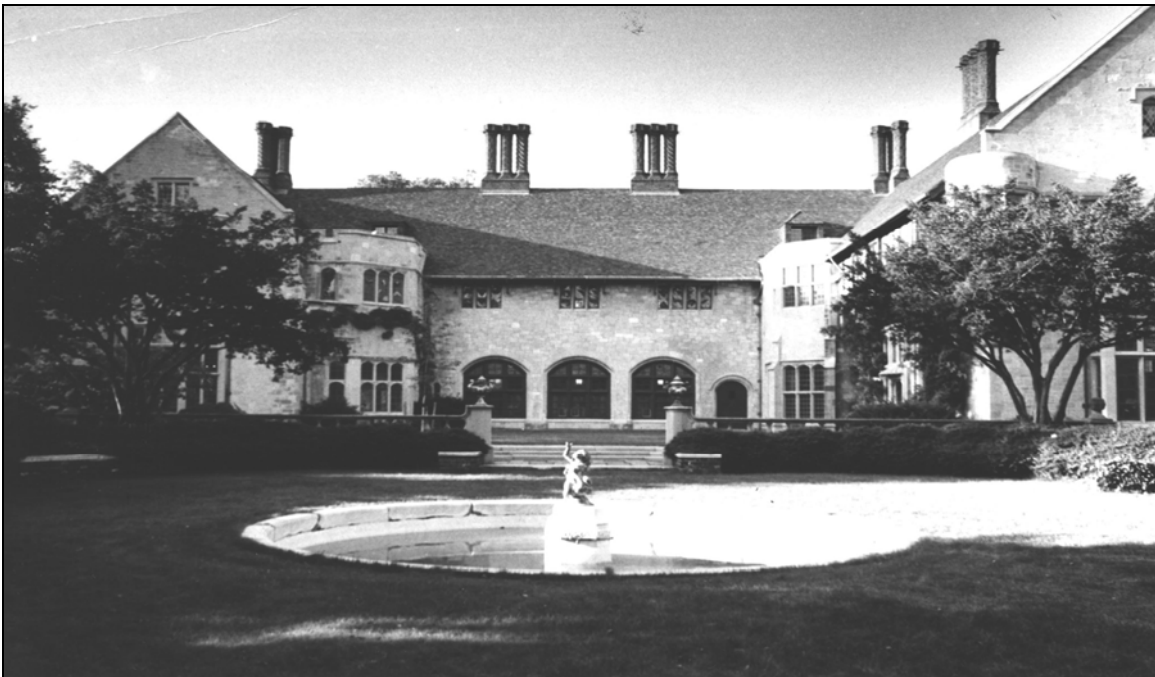
During most of my years of academic experience, mandatory retirement was an established fact of life. In the SUNY system the mandatory age was 70, and I was thoroughly adjusted to that landmark as my own 70<sup>th</sup> birthday approached. By the time that birthday arrived, on December 18, 1991, the mandatory retirement statute was no longer in effect, but I had so internalized the idea, and become so adjusted to it, that I decided to go ahead anyway. I retired on July 1, 1992. I had not been faring well with NSF, my principal source of research support, and might have decided to stay on had it been otherwise.

Doing my best to cope with my new life as a widower, I gave renewed attention to my musical interests, which had been seriously neglected for several years. While I was by training and experience a violinist, I had purchased my first viola in the early '60's, and by this time viola had become my exclusive instrument. I played in the University Orchestra, later in the Sound Symphony, and also enjoyed playing chamber music when there were opportunities to do so. In the summer of 1992 I went to my first chamber music workshop, a full week of intensive string quartet playing offered and coached by the Manhattan String Quartet in Kent, Connecticut. It was a demanding and enjoyable experience, and I made some new friends, one of whom was a violinist from Connecticut named Susan. Susan called in December to tell me that she was putting together a quartet to attend a weekend workshop in Oceanside, New Jersey, and wanted me to be the violist. When I arrived to attend the workshop Susan had already arrived, and with her were the other quartet members, a violinist named Jane Carlberg and a 'cellist named Caroline White, both good friends of hers from Connecticut. I hadn't been there very long when Susan discretely informed me that Jane's husband of thirty plus years had died several years previously. The musical part of the weekend was thoroughly enjoyable, but the getting acquainted with Jane part was even better. I learned later that this wasn't the first time Susan had demonstrated matchmaking instincts. She was pursuing an intuition that Jane and I would go well together, and that turned out to be exactly right. It was in January, 1993 that we first met, and by the following summer we had become

inseparable. We were married on New Year's eve, 1994, with Jane's three daughters, two sons-in-law and two grandchildren, and my two children and their spouses all participating. Not a day goes by that we fail to remind ourselves of our great good fortune in finding each other.

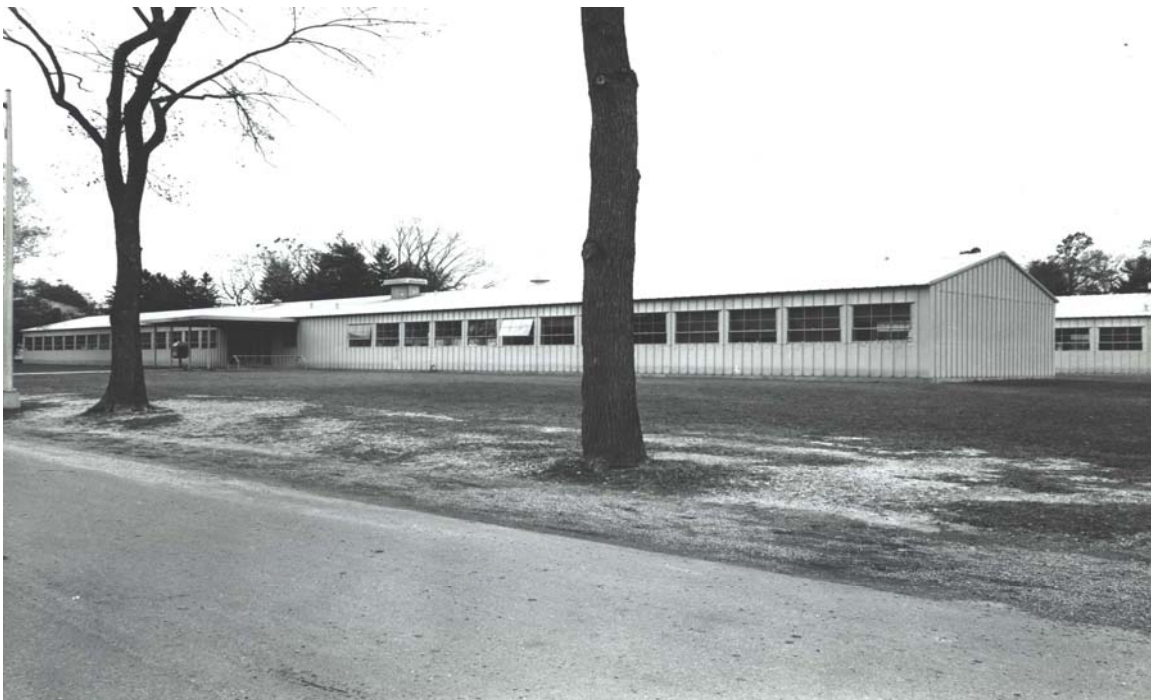


Planting Fields: “The Great Lawn”



Planting Fields: Coe Hall, on the beautiful Coe estate.





Planting Fields: A Butler Building, first Home of the Chemistry Department.



On June 4, 1961, President John F. Lee and Dean Leonard K. Olsen presided over SUCOLI's 1<sup>st</sup> commencement.





Stony Brook: The “New” Chemistry building under construction.



Stony Brook: aerial view of campus, 1964. The completed Chemistry building is featured in the center of the photo.



Chemistry Department in 1968 (partial).  
Top: Lauterbur, Friedman, Bonner, Haim, Wolfsberg, Kosower.  
Bottom: Front Row: Sujishi, Hagen, Kerber, Whitten.  
Second Row: Schneider, Goldfarb (others unidentified).



The legendary "Bridge to Nowhere" (1967)



New Chemistry Building, circa 1980