Directory of some writings of Robert L. Sproull (RLS)

This is a listing that identifies a set of documents written at various times by RLS and scanned into PDF files. These are documents that his children found in his possession when he died. Many of these documents were accompanied by notes written by RLS: these notes are bracketed < > here for clear identification.

The exact provenance of many of these documents is murky. Several underwent revisions; we have tried to find a complete version, which may not be what RLS viewed as the final version. We have made no attempt to capture multiple versions. Some of these writings have appeared in other places, perhaps with changes. Some are transcripts or other records of presentations RLS made.

We have made no attempt to collect RLS writings as a research physicist; most are presumably in the technical literature.

Scanned files are cited in brackets: [F02] points you to the file F02.PDF.

Biographical writings and talks

RLS gave extensive interviews to UofR to document his time in the administration. He supplemented it with a written account of time when he was not at UofR. This is [F01]. It refers to a collection of "ingredients" or vignettes, using a notation "@title". These ingredients can be found in [F03]; for "@5 Trembling" see also [F26]; for "DS" see [F27]; for "CP" see [F09] and [F10].

RLS also gave several talks about his life. At Deep Springs, he was invited to give the Withrow Lectures, which touched on management and leadership: "Three by Three" [F02].

"Careers of One Physicist," 21 October 1998. [F09]. <The Physics Colloquium Committee of the UofR suggested that I give this colloquium.> Also includes some notes [F10], perhaps of material that RLS chose to omit from the talk.

A collection of short bios and one version of his CV is [F53].

A partial list of notes, reports, speeches, and talks is [F54].

ARPA and government

"The Interdisciplinary Materials Laboratories—An Appreciation," 8 April 1965. [F37]

"House Armed Services Committee," 8 July 1987. [F38] <Congressman John Spratt of the House Armed Services Committee arranged a session in which I was the sole witness. About 5 of the Committee were present. There were sharp and naïve questions from Bennet and ??? (I believe from Berkeley) but Spratt asked good questions and kept control. Congreeeman Jim Olin had introduced me to Spratt.>

"House Science and Technology Committee," 8 February 1989. [F41] Testimonry by RLS as chair of Dept. of Energy Basic Sciences Advisory Committee.

Deep Springs

Speech on co-education, 19 October 1986. Transcript. [F15]

"A Unique Institution," 9 March 1996. [F27]

"Report of the Long Range Planning Committee," November 1980. [F43] This report begins "This is the report of a Committee that never met."

Two interviews with Brad Edmondson, late 2002, corrected in early 2003 [F56]

Cornell

"Early History of Clark Hall," 20 October 1965. [F49] Remarks at dedication of Clark Hall.

University of Rochester

"University of Rochester—Goals and Aspirations," talk to Public Affairs administrative staff, 17 October 1972. [F34]

Note to Steering Committee of Faculty Senate, 13 January 1969. [F39]

Inaugural speech, 1 February 1975. [F05]. This was of course reprinted elsewhere, notably the *Rochester Review*, Spring 1975.

"The Strange World of Nuclear Deterrence," 18 April 1984. [F45] <"Wednesday Evening Talks" were administered at my request by Kenneth Clark. I had proposed them when thinking that our faculty were giving public lectures to many audiences but not to the campus. This may have also been used for Pundit and Fortnightly.>

"Why Quantum Mechanics Matters," 19 November 1985. Talk given in Katherine Hunter's course for UofR medical students. [F08]

"The Computer Science Department," 13 October 2000. [F44]

Administration and management

House Rules, 1974 and 1981. [F24] RLS was a stickler for editing and preparing documents; these were the “house rules” for the UofR staff.

“Some Principles of Bureaucracy,”1979. [F46]

“Sybron Corporation: an illustration of corporate governance” [F28]

“Discussion of the Future,” 9 February 1988. [F47] Apparent transcript of a talk to the Commonwealth Fund Board of Directors. Notes are in [F48].

Talks about universities, science, research, and education

“The University, Today and Tomorrow,” 26 May 1976. [F06]<Talk at Conference on Universities and Medical Education. Leeds Castle, Kent, UK. There was one speaker from UK on universities and one from UK on medical. There was one from US on universities (me) and one from US on medical. I think Hugh Luchey of Cornell was responsible for my invitation. This was a very formal affair (dressing for dinner, for example) at the renovated castle given to the British Medical Society by the American Fairfax family.>

“Kazakhstan and Georgie: Science and Survival,” 28 October 1994. [F13]

“Global Warming,” 1996. [F14]. Prepared as part of RLS role in Republic of China’s Science and Technology Advisory Group (STAG). <This was additional language appended to my report to the Premier. The crossed out portions were eliminated to fit within the allotted time.>

“A Research Educator’s Viewpoint on Science, Technology, and the Modern Industrial Corporation,” 6 April 1986, remarks to General Motors advisory board. [F16]

“Striking the balance between public and private interests and responsibilities: situation in the United States,” November 1987. Delivered at a conference sponsored by *The Economist*. [F18].

“University-industry cooperative research relations.” [F19] “Rochester” version of “Striking the balance…”

“Multidisciplinary Research and Education,” with Harold Hall. 1987. [F20]

“From Blue Books to Bandwidth: The Future of Higher Education,” 13 October 2000 [F50]

“Nuclear Test Detection,” ~1993. [F31] <This was a paper at a small conference organized by Fred Seitz and John Moore. The subject was the role of science in

policy decisions. I have forgotten the sponsors and remember only the location: the American Chemical Society's building in suburban Baltimore. See also the first paragraph.>

"Nuclear Test Detection," 1994. Outline of presentation. [F32] <Talk on science base for policy decisions, the example of nuclear test detection. "Technical Resources" was (I think) a beltway bandit organizing the session but I have forgotten the occasion and the audience.>

"The Future of the U.S. Academic Research Enterprise," 9 December 1991. [F42] <The Government University Industry Research Roundtable was organized by Dale Corson and the National Academy of Science staff about 1985. Initially, it was composed of a Council and its committees. The Council members were agency heads or agency science heads, CEO's and VP's for research of corporations, and a few working scientists and university presidents. For at least 10 years it was an effective element of the Washington scene. One of the products was a report "The Future of the U.S. Academic Research Enterprise." A conference at NAS was held to critique the penultimate version. Although my talk should be read in conjunction with that draft, the basic ideas are understandable without it.>

"Defence, Science, and Defence Science," 17 November 1970. [F51] <Talk to the annual meeting of the Canadian National Research Council, Ottawa.

Clubs: Fortnightly, Pundit, Chatterbox, Torch

Sumner: 1974. Pundit and Fortnightly. [F04]

"Universities and activism", 16 April 1985. Torch Club. Notes. [F07]

"A Tale of Two Countries: Taiwan and Kazakhstan," Fortnightly (11 October 1994) and Pundit (25 October 1994). [F11]

"Mission to Kazakhstan," Chatterbox Club, May 1994. This talk was given jointly by RLS and Mary Sproull. [F12]

"The fuss about superconductivity," October 1987, Pundit and Fortnightly talk. [F17]

"National Energy Strategy," November 1990. Pundit and Fortnightly talk. [F21]
Also sent to Allan Bromley at the White House [F22]

"Skiing, Gold, and AC-DC", 1999. Pundit and Fortnightly talk. [F25]

"A Quick Romp Through the Quantum Century," 2006 [F29]; see also Bethe tribute [F30]

"The Palomares Incident," October 2002 [F33]

“Taiwan, Self Indulgence, and Education,” 17 October 1989. [F35]

“Dexter Perkins,” 20 October 1992. [F36]

“The Strange World of Nuclear Deterrence,” 18 April 1984. [F45] Talk prepared for UofR Wednesday evening talk that may have also been given at Pundit and/or Fortnightly.

“Carnassa Park,” 7 October 2003. [F55] It’s unclear where this short talk was presented; it may not have been one of the Rochester clubs.

Tributes, memorials, etc.

“The Future Comes Quantized,” 21 April 1990. [F23] <Principal address at retirement dinner for James Krumhansl, Cornell.>

“Hans Albrecht Bethe” [F30]

“Allen Wallis Memorial,” 30 October 1998. [F40]

Robert Berman, 1992 [F52]

Robert L. Sproull

A recent reading of Prof. Asuhel Kendrick's biography of Martin B. Anderson has prompted me to think about the extent to which biographical information on me in the University's Archives is in any sense complete. I do not, of course, know whether scholars of the university's history will ever be interested in my life **outside** the University, but I will attempt in this document and its attachments to help them if any care.

I believe my life **inside** the University is well documented already. The substantial (265+26 pages) oral history with Kenneth Wood is in the Archives. It "covers" 1968 to 1984 plus a little post-1984. Voluminous files and my handwritten journals are also in the Archives.

I will attach to this account several papers that were prepared for different purposes.

The first attachment, labeled "**CP**," is a talk I gave to mostly physics graduate students in 1998. I emphasized in it the role of physics in my life. It will probably be referred to in this narrative.

Another attachment ("**DS**") describes Deep Springs, the unique institution that played a large role in my life.

The talk ("**TA**," "Skiing,. . . ") explaining Telluride is only peripherally involved but is included to avoid any mystery about "Telluride." A supplemental paragraph added to the talk gives a brief explanation of the Telluride Association.

I 1918-1938

It is customary to start with birth, so here goes. I was born on August 16, 1918, in Lacon, Illinois. I do not wish to p;lay the Abe Lincoln act, and my birthplace was not a log cabin. But it was a small house with no indoor plumbing; the little hand-operated pump was directly over the well and poured into the kitchen sink; and the outhouse was not far from the kitchen door.

My father was born in Bedford, Indiana, and grew up in Lawrence, Kansas. He attended the engineering schools of the Universities of Missouri and of Kansas, but I do not believe he graduated. My mother was born in Dunlap. Kansas, and grew up on a farm near there. She graduated from the University of Kansas and taught school in Council Grove, Kansas, until her marriage. Father worked briefly for the Pullman Company and then for the rest of his life for the Public Service Company of Northern Illinois, the electric and gas utility for most of the towns around Chicago. I enjoyed accompanying him when, as local manager, he cared for the needs of important customers during or after electrical storms.

We moved to a different town every two or three years, as my father was promoted. Although this disrupted classes and friendships, I was a good student and even managed to skip a grade in one of the transitions. At every point my mother encouraged reading, academic achievement, and ambition. She was especially hard working and frugal, which brought our family through the Great Depression. .

We moved to Morris, Illinois, when I was about eight years old and then to Joliet. We moved to Highland Park to start high school in 1931.

An incident during my sophomore year in high school made a deep impression on me. It was in 1933, the bottom of the Great Depression. Unemployment vastly exceeded any numbers before or since. School teachers in Chicago were being paid in scrip (not dollars); to buy groceries they had to suffer huge discounts, and many had to go hungry.

A Deerfield Shields Township High School (where I was for my first two years in high school) each student was assigned to a "home room," and the first half hour of the day was spent there, usually discussing news, monitored and led by the home room teacher. On a January morning in 1933 the news was that in Miami a sniper had fired on a car containing Cermak, the Mayor of Chicago, and Roosevelt, the President-elect. Cermak was killed and Roosevelt was slightly wounded.

Edward Burwell, our teacher, opened the floor for comment. A student volunteered: "It's too bad they didn't get Roosevelt." Burwell lashed out at the student with a calm but powerful reaction. I remember exactly some of the words: "Of all the methods of government, surely government by assassination is the worst."

The student was doubtless only repeating what his parents, on the wealthy North Shore of Chicago, had been saying at the breakfast table. Burwell knew that at the dinner table that night he would be castigated as a communist and that he might very well be fired and unemployable.

I have never faced quite such a direct threat but I have often thought of Burwell's courage and tried to rise to his standard. What has happened many times in my life is that I was faced with choices at least one of which involved great risk I usually took the riskier one, and I do not believe I ever chose the path of lesser risk because it was the lesser risk. (Instances of situations requiring courage will be found in the 1968-1984 archive.) I am not claiming that this rises to the height of courage exhibited by Burwell, but it is similar. To take risks requires confidence. Many times I have said "I do not scare easily." I believe that a large part of my confidence came from the Deep Springs experience, to be reported below.

Transferring in 1933 from one of the best schools in the country to the mediocre Morris High School was a large comedown, but it had a great reward: There I met and began to court Mary Knickerbocker, who decorated the rest of my life and was a vital contributor and supporter in everything I have done. I was very pleased by the action of the University of Rochester trustees when I retired: They included Mary's name with mine on a program of graduate fellowships, recognizing that the Presidency was a two-person job.

An incident at Morris High School made a lasting impression. Once each week we were all assembled in the auditorium to listen to an "outside" speaker. The only talk I remember was a temperance lecture during which the speaker poured 200ml of alcohol and 200 ml of water together, and the result was demonstrably less than 400 ml. The speaker's loud, emotional conclusion was that booze "dried up your insides." I was intrigued and mystified, but I got no help from the physics-chemistry teacher. This apparent paradox was one of the several impetuses driving me toward science, and only later, in the Deep Springs Library, did I understand it.

I should mention that I had a brother, older by two years, although he was not an important part of my life.

I built two boats and experimented with radio. Summers, I painted our house and worked on the Chester Hunt farm a few miles north of Morris. Farm work ranged from the hard and dull (de-tasselling hybrid corn that was seven feet high) to the easy and interesting (caring for and recording the attributes of inbreeds that were candidates for white sweet corn hybrids).

My parents had skimped and saved to send me to college, but their savings and the savings of thousands of others had been lost–stolen, really–by the fraudulent activities of Samuel Insull, frauds that sent him to prison and were the key stimuli for the creation of the Securities and Exchange Commission. It had been made clear that if my father wished to keep his job, he must surrender his Public Service Company stock (sound and valuable) and receive stock in Insull's Middle West Utilities (almost pure water, worthless).

What little cash there was had to go for my brother at the University of Illinois. Although I was offered a Harvard College Fellowship, the prospects of a job in Cambridge were so poor that I could not take it. I was saved by being admitted to Deep Springs. I had learned about Deep Springs at my earlier high school. I applied, wrote papers, and was interviewed. I was delighted to be accepted since Deep Springs charged no tuition or board-and-room, a perfect match to my resources.

At this point I refer the reader to attachment '"DS."

Deep Springs was at that tim e a three-year program, nominally only two college years because of the work component. I entered a few days after my 17th birthday and left three years later a totally changed person. In my first year Horace Peterson opened my eyes to history and government study in a depth I had not imagined. Elmer Johnson taught drama and created a lifelong interest. Larry Kimpton interested me in symbolic logic although he did not understand it well enough to teach it. My work assignment the first semester was the laundry, the least popular job. In recompense, it was the garage the second semester, the most popular job and one that taught me a great deal.

In High School I had aspired to be an electrical engineer. At Deep Springs, by reading and talking with Kimpton and Peterson I decided to change to physics. Especially important was my discovery in the Deep Springs library of a book by Karl K. Darrow; his book *Introduction to Contemporary Physics* (the realm of quantum mechanics) was written at a level I could understand and opened an intriguing world to me. (Many years later I got to know Darrow, a fascinating character who ran the American Physical Society.)

In my third year I was elected Student Body President and Labor Commissioner. The Labor Commissioner assigns tasks and manages work projects. I designed and Herb Gustafson and I built a poured concrete culvert, over an irrigation ditch, that is still in use, one of my life's tiny but satisfying achievements.

I enjoyed the third year (Deep Springs changed permanently to a two-year program during World War II to allow deferment from the draft). Because of our greater experience, we third-year students were trusted with greater responsibility, especially in the garage, cattle operations, and use of trucks and machinery. Since our class had entered at the same time as Larry Kimpton, the fresh-caught Cornell Ph.D. who was Dean-Director in my second and third years, he had to treat us more nearly as equals.

The one- and two-week-long spring trips loom large in my memory as deepening my appreciation of nature (especially mountains) and as socializing and learning to know my fellow students.

We were blessed with many distinguished visitors. Even without consulting any records, I can still vividly remember E. T. Bell, James B. Conant, Sir Neville Sidgwick, Charles Coryell, Christian Midjo, and P. N. Nunn, and there were many others

Also in my third year, the faculty member hired to teach mathematics and chemistry canceled at the last minute. Larry Kimpton asked me to teach calculus as part of my work program. I was very happy to do it and I believe I became a good teacher, but it was hard keeping up with bright students; we actually "covered"(as attested by exams) the usual first-year university course plus a good deal of differential equations.

I next recount my experience as a house-guest of the P. N. Nunns' ménage à trois in January of 1938. You know about P. N. Nunn from attachment "TA." When I arrived at Deep Springs. P. N. was Chairman of the Board of Trustees, in residence for a few weeks, and he frightened all the first-year students. Fast forward now to 1938. The wood-fired heating plant was rapidly giving out by corrosion, and the Trustees had decided to replace it by an oil-fired central boiler and modified piping and controls. The vendor had been chosen and we students had laid out the working drawings for the installation. But everything had to have P. N.'s approval, since he was not only Chairman but also the engineer on the Board. Larry tasked me with taking all the drawings to San Diego and staying at the Nunns' mansion until P. N. approved. P. N. was interested in every detail, especially the sizing of all the components, down to the size of the wire from the thermostats. Two and a half years at Deep Springs had enlarged my competence and deepened my confidence to the extent that I enjoyed the job, and P. N. ultimately agreed without changes and with a smile.

The Nunns in San Diego were a *ménage à trois*: P. N., Mrs. Nunn, and P. N.'s "adopted daughter." At breakfasts for the four of us each of the three would have a newspaper, and the conversation would start by a different person on a different subject. But the conversation always stopped on the same sentence: "Oh, well. What can you expect with a crazy man in the White House?

II Cornell, 1938-1943

In my third year at Deep Springs I had applied for admission as a transfer student to Cornell, for membership in the Telluride Association, and for the privilege of residence at the Cornell Branch of TA on the Cornell Campus (the latter is called "preferment" in Telluride language). (See "TA") Although I could have transferred to another university, Cornell and the Telluride connection were the obvious choice. I was the only Deep Springer elected to membership, and Bob Henderson and I were the only ones granted preferment. And so, in September, 1938, I started as a transfer student at Cornell living in Telluride House.

Bob Henderson and I had had a course in chemistry at Deep Springs with Charles Coryell, a Postdoc at Cal Tech. (Coryell became rather famous in the Manhattan Project but died young.) We saw him only twice in the year, but he taught well by correspondence and we learned a lot in the rudimentary lab. (Henderson did anti-malaria drug research during the War and became a professor in the California college system.)

We had not been so fortunate in physics. We had a course nominally taught by Bill Mersman, a mathematician who had not a clue about physics. Also, there was almost no apparatus for experimentation. We received transfer credit to Cornell, but the only physics we learned was through unsupervised reading.

Thus at Cornell I skipped freshman and sophomore physics and began to take all the upper-class undergraduate physics courses that were offered. Since Cornell ruled that I was "deficient" in language instruction--I had had two-year exposures to Latin, French, and German, but not the three years Cornell would count--I had to take another language course. I chose German since I had been reading German most recently, and I immediately enrolled in upper-class courses taught in German. Thus I accidentally accumulated enough credits to graduate with majors in both physics and German in February, 1940.

My first semester was precarious. I was taking five courses instead of the usual four (in the spring it would be six) in order to accommodate both German and physics and in addition to take the great Carl Becker's course in modern European history; I had read several of his books and was determined to hear his lectures.

It was then the practice in that course (as in most Cornell courses) to set an hour-long examination half way through the semester. The crowd that arrived for the exam was about twice or three times the size of the class of students who had attended the lectures; I was astounded at the size of the group who had chosen to miss one of the greatest lecturers they could experience.

The examination room was the main lecture theater in Boardman Hall, a building that was originally the Law School and that was later (about 1960) torn down. A distinguishing feature was the presence of columns, required to support the upper floors but restricting viewing from seat to seat.

The proctor, evidently a graduate student, gave each of us a "blue book," the blank book for our answers, and a sheet of questions. I quickly read the two questions, each for half of the score. The first question was: "In what way were the circumstances at the founding of the Third French Republic more auspicious than at the founding of the First?"

I sat there, trembling. With an open-ended question like this, how was I going to rise to the standard set by students from Eastern high schools and prep schools, with two or three years of Cornell experience, and with confidence so great that they had not felt it necessary to attend the lectures? My own credentials looked good on paper, but they were from a miserable high school and a tiny, strange, and little known junior college.

I was so frightened that I could not put pen to paper. Then a student must have raised his hand (he was behind a column and I could not see him) and asked: "Sir,what does auspicious mean?"

I was off and running, with instant return of confidence. I remain indebted to that student, whom I never saw, who quite possibly saved my fledgling academic career.

A Telluride friend Anderson Pace had recommended that I ask for Lloyd P. Smith to be my undergraduate adviser. He was one of the youngest professors in the Physics Department and one of the few active in research. (I later learned that he was responsible for attracting Hans Bethe to Cornell, an acquisition that has made all the difference in Cornell's future.) When I went to ask him, he said that he had no undergraduate advisees and he wasn't familiar with all the College rules and options, but he was willing to sign my papers. I pointed out that my situation was so odd that a routine adviser wouldn't help much. Thus an association started that lasted for twenty years.

In my first semester at Cornell I took John Curtiss' course in Advanced Mathematical statistics (I cannot remember why). I cannot remember how I "raised my head above the grass"--I suppose it was questions I asked in class--but after a few weeks Curtiss asked me to write up my notes to serve as the first draft of a textbook he was planning. Although it was never articulated, he compensated me by recommending me as a calculus tutor serving those in his class (mostly graduate students in the Ag School) who were having trouble building an understanding of advanced statistics on the sand of their imperfect mastery of calculus. I soon had more business than I could handle, and my client list expanded to include undergraduates in engineering,

An incident of tutoring happened three years later: By 1941 almost all the physically fit male undergraduates were engineers, others being drafted. To stay in school they had to pass calculus; in those day calculus was almost exclusively a college subject, not a high school subject. By then I had the reputation of being the best calculus tutor on the campus ("best" a measured by the success of the tutored students. In any event, the Cornell Alumni Association hired me to tutor the footballers, freshmen and sophomores who had flunked. I was working successfully but had not been paid. I bought a program at a football game and annotated it to identify all the players who would not be eligible unless they passed calculus and who had passed calculus with my help. I sent the list to the Alumni Association and was paid within a week! [The lesson for entrepreneurs is : "Make your service a necessity for your clients and then watch your accounts receivable."]

I began to take a strong part in Telluride House management, becoming House President in (I believe) 1940. I was elected Secretary of Telluride Association, a post that at that time carried with it membership on the board of "Custodians," not janitors but the stewards of the TA endowment of scores of million dollars. We practically memorized a book by Graham and Dodd, Columbia University professors (I believe) titled *Security Analysis*. We did very well but we were too conservative to do spectacularly.

I graduated in February, 1940, but remained at Cornell, still taking undergraduate physics courses. I applied for graduate admission, fellowships, and assistantships at Columbia, Michigan, and Cornell. All responded positively, with varying amounts of support. It was easy to choose Cornell, because I was offered the President White Fellowship, I could stay at Telluride House and continue work in the Telluride Association, and there were still Cornell Faculty (notably Smith and Bethe) with whom I had not studied.

The War was looming. I was willing to be drafted but not enthusiastic. I was excited about physics, I was heavily involved in Telluride operations, and I was in love. R. C. Gibbs, the Chairman of the Cornell Physics Department, was becoming an expert at securing draft deferments; I was deferred as "essential to the war effort."

A Cornell Ph D. candidate chose a "special committee" of three to pilot and assess his or her graduate career. I was fortunate that Lloyd Smith (Chairman, experimental physics), Hans Bethe (theoretical physics), and John Gamble Kirkwood (physical chemistry) agreed to serve as my committee.

In the spring of 1940 I applied for and was hired to a summer job at Eastman Kodak, as part of the expanded staff to deal with the bulge in Kodachrome processing that occurred every summer. But then a letter arrived saying that all such employees had to be in Rochester for training at a date in June. I was forced, very reluctantly, to resign the job since I was Secretary of the Telluride Association and must attend the Telluride annual business meeting at exactly that time (incidentally, to make matters worse, the meeting was at Deep Springs).

But with astounding good Jeck, a second letter invited me to a much better job for the summer in the Development Department (apparatus development, not developing film). I raced back from Deep Springs to start as soon as possible, and the summer was a great learning experience (and, incidentally, one of only three times in my life that I worked a 40-hour week, and never did I have my evenings free).

I learned much physics from my boss, Steve MacNeill, and mechanical design from Fordyce Tuttle, but I profited even more by learning to work within an organization, to make effective use of highly capable technicians, machinists, secretaries, and others.

Kodak offered me job again the next summer, but by the spring of 1941 it ws clear that a war was coming, and I learned that Bell Telephone Laboratories was heavily involved in electron tube development for secret applications (radar, as it turned out). I was delighted to get a job at BTL, which was then in Manhattan (Murray Hill had not yet been built). Learning was even more intense than at Kodak, witih 6+ day weeks and working for and with J. O. McNally, Jerry Shepherd, and especially J . R. Pierce, the most ingenious engineer I have known.

Both jobs taught me to understand what a fully supported scientist or engineer needs and how to get it. The standard of machinist support, for example, was far above Cornell's. Many years later, in 1960-63, when we were organizing the Material Science Center and planning Clark Hall, I made good use of this instruction in standards of support. Meanwhile, both jobs had given me a healthy respect for applied problems, an appreciation of the benign working conditions in at least some industrial laboratories, and an introduction to the management of technology-based companies.

Lloyd Smith suggested a thesis research experiment to detect "quantum noise," the fluctuations in the electromagnetic field of a resonant cavity required by quantum mechanics. It took only a little study on my part to see that the experiment was not possible with the state of the art electronic apparatus (the experiment was done successfully after the War, with microwave equipment that had been developed for radar).

Smith then told me about an observation that had just been made in a classified electron tube development project: Greater current density of electrons from oxide-coated cathodes could be obtained if drawn for only very short times (microseconds). Smith was not free to tell me but I realized from my BTL experience that he was talking about cathodes of magnetrons, the key microwave generators for radar. I set out to learn why this enhancement occurred and how to take advantage of it. I did not learn a great deal, but I guess some of it was useful and publication was held up until the end of the War. In any case, I earned a Ph.D. that was granted in April, 1943.

Along the way I had to pass the "Part A" exam , both written and oral, in both the major and both minors. At the oral exam, Kirkwood was the first examiner. He started by saying: "Sproull, we haven't seen much of you recently." True. His course in advanced chemical thermodynamics was taught at the same hour as Bethe's course in "Electric Waves" which was really a course in microwave technology based on Bethe's work and his commuting to the MIT Radiation Laboratory which was developing radar. I had been attending Bethe's lectures and had arranged with a fellow student to study his notes of Kirkwood's. Kirkwood posed a question and I covered three blackboards with partial derivatives; I arrived at the result he sought but by then was so confused that I did not recognize it! A disastrous beginning.

Bethe was next and asked a question about the "philosophical" foundations of quantum mechanics. He knew that I knew the answer well

(I had learned it from him), and I was able to regain my composure and deal adequately with Smith's questions. The committee passed me, and years later, when Kirkwood was dying of cancer, he visited my lab in Brussels and referred to the incident with more humor than rancor.

Mary and I were married on June 27, 1942, in Morris, Illinois. I had managed to buy a rather tired small Ford coupe and save enough gasoline coupons to drive to Morris. We then drove straight back to Ithaca, stopping for only two days at Allegany State Park, our honeymoon.

Mary got a job doing statistical work for the Agricultural Economics Department at Cornell and I was working as a Research Assistant on another Smith project as well as finishing my thesis research. It was easy to decide what to do next: Since anything I could do in the War would have a long time developing, I had best make a transition with as little lost motion as possible. That meant following Smith to RCA Laboratories in Princeton to work on microwave radar. I finished my thesis on 1 April 1943.

III Princeton

Mary had gone to Princeton to explore the availability of housing; the essence of her report was "there wasn't any." RCA and a penicillin factory had moved to Princeton after it had become impossible to get priority for residential building We went anyway and started living in a single room in April of 1943. [We soon moved to the attic of a garage, then to former slave's quarters, extensively remodeled, and then to a renovated attic in Penns Neck.]

Mary worked for a League of Nations statistical unit that was housed during the War at the Institute for Advanced Study. Once she came home glowing: "Einstein opened the door for me!" I was impressed by what a good sport she was with all our travel and housing problems and started abbreviating it to "sport," which continues as a nickname.

I worked at RCA Laboratories six days a week on microwave radar projects, mostly for the Navy. My only real output, however, was on an Army project for X-band (3 cm) radar, called BUPX for "Beacon, Ultra Portable, X-band." But it came too late to have any impact.

Evenings for three semesters I taught physics to Marine V-12 students and a few civilians at Princeton University. Also in evenings, I taught at Camden a classified course, (sponsored by the University of Pennsylvania) in microwave physics and engineering for RCA engineers and officers at the Johnsville Naval Air Development Center.

By late 1944 I became convinced that the war was almost over. I had been working with Navy officers in the Airborne Coordination Group who were moving experimental equipment into the Fleet. They got me a billet as Lt. JG and I passed the physical exam. But RCA opposed my release and my draft board would not release me; RCA was fighting to retain staff since they expected difficulty in so doing at the end of the war.

In the early winter of 1946 we had to decide among jobs. RCA made a good offer, but it was obvious that the Laboratories were going to concentrate on television, and I had seen enough of experimental television to know that I wanted nothing to do with it. Cornell, under the leadership of Bethe, Bacher, and Smith, was greatly expanding its Physics Department, and Smith offered me an Assistant Professorship in the physical electronics group. I accepted and started on 1 April 1946.

IV Cornell, 1946-1963

We again faced a housing shortage We took the risk of buying a modern house on South Hill, outside of town. We had very little savings, largely because of paying exorbitant prices for bad housing during the war, and there was a risk that I would not succeed as a physics professor. But it worked out very well. Dal e and Nellie Corson bought half of our lot and built on it; the 22-year association of our families has been a major enrichment of our lives. Dale as mentor, associate, and boss has been a strong and positive factor in my professional life.

I had been very curious about electronic processes in Barium Oxide crystals. Extensive experimentation on alkali halides had produced much understanding but no work had been done on alkaline earth oxides, putatively the next simplest solids and practically important since they were vital components of all electron tubes. The reason for the absence of work was that Barium Oxide (and to a lesser extent the other alkaline earth oxides) is the most powerful chemical drying agent, and the slightest exposure to water vapor destroys it. It is also refractory. No one had grown crystals of it. I set out to do so.

In 1946 Lloyd Smith and I had one of the first Office of Naval Research Contracts One feature of our contract was that we contracted to do work *such as* some specified research topics. I believe our contract was the first (or perhaps onliy one of the first few) contracts to use *such as* language rather than to specify what was to be done. But "Success has a thousand fathers; failure is an orphan."[See page 4 of my *Annual Reviews* article, *Annual Review of Material Science 1987*]

We all had excellent graduate students in the period after the war, and I believe I was especially blessed. We eventually succeeded in growing BaO crystals, by a novel method, and exploring their properties and processes, never, however, learning much about why they are such good electron emitters. The Office of Naval Research supported this work through one of their first contracts.

I will now continue the topic of reporting research, although it is a little ahead in this narrative. The BaO work continued until about 1955. At that time I was stimulated by a paper by W.W.Tyler, a former thesis student of mine, to begin a program of using very low temperature (down to 4 degrees Kelvin, the boiling point of liquid helium) thermal conductivity measurements to study imperfections in nearly perfect crystals. Heat conduction in solids is by quanta of vibrational energy called phonons; phonon processes form a rich segment of modern physics.

My teaching assignment was the usual upper-class physics, analytical mechanics and advanced laboratory for physics majors. In addition I taught a course in "modern physics" for electrical engineers.

This was atomic, molecular, nuclear and solid-state physics, applying one-dimensional quantum mechanics. Although there were many courses with a title like this without the "solid-state," they were oriented toward physics students and mostly taught by high-energy (nuclear) physicists.

The invention of the transistor by Bell Telephone Laboratories people in 1948 and the junction transistor in 1951 made all investigations of the solid state exciting. It was obvious that important devices would be developed, although I know of no one who anticipated the spectacularly capable computer-on-a-chip. Courses like mine became in high demand; I even taught several out-of-hours courses for nearby industry.

Since there was no textbook, I began to write up my notes and problems for the student. Much of this was done on airplanes or in hotel rooms, and the rest evenings at home. Publishers encouraged me to make these notes into a book. The rough manuscript produced offers from five major publishers; of these the finalists were Prentice-Hall and John Wiley & Sons. I chose Wiley because I liked the way Wiley's salesmen represented the authors of other books and I appreciated Wiley's study of and comments on my manuscript. I have never regretted that decision, which (as you will see) had major consequences, all favorable. The book was published in January of 1956. It was immediately widely adopted and made many friends for me. It was translated into (I believe) five languages (although I can name only four). The royalties and translation-rights payments were welcome but were not the major reward. People are still coming up to me at airports and thanking me for the insights they gained from the book. My interaction with the Wiley staff when preparing the manuscript for printing widened and deepened into my service as Wiley's chief physics advisor.

The ONR organized a committee, largely of their contractors (like me), to visit Navy laboratories. My service with this Solid State Advisory Committee (which was soon called "The Chowder and Marching Society) began a lifetime of volunteer committee activity, almost exclusively for the Federal Government, directly or indirectly. I frequently ended as chairman. I have wondered why; I believe a major reason is that I took committee activity seriously and did not join unless I could be at virtually every meeting, whereas many fluttered in and fluttered out.

I also began consulting, typically at companies to which my Ph.D. students had gone. (But the first was Bendix Aviation, which did not fit that pattern.) The most serious were Union Carbide and Oak Ridge National Laboratory. The latter connection started when we went to Oak Ridge for a sabbatic in the the spring semester of 1952. I began research on radiation damage to nonmetallic solids, a program that continued after I left under my direction from Ithaca plus frequent weekends in Oak Ridge.

At Cornell an expanding group of solid-state experimentalists was trying to do research in the dirty and inadequate old wooden-framed Rockefeller Hall. I began to spend a larger fraction of my time raising money for major equipment and renovations for others as well as for my group, piecing together support from several sources to get a Collins helium liquefier, for example. I did not realize it at the time but I was sliding into the role of facilitator.

I began working for the American Physical Society. I became Chairman of the Division of Electron and Ion Physics and planned several of its meetings. In this work and in planning an APS meeting in Ithaca, I associated with Karl Darrow who was the real chief executive officer of the APS. I served for three years as Editor of the *Journal of Applied Physics*, which is (like the APS) a unit of the American Institute of Physics. This service made few friends and many enemies; the JAP had to be self-supporting, without advertising revenue, and I had to reject more manuscripts than I accepted.

At a meeting chaired by Frederick Seitz in the spring of 1957, the ONR's Solid State Advisory Committee became concerned about the Federal support of research; it had stopped expanding and possibly was declining. Seitz appointed a subcommittee with me as chairman to study and report. We worked during the summer of 1957 and produced a report that documented the decline and gave concrete examples of promising avenues of research that were foreclosed by lack of funds. The report *Solid State Physics Research, Performance and Promise,* was issued in

October, 1957. Ordinarily it would have been acknowledged and filed away, but a few days later Sputnik appeared. Suddenly the "name of the game" in Washington was no longer "How can you get by with less?" but "How can you effectively spend more for research?" Our report was thus accidentally effective, and some of the fame rubbed off on Cornell and on me.

Meanwhile there was another sabbatic to plan. I looked for someone to lead my lab temporarily, in my absence. By this time I had a number of European friends and I wrote to them for suggestions. Mollwo at Erlangen had already suggested that a student of his, Robert O. Pohl, who was doing a thesis on ZnO, write me to learn if he could work in my lab. Mollwo and Martienssen of Frankfort recommended Pohl highly, and I offered him a job as postdoc for a year, paying out of my NSF grant. It was one of the smartest things I ever did: Bobby came on April 1, 1958, and by the time I left on August 1 he was running the lab more effectively than I had been doing. He quickly became well-known in the Department and was appointed an Assistant Professor; he built a lively research group which has produced many interesting and important papers on phonon physics. He advanced rapidly and has now retired as Professor Emeritus.

There were several possibilities for interesting posts during my sabbatic. The finalists were a one-year appointment as Webster Professor of Engineering at MIT and "collaborateur scientific" at the Union Carbide Laboratory in Brussels. The latter had been proposed and arranged by Jim Krumhansl, then the Director of the Carbide Lab in Parma, Ohio. Jim had been a close associate and office-sharer at Cornell for ten years before going to Carbide, and I was a consultant at Parma. Mary and I chose the Brussels job in large part because we expected it would be valuable experience for our children, then 8 and 10 years old.

It was a tough year for all four of us, but rewarding. It was possible at all because of the gracious support by Guy Pevtchin (the Lab's Counsel and Administrator) and Roger Gillette (the Director and only American in the Lab). I advised on publishing research papers in American journals, edited papers and reports to Carbide, and carried out an investigation that was published in the British *Philosophical Magazine.* I also lectured around Europe on the results of our Cornell research, supported by NATO.

The decision to spend a year in Brussels was taking a major risk, but we lucked out. Our children were in a French-language school (Nancy's teacher knew not a word of English) and the work was more advanced than in American schools for their ages. Mary helped them evenings with French and I with "calcul." At the end of the year the school urged Bob to take the examination that all students at his age take to sort those who will go on to the lycée (college-bound) from those terminating school. The school was very proud that Bob passed with flying colors, especially in the language section of the exam.

Meanwhile, back in the U. S., Dale Corson was leaving the Cornell Physics Department to become Dean of Engineering. Lyman Parratt was ambitious to replace him and would make an excellent Chairman for teaching and local administration, but he was completely disconnected from Washington and unable to attract students to his precision X-ray research because his only student was taking ten years for his degree. Solid-state and low temperature faculty (now called "condensed matter" physicists) led by Paul Hartman with effective support by Corson (who was well connected and highly respected in Washington) wanted an organization to enhance funding and above all to get a replacement for Rockefeller Hall. I was the obvious choice to lead such an organization, and Hartman called me to learn if I would be willing. I agreed since I recognized the need and realized that my agreement would make it possible for the faculty to agree on Parratt as Chairman and return the Department to full speed. I probably agreed too readily (I was well aware of the cost of the transatlantic telephone) since as soon as I agreed I lost all ability to get concessions or commitments. But it worked out all right.

We had already laid plans and made commitments for an extended vacation in the summer of 1959. We drove our little car through Switzerland and Germany, chartered a sailboat on the Dutch canals, and climbed mountains in Norway. It was the last period for 25 years that I did not have administrative responsibility.

During the academic year 1959-1960 we organized the Laboratory of Atomic and Solid-state Physics and used it as a base to compete for one of the materials science contracts that were being proposed by ARPA, the Advanced Research Projects Administration. This program is described in the *Annual Reviews* article included with this text. We were interested in the "umbrella" nature of the proposed contract, which permitted local allocation of funds. But we were primarily interested in the provisions for new buildings. We won the largest of the first three of the contracts, and our contract provided $4 million over 10 years for new space. At last we had the solution to our building problem.

I had been the chief author of our proposal to ARPA, ably assisted by Henri Sack, and it was clear that I should be the Director of the new Materials Science Center. But that meant I had to give up physics research and bet that I could succeed as an administrator. [I did continue to teach, however, including the upper-class course in quantum physics, using the excellent text by Robert Leighton.]

Most of my time was devoted to planning the building. No one asked me to do it, but I became the client for the building, to work with the architects and engineers and represent not only the immediate users of the building but also (in as far as possible) the users in subsequent generations. Someone had to do it, and I was responsible for satisfying the building provisions in the ARPA contract.

Cornell decided to build roughly twice the space supported by the ARPA contract, in order to accommodate needs in other departments, and ultimately obtained a gift from the Clark family for something like $4 million. In the end there were seven units (MSC, LASSP, Physics Department, Engineering Physics Department, Chemistry, Astronomy Department, and Space Science Center), each represented by a powerful champion wanting the most space and best location he could get.
John Burton, the Vice President for Business, had the authority to plan the building, but he lacked the patience and the competence to deal witih the (often highly technical) problems; he had the good sense not to delegate to his own Building and Properties staff, who were only marginally competent and were kept busy dealing with the connections of the

building into the campus system of services.

I visited a number of academic and industrial laboratory buildings, took many photos, and shared them with the faculty who expected to be residents. I was given an absolutely firm upper limit for the estimated cost. John Burton had chosen the engineering firm of J. Fruchtbaum to design the building, an engineering firm instead of the usual architect on the theory that it would produce the best working laboratory. Unfortunately Jack Fruchtbaum, while agreeable and trying very hard to satisfy, had no imagination whatsoever. But we were saved because the architectural firm under Jack was Warner, Burns, Toan, and Lunde, and Charles Warner had abundant imagination. Many times it would be Warner and Sproull *vs.* Fruchtbaum; though Jack nominally could dictate, we always won. (And I don't think he ever complained to Burton.)

Fruchtbaum had drawn an increasingly complicated basement, in his attempt to accommodate every wish. It became so byzantine that it was possible to throw it out and start over with the modular plan that seems to have worked very well for forty years. The only specialized deviations from the standard module were for David Lee's troughs to accommodate his extra-high ultra-low-temperature apparatus.

I remember vividly a key occasion. The boundaries between units were always disputed: Everyone wanted more space. I brought the penultimate plans and all seven champions to our living room one evening and announced that there would be no drinks until everyone signed off. After much discussion some minor modifications were agreed to, and everyone signed a little before 11:00 PM. And we all remained friends.

The top floor was used as a club over my head: If the bids had not come in under the budget (with an adequate contingency provision), the top floor (seventh, I believe) would be omitted. Fortunately, holding the line on details kept the cost down, and the floor was retained.

By the winter of 1962-63 construction of the building was well under way and I was rarely needed. Fully occupied as MSC Director and client for the building, I had cut my ties to substantial research. Bobby Pohl had taken over my lab and was doing far better, with more and better graduate students, than I had done. Paul Leurgans was the very capable Associate Director of the MSC. Among other possibilities, I was considering seriously becoming Dean of Science at Wesleyan University (Connecticut).

James Perkins had just been appointed President of Cornell , and he was selecting his staff; his first choice was his best, he recruited Dale Corson to be his Provost. Corson told him of my imminent motion to a permanent job elsewhere. Perkins called his friend Jerome Wiesner, Kennedy's Science Advisor (who was especially powerful because he had helped Kennedy become elected). Wiesner discussed two two-year posts that might be negotiated, which could result in my return to Cornell. I was much impressed by Perkins and flattered by his attention. I went to Washington to be interviewed by Wiesner and to discuss the jobs, the Deputy Director of the NSF and the Director of ARPA . I was much more interested in the latter and so went first to the Pentagon to sessions with Harold Brown (Director of Research and Engineering), Gene Fubini (Deputy) and Jack Ruina (the ARPA Director who was leaving May 1). Harold offered me the job and I accepted, committed for two years.

V ARPA

I had promised only to start at 1 September, but Jack left on his schedule May 1 and so I tried, by several trips to Washington, to cover for the Agency during the summer. The most urgent task was to help the Joint Chiefs prepare testimony supporting Senate ratification of the Partial Nuclear Test Ban, but there was also much I had to learn about other ARPA tasks and ARPA people.

ARPA, the Advanced Research Projects Agency, was created in 1958 as a response to Sputnik. It managed the big space programs briefly, until NASA was born. Thereafter its program consisted of projects that were too advanced for the Services or that would benefit more than one Service (Army, Navy, Air Force). The Director of ARPA was also one of the Deputies of Harold Brown, the Director of Defense Research and Engineering. Harold reported directly to the Secretary of Defense on all matters of research, development, and acquisition of weapons systems It was inevitable that there would be conflicts between ARPA and the Services, colored by envy of our close connection with DDR&E. Harold invariably defended us; he was fond of saying "I appoint the people; I don't have to control the program."

Lee Huff has written a history of ARPA, not published, I believe, but available in manuscript form from the Institute for Defense Analyses (IDA). IDA had contracted with Richard J. Barber Associates, Inc. to support Huff's research and writing ARPA's history. One chapter is "The Sproull Years."

The two years in ARPA were great experience but very hard work. There was much traveling to do (twice around the world) and always ten-hour days and Saturday work. There was demanding and fascinating interaction with Congress, with other agencies in the Executive Branch, and with foreign defense research direction. Mary and Nancy did not have so interesting an experience and had little fun living in an apartment. but they were good sports. Bob spent rewarding years at Exeter and Harvard.

VI Cornell 1965-1968

In the spring of 1965 Perkins and Corson offered me the position of Vice-President for Academic Affairs, and I readily accepted. My roles were: 1.) Working with the deans on appointments, notably tenured appointments. 2.) Supporting several initiatives to improve undergraduate education. 3.) Connecting the Art Gallery and the libraries to the Central Administration and reviewing their budgets. 4.) Miscellaneous tasks as a member of the President's staff.

VII Rochester, Oral History with Ken Wood

At this point the account of "outside" activities is interrupted and the reader is referred to the oral history of my tenure at Rochester. The oral history begins with my arrival at Rochester on 1 September 1968 and is better organized and more detailed than this account of "outside" activities. It covers the period from the summer of 1968 until retirement on 1 July 1984. It is in the University of Rochester Archives in Rush Rhees Library.

VIII **Other Activities**

I return below to the account of "outside" activities, mostly after retirement from the Presidency of Rochester on 1 July 1984.

Corporate Boards

My corporate service began with consulting and becoming the physics advisor for John Wiley & Sons. In 1965, after over a century as a family company, Wiley became a public company. I was elected one of the first "outside" directors. Over the next few years I learned how to be an effective director, in the most benign environment. I am most indebted to Wiley people, especially Andy Neilly.

In about 1970 United Aircraft invited me to join their board. I had become something of an aviation buff through my Pentagon experience and I was a consultant to UAC's laboratory; I believe Fred Seitz, who had been on the board but resigned to join the Texas Instruments board, must have suggested me. A most impressive team (Art Smith, Earl Martin, and the legendary engineer Bill Gwinn) flew to Rochester to recruit me. The exploration occurred at lunch at the Genesee Valley Club, and thus began a most interesting and rewarding association, until my retirement 19 years later. I became, among other posts, Chairman of the Pension Committee. I learned a great deal and was in the entire period the only technical person on the board. (UAC changed its name to United Technologies in about 1974.)

In about 1971 Don Gaudion, Chairman of the U. of R. Board and CEO of Sybron Corporation, asked me to join the Sybron Board. I was reluctant to do so because of my other commitments, but he insisted and I was in no position to refuse. Its meetings were in Rochester and so it was not time-consuming. But it became a real problem, which I have described in another paper (**Sybron**) to be filed with this account.

In about 1974 Jim Wilmot, Roger Lathan, and I flew to Westchester on a Xerox plane to make a presentation to Peter McColough for our capital campaign. (In any U. of R. campaign, Kodak was the first and Xerox the second stop, to set the scale of the campaign, to use in all our solicitations of others.) The meeting went well and Peter indicated that our request would be sympathetically viewed. Although he did not write a check, we expected, and later received, all we had asked. As the meeting was breaking up, Peter took me into an adjacent room and asked me to join the Xerox board. Although I was already on too many boards, I believed I had to say yes. Service on the Xerox board was pleasant but the board was much less useful and effective than the UTC board. At least a major part of this lack of effectiveness was because of the tradition of Joe Wilson, who did not need a board. Another part was the domination by Bob Strauss and Bill Simon.

When Allen Wallis became Undersecretary of State for Economic Affairs he had to resign from the Bausch and Lomb board. I was glad to be invited to replace him, since I was close to retirement and since B & L was still a technically based company. Then I was the B&L director designated also to be a director of its laboratory animal subsidiary, Charles River Associates.

In addition to service on boards, there was one major corporate involvement of a different kind: I was one of the initial members and the second chairman of the General Motors Science Advisory Committee. We took our work very seriously since we each year had a half-day session with the Executive Committee of the Board of Directors. In return, they took us seriously and adopted most of our recommendations. (In retrospect, we should have been even tougher on them.)

At this point I insert a listing I made of my "post retirement " activities, in response to a request from the University's PR Department (my guess is they wanted something to put into their obituary file). Some of these began before my retirement from the University in July, 1984.

My successor, Dennis O'Brien, made it immediately and abundantly clear that he wanted to see as little of me as possible. He demonstrated this even before 1 July 1984. For example, he replaced me on the Wilmot Foundation Board and even captured the University's Annual Report for 1983-84. Although there were many ways I could have helped him and served the University, I recognized that retired presidents can, at least in the eyes of the incumbent, be mischievous.

Accordingly, I responded to only **out-of-town** invitations to serve in volunteer activities.

U. S. Government

Department of Energy

Founding Chair of Basic Energy Sciences Advisory Committee

Chair of Inertial Confinement Subcommittee on Fusion Policy Committee

Consultant and member of several classification exercises

Department of Defense

Ballistic Missile Defense Advisory Committee. Chair of C-cubed-I panel

Institute for Defense Analysis (primary "think-tank" for DoD). Trustee and member of Executive Committee

House of Representatives Armed Services Committee. Solo testimony on strategic deterrence and ballistic missile defense, three hours, 8 July 1987,

(prepared and given without any staff support).

National Academy of Sciences

Institute of Medicine. Appointed by Frank Press (President of NAS) to create and chair a committee to bring back to life the failing IOM, after NAS and NAEngineering the third academy. NAS took our recommendations, and IOM is now alive and healthy.

Government-University-Industry Research Roundtable. Member and member and chair of several subgroups.

National Research Council committees.

Corporate

Member of Board of Directors of

Bausch and Lomb

Charles River Laboratories

John Wiley & Sons

Sybron Corp.

United Technologies

Xerox Corporation

(at most four at any time)

In addition to the usual committee memberships and chairmanships, I served as Chair of the Pension Committee of UTC for several years, supervising a few billion dollars of pension fund investments. Also, I was temporarily chair of the outside board committee that operated and peddled Sybron when it had been attacked from the inside in 1985.

Republic of China

Science and Technology Advisory Group. Eight members from five countries advised the Premier and helped ROC to transit from making junk like Christmas tree ornaments to making high technology computer and communications gear.

China Foundation for the Promotion of Education and Culture. Supports education and culture in Taiwan through grants and fellowships. I am one of four American trustees. (Curiously but not entirely accidentally, we all have University of Rochester connections.)

International Executive Service Corps

Mission to Kazakhstan, to rejuvenate and connect to Western science the Kazakh Academy of Science. (A short article in the *Rochester*

Review by Tom Rickey reported briefly on this.)

Mission to the Republic of Georgia, to create a "think tank" for economic development, reporting to Prime Minister Shevardnaze and his deputy.

K-12 Environmental Education

Independent Commission on Environmental Education. This group's goal was to assess environmental education in the schools, to get more science into environmental education, and to promote avenues for other improvement. Fred Seitz, Jeffrey Salmon, and I recruited and organized, and I chaired the group, 1987-90.

Environmental Literacy Council, a standing follow-on to the above. I was CEO from 1990 to 1996 and then Chairman of the Board until 2005.

Other

Harvard Community Health Plan. I recruited, organized, and chaired the Loran Commission to recommend policy on extremely costly medical interventions. Among other distinguished members were Betty Friedan, Hayes Rockwell, and Robert Freeman.

Johns Hopkins University. Consultant on the design of a Mind-Brain Institute

New England Conservatory. Honorary Doctor of Music degree

A Scientist's Tools for Business, a small book published by the U. of R. Press.

Link Foundation. Chair of the Technical Advisory Board.

Commonwealth Fund. Trustee.

Poona, India. Consultant on the design of a technical university.

Continuing Activities, 2006

Environmental Literacy Council
China Foundation
Marshall Institute
Link Foundation (Special Advisor)

Return to 2006 Manuscript

As the reader can see, my activities were pretty miscellaneous and mostly short-lived.

I should add a note about my present involvement, although small, with the George C. Marshall Institute in Washington. It came about because of my interest in strategic missile defense. I had become involved as a consultant and committee member after President Reagan's 1983 speech. In that speech he called for a "shield" of defense against the threat of attack by the Soviet Union. A shield against tens of thousands of warheads and perhaps hundreds of thousands of "penetration aids" (mostly decoys) was appealing but clearly impossible with 1983 weapons and technology.. Yet the President *must* have some alternative to the "massive retaliation" called for in the then current doctrine. Also, new threats, from so-called "rogue" nations, were of more manageable size.

In ARPA we had studied the problems of a one-on-one engagement, mostly interceptor performance and decoy discrimination . The President's speech made it possible for the first time to study the command-and-control problems of a missile defense system , and they seemed to many to be the "show-stoppers." I thought that the future lay in continued development of interceptors, continued progress in reduction of number of warheads by arms control negotiations, and development of systems. We seem finally to be on such a course. Unfortunately a great deal of money and some time have been wasted, mostly by expensive projects in the National Laboratories and by premature deployment.

It has been impossible to discuss all this on any university campus. It is "politically incorrect." The combination of launching by a Republican president, the "shield" nonsense, and the totally inappropriate labeling of the program as "star wars" have made public analysis impossible. I have naturally joined the discussion on the politically incorrect side by serving on the Strategic Defense Initiative Advisory Committee, by my Congressional testimony, and by joining the Board of the George C. Marshall Institute (which espouses unpopular causes).

The Marshall Institute is currently concerned mostly with "global warming." again on the unpopular side. I know little about it but I support the Institute because I believe the subject needs more thought and science than it is getting. [One of my favorite aphorisms is "Where everyone thinks alike, no one thinks very much."] My current, highly tentative, views are: 1. There is probably some secular increase in the average surface temperature of the earth. 2. There is probably an appreciable fraction of this that is man-made, (slightly) by breathing and (mostly) by the combustion of fossil fuels. 3. The portrayal in the press, on TV, and now in the movies of the effects of global warming are almost entirely assertions and speculation and encourage a vast exaggeration of the probable effects of warming. 4. Regardless of whose fault it is, and even if it is just natural variation like that which has often occurred, we should be experimenting with ways of reducing greenhouse gases and their effects and we should create a worldwide measuring and monitoring system. [The Dutch did it to improve their country, with no nonsense about "global catastrophe."] 5. The only promising alternate fueling is by electricity or hydrogen as energy carriers from nuclear reactor generation; this gives the "left" and the "green" factions a problem because of their long-standing opposition to the safest form of energy.

Robert L. Sproull
June 2006

Three by Three

Withrow Lectures, October 2004
Robert L. Sproull

I am extremely pleased that you have invited me to serve in the Withrow Chair. It is an opportunity that I could not pass up, even though I am less capable than I was some years ago. I should like to stimulate analysis and discussions of three major themes or issues: **leadership, management,** and **governance**, which we will encounter in three environments: **government, universities,** and **industry**. There will be other major motifs, notably **responsibility**, and minor threads such as **accounting.**

Now if this vast area were to be handled in the best academic tradition, I would create a three-by-three matrix and we would systematically deal with the nine entries. But that would be as dry as Eureka Valley, and I do not believe I could hold your attention. Furthermore, I do not have anything fresh and new to say on all of the nine pairs. Instead, I have prepared this autobiographical essay as a ladder on which to hang several dozen accounts of incidents or activities that illuminate eight entries, in the hope that real people and first hand reporting of actual events will give life to the account. Hearing how I learned about these may help you learn.

Leadership is frequently invisible anf so you may have to look for it within the context of management. Leadership and management will be addressed in the context of individual behavior, whereas governance is an institutional affair. It will become apparent that I have little to say about the government-governance arena, which is the realm of professional political scientists, but the other eight hyphenated pairs will get attention.

One further comment: Finley Peter Dunn invented a character "Mr. Dooley" who had many trenchant, if slightly illiterate, sayings. One was: "It aint what I doesn't know that hurts me. Its what I does know that aint so." (Recently, William Safire quoted a corruption of this and ascribed it to Mark Twain.) You have been warned, but I think I can guarantee accuracy in everything except possibly some dates and titles.

I apologize for the amount of "I," but "it comes with the territory." I recently read Walter Isaacson's new biography of Benjamin Franklin, in which he quotes Franklin's acknowledgment of his tendency to "indulge the inclination so natural in old men to be talking of themselves." You will have to be the judge as to whether you can mine useful ore from this pile of rock.

In my manuscript I have marked the major takeoff points with the @ sign. I shall be prepared to talk on each. of these, but there will not be time for all of them. If I get no substantial feedback from a limited exposure of this document, I will make my own selection of these.

Why should you listen to what I have to say? I hope it will be interesting. But I also hope it will stimulate some thought about careers and choices. I have often explained to "outsiders" that one of the purposes of Deep Springs is to encourage—virtually to force—young men to think *early* and deeply about their careers and how they are going to make an impact. This aspect of Deep Springs is in sharp contrast to the experience at conventional colleges and universities, where career planning almost invariably comes later (which means many paths are rendered difficult or even foreclosed) and rarely includes any elements except money and job security.

@1 (Withrow)

@1A (Courage)

My four years in high school in Illinois nearly coincided with the worst years of the Great Depression. I worked in corn fields in the summer but could not save much money. My parents had saved to send me to college, but all their savings and those of thousands of others had been lost—stolen, really—by the fraudulent activities of Samuel Insull, frauds which sent him to prison and were the key stimuli for the creation of the Securities and Exchange Commission.

Although I was offered a Harvard College Fellowship, the prospects of a job in Cambridge or Boston were so poor that I could not take it. I was saved by being admitted to Deep Springs. Then as now Deep Springs charged no tuition or board-and-room, and that zero was a perfect match to my resources. In retrospect, it was the best thing that could have happened to me, and I have tried to repay my debt to the school by serving on the Board of Trustees and by raising funds.

I will quickly pass over my adventures **@2 (Deep Springs)** here, except for three remarks. First, I had intended to study electrical engineering, but at Deep Springs, although we had only marginal physics instruction by a non-physicist instructor, I read enough to be intrigued by physics. Also,Larry Kimpton, a fresh-caught Ph. D. in philosophy from Cornell, interested me in symbolic logic, and studying it "on the side" intrigued me about pure mathematics. Then, in my third year, a last-minute defection from the faculty left the school without a calculus teacher, and the Dean-Director **@3 (Larry Kimpton)** asked me to take over as part of my work program And finally, as I realized only much later, I was simply a different person after three years of Deep Springs; I believe most of the difference came from accepting *responsibility* in a variety of jobs.

At the T.A. Convention in 1938 I was elected to Telluride Association. I transferred to Cornell and lived for three years in Telluride House. **@5(Trembling_** I became a Custodian (financial officer) of the Telluride endowment even though I was too young to sign the papers; I had to learn substantial corporate accounting **@4 (Accounting)** in a hurry. Calculus tutoring **@6 (Tutoring)** became a major part of my financial support. Summer jobs at Eastman Kodak and Bell Telephone Laboratories also helped financially, but their major contribution was education: I learned more **@7 (Summer Jobs)** in each of those two summers than in any year in the university. To go now to 1938, my first semester at Cornell was the hardest. I was taking six courses, tutoring calculus to pay for tuition, and participating in House activities. I had to learn physics rapidly.

I was fascinated by quantum mechanics, which was not all that old or seasoned then. My first course in it was under Earl Kennard who had written a splendid book on the kinetic theory of gases but who did not believe in quantum mechanics. But then—saved again--I had the blessing of an advanced course with Hans Bethe, who is one of the half-dozen giants of quantum theory and its applications. Incidentally, Bethe had lived in Telluride House when he came to Cornell as a German refugee.

A major event was marriage to Mary in 1942; she has supported all I am describing in many ways. Although her vital contribution was always well known within our family, it was not until near my retirement from the University of Rochester that it was publicly accredited; then under the leadership of Jim Wilmot, the Trustees acknowledged that the Presidency was a two-person job.

The War was coming on, and we were all in a hurry. I was given deferment from the draft as "essential to the war effort." Fortunately I did not have to prove I was a necessity. I did an experimental thesis, which became classified because of its application to microwave magnetrons, and I went to RCA Laboratories in Princeton to work on Navy radar. **@8 (Radar).**

That was during the daytime; evenings, I taught physics to Navy and Marine students at Princeton University and microwave theory and technique to Navy and industrial engineers for the University of Pennsylvania. Mary worked for the League of Nations unit in Princeton. **@9 (Princeton)**

Let me pause here for a little stocktaking of what I had learned. By this time I had learned to take satisfaction from **teaching**. I had learned the sobering discipline of accepting **responsibility @10 (Responsibility).** I had learned **respect for applied problems;** later in this talk we will see more of this. I had learned that hiding within an applied problem could be a **fundamental** question which when explored had wide implications. I had learned the powerful role of **connections @11 (Connections)** between fields of science, notably physics, mathematics, and chemistry; for example, I learned that with Maxwell's equations and a little mathematics, one could "make mermaids" in microwave engineering and devices and that Hilbert space could serve electron beam excitation of microwave resonant cavities as well as it serves to reconcile Heisenberg with Schrödinger, I had learned that, with the investment of only a few dozen hours, learning the elements of **accounting@12 (Accounting)** has far-reaching applications.

You will not be surprised, then, that after the war I had no desire to remain at RCA, where everyone was going to work on television, and gladly accepted an Assistant Professorship at Cornell. Cornell, like Rochester and everyone else, was planning to move from the "love and string and sealing wax" age to the electronic age in experimental physics and was assembling a staff. Most came from Los Alamos, but some of us came from the radar community. I set up a program to grow BaO crystals, which had never been grown, in order to explore their intriguing and unique electronic properties, a substantial step from the much-researched alkali halides.

Lloyd P. Smith and I had one of the first Office of Naval Research (ONR) contracts--I believe it was the fifth. Incidentally, I believe I was one of the first physicists to hire a chemistry post-doc on his physics contract. I wish I could take time here to describe **@13 (ONR)** the contracting innovations and the enormous contribution that ONR made to science by developing its mode of support, later copied by NSF, AEC, and others. In talks and papers I have frequently acknowledged that contribution and examined how well other agencies have served as stewards of the ONR tradition.

The decision by ONR to support research and graduate students in universities was a momentous one, profoundly affecting the character of universities. Yet it was not a "decision" at all, in the usual sense. A few people in ONR, well below the presidential appointee level, pushed this development up from the bottom by courageously writing different contracts. Some of the names were Lawson MacKenzie, Shirleigh Silverman, and Emanuel Piore, but there were others. They called themselves "the bird dogs" and waved accounts of the World War II atomic energy development and (especially) radar to convince their superiors that the country would be stronger if Naval laboratories and industry were supplemented by basic research in universities. The new pattern was presented gradually and skillfully to Congress, where the aura of radar success was powerful and pervasive. The bird dogs' leadership probably will not make it into the history or political science books, but they changed the academic, and even the real, world. "You either get something done or you get the credit, never both."

@14 (Federal Support)

The next few years were a golden period for me, and for most physicists. The graduate students were superb. Morale was high, even though we were trying to do research on nearly perfect crystals in an old, dirty, wooden-framed building. One had, of course, to compete hard for money, but the money supply was expanding and the program managers in Washington were able and unbureaucratic.

We all worried about the possibility of Federal direction of research, but it never happened in my experience The most imminent threat was the infamous Mansfield Amendment, Section 203, but Congress recovered its sanity within a year, and it became safe to ignore it.

The landscape of Federal support has deteriorated some in the last few years, primarily by the rise of interest and power by the Office of Management and Budget. Auditors from the granting agencies have also become more numerous and bureaucratic.

Mansfield, 203 Multiple sources of support

@15 (Book)

One of my teaching assignments was a course in "modern physics" for engineers. I broke rather sharply from the tradition of modern physics courses established at Cornell by Floyd K. Richtmyer and continued in his book for many years by co-authors **@15 (Book).** I emphasized applications to molecules and solids and—horror of horrors—used MKS units (later modified to be more fundamentally based and called "S.I.", the units used by engineers) instead of cgs (the units used by scientists). Evenings and on airplanes I wrote my notes and problem sets into a textbook, which was the only one of its kind at that time. Some reviewers were appalled at seeing the electronic charge expressed in coulombs, but the book was widely adopted, largely because, I believe, of the dynamic growth of interest in solid-state devices. (The second edition, years later, was a much better book, since it profited from more teaching experience, including extramural teaching, and writing it was not so rushed.) The book made many friends and opened many doors for me. Among other outcomes of door openings, I became the chief physics advisor for the publisher John Wiley & Sons, and when they became a public company in 1965 I became one of the first two "outside" members of the Board of Directors. **@16 (Directors)**

I also did considerable consulting in industry and began service on Washington committees.**@17 (Committees)** Consulting is both a problem and an opportunity. The problem arises when a professor is away from campus too much or gives consulting a priority call on his time. The opportunity arises when the consulting work enriches his teaching and on-campus research and provides useful connections with industry for students' jobs and company gifts to the university. An extreme but illustrative example is the professor of chemical engineering who teaches the design of petrochemical plants. He can hardly construct any such on campus but can participate with the builder of such plants and use his consulting to stay at the moving edge of his field.

For a few years I was the Editor of the *Journal of Applied Physics*, which made few friends and many enemies: Since there was no society behind it, I had to reject more papers than I could accept. I used a sabbatical at Oak Ridge **@18 (National Laboratories)** to immerse myself in radiation damage in solids. A sabbatical at European Research Associates **@19 (Brussels)** in Brussels gave our family an interesting experience, gave me a chance to lecture in eight European laboratories, and produced a paper on the motion of charged dislocations in LiF.

There was still a lot of life in BaO research but I became intrigued with the promise of using phonon scattering at very low temperatures as a tool for studying imperfections in nonmetallic crystals. Although this approach started a lively program with excellent students, I was spending more and more of my time working for others, especially getting money for electron microscopes **@20 (Management)** a helium liquefier, and other major equipment, and upgrades to the electrical supply and plumbing of an impossible building, Rockefeller Hall. It was in this connection that I learned a fact about management in the Government. I had applications in to several Federal agencies and several foundations to support this work. One day I received a call from NSF: " Would $16,763 do you any good?' I replied, of course, "That is exactly the figure I had in mind." It was in the month before the end of the fiscal year, and every agency must spend all "its" money (if it did not its appropriation for the next year would be cut).

We organized the Laboratory of Atomic and Solid State Physics to provide a base for such activities and especially to get, somehow, a new building, and I became the first Director. With that as a launching pad, we entered the competition in the Advanced Research Projects Agency (ARPA) for an interdisciplinary materials science contract. We won one of the three first, and the largest, of these contracts. I believe the principal edge we had in the competition was that we had had an outstanding group of Ph.D. students **@21 (Students)** since the War, we knew where they were and what they were doing; and they were almost all working in areas that made full use of their graduate education. I became the first Director of Cornell's Materials Science Center supported by that contract.

There were two major contributions from the contract. First it served as umbrella support to augment and fill in the gaps in contracts and grants for individual projects; this *local* allocation and management was highly efficient. Second, it provided funding for a new building. My central task was acting as the client for the new building. ARPA contracted for $4 million over 10 years **@22 (Clark Hall)** for a building, but more money was needed if we were to get a prime position on the campus, and ultimately the building, Clark Hall, housed elements of six different academic departments and centers. Each of these users was represented by a chairman or director, and getting all these powerful horses into the starting gate was a major problem.

I imagine you have already seen what was happening to my career. I had helped bring in very able faculty and get the initial research support for them. They were brighter and better physicists than I was. As I drifted more and more into the role of facilitator, it was obvious that the better graduate students were going to work with them, not with me. So, I really had no choice, I became an administrator.

I pause again to take stock of what I had learned. I will not dwell on the lessons that all who do experimental research have learned, such as how to fight with the physical plant and purchasing departments, how to survive with machinists and glassblowers, and the necessity for back-of-the-envelope accounting **@23 (Accounting)** of research funds. I reinforced my view about the difference that taking responsibility makes, but in addition learned the burden that accrues from needing to take responsibility for the actions and inactions of others **@24 (Responsibility),** associates and colleagues.

I learned in my research that I should always think about a question first, and only then study the literature; in that way I had the greatest chance of creating a fresh, or even novel, approach. I learned the absolute necessity of having at least one **other major activity**, in addition to and largely independent of one's employment (See 15, Book).

I learned that decisions **@25 (Wheedling)** in a university are made in different ways at different "heights" in the academic hierarchy. Most administrators draw organization charts of their territories. I have always refused to draw one for a university. This is because the chart would be different for different decisions (tenure for faculty different from annual budget, for example) and the connecting lines would have to be of varying strength. Also and much more important, most of the important direction of a university is leadership by example and persuasion, not management by ukase.

We are now at 1963. I was recruited by Jerome Wiesner, President Kennedy's Science Adviser, and Harold Brown, Director of Defense Research and Engineering, to become Director of ARPA. The argument was that they and ARPA had delivered a big boost to Cornell, and now it was only fair for Cornell to deliver me. I was attracted to the Kennedy administration **@26 (Kennedy)** because of its center-left position and its commitment to controlled response, rather than massive retaliation, as the basic posture of the U. S. in the nuclear age. I knew that ARPA, even though a small agency, was involved in some fascinating operations. I knew that ARPA was a major player in supporting the negotiations for a ban on nuclear testing. I later learned, of course, that there were in addition far more interesting operations that I had **not** known of.

ARPA was only 5 1/2 years old at that time. It had been created in the post-Sputnik rush to push the space program, and for a short time (until NASA was created) ARPA managed all the big space projects; they were all gone (to the newly created NASA) by the time I arrived. ARPA could, and several times on my watch did, write checks the same day a project was proposed. Compare the typical Federal program in which six to eighteen months is the typical time required. The Director of ARPA had great latitude, since he was only the second level down from the Secretary of Defense, and my boss (Harold Brown) stated publicly "I control the people and so I don't have to control the programs." Our biggest treasure, however, was our relations with the science and engineering community **@27 (ARPA Strength).**

ARPA is now 45 years old and is still lively. Of the dozen or so ARPA programs during my watch I will describe only two: **@28**

(ARPA Program I) @29 (ARPA Program II).

To take stock again, first, the responsibility theme: The ARPA experience intensified my understanding of the difference between advising the Government and directing a Government agency, I had done a great deal of the former, but in that role I could always pack my briefcase and return with relief to my university. Now, in ARPA, there were decisions that must be made, on time, and with attention to all the implications. And, in the midst of an orderly approach to an **important** opportunity, the red telephone would ring with an **urgent** problem, and there is a Gresham's Law of management, "The urgent drives out the important."

@29A (Federal Government)

In ARPA I interacted with the press, almost for the first time. **@30 (Press)** I learned how to behave when I did not have freedom of speech. In ARPA there were serious constraints on my speech, mostly by classification and by the proprietary rights of contractors.

Much later, at the University of Rochester, I had an especially bitter reminder of this. **@31(Alexis)**

@32 (Palomares)

To return again to my narrative, in 1965 I returned to Cornell, as Vice President for Academic Affairs. I continued to teach physics, but it had to be at 8:00 AM, since the President, James Perkins, owned me for the rest of the day (and many evenings). By then I had worked my way down from teaching advanced graduate courses to teaching the sophomore course for physics majors, plus a few chemistry and mathematics majors.

My responsibilities were mostly working with the deans on academic appointments and tenure decisions, improving undergraduate education, especially in the College of Arts and Science, and being the administration contact and budget officer for the Art Gallery, the Libraries, and the Admissions Office. But I also ran errands for the Provost and President and pitched in with others in alumni, community, and Federal relations. I learned a great deal about university administration and working with trustees **@33 (Trustees)**. I also learned much about New York State financing, because of the four New York State "contract colleges" at Cornell.

Then in 1968 I was invited to be Provost of the University of Rochester I had known a good deal about the University: While I was a student, several fellow graduate students had taken jobs at Rochester. There had been several joint brunches of the Rochester and Cornell physics departments at Krebs Restaurant in Skaneateles. I had lectured in the Eastman Theatre for the Rochester chapter of the American Chemical Society. All of this experience attracted me to the University.

But my decision to go there as Provost was perhaps a foolhardy decision; it certainly involved great risk. My predecessor had been forced to resign, and the campus was in an uproar. I quickly became what would be called in a corporation the "Chief Operating Officer." But a university should not be forced to have a COO! The problem was that the times demanded that that function be carried out. Many of our trustees, like university trustees everywhere, believed that we should have a Cyclone fence around the university and armed guards at the gates, but the faculty and administration were committed to an **open** university, where every position and point of view could get a respectful hearing. That clash was bad enough, but an even worse one was the clashes among groups that argued by slogans at the bumper sticker level, whereas the university was committed to **raising** the level of discourse.

Well, I don't want to take your time with more about "the bad old days," but I will be at your disposal for questions **@34 (Bad Days)** My predecessor, Allen Wallis called the times "the great campus craze of the sixties." I called it, perhaps over generously, the "student shenanigans." Although the draft and the Viet Nam war played a prominent part, there was clearly more to it than that, since the worst student riots were in Paris. At each American university the focus of excitement was different; at Rochester confrontations developed around the Center for Naval Analyses **@35 (CNA),** a controversial tenure appointment, and numbers of black faculty. The spirit of a university campus simply must change every few years, and each year after about 1970 was a little better, both at Rochester and at other places. By "better" I mean we did not have to concentrate on keeping the peace and could address educational problems. The transition came none too soon, since we had to become preoccupied with inexorable budget problems.

In 1970 Allen Wallis, the President, thought I was going away to become President of Brown. As a counter move, he got the Board to approve a change of titles, in which he became Chancellor and I became President, thereby creating considerable confusion. There were only two things wrong with this perception: Brown wasn't going to ask me and I wasn't going to accept if asked.

In 1972 Allen announced that he would like to retire, and the Trustees came to me to ask if I would be President and Number 1, which would be labeled in a corporation as "Chief Executive Officer." I replied that there was nothing I would rather do but that I could not accept an appointment like that; the job was difficult enough at best but impossible if I came in under a cloud of illegitimacy, as viewed by faculty and students. If the Trustees were to conduct a conventional national search and to identify me, as the best they could attract, I would be happy to accept. So they mounted a search, and in order to be absent while the search was conducted I spent a semester at the Center for Advanced Study in the Behavioral Sciences at Palo Alto, which (with apologies to Thorstein Veblen), has been called "the leisure of the theory class." The Trustees came to what I considered to be the correct conclusion, Allen retired, and in 1974 I became Chief Executive Officer, as well as President, with a full plate of responsibilities.

I could not continue to teach physics. Not only would it have been hard to find the time, but also it would have been a disservice to students, since I could not continue to enrich my teaching, at whatever level, by contact with the frontiers of physics. I continued to do some physics extramurally as Chairman of the Defense Science Board and later as Chairman of the General Motors Science Advisory Committee.

I learned a vast amount in these years, some of which I wish I had not learned. The whole field of medical **@36 (Medical)** education and practice was new to me. The responsibility theme appeared again, even more virulently; I was amazed that the media, and even many of the University's friends, were unaware of the lack of symmetry in a conflict between a person with responsibility and one without. The major new pattern I learned was the difference in the behavior of the same individual when he was constrained by his ties to a **constituency @37 (Constituencies)** from that when he was thinking and acting as an individual.

I retired in 1984. I could have stayed on, but I had resolved not to stay a day longer than allowed by the faculty rules **@38 (Pepper)** that had been in effect earlier. Also, I had served 16 years as No. 2 and No.1, and it was time for a change. My successor gave abundant signals that he wanted to see as little of me as possible. That was fine with me, and I went off to other things.

By 1984, partly in preparation for retirement, I had joined several corporate boards, continuing the Wiley Board and adding United Aircraft (which changed its name to United Technologies), Xerox, **@39 (Xerox)** Sybron, **@39A (Sybron)**, and later Bausch and Lomb.

The Commonwealth Fund, a medical foundation, had given us several million dollars for a premedical education program, and Commonwealth, like Wiesner and Brown, said that since they had done that, I ought to serve on their board. Similarly, after the meeting at which Xerox gave the pacesetting gift to launch our $108 million campaign, the CEO took me aside, and you know what!

Retirement from the University came just in time since various crises (**@Sybron**) in these companies demanded increased participation, often with a technical content, and I was invariably the only scientist or engineer on each board.

I will take your time to report only a few highlights of my other post-retirement activities.

I chaired a committee **@40 (IOM)** for the National Academy of Sciences to reorganize the Institute of Medicine, the third National Academy (after Science and Engineering), which had been collapsing. Then I chaired a group to advise the Harvard Community Health Plan; the talented group included Betty Friedan. Our task was, fundamentally, to recommend a policy for dealing with extraordinarily expensive interventions..

For twelve years I was a member of the international Science and Technology Advisory Group (STAG) for the Premier of the Republic of China, Taiwan. I chaired the Technical Advisory Board for the Link Foundation, was a board member and member of the Executive Committee of the Institute of Defense Analyses, and did much advisory work for the Department of Energy and the Department of Def18ense. I wrote a little low-level book for business people. And there were a good many other activities. As you can see, it was a pretty miscellaneous array, These were all in addition to continuing to help in raising funds for the University.

But the most interesting and challenging work was two missions for the International Executive Service Corps, to Kazakhstan **@41 (Kazakhstan)** and to the Republic of Georgia **@42 (Georgia)**.

In Kazakhstan I was to reorganize the Kazakh Academy of Sciences, which turned out to be impossible, at least within my limitations. But I succeeded in making some connections to the West and in helping individuals to end their isolation from world science.

In Georgia I helped five young intensely patriotic engineers and scientists to create a "think tank" to conduct studies for the rehabilitation of Georgia. This was a remarkable experience because my point of attachment was Khabashvili, Shevardnadze's principal deputy, and because I had a productive session with Shevardnadze himself. You will have read that Eduard Shevardnadze was the Soviet Foreign Minister who with Gorbachev negotiated the Russian side of ending the Cold War and the break-up of the Soviet Union. The last I knew the "think tank" wa still operating although Schevardnadze has been dumped by his legislature.

Among other things, I learned from these two experiences that, far from being a union of republics as advertised, the USSR had been a brutal colonial empire. Russia made all the decisions, especially those allocating resource use and the location of manufacturing jobs. Kazakhstan is rich in minerals, gas, and oil, and Georgia is rich in land and climate for orchards. During the days of the USSR, K's ores or partially refined ores were shipped to European Russia , refined, and turned into manufactured goods. These were then sold to K. This is exactly the pattern that both U. S. and Europe exhibit with respect to, say, chromium mined in Africa; the good jobs are in America or Europe, and the poor, dangerous jobs are in Africa. It will take many years to build manufacturing industry in K. and to create a structure to market the produce from Georgia.

A particularly tragic exploitation by Russia is the city of Rustavi, near Tbilisi in Georgia. It was built by Russia to manufacture heavy iron and steel products like railroad cars and military tanks. Now, the Russians have walked away and the manufacturing plants are deserted and trashed. Even the copper and aluminum conductor has been stolen from the electrical transmission lines. The people are still housed in the forbidding Soviet-style apartment buildings (eleven stories high and a quarter of a mile long), but they have no income. They are fed by relief funds from Europe and the U.S.

Now, almost all of my volunteer work has come to an end. I am still the head of a group, the Environmental Literacy Council, that is attempting to improve the quality of environmental education in the schools, K-12. I guess it is a continuation of my progression from teaching advanced graduate courses to the sophomore course for physics majors, and now to kindergarten. That has to be the end of the line!

Finally, a few comments about the three-by-three themes. I believe leadership was specifically called out only once (Licklider's seminal achievements). But leadership was in the background, shaping choices and selecting and motivating people and thereby forming the framework within which management functioned.

I am proud of the fact that I "played the hand that was dealt me." That is, when entering a new position I did not cause wholesale firings or abrupt turns in direction. But a few firings made a big difference from to time. In ARPA I found jobs outside the Agency for two professionals each year (one doesn't fire in the Civil Service system}; the effect on the remaining staff was palpable and positive. At the University of Rochester I had to fire the black man whom we had hired to support black students; he had been dealing drugs. Again, the effect was strongly positive, although I could not be sure of that in advance.

Maybe incidents like these should be called "management" rather than "leadership", but in any case they are influential in setting the tone and spirit of an enterprise.

As to governance, no matter what you may think about the current occupants of the major positions, the management structure appears to be stable and effective in universities;.nothing in my experience points to a need for substantial change in structure. (I have said that I would not discuss governance in Government, but it is easy to imagine healthy changes in Congress, none of which is likely to happen.)

There is need, however, in the governance of corporations. Major structural change is probably necessary, not just cosmetic changes and sending a few felons to jail. The Sarbanes-Oxley legislation has been loudly touted but does little. New ideas are needed. Until then, the system depends on conscientious directors and honest CEO's but cannot guarantee that such will be in charge.

6 October 2004
1 **Withrow** 17 March 2006

Note : This page-and-a-half is included to complete the Deep Springs lecture; it is only peripherally related to the autobiography. 17 March 2006

Jim Withrow was a major contributor to the revival of Deep Springs after the dark ages of the original Board of Trustees and the night of the Fort far right. His service is not (according to me) well appreciated, in part because of his blunt language and unpopular positions and in part because he was eclipsed by Bob Aird. I have called Aird the "Second Founder" of Deep Springs, and he certainly was the chief figure in returning it to academic respectability. But Withrow also played a key role, raising the standard of stewardship of the Board, working for fiscal responsibility, often writing checks on the spot to avert a budget crisis, He suffered unpopularity because of his opposition to drugs and the excesses of Berkeley and because of his intolerance of shoddy argument and feeling as a substitute for thought.

It is unfortunate that Aird and Withrow were not friends, primarily (I believe) because each envied the other, although each also respected the other. Aird (Yin) tended to downplay Withrow's crucial financial support and his helpful hard-nosed appreciation of people. Withrow (Yang) tended to downplay the essential contributions that Aird had made before Withrow became active. They were both essential to the rebirth of Deep Springs, and we owe them much.

I also owe Jim for much instruction, help, and friendship in Telluride operations. He served as President, Custodian, and later Treasurer during turbulent times. A typical example was the help he gave me (when I was House President) to neutralize antediluvian alumni opposition to granting preferment in Telluride House to Fenton Sands, the first black resident.

I felt honored to be invited to speak at the New York City memorial service for Jim.

We are also indebted to Jim's consort, Bea Renfield, a remarkable business woman, for her generosity to Deep Springs and her care of Jim.

Donovan, Leisure, Newton, Lumbard, and Irvine
Cement case OSS (ask Paul Todd)

1A Courage

An incident during my sophomore year in high school made a deep and lasting impression on me. It was in 1933, the bottom of the Great Depression. Unemployment vastly exceeded any numbers before or since. School teachers in Chicago were being paid in script (not dollars); to buy groceries they had to pay with heavily discounted script, and many had to go hungry.

On the North Shore of Chicago at Deerfield Shields Township High School (where I was for my first two years of high school) we each were assigned to a "home room." The first half hour of the day was spent there, usually discussing news, monitored and led by the homeroom teacher. On a January morning in 1933 the news was that in Miami a sniper had fired on a car containing Cermak, the Mayor of Chicago, and Roosevelt, the President-elect. Cermak was killed and Roosevelt was slightly wounded.

Edward Burwell, our teacher, opened the floor for comment. A student volunteered: "It's too bad they didn't get Roosevelt." Burwell lashed out at the student with a measured but powerful reaction. I remember some of the exact words: "Of all the methods of government, surely government by assassination is the worst." The student was doubtless only repeating what his parents, on the wealthy suburb of Chicago, had been saying at the breakfast table. Burwell knew that at the dinner table that night he would be castigated as a communist and that he might very well be fired and unemployable. But nevertheless he courageously took a strong position and taught brilliantly.

I have never faced quite such a direct threat, but I have often thought of Burwell's courage and tried to rise to his standard. What has happened many times in my life is that I was faced with choices at least one of which involved great risk. I usually took the riskier one, and I do not believe I ever chose the path of lesser risk *because* it was the lesser risk. I am not claiming that this rises to the height of the courage exhibited by Burwell, but it is similar. (Many times I have said: "I do not scare easily.") To take risks requires confidence. I believe that a large part of my confidence came from the Deep Springs experience.

Another incident from my high-school years occurred in the very poor high school at which I was for my junior and senior years. Once each week we were all assembled in the auditorium to listen to an "outside" speaker. The only talk I remember was a temperance lecture during which the speaker poured 200ml of alcohol and 200ml of water together and the result was demonstrably less than 400ml . The speaker's loud, emotional conclusion was that booze "dried up your insides." I was intrigued and mystified, but I got no help from the physics-chemistry teacher. This apparent paradox was one of the impetuses driving me toward science, and only later, in the Deep Springs Library, did I understand it.

INGREDIENTS

4

2 Deep Springs 1935-38

I suspect you will have heard more than enough reminiscences in informal settings, but I will mention here some salient points in my experience in 1935 to 1938.

It was then a three-year program, and I was very grateful for the third year. Because of our greater experience we third-year students were trusted with greater responsibility, especially in the garage, cattle operations, and use of trucks and machinery. Since our class entered at the same time as Larry Kimpton, the fresh-caught Cornell Ph.D. who was Dean-Director in my second and third years, he had to treat us more nearly as equals.

The one- and two-week-long spring trips loom large in my memory, as deepening my appreciation of nature (especially mountains) and as socializing and learning to know my fellow students.

We were blessed with many distinguished visitors. Even without consulting any records, I can still vividly remember E. T. Bell, James B. Conant, Sir Neville Sidgwick, Charles Coryell, Christian Midjo, and P.N. Nunn, and there were many others.

As I remarked at the beginning, one of the features of Deep Springs is its encouragement to think early and deeply about a career. When I arrived I had planned to become an electrical engineer, and "service" was not part of my vocabulary. When I left, I had resolved to become a physicist and I hoped to serve in a career as a teacher. I cannot now be certain how these changes came about. But I believe the change in subject was owing to reading, notably *Introduction to Contemporary Physics*,(QC21 D22i, 1926) a book on quantum physics by Karl K. Darrow (a fascinating character whom I got to know much later), and the resolve to serve was owing mostly to submersion in L. L. Nunn's writing and discussions with Bob Henderson and Ed Cronk.

I should recount my experience as a house-guest of the P. N. Nunn's *ménage å trois* in January of 1938.

First let me say a little about P. N. As you know, he was L. L.'s brother, a schoolteacher recruited by L. L. to electrify Telluride mines. The famous letter from L. L. to P. N. in Ohio implored him to come but not to reveal that he was not an engineer. He trained himself rapidly and "talked a good line" in negotiations with Westinghouse as well as in laying out the power lines to the mines.

When I arrived at Deep Springs in 1935 P. N. was Chairman of the Board of Trustees and was in residence for a few days at the start of the term. He assembled all the new men (12 of us, an unusually large class) in the conference room . He sat at the head of a long table. We were frightened enough at the start and more so after a long stage pause. He then boomed out: "Young gentlemen, you are here to be exploited." The meaning was clear to those of us who had already read the "Gray Book," but others were mystified. The meeting proceeded in a confused manner, almost torpedoed by Bob Henderson's antics, but we had a safety net in that P. N. did not know any of our names.

Fast forward now to January of 1938. The wood-fired heating plant was rapidly giving out and the Trustees had decided to replace it by an oil-fired central boiler and modified piping and controls. The vendor had been chosen and we students had laid out the working drawings for the installation. But everything had to have P.N.'s approval, since he was not only Chairman but the engineer on the Board. Larry tasked me with taking all the drawings to San Diego and staying at the Nunn's mansion until P. N. approved. P. N. was interested in every detail, especially the sizing of all the components, down to the size of the wire to the thermostats. Two and a half years at Deep Springs had enlarged my competence and deepened my confidence to the extent that I enjoyed the job; and P, N. ultimately agreed without changes and with a smile.

The Nunns in San Diego were a *ménage à trois*: P. N., Mrs. Nunn, and Mrs. Julia Hamilton, P. N.'s "adopted daughter." I ate meals with them. I remember three (possibly four?) breakfasts. Each of the three would have a newspaper, and the conversation would start by a different person on a different subject. But the conversation always stopped on the same sentence: "Oh, well, what can you expect with a crazy man in the White House?"

The Board of Trustees in the 1930's almost killed Deep Springs. They were all cronies of L. L. and I believe were all selected by him (but I may be wrong about one). The most sensible Trustee and the only one really concerned for Deep Springs was Harold Waldo, a Salt Lake City lawyer. (others–not me–denigrate his concern, which was well short of devotion, to the circumstance that two of his sons were D. S. students). The noisiest and most mischievous was F. C. Noon, probably the smallest-minded man I have ever met. Fanny Noon was an executive of some kind in a savings and loan company in Los Angeles. W. L. Biersach was the Treasurer, a dull, sweet, harmless man who kept the books; the capital accounting was all done in Richfield, Utah, and none of us ever saw it.

Carroll Whitman, I believe L. L.'s nephew, visited from time to time and stirred things up; although he was a Trustee for a few years, he rarely attended meetings. I believe it was he who told us about the principal investment of the Trust, the Telluride Motor Company in Provo, Utah, and he alleged that the Trustees had major investments of their own in the Company. We had no way of checking, but the income from the Trust was small and declining. My memory is imperfect, but I believe that later (when Parker Monroe joined the Board) we verified this illegal mingling of interest.

What, you may ask, about the Student Body Trustee? It may have been through him that we became convinced of the malfeasance. He must have seen at least some of the books, although the real business of the Board was done in rump sessions, without him, at the Jonathan Club in L. A. In any case, we believed the story, whether on the evidence of our own Trustee or on Monroe's.

Power pole Isolation Culvert

3 Larry Kimpton

Larry worked hard behind the scenes my (and his) first year to get the Trustees to fire Walter Crawford (the Dean-Director) and install Kimpton. In the next two years he continued as an accomplished political animal. But he was an intellectual, and Crawford was not. He had good taste, but bad luck, in hiring faculty. It was a blessing that faculty could be hired for board and room and almost no cash, since the Trust was providing very little cash.

He left several years after 1938 to become a junior administrator in the Manhattan Project (atomic bomb) at the University of Chicago. From that position he bored into the U. of C. administrative structure and eventually became President.

INGREDIENTS

Genevra and Collingwood Evangeline MacKenzie

Midjo Cattle fraud?

(Later) Visit to Telluride House by L.K. and Marcia K ;

Mortimer Adler and the Great Books.

4 Accounting

My own introduction to accounting was: Keeping records of my boyhood Saturday operation of collecting old newspapers, baling them, and selling the bales to the local cardboard box factory; then the office job, double entry, at Deep Springs, which carried with it at that time doing the operations (not the capital) accounting of the Trust; and then learning elementary corporate accounting as a Custodian of Telluride Association, from books and older members.

Whether you do it in that inefficient way or more efficiently by a college course, I strongly recommend that you do learn elementary accounting. It is usually denigrated because the intellectual content of basic accounting approximates that of learning to type, but like learning to type it is vital to a host of operations and studies. No matter how good a leader you are, if you do not understand what the books are telling, you will not be in charge.

As an aside, I might note that one of the accounts on the DS books was "Maintenance Institution Personality," evidently a "correction" by someone who thought "personalty" was a typo. I have thought many times in my interaction with the corporate world that "personality" is *le mot juste* to apply to corporations, which have all the idiosyncrasies and individual traits of people. (This anecdote also illustrates the difference between producer's language and consumer's language, which I can expand on if you are interested.)

6 Tutoring

In my first semester at Cornell I took John Curtiss' course in Advanced Mathematical Statistics (I can't remember now why). I cannot remember how I "raised my head above the grass," but after a few weeks, Curtiss asked me to write up my notes to serve as the first draft of a textbook he was planning. Although it was not articulated, he compensated me by recommending me as a calculus tutor to those in his class (mostly graduate students from the Ag School) who were having trouble building an understanding of advanced statistics on the sand of their imperfect mastery of calculus. I soon had more business than I could handle, and my client list expanded to include undergraduates in engineering.

By 1941 almost all the male undergraduates were engineers, others being drafted. To stay in school they had to pass calculus; in those days calculus was exclusively a college, not a high school, subject. By then I believe I had the reputation of being the best calculus tutor on the campus ("best" measured by the success of the tutored students). In any event, the Cornell Alumni Association hired me to tutor the footballers, freshmen and those sophomores who had flunked. I was working successfully but was not being paid. I bought a program at a football game and annotated it to identify all the players who would not be eligible unless they passed calculus and who had passed calculus with my help. I sent the list to the Alumni Association and was paid within a week!. The lesson for all entrepreneurs is: Make your service a necessity for your clients and then watch your accounts receivable.

7 Summer Jobs

In the spring of 1940 I applied for and was hired in a summer job as part of the expanded staff at Eastman Kodak to deal with the bulge in Kodachrome processing that occurred every summer. But then a letter came saying that all such employees had to be at Rochester for training at a date in June. I was forced, very reluctantly, to resign the job, since I was Secretary of the Telluride Association and must attend the Telluride Convention at exactly that time (incidentally,to make matters worse, the Convention was at Deep Springs).

But with astounding good luck, a second letter invited me to a much better job for the summer in the Development Department (apparatus development, not film development). I raced back from Deep Springs to start as soon as possible, and the summer was a great learning experience (and incidentally one of the three times in my life I worked only a 40-hour week, and never did I have all my evenings free).

I learned much physics from my boss, Steve MacNeill, and much mechanical design from Fordyce Tuttle, but even more valuable was learning to work within an organization, to make effective use of highly capable technicians, machinists, secretaries, and others.

Kodak offered me a job again the next summer, but by the spring of 1941 it was clear that a war was coming, and I learned that Bell Telephone Laboratories was heavily involved in electron tube development for secret applications (radar, as it turned out). I was delighted to get a job at BTL, which was then in Manhattan (Murray Hill had not yet been built). Learning was even more intense than at Kodak, with 6+ day weeks and working for and with J. O. Mc Nally, Jerry Shepherd, and especially J. R. Pierce, the most ingenious engineer I have known.

Both jobs tutored me to understand what a fully supported scientist or engineer needs and how to get it. The standard of machinist support, for example, was far above Cornell's. Many years later, in 1960-63, when we were organizing the Materials Science Center and planning Clark Hall, I made good use of this instruction in standards of support. Meanwhile, the summer jobs had given me a healthy respect for applied problems, an appreciation of the benign working conditions in at least some industrial companies, and an introduction to the management of technology-based companies.

5 Trembling

I entered Cornell as a transfer student in September 1938. I launched a physics major, and its requirements plus the college's "distribution" requirements called for five courses instead of the usual four. In addition, I had read much of the work of the great Carl Becker, and I was determined to take his course in modern European history even though that made six in the spring semester.

It was then the practice in that course (as in most at Cornell) to set an hour-long examination about half way through the semester. The crowd that arrived for the exam was about twice or three times the size of the class of students who attended the lectures; I was astounded at the size of the group who had chosen to miss one of the greatest lecturers they could experience.

The examination room was the main lecture theater in Boardman Hall, a building that was originally the Law School and that was later (in 1960) torn down. A distinguishing feature was the presence of massive columns, required to support the upper floors but restricting viewing from seat to seat. .

The proctor, evidently a graduate student, gave each of us a "blue book," the blank book for our answers, and a sheet of questions. I quickly read the two questions, each for half of the score. The first question was: "In what way were the circumstances at the founding of the Third French Republic more auspicious than at the founding of the First?"

I sat there, trembling. With an open-ended question like this, how was I going to compete with these students from Eastern high schools and prep schools, with two or three years of Cornell experience, and with confidence so great that they had not felt it necessary to attend the lectures? My own credentials looked good on paper, but they were from a miserable high school and a tiny, strange, and little known junior college. I was so frightened that I could not put pen to paper. Then a student must have raised his hand (he was behind a pillar and I could not see him) and asked "Sir, what does 'auspicious' mean?"

I was off and running, with instant return of confidence. I remain indebted to that student, whom I never saw, who quite possibly saved my fledgling academic career.

8 Radar

It has appropriately been said that "Radar won World War II; the atomic bomb ended it." I cannot claim to have helped much. Only my summer job at BTL in 1941 was early enough to have appreciable impact; I processed 40 or 50 microwave "McNally Tubes," 10 cm reflex klystrons, and the last ones were more powerful and more reliable than the first. These tubes were the heart of radar receivers being developed at the Radiation Laboratory at MIT.

My thesis research may have helped microwave magnetron development, but only after 1942. Almost all of my RCA work was too late to matter.

9 Princeton

There is little to say here except to report miserable housing. And that would seem to be whining since it was still enormously better than fighting men suffered. For several years I taught evenings at Princeton, mostly to Marines. I also taught a classified course in microwave techniques, evenings in Camden.

The high point in our Princeton experience may have been when Mary reported that Einstein had opened the door for her. They both worked at the Institute for Advanced Study, which housed several League of Nations programs during the War.

I learned much about facilities and support people. Later, in planning

Cornell developments, this was helpful in two ways: It taught me how to balance funding of research with funding for research support like machine shops and glassblowing . It also gave me some standing in the inevitable disagreements with architects and building engineers when directing building construction or modification.

I also learned a little bit about corporate strategy. The Director of RCA Laboratories gave a speech to licensees of RCA patents in which he said: "RCA is preeminent in electron tubes {true} and nothing will ever replace the electron tube {spectacularly false}." At least a score of physicists in his laboratory had been following the relevant technical literature and were confident that solid-state devices would be developed that would take over the functions of electron tubes. No one knew exactly what materials and what processes would emerge first, but prewar articles in both the German and American journals gave generous hints.

The Lab Director had only the most formal interactions with his staff; if he had joined our lunch tables from time to time he would not have been so foolish, and his company would have been better prepared for overwhelming change.

I witnessed this isolation of the executive suite again in the Xerox Corporation many years later, with even more damaging consequences. I believe good management requires mixing of people at all levels; the lunchroom is one way but there are other informal settings.

10 Responsibility

This is the second mention of what will be a recurring theme. One of the principal features of Deep Springs is the maturing effect of accepting responsibility, and participation in Telluride House and Telluride Custodians continues and deepens this. Accepting responsibility is essential if you are to be effective in any of the three environments (government, universities, and industry) and also if you are to be an independent professional (doctor, lawyer, etc.).

But responsibility injects a fundamental asymmetry into the negotiation of policy decisions and their portrayal in the media. The person with responsibility is constrained in actions and in speaking out while his or her antagonist "on the outside" enjoys almost no constraints. You have probably noticed this in the DSSB deliberations. It is worthwhile to examine it in any media presentation of a controversial subject. I do not mean that the person "with the monkey on his back" is always "right" but only that in reading or listening to the public accounts, one should recognize the constraints and try to construct a balanced portrayal.

11 Connections

In pre-WW II science, interdisciplinary programs were rare and often weak. Home economics is an example of an interdisciplinary program, based usually on weak disciplines, which gave such programs a bad name. Nevertheless I was convinced that I needed almost as much chemistry as physics in my research and I chose physical chemistry as a minor both as an undergraduate and as a graduate student.

In the post-War period, the connections among the sciences have become more prominent. And with Crick, Watson, and Franklin and the rise of DNA, much pure physics is less important than ever and hyphenated physics (with biology usually the more prominent player) has become most promising. Cornell is spending $100 million on a building that will be bio-everything; Rochester is already bio- a lot and now is expanding into opto-everything.

12 Accounting

Here we see again the importance of accounting. Although the official, audited university accounting dominates the scene, the unofficial (often back-of-the-envelope) accounting by the investigator is what counts for management, since it is more up-to-date and includes future commitments.

13 ONR

Toward the end of World War II, some far-sighted people in the Federal Government recognized the contributions that university-based science and engineering had made and engineered a basic policy decision: Federal support of research would henceforth not only be in Federal laboratories but also would be in universities, bringing along new generations of researchers. The decision could have been otherwise, Federal contract and grant money could have gone only to Armed Services labs or only to industry. This was a policy decision with the most major consequences and was made at a level below that of presidential appointees. The instigator and the first manifestation of this policy was the Office of Naval Research, a prime example of leadership.

See p. 4 of my Annual Reviews article

Lloyd Smith and I had one of the first ONR contracts, in 1946. One feature of our contract was that we contracted to do work *such as* some specified research topics. I believe our contract was the first Federal contract to use such language, rather than to specify what was to be done. But "Success has a thousand fathers; failure is an orphan."

14A Mansfield Amendment

Section 203 of the 1970 Defense Appropriations Act: "None of the funds authorized to be appropriated by this act may be used to carry out any research project or study unless such project or study has a direct and apparent relationship to a specific military function or operation."

15 Book

Much later, at Rochester, the Dean of Arts and Sciences invited me to speak each year to new faculty (mostly young assistant professors). One of the points I made was that everyone should have some other vital interest or mini-career in addition to his or her university position. Although that second career would have its frustrations too, they would not usually coincide with the inevitable reverses and frustrations that one faces in research and scholarly writing.

For me, that second line of work was the textbook *Modern Physics*. (Actually, I had even additional lines in consulting and Government advising.) It was fortunate and necessary that I had a supportive family since this "second line" was necessarily prosecuted evenings and weekends.

Course   Nights and airplanes   Gøteborg

16 Directors

We will deal more thoroughly with this subject later, when discussing the industry environment.

Meanwhile, I should note that service on that board was very satisfying. I was the only Wiley author or teacher on the board, and I had close working relations with almost every department of the company. I could therefore serve from a strong basis to monitor the quality of people and product, a board responsibility.

As a learning experience, I got to know lawyers, bankers, and business executives on the board. Up until then, I had had little respect for such, but I found that was mainly my ignorance, that there were able,thoughtful, and conscientious people who were not authors, researchers, or teachers.

The Wiley family and their various trusts owned more than half of the stock and could have acted like the sole owners. In fact, although Brad Wiley was Chairman of the Board, the Directors ran the company. In thinking about corporate governance, I resolved that I would not become a director of a company unless I owned enough stock to be seriously hurt if the company did not flourish. This was difficult for me to do, since I had to find the cash to buy stock at that time with a mortgaged house and a young family, but the "stretch" to buy Wiley stock turned out to be one of my most successful investments.

17 Committees

One of the principal ways in which the Federal Government harvests the investment it makes in research in universities is to get advice from the investigators. Some of this is by retaining consultants but most of it is through advisory committees. I started with the ONR's Solid State Sciences Advisory Committee when it was initiated in about 1949 and have served on many committees since then. Listing them is like alphabet soup: AEC, AFOSR, NRAC, ARPA, DoE, NRC, IDA, NSF, and probably others. There is much cynicism about such participation, but it probably does contribute to a more intelligent governance of the Federal Government.

Much of this service, always without pay, was for administrations of which I did not approve. But I firmly believe it is your duty to help, even if you voted for the other guy; it is less fun, but only fair, to let the voters decide for whom you work voluntarily. "You have only one President at a time."

[However, in the industry context, I thought differently. At the end of the War it became apparent that if I stayed at RCA Laboratories I would be working for many years on television, and I had seen enough of it during the War to know that I wanted no part of it. I did not feel an obligation to making RCA flourish like my obligation to help make the Federal Government flourish]

It may take you some years before your advice is sought at this relatively high level. Meanwhile, however, there are other ways to serve the public. Many professional societies (like the American Chemical Society) sponsor "Washington Fellows," young people who spend a year or two attached to a Congressperson or agency head. There are similar programs in many state governments. I strongly recommend such service. It may very well lead to new opportunities. And at the very least you will read the newspapers differently the rest of your life.

On campus, a continuing source of tension is the circumstance that there is essentially zero Federal support for the humanities and minimal for the social sciences. Usually, the professors in these areas accuse the university administrations of favoritism of engineering and the sciences. Far from it, Federal funds in these areas help to mow the grass and heat the library. Faculty who are principal investigators frequently work hard on committees and thereby become respected in Washington. Some of this respect is converted to research support.

When I went to Rochester, the famous Kant scholar Lewis Beck came to see me. I was delighted to listen and talk, but what he wanted to do was to boast about being appointed to the selection committee for fellowships by the National Endowment for the Humanities. "And of course I turned it down, Mickey Mouse stuff." He had passed up a rare opportunity to help and probably to benefit.

The dynamics of committee operation has fascinated me for nearly sixty years. I cannot possibly say much here. But I can relate the famous account of committee decision making by Warren Weaver, then President of the Rockefeller Foundation. And I will now quote a passage from *Swan Song*, the sixth volume in Galsworthy's *Forsyte Saga:*

"In an Age governed almost exclusively by Committees, Michael knew fairly well what Committees were governed by. A Committee must not meet too soon after food, for then the Committee would sleep, nor too soon before food, because then the Committee would be excitable. The Committeemen should be allowed to say what they liked, without direction, until each was tired of hearing the others say it. But there must be some one present, preferably the Chairman, who said little, thought more, and could be relied on to be awake when the moment was reached, whereupon a middle policy voiced by him to exhausted receivers, would probably be adopted."

18 National Laboratories

Oak Ridge National Laboratory is one of the four laboratories that were created during the War to make atomic bombs. Their missions and management are interesting subjects, but I will have to leave them as subjects for other times and places.

My brief time at Oak Ridge was purely enjoyable research, since I had no responsibility. Much later, I became Chairman of the Laboratory Management Council, with much responsibility and worry.

19 Brussels

The decision to spend a year in Brussels was taking a major risk, but we lucked out. Our children were in a French language school (Nancy's teacher knew not a word of English) and the work was more advanced than American schools for their ages. Mary helped them evenings with French and I with "calcul." My graduate students had promised to finish their theses by the time we left the U. S., but of course they had not, and so I spent many evenings on correspondence with them. But I took Saturdays and Sundays off for only the third time in my life, and we went on excursions almost every weekend.

This was the winter of 1958-59, one year after Sputnik and at the height of Senator McCarthy's wickedness. Defending the United States was not easy, but we did our best.

20 Management

One decision in the midst of all this effort is a good illustration of research management.

I was raising funds for equipment, usually piecing funds together from several sources in order to get a major instrument. It was hard to put together $75,000 for an electron microscope but I ruled that we would not buy one but would work at fund-raising until ws got twice that sum together and buy two. I was convinced that we needed a professional electron microscopist and that he or she would insist on having an instrument that could be modified; it would frequently be "down," and so we needed another machine that would not be tampered with but would be made available (with instruction) for use by others in our laboratory.

A popular point of view, peddled especially by business schools, is the so-called "universal manager" approach which claims that if you are trained in management it does not matter whether you are familiar with the products and processes you are managing. But I do not think a universal manager would ever have insisted on two instruments, which turned out to be just right.

21 Students

In the period up to Sputnik (October 1957) a great amount of Federal grants and contracts had supported thesis research, but educating students was not considered part of the "output" of the grants and contracts. We principal investigators and the Washington program managers knew that this was the major output, but we carefully omitted it from proposals and reports because of Congressional nervousness about the "missions" of the supporting agencies, which were not education.

In 1958 and 1959, student support "came out of the closet" through the leadership of some very able people (Herbert York, Harold Brown, Donald Stevens, and others) and the strong position they occupied because of Sputnik. Thus at the time of the ARPA competition for the materials laboratories one could take credit for students, and, to ARPA's credit, it made them a major consideration in the selection of "winners."

This helped us at Cornell in the competition, since we knew where all of our Ph.D.'s were, what they were doing, and how they were making use of their educations. This was a major reason Cornell received one of the first and the largest of the materials laboratories contracts. This large umbrella contract permitted management at the local level. One result was the support of Bob Richardson's work for which he got a Nobel Prize; I believe he had been turned down by NSF and several other agencies in Washington.

22 Clark Hall

Although the solid-state groups in Physics and Engineering Physics were the leaders, both in getting support for and in using the building, there were a least four other units which had to be accommodated into the building. They, like Physics, had been promised new buildings, but no new research construction had occurred since the War (except for the quick, small, and cheap Newman Laboratory for the nuclear physicists).

No one asked me to do it, which is an interesting example of leadership and management in a university setting. But I became the client for the building, to work with the architects and engineers to represent not only the immediate users of the building but also (in as far as possible) the users in subsequent generations. I toured research facilities throughout the country, extensively photographed the best ideas, and shared my observations and tentative responses to their needs with the users. There was constant tension with the traditional engineers (J. Fruchtbaum of Buffalo) and the Cornell buildings department, but there was also imagination, understanding, and sympathy from Charles Warner of the architectural firm Warner, Burns, Toan, and Lunde. The modular basement and the utility corridor system were unique in academic settings and have been much admired and copied. Forty years later, the building is still serving well after only a single renovation.

23 Accounting

This accounting is almost trivial. But I mention it here to explain that it is accounting for *management*. That is, although it may be only approximate,it is timely and even anticipates future commitments. This is distinguished from accounting for proof of what happened, performed by the central administration, which is too late to use for management.

The recent scandals of corporate accounting highlight the importance–indeed the necessity–of outside directors all of whom understand more than a minimum of corporate accounting. The typical corporate board has three kinds of directors : 1.) Insiders. 2.) Outsiders who are CEO's of other companies. 3.) Outsiders who provide some "diversity" (women, African-Americans, college presidents). If those of class 2.) are too chummy with those of class 1.), the burden falls heavily on those of class 3.) to challenge the administration's manipulations. This is difficult at best but it is impossible without substantial knowledge of accounting.

24 Responsibility

It is sobering enough to have to take responsibility for your own actions but as you begin to have larger scope it becomes necessary to accept responsibility for the actions or inactions of others. Doing this graciously is hard enough, but you must do it cheerfully. If the offending member of your staff is worth having, he or she will not only be more responsible in the future but will even be more loyal. Conversely, if you reverse decisions or approaches by one of your staff, that in effect removes his or her responsibility, and you have lost that person's effectiveness.

14 Federal support of research in universities.

From time to time and at virtually every major university there has been tension or even confrontation about the support of research on campus by Federal agencies. The spectrum has extended from the CIA (highly suspect) to the NIH (usually considered benign except for laboratory animals).

Part of the argument is that it is alleged that military agencies direct the research. I believe my own experience is typical. I have never known any direction or interference or limitation or delay of publication with the work of my students or myself. Early in my research I was supported by the ONR. When I later had NSF grants, the administration of them was indistinguishable from that of ONR.

The other principal part of the argument is that helping the military is immoral. That leads us into very deep water indeed. My own position, called a "cop-out" by many young people, is that in a democracy one does not always have his way, and I will help whatever government is in power at the moment. That is not very satisfying, especially today, but it has a firmer basis than refusing to pay taxes.

25 Wheedling

The decision-making process in a university is not as simple as most students think. Decisions are made in different ways at different heights in the organization. One aim of management (in all three environments) usually is to have the decision at the lowest level possible. The primary reason for this is that the knowledge on which a decision is based is more likely to be there than higher up.

Students think in terms of "power," and complain because so much power is in the hands of the administration. Many students' concept of the president's office is that it is like the control room of a television network or a nuclear power station, with scores of meters, switches, and knobs, and they would like to turn those knobs.

In fact, what the president has is not so much power as influence. Most of what he or she accomplishes in giving direction to the institution is done by wheedling, by persuading others, mostly faculty, that new programs or directions are sensible and in the interest of the players. Even though the president has the power of the purse, to use it without acceptance by the community would backfire, and so even that power is more like influence. The president's activity is thus more nearly leadership than management.

Once when I had gone to Hans Bethe for advice about taking on a Cornell directorship, he told me that the higher one goes in a university, the more wheedling is required to make things happen. I learned that, as usual, he was both right and perceptive. (Hans himself was much too bright to have become an administrator.) I learned that when the depredations of academic politics became unbearable, a rereading of F. M. Cornford's little gem, *Microcosmographia Academica,* would bring back steadiness, tranquillity, and even smiles.

26 Kennedy

I was not a presidential appointee (my boss was), but I definitely enjoyed my job more because I was sympathetic to Kennedy. But I believe that if I had been recruited under a Johnson (or even, horrors, a

Nixon) administration, I would have followed my advice (17 above) and agreed to serve.

27 ARPA Strength

I learned what made ARPA such a lively and vital operation, although I would still never have predicted its remarkable vitality, still going strong after 45 years. First, the directors and assistant directors have all served for very short terms; the average of directors has been about two years, which is also the time I served. This pattern produces constant rejuvenation and renewed introspection of what are the most interesting problems and most promising approaches. Second, ARPA was **expected** by the Secretary and his deputy to be such an agency and treated as such. You may remember that in Shaw's original *Pygmalion,* and repeated in *My Fair Lady,* Eliza says "the difference between a lady and a flower girl is . . . how she's **treated**."

Unfortunately, no other Federal Agency is like ARPA.

ARPA had power beyond its budget to influence other agencies and programs; its leadership went beyond its scope as management. Part of this came about because of our flexibility: We could add a little money to solve problems in a program in one of the services. Part came because we could open channels for the Armed Service agencies to the scientific and engineering community; contractors and potential contractors visited us and we visited them. We established the practice that, no matter how hungry or even greedy a visitor was, there was **always** something useful to be learned from him, usually about interesting things going on in his shop. (This contrasted sharply with the experience I had had of visits to NSF, where the program managers dominated the sessions with whining about their budgets and their file-drawers full of proposals and never learned anything interesting that was going on in the field.)

28 ARPA Program I

The first was nuclear test detection, which had been my principal interest in joining ARPA. In the summer of 1963 negotiations with the USSR for a nuclear test ban were in the final stages, and I was commuting to Washington to participate; my predecessor had left in May. ARPA had two roles: 1.) We were harvesting the work that had been done mostly at the National Laboratories and the Air Force, much with ARPA support, to convince the Joint Chiefs of Staff that they could testify in September and October Senate hearings that they could assure the safety of the country in a test-ban environment. 2.) We were heavily involved in the attempt to include in the treaty underground tests, the fourth environment, where the identification of nuclear explosions and distinguishing them from earthquakes was still in a primitive stage, and the argument was heated over how many on-sight inspections would be needed. The activity we helped in our first role was successful, and the three-element test ban, atmosphere, oceans, and space, was signed and ratified; it was the first glimmer of hope in the Cold War. The second failed because ultimately the decision did not rest on science and technology; neither side really wanted to give up testing, and so the comprehensive test ban was deferred for over two decades. This is an example of a frequently occurring pattern:: Politics often trumps technology.

The ARPA program continued with research on underground explosions and with the VELA satellites to explore space-based detection of nuclear tests in the atmosphere. Although the satellites were never intended to be an operating system, they endured far beyond their expected life and did in fact detect a clandestine South African test; politics,, however, obscured that success. Another example where politics trumps science or technology.

Amchitka: A. National Forest Juneau to NTS cf to A.
ARPA staff going-away book: Potomac River
STAR Kermit Gordon

29 ARPA Program II

The other ARPA program I shall mention was initially a much smaller program called "Command and Control Research." ARPA had negotiated a mission to develop computer aids to battlefield command and control. Since all three of the Armed Services were possible beneficiaries and since it required close contact with the science and technology communities, it was a natural ARPA program. The real genius in charge of the program was J. C. R. Licklider, who, incidentally, was a Ph.D. in psychology from the University of Rochester.

When I arrived in ARPA the program was on the chopping block for cancelation, since I was obligated to cut $15 or $20 million out of the budget and this program did not fit securely into the total ARPA plan. But Lick convinced me that the program should not only be kept but should be expanded and become more general. Here was a case of pulling fundamental, researchable questions out of an initially applied problem; this is just the process I described earlier, but here on a grand scale. We got permission from the relevant Congressional committees to change the name to Information Processing Technology, and we broadened the program to explore a wide range of computer possibilities that were germinating in universities, in a few small companies, and in Licklider's mind. Among these were time-sharing, networking, natural language articulation with the computer, teleconferencing, and computer graphics. I am sure all of these developments would eventually have happened without ARPA attention. But the bulk of the computer industry ("Big Blue"), was not interested, and ARPA must have accelerated by many years the computer uses and methods that we now take for granted. The program, only marginally related to defense, clearly has strengthened the country more than many much more expensive weapons systems.

This development is also an example of the distinction between direction and leadership. Lick *directed* a small program within ARPA, but he *led* an industry, personal computing. He did this by visits, lectures, research papers, and training assistants. This leadership had far wider and deeper consequences than the ARPA money.

29A Federal Government

I include here two short essays on the way the Federal Government works. These are essentially less important footnotes in an analysis of the Government; of course the more important branch of political science pertains to the election of a President and members of Congress, but I have had no experience with that. Earlier Withrow lecturers, notably Conable and Van den Heuvel, must have provided great insight and accounts of personal experiences to earlier audiences here. (I recommend *Window on Congress, A Congressional Biography of Barber B. Conable, Jr.* which was published a few months after Barber's death a year ago.)

To appreciate fully my reporting here you should have watched "Yes, Minister" on PBS some years ago. That British series was about the way the Minister (whose tenure was only as long as his party dominated Parliament) was "guided" (actually, of course, managed and bamboozled) by the Permanent Undersecretary (superbly played by Nigel Hawthorne).

The pattern is little different in the U. S. Government. The officials right up to, but not including, presidential appointees, have over time an enormous impact on programs and directions. Of course Congress and the President dominate the big decisions, but in the continuing operation of the Executive Branch, it is these people, not the Secretaries, who call the shots.

It is well known that status dominates the behavior of these people, but until I went to ARPA I had not realized how thoroughly and openly that was true. I rated a secretary whose own rating was at the very top of civil service secretaries. Mrs. Erma Tucker was excellent, if perhaps unimaginative and completely lacking in humor. We got along fine, but she never approved of me; I would do weird things like sometimes reading my mail before she had read it and had it thoroughly vetted by staff.

Erma took charge of my introduction to Federal rules on everything. During the summer of 1963 I had made several trips to ARPA; I could not immediately disconnect myself from Cornell but I needed to familiarize myself with the nuclear test detection debate. On one of these in July I drove one of our two cars and left it with friends in Alexandria. Then in late August Mary drove our other car while son Bob and I arrived by a sailboat on a two-week cruise from Ithaca. I filled out the proper form (which Erma had given me) with all this information, explaining that I had already been paid for the car trip in July and that I realized that the Government would pay nothing for the boat trip. I submitted it to Erma, who said "Yes, sir." In about two weeks I asked about it, explaining that we were short of money because of our move and paying for moving furniture. She said that I must be patient, that things took longer in the Government. I asked again in about a month, and then, at two months, I said I must have done something wrong. She said "Well, Dr. Sproull, I really must ask you how you came to Washington." I replied, "Erma, it is just as I told you and put on the form, I came by boat." She said, "You cannot expect me to take that to the little snips in Travel; they will know you are pulling my leg." The form was still on her desk, from which it had not moved! Status!

ARPA and its programs were subject to attack, usually envious, almost every day, at all levels. In choosing members of my staff for the inevitable meetings, I honored the dominant role of status: "Never send a priest to argue with a bishop."

The other footnote in this mini-lecture on political science is concerned with Congress. It would be instructive to follow a request for an annual appropriation as it weaves its way first through the shaping and review process internal to the unit (such a ARPA) of the Executive Branch that it will fund, but I will not do that. Instead I will pick up the process as the request is delivered to Capitol Hill, using the labeling of committees that was in effect in 1964.

The request must first be "authorized," and for this it goes to the Armed Services Committee which in due course schedules hearings. The printed version of these hearings for Defense runs to several thousand pages. The conclusion is the "Committee Report" which says not only what the unit may spend but also contains many "you must do this" and "you may not do that" restrictions (called "language"). The request is then carved up to obey the House action, since nothing can be appropriated unless it has been authorized, and is next sent to the Defense Appropriations Subcommittee of the House Appropriations Committee. Again, hearings are held, and a Report ensues with its own restrictions. Then the whole process is repeated in the Senate.

In all this, the Congressional staffs of each committee play the central role. My staff talked with their staffs, even though (status!) we were supposed always to go through the Assistant Secretary of Defense for Legislative Affairs. We were blessed with the work of Ralph Preston, the senior staff person on HASC. He was respected by the staffs of the other committees, and staffs worked together to iron out any glitches in this complicated process.

When the appropriation was finally enacted, my staff evaluated my performance by the amount of funding and the presence or absence of damaging language in the committee reports. I, of course, was sensitive to evaluation by these criteria. But in addition I sought *understanding* by the committees and especially by their staffs. If our programs were well understood they were less fragile in the face of the mistakes, changes, and disasters that inevitably accompany an experimental program. If a problem or an opportunity arose we relied heavily on this understanding as we negotiated with the committee staff for release from restrictions or changes in the distribution of funds.

The complexity of Congressional committees is enough of a problem during ordinary years, but it becomes potentially dangerous when one is trying to transfer a program from one agency to another. For example, we started in 1964 the transfer of the support of the 125 worldwide seismic stations to the Coast and Geodetic Survey in the Department of Commerce. But at least six committees claimed jurisdiction over this program, and a committee is reluctant to give up a program since the committee's importance is proportional to the size of its programs. Transfer is simple only if the purpose is to kill the program. This transfer was eventually effected in 1970, but it took the patience of Dr. Charles Bates, three successive ARPA Directors, and extensive staff-to-staff interaction.

During the years I was involved, Congressional staffs were the fastest growing segment of the U . S. economy. As you have seen, this has both a good and a bad side. Among the "bad" features is the circumstance that as staffs take over more of the functions of the Executive Branch, accountability becomes fuzzy or lost: If Congress is doing much of the managing, as well as deciding what is to be managed, who is responsible for success or failure?

30 Press

At ARPA I was very nervous about the press. I had a press officer, but his approach was to say as little as possible and his lack of technical background and lack of a high position in the Agency meant that reporters wanted to talk with me, not with him. Most of my interviews resulted as you would expect, in confused accounts replete with errors and voids. But one was different. After my first meeting with Howard Simon of the *Washington Post* he called me at home in the early evening and read his story to me. This began a pattern. Each time we talked he would call and read his draft. The rules were obvious but neither of us ever stated them:: I could ask for a change to fill in, to explain, or to correct, but not to make me or the Agency look better. The resulting stories served the readers far better than the usual journalism. (Simon was rapidly promoted to Editor of the *Post*.)

31 Alexis

A particularly bitter example of curtailing free speech occurred at Rochester during a sit-in by undergraduates in the Faculty Club. They were protesting the paucity of black faculty. The Dean of Arts and Sciences arranged a meeting of department chairmen that was open to everyone, including people with no connection to the University. Each chairman told what he or she was doing to get black faculty. In the midst of this, Marcus Alexis, a black professor in the Business School, blurted out: "They are all lying. If people were making that kind of effort I would have many offers and I have had none." Well, he had been in my office twice in the preceding weeks to threaten acceptance of an offer from Northwestern unless I added outrageously to the already high salary that his dean had offered to try to keep him. If I had reported that at the meeting, the campus would probably have exploded. I had nothing resembling free speech. This incident was also another example of the asymmetry between the person with responsibility and the person without.

32 Palomares

In January 1966 an incident occurred that made demands on the **breadth** of my education. I was asked to chair the Search Evaluation Committee that was organized to oversee the search for a hydrogen bomb which the Air Force had lost over the Mediterranean coast of Spain. The refueling boom of a tanker, probably because of violent turbulence, had driven through the bomb bay of the B-52 being refueled. Three of the four live bombs had floated down under their parachutes and had been recovered, but several weeks of search had not found the fourth.

I believed I was chosen because I still "had all the tickets," clearances from my recent ARPA duty, and because I was expendable; the Secretary was certain there would be a bruising Congressional inquiry if, as seemed likely, the bomb was not found, and anyone who had to explain to Congress that the search was being abandoned would be a sacrificial lamb. Leaving a hydrogen bomb in Spain would create a host of dangers.

Search priorities and evaluations required knowledge of bomb physics, material physics and chemistry, metallurgy, corrosion chemistry, meteorology, photographic science, aerodynamics, aeronautical science and engineering, radiation biology, geology, oceanography, law and political science, and even agriculture and animal husbandry. In the end, it was psychology that was most important. We tested many witnesses and, against much conflicting advice, placed our bets on a single fisherman because of his competence as revealed by some simple tests. The Navy followed his directions and found the bomb, hanging by its parachute shrouds in water nearly as deep (2000 feet) as could be explored, at the edge of a much deeper chasm. Again, good luck!

Talk to Pundit + Fortnightly 1974

Tonight I am going to tell the story of a remarkable invention. More than the case of perhaps most inventions, we know how this one happened. We have in the inventor's own words a chronicle of how he happened to do and think what he did and thought, or at least how he thought he happened to do and think it. The invention is interesting for another reason: Although it involves a wholly different area of human activity, it is structurally like a recent management invention at the University of Rochester, and at the end of this talk I shall return briefly to this similarity.

The invention I am going to discuss tonight is the Sumner Line. In order to explain it and how it happened to be invented, I must first give you the setting of celestial navigation in the early 19th century. The science and practice of celestial navigation had been in a very rudimentary state until about 1730 when the basic device that developed into the modern sextant was invented simultaneously by Thomas Godfrey in Philadelphia and by John Hadley in England. As it turned out, Sir Isaac Newton had also invented it, 30 years earlier, and had sent his manuscript to Halley, President of the Royal Society, who for some reason or other kept it among undisclosed papers—it was not discovered until after Halley's death in 1742, 15 years after Newton died. With the sextant, it was easy to learn one's latitude at sea as I will explain in a moment. Longitude was a harder matter, since the same observations on the sun or a star that are made at a certain longitude can be made at an infinite variety of other longitudes if the observations are made at different times. Thus the key to solving the longitude problem was an accurate clock, and it was not until about 1770 that Harrison's chronometer, itself a brilliant invention, became available in useful quantities. About 1837, the chronometer was still very expensive, but one was to be found on every ship in intercontinental trade.

With these two devices, it was possible for a reasonably skillful navigator to keep track of his ship's position anywhere in the world. (Note I do not say find his position!) The way he did this was as follows:

The first and most basic measurement was the "noon sight for latitude." A little before noon, the navigator would go on deck with his sextant. He would "bring the sun down" until it just "kissed" the horizon and read off on his instrument the altitude, that is to say the angle of the sun above the horizon. A few minutes later, he would make a similar measurement and would find the sun to be a little higher. He would watch and carefully get the maximum altitude of the sun in this fashion. Working up his data to give latitude was really very easy. There were a few small corrections to apply before he went into tables of the declination of the sun, given for each day of the year. Then from these tables and the observed altitude he could read off his latitude. I think I can show you on this globe why that should be possible:

This noon sight for latitude had been unchanged for hundreds of years, except for the fact that the instruments and tables had gradually become more accurate and more convenient. Note that the navigator needed neither time nor longitude in order to find his latitude, although of course a crude clock was helpful to tell him what time to go on deck.

Toward the late afternoon, an hour or so before sunset, the navigator would then take his "time sight" or "longitude sight." He would observe the altitude of the sun when it was just a few degrees above the western horizon; he did not use actual sunset because of the large and largely unknown correction for refraction that must be inserted when the sun is near the horizon and its light comes through a great distance of atmosphere of unknown moisture content.

The late afternoon and early morning sights could then be worked up to give longitude, but with one centrally important proviso: It was necessary to know the latitude in order to do the calculation. I think I can show why this is so by looking at the globe; for this demonstration let us assume that the sight is taken at sunrise, that is, a sun altitude of zero. Although, as I have said, an altitude of 5^{o} or more would be used in practice to avoid refraction errors, the demonstration based on 0^{o} ought to convince you that a similar demonstration for (say) 5^{o} would apply. Now let us look at the globe and note the "terminator," the curve that separates the sun-up from the sun-down parts of the earth. At each point on this line, an observation of the sun's altitude at this time would give the same altitude (in our case, 0^{o}). Thus, from this altitude measurement one cannot tell his longitude without knowing his latitude. Note that the longitude error from an imperfectly guessed latitude becomes greater in high latitudes.

Note that this longitude sight contrasts sharply with the situation for the noon sight for latitude, where no knowledge of longitude was needed. Note also that the precision with which one needed to know the latitude depended on the time of the year and the latitude itself. In the tropics, for example, the precision was not large, especially at times of the year near the equinoxes. In any case, the practice was to use the "dead reckoning" of the ship since its last position was known to estimate the latitude. The dead reckoning came from a relatively crude speed indicator and a somewhat more precise compass reading of the ship's direction. Unfortunately, dead reckoning could be seriously in error because of currents, wave action, compass errors, and leeway.

But under good conditions, such as sailing across the central Pacific in good weather, this morning-noon-and-night sequence of observations provided a three-times-a-day trimming up of the dead reckoning, so that errors did not accumulate in it. It was the standard way of navigating a ship at sea. It obviously, however, led to problems in bad weather and near shores. Also, we might note here, well ahead of our story, that it was wholly unsuitable for air navigation; even though a special sextant with an artificial horizon could be and was developed, an airplane could not survive on three readings a day, none of which was a real "fix" but only a reading of latitude or longitude.

But this was the situation in December of 1837 when Captain Thomas Hubbard Sumner was approaching the British Isles under rather frightening conditions. He had sailed from Charleston, South Carolina, on the 25th of November bound for the Clydebank in Scotland. After passing the Azores, the weather (which had been bad over the whole journey) turned worse, even though the weather was "good" from the standpoint of a fast passage, since gale force southerly winds enabled him to make rapid progress to the east and northeast. But he found himself on the evening of 17 December in a dangerous situation. The last sights he had been able to make were in longitude 21 degrees West. He had sailed for three days and now estimated from his dead reckoning that his position was about 30 or 40 miles from either of two sets of rocks, both terribly forbidding.

He could not go on, and so about midnight started tacking back and forth trying to keep his position. As you know, a square-rigged ship can hardly make way to weather, and he dared not heave-to because that would have meant slowly drifting down onto the southeast coast of Ireland. This must have been a rough night for all on board, since the wind continued to freshen out of the southeast and had developed into a full gale by morning. At any moment a frightened sailor could expect the lookout to shout "breakers," followed a few seconds later by the crashing of the ship onto rocks presumably with the loss of all hands.

But Sumner was an experienced and skillful master and managed to maintain his position in deep water through the threatening night. When it became light, the sun was still obscured, but at about 10:00 it momentarily was visible. Sumner quickly got a sight, and one can well believe that even though the conditions were difficult, as an experienced captain and navigator he managed to get a quite accurate one. On the other hand, he must have felt that it was scarcely worth doing the trigonometry to work up the sight, since his latitude was in such great doubt, dependent as it was upon guesses as to how the ship had moved in the last three days.

Nevertheless, he did work up the sight using his latitude from dead reckoning, and a quick calculation led to the position here on the chart.

But Sumner knew that the dead reckoning latitude error might give him a longitude error so great that relying on it could easily pile his ship on the rocks.

He next did an unusual, but probably not unexampled, thing: He assumed a latitude 10 minutes farther north than his dead reckoning, that is to say 10 nautical miles farther north, and recalculated the longitude. When he did this, he found that the new calculated position of the ship was 27 nautical miles east northeast of the first position that he had calculated. He then did an even more unusual thing, and perhaps no one had ever done this before: He tried again with a third latitude, this time 20 minutes north of the dead reckoning. This third calculation placed the ship still further east-northeast and another 27 nautical miles further from the first position. It was now apparent to Sumner that these three positions lay on a line.

The next step that Sumner made was the truly brilliant stroke: He realized that all such groups of measurements must lie on a line, since they all correspond to the same instant of time, the same position of the sun, and the same observed altitude of the sun. Again, I think we can see this best from the globe.

Now as it happened, this particular line lay east-northeast, almost exactly through Small's Light off the Welsh coast. The fact that it lay through the Light turned out to be a great convenience, although it was incidental to the brilliance of the discovery. What Sumner had discovered was that a single observation of the sun or a star led to fixing the ship's position absolutely on a line, without recourse to dead reckoning except in the grossest possible terms and without assuming a known latitude. Now he also quickly realized how such a line, even though one's position on it is uncertain, can be of vital utility in answering the only question that needs to be answered: "What should I do?" Previous navigators had been so mesmerized by what they thought was the need to know latitude and longitude that they had failed to realize that the only reason for navigation in the first place is to answer the question: "What do I do next?"

In Sumner's case, the answer was easy: Sail a course of east-northeast and keep a sharp look out for Small's Light. The light was sighted in less than an hour, the ship could then safely be turned to the north, and the danger was past. We do not have a record of what

Sumner's shipmates said or thought of their escape. It would be nice to think that at least one appreciated how a brilliant stroke of the mind had saved their lives, but of course most seamen thought of celestial navigation as something akin to witchcraft anyway and may not have found Sumner's invention remarkable.

Let us review the bidding: Latitude and longitude had been so thoroughly ingrained into earlier navigators that they failed to recognize that a single sight can yield a line of position. This failure is the more remarkable to us because the noon latitude sight was, of course, a special case of this in which the line of position was exactly east and west. But of course after a discovery of this kind, it will always seem queer that earlier people had not discovered it.

Once this basic discovery had been made, it is clear that a whole new method of navigation was available. One could use two stars at sunset, when the horizon was still visible, or the moon and a star, or the sun and the moon during daylight, or any other two heavenly bodies when the horizon was visible, as on moonlit nights. From each observed altitude and the chronometer time, he would get a line of position. These lines would always cross, and if the bodies had been selected appropriately, they would cross at a reasonably large angle. Where they crossed was, of course, a "fix," the position of the ship as determined entirely by celestial navigation. Furthermore, even in conditions where only a single body was visible, we have already seen that a line of position could be vitally useful.

Sumner's method was quickly exploited by the better navigators. A couple of decades later a French admiral Marcq St. Hilaire developed extremely simple ways of doing the calculations required to get a fix most accurately and most quickly. Later, the U. S. Hydrographic Office developed a whole series of computational aids and tables so that a fix could be obtained from a pair of observations by calculations occupying only four or five minutes, and even much of that work could be done in advance of the actual observations. Thus the groundwork was laid for celestial air navigation, without which the great air exploits of the 1920's and 1930's would have been impossible. These methods were also, of course, widely used in World War II for both ship and air navigation. Now, of course, the rapid advance of radio aids, inertial navigation systems, and even satellite-aided navigation mean that the days of the sextant and chronometer are numbered. I can tell you from my own experience, however, that it is still an exciting thing to stand on the deck of a small boat, take two observations, go below for a little bit of calculating, and as a result know one's position within a nautical mile or so on the vast, open ocean.

In conclusion, I promised to draw a parallel with a recent decision at the University of Rochester. For this purpose let me first review the structure of Sumner's analysis. He could have said "I can't know my longitude without knowing my latitude, and my latitude is known only so imperfectly that I am probably tens of miles in error on my longitude." This would presumably have led to throwing up his hands and sailing his ship onto the rocks. What he actually did instead was to note that he did not need latitude and longitude, but these were merely constructs of an arbitrary sort. What he needed was information that would tell him what to do. He could get, as he discovered, this information without ever knowing what his latitude and longitude were.

To me this is comparable to the now famous Wallis-Jensen-Meckling analysis of University expenditure policy. Other universities, of which Yale was the most widely known case, had spent years agonizing over how to define "income" from university endowments. Clearly income was distinct from cash flow, just the dividends and interest paid month-by-month and year-by-year, since stocks like Texas Instruments paid no dividends yet had steadily growing earnings. But just what "income" was was a matter of deep and widespread controversy. The essential contribution made by these three individuals now seems obvious to us, like the Sumner line. They noted that even if one had income defined for him, he would still need to know how much of that income (perhaps 70%, perhaps 130%) it was good policy to use in any one year. Income, like latitude and longitude, was simply a construct, not given in the statement of the problem and not needed in the statement of the solution. They therefore proposed to avoid defining it altogether and move directly from past experience with a portfolio and with other portfolios to various alternative spending policies. Again, as in Sumner's case, they vaulted over unnecessary constructs and moved directly to the question: What do I do now? It was a discovery which in its own way was comparable in brilliance to the discovery of the Sumner line. And you should note by the tone with which I have discussed that line that I conceive this to be high praise indeed.

RLS:vfc
13 December 1973

RLS Inaugural Speech
1 February 1975

I

I begin my service with deep feelings: First, of gratitude to the Board of Trustees, the faculty, and the other elements of the University for the confidence they have placed in me; second, of respect for the awesome responsibility of being the chief steward of the resources of the University, resources created by previous faculties, student bodies, and other friendly workers.

Tonight is only a step in a continuous transition. Chancellor Wallis is not leaving and I am not arriving. The ultimate responsibility of recommending to the Board actions, budgets, plans, and appointments now changes hands; but I have been sharing to a growing extent this responsibility and Chancellor Wallis' share will be no means disappear. My personal goal is to secure as much of his participation as possible, with especial responsibility for the creation of a unique school of law.

The ceremony this evening therefore punctuates the transition process with something between a comma and a semicolon, not at all with a full stop. Yet it is nevertheless appropriate that we take stock of where we are and where we are going, and that I state publicly my intentions and aspirations for the University in the next decade and beyond.

I pledge to you that strong leadership of the University of Rochester will continue. I wish I could promise that it will be as strong as in the last dozen years; I can only promise to do my best. A gradual change in manner of leadership has been occurring in recent years, and this change will continue; the leadership—by direction, management, and example—is being shared more deeply in this University. I call this a "resilient" strength, in which the various levels and parts contribute, rather than a more "brittle" strength, in which if the key man were to stumble the whole enterprise could fracture. Fortunately, as we know, he never did. Do not worry: I do not propose to substitute for central strength an intricate participatory democracy in which everyone is happy but the institution is not facing its problems and opportunities, is "not going anywhere." There will be strength in depth, but the strength will come to a focus in the President, and I do not shrink from the leadership responsibility.

II

Where we are in American higher education is easy enough to state, but the statement is enough to make the strongest people cry. Universities are being battered by a preposterous assortment of damaging blows.

First there is the inflation, much worse of late, hitting especially hard at institutions like universities where personal services consume most of the budget. Productivity increases in advanced teaching, laboratories, and supervision of graduate student research have occurred, through the better use of libraries, more powerful research equipment, and the introduction of the computer. But these have been swallowed up in doing a better job; an upperclass course or a Ph. D. means more, has more content than it did a quarter century ago. Technology and capital investment have increased productivity in other areas, notably manufacturing, but have been of little help to universities as counters to inflation. Thus it is remarkable that for four decades or more the price of a year at the University of Rochester, including board and room, has remained about equal to the price of a full-sized Chevrolet. That the inflation of our prices has not been worse than this is a tribute to superb investment management and to generous support by donors. Our costs are now rising so sharply that even this comparison, cold though its comfort may be, may soon be vitiated.

Next there are the costly restrictions and requirements being placed on universities by government. Dale Corson has listed nine such by the Federal Government alone. I could list several more, such as the erosion of tax exemption, added by State and local governments. However much one agrees with the social desirability or even necessity of such changes, they unilaterally raise universities' costs without any compensation in increased income.

A more insidious and potentially even more damaging intrusion by the Federal Government is its number-happy attempts to assign unit costs, such as the cost per student of a 16 Century English history course or the cost per student of a biochemistry laboratory. The serious mischief will be worked, and is already threatened, if eligibility for Federal funds becomes dependent on "efficiency" measured in such simple-minded ways, or, as is the case with some other government intervention, if permission to continue to give degrees is jeopardized. The accommodation required by universities—giving up the more expensive courses and programs, restricting student choice—would be educationally disastrous.

Creeping homogenization of American higher education by government leveling implies an especial danger for private universities. We must be different from public universities in order to survive—why else should someone pay us for something he can get elsewhere at a much lower price because it is tax-supported? This variety in American higher education is its greatest distinguishing feature, much admired by European educators. Any student can find an institution and a program that is especially suited to his interests, needs, and abilities. This is the true meaning of "open admissions," much more consequential than the "buzzword" use to advertize futile attempts by a single institution to be all things to all people. Loss of even a few private universities would produce an unfortunate narrowing of the spectrum, a profound disservice to coming generations.

We and other private universities have managed to stay afloat as our costs were raised primarily because of greater giving by alumni, foundations, corporations, and friends and because of greater use of funds generated by endowment. Both sources are now threatened by the crushing lack of confidence in the future earnings of savings, most commonly measured by the well-known averages of the securities markets. In the face of the combination of this calamity and the soaring costs, some colleges and universities seem about to take to the lifeboats.

We at Rochester will not lose our nerve. Further, we are determined to maintain the quality of faculty and students and the depth of programs. Standards of support services may have to suffer temporarily, and a modest decrease in size may be necessary for a while. Innovation and responsiveness to student needs will have to come by "balancing cuts and fills" (in the engineer's idiom) rather than by the easier route of expansion. But I am confident that quick, imaginative, energetic adaptation to the unfortunate conditions will save the quality of the University.

III

But it would make ill use of the present occasion and this welcome audience to dwell on the grinding-down influences of the present and the details of coping with them. It is more appropriate tonight to take a much longer view of the future of universities.

Of all man-made institutions, the university takes the longest view of time. The primary missions of universities are to help succeeding generations prepare themselves for lives of service and to create new knowledge that will enrich the lives and enlarge the opportunities of succeeding generations. One does not give his life to a university unless he is optimistic about the 21st Century.

The goals of the university are so lofty that it makes us a little uncomfortable to say them out loud. When we do so, we hear phrases like "help each individual to develop the best that he is capable of," "prepare students for lives of service," "push forward the frontiers of knowledge," "preserve, communicate, and extend the wisdom of ages," "serve society's long-term needs," and, from Chancellor Wallis' inaugural speech, "To Each his Farthest Star." Out of fashion though it may be to use such elevated language, it is the only language appropriate. Universities are notoriously expensive institutions, requiring for their continued existence that students, parents, faculty, staff, and donors forego pleasure and self-gratification. We could not justify our existence with less than lofty goals.

The most central of these is education; its imperatives shape the institution most directly. At universities like the University of Rochester undergraduates experience association with faculties that are concerned both with undergraduate teaching and with graduate teaching and scholarship. Such institutions demand a more vigorous and independent student but reward the student handsomely for his vigor. A professor who is at the frontier of his subject communicates some of the excitement of the frontier, some of the rewards of nourishing the life of the mind. I once interviewed a dozen graduate students who had been undergraduates at the same excellent liberal arts college, one that would be in the top half-dozen on anyone's list. Because of their independence and the flexibility of this college, most had avoided the frustration, so pervasive among other colleges, of the "student outrunning the master" in their upperclass years. But all of them remarked what a refreshing and profoundly educating experience it was when graduation put them in contact with a graduate faculty. They had not earlier witnessed two professionals in scholarly argument, differing over the interpretation of a work of literature, the domain of validity of a theory, or the meaning of an experiment.

A university is not for every undergraduate. But for the student with sufficient strength of character, independence, and intellectual ability to take full advantage of a university faculty, a research library, and advanced laboratory and computing facilities, the rewards are incomparable.

High among these rewards is the opportunity to create for himself the beginnings of a liberal education. He cannot demand to be given a liberal education. The student himself must play the key role in assembling an array of courses, augmenting them by independent study and out-of-class discussions, establishing the connections among courses and fields of study, and unifying all these elements within his own structure of values. To be sure, there will always be tension between the demands of professional and liberal education; even within a single lecture in a single course, the subject may be developed primarily in terms of its use as a base for further professional use or primarily in terms of its illumination of the broad sweep of civilization. But even the most narrowly professional performance of professors can be integrated into the student's own design of a liberal education.

There must be no doubt of its value. The late Joseph C. Wilson noted that however valuable a professionally oriented education might be to a young person in securing his first job, it was the strength of his liberal education that counted in considering him for the first and all subsequent promotions to more general responsibility. Edward Weeks has noted how important a liberal education is to the growing numbers of individuals who, long after their schooling days, cap one career by a quite different one. And, of course, the great and enduring value of liberal education is to establish the breadth and depth of leadership of society, without which the chaos and hedonism of the jungle would prevail.

But even for the undergraduate whose goal is a liberal education, I would strongly urge that he supplement this quest with the acquisition of some substantial competence—not substitute, supplement. This competence, the ability to do work that others find useful, may not even arise from study at the university; it may come from a summer job or volunteer work in the community. It gives the student a chance to connect himself to society in rewarding ways; those without any competence that is needed or wanted are necessarily frustrated and antagonistic.

The so-so writer is not making a great contribution to his fellow man, he is just self-indulgent if he thinks others should support him when what they need is a good computer programmer. The so-so mathematician might better serve as an excellent copy editor for a publisher. E. B. White, as reported by Israel Shenker, has said this much more felicitously: "I do have a tremendous respect for anyone who does something extremely well, no matter what. I would rather watch a really gifted plumber than listen to a bad poet. I'd rather watch someone build a good boat than attend the launching of a poorly constructed play."

Graduate and professional education needs no defense. The universities must do this well, whether it be music, medicine, or mathematics, and at the highest level. No other element of American society accepts this responsibility. There is no lack of ideas or failure of commitment of American universities to graduate education; there is only the single vexation of the distressing cost.

The second main goal of the university, research and artistic and scholarly creation, is of course strongly bound to advanced education. Its necessity as part of doctoral education sometimes obscures its importance in its own right. Industrial and Government research establishments extend the frontiers in some areas, but not in the humanities and not even in those branches of science and engineering with far-future implications. Universities are the only institutions that accept the mission of a universal spectrum of scholarly and creative work.

The tension related to this goal is in the question: How much research? Some areas have been built up with massive injections of Federal funds, only to have those particular bandwagons move on down the street and leave the universities with obligations but no support. The mischief worked by the capriciousness of Federal support is compounded by the Balkanization of university structures, which prevents timely and coherent response to changing support patterns.

There is no simple answer to the rate at which research and other creative activity should be supported, once one equals or exceeds the minimum rate necessary to sustain advanced education. On the one hand, there is no theorem that states that everything must be discovered in this century, and so what is the hurry? On the other, for the child who died of poliomyelitis the year before research produced a successful vaccine, the rate was not fast enough.

Finally, the third goal of the university, more peripheral than the other two, is public service. In the long run, universities serve the public best by being the best institutions they are capable of being, by performing education and research as well as their resources permit. The conflict in time scales here is acute: Whereas, as I have said, the university looks decades ahead in its concept of service, the neighboring community wishes "here and now" help, and its leaders who are carrying the responsibility of managing under difficult financial restraints sometimes view as a luxury the freedom to look beyond next week. There is another source of tension here: Much of the university's public service is necessarily invisible. Students and faculty members, especially in the liberal arts college parts of universities, have accused the university of lack of "relevance," misusing that word as usual to mean "immediate applicability." James Perkins has noted that the current contributions of universities to their neighbors are largely through their professional schools, especially medicine, and largely unknown to the arts faculty and students. Cold, or at best warmed-over, comfort is provided by the old aphorism: "You either get something done or you get credit, not both."

IV

Up to this point, I have stated some of my views and convictions on universities as institutions; I now turn to my intentions and aspirations for the University of Rochester.

We must first remind ourselves how recent has been our transition to a full-fledged university, strikingly illustrated by the three recipients of President's Medals this evening. Dr. George Whipple piloted the creation of the School of Medicine and Dentistry, Howard Hanson piloted that of the Eastman School of Music. Dexter Perkins led the development of advanced scholarship and graduate work that initiated the transition to university status of the two liberal arts colleges. Cornelis de Kiewiet and Allen Wallis led the University as those two colleges became the launching pad for four more professional schools and became a College of Arts and Science with work in nearly every subject at the most advanced level.

This transition has been costly. It has required all the resources that could responsibly be applied. Deciding on the rate of applying resources in the future will not be easy. The principle is clear enough: We must manage in such a way that we bequeath to our successors at least as much ability to mount educational programs as our predecessors bequeathed to us.

To expend currently more resources would be irresponsible; to expend fewer would provide for healthier growth and innovation later but under current conditions would risk losing our precious investment in high quality faculties and facilities. Applying the principle means predicting the great unpredictables of inflation, government policy, and the future earning power of investments. Thus there can be no certainty here; I can only pledge adherence to the principle and a commitment to conserve the human resources attracted to the University during its transition to a major institution.

This transition has seemed slow to the participants but has, by standards of institutional change, been almost incomparably rapid. Recall that changing a curriculum proceeds with all the speed of moving a cemetery; institution building usually goes even more slowly. Since our motion has been so fast, there is considerable unevenness of attainment among the various colleges and departments and often incongruity between quality and the perception of quality by outsiders. For the same reason, there is much unfinished business, most notably in the development of the library collections.

Our major tasks in the next decade, all related to this remarkable academic development are three: 1.) to solidify and make universal the quality of faculty and programs at the high level already established in most departments; 2.) to secure the recognition required to continue to attract and retain the students and faculty we deserve; 3.) to obtain the financial resources we need to complete this academic development and to guarantee that it will endure.

We are determined to compete with the best universities for undergraduates who are strong in character, energetic, and academically able. We will compete with the best for graduate and professional students of greatest promise. We will compete with the best for faculty who are concerned and effective teachers and at the same time outstanding and productive scholars. We will compete with the best for staff who are oriented toward serving the educational and research programs.

We will not be content in any place in the ranks of American universities except among the few at the top. Of course there will be the inevitable jockeying and leapfrogging, in which the retirement or departure of a few key faculty can lower the position of a department from the top to the tenth. It thus would be extravagant nonsense to aspire to be at the very top, however defined, in every department at all times. But we intend to compete as actively and ambitiously as any, and to remain dissatisfied with any quality but the best.

To attain this position, we will forego the possibility of being big. Indeed, small size will continue to provide a special flavor of informal association and cooperation in interdepartmental and intercollege programs. The creation of a law school is the only addition contemplated, and the student body size will not increase.

To attain this position, we will forego the possibility of serving as a model for others. Indeed, our goal is that we plus other institutions add to a spectrum of institutions that will serve the country well. We will remain different, eclectic without being queer. No one should be misled into thinking that because we choose our particular course we recommend it for others.

In approaching these goals we have substantial advantages. Let me remind you of the major ones:

1. The continued dedicated participation of Chancellor Wallis.

2. A strong and devoted Board of Trustees.

3. A faculty at once concerned with students and with the frontiers of knowledge, a faculty that is increasingly loyal, if you will pardon an old-fashioned word.

4. Deans who are masters of leading their colleges in our decentralized governance but at the same time cooperative in university-wide programs.

5. Our setting in Rochester, a community that values education, quality, and quality education, a community that has exhibited remarkable patience in helping the University even though the really spectacular return to the community is mostly in the future.

6. A small but dedicated and yearly strengthened alumni and alumnae body.

7. A physical plant that with a few important exceptions is well maintained and adequate in size.

8. Finally, and less jocularly than you think, bad weather, which encourages intellectual work most of the year and permits our graduates to choose among jobs anywhere in the world, including the Northeast.

These are powerful foundations on which to build our future service. Further, we are continuing along a familiar road; I can say, as Chancellor Wallis did on the occasion of his inauguration, "The job ahead of us; fortunately, is not to find the course but to adhere steadfastly to it."

All that is needed in addition is a confident spirit. I have that spirit. May each of you be animated with it.

RLS:cb
30 January 1975

INAUGURATION speech sent to:

Mrs. Harper Sibley — 3 March 1975

Mr. Alfred C. Aman, Jr.
(Sutherland, Asbill, & Brennan) — 13 February 1975

Mr. Albert K. Chapman — 3 February 1975

Dean W. Robert Marshall, College of Engineering, University of Wisconsin -- Madison

Professor John E. Willmer, Dept. of Georgraphy, SUNY - College at Cortland

Mr. Orrin Nearhoof, Director of Teacher Education, State of Iowa Dept. of Public Instruction, Des Moines, Iowa

Professor Gary L. Miller, Dept. of Biology, Eisenhower College

Paul D. Lane, Chairman, Dept. of Physics, College of St. Thomas, St. Paul, Minnesota

Professor L. C. Eggan, Dept. of Mathematics, Illinois State University

Professor Hla Shwe, East Stroudsburg State College, East Stroudsburg, Pa.

Professor Benjamin F. Perry, Jr., Stronomy Department, Indiana Univ.

Duncan E. Poland, Chairman, Division of Natural Sciences, California State College, Sonoma, Rohnert Park, California

Professor Evan B. Hazard, (Biology), Bemidji State College, Bemidji, Minnesota

[Rochester Review (Inaugural issue) sent to Geneva Johnson (6 copies)]

G. R. Gottschalk, Director, Office of Estate Affairs, Cornell University

Alfred F. Borg, Group Director, Experimental Projects and Developing Programs Group, National Science Foundation, Washington, D. C.

Professor Jesse L. Parks, Springfield College, Springfield, Mass.

John F. Corey, Assistant Vice President, Student Services and Special Programs, University of North Carolina, Chapel Hill, N. C.

Mr. Howard E. Holcomb, Association of American Colleges, 1818 R Street N.W., Washington, D.C.

Donald F. Ursino, Chairman, Department of Biological Sciences, Brock University, Glenridge Campus, Ontario, L2S 3A1, Canada

Peter Fenner, Acting Dean, College of Environmental and Applied Sciences, Governors State University, Park Forest South, Illinois

Dr. A.T.K. Cockett, Medical Center, University of Rochester

Dean James A. Rutledge , The Graduate School, Northern Illinois University

E. Keneally (science instructor) John Adams High School, Ozone Park, NY

Professor George Macinko, Department of Geography, Central Washington State College, Ellensburg, Washington 98926

Norman Barton-Chairman, Mathematics and Science, Vancouver Community College, Canada

Dr. S. Frederick Seymour, Department of Sociology, Northern Illinois University, DeKalb, Illinois

Kenneth G. Johnson, Chairman, Department of Geology, Skidmore College

Professor W.H. Pfander, College of Agriculture, University of Missouri (Columbia)

Professor H. Cruz, Zoology, University of Sri Lanka, Ceylon

Professor William S. Dorn, Chairman, Dept. of Mathematics, Univ of Denver

Leslie C. Drew, Director, Museum of the Rockies, Montana State University Bozeman, Montana

Mr. Bolton C. Price, 5661 Coliseum Street, Los Angeles, California

26 May 1976

The University: Today and Tomorrow

Robert L. Sproull
University of Rochester

I. Introduction

I shall organize my remarks by aspects of universities as institutions and their connections with their clienteles. "Today" and "tomorrow" will be considered under each aspect, in varying proportions. Prediction of "tomorrow" is of course where the interest is but sets an intimidating task. I am sure I shall fall into the error of confusing what I wish with what I expect, but I shall try to separate these as clearly as I can.

In approaching "today and tomorrow" I intend to take a long view of time. The university, of all human institutions, takes the longest view of time.* The centers of gravity of the productive lives of our graduates are about 20 years after graduation (half-way through a period of about 40 years), and changing the curriculum is known to proceed with all the speed of moving a cemetery; any proposal for a major change in content must therefore be suited to a society at least a third of a century hence. Moreover, the research rôle of the university also takes this long view: Results of research on the physics and technology of fusion will be applied to human needs only some 30 or 40 years hence; for different reasons, research on chronic effects of air pollutants will take 25 years or more to produce rewards to society.

Another reason for the long view of time applies especially to the "today" part and is well summarized by what physicists call Dyson's Theorem, stated originally about books: If one attempts to be up-to-date to within the last n years, he will go out-of-date within the next n years. Hence my "today" will be averaged over the last five or ten years in order that it not be supplanted immediately by "tomorrow."

*In the U.S., the church no longer is as interested in eschatology and immortality as it is in today's minimum wage or last night's knifing in the inner city.

I warn you at the beginning: I'm an optimist. That should come as no surprise, since one doesn't take on the leadership and management of a university unless he believes that there will be a Twenty-first Century and that the university will be an important part of it. My optimism is buoyed by the recent collapse of the student shenanigans. As Winston Churchill wrote almost 80 years ago: "Nothing in life is so exhilarating as to be shot at without result."

II. Survival

The student activists in the late sixties claimed that they occupied the highest moral ground. From this elevated position they sought to use the university as an instrument of their causes. Many faculty in joining them tacitly assumed the university was somehow guaranteed to be immortal, and thus using it as their plaything would be without adverse consequences. These groups did not care whether the university survived whereas others, including most faculty, did; thereby originated much of the bitterness of those years.

Survival is now threatened in a cruder, less complicated way, simply through lack of money. Financial worries dominate the present and the near future of all American universities. Accommodations, sometimes frantic, to skyrocketing costs and abrupt loss of external support are commonplace. Unlike Tolstoy's famous categorization of families in *Anna Karenina*, we are all alike in our unhappiness. Taxpayer revolt and the backlash from overbuilding threaten state universities. Failure of securities yields to keep pace with inflation and restricted disposable real income of the upper middle class threaten private institutions. For the first time in living memory responsible speakers question the survival of universities.

One certainty in a world of uncertainties is that it takes a little over 18 years to produce an 18-year old. The demographic data pertinent to 1984 enrollments have been known for a decade. Even if the current 60% of high-school graduates who immediately go on to college and universities rises to 65%, the total entering enrollments in higher education must drop 20% in the next 15 years. Yet many universities, especially public ones, have continued to expand, and, much worse, to freeze-in expansion through construction and tenured appointments. The results will be agonizing contractions in universities and the liquidation of some graduate programs and possibly even of whole professional schools. But universities will survive.*

*Not all colleges will, however, and mergers such as those now prevalent in divinity schools will become frequent among colleges.

The problem for universities will not be survival itself but will be to remain worthy of survival. The grinding down influence of financial stringency is bad enough. But in addition, non-expansion, and even contraction, will stress the academic fabric to the breaking point. A department chairman who has had the ability to appoint a new faculty member or two each year could give life to his view of his field, could swamp his mistakes by later successful appointments, and could accommodate to internal changes in his discipline or to changes in student patronage. A department chairman who does not have this freedom, or who even must let superb junior faculty go in order to decrease the size of his department, cannot be counted on for the imaginative and energetic management that will be required. Virtually no faculty remain who experienced a pre-war university, and expansion has been monotonic in the last thirty years; thus faculty, without thinking about it, regard expansion as an immutable characteristic of universities and may expect their chairmen, deans, and presidents to produce it regularly. Therefore the disease of financial undernutrition will have the complications of low morale and of unreasonable expectations of what leaders can supply. Adaptation and responsiveness to changes in society and to developments in the academic disciplines will be inhibited by this disease.

The famous remark of the Abbé Sieyès when asked what he did during The Terror ("I lived") is surely one of the most cynical of all time. From his record of non-accomplishment afterward, despite his advantageous position, one wonders why he bothered to live. Universities must have more to say for themselves or they will not get or deserve the public support to assure survival.

To be worthy of survival and in fact to survive: The rest of this talk will be concerned with my expectations and my agenda for these imperatives.

III. Structure, Governance, Leadership

The first item on the agenda is the re-invention of the university's central administration. I do not consider this a self-serving remark because the administrations are in place, being paid, not being shot at much now, and so it is only a question of what they are in gear with, how hard they work, and how they invest the limited amount of good will that is returning to campuses.

Many institutions have Byzantine participatory democracies as legacies from the Sturm und Drang period. Dr. Jean Mayer warned then: "Don't follow the French, who generate a new constitution in each crisis," but few heeded him. Responses to disorders that should have been localized in space and time have been institutionalized and perpetuated. They were called "reforms," since for some unfathomable reason all changes in university governance are called "reforms," and they must be killed or encouraged to die quietly.

In their place, there must be a strong central administration and a strong faculty organization. The former is needed to deal with the overwhelming financial problems and to keep governments from destroying the universities. The latter is needed to plan and direct faculty effort toward academic goals, toward securing the clientele the institution deserves, and toward insuring that the no-growth steady-state will be a dynamic and intellectually challenging and rewarding ferment. I wish I could estimate the likelihood that both these developments will occur. Without the former, the university can't be saved; without the latter, it's not worth saving.

Let me put footnotes to this last pair of pleas. First about central administration, there are many reasons for the weakening that has occurred. Externally, American university presidents now have all the influence of high school principals and preparatory school headmasters. The respect for them and their universities was fueled by fear in the 40's and 50's; the nation needed Ph. D.'s to regain its confidence with respect to the Soviet Union, and even the humanities benefited substantially through the National Defense Education Act fellowships. During the time of the Viet Nam adventure and the student shenanigans, the public disrespect was fueled by fear of violence but also by the frequent presidential statements, especially statements on public policy, that seemed more directed toward saving presidential skins than toward defending free speech, academic freedom, and the rights of individuals in the university. We rationalized that it was not our own necks we were saving, but (more highmindedly) our institutions', but we folded just the same. Now, our pre-occupation with financial survival works against regaining of respect. We need to be bolder, to speak out on educational imperatives, to be proud of intellectual pursuits, accomplishments, and elitism. It will be hard. The young people who write for the press and the tube believe in egalitarianism as a religion. At best, these media are low-pass, low-fidelity filters incapable of treating ideas of any complexity; at worst they are dishonest and self-serving. Even with courage and good luck, we cannot expect to return to the kind of respect in the statement "President Lowell is in Washington to talk with Mr. Taft," even when poking fun at Harvard's provincialism.

Central administrations will have to spend more and more of their time in court, in defending the university against the promoters of causes (who would like to kidnap it as an instrument of their causes), and in fending off government interference. More lawyers will become presidents. More faculty suspicion will be generated: When the administrators say that some ridiculous, obnoxious, or pernicious procedure is required by Federal law or Executive Order, are they making that up?

To footnote the other point, the faculty's organization for participation in educational design, first I note that the growth now lies elsewhere, in faculty pre-occupation with their own economic status. Unionization has invaded public universities and will quite possibly be the norm in those institutions in a decade. Thus far, faculty unions have no foothold in any major private university, but the future is uncertain. The two kinds of universities are substantially different in their relations to unions. A faculty union at a state institution can upon occasion increase the size of the pie that is being cut; the state legislature may, when providing the additional support to compromise with the union, retaliate in educational decisions by limiting out-of-state students and reducing programs unpopular with the lawmakers. In private institutions, union activity cannot increase the size of the pie (and probably decreases it), and since most of the discretionary expenses are already faculty salaries and perquisites unionization can only redistribute (presumably in a leveling direction) dollars among professors.

We must get the faculty's minds off their economic status. Continued pre-occupation with dividing up the pie will mean that there will be no pie to divide. There is so much to be done — I'll return to this in the next section — that only the faculty can do. It is obviously illegal, immoral, and fattening for administrators to have educational ideas.

The basic structure of the university, I believe, will remain as it is. First, it will continue to combine teaching, research, and public service in a single institution. Vice-Chancellor Habakkuk and others have questioned the durability of this concept but have answered their own question as I have. Public service must be on a different plane from the other two. Teaching and research are inextricably joined; even the relative emphasis differs little from one institution to another in the fifty universities that compose the Association of American Universities. Public service, on the other hand, is subject to decision making, except for the self-supporting core in public universities. Private universities do most of their public service through their professional schools, most notably their medical schools and associated hospitals. I see no reason for this to change. We shall have to become more hard-nosed about reimbursement, however.

Second, the basic structure of professional schools and of departments representing academic disciplines will endure. I do not believe that the popular parades such as energy or environmental protection will result in structural changes. Some universities have departments devoted to these problems, but I doubt these will continue. People now realize that there is no such thing as "space science," and space science departments are being liquidated. There will be few energy departments, but many energy programs or centers. Interdisciplinary studies will continue to grow, but we will be increasingly cautious about creating interdisciplinary programs without the disciplines — the way home economics sometimes is. The disciplines — economics, or mathematics, or history — will continue to be in one-to-one relation to departments, the key structural elements of universities. Journals and professional interactions and associations will continue to maintain the standards of verification and originality in each discipline. A problem-oriented program, like energy or health care in rural areas, will bring people together from several disciplines, but their permanent homes will remain in the departments.

Finally, there is a major opportunity, which I hope will be exploited, to bring the people, programs, and courses of the professional schools to serve the undergraduate. A similar opportunity exists in the closer articulation of the professional schools with each other. I shall have more to say about examples in the next section, but meanwhile note only that to accomplish this will require individual faculty initiative plus the two developments I have called for earlier, a strong central administration and faculty organizations oriented toward educational goals.

IV. Content and Method

"Schools are a mistake." This radical thought is never stated so baldly in James S. Coleman's "Youth: Transition to Adulthood,"* but the

*Report of the Panel on Youth to the President's Science Advisory Committee in June 1973 (when PSAC had just been killed).

thought is there. For reasons associated with the flight from the farm and the increasing professionalism of teachers, schools have developed in directions Coleman deplores, because they delay the growth of adolescents into productive, responsible adults. Youths no longer help each other through the mixture of ages during teaching situations, they are separated from adults (except the one adult who is trying to maintain a learning environment, sometimes against overwhelming odds), they do not witness adults involved in any productive activity (as earlier pointed out by Urie Bronfenbrenner), and they are protected from situations in which the well-being of one youth depends on the good sense and responsible behavior of another.

Universities share this indictment. Summer jobs, a year or two working between two segments of university work, or the "co-op" programs common in engineering schools are good ways of making the university plus the outside experience add to a healthier transition to adulthood.* There is,

*Part-time work during the academic year is frequently less successful, since many of the jobs are artificial and do not involve growing responsibility.

of course, a limited influence that the universities have here; just as the end of conscription permitted male students the freedom of a year or two "off," the economic slump made jobs difficult to find. But we should be creating "co-op" programs and facilitating shouldering of responsibility in any way we can. The rapidly rising tuition and maintenance charges at universities may actually help here (not that this unintended result justifies them, of course).

Of course these considerations of the growth of the individual must be balanced against the effectiveness of the university as a cloister, a place removed from the daily distractions and pressures, where the life of the mind can be explored. This tension and alternation between connection to and disconnection from responsibility will doubtless continue in universities. To the extent that a student can do "both and" by alternating job and school, to that extent it seems to me he can reduce the tension and experience a more effective education.

A similar tension exists in undergraduate education between the development of competence and a base for graduate and professional schooling on the one hand and a liberal education on the other. There is necessarily competition because of the finiteness of time, but it seems to me that a university student should always strive for both. Competence, the ability to do work that helps others with the result that others are willing to pay one to do it, is nothing to be ashamed of, despite the attacks from those segments of the university that don't facilitate it. E.B. White was reported by Israel Shenker as saying: "I do have a tremendous respect for anyone who does something extremely well, no matter what. I would rather watch a really gifted plumber than listen to a bad poet. I'd rather watch someone build a good boat than attend the launching of a poorly constructed play." This competence may, of course, be developed outside the university; it doesn't matter, as long as it is developed. Not to do so is the ultimate in selfishness and self-indulgence, unless the student happens to be Beethoven or Einstein.

The other partner to the tension is the beginning of a liberal education; I say "beginning," of course, because that is all that can be expected in a few years at a university. Colleges can provide this, too, but a liberal education in a university setting (with faculty and graduate students active at the frontiers of knowledge, with contiguous professional schools) has its own special flavor. For one thing, a university is more likely to include scientific literacy along with ordinary literacy in its liberal education, but there is far more than this. Perhaps the most important element is the humility of the professor who is trying to extract Nature's secrets or who is struggling to create a fresh interpretation of Thomas Hardy. The undergraduate teaching by such university professors is simply different from that by a college faculty. The special vulnerability of the university student, however, is that he may too early put on the blinders of a pre-professional track and fail to begin the broadening process before it is too late.

I see no solutions to this old and enduring tension. One of the reasons for the change in faculty focus that I urged earlier is so that faculties could attend to this tension and keep both developments healthy.

I should like to see far more motion in the curriculum, in requirements for graduation, in the variety of teaching methods, and especially in the articulation between undergraduate programs and professional schools. I do not call this "reform" because it is back-and-forth motion. There is a theorem about undergraduate education: "Everything works for a while." The corollary is, of course: "Nothing works forever."* We should exploit

*This is not exactly the "Hawthorne Effect" but is of course similar.

this theorem by introducing new programs even if we fully expect that the "innovation" of five years hence will be in fact a return to where we are now.

There are many possibilities: let me suggest a few as illustrations: 1.) A junior-year-at-home, an interruption of the courses for the major to spend a year (without leaving the university) on an entirely different major, or on Daniel Bell's "third tier" courses unrelated to one's major. 2.) "Flight plans" (named after the airplane pilot's plans) in place of requirements, to allow a student and his advisor to put together a personal curriculum plan, unlike any other student's. 3.) Requirement for a certification of writing competence, to cope with the degeneration of writing ability and performance between the Freshman and Senior years.*

*See the Carnegie study by Kitzhaber in 1963.

4.) Exploitation of societal problems (e.g., urban transportation) as motivations for studying the disciplines, without which only fairy stories can be told about the problems. You can add many more. The key consideration is, of course, that we not become emotionally attached to any such development, in order that we can drop it and start something else. In a way, I am revealing my lack of faith in educational doctrine, my lack of confidence that there is one "best" way. If there are many ways not differing much in probable success, then we might as well invigorate our institutions by taking first one and then another.

To turn to graduate programs, I advocate one change but acknowledge that I have little confidence that it will happen: I should like to see a transfer of substantial numbers of students from Ph.D. to M.A. and M.S. programs. Industry has complained for years that they would like the ability typical of doctors but the willingness to contribute in ways other than original research that is typical of masters. I was struck recently by the realization that the university as employer also wanted such people for advising, placement, admissions, and similar positions. As long as all the best students go on to Ph.D. work, we shall have to employ Ph.D.'s to get that intrinsic quality; but masters are what we want. We in the universities could do a better job of educating them than the colleges and technical schools now turning them out, and moving in this direction should decrease our costs.

There is a large problem in American universities associated with the decline in Federal support of graduate students and of research and with the narrowing of job opportunities for Ph.D.'s. The numbers of graduate students, particularly of Ph.D. students, have accordingly been decreased at most schools. The largest schools, however, use many graduate students as teachers in the elementary undergraduate courses; these numbers have not decreased. Thus new graduate students are admitted in numbers that have no relation to the jobs that will be available when they receive their Ph.D.'s. In time, it is possible that, far from the present situation where the best students go on to Ph.D.'s, the poorer students will enroll as Ph.D. candidates in order to take these teaching assistant jobs, whereas better students will go to other graduate or professional school programs or take jobs with B.A.'s. The problem is acute in fields like history or comparative literature, where there are traditionally almost no good jobs for Ph.D.'s except in colleges and universities, and in the present time of no growth there are almost no jobs.

It has been proposed that adult education will become a much more prominent part of universities in the future. Last summer Vice-Chancellor Habakkuk called this a "romantic notion" and reminded us that there are good reasons for concentrating on the young. I agree, although I do expect a continued slow growth in "retreading" courses in engineering, medicine, business administration, and the like. These are a useful public service and frequently pay their own way, but they are unlikely to form a major part of most universities. There is one frightening aspect of the adult education talk in America: Many state institutions are overbuilt and seek a way to defend budgets, buildings, and faculty size in the face of the inevitably declining enrollments of recent high school graduates. When they project rapid expansion in adult education, one is tempted to be skeptical of their motives as well as of their projections.

V. Access, Clientele, and Interaction with Society

Access to undergraduate higher education exists now for nearly everyone who wishes to attend. The word of art in that sentence is not "nearly," as you might expect, but is "wishes." The young people who do not have access are those who arrive at age 17 without the wish to attend, whose earlier family or school experiences have denied them the ambition, drive, patience, personal organization, good health, or other elements of the desire to continue with their educations. There are a few who have the drive and do not go on, most likely either because of an exaggerated view of their responsibilities to younger siblings, because they are unwilling to forego current earnings and self indulgence in order to enroll in the only kinds of institution that are willing to admit them, or because they are unwilling to take on reasonable indebtedness.

Alan Cartter in a recent Carnegie study* predicts that the fraction

*Ph. D.'s and the Academic Labor Market; New York; McGraw-Hill Book Company, 1976.

of high school graduates who immediately start some kind of higher education, which is now about 0.6, will vary between 0.6 and 0.7 between now and 2000. These students will be supported just as they are now, on a combination of parental aid, scholarship grants from the institution, jobs, loans (including Federally assisted loans), and Federal and state grants-in-aid. Aid packages are already not just Baroque, but Rococo in their complexity. They probably will become more complicated before they become simpler. There continues to be some talk about the "voucher system," in which every youth would receive from government a grant of the same size; he could then apply this to the cost of education at any institution he chose. In addition to its simplicity and encouragement of diversity among colleges and universities, such a system would almost single-handedly solve the "accountability" problem. But this scheme is much too fair and simple to succeed, and anyway, if we had it what would the people in the Office of Education and the state departments of education do next year?

There is fear in private colleges and universities that we may experience a bimodal distribution of family incomes, taking only the rich and the poor — the latter with special Federal and state aid programs — but not the middle class. This unpleasantness has not occurred yet, but the financial plight of the private colleges and universities may produce it. To avoid it will require great effort and good luck to generate the additional financial aid each year, especially since Federal and state programs restrict eligibility to those from families with less than certain income ceilings and these ceilings tend not to rise as the value of the dollar declines.

There is one ray of hope that seems to have gone unnoticed: The same demographic changes that are now beginning to result in fewer high school graduates each year than the year before are resulting in fewer children per family in families with at least one child.* [The well-known decline in birth rate would not necessarily produce this result: It could have been that there were simply fewer marriages, or more married or otherwise attached couples with no children.] In the past, a family's income and savings might have to stretch over three or four children in colleges or universities; in the future it will more often be concentrated on a single child. I have found it difficult to get good data to make a quantitative statement, but what data there are suggest that this effect will be an important aid in permitting children to take advantage of high-priced education.

*U.S. Bureau of Census, Current Population Reports, Series P-20, No. 277 "Fertility Expectations of American Women: June 1974" U.S. Government Printing Office, Washington, D.C., 1975. For example, 26% of women who were 18 - 24 years old in 1967 were expected to produce 4 or more births in their total lifetimes; only 8% of women who were 18 - 24 years old in 1974 were expected to produce 4 or more. The comparable numbers for women 25 to 29 years old were 30% and 12%, respectively.

The graduate students in professional schools, like law, medicine, and business administration, will continue to have support packages similar to undergraduates, but loans will probably play a more prominent part. Most, including me, expect a slow growth of numbers of professional students between now and the end of the century. I shall be fascinated to hear the projections at this conference for medical students.

Students in Ph.D. programs in America have recently suffered a partial return to the pre-World-War-II pattern in which the only prevalent support was through teaching assistantships and in which fellowship support was extremely rare. The Federal press for advanced training was on from 1946 to about 1966 and then tapered off, at first slowly and now more rapidly. During that period, most graduate students were supported well enough that they did not have to go into debt. Federal fellowships and training grants are now rare again. Although support of graduate students by research assistantships on Federal research grants also diminished, there are signs of revival here. I believe the lot of the graduate student will not improve much in the next 10 or 20 years. The Ph. D. winners are simply not scarce enough for another big Federal push to create more of them, and Ph. D. candidates face a future for support much like that of law students, namely a future with heavy use of family resources, summer jobs, and borrowed funds.

The clienteles universities serve are intimately related to their interactions with society. By "society" I mean more and more the Federal Government which arrogates the privilege of acting as society's surrogate. When American society feared the USSR, the Federal Government wanted more research and more Ph. D. scientists. We then had as a consequence the Office of Naval Research, the Atomic Energy Commission, the National Science Foundation, and others, each with research support and graduate student support programs; we also had the National Defense Education Act Fellowships in the humanities. Now that the unreasonable fear of the Soviet Union has been replaced by an unreasoning fear of technology and even of progress, the Federal Government is warring with the universities and holding graduate students as hostages.

There was a partnership in research and advanced education between the Government and the universities. Everybody won. Now the Federal part is being unilaterally abrogated, with bizarre complications: A university can join with the Energy Research and Development Agency to create a laboratory to solve the energy problems of the Twenty-first Century. After the university has made a substantial investment of its own money, the Department of Health, Education, and Welfare can then come along and cut off the Federal share because of an alleged failure of the coaches of intercollegiate athletics to comply with some social action legislation — or rather with the hundreds of pages of Executive Orders and regulations tenuously based on this legislation.

The events of the sixties should have warned us that the university-society interaction would become stronger. The university is like a watch: Its mainspring is society; its support comes from society and in the end it doesn't operate at all unless this connection remains healthy. But its balance wheel is the integrity of its own independence, the "inner logic" in Eric Ashby's phrase. The trick is to use the energy supply from a well-wound mainspring and yet preserve the regulation of activity by a nearly independent balance wheel. Since John Harrison's invention of the chronometer in the mid-Eighteenth Century this problem has been solved for timepieces, but we are clearly going backwards in universities.

Does it matter? Yes, indeed. Society, especially as represented by the Federal Government, is pre-occupied with the "here and now." If one tries to get a politician to look a decade ahead he replies "I can't even see into next week." The university, as I said earlier, must take the longest view of time. It cannot function, its faculty cannot pursue the avenues of research and scholarly work that will make the Twenty-first Century worth having, unless the university has almost complete independence from the political process and the imperatives of the current causes.

State universities in America have learned how to handle this connection-disconnection dilemma, not always well, but usually acceptably. Even here, though, the "taxpayers' revolt" coupled in some states with overbuilding of universities has stiffened the backs of those legislators who have no use for academic freedom. In America we are repeatedly reminded that we live on the very edge of the jungle, most recently by the revival of the occult and by the threats to treat supernatural creation as the head-on competitor to biological evolution.

The private universities are in an even more difficult situation: Government control, with or without government support, is leveling the peaks of excellence, is making it harder for private universities to have distinctive programs. Why should a student pay three to seven times as much tuition to attend a private university if government intervention has rendered it just like a state university? Will it be enough more valuable to a graduate, in lifetime earnings or in the quality of his life, to go deeply into debt to attend the private university?

The deterioration of the relations of the university with society are not all the fault of governments. When societies were most concerned about agricultural growth, military strength, and industrial growth, universities were respected contributors; plant genetics, radar, and polymer chemistry were areas universities and university people could do something about. The common perception now of society's needs — though I doubt this perception is correct — is of problems such as pollution, population growth, urban decay, nationalistic fragmentation, moral deterioration, and the care of the indigent, handicapped, aged, and unlucky. These are difficult problems having one feature in common: Anyone who is going to contribute powerfully to their solution must spend most of his life building a political base. Universities and university people therefore are not likely to contribute much, other than repetitive scholarly proofs of how intractable the problems are, and the universities lose respect.

The universities that I see little hope for are those with the faculty's academic interests and aspirations sandwiched between government control on top and faculty unions dominated by bread-and-butter concerns on the bottom. I do not see how such institutions, which seem to be common enough elsewhere in the world, can serve their clienteles well enough to be worthy of survival.

What of the future of the interaction of universities and society? I wish I could be sanguine, but I simply do not know. All the trends of the last decade are depressing. In the last few months (but it may be only because of the euphoria gonflé by a Presidential election year) I have become a little more optimistic. There is at least the possibility of a reaction to government's pervasive interference in the affairs of individuals and institutions; if a reaction comes it will not be because of sympathy for universities but because other, more powerful, elements of society will discover that their oxen are being gored.

VI. The Ensemble of American Universities

Up to this point we have been considering universities one at a time. I now turn to the ensemble of American universities.

It is quite possible that no American university is as great as the greatest British or European universities. I obviously do not wish to argue the matter, or even to define "great." But we in America are extremely proud of the ensemble of our diverse institutions. It is quite likely that "greatness" in America resides in the array, not in the individual institution.

In the last few years one has heard a lot in America about "open admissions," a "buzz-word" used in a hard sell of some institutions and some policies. The true meaning of open admissions is in the ensemble: Every student can be admitted to an institution — a university, college, or junior college — suited to his interests, needs, and abilities. Not every institution is good for everything; you do not take your ailing watch to a cat hospital. Junior colleges may be better for the vocationally oriented, for the brief capping of a high school education. Colleges may be better for the student who wishes to learn why he was born. Universities are for the strong in character and preparation, with the drive and determination to design their own education with no limits on its depth and breadth.

Among universities, a subset of all the institutions of higher education, there is also impressive diversity. Some serve largely local and present needs and aspirations, some are unlocalized in space and time. Some are large, some are small. Some have one spectrum of professional schools, some another. Some are state-supported and manage large public-service operations; some are private and have more selective public service functions. Some are parts of state systems, some stand alone. None is rich, but some are poor. Some are technically-oriented (like the Massachusetts Institute of Technology); some are humanities-oriented (like Yale). Some charge high tuition, some charge no tuition (although the costs at all are about the same, the differences lying in what price is charged and who pays the cost).

This diverse array has grown up amicably and served the country, and many students from overseas, well. The diversity is now seriously threatened by a large number of homogenizing influences. Among the earliest were the activist student organizations, which attempted to make every university an instrument of the same cause. A little-recognized influence is the joint effect of high-school guidance counselors and parent-to-parent gossip: The end effect is that universities are given an unwritten "prestige" ranking and we all try to be alike in seeking greater "prestige." Related to this is the effect of published aids like Barron's, which keep score on this peculiar race. There are some indications that corporate support, up until now the most benign support of all, may be restricted in ways promoting sameness. George Stigler has noted that universities are even being narrowed by faculty action, in that faculties restrict the range of speakers who can be invited to campuses, the range of ideas that can be given a hearing. The financial exigencies of all universities are forcing us to make similar responses, which make us more like one another. Faculty unions are just beginning to be the strong homogenizing force they seem almost certain to become (they probably will have also the effect of suppressing diversity and peaks of quality within a single institution). State governments have rendered the state-supported subset of universities alike in size, spectrum of professional schools, and other important features.

But by all odds the most impressive and dangerous performer in the homogenization drama is the Federal Government. When a bandwagon goes down the street, whether it is the purity of research grants or affirmative action or some other popular program, the orders go out to make all universities conform. The popular view seems to be that the Government "gives" the universities generous "aid," and that it should therefore have the right to take it away for any reason or no reason. There is almost no such aid; almost all Federal funds granted to universities are to pay for something the Government wishes to make happen. Furthermore, the Government has moved beyond the enforcement procedure of threatening to withhold funds and now in addition threatens fines, denial of tax exemption, and criminal penalties against individuals. If I were to try to explain to you the leveling effect of the Federal Government on American universities, I should first have to explain what OE, HEW, HEGIS, NCHEMS, ERISA, ERDA, OSHA, EPA, DOD, OMB, NSF, EEOC, IRS, are and through what tenuous theories they try to manage universities. The important points are that they do (always in some cause that is on the side of the angels), that they are more influential every day, and that their influence is to make Yale like Rochester, Rochester like Cal Tech, Cal Tech like Santa Cruz, Santa Cruz like City University of New York, and all of us like the Tompkins-Cortland Community College. If your patience and your stomachs can stand it, I shall be glad to provide some gory details.

The private — now often called not entirely accurately "independent" — universities still have an edge in directing our own destinies, since the state governments do not meddle so heavily in our affairs. We shall try especially hard to maintain the diversity of the ensemble. At least we can preserve a variety of sizes of institution, and having a few 5,000 student universities will be a healthy counterpart to the canonical size (30,000 to 40,000 students) of state universities. I believe the "privates" must do far more than that. Many state universities have been created and have survived as great institutions in large part through the argument that "Minnesota deserves a Princeton" or "Utah needs an MIT." The "privates" have an important role to play in showing what quality must be and in preserving academic freedom of faculty and freedom of direction of institutions. In this connection, I am fascinated by the creation of the University College at Buckingham, at whether it can survive in a setting where government has been a dominant influence for some time, and at what countermeasures may be generated by the government (I understand the countermeasures have begun by denying the right to confer degrees, permitting only "licenses").

The Second Law of Thermodynamics should warn us that the low-entropy state of diversity, of "centers of excellence," and of open admissions through a variety of institutions will be hard to maintain. It may not be hopeless, but (except for the tiny ray of hope I mentioned in the preceding section) I have little that I can point to in order to convince you of that.

VII. Conclusion

In this section I should like to summarize what I see when I look at American Universities ten or twenty years ahead.

First, I see the same ones I see now. Few will disappear, although some small colleges will be gone.

Next, the clientele will be nearly the same as now. There will be less accidental discouragement of students before the crucial age of 17 and therefore more nearly universal access. There will be 20% or so fewer students in the totality of higher education, but the population in universities will probably not decline to that extent.

The trauma of the transition from expansion to a steady-state and of the pervasive financial problems will have passed but will have taken its toll. A generation of young scholars, especially in the humanities, will have been lost. By 1990 departments will again have a satisfactory flow of young faculty into them, since the faculty hired in the big post-war expansion will begin to retire.

It is possible that some public respect and even admiration for universities will have returned. Governments know that political clout builds roads in the short run. By the 80's and 90's they may have learned that in the long run it takes high technology to produce adaptive vehicles to run on the roads when the oil runs out. On the other hand, there is a theorem that says "governments do not learn."

The mix of professional schools will be about as it is now. The promoters of "colleges of energy" and other problem-oriented structures will have had their say and been forgotten, and society's problems will still be attacked by centers, institutes, and ad hoc groups of faculty. Co-operative programs among professional schools and between one of them and the undergraduate college will become prevalent.

I have no predictions about the content of university education. Certainly there are many opportunities for improvement. The present pre-occupation of faculty, deans, and presidents with economic imperatives produces a myopia that destroys the ability to predict. I do freely predict, however, that we will all make a great deal of noise about our "reforms."

Adaptation to external stimuli will enable universities to survive, but because of the leveling nature of these stimuli survival will be bought at the price of a substantial decrease, possibly the destruction, of diversity among institutions. The country and the world will be the poorer for this creeping homogenization of its windows on the Twenty-first Century.

My crystal ball clouds rapidly if I allow for the possibility of an East-West mutual thermonucleation, or even of a major war in the Mideast.

But after all the speculation and warnings of doom, I still remain optimistic. The brightest young people will still be spending at universities four to eight years of their lives at their most imaginative ages. Universities must therefore remain most attractive places at which to work and remain a major part of the hopes of civilization.

RLS:cb

UNIVERSITY OF ROCHESTER

VICE PRESIDENT FOR
PUBLIC AFFAIRS

July 16, 1976

For Your Information:

From: George M. Angle

This spring, President Sproull was invited to deliver a paper at a unique conference in England.

Sponsored by the Macy Foundation, the Conference was held in Leeds Castle, Maidstone, England, with only ten conferees present; five from the United Kingdom and five from the United States. The Presidents of The New York Hospital - Cornell Medical Center and the Josiah Macy, Jr. Foundation were among the U.S. representatives. Others, in addition to Mr. Sproull, were the Chairman of the Department of History and Sociology of Sciences at the University of Pennsylvania, and the Hamilton Kuhn Professor of Biochemical Sciences at Harvard. The United Kingdom was represented by officers and faculty from the Universities of London, Oxford, Cambridge, and Birmingham, and the Right Honorable Lord Geoffrey-Lloyd, host.

The theme of the Conference was: "The University and Medicine, The Past, Present and Tomorrow."

The papers were presented in pairs -- a U.S. presentation and a U.K. presentation, each followed by general discussion. Mr. Sproull's assignment was to explain the present and future of American universities to the U.K. participants. I think you'll find his paper of interest. I am sharing it with members of the Board of Trustees, the Trustees' Council, the University Senate, deans and directors, department chairmen, and members of our Alumni Boards. If you know of others who would be interested, please let me know.

ROCHESTER, NEW YORK 14627 · TELEPHONE (716) 275-3322

Universities & Activism

16 April 85 Torch Club Geneva?

- Some diffidence
  - Everyone considers his own field the most refractory
  - Inevitable that some self-justification will creep in
  - Obviously pers. & idiosyncratic
  - Only excuse is that there is gen. interest in univ.; if it stumbles or falls, much lost — leaders of country; colleges; J.C.'s.
    Immortal? Safe to take potshots at it
- Stud. sleravs. focused bright light on univ, its perform. & its governance. "Riot" journalist word
  →CARD←
- Start with some comments on that era
  (Columbia now — hardly get steam up)
- always being criticized: why not control the place
  Open institution, no cyclone fence, visitors welcome but can take advantage; bona fide stud organ.?
- Always issues. VN a real issue, consumations. But not the central issue people at a distance thought.
  Studs. asked me to turn off war. — T. V. control room
  But: Paris 1968 – Berlin – Munich – Frankfurt – Saar
  2¢ "milk strike" at Cornell in '64.
  Draft.
- Overwhelming issue: Who owns the university? Who can enlist it in their own causes?

- Faculty know answer. Community of scholars - but no respons.
- Benign side of stud activism is that most studs. genuinely wanted to feel pride in "their" inst. - wanted to boast at Tgiving or Xmas. justify their choice
- Legal answer: T's or Regents. But power shared.
- Reason: Education different. UAL "Friendly Skies" "American Way" flight arrives whether or not passengers listen
  Not so with univ.: Students listen or not. Faculty vote with their feet - allocate largest resource.
- So tension over control
- also " " time, relevance.
  Univ. put here by contributions of predecessors.
  "Saving," foregone self-indulgence - not just donors, but selfrestraint, voluntary actions of studs. fac & even admin.
  Biggest tension over immediate exploitation of all that for one's views of the moment. and to throw everything into breach
- Admin. accepts rôle of stewards of these resources ∴ labeled as antediluvian conservatives.
  Others chose to put life work into this particular instrument of society - an act of faith as deep and consequential as religious faith
  1. Respons. as an admin to these people.
     Fiducial respons. for credentials of alumni.
  2. Fiscal respons.: To preserve resources so that our successors will have at least as much opportunity to make educational contribs as our predecessors provided for us.
     Bad news from 72-84; now at ~~the~~ √72
     72-(1778), & √½ Double digit inflation
     Meanwhile, state institutions expanding, competing

Worse than that, by 78 no one on fac. who had been fac. of pre-WWII univ. — thought of expansion as guaranteed.

(3) Subject to these, best job one can do of teaching & research, of response to public wants & needs. Best servant of local community by being best univ we can be
(4) Raise level of discourse (↑ bumper sticker).

Three big areas of Presidential concern / influence / power:
Pres guru? → CARD ←
Fac. appts. — Audit process — fac. centered
Shapes the univ.
Allocation of resources among colleges & depts
Local leadership. Also stud. interests, society
Pres. vs future
"Brittle" vs resilient institutions. Not soft Jean Mayer
Now to personal level. → Limit to delegation.

— Acad. Off. Comm. — interaction — not kitchen cabinet.
I pledged not to hide behind them. — took advice seriously
Excellent info. source. Can do it if you must, but.
— Fac. Senate
Advisory — always wants to legislate. But not the teachers & researchers who make the place go — out of steam, frustrated ambition
— Informal interactions with productive faculty. But can't satisfy
— " " with students [one of reasons 16 years is enough]
Asked alumni affairs people to identify future leaders

- Comparison with other insts. - "paying dues" & AAU
- Outside activities generally: GM, boards.
  - Help: When plagued with doubts, impossible probs., reassuring that others appreciated.
  - Brought back mgmt. ideas, Fed. interaction
-

- Bright side: Supportive Board & alumni body.
  Letters from faculty after retirement: Couldn't write earlier.
- Dark side: Impossible to have close friends. Lonely.
  Constantly wrestling with inadequate resources. Grinding down. "Why should I bust myself for them?" Admin. who had enough of "Peter Principle."
- What keeps you going are the students, the change in them. "Passion"

  - Sr. picnics - better every year -
  - Sociologist - VN - Nucl. War - diatribe - "You know, I got a helluva good educ here."
  - "If only I'd known as a frosh what I know now."

The private position, even of the Ed. of the CT, reassuring.
- Love off with univ. as an inst. 50 years old.
- Close with "auspicious."
- Started writing furiously - been writing & working for univs. ever since.

K. Hunter's Seminar 19 Nov 85

this was a talk in Katherine Hunter's course for U of R medical students.

— Why QM matters?

— Poor pun in title

— a) History of the greatest scient. devel of 20th cent. — poss. greatest of all time, certainly greatest since Galileo + Newton

Why not relativity?

Depends on "great" meaning:

Relativity: philosophy, cosmology + $E = mc^2$

QM " " , human scale, chem, bio.

what holds matter (incl us) together?

" " " " apart?

b.) some lessons from it. Later

[they will be ① – tech not just appl sci; sometimes sci. is appl. tech.

② – "relevance" not immediate applicability — instrumentation, pure crystals

– Pohl crushing diamonds → transistor

– Stern Gerlach + molecular beams → maser → laser

③ { – "pure" ≠ "basic"

– wine spectrum led nowhere – rhetoric justifying work on it fraudulent. How do you know in advance? What makes a scientist!

④ – person who can handle math, be sound in own disc., can make hay by crossing disc. lines

Pauling - Nature of Chem. Bond 1939 based on 1931-32 P.'s papers

picked up Heisenberg's 1926 idea of "resonance" – (variation princ.) + resonant energy]

Planck : Didn't believe it; complicated notation, cit. kT
(P's const - like Swiss cheese)

Einstein: 1905 (same year as Spc. Rel.)(Brownian motion)
↳ size of atoms
- P.E. effect.
- took real guts
expers no good - Millikan 10 years later
- but young, brilliant, no reput. to preserve, upset applecart

Bohr: 1918 (?)
- Balmer series - rules based on nuclear atom,
Rutherford

Elaborate tries to preserve class. mech.
- "If q. theory doesn't soon get He atom, He atom will get q theory"

I Schrodinger - Heisenberg Q.M (wave mech)
1923-7
"Q. states" "stationary states"
↳ but motion!

II Pauli Exclusion Princ.
- From spectra
- "Identical" or "indistinguishable" particles
Phil: if in same state, would be claiming could keep track of A & B - nonsense
- Elegant expression: ψ antisymmetric in exchange

III Dirac & others: Relativistic QM
- Other refinements since: QED - self-energy of a particle } to be expected

Election

Questions in QM:
1.) ask a foolish one, get no answer
2.) Shopping q. all important

Paradoxes

G & S
2 slit

Fun, but if ask sensible (i.e., observable) questions, get precise answers;
- Spectral lines
- Flux of particles
-

ψ a construct: "spreads out" probably of finding an election - but who wants to?
Small - not spread out - important for later
Interference of one with another via exclusion princ.
This is what gives size to matter.
"What holds matter apart?" The election - election repulsion, as governed by the Exclusion Princ.

"What holds matter together" The electrostatic attraction of elections and the protons in nuclei, as governed by Q.M. Earnshaw's Theorem

Note: Nuclear matter same except nuclear force replaces electrostatic force

Solids: Consequence of small election & yet large space is that "plenty of room." Elections mostly bound to individual atoms, but outer elections belong to solid as a whole in metals.
Leads to tremendous variety of properties
Since Hertz in 18 , people have had vision of an amplifier for e. m. signals.
Vacuum tube amazingly ingenious device, but short lived:
1920-30 rarity - radio, talking pictures
1930-60 hey day. Now gone (except for a few specialties like picture tubes & high freq. transmitters.)
Solid state amplifier & data processor.
Prob. biggest change in our lives since steam engine.

Q.M. made it all possible in 2 senses — that's the way the world is — tinker, materials for variety of properties
— understanding of QM led to theory of SC's, theory of p-n junction, etc.

Return to p. 1 for "lessons"

Ditto for modern chemistry & biology. Continuing superconductivity, tunneling,

Philosophical implications 1.) Paradoxes: Wrong questions.
2.) Uncertainty "Mitbestimmungsgesetz"

- Unbestimmtheitsprinzip
- Mislead philosophers with inadequate understanding of German language & no understanding of physics.
- Just as "certain" as Newtonian physics.
Given: Some starting conditions, specified to a certain accuracy
Predict: Some consequences, " " " "
Can do just as well as Newtonian mechanics.
- ~~Stationary states~~ - accurate to at least one part in 10^{14}
3600 secs/hr.
24 hr/day. ~100,000 sec. = 10^5
365 days/year. ~4×10^7 sec.

Bohr - Einstein - Copenhagen School - Probability. "God doesn't throw dice"

1 sec in 1 million years
or 24,000 × 5000 ≐ 100,000,000 feet in earth's circum = 10^9 inches; 1 inch to 100,000 times around the world.

Mind-brain problem JHO getting up its courage.
- I don't know how it will come out, but QM "uncertainty" is not the key to free will vs determinism.
Keep looking for extensions, connections e.g. in first minute of universe

Much open endedness ahead
- First 3 minutes; mind brain problem; it's going to be very interesting.

Summary

QM matters because

1.) It is the basic set of laws on which universe runs
2.) In particular, it tells us what holds matter together, what holds it apart, and predicts a spectacular array of processes in & properties of matter.
3.) It is a beautiful, elegant theory with no queasy or sloppy dark corners.
4.) Throughout its 60-year history it has met every new test - 2 notable extensions, but no contradictions & no need to go back & correct earlier predictions

Careers of One Physicist [This was an invited talk to the Physics Department Colloquium, U. of Roch.]

Robert L. Sproull
21 October 1998

A number of people have noticed that I have had some varied careers and have encouraged me to give this talk. But autobiography can be pretty boring, except to the author, and so I hasten to state that you have only me to blame if this talk falls flat.

My position at the moment reminds me of the story about the sixth ±1 husband of one of the famous Gabor sisters: He remarked at his wedding that he knew what was expected of him but he didn't quite know how to make it **interesting**. I shall try to make this talk interesting in two ways: I shall stop from time to time and note the most consequential things I had learned to that point. And I shall note at many points how **physics** underlies activities in these careers.

My four years in high school in Illinois were the worst years of the Great Depression. I worked in corn fields in the summer but could not save much money. My parents had saved to send me to college, but all their savings and those of thousands of others had been lost—stolen, really—by the fraudulent activities of Samuel Insull—frauds which sent him to prison and were the key stimuli for the creation of the Securities and Exchange Commission. Although I was offered a Harvard College Fellowship, the prospects of a job in Cambridge or Boston were so poor that I could not take it. I was saved by being admitted to Deep Springs, a tiny work-study junior college on a cattle ranch in Eastern California. Deep Springs charged no tuition or board-and-room, and that zero was a perfect match to my resources. In retrospect, it was the best thing that could have happened to me, and I have tried to repay my debt to the school by serving on the Board of Trustees and by raising funds.

I will quickly pass over my adventures there, except for two remarks. First, I had intended to study electrical engineering, but at Deep Springs, although we had no physics instructor, I read enough to be intrigued by physics. Second, in my third year, a last-minute defection from the faculty left the school without a calculus teacher, and the dean asked me to take over as part of my work program.

After three years of half-time study I transferred to Cornell, where a companion institution, the Telluride Association, had established a house. I became a financial officer of the Telluride endowment even though I was too young to sign the papers; I had to learn substantial corporate accounting in a hurry. Calculus tutoring became a major part of my financial support. Summer jobs at Eastman Kodak and Bell Telephone Laboratories also helped financially, but their major contribution was education: I learned more in each of those two summers than in any year in a college or university.

You have already heard how **luck** played a huge role on my side. Another lucky incident occurred half-way through my first semester at Cornell. I was taking a course in Modern European History from the great Carl Becker, Cornell's Dexter Perkins. I had read several of his books and enjoyed his lectures. I was astounded that many more people turned up for the first hour-exam than had been at the lectures, which amazed me since they had missed a marvelous opportunity. When the question sheets were passed out at that exam I was very frightened, since I had no idea whether I, with my background in a feeble high-school and an off-beat junior college, could perform in competition with students who had come from city high-schools and had already been two years at Cornell.

The first question, half of the exam, was: "In what way were the circumstances at the founding of the Third French Republic more auspicious than at the founding of the First." There I sat, paralyzed with fright, then literally shaking. But then a student whom I never saw–he was behind a pillar– evidently raised his hand and he asked the proctor: "Sir, what does 'auspicious' mean?" My confidence was immediately restored, I started writing, and I was on my way. I am most indebted to that nameless student, who quite possibly saved my academic career.

I had to learn physics rapidly. I was fascinated by quantum mechanics, which was not all that old or seasoned then. My first course in it was under Earl Kennard who had written a splendid book on the kinetic theory of gases but who did not believe in quantum mechanics. But then—saved again—I had the blessing of an advanced course with Hans Bethe, who as you know is one of the half-dozen giants of quantum theory and its applications.

The War was coming on, and we were all in a hurry. I did an experimental thesis, which became classified because of its application to microwave magnetrons, and I went to RCA Laboratories in Princeton to work on Navy radar. That was during the daytime; evenings, I taught physics to Navy and Marine students at Princeton University and microwave theory and technique to Navy and industrial engineers for the University of Pennsylvania.

Let me pause here for a little stock-taking of what I had learned. By this time I had learned to take satisfaction from **teaching**. I had learned the sobering discipline of accepting **responsibility.** I had learned **respect for applied problems**—later in this talk we will see much more of this. I had learned that hiding within an applied problem could be a **fundamental** question which when explored had wide implications. I had learned the powerful role of **connections** between fields of science, notably physics, mathematics, and chemistry; for example, I learned that with Maxwell's equations and a little mathematics, one could "make mermaids" in microwave engineering and devices and that Hilbert space could serve electron beam excitation of resonant cavities as well as it serves to reconcile Heisenberg with Schrodinger, I had learned that, with the investment of only a few dozen hours, learning the elements of **accounting** has far-reaching applications.

You will not be surprised, then, that after the war I had no desire to remain at RCA, where everyone was going to work on television, and gladly accepted an Assistant Professorship at Cornell. Cornell, like Rochester and everyone else, was planning to move from the "love, and string, and sealing wax" age to the electronic age in experimental physics and was assembling a staff. Most came from Los Alamos, but some of us came from the radar community. I set up a program to grow BaO crystals, which had never been grown, in order to explore their electronic properties, a substantial step from the much-researched alkali halides.

Lloyd P. Smith and I had one of the first ONR contracts—I believe it was the fifth. Incidentally, I believe I was one of the first physicists to hire a chemistry post-doc on his physics contract. I wish I could take time here to describe the contracting innovations and the enormous contribution that ONR made to science by developing its mode of support, later copied by NSF, AEC, and others. In talks and papers I have frequently acknowledged that contribution and examined how well other agencies have served as stewards of the ONR tradition.

The next few years were a golden period for me, and for most physicists. The graduate students were superb. Morale was high, even though we were trying to do research on nearly perfect crystals in an old, dirty, wooden-framed building. One had, of course, to compete hard for money, but the money supply was expanding and the program managers in Washington were able and unbureaucratic.

One of my teaching assignments was a course in "modern physics" for engineers. I broke rather sharply from the tradition of modern physics courses established at Cornell by Floyd K. Richtmyer and continued in his book for many years by co-authors. I emphasized applications to molecules and solids and—horror of horrors!—used MKS units (later S.I.) instead of cgs. Evenings and on airplanes I wrote my notes and problem sets into a textbook, which was the only one of its kind at that time. Some reviewers were appalled at seeing the electronic charge expressed in coulombs, but the book was widely adopted, largely because, I believe, of the dynamic growth of interest in solid-state devices. (The second edition, years later, was a much better book, since it profited from more teaching experience, including extramural teaching, and was not so rushed.) The book made many friends and opened many doors for me. Among other outcomes of door openings, I became the chief physics advisor for John

Wiley and Sons, and when they became a public company in 1965 I became one of the first two "outside" members of he Board of Directors.

I also did considerable consulting in industry and began service on Washington committees. For a few years I was the Editor of the *Journal of Applied Physics* , which made few friends and many enemies: Since there was no society behind it, I had to reject more papers than I could accept. I used a sabbatical at Oak Ridge to immerse myself in radiation damage in solids. A sabbatical at European Research Associates in Brussels gave our family an interesting experience, gave me a chance to lecture in several European laboratories, and produced a paper on the motion of charged dislocations in LiF.

There was still a lot of life in BaO but I became intrigued with the promise of using phonon scattering at very low temperatures as a tool for studying imperfections in non-metallic crystals. Although this approach started a lively program with excellent students, I was spending more and more of my time working for others, especially getting money for electron microscopes, a helium liquefier, and other major equipment and upgrades to the electrical supply and plumbing of an impossible building, Rockefeller Hall. We organized the Laboratory of Atomic and Solid State Physics to provide a base for such activities and especially to get, somehow, a new building, and I became the first Director.

With that as a launching pad, we entered the competition in the Advanced Research Projects Agency (ARPA) for an interdisciplinary materials science contract. We won one of the three first, and the largest, of these contracts. I believe the principal edge we had in the competition was that we had had an outstanding group of Ph.D. students since the War, we knew where they were and what they were doing, and they were almost all working in areas that made full use of their graduate education. I became the first Director of Cornell's Materials Science Center supported by that contract. My central task was acting as the client for a new building. ARPA contracted for $10 million over 10 years for a building, but more money was needed if we were to get a prime position on the campus, and ultimately the building, Clark Hall, housed elements of six different academic departments and centers.

I imagine you have already seen what was happening to my career. I had helped bring in very able faculty and the initial research support for them. They were brighter and better physicists than I was. As I drifted more and more into the role of facilitator, it was obvious that the better graduate students were going to work with them, not with me. So, I really had no choice, I became an administrator.

I pause again to take stock of what I had learned. I will not dwell on the lessons that all of you who do experimental physics have also learned, such as how to fight with the physical plant and purchasing departments, how to survive with machinists and glassblowers, and the necessity for back-of-the-envelope accounting of research funds. I reinforced my view about the difference that taking responsibility makes, but in addition learned the burden that accrues from needing to take responsibility for the actions and inactions of **others,** associates and colleagues, I learned in my research that I should always think about a question first, and only then go to the literature; in that way I had the greatest chance of creating a fresh, or even novel, approach.

I learned the absolute necessity of having at least one **other major activity**, in addition to and largely independent of one's employment. Years later, Dean Kenneth Clark invited me each year to talk to new faculty here. I pointed out to them that there would certainly be occasions when they were profoundly frustrated in their main line, perhaps because of being scooped in their scholarly work or research, or not appreciated by students, or unfairly treated by chairman, dean, or president. I strongly urged them to have a second life, a serious interest which, although it, too, would have its frustrations, would not likely have its frustrations occur at the same time.

I learned that decisions in a university are made in different ways at different "heights" in the academic hierarchy. Once when I had gone to Hans Bethe for advice about taking on a Cornell directorship, he told me that the higher one goes in a university, the more **wheedling** is required to make things happen. I learned that, as usual, he was both right and perceptive. (Hans himself was much too bright to have become an administrator.) I learned that when the depredations of academic politics became unbearable, a re-reading of F. M. Cornford's little gem, *Microcosmographia Academica* would bring back steadiness, tranquillity, and even smiles.

To return to my narrative, we are now at 1963. I was recruited by Jerome Wiesner and Harold Brown to become Director of ARPA. The argument was that they and ARPA had delivered a big boost to Cornell, and now it was only fair for Cornell to deliver me. I was attracted to the Kennedy administration because of its commitment to controlled response, rather than massive retaliation, as the basic posture of the U. S. in the nuclear age. I knew that ARPA, even though a small agency, was involved in some fascinating operations—I later learned, of course, that there were far more that I had **not** known of.

ARPA was only 5 1/2 years old at that time. It had been created in the post-Sputnik rush to push the space program, and for a short time (until NASA was created) ARPA managed all the big space projects. It could, and several times on my watch did, write checks the same day a project was proposed. The Director of ARPA had great latitude, since he was only the second level down from the Secretary of Defense. Our biggest treasure, however, was our relations with the science and engineering community. Contractors and potential contractors visited us and we visited them. We established the practice that, no matter how hungry or even greedy a visitor was, there was **always** something useful to be learned from him, usually about interesting things going on in his shop. (This contrasted sharply with the experience I had had of visits to NSF, where the program managers dominated the sessions with whining about their budgets and their file-drawers full of proposals and never learned anything interesting that was going on in the field.)

Of the dozen or so ARPA programs I shall mention only two.

The first was nuclear test detection, which had been my principal interest in joining ARPA. In the summer of 1963 negotiations with the USSR for a nuclear test ban were in the final stages, and I was commuting to Washington to participate—my predecessor had left in May. ARPA had two roles: 1.) We were harvesting the work that had been done—mostly at the National Laboratories and the Air Force, much with ARPA support—to convince the Joint Chiefs of Staff that they could testify in October Senate hearings that they could assure the safety of the country in a test-ban environment. 2.) We were heavily involved in the attempt to include in the treaty underground tests—the fourth environment— where the identification of nuclear explosions and distinguishing them from earthquakes was still in a primitive stage, and the argument was heated over how many on-sight inspections would be needed. The activity we helped in our first role was successful, and the three-element test ban—atmosphere, oceans, and space—was signed and ratified; it was the first glimmer of hope in the Cold War. The second failed because ultimately the decision did not rest on science and technology; neither side really wanted to give up testing, and so the comprehensive test ban was deferred for over two decades.

The ARPA program continued with research on underground explosions and with the VELA satellites to explore space-based detection of nuclear tests in the atmosphere. Although the satellites were never intended to be an operating system, they endured far beyond their expected life and did in fact detect a clandestine South African test—politics, however, obscured that success.

The other ARPA program I shall mention was initially a much smaller program called "Command and Control Research." ARPA had negotiated a mission to develop computer aids to battlefield command and control; since all three of the Armed Services were possible beneficiaries and since it required close contact with the science and technology communities, it was a natural ARPA program. The real genius in charge of the program was J. C. R. Licklider, who, incidentally, was a Ph.D. in psychology from the University of Rochester. Lick convinced me that the program should be expanded and become more general. Here was a case of pulling fundamental, researchable questions out of an initially applied problem; this is just the process I described earlier, but here on a grand scale. We changed the name to Information Processing Technology and broadened the program to explore a wide range of computer possibilities that were germinating in universities, in a few small companies, and in Licklider's mind. Among these were time-sharing, networking, natural language articulation with the computer, teleconferencing, and computer graphics. I am sure all of these developments would eventually have happened without ARPA attention. But the bulk of the computer industry—Big Blue—was not interested, and ARPA must have accelerated by many years the computer uses and methods that we now take for granted.

To take stock again, first, the responsibility theme: The ARPA experience intensified my understanding of the difference between advising the Government and directing a Government agency, I had done a great deal of the former, but in that role I could always pack my briefcase and return with relief to my university. Now, in ARPA, there were decisions that must be made, on time, and with attention to all the implications. And, in the midst of an orderly approach to an **important** opportunity, the red telephone would ring with an **urgent** problem, and the urgent always drives out the important.

I learned, almost for the first time, how to behave when I did not have freedom of speech. Much later, **in this room**, I had an especially bitter reminder of this.

Hardly an hour went by in my two years at ARPA that I did not use something from my physics experience.

I learned what made ARPA such a lively and vital operation, although I would still never have predicted its remarkable vitality, celebrated last spring at its fortieth anniversary party. First, The directors and assistant directors have all served for very short terms; the average of directors has been about two years, which is also the time I served. This pattern produces constant rejuvenation and renewed introspection of what are the most interesting problems and most promising approaches. Second, ARPA was **expected** by the Secretary and his deputy to be such an agency and treated as such. You may remember that in Shaw's original *Pygmalion*, and repeated in *My Fair Lady*, Eliza says "the difference between a lady and a flower girl is . . . how she's **treated**."

To return again to my narrative, in 1965 I returned to Cornell, as Vice President for Academic Affairs. I continued to teach physics, but it had to be at 8:00AM, since the President owned me for the rest of the day. By then I had worked my way down from teaching advanced graduate courses to teaching the sophomore course for physics majors, plus a few chemistry and mathematics majors.

My responsibilities were mostly working with the deans on academic appointments and tenure decisions, improving undergraduate education–especially in the College of Arts and Science, and being the administration contact and budget officer for the Art Gallery, the Libraries, and the Admissions Office. But I also ran errands for the Provost and President. I learned a great deal about university administration and working with trustees. I also learned much about New York State financing, because of the four "contract colleges" at Cornell.

In January 1966 an incident occurred that made demands on the **breadth** of my education. I was asked to chair the Search Evaluation Committee that was organized to oversee the search for a hydrogen bomb which the Air Force had dropped over the Mediterranean coast of Spain. The refueling boom of a tanker had, because of violent turbulence, driven through the bomb bay of the B-52 being refueled. Three of the live bombs had floated down under their parachutes and had been recovered, but several weeks of search had not found the fourth. I believed I was chosen because I still "had all the tickets"—clearances–from my recent ARPA duty, and because I was expendable—the Secretary was certain there would be a bruising Congressional inquiry if, as seemed likely, the bomb was not found, and anyone who had to explain to Congress that the search was being abandoned would be a sacrificial lamb. Search priorities and evaluations required knowledge of bomb physics, material physics and chemistry, metallurgy, corrosion chemistry, meteorology, photographic science, aerodynamics, aeronautical science and engineering, radiation biology, geology, oceanography, law and political science, and even agriculture and animal husbandry. In the end, it was psychology that was most important. We tested many witnesses and, against much conflicting advice, placed our bets on a single fisherman because of his competence as demonstrated on some simple tests. The bomb was found, hanging by the parachute shrouds in water nearly as deep as could be explored, at the edge of a much deeper chasm. Again, good luck!

Then in 1968 I was asked by Joe Wilson and Allen Wallis to come here as Provost. I had known a good deal about the University: While I was a student, Joe Platt was a fellow graduate student at Cornell and Bob Marshak was a very senior graduate student. There had been several joint brunches of the Rochester and Cornell physics departments at Krebs Restaurant in Skaneateles. I had lectured in the Eastman Theatre for the Rochester chapter of the American Chemical Society. I knew Mac Hazlett from organizing the Five Associated University Libraries and Roy Thompson and Ernst Caspari as trustees of Associated Universities Inc. All of this experience attracted me to the University.

But it was perhaps a foolhardy decision. My predecessor had been forced to resign, and the campus was in an uproar. I became what would be called in a corporation the "Chief Operating Officer." But a university should not be forced to have a COO! The problem was that the times demanded that that function be carried out. Many of our trustees believed that we should have a cyclone fence around the university and armed guards at the gates, but the faculty and administration were committed to an **open** university, where every position and point of view could get a respectful hearing. That clash was bad enough, but an even worse one was the clashes among groups that argued by slogans at the bumper sticker level, whereas the university was committed to **raising** the level of discourse.

Well, I don't want to take your time with more about "the bad old days," but I will be at your disposal for questions. Allen Wallis called the times "the great campus craze of the sixties." I called it, perhaps over generously, the "student shenanigans." Although the draft and the Viet Nam war played a prominent part, there was clearly more to it than that, since the worst student riots were in Paris. At each American University the focus of excitement was different; here confrontations developed around the Center for Naval Analyses, the Genovese appointment, and numbers of black faculty. The spirit of a university campus simply must change every few years, and each year after about 1970 was a little better, here and at other places. By "better" I mean we did not have to concentrate on keeping the peace and could address educational problems. The transition came none too soon, since we had to become preoccupied with inexorable budget problems.

In 1970 Allen Wallis thought I was going away to become President of Brown. As a counter move, he got the Board to approve a change of titles, in which he became Chancellor and I became President, thereby creating considerable confusion. There were only two things wrong with this perception: Brown wasn't going to ask me and I wasn't going to accept if asked.

By 1974 I had become Chief Executive Officer, as well as President, with a full plate of responsibilities.

I could not continue to teach physics. Not only would it have been hard to find the time, but also it would have been a disservice to students, since I could not continue to enrich my teaching, at whatever level, by contact with the frontiers of physics. I continued to do some physics extramurally as Chairman of the Defense Science Board and later as Chairman of the General Motors Science Advisory Committee.

I learned a vast amount in these years, some of which I wish I had not learned. The whole field of medical education and practice was new to me. The responsibility theme appeared again, even more virulently; I was amazed that the media, and even many of the University's friends, were unaware of the lack of symmetry in a conflict between a person with responsibility and one without. The major new pattern I learned was the difference in the behavior of the same individual when he was constrained by his ties to a **constituency** from that when he was thinking and acting as an individual.

I retired in 1984. I could have stayed on, but I had resolved not to stay a day longer than allowed by the faculty rules that had been in effect earlier. My successor gave abundant signals that he wanted to see as little of me as possible. That was fine with me, and I went off to other things. But I was delighted when the Department of Physics and Astronomy offered me an office.

By 1984, partly in preparation for retirement, I had joined several corporate boards, continuing the Wiley Board and adding United Aircraft (which changed its name to United Technologies), Xerox, Sybron, and later Bausch and Lomb. 10
The Commonwealth Fund, a medical foundation, had given us several million dollars for a pre-medical education program, and Commonwealth—like Wiesner and Brown—said that since they had done that, I ought to serve on their board. Similarly, after the meeting at which Xerox gave the pace-setting gift to launch our $108 million campaign, Peter McCollough took me aside, and you know what! Retirement from the University came just in time since various crises in these companies demanded increased participation, often with a technical content, and I was invariably the only scientist or engineer on each board.

I will take your time to report only a few highlights of my other post-retirement activities.

I chaired a committee for the National Academy of Sciences to re-organize the Institute of Medicine, the third National Academy (after Science and Engineering), which had been collapsing. Then I chaired a group to advise the Harvard Community Health Plan—the talented group included Bob Freeman of the Eastman School of Music and Betty Friedan. For twelve years I was a member of the International Science and Technology Advisory Group for the Premier of the Republic of China, Taiwan. I chaired the Technical Advisory Board for the Link Foundation, was a board member and member of the Executive Committee of the Institute of Defense Analyses, and did much advisory work for the Department of Energy and the Department of Defense. I wrote a little low-level book for business people that was published last year by the University of Rochester Press. And there were a good many other activities. As you can see, it was a pretty miscellaneous array, yet all these activities shared the physics underpinning. These were all in addition to continuing to help in raising funds for the University.

But the most interesting and challenging work was two missions for the International Executive Service Corps, to Kazakhstan and to the Republic of Georgia. In Kazakhstan I was to rejuvenate the Kazakh Academy of Sciences, which turned out to be impossible. But I succeeded in making some connections to the West and in helping individuals to end their isolation from world science. In Georgia I helped five young, intensely patriotic engineers and scientists to organize a think tank to conduct studies for the rehabilitation of Georgia. This was a remarkable experience because of their dedication and because my point of attachment was Khabashvili, Sheverdnadze's principal deputy, and because I had a productive one-on-one session with Sheverdnadze himself. Among other things, I learned from these two experiences that, far from being a union of republics as advertised, the USSR had been a brutal colonial empire.

Now, almost all of this work has come to an end. I am still the head of a group, the Environmental Literacy Council, that is attempting to improve the quality of environmental education in the schools, K-12. I guess it is a continuation of my progression from teaching advanced graduate courses to the sophomore course for physics majors, and now to kindergarten. That has to be 11 the end of the line!

I want to close with some remarks about physics. First, I once tried to market the thought that physics was "the Greek of the 20th Century." By that, I explained, I meant that it could play the same role at the heart of an education that the study of Greek played in a classical education in the 19th Century, where Greek was the core of disciplined study. The phrase never became a household word, but I think the thought is still valid, and I hope that today I have shown an example, in reporting how physics underlay my participation in the careers I have described. Physics is, of course, much more than just an exercise in disciplined study, since it illuminates a major part of our interaction with the physical world. (Parenthetically, I tried to peddle another phrase, "relax into action," in the confrontations of the sixties, with similar lack of success.)

Second, I admire those of you who are working on the great, seminal problems of particle physics and cosmology. I regret that long ago I ceased to understand that frontier. Now, I cannot even comprehend it at the *Physics Today* level. I do not have much confidence that there will soon be a "theory of everything" as promoted in British pot-boiling books. It seems to me likely that there will be many fascinating discoveries ahead and much to be explored before "everything" is cleared up.

Third, the same is true in the many other branches of physics. Some of these are often denigrated as "not exciting," or as "only chemistry." But there are profound mysteries here, too, and basic physics research questions are hiding in applied problems. My favorite is ferromagnetism. The magnitudes of the atomic constants h, e, and m are just such that interaction among the interatomic spacing, the broadening of the 3d energy band, and the exchange integral produces ferromagnetism in iron, cobalt, and nickel (plus copper alloys with the same interatomic spacings, and gadolinium and dysprosium with 5d bands). Is this accidental? Only a slight change in any of these constants would have made the world be without ferromagnetism, and that would have been a quite different world—very little technologically beyond the world of the year 1800.

Finally, if you or your colleagues can explain these values and thus why ferromagnetism occurs, you can then tackle what may be the greatest mystery of all, and one to which physics must eventually contribute. I refer to the mind-brain problem. Although a phenomenal amount is known about the brain, essentially nothing is known about how the mind tells the brain what to be interested in, and for that matter, what is the mind anyway? This problem is still in roughly the state it was left by Descartes, 350 years ago.

I wish you luck, thank you for hearing me out, and good-bye.

Tip?

Not included

- C_2H_5OH & WCTU speaker
- DNR history & impact
- 1960 ± when one could talk ~~student~~ output & Govt.
- ~~[illegible]~~
- Emma, and "How did you get to Washington?"
- Kermit Gordon "go over his head"
- UR History Dept.
- time the cops came on campus
- laser lab
- black students in Faculty Club
- GUIR
- cinders in Rock. Hall
- Sybron kidnapped by 5 top execs.

Sun. Roft.
Tues. dinner with Freemans

N 4:30 Sun.
Niles

---

461 2040 Foore

Latham for the weekend?

By 1984, in preparation for retirement, I had gained several corporate boards, continuing the Wiley Board and adding United Aircraft (which changed its name to United Technologies), Xerox, and lota Bausch + Lomb. The Commonwealth Fund, a foundation, had given VR several million $ for a pre-medical education program, and (like Wessier Brown) said that since they had done that, I ought to serve on their board. Similarly, after the meeting at which Xerox gave the pace-setting gift to start our $108 million capital campaign, Peter McCollough took me aside, and you know what! Retirement from VR came just in time since various crises in these companies demanded increased participation. I was invariably the only engineer or scientist on each board.

I do not have time to mention all my other careers in "retirement" but will give a few highlights.

First, I chaired a committee for the NAS to re-organize the Inst. of Medicine, the third National Academy (after Science + Engineering) that was failing. Then I chaired a group to advise the Harvard Community Health Plan. The talented group included Bob Freeman of ESM and Betty Friedan.

Era or Tsa Tsa
Gabor

All the several "careers" are mine
Autobiog can be pretty boring stuff, except to the author
Start cornfield - inbreds
DS
Calculus
Part A exam auspicious
Thesis - classified - Kodak - BTL
War - Penn / Princeton
Class - V-12
Camden

Learn accounting early.
Respect for applied problems
Satisfaction in teaching
~~Diff between~~ Diff respons. matrices.
Hilbert Space / Reson. cavities excitation
Connections.

Add so far

To 1946

Return to Cornell
Love string & sealing wax → electronics
→ Wooden bldg - cinders → electronic properties of nearly perfect crystals
One of first ONR Contracts - summer of 1946 - contracts, but "work statement" was "such as."
Excellent students
Other agencies followed ONR lead - notably AEC + NSF
→ Book
J.A.P. - more enemies than friends - no society behind
more time raising $ to keep going

[illegible] again, but now associates, two
Need for a second [illegible]
think first, then go to literature.

BaO
Sabbatics at ORNL (rad. dam. in solids)
and ERA (charged dislocations in LiF) (14 nationalities)
Phonon scattering.

→ becoming apparent that people I had helped to bring in were brighter & better physicists.
MSC
Clark Hall

I was a member of the Science & Tech Adv. Group (STAG – unknown in U.S.), group advising the Premier of ROC (from 1986 to 98). I became a member of the Board & Exec Com of IDA and chaired the Adv. Bd for the Link Foundation. I and did much advisory work for DoD & DoE. I wrote a little low-level book for business men that was published last year by U. Press. And others.

But the most interesting, and challenging, work was two missions for the Int. Exec. Serv. Corps, one to Kazakhstan & one to the Rep. of Geo. In K. I was to rejuvenate the K. Acad. of Sciences, which was impossible. But I succeeded in connecting it to the West & helping many individuals rise above their isolation. In Georgia I helped five young, intensely patriotic engineers organize a think-tank to conduct studies for the rehabilitation of Georgia. This was a remarkable experience because of their dedication & because my pt of a was attached to Schevardnadze's deputy, Khabashvili, and became I had a productive one-on-one session with Sheverdnadze himself.

Now, almost all of this work has come to an end. I am still the head of a group, the Envir. Lit. Council, attempting to improve the quality of environmental education in the schools, K-12. I guess it is a continuation of my drift from teaching advanced graduate courses, to the sophomore physics major course, now to kindergarten. That has to be the end!

I want to close with some remarks about physics. I once tried to market the thought that physics is "the Greek of the 20th Century." By that, I explained, I meant that it could play the same role at the heart of an education that the study of Greek played in a classical education in the 19th century, where it was the core of disciplined study. It never became a household word, but I think the thought is still valid, and I hope I have shown how physics underlay my participation in the careers I have described. (I tried to peddle another phrase, "relax into action," at the in the confrontations of the 60's, with similar lack of success.)

Physics is, of course, more than just an exercise in disciplined study, since it underlies a major part of our interaction with the physical world.

I admire . . . .

Where does physics come in?

"Physics is the Greek of the 20th Century" "relax into action"
Rôle Greek played in a 19th Century educ.
Habit of disciplined study
Physics is, of course, more than that since it underlies a major part of our interaction with the physical world.
It is a major piece of *education*, as distinct from *training*.

— I was chm because I had the credentials & was expendable
Search Evaluation Comm.
Close with Jan 1966 Palomares Physics: aerodyn, fluid dyn, meteorology ~~materials~~ deformation of solids, radioactivity & shielding, physics of nuclear weapons. Plus Chem (corrosion), biology (agriculture, tomatoes), geology (lead mines)
(Gadolinium & Dysprosium) (Lanthanide) (4f & 5d)
Orts: went to same place
You never know when the breadth of your educ. & experience will be needed.

Finally, a message about physics:
I admire those of you who are working on the great, seminal problems of particle physics & cosmology. I regret that long ago I ceased to understand that frontier. Now I cannot even comprehend it at the Physics Today level. I don't have much confidence that there will soon be a "theory of everything" as advertised in British pot-boilers. It seems likely that ~~there~~ will be many fascinating

discoveries ahead and much to do.

~~But~~ And the same is true in other ~~parts~~ branches of physics, often denigrated as not exciting, or "only chemistry." There are profound mysteries there too. My favorite is ferromagnetism. The atomic constants h, e, and m are just such that the interaction between the inter-atomic spacing, the broadening of the 3d ~~the~~ energy band, and the exchange integral produces ferromagnetism in iron, cobalt, & nickel (plus Cu alloys with same interatomic spacing, plus Gadolinium & Dysprosium (5d)). ~~It would seem that a~~ Only a slight change in any one of these would have made a world without ferromagnetism, which would be a quite different world — very little beyond 1800. Is it only accidental?

And, if you explain why these values occur, you can then tackle what surely is ~~one~~ the greatest mystery of all: the mind-brain problem, which is still in about the state it was left by Descartes 350 years ago.

Wiesner recruited; JFK controlled response vice massive retaliation

ARPA

we had received one of the first 3, & the largest; ARPA Mat. Sci contracts incl. 10 M for a Bldg.

- Joint chiefs & nuclear test detection — made possible 1963 3-environment treaty
  — prepared for comprehensive (UG) treaty

- CC Res → IPT: Reaching into an applied problem & pulling out a basic question. On a grand scale.

Big Blue not interested

Would have come anyway, but much faster with ARPA stimulus

time sharing   natural language   teleconferencing

→ networking   graphics

Internet an outgrowth of ARPANET, after my watch, but by people I brought in.

→ Everyone, no matter how hungry, had something to tell us. cf. NSF

Learned: 1. Diff. between packing briefcase & leaving & staying with monkey on back. Respons. again. [Flows of Responsibility, again]

2. A lively agency, steered between ambitious idiots & GS time servers by constant rejuvenation — 40th ann. recently, still vital. 2 yr ave tenure of Directors (one: 6 weeks)

3.

3 yrs as V-P Acad Aff at Cornell
Soph. physics course for phys majors (plus some math & chem)

Palomares

1968
Provost had been forced to resign
Joe Wilson & Allen Wallis recruited me
Skaneateles; ACS East. Electric; Hazlett FAUL; Ernst Caspari AVI
Platt & Marshak Thompson –
Ph.D 42 39 Cornell

GM Sci Adv. Comm. Chas. Campbell Robt Morison

DSB

CNA

COO : Univ. shouldn't need a COO
" didn't want to deal with budget
" wanted to call the cops; we never did
Open campus. Many T's thought we should have a cyclone fence

Each year a little better (except finance).

1984 Retired, though could have stayed
But Hess & I had opposed Claude Pepper legis
(we and Princeton only ones that August as he
sneaked it through Congress without hearings)
So I would not stay one day after "old" faculty rules

My successor gave abundant signals that he wanted to
see as little of me as possible.
So I went off to other things

Corp. Boards
Commonwealth
Sybron (chm.) 5 top execs. attempted kidnap
IOM

HCHP
Chinese: STAG CFPEC
Greece + sailing (sold our sailboat soon after coming to Rock)
Kazakhstan & Georgia IESC
IDA Link Found.
DoD/DoE

Fusion policy

Book

Environ

Coming winter really retired – a little frightening.

777 1611
telephone repair

Lessons

- ~~Always have ≥ 1 major other interest or sub-career~~
- Keep a lively interaction with your sponsor ←
- Maintain by example teach your students to have respect for applied problems.

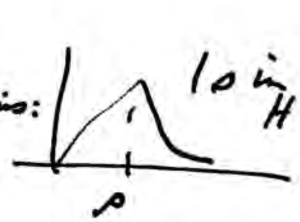
transistors — minority carriers

Concl.

- Don't let anyone tell you physics is dead, or has diminishing returns.
- " " " " " that the only worthwhile questions are the huge, seminal questions of particle physics

$$\frac{eh}{4\pi m} = \text{Bohr magneton} = \frac{e\hbar}{2m}$$

$$\rho = \frac{h^2 \epsilon_0}{\pi m e^2} = \frac{2h\epsilon_0}{me^2}$$

radius: 

exchange energy vs. Fermi energy

why does ferromagnetism exist?

Fe Co Ni + Heusler alloys

p. 327 of 2nd Ed.

Mind-brain problem: Now there is a monstrous one! Physics must eventually help - $4kTR\Delta f$?

Oct 94

A Tale of Two Countries: Taiwan and Kazakhstan

Robert L. Sproull

More or less accidentally I have learned a great deal about two fascinating countries, the Republic of China on Taiwan and the Republic of Kazakhstan. Although they are virtually identical in population--21 million in Taiwan, 17 million in Kazakhstan--they are profoundly different in almost every other respect. This talk will attempt to report a brief comparative study of the two. As usual, I shall attempt to draw out some messages from the study.

But I will not delay until the end the overarching message that is scarcely surprising: Capitalism is "better" than socialism in every important dimension: economic prosperity *vs.* poverty, freedom *vs.* oppression, satisfaction *vs.* frustration, and many others. I will identify several other messages at the end of my talk.

I will first explain my connection with these countries, then outline their geographies (especially of Kazakhstan, since that may not be familiar to you) and histories, then comment on their problems and futures, and finally extract messages.

I

I have been going to Taiwan once or twice a year for ten years as a member of an international team called the Science and Technology Advisory Group--"STAG," consider the impossibility of such a name in the politically correct United States! We report once a year to Premier Lien Chan, who hears brief individual reports from us and then sits with us foreigners and our Chinese counterparts for a half day of presentations and discussions. He and his ministers take our advice seriously. We foreign advisers are probably not very useful now, but a decade ago my predecessors were extremely effective in guiding the development of Taiwan.

The Kazakhstan connection was quite different. Last year I was invited to spend a month in Kazakhstan as a volunteer for the International Executive Service Corps. My mission was to help connect the Kazakh Academy of Science to the West, to help science and technology in Kazakhstan recover from 75 years of isolation.

II

You all know the geography of Taiwan, an island about 80 by 230 miles situated 100 miles off the Chinese mainland. But you may not realize that it is largely mountainous and that only about 5% of the land is suitable for agriculture or cities, an area about the size of New Jersey. It has no oil or gas, no metallurgical coal, and in fact almost no natural resources of any kind.

Taiwan has spent most of its recorded history as the home of small Malay Indian tribes. In the 16th, 17th, and 18th centuries it was fought over by Chinese, Portugese, Spanish, Dutch, and non-governmental pirates. In this period it was known as "Formosa," a rich island exporting valuable camphor, but it was "rich" only because it needed to support only a tiny population. The population gradually became mostly Chinese. In 1895 it was annexed by Japan. As a result of World War II it was returned to Chinese rule, and then in 1949 it became the refuge of Chiang Kai Shek and his Kuo Min Tang when they were driven from the mainland by the communists.

The KMT has gradually relinquished its hold. For many years the members of parliament claimed that they had been elected from regions of the mainland, and, since it was not possible to hold new elections there, they continued to serve. Within the last four years this "rotten borough" fiction has been replaced by Taiwan elections, a minority party has flourished, and a President, Lee Teng-hui a Cornell Ph.D. who was born on Taiwan, has taken office. The minority party has a good chance of becoming the majority in the elections later this fall. The political situation is essentially democratic and is realistic with one glaring exception: The Government still claims it is the legitimate government of *all* China and that eventually the ROC and the PRC will be merged under its governance. Although I scrupulously refrain from discussing politics in Taiwan, I am confident that *everyone* understands that this is just a convenient mythology, that the 21 million are *not* going to take over the 1500 million. Meanwhile, the ROC is beginning to behave like an independent country. It is already accepted as such, and in some instances is a leader, in several Asian basin organizations, and it is attempting to enter GATT and the UN, against, of course, powerful objection from the PRC.

Massive economic aid was given by the U. S. to the ROC in the early years following 1949. In addition, the U. S. rattled sabers and sent military aid when the PRC and the ROC exchanged artillery barrages from the islands in the Taiwan straits. The Taiwanese are appropriately proud of being the first country to voluntarily give up U. S. economic aid as their economy began to prosper.

But the initial prosperity came from selling junk with a high labor content. The ROC Government was wise enough to see that there was no future in that trade and developed strong schools, colleges, and technical schools as it perceived its future was dependent on an educated and trained population. Many of their finest students came to the U. S. and profited not only from advanced (usually science and engineering) education but even more importantly from positions for a decade or so in the most technically advanced U. S. corporations. Now that the infrastructure has been built up on the island and many high-tech companies and even universities are offering upper-level employment, many of these are returning.

Taiwan still produces some junk, but the manufacture of most window-shades and Christmas tree ornaments has moved to Korea and mainland China. The ROC believes its future lies in products with a high engineering content that take full advantage of its educational system and the hard work of highly trained and motivated people. With almost no natural resources and little arable land, Taiwan's only possible strategy is to compete in world markets, and successful competition in high-tech products will mean higher wages and a higher standard of living in Taiwan.

The transition is being made entirely under market forces and the initiatives of a large array of entrepreneurs; there is no "technology policy," as the term is understood in the U. S., wherein the government attempts to outguess the market and to pick winners.

The key element in the Taiwan success story has been the *outward-looking* approach by the leaders of government, industry, and education. They observed and responded promptly and sensitively to developments everywhere in the world.

Now to Kazakhstan, and I guess I should first explain the geographical setting. The "roof of the world" is a giant horseshoe, beginning with the eastern Himalayas and Mt. Everest, going west and then north through the Karakoram and Hindu Kush, turning east and ending with the Tien Shan range and finally Mt. Pobedy at 24, 200 ft. Enclosed in the horseshoe are Tibet at the south and Sinkiang at the north.

Immediately north of the Tien Shan mountain range is Kazakhstan, a country with the area of all of the U. S. east of the Mississippi. The Tien Shan effectively blocks surface communication on its southern border. On the west, north, and east is the Russian Federation, the giant core of the old Soviet Union that extends through 160° of longitude, eleven time zones, from Poland to Alaska.

The capital city is Almaty, the Kazakh spelling; when in the USSR it was called Alma Ata. It lies snuggled up against the Tien Shan on its southern edge. The famous Steppes of Central Asia begin at the northern edge of the city. Almaty was a stop on the Silk Road from China to Venice and Persia, although not as famous as the legendary cities Tashkent, Samarkand, and Bukhara to the west.

Almaty is a long way from anywhere. It is, as you know, 6 1/2 hours by non-stop jet from Kennedy to Frankfurt; it is 7 hours by non-stop jet from Frankfurt to Almaty.

In sharp contrast to Taiwan, Kazakhstan has immense natural resources. Its oil and gas reserves are reputed to equal or exceed those of the

U. S., which makes them luxurious for a population 1/15 as large. It has coal, copper, silver gold, lead, tin, zinc, and rare earths. The Steppes supply meat, wool, and wheat, and cotton is raised on irrigated land near the Caspian Sea. Kazakhstan will be a rich country if it can get its act together and if the Russian Federation does not challenge it.

Until the 19th Century, Kazakhstan was not a country but only an area of the great Steppes, thinly occupied by nomads. Although the Silk Road traversed along its southern border, that traffic did not produce substantial interaction with other countries. Early in the 19th Century Imperial Russia annexed Kazakhstan and the neighboring "-stans." In the three-way war of 1917-21 (reds, whites, and Kazakhs), the reds won. Although the USSR was nominally a "union" of quasi-independent "republics," in fact it was still a Russian empire.

The 75 years as an element of the Soviet Union have inflicted enormous damage on Kazakhstan. There are three related aspects of the damage:

1.) **Colonization** "Great Russia" (Moscow, Leningrad, European Russia) treated Kazakhstan (and, I believe, all the other "-stans") as a *colony.* Raw wool was sent to Russia to be made into cloth and clothes; partially refined ("blister") copper was sent to Russia to be electrolytically refined and made into wire, pipe, and electrical machinery. The skilled jobs were in Russia, and the products were sold back to Kazakhstan with the "manufacturing value added." Almost no manufacture of consumer goods was established in Kazakhstan, which was treated by Russia just as the Belgians treated the Congo or the British treated Rhodesia.

Further, Russia established its space-launch facility at Baikonur in Kazakhstan where from time to time accidents ruined some real estate (just as the French still use French Guiana). Much worse, the Russians did most of their nuclear weapons testing at Semipalatinsk in Kazakhstan (just as the French use their Pacific Island colonies). This area is now dangerously contaminated with radioactive fission products, and Russia would like to walk away and leave all the problems to the Kazakhs. The technical and managerial positions at both of these installations were always held by Russians.

Perhaps your reading was more thorough than mine, but until the break-up of the Soviet Union I was unaware of the colonial pattern. I did read, of course, about Russia's depredations in Hungary, Czechoslovakia, Poland, and the Baltic republics. But Russia's colonial exploitation and suppression of the lesser republics to the south and east has now been revealed as even more damaging.

2.) **Isolation** You well know the isolation of the Soviet Union from the world community. The "Iron Curtain" was only part of this. What trade there was with external countries was carried out with artificial pricing (like subsidizing Cuban sugar) and so the Soviet economy was not tested and made realistic by world competition. Eventually this isolation was tempered by radio, television, movies, and travel, and these plus the failing economy are usually credited with breaking up the old order.

Kazakhstan shared in this isolation, but its isolation was even more profound. This was partly because of its remoteness but also partly because of the domineering Russia. In science, for example, if an invitation was extended to the USSR by a Western country to an important technical conference, it would be exploited by Russia and never reach any of the outlying republics.

There is an odd and discouraging twist to this isolation. The state-run television has only three stations, two of which are quite approriately local, Kazakh-language stations. The third, using the opportunity to broadcast the best the world has to offer, is MTV! I speculate that perhaps officials think this is a way to maintain contact with youth. A sadder speculation is that officials may chose MTV to discredit the U. S. and the West.

3.) **Inward-looking** This is related to isolation but it is not exactly the same. The attitude in the economy, in literature and the arts, and in science and technology is to survive or excel in competition with only other *Kazakhstan* efforts. In science, for example, people publish in Kazakh-language and some local Russian-language journals and award each other prizes. They seek to be members of the Kazakh Academy of Sciences and believe that somehow being an "Academician" entitles them to recognition anywhere. They believe that they should be supported doing pure research, unrelated to possible applications, when their research is inconsequential by world standards. There are, of course, many bright young people, but they are held back by this inward-looking attitude. Some are only now beginning to realize that there is another, greater world "out there," but the realization that their work is not world-class is a traumatic experience.

III

What of the *futures* of these two fascinating countries? I believe Taiwan's emphasis on education, hard work, and entrepreneurship will serve it well as it competes with Japan and the other "little tigers of the Pacific" (South Korea, Hong Kong, and Singapore). Its increasingly broad-based political system seems to be secure and resilient. It must deal with a small island's problems of waste management, pollution, and congestion as the standard of living continues to rise and automobiles replace motorbikes, but it has a realistic view of these problems and is aggressively tackling them.

The big unknown in the future of Taiwan is the action of the Peoples Republic of China, and that in turn is likely to be strongly influenced by U. S. policy. The PRC's actions with respect to Hong Kong may give some indication as to whether it can live indefinitely with a nearby demonstration of the success of a Chinese region with economic and political freedom. The U. S. knuckled under to PRC demands that Taiwan have no embassies or even consulates in the U. S. and not be represented in the U. N. Whether this U. S. policy was necessary in the negotiations leading to recognition of the PRC and whether it is necessary now are questions about which I have no competence. But I do believe that if the U. S. shows signs of softening or compromising in its support of Taiwan, the PRC will move in rapidly and even possibly brutally.

But nevertheless I am highly optimistic about the future of Taiwan.

The future of Kazakhstan is also clouded by a huge question about a dominant neighbor. On all of the sides where land communication is possible, it is hemmed in by the Russian Federation, which doubtless is eyeing Kazakhstan's natural resources enviously. The economic and political chaos in Russia could seriously threaten Kazakhstan. As in Taiwan, the attitude of the U. S. will be critically important: What signals would we send if there are indications of the re-emergence of the Russian Empire?

Meanwhile, Kazakhstan is carrying out the painful process of privatization, complicated by the lack of factories and consequent reliance on other countries for consumer goods. For example, the buses in Almaty were made in Hungary and are in sad condition; the Hungarian factory is now closed and only cannibalizing other buses can provided the spare parts to keep the diminishing fleet going. Although people have enough to eat--bread is heavily subsidized and one buys three loaves for two cents--their living standard is extremely low by Western standards. And it is Western standards that they think they deserve. Will they have the patience to work and wait for the changes that will require not just a few years but a generation?

Privatization brings demands for managers with a totally different spirit from Soviet apparatchiks. It also requires accountants, banks, and a system of commercial law and courts. Providing these will take many years. Kazakhstan needs capital to develop its manufacturing and distribution system, but it needs skilled management even more urgently, It is beginning to get both through joint ventures, such as Chevron's venture in developing the Tenghiz oil field on the shores of the Caspian Sea. Kazakhstan is a marvelous frontier for young MBA's, and some are coming from the U. S. under the auspices of accounting firms and banks. More would help both Kazakhstan and the U. S., since we have much to gain if Kazakhstan looks to us as a helper, friend, and trading partner. The Germans are likely to upstage us, however, since they are moving in rapidly and there is a substantial German minority from former prisoners of war. Lufthansa was the first non-Soviet airline flying into Almaty. Pakistan is moving in rapidly, again with their national flag airline, and their association makes especially good sense since they are a fellow muslim country and in the air age virtually a neighbor (only 1 1/2 hours by air from Islamabad to Almaty). It is too bad that U. S. policy fragments our air service and we have no national airline that could sustain the initial losses incident to pioneering.

The political situation appears to be currently stable but somewhat precarious. The President of the Republic is Nursultan Nazarbayev, a former member of the Soviet *nomenklatura* who appears to be popular and entrenched. But what I could see of his bureaucracy was not reassuring; there appears to be little direction and much frustration and in-and-out running, with the primary objective of personal survival.

Although politically independent since December, 1991, Kazakhstan has until last winter necessarily remained in the ruble block, since their consumer goods, including much of their food, were bought in rubles. They have now courageously issued their own currency and hope to experience less inflation than the disastrous ruble inflation.

The population is about 40% Kaz akh, 40% Russian, and a wide assortment of others, including many Germans. The official language has been changed to Kazakh, but most of the managers and technical experts are Russian and speak Russian. The transition to Kaz akh will be painful and longer than they think. Somehow Kazakhstan must avoid the paralysis of a two-language society that has, for example, kept Belgium from becoming the European leader it otherwise could have been.

But most of all, Kazakhstan to survive and prosper must become less inward-looking. My sphere of contacts may have been inadequate or misleading, but the scientists and engineers I associated with were not working hard, or not working at all, and were working with abysmal equipment on problems long since solved elsewhere, and yet they were confident they were doing world-class work. I am afraid they, and perhaps the whole country, faces a serious catharsis before they and it emerge as contributors and competitors on the world scene.

IV

So, here are two countries, of the same population size but otherwise sharply different. Both are highly promising, Taiwan because of its educated and effective work force and its national spirit and direction, Kazakh because of its rich natural resources. I hope I have intrigued you enough that you will watch closely how they develop.

Now, in closing, I should liked to extract some messages from this brief comparative study. I may be wrong; I was educated as a scientist, and the essence of science is acknowledgement of the capacity for error. But you can draw your own conclusions.

First three messages from Taiwan:

1,) Respect for hard work, family, and education still can produce a flourishing society, even in a region with no natural resources.

2.) What was a "rich island" with a few tens of thousands of inhabitants becomes disastrously poor with millions unless totally different forms of wealth are created. This is a lesson for the whole world in the 21st Century.

3.) The nationalist, militarist control of Taiwan by the KMT, which almost all of us in the U. S. deplored, may have been necessary during the siege by the PRC and the subsequent economic development and gave way in time to a free, open, democratic society.

Next three from Kazakhstan:

4.) Isolation and inward-looking are heavy burdens, impeding development and leading to dangerous over-confidence.

5.) Capitalism, with all its faults, wins handily over socialism.

6.) As Dexter Perkins presciently said to the Club after the 1962 Cuban Missile Crisis, "nationalism is a stronger force than socialism."

Finally some messages for the United States:

7.) It is unfortunate that the U. S. does not have a national airline to aid in projecting U. S. interests abroad. [Perhaps the combination of USAir and British Airways can do some of this if USAir does not become bankrupt.]

8.) The U. S. is not serving its own interests by *concentrating* so heavily on Somalia, Bosnia, Rwanda, South Africa, and the Middle East, while other countries, such as Germany and Pakistan, build favorable connections with potentially rich partners. Humanitarian aid is certainly fine (and it's too bad we don't do it more skillfully), but *in addition* we should be looking to our own interests.

9.) Finally, although the press would have us believe we never do anything right, sometimes we do: The U. S. policies of the Marshall Plan, support to Taiwan, and steadfastness in countering the USSR that led to the independence of the exploited colonies are all great success stories.

M & R talk to the Chatterbox Club, May 1994

MISSION TO KAZAKHSTAN

MLS

This is not a travelogue, but it starts with travel. We had already planned one major trip last year, but in the late winter an invitation came from the International Executive Service Corps to serve for a month in Kazakhstan. It was too intriguing to pass up.

As many of you know, the IESC is a non-profit organization, mostly of retired executives, which sends volunteers to help enterprises and governments, mostly in the third world. They describe their role as "volunteer executives delivering one-on-one technical and managerial assistance." A typical mission would involve a volunteer who had been the vice president for engineering of an American tire company who would spend a few months in an African country that had gotten into difficulty with a truck tire plant. The volunteer contributes his time, the IESC pays for transportation and some in-country support, and the host country provides in-country maintenance and services like translators.

The IESC tells its volunteers: "You must be flexible, patient, understanding, tolerant, respectful of others and their beliefs--and have a sense of humor!" We soon learned that they had those requirements exactly right!

Since geography is important for our story I should explain a little about Kazakhstan. It is the third largest of the former Soviet Union republics. Its area equals that of all of the United States east of the Mississippi. Its population of 17 million consists of 40% Russians, 30% Kazakhs, and a dozen other ethnic groups.

Kazakhstan is bordered on the north by Siberia, part of the Russian Federation which extends across Asia through 11 time zones, 160° of longitude. On the south it is barricaded by the massive Tien Shan mountain range, the northeast end of the vast horseshoe of mountains beginning in the southeast at Mt. Everest and continuing with the Himalayas, the Karakoram, the Hindu Kush, and finally the Tien Shan. Inside the horseshoe are Tibet, the high plateau, and Sinkiang, a huge desert, two of the most remote regions of the world.

The capital city, where we spent almost all our month, was called Alma Ata in the USSR days, but now the Kazakh version Almaty is preferred. It is nestled up against the Tien Shan, which rises to 15,000 feet within a few miles of the city. Thus it is near the southern border of Kazakhstan; to the northwest, north, and northeast the famous Steppes of Central Asia begin at the doorstep of the city and extend for 500 miles in each direction. Finding one's way in the city is easy because it *slopes*; uphill is always south. Almaty was one of the points on the Great Silk Road from China to Persia and Venice, although not so famous as its western neighbors Tashkent and Samarkand.

RLS

Before telling you a little about what we did I wish to put in a plug. Why might you want to become interested in Kazakhstan? Well, it is going to become a *rich* country, with *per capita* resources vastly exceeding the United States. Its transition from a Soviet Union possession to a world citizen republic will be fascinating to watch. And if Kazakhstan cannot make it successfully there is no hope for the other "-stans," Kyrgyzstan, Tajikistan, Turkmenistan, and Uzbekistan (as well as Armenia, Georgia, Azerbaijan, and Moldava). The U. S. has much to gain by helping the Kazakhstan Republic in its formative years.

Our mission was to connect the National Academy of Sciences of the Republic of Kazakhstan to Western science and to help Academy scientists publish in the West and compete for support in Europe and America.

I need to explain what the Academy is not and what it is. It is *not* like our National Academy of Sciences or American Academy of Arts and Sciences, which are honorific groups which sponsor small studies but do not have laboratories or large staffs. It *is* like our National Laboratories, such as the Los Alamos National Laboratory or the Lawrence Livermore National Laboratory. The Kazakh Academy has 8000 employees and extensive laboratory and office buildings throughout the Almaty region.

The Academy is divided into 44 "institutes." American scientists view such fragmentation as an impossible way to do science, but it is the way all Soviet laboratories were organized. The small units make it more difficult for any to become strong enough to challenge the central authority. Also, only by becoming the director or deputy director of an institute can a scientist get amenities like a car or permission for foreign travel, and so, the more institutes, the more of the better jobs.

But people in one institute are both organizationally and geographically separated from people in others who should be their colleagues, and no institute has sufficient strength to compete with science outside the country. There are, as you might expect, a few exceptions: Archaeology, Extractive Metallurgy, one of the mathematics institutes, and perhaps others.

I visited 21 of these institutes and talked with the directors, deputy directors, and the scientists they picked as their pride. I concentrated on the physical sciences and engineering, since that is what I knew best, but I roamed afield. The visits were much like those I have been doing in Taiwan for the last seven years--more of that later.

I regret that a scheduling snafu prevented me from meeting with the Archaeology Institute and seeing its museum. I believe it is the institute that is most respected by foreigners. It is reputed to take full advantage of the unique artifacts, going back to the Scythian kings, the Silk Road, and the life of the nomads on the Steppes. Similarly, the Institute for Extractive Metallurgy takes advantage of the enormous mineral resources of Kazakhstan and has a mission to add value to ores by refining, rather than ship the raw ore as they were forced to do during the Soviet period. But with a few exceptions like these two the institutes had no clear idea of their missions, of what they should be doing and why.

Morale is very low. The good people are leaving to become traders and entrepreneurs. There is a tremendous amount of unused space. I opened two doors at random, and in both rooms there were two or three people facing empty desks and staring at the ceiling. There is really no mission for almost all the institutes, no need for whatever work they might do.

The outstanding characteristic of Academy science and scientists is *isolation*. I was aware of this to some extent before going to Kazakhstan, but I was surprised and impressed with the profound isolation I discovered. Almost all the scientists and engineers are completely unaware of what is happening in their fields but not in their country. They read and sometimes publish in local journals, in Kazakh or (in the better journals) in Russian. Only rarely does anyone publish in a journal or book produced outside the Former Soviet Union, and then only in proprietary journals (journals in which contributions are solicited and are not subjected to peer review or any selection process). I did not find a single article published in a Western journal produced by a technical society (where it would have to have survived a selection process). Thus most of what little work is done merely repeats, with poorer apparatus, research done elsewhere.

There are at least six components of this isolation: The first three are general:

1.) The Soviet Union was isolated from the West. The Iron Curtain was a substantial impediment and there was little incentive for a Western scientist to communicate with people in his or her field in one of the Soviet republics.

2.) Within the Soviet Union, any invitations to conferences and other opportunities for collaboration were filtered by Moscow, and only the dregs were communicated beyond Moscow, Leningrad, and a couple of other centers.

3.) Almaty is a long way from the West. We learned that the hard way: It is seven hours by nonstop jet from the East Coast of the United States to Frankfurt; it is *eight* hours by nonstop jet from Frankfurt to Almaty, and that twice-a-week flight is an only recently added Lufthansa improvement over the tedious, unsafe, and unpleasant connection through Moscow on Aeroflot.

The next two are special to the Academy:

4.) The compartmentalization that I have already cited limits the impact of a rare trip to the West or Western publication.

5.) Young, talented people rarely get to travel. The very few opportunities that exist go to administrators and a very few senior people.

Finally there is a recent, and I fervently hope temporary, component:

6.) No Western journals have been received by the Central Library since independence in December of 1991. Before independence, the Academy Library spent $350,000 per year on hard-currency journals, and in addition it received many more journals at almost no cost through reprints illegally pirated by Moscow. It has had no hard currency since independence and hopes to get only $50,000 next year.

My mission was really to start the process of ending this isolation, to open connections with the West. I provided many suggestions of people and organizations in the U. S., the U. K., and Germany that could profitably be contacted by individuals. I gave two seminars on the way to approach publication and funding in the U.S. and wrote a handbook on those subjects. Since returning to the U.S. I have arranged for free subscriptions to a number of journals and showed the Foreign Secretary of the Academy and the Central Librarian how to get additional subscriptions.

A conscientious, able young scientist is caught in a "Catch 22" situation: He or she has no equipment or helpful colleagues. Until he gets modern equipment he cannot do the kind of work that is publishable in Western or Japanese journals. Until he has such publications he cannot get Western support. Until he gets the hard currency of that support he cannot get modern equipment. [To be sure, the young American scientist is in a comparable situation, but if he is able enough to get an academic or industrial research laboratory appointment, his senior colleagues get him or her started with equipment, assistants, and funding.]

The escape from this vicious circle for the young Academy scientist will be by corresponding with a respected Western scientist in the same field who then can appreciate *promise* as well as performance. With the endorsement of such a correspondent, invitations to spend a period in a Western laboratory can develop. Also the U. S. scientist can apply to the National Science Foundation for support of the collaboration, including the purchase of equipment to go to Kazakhstan. Some of these operations are already under way, supported by an NSF program designed specifically for FSU scientists. The Soros Foundation also provides such support.

It was a hard sell, however, to persuade the Academy scientists, especially the directors of institutes and others of their age group, that there was no shortcut around this lengthy process. In their isolation, and with the blessing of their titles as "Academicians," that is, members of the Academy, they fully expected that great quantities of U.S. dollars would be dropped on them. Many had the additional misapprehension that I came with that checkbook.

My contact was a very energetic Foreign Secretary of the Academy, Dr. Bektur Baijanov. His English was almost zero, my Kazakh was exactly zero, and my knowledge of Russian was limited to being able to make out some words by knowing the Cyrillic alphabet. His bad French was about equal to mine, and so most of our communication was bad French. I proposed that we record our conversations and sell the recordings for laughs in France. I have hopes, but limited confidence, that Bektur is continuing the procuring of journals and the acquiring of one-on-one Western correspondents.

I was given an office in the Presidium Building of the Academy, an extremely grand and largely empty building in downtown Almaty. Two translators suffered with me and my occasionally metaphoric or colloquial English; I was usually left on my own when the conversation became technical, but sketching, marking up illustrations in technical articles, and writing out key words carried us over the tough spots.

My charter did not extend to recommending a course of action for the Academy to serve the country and to retain able people and become a modern research institution, although that is, of course, what I should like to have done. As they say in Washington, that was "above my pay grade." I could be candid, however, in responding to the invitation from our Ambassador, William Courtney, to make recommendations, and I believe my analysis got across to the President of the Academy and a few other officials, even though I had to speak very softly.

What I believe should happen is for the Academy to become very much smaller--perhaps one-tenth as many people--and be divided into only about ten institutes. It is far too large for a small country, and much of what it tries to do should eventually be done by industry. It ought to concentrate in areas like petroleum geology, seismology, and dryland agriculture, where natural resources and the geographical setting provide an advantage. It should not try, for at least a couple of decades, to compete in fundamental science on the world scene.

There is some evidence that motion in the correct direction is occurring. I have learned that since my visit the President of the Academy has been relieved of his job and the Council of the Academy has been broken up and reorganized. I doubt if my visit prompted any of this, but perhaps the forces at work to invite me were also the forces that produced this motion.

I turn now from Academy matters to other aspects of our stay in Kazakhstan and say a little more about the country, its past and probable future.

One of the most rewarding, but also exhausting, experiences in Almaty was an evening with an English-speaking club. This was a group of all ages, from high-school students to retired people, that met once a week to improve their English. They called themselves "The Yellow Submarine Club," I assume after a Beatles song. From their questions and comments I learned that they were very bright, but here again the isolation was prominently displayed. For example, they had never heard of the Marshall Plan, and their eyes glazed over in disbelief when I reported that Marshall Plan assistance had been offered to the Soviet Union. Some of the most impressive members were young Kazakh women working for U. S. companies. I believe that the isolation is being cured much more rapidly in the commercial sector, under market pressures, than in the research sector supported by the government and insulated from competition.

What became strikingly apparent to us as we learned and listened was the extent to which the USSR, dominated by "Great Russia," had treated Kazakhstan, and presumably the other smaller units, as a *colony*. Wool and cotton from the Steppes was sent to Moscow and other Russian centers, made into cloth and clothing, and then sold back to Kazakhstan. The "manufacturing valued added" was in the Russian cities, and the Kazakhs remained in unskilled jobs with a lower standard of living. Similarly, copper ore or semi-refined ("blister") copper from Kazakhstan's mines was made into copper wire, copper pipe, and electric motors in Russia.

Thus Kazakhstan has an almost total lack of manufacturing facilities and will need massive injections of capital to become a modern country.

This will take time, but it is almost certain to occur. Kazakhstan is a potentially extremely rich country. It is reputed to have more gas and oil reserves than the U.S., which means that *per capita* it has 15 times as much. It is rich in gold, silver, copper, lead, zinc, coal, and many other resources. Shortly after independence in December of 1991 a contract was written with Chevron to develop the Tenghiz oil field at the edge of the Caspian Sea. Additional joint enterprises have been undertaken with Shell, Mobil, and others. There is every sign that the hard currency from these will go into further development, and not, as in so many third-world countries, into the Swiss bank accounts of government officials.

The political situation after independence appears to be stable and promising. The President of the Republic is Nursultan Nazarbayev, a former Russian Soviet apparatchik. He seems to be popular and capable, but of course there are many shoals ahead. Kazakh has been named the official language, and the transition to it may be stormy. As in Russia, there is certain to be resentment that consumer goods suffer as the priority is given to creating a manufacturing plant. The infrastructure of roads, buses, trams, and airports is in terrible condition.

We could not help contrasting Kazakhstan with Taiwan, where we have been going once or twice a year for the last seven years as part of the Science and Technology Group reporting to the Premier. Taiwan has 21 million people; Kazakhstan has about the same number, 17 million. But Taiwan has almost no natural resources, very little arable land, and only 1/75 the area. Nevertheless, by hard work, emphasis on education, and initially some support from the U. S., Taiwan is a prosperous country with a positive balance of trade *per capita* equal to the U. S. negative balance of trade per capita. The Taiwanese are much better off in every way. The difference was the economic and political systems and the treatment of Kazakhstan as a colony.

I did not have the time or the charter to study the educational system, although I talked with the presidents of the University and of the Technical University. Literacy is universal, but the blinders of provincialism are forbidding. Much will depend on how the transition to Kazakh as the official language is handled.

Privitization is in full swing, but it is going to be complicated and take many years. The legal structure and the courts are ill equipped to enforce contracts, and accounting is primitive. U.S. accounting and management consulting firms are heavily occupied in support of the creation of joint ventures, which are the obvious next step in development. The Germans are ahead of us, in part because they are closer and in part because there is a substantial German population, derivative from large numbers of prisoners of war from World War II. But there are great opportunities for Americans: A young American MBA who was willing to learn Kazakh could do good and do well at the same time.

At the time we were there, Kazakhstan was still a prisoner of the ruble block, since almost all their consumer goods had to be bought in rubles. They had printed their own currency, and I have been told they issued it later last year. It was a very courageous step.

The Russians used Kazakhstan as colonial real estate to test nuclear weapons, just as the French used their Pacific island colonies. The test site at Semipalatinsk is now thoroughly contaminated and much of it is highly radioactive. The Russians would like to walk away. Negotiations are proceeding, but the expense of the cleanup and the liability for the effects of radiation on the health of nearby residents are tremendous problems.

The Soviet launch center at Baikonur in northwestern Kazakhstan presents another problem. Russia seems determined to continue a bankrupting space program and has forced Kazakhstan to pick up 5% of the fixed costs of the Cosmodrome. This is a lot of money for a small country that has no business spending money on its own space hardware. There are conflicting reports of the state of maintenance and security at Baikonur, but the reliability of Soviet launches, which was never very good, is certainly questionable.

Kazakhstan has 104 SS-18 nuclear tipped missiles. It agreed that it would give them up and sign the Nonproliferation Treaty, but for a year our administration did not take them seriously enough to give them attention similar to Ukraine. Nazarbayev finally negotiated a signing ceremony at the White House, and the SS-18's, the most threatening of any missiles anywhere, are being dismantled.

Kazakhstan's greatest worry is the Russian Federation, which encloses it on all the sides permitting surface travel. If Russia stumbles on its way to a democracy and becomes militant again, it would be bad news in Almaty. The Kazakhs lost in the three-way war in 1917--Reds, Whites, and Kazakhs--and they would lose again if they had to fight the Russians alone. Clearly Nazarbayev wants to have enough German, U.S., and other foreign investment that he can get diplomatic and even military support if threatened by Russia.

One note about our embassy in Almaty: I have not always been impressed with the embassies with which I have worked, but I was very favorably impressed by our representation in Kazakhstan. The Ambassador, William Courtney, is a career diplomat with experience in arms control negotiations and is, I believe, very effective. He and his staff were very helpful to our mission.

But the real prize for effectiveness and helpfulness has to go to Susan Johnson, the country director for the IESC. Her energy, competence, knowledge of the country, and friendliness made all the difference in our work. She is a former State Department employee and is highly respected by the Ambassador, and this respect lubricated my operations immensely. Her husband is the Pakistani Ambassador, a former U.N. official and analyst of the USSR and the Afghan War.

MLS

After two days in the depressing slum of the Hotel Kazakhstan we moved to an apartment which was leased by the IESC. The usual occupants were on home leave and left the apartment well furnished and supplied. It was small, clean, conveniently located and as comfortable as crossing the Gulf Stream in a small boat. It was on the second floor of a 12 story building, which was fortunate because the elevator was a chancy operation and we never used it after the first trip. The stairwells and outdoor stairs were a mess, each night decorated with additional trash and broken glass. Nobody seemed to care.

Buying food was a problem and an experience. I went to the Central Market once a week, with an interpreter. Farmers bring in their wares and exhibit them attractively. Food cost about the same as in Rochester at the street rate of 1700 rubles to the dollar (the bank rate was 930). Bread was very cheap, seven or eight rubles for a loaf which weighed about the same as here but was smaller and denser; in other words, two loaves for one cent! Clearly bread is heavily subsidized in order that everyone gets enough calories.

But with our tiny refrigerator, most of my buying had to be at the nearby kiosks and windows--it would be an exaggeration to call them "stores." I could never tell what a merchant had until I watched people exiting. One day he would have only cabbages, the next only carrots, and the next only matches. IL was very lucky one day to spot a lady at a card table on the sidewalk selling paper goods, which I had found nowhere else up to that time, and we urgently needed to replenish our supply.

Our closest supplier had bread (always) at one side, and on the other side at the end of a corridor a clerk was selling whatever they had that day (sausage? cookies?). Lining the corridor were scores of gallon bottles of pickles! They may have been there since the Czars. We never saw any motion in the racks of them.

I managed to communicate what I wanted by a combination of pointing, writing, and securing help from the always friendly fellow marketers. Since there are no coins, I had to sort through a fistful of rubles to get, say, 2137 to give to the clerk.

There are small "dollar" stores in hotels, and the Germans opened a larger one, freestanding, a few days before we left. A very eclectic collection of imported merchandise was carried in these stores and at very high prices. But if we wanted marmalade, packaged milk, or Scotch we had to deal with them (vodka was available at extremely low prices on the street, but we didn't buy any).

Walking along the tree-lined streets was a pleasant experience. There was a fascinating mix of racial features, and all races were apparently at ease with one another. The people were uniformly polite and neatly dressed. We saw more color in their clothing than we had seen a few years ago in European USSR. We saw only one drunk and no beggars.

We did not get a chance to sample the cultural offerings. The season had closed by the time of our arrival, and we had work to do virtually every evening. We enjoyed several short trips into the Tien Shan, especially a day at the Ionospheric Research Laboratory at an elevation of 10,000 ft., even though the trip up and down was on a primitive road in a Russian copy of a Jeep. We also traveled with Susan Johnson and her husband one weekend to Tekeli, a small mining town 300 km northeast of Almaty, where another IESC volunteer was working.

There are three television stations in Almaty, two of which are of course in the Kazakh language. You might think that the third would exploit the rich array of German, British, or U. S. fare. Well, with some embarrassment I must tell you that the third station is MTV! We wondered if the managers are trying to recover their youth or think that in this way they can maintain contact with the young. A more disturbing hypothesis is that someone is trying to discredit the U. S.

I had offered to teach English or participate in an English conversation course. But the Embassy people were slow in making arrangements, and my work could have started only a few days before we left. Anyway, I was pretty busy marketing, painting, and supporting our mission.

The International Executive Service Corps literature states that serving as a volunteer is an experience you will remember for the rest of your life. They are right!

Kazakhstan & Georgia: Science & Survival

28 October 1994

Two countries of FSU.

Questions: 1. Chances of survival?
2. Strategies, routes, to survival?
3. Role, if any, of science in survival?

I How I happened to be involved
- IESC   USAid, corp., ind.   Typical mission.
- My missions: K. Connect & West. Revive.
G. Create RAND-type studies & analyses.

II Geography & People   K. Horseshoe – Ev., Him., Kara, H. Kush, Tien Shan – Pobedy
Steppes. Russ. Fed.; area: U.S. E. of Miss.
Isolation: 6½–7 hours. Germans / Pakistani.
Muslim, recent country.
17 million   40% K, 40% R, others Germans etc.

G. Small 60 x 300 miles   Caucasus
Christian.
5.5 million Georgians +

III History.   K Nomads until Czar. R. in 19th Cent.
G Kings – Country with integrity since 600 BC
Both: 3-way war 1917-21

Colonies   K.
G. Rustavi; marketing of fruit; Turkmen. gas dependence

IV Economy

K. Privatization deliberate – now accelerating. Dollar stores

Joint ventures.

German, Pakistani

Tied to ruble until 6 mos. ago. Infrastructure – buses – shops

Will be a rich country; patience.

Nursultan Nazarbayev

G. Privatization abrupt & then civil war fomented by Russia.

Abkhazia / Sudetenland.

Rustavi: apts. given to occupants, but no management.

Apple orchards.

Symbolic salaries. Coupons. Govt. revenues ~20% of expends.

Infrastructure: roads, electricity, gas: Joint ventures difficult.

V Science: Scientists well treated (comparatively) during Soviet days (Sakharov etc ampli

K. Univ. not strong – little res. even in good times.

Acad. 8000 people 44 institutes (5 "physics")

Potholes in corridors. Russians dumped. Little work.

No journals since 1991. No apprec. of world-class science.

Some institutes ought to be preserved (extractive metallurgy, catalysis, ~~&~~ seismology).

Los Alamos disease.

"Complete overhaul" needed

Science could help, but no point in pure science – poor by comparison. Instead, young scientists are becoming entrepreneurs.

(Kramers & enclosing dikes)

(All my life defending basic research) (Clothing industry – no pt. in inventing equip., but scientist as consultant useful

G. Univ. & some institutes were strong under USSR (Andronikashvili)

(Rad. dam. in solids;) LT physics (liq. He 3)

Acad. 6000 people for country of 5.5 million. (If same ratio in US, 300,000!)

Individual scientists scrabbling for external funding; CERN, etc.
Impressed with commitment to country. My team of 5. Energy.
Right attitude but standing on a much more insecure base than K.
Exploration geophysics; earthquake res. engineering structures; technical auditors of joint ventures.

VI Pleas, please

1. Don't lump "-stans" with Russia
2. Forget CIS
3. Seek out colleagues in NIS
4. Ponder relation between balance <pure / applied> and wealth of country.

KAZAKHSTAN + GEORGIA
Science + Survival

I International Executive Service Corps

II Geography + People < K / G

III History < K / G (Colonies!)

IV Economy < K – L. without M. / G – Market

V Science + Technology
- Universities
- Academies
- Baikonur / Semipalatinsk
- Rôle in Survival

VI Pleas, Please

This was additional language attached to my report to the President. The crossed out portions were eliminated to fit within the allotted time. (RLS Jan 2000)

STAG 1996

GLOBAL WARMING

It is a credit to the Republic of China that it is concerned about the possibility of global warming and is willing to participate in international programs related to it. Its participation is not in any way required; it will be costly to comply with international norms~~,~~ and the R.O.C. must suffer the indignity of participating only as a "Non-Governmental Organization." But the R.O.C. needs to retain and recruit friends, and its generous attitude toward perceived world environment problems earns it respect and friendship.

Nevertheless it is important that the R.O.C. response be carefully modulated. The R.O.C. has many other concerns (~~world competition, defense, others~~) and only finite resources. Specifically, it would be wise to treat the models of predicted global warming as "wake-up calls" of what might happen, not as quantitative predictions requiring immediate action. Their importance lies in the circumstance that much of the world believes in them. (~~If most of the world believes in witchcraft, one must act as if witches abound; even if most believe in them witches are not a fact, but belief in witches is a fact.~~)

Research may very well be focused on causes and results of possible global warming that are special to the island of Taiwan, but there needs to be no R.O.C. research on the global questions of the amount and timing of global warming. Others will do this with much greater resources, and the international conferences' conclusions from that work will have to be assumed to be correct~~.~~ any case. The current atmospheric models can be used but they should be used with a healthy skepticism, especially for the "prediction" for the year 2100 (or even 2500!)

R.O.C. Response to Global Change

The impact of possible global warming on life on Taiwan needs research, especially concerned with the features peculiar to the island: the importance of coastal plains, short and steep rivers, etc. But it would be easy to do too much of this. It will be more efficient to wait as long as possible, in order that the effects of warming become better known (the models are certain to change) and because of the " time value of money"; spending money now to guide interventions that will occur only in 2050 (or even 2100) is not cost-effective.

Adaptation to possible global warming will take many forms. First, there is no reason to believe that all of the effects will be bad. I find it ironic that the assumption that *any* effect will be damaging is being made by the same reform groups that have for several centuries bitterly attacked the famous aphorism of Alexander Pope: "Whatever is, is right,"
an expression of love of the status quo that was effectively buried by Voltaire's *Candide.*

Second, animals and insects have adapted to far greater changes in global climate. To be sure, they have usually enjoyed long transition times. But surely man can outdo them in adaptive capability.

Thus I recommend that attention be given now only to "very long lead-time" items such as construction standards for major structures in coastal plains. There will be plenty of time for deliberate study of appropriate adaptation to other consequences of global warming, even if the dire consequences popularly predicted actually occur.

Carbon Dioxide Production and Sustainable Development

The six-point program outlined to us by the Energy Commission of MOEA makes excellent sense. It will not limit CO2 emissions to as low values as the UN groups wish, but it will limit CO2 production by accepting the higher monetary costs of expanding LNG use and the higher political costs of expanding nuclear power. Conservation and renewables are used to the maximum extent possible.

If the R.O.C. believes it must further decrease CO2 from the values implied by this program, additional nuclear sites and units should be built and an "all-electric economy" should be approached. To be sure, there will be protests, but I sense some weakening of the spirit of the nuclear protesters, some of whom now recognize that nuclear power is the only base-load power option that is non-polluting.

In the past decade there has been only a 1% difference between the GDP annual growth rate of 7.7% and the annual energy consumption growth rate of 6.6%. Further, the GDP includes some " quality of life"

items, and so even if the R.O.C. were to be satisfied with a near zero growth of the economy (but no decrease in living standards), energy consumption would have to grow. Thus it is totally unrealistic to say that one can freeze energy consumption and by this act keep CO_2 emissions from growing.

Photochemical Smog

The problem of photochemical smog, although sometimes confused with "greenhouse gases," is quite different. It is a real problem of air quality affecting health, safety, and esthetics, but it is a local problem and subject to local action and solution. The ITRI work on electric motorcycles seems to be successful but is now stalled, and the effort on electric cars seems to have been stopped. It is unfortunate that more progress has not been made on these programs since the smog problem from unburned hydrocarbons in gasoline engine exhausts is serious, especially in the Taipei Basin.

Vehicles are a major contributor, and electric vehicles are under developed to reduce emissions.

The problems with electric vehicles have been cost and range. Cost can probably come down with popular acceptance and thereby large volume, but range has been stubbornly limited to around 100-200 kilometers because of the power/weight limitations of existing batteries. Further, even if the quoted range of a car were (say) 150 kilometers, the driver would be reluctant to go more than 20 or 30 kilometers from home because of the possibilities of detours or side trips and his reluctance to shorten battery life by deep-discharge. He must constantly put uppermost in his driving plan the necessity for getting to a charging station.

Thus until batteries are greatly improved, the hybrid gasoline-electric vehicle is very promising and could get smog-producing vehicles off the road earlier than if one waited for an all-electric vehicle with long range. (We have recommended work on hybrids in an earlier report). In the hybrid, a very small gasoline engine, with computer control, added to the battery pack gives flexibility in performance and a "get home" capability. A driver will be willing to use the vehicle for a much larger fraction of its advertized range when he knows he has a reserve engine to

get him home. Such an engine should be inconsequential as a polluter because of its small size and because it would never be "cold-started." (The first minute or so of the start of a cold engine is responsible for much of its pollution; the hybrid engine would be warmed electrically before starting.)

A hybrid should be an acceptable form of transportation long before battery development makes the all-electric car popular (if it ever does) and would not only improve air quality but would also contribute to lowering CO2 emissions.

White Paper on Science and Technology

The National Science Council has produced a most impressive paper with this title and the sub-title "Vision for the development of Science and Technology into the 21st Century." This paper deserves much more study and analysis than could be accommodated at the 18th STAG meeting. I hope we may study it carefully in the next twelve months and analyze it at the 19th STAG meeting.

Meanwhile I make only a single remark, based on a necessarily superficial study of this comprehensive document: "Strategy 3" is a particularly welcome statement. It calls for the focusing of talent and support into a limited number of specialties. Although this selection strategy is addressed, it has evidently not proceeded as far as earlier STAG recommendations. It is, of course, disappointing to have to narrow the country's view like this but it is absolutely necessary in order to be "world class" in the remaining specialties. [We heard at this meeting a convincing complaint from some industry people: "We must compete with the most advanced companies in the world that are buttressed by affiliations with world-class universities; we must have world-class universities in the R. O. C. to help us."]

Bob Sproull's speech on coeducation at Deep Springs delivered October 19, 1986 on the occasion of his retirement from the Board of Trustees:

I...appreciate the invitation to speak today. In twelve years on the Board and a couple of other times here during off periods for the planning group, this is only the second time I've been asked--and I think you'll understand after this talk why this is only the second time I've been asked. It reminds me at the very beginning of one of the subjects, and I'll refer to that in a minute, that I would like to do. I was given my choice of subjects, and that was a little bit awkward because there are so many things I'd like to talk about. One of them has to do with arms control, and one of the cliches or pieces of rhetoric that goes with that is called damage limiting, and this talk reminds me a good deal about damage limiting because you have seen to it now that your damage is severely limited since after I talk, I leave, and that puts an upper limit to the amount of mischief that I can create. So I think it's a very clever invitation.

The the D.S. issue I woulkd like most to talk about is the question of how to insure the survival of the institution. Survival isn't everything, but if you don't do it, there's nothing much else that counts. Our 1980 planning report attempted to focus attention on the total contribution that Deep Springs would make over its lifetime. I emphasize the total contribution that the institution makes over its lifetime, not the contribution per year. Students, faculty and even the president have strong incentives to focus on the contributions per year: the quality of the program, the amenities, if any, and other attributes, as perceived by the people here, and it's easy to see whether they are better next year than this year, or worse next year than this year, and these qualities of Deep Springs that lead to changing the lives of the students involved. And that's quite proper, that the students, faculty and even the president have these strong incentives. But I submit, and that was the theme of our 1980 planning exercise, that suppose you have a program that's only eighty percent as effective in producing lives of service for an institution that lasts forever; but that's clearly more of a contribution, a total contribution to the institution, than if it were a hundred percent but lasted only twenty years, say. And that was the point we were trying to focus on: whether one shouldn't, instead of just trying to maximize the contribution per year, try to maximize the ultimately total contribution of the institution, and we pointed out that that's different from any other kind of academic planning because almost every other academic institution tacitly assumes from the very beginning that it's going to have an infinite lifetime. One of the points we called attention to was the enormous leverage of changing the strategy when and if one got close to a break-even budget. If you're a long way from a break-even budget, there's no particular leverage. The institution will go down the tubes after a few years, and you might as well maximize the quality per year. But in the years since 1980, by excellent leadership and by a dedicated and very energetic fund-raising effort, we are now close enough to a break-even to make this line of thought important, and I should like to urge strongly that everyone study whether it is now possible to plan for an infinite life, and, if so, what moves one should make to see that that happens because the leverage is so great if one can extend the life indefinitely that it's an important task for everyone. This may be the only chance. The classes of Deep Springers up through the Whitney years are beginning to have their earnings decrease by retirement. In ten to twenty years the highest-earning alumni will be from the bad old days of--what was it, Fort? Wasn't that the character, Bob? (to Aird)--anyway from those bad old days, and their devotion is in serious question. So the next few years may be the years of greatest fund-raising potential for some time to come, and if we cannot make the transition, then, to infinite life within the next decade, we may have lost our chance. Well, that was the issue I would like most to talk about, but I'm not...having said that.

The national issue I would like most to talk about is the mutual assured destruction ballistic missiles defense issue--how we can secure the best chance of having a twenty-first century. In some ways it's sort of like having a twenty-first century for Deep Springs too; it's also future-oriented. I won't talk about that, but I just would like to note that I am extremely pessimistic, partly because of actions and inactions by the Reagan administration and partly because of the extremely low level of discourse on university campuses and in the newspapers of both coasts. I believe it is vital for your generation to study these issues in great depth and prepare for leadership positions. Otherwise there won't be a twenty-first century. But I won't discuss either one of those--either my highest priority Deep Springs issue or my highest priority national issue.

Because of the importance to Deep Springs, and because there are few at Deep Springs now who have heard the last discussion of coeducation in which I for one participated, and because I am anxious that the discussion at Deep Springs be exposed to the widest spectrum of views, I...I wrote here, "I wish to present my own view of this issue." That's not true. I don't wish to present my own view of this issue, but I'm going to present my own view of this issue. I wish I didn't think I had to.

The first point I want to make is I want to acknowledge that everyone to whom I've listened has had the highest motives in these discussions. One of the fine things about people involved in Deep Springs is almost everyone wants to improve the place, and so I think that everyone is working from the same kind of motives, and I think that's one of the things that makes the whole discourse worthwhile. But the fact that one's motive is high, or that one's motive is higher than somenody elses doesn't make his course necessarily the right course of action. You have to have more than the right motives. Also, I want to point out an anecdote that may put a little bit of restraint on this idea of imporovement. There was a very distinguished colleague of mine when I was in my Cornell days who had a lab across the hall from me. I don't know whether this anecdote will get across because it's a little bit, how shall I say, a little bit parochial in the experimental physics profession, but I think maybe you can understand it. He had an old brass vacuum system--these were the days when people were beginning to use stainless steel and fancy post-war electronics and so on--but this was an old brass vacuum system held together with Glyptol. Have you ever seen Glyptol? (to Kehoe) "Have I ever!" (Pell) You've seen it! I know you have; I was talking to Brandt. It was a General Electric lacquer or varnish that was used for, I guess for insulating wires and transformers or something, but it turned out to have a very low vapor pressure, so you fixed holes in vacuum systems with it. Anyway, this was a very old system held together with Glyptol and love and string and sealing wax, and all of his graduate students were always improving it, but of course every time they improved it it wouldn't draw a vacuum again for another two or three weeks, and he put a sign on his door saying "The next person to improve this apparatus will be thrown out of the lab." So, you've got to be a little bit cautious and self-restrained about improvement; that's the only point of that anecdote, and it probably doesn't have much point. The second--so I first acknowledge the highest motives, and all of us want to improve the place--the second thing I want to do is to ask you, the Student Body, to acknowledge that those Trustees who oppose coeducation might also have high motives. In particular, though, I ask you to acknowledge that those of us who oppose it are not necessarily sexist or not necessarily people that are properly characterized by wanting to keep women in the home, and we don't want a throw-back to the nineteenth century--all of the cartoon-like categorization of us octagenarians. I just wish you to acknowledge that we're not necessarily in that camp of that anachronistic, chauvenistic bunch just because we oppose coeducation at Deep Springs. They're two quite separate issues.

I think I'm speaking for everybody up to this point, but from here on I speak only for myself. And so I may be in an idiosyncratic position all of my own, but, nevertheless, that's all I can with any confidence speak from. I have spent all but three years of my life in coeducational institutions. I probably have worked harder and longer for coeducation, coed education, than most of you will. I found it very rewarding, incidentally, and I would strongly recommend that kind of life if you have any interest at all in it. Perhaps the three years that I spent in a single-sex institution, namely Deep Springs as a student--of course with vacations and other leavening it wasn't really a single-sex experience--perhaps those three years poisoned me. But the jury will have to remain out on that. I did not, and do not, oppose Telluride House and Telluuride Association going coeducational. I think it's perfectly appropriate, I think it's worked out well; I did not oppose it, I do not oppose it--it's a totally different question. In today's environment, I would probably not have the courage, if I were L.L. Nunn, I would probably not have the courage to create a single-sex college, if it did not already exist. But that's probably more a remark about my lack of courage than anything else. And I know I would not try to create a coeducational college in the D.S. mold, however. The third point that I want to make is that Deep Springs, according to me--remember that I'm speaking entirely from my own point of view here. I can't claim to have any support point by point for anything I'm saying. In my view Deep Springs in no sense should be looked upon in the sense of Mr. Kant's categorical imperative. We have not asked to be copied by anyplace else; we are not asking any other institution to copy or emulate us. We aren't asking them to join us in isolation, for example, in this kind of location; we're not saying that other institutions ought to have the intricate interweaving of work and study and student body government that we have; and, really getting now down to gutsy sort of issues, we're not asking any other institution to have the zero tuition, board and room, which, according to modern sociologists, educational theorists, all of the conventional wisdom of modern social science, would say "It's a mistake, it's wrong, it's immoral, it's dishonest and fattening." We are simply not asking any other institution to emulate us in that. So, if Deep Springs remains single-sex, it is making no statement about anything except its own destiny and its own modus operandi. It's different, and it recognizes that it has to be different to remain worthy of survival. So, that's the third point: namely that we don't look upon it in the light of the categorical imperative. So why am I--and perhaps others will join me in this particular way of looking at it, I don't know--so why am I opposed to coeducation at Deep Springs? It is certainly not on any doctrine or social theory basis, nor is it on Mr. Nunn's writings or history. I find him a very interesting resource, and of course we all have to abide by The Deed of Trust, but those are not the central positions. I join with probably almost everybody in this room in a basic appreciation of the fact that there is no distinction according to sex on any kind of ability or motivation or in any other way--the capacity to make a contribution to modern society. So there's no doctrine or social theory or anything like that that separates me from the people who are in favor of coeducation. I believe we all agree on these.

My opposition is for intensely practical considerations. You may call them grubby; lots of people when they look at practical considerations think of them as grubby. I would prefer to call them practical, and I hope you don't look upon this as too low to the ground. These practical considerations arise out of Deep Springs' mode of operation. The prominent elements of what Deep Springs' mode of operation is are really a convolution of three things, and they're so convolved that you can't really separate them. There's small size, isolation and location. Now in addition there's the body of theory, and there's the body of tradition and so on. I don't mean these are the only things that Deep Springs has as its mode of operation; but the convolution of small size and isolation and location are the parts of the Deep Springs mode of operation that I find would be most threatened by conversion to coeducation. And these are not there just accidentally. The small size, the isolation and location all have a great deal to with what Deep Springs' purpose is. So the combination of a purpose--a lot of institutions have forgotten that they have a purpose or don't really have any puurpose or their purpose is only to solve some local economic problem or make some politician happy--but Deep Springs, unlike most modern institutions, has a real purpose and it has a method of getting about its purpose, and a good part of that method has to do with the small size, the isolation and the location.

But before going into these and how I think they are threatened, how this effectiveness and its accomplishment of its purpose would be threatened by coeducation, let me make one comment on small size. When I do this I have to admit that I tried this once before on the Deep Springs Student Body, and it went over like a lead balloon. I'll try it once more, it'll go over like a lead balloon, but nevertheless I simply have to try. And this is a consequence of the small size; it is not the central point, but I just urge you to think about it. Spokesmen for coeducation have usually assumed nearly equal populations, not necessarily ten and ten, but say eight to twelve on one hand or twelve to eight on the other. I think that's the tacit assumption that most people make. Whether it's made explicitly or not I don't know; I haven't seen any thoughtful, explicit analysis of this question. I simply want to point out that over the years--maybe the first year this would be the case--but over the years it is very unlikely without grossly complicating the admissions, the selection and the readmission process. When I gave the lecture on the Bernouli distribution or the binomial theorem, which amounts to the same thing here, as I say, it was not overwhelmingly applauded. Maybe Brandt or Peter or Jim Schaal--I understand they teach Calculus--can take over. The binomial theorem happens to be more important than anything you will hear this morning, so you might learn it for other reasons. But that's only the beginning because when you put things into the Bernouli distribution, you're likely to put in an 0.5 a priori probability, and my guess is that that a priori probability is going to vary all over the map as the years go on depending on what one does within the applications process etcetera. So, remember I'm saying this is a detail, it's not the central point, but you really ought to think through what happens if you have a five to fifteen or a fifteen to five distribution. And so I don't think you ought to put on your rose-colored glasses and think that, some how or other, the distribution is going to be very equal. I do not envy you the endless argumants about whether to take the next most promising student on the list of applicants or to take the student who would tend to even the sex ratio. I can just hear those arguments, and I sure don't envy you them. They will infinitely complicate your lives. That's a detail, it's a footnote, it's not the central point.

My main problem with coeducation has to do with the intensity of the Deep Springs experience. The interaction, the isolation, the interpersonal tensions--somehow or other relations between people are always called interpersonal relations; I don't know what other kind of relations there are, but that's a sociological term that you have to adopt. The intensity, the isolation, the interpersonal tensions, the effect of small numbers and, above all else, the circumstance that there is no place to hide; everything comes out in the end. One's must continue to live with one's associates no matter what happens or else one must leave the valley with a deep sense of failure, which may have more to do with the rest of one's life than anything else he or she has done. This is the crux of the matter from my standpoint. Now the next I assure I'd be better off if I didn't say because it may sound condescending, or it's the kind of thing you expect from octogenarians like me, but it is necessarily a fact and I think I just have to say it, and that is that the period eighteen, nineteen and twenty years old is a period of tension in everyone's life anyway, even outside of Deep Springs, so it's already a period of rapid adjustment, of rapid change, of fixing things in one's mind and one's motivation structure that may determine how the rest of one's life is spent. I'll come back to that in a minute because of course that's where the real power of the Deep Springs modus operandi (lies). I might point out that, as I understand it, in your age group, second only to auto accidents, suicides are the biggest source of removal of people from your age cohorts. Now I'm not making any dire predictions. Do not please remember that Sproull is saying there're going to be suicides, because I am not, repeat, not saying that; I'm simply saying that it is a tense period of development. It's an extremely important period, that's what Deep Springs is all about, but it's a period where the tensions are already there, even before indulging in a tension enhancing experience like Deep Springs. And that's all I'm trying to do is to emphasize that for the...(end of sentence garbled) Part of the Deep Springs idea, according to me, is to take this key period in one's life and to let the individual have the luxury of being removed from the complicating elements of all kinds. Most people at this time of their lives have enormous complications. People who don't go to college, who aren't removed from the mainstream of society to that extent, go to work. They have to hear an alarm clock for the first time and so on. It's a period when in societies before the twentieth century almost everybody was beginning to get into considerable complications, and the luxury of attending college and being allowed to think through things in a more calm way without the urgency of making a living, etcetera, was a great luxury. We've gotten more accomodated to it, and most students now, most people, young people now go on to some kind of higher education or other. But the Deep Springs experience is an especially luxurious one in removing the necessity for all kinds of complications during a period usually of two years. One is put into a community where he is supported by a relatively homogenious community, and it's done for a purpose. The purpose is stated differently by different people. I think a major part of the purpose is to allow young people, almost for the first time, to think deeply about their relation to society, about their career choice, to make personal commitments to a career choice. These are rarely ever spoken, but quite frequently people will acknowledge--and if I were to put my heart on my sleeve, I would acknowledge it myself--that one's whole lifetime commitment to a way of approaching his interaction with society, his dedication to service, came from the period at Deep Springs. The fact that it's not spoken probably makes it even more vital and important than if a person went around chatting about it. This is what seems to me the central core of the Deep Springs purpose is, and the central core of the way it goes about it is to allow people to be, for a couple of years, remote from urgencies and complications of all kinds and to get a chance to do the deep thought and commitment that follows out the Deep Springs purpose. It is this sense of purpose and this dedication to making the circumstances for young people to get on with it that gives this place its unique character. It does not seem to be like any other place in that respect. I might have put a footnote--again I urge you not to think that this is the central feature of my remarks. This emancipation from everyday complications is extremely expensive: it's something like twenty thousand dollars per year for food. I don't begrudge that at all--for that kind of high purpose and that kind of degree of removal from everyday urgencies, because we have to pay for it. But I just point out that people are making an enormous investment in you, and that gives you the possibility to make the best of it. To me, preserving and enhancing this freedom from complications of all kinds is so close to the core of the Deep Springs idea that I don't want to tamper with it. I regard it as as invariable as anything here.

There always pressures to make Deep Springs like every other institution. But I think most of us would agree that if it does become close enough to other institutions, there's no point in it. It's so expensive, it takes so much out of its friends and supporters and alumni and others that if it becomes close enough to other institutions, to hell with it. There's no point in extending this elaborate expenditure of human energy on it if it's just like other institutions. At this point I want to tell one anecdote, and it goes into a little bit of, not exactly educational theory, but talks a little bit about the language at least that's talked about in universities around the country and around the world. The anecdote has to do with one of L.L.'s cronies--and I've got to be careful, Bob (to Aird), because I may be inheriting sacred territory with you--by the name of W. L. Biersock. This is W.L. Biersock senior; there was also Billy Biersock. I think Billy--did Billy ever die, or what ever happened to Billy? Did Billy die? (to Aird) "Oh yes, years ago." (Aird) Billy was a leightweight, I'm sorry to say. W.L. Biersock senior was an interesting guy, he was devoted to L.L. Nunn, he was devoted to Deep Springs, he was one of L.L.'s cronies, he was one of the original Board members. Unlike some of the others, he was not viscious, but he was not bright either. He was a kindly gentleman, and he was devoted to Deep Springs. He was the treasurer during my period here and for a long time afterwards, and we had to do the books here in such a way as to fit in with his language and his way of doing things. There was one account that was called "Maintenance, Institution, Personalty." Now at that time nobody at Deep Springs, myself included and Larry Kimpton included who was a Cornell Ph.D. in philosophy, nobody knew what personalty meant. It was a new word to us. And of course we were all too arrogant and parochial ever to look it up in the dictionary. There were a lot of other words that Biersock used that we didn't know either, and we naturallly assumed it was a mistake for "personality," and so we called it "Maintenance, Institution and Personality." H.L. Mencken once wrote an article about the words that you could not get into an American newspaper. The reason you couldn't get them into a newspaper was that they were words that looked like more common words, so the proof-reader would always change them to the more common word. A good example would be "adsorb." You know, you can't get "adsorb" into the newspaper; somebody'll change it into "absorb." A more common one was "causal." You can rarely get "causal" in because it always comes out "casual." Well poor Biersock tried to get "personalty," but it came out "personality", because none of us knew that. But you know it's an interesting thought: institutions do have a personality, and I've thought of it many times since, most of my life having been spent in institutions. I'm not talking about penal institutions, I'm talking about educational institutions. All of them had a personality, and Deep Springs' personality started out about as different as you could imagine from any other institution with the possible exception of a monastary.

Now statisticians talk about relaxation towards the mean. Deep Springs has been doing a fair amount of relaxation towards the mean in the sense that it's becoming more like other institutions. I think all of us would disagree on how much of that becoming like institutions is wise, how much of it is simply accomodating to changes in American and world society and how much of it is simply not making the enormous effort it is to keep different. There's always an effort to keep different from any place else; it's much easier just to go along with what is happening. I remember early, before the sixties, actually, in--where's Charles Christensen? Is he still around? "Yea" (Christensen)--in the days of the Pasadena branch, before the bad old days of the sixties, there was a New Yorker cartoon which I wish I had, but I treasure the memory of it. A bearded young man--and beards were not so common in those days--and his pad-mate were in what looks like a dirty old attic. He is standing in front of an easel putting slashes, presumably of different color although this is in black and white, on a canvas, she is over a hot-plate working out what passes, I suppose, for their evening stew, disorder and chaos surrounding them all over, and she says to him--it wouldn't matter, he could say to her, it's not a sexist story--"Why do you have to be a non-conformist like everybody else? Well, it takes a lot of effort to maintain an institution different, so for one reason or another or no reasons, there's been a steady--not exactly loss, but nibbling away at the "differentness," the extent to which Deep Springs is different from other institutions. It's now called a college, for example. If you look in L.L. Nunn's writing, he was dead set against the idea of calling it a college. He didn't like the idea of putting it in the same norms, norm distribution as other colleges. Well, you know, I don't oppose it's being called a college. It's got lots of problems as a kooky institution, and trying to defend it to people for the first time, it's probably helpful to call it a college and so on. That's just one example of the changes that have been going on making it more like other institutions. The role of the faculty: if you look at L.L. Nunn's writing, he almost never mentions faculty. He didn't think of this place as a faculty-controlled institution the way an American university or even little colleges are essentially institutions where the faculty is the institution. But the gradual increase in the role of the faculty is something that makes it more like other institutions, and so on. The isolation has been nibbled away at by the fact that it's been the pattern--it's nothing much in doctrine, it's still there in doctrine--but the pattern is that people leave the valley in the course of the year a good deal more times than they used to, and so on. I am not arguing with any of these; I am simply saying the facts that the extent to which Deep Springs is different is changing, and it has become less different. My guess is that all of the ways I've just mentioned are probably good things, but nevertheless one simply has to note this and note that the pressures are always on to make Deep Springs like other institutions, and so one always has to think, "Am I really (acting) from the standpoint of design and protection of the Deep Springs purpose, the Deep Springs modus operandi, should I resist those pressures or should I go along with them?"

Now I want to say one other thing at this point, and this is the only point at which I get into contact with the great outside world of college and university theory and practice. Much of the writing on universities speaks of integrity. You have heard that a great deal: whenever the president talks about anything he doesn't like it's something that threatens the integrity of the institution, in his speeches. It's threatened by government, it's threatened by the crazies, it's threatened the fragmentation in the departments, it's threatened by professionalism--you know, everybody talks about the threats to the integrity of his favorite institution. I have never liked that use of the word, I've never liked that line of argument. My problem with it is that integrity, to me, although it's a quote "good" end quote word, doesn't express what I think one should defend in a university or a college or Deep Springs. The reason is that a rock has integrity, and that's not the kind of integrity one's trying to preserve. A far abler character than me, Erik Ashby, and I hope many of you have read some of his writings--he's one of the most thoughtful writers on educational subjects. I guess he now has the marvelous title of Lord Ashby of Cheek; isn't that what Ashby is now? Anyway, he's a very interesting and able and thoughtful guy. He tried to get the language used--it never succeeded except with me and I suppose a few other people--what he calls it is the "inner logic" of a university. It's the "inner logic" of an institution, the "inner logic" of a college or university or Deep Springs, that I believe is a sensible way of looking at it. What he means by "inner logic" is very close to integrity, but it's a dynamic thing, it doesn't mean the static integrity of a rock, it's the practices, the doctrines, the conditions, if you will, that distinguish it from others and that enable it to do its work. That's what he means by "inner logic," and that's what I think has to be valued and preserved at Deep Springs, is its "inner logic," its purpose, its way of accomplishing its prupose, the things that distinguish it from conventional institutions and enable it to get on with what it's doing. Part of the "inner logic" is the small size, the isolation, the location, the condition, L.L. Nunn's writing, and so on. With the friendships we make it's the same, the people, the way that we look at each other's careers after Deep Springs--all of this is the "inner logic" of Deep Springs. As I have said, these things implement the purpose of altering promising young people's lives towards preparation for and dedication to a life of service. Now a certain amount of mutatis mutandis can be accomodated, of course, without threatening the inner logic of the institution. But to me the complications of coeducation in my view threaten the inner logic of Deep Springs, and that's why I am opposed to it.

Now I'm approaching the end, so don't give up hope. Up to this point I've said nothing about alumni contributions in dollars and cents and so on, and I want you to remember that because that is not the focus of my attention or my problem. Paranthetically, my guess is that if you actually put names down with realistic positive and negative signs after them that you would find that coeducation was going to be a complication for fund-raising--but that's only a guess and it's by no means my central point. As you know, I want no part in threatening shortening the life of Deep Springs by even a single day. But since I cannot quantitatively figure out what's going to happen with contributions because a lot depends upon the dynamism of how you go about it and how many people complain and things like that, I think we cannot have any confidence about the effect on alumni and others' contributions. I admire in a remote way the people who do have confidence about that, but I don't have any.

I want to make one final remark before concluding, and that is that, according to me, there is no such thing as an experiment or a trial in a situation like this. That statement is not quite as firm as the second law of thermodynamics, which says the same thing about physical systems, but it's a social law that's doggone near as firm as the second law of thermodynamics. Once you go in the direction of broadening and becoming more like others, very rarely can an institution refocus it's activity into something different from others. I ask you for counter-examples, I'd be glad to hear them, but it just isn't the way society works. It works in the direction of broadening and relaxation towards the mean, of the institution's becoming less different rather than more different. Thus if one decides to go coeducational, I don't think it is realistic to talk about it as a trial or an experiment, I think you must look at it as making a substantial change with great risk to the contribution Deep Springs can make to society and with no realistic possibility of turning back if it is a mistake.

Well, this is the way I look at it. Of course I could be wrong, I've been wrong before. I doubt if there's any other Board member who, even if he agrees with my conclusions, views it just exactly the same way that I do. As I have said, I respect the motives of everyone, but I urge you to think long and hard about losing the distinctive features of Deep Springs and its own way of accomplishing its purpose. Thank you again for inviting me to speak.

Questions:

Random Turner-Jones, student: Why do you think the complications that would result from sexual relationships between men and women here, were Deep Springs to go coed, would be any more problematic than those that result from existing homosexual relationships?

Sproull: You've gone over my head already...Alliances usually don't complicate things nearly so much as breaking up alliances, and I really...I'm no sociologist, I'm no psychiatrist. I hope there's a good psychiatrist in Bishop, but I'm not sure about that. If you go coed you're going to need it. Maybe you already need it, if you have the type of alliances you're hinting at. But you're over my depth, and I can't help you.

Peter Rolnick, faculty-member: Can you say something about...there's so much talk, from one point of view or the other, about coeducation, do you have an idea of how to go about making a decision about it?

Sproull: Sure.

Rolnick: Well besides just saying one thing or the other, how to go about bringing out discussion and deciding how to weigh one point against the other.

Sproull: That's a second question, you have two questions. The first question is infinitely easier than the second. The first question, I have to agree with my friend Mr. Odell, there's only one answer to that: the trustees have to decide. The trustees when they signed on said that that's what they would do--that they would carry out the Deed of Trust, and they would decide questions like this. If you became a trustee without that conviction that you were going to decide, you're being dishonest, and you should resign from the Board. That's easy. The second part is more difficult. I think everybody would differ on that. My own view is one of disappointment that the drum keeps rolling here year after year after year on coeducation. I guess the trustees--well, I can only speak for myself--I would prefer if one went in a systematic way about it every five years or ten years or something like that, and the rest of the time one got on with the work of Deep Springs. But I guess that's unrealistic, to think that a bunch of bright and able people can help but speak their minds. So what I would prefer, in answer to the second question, is that one go through the usual apparatus of committees and discussion and so on once every five or ten years and really bring it to a focus in terms of putting the strong arguments forward on both sides and try to get what is generally called "second order agreement." I strongly believe that if I can state your position in a way that you say, "Yes, that's a good statement of my position" that I think we're making some progress. If you can state my position like that--in other words, we get what's called "second order agreement." So, you know, if I had my rathers, the answer to your second question is we would set up apparatuses of any interested parties, and everybody's interested in this--alumni, trustees, students, faculty, president and so on, even the Telluride Association has now gotten seriously interested in it--and then we try to get second order agreement before we try to get agreement, then the trustees'll just have to decide. A person has absolutely no business signing on to the Board of Trustees unless he intends to make decisions. If he intends to let the student body vote do it, I think he's being dishonest.

Norton Dodge, faculty member and alumnus: Bob, may I ask a question that follows a bit along those lines. Peter was asking how you arrive at a sensible, valid decision on this issue. I was very interested to hear you admit that it is beyond your depth; that's the first time I've ever heard you say that.

Sproull: You haven't been around me much in recent years, Norton.

Dodge: As you've grown older, I guess you've grown more aware of certain things. The situation that I'm concerned about is that you are making assumptions about enormous tensions, tensions that are greater than coed...and that's an issue where you're beyond your depth, and it seems to me that...

Sproull: Sorry, you've got to be a little bit more careful there. It's the homosexuality that's beyond my depth.

Dodge: Ah, only that.

Sproull: I've worked not just with my own children but with eighteen to twenty-two year-olds in thousands, in scores, in dozens, single individuals; I've seen the problems that we've gotten into in dormitories and so on. The University of Rochester was the first place that I know of--the first sizeable institution--to have coeducational dormitories. It's gone very well. My problems haven't been associated with that...I hope I didn't make a disclaimer that this whole area of what kind of complications coeducation would mean at Deep Springs is necessarily over my depth. It may be, but I wasn't saying that.

Dodge: Well, what I was wondering was whether this might suggest that we shouldn't do some serious research and investigation into this whole issue. I have had experience at the university where I teach, at Deep Springs and at Pomona, my farm, where they ran the first coed summer program, for three different summers with no problems. And that's also isolated, there's no way to get away...

Sproull: Yeah, I know. That's why some time back there was a lot of talk that maybe a coed TASP would be a way of learning things about that. I was very leery about a coed TASP because I'm afraid that that's exactly the conclusion to be drawn from it. I don't anticipate any problems at all in the first six weeks or even the first six months of a coed Deep Springs. That's not when the problems arise. As I say, the alliances are easy, it's the disalliances that are infinitely more messy, where the complications seriously invade the concentration people can put on the purpose of Deep Springs and the purpose of their being here, which involves the exercise of some self-restraint, much more than if one were just going to an ordinary university.

Stephen Longmire, student: Whether or not you regard it as desirable, do you consider coeducation at Deep Springs inevitable?

Sproull: (laughs) Oh, I don't know. Death is inevitable, that's the only thing I know about. I don't understand what the force of your question is. I'm sure you're curious about it, I've been curious about that question myself. But no, it really doesn't. If you consider, I for example consider, if we continue the way we're doing now, as I mentioned before, mutually assured destruction, I consider the liquidation of western civilization inevitable. And that forces me to do a lot of work. On the other hand, if I considered that it was inevitable for Deep Sprionqs to go coed, would I just quit giving talks like this and so on? I don't know. I might still try to fight a losing game, I don't know. I really don't know. I think a lot depends upon what the supporters of Deep Springs look upon its way of accomplishing its purpose (as being), and maybe they'll have a different idea about that.

Ken Odell, alumnus, legal counsel to the Board and former trustee: I'd like to make a response to that also. Whether it's inevitable or not, I think it's equally likely that Deep Springs will become coed or end....Bob likes to talk about the eternal life of Deep Springs. I don't think anything lasts forever myself, particularly not from the legal stand-point....

The next question is inaudible, but the response is as follows:

I think that's an excellent question--there a lot of questions subsumed in what you said. Remember that I have not quoted from L.L. Nunn, I have not used the catechism according to L.L. as the Bible by which I then decide basic issues like coeducation. I think the approach I've tried to take--and I don't know how well I've gotten it across--is to try to figure out what is essential to accomplising the purpose that L.L. set this place up for, and that we have signed on (to), by becoming members of the student body or members of the Board--what is essential to accomplishing that purpose, and if it needs to be translated into a different society seventy years later, make that translation. As I say, I was not opposed to calling it a college because I could see that it did not threaten the purpose that L.L. set it up for and in fact in many ways, in getting attention through publications and accreditation and a lot of the bureaucratic apparatus that goes along with modern civilization, it was helpful. So, it did not threaten the basic purpose. I don't know what L.L. would have thought if he'd been alive, but you try the best you can to see what's essential and what's only peripheral. Different people would have different views about to what extent, for example, the nibbling away at isolation threatens that purpose. My view is that the basic emancipation from current responsibilities outside, from complications and so on, the ability to study deeply into history and literature and so on, to interact with one's peers and different age groups and make a commitment to his own career--that peroid of two years is a very luxurious experience that is made available to you, and I just believe that coeducation threatens that. So it is not a question of trying to go point by point and seeing what L.L. would have permitted and what he wouldn't have permitted. I just don't find that very rewarding, and I think this is the basic role of the trustee: the reason you have trustees is so that you don't put everything down on paper and say exactly, this is the way it's going to be, because you recognize that society changes, and so each one of us has to come to that conclusion ourselves. But I think in order to do that we ought to go back and recognize each time that this place was not set up to be just another junior college. There's no point in that. It would be dishonest to sit on the Board if that's all you want to get from this place, or give to this place. So we have to ask, whenever any basic issue comes up, to what extent does that interact with the ability of this institution to get on with its purpose. I don't think I've answered your question very well, but it's a deep question and I've done the best I can.

Robert Sterbal, student: What is the difference between the trustees' feduciary role...and their responsibility to the student body...? In other words, when I look at the Trust I wonder, did L.L. Nunn really want to commit major decisions to the trustees, or have the trustees just assumed the responsobility...?

Sproull: Well, you've probably read the writings more recently than I have, but I think you're wrong. I just don't think that's the way that I remember reading the writings, and it's not the way I remember the accounts of history, some of them written, some of them oral, that I've heard. We were talking about this the other day--L.L.'s second thoughts after setting up the Telluride Association. Remember Deep Springs was set up after T.A., and it was not set up like T.A., it was not set up with the ultimate authority in the hands of the beneficiaries of the trust. And that was intentional--but Bob Aird knows much more about this than I do. I guess I don't think there's any doubt but that the trustees have the feduciary responsibility. They certainly have the responsibility to listen to the student body, and the student body has the responsibility to think and analyze any question like this as deeply as it can and to make sure that it is heard in as effective a way as possible. But I don't think there's any question but that thew ultimate responsibility lies with the Board. Maybe I better go back and read L.L.'s writings again, I don't know....Is that a serious question? What about other trustees; how do they feel about this?

Edwin Cronk, chairman of the Board: I think you're absolutely right. He refers to the students as the "beneficial owners," implying that they're the ones that reap the benefit of the institution, and the whole thing is here for them; but the decisions are made by the Board.

(voice onrecognizable): It also says in one of L.L.'s letters to the student body that the student body must try to wrestle all the power they can from the trustees. He says very obviously that there's going to be this give and take between the trustees and the student body.

Odell: I think the comment made was a very accurate description of the four corners of the Deed of Trust, which basically says that all the power is in the students, and the only reason there are trustees is that the students are too young to hold title to property....I think, on the other hand, as we were saying last night, that L.L. Nunn was very adept at talking out of both sides of his mouth. If you read the letters, in contrast to the Deed of Trust, you get the impression of a dictatorship, that if you left the gate unlatched, you'd be crucified. Putting the two together, it's clear to me that the power is with the trustees, regardless of what the Deed of Trust says. It says in the letters that the students have to wrestle the power from the trustees because that's where the power is.

Cronk: Well Bob, thanks very much.

023 Deep Springs -1-
Sunday morning talk 19 Oct 86

— "Damage limiting" — you chose properly, only second time in 12 years invited to speak, & now safe.

— The D.S. issue I would like most to talk about is the question of how to make the institution survive. Our 1980 planning report attempted to focus attention on the _total_ contribution DS would make over its lifetime. Students, faculty, and even the Pres. have strong incentives to focus on the _contribution per year_, the quality of the program, amenities (if any), and other attributes perceived by people in the system and leading to changing the lives of the students involved. But a program that is only (say) 80% as effective in producing lives of service for an inst. lasting ~~for~~ forever is clearly more of a contribution than 100% for 20 years. One of the points we called attention to was the enormous leverage of changing strategy when and if one got close to a breakeven budget. In the years since 1980 by excellent leadership and dedicated fund raising efforts, we are now close enough to make this line of thought important. I should like to urge strongly that everyone study whether it is now possible to plan for infinite life.

This may be only chance. The classes up through the Whitney years are beginning to ~~die off. In 10-20 years~~ have their earnings decrease by retirement. In 10-20 years, the highest earning alumni will be from the bad old days of the Fort [illegible] period, and their devotion is in question. So the next few years may be those of greatest fund raising potential for some time to come. If we cannot make the transition in the next decade, we may have lost our chance.

- The national issue I would like most to talk about is MAD & BMD — how we secure the best chance of having a 21st century. I am extremely pessimistic, partly because of actions & inactions by the Reagan administration & partly because of the extremely low level of discourse on university campuses & in the newspapers of both coasts. I believe it is vital for your generation to study these issues in great depth and prepare for leadership positions.

- But I won't discuss either. Because of the importance to D.S., because ~~I am uncertain that~~ there are few at DS now who have heard the last discussion of coeducation in which I participated, and because I am anxious that the discussion at DS be exposed to the widest spectrum of views, I wish to present my own view of this issue.

Damage limiting.

1. Ack. highest motives
   - one offering things that almost everyone wants to improve the place
   - but the fact that one's motive is higher than another's doesn't make that his necessarily the right course of action.
   - and brass vacuum system

2. Ask you to ack. that those T's who oppose also may have high motives
   - In particular, ask you to ack. that those of us who oppose are not necessarily sexist, keep women in the home, don't throw back to 19th century
   - Speak only for myself: Spent all but 3 years of my life in coed institutions. Probably worked harder & longer for coed education than most of you will. Perhaps the 3 years in a single sex institution, with vacations of course, poisoned me, but the jury will have to remain out on that. I did not & do not oppose TA House / TA going coed. In today's environment

3. ~~So why do I oppose coed at DS?~~ I would probably not have the courage to create a single sex college if it did not already exist. But I know I would not try to create a coed college in the DS mold.

3. DS in no sense should be looked upon in the sense of the categorical imperative. We have not asked / are not asking other insts. to emulate us — in isolation, in intricate / interweaving work / study / SB govt., or in zero tuition / board & room (according to the sociologists / educ. theorists an anachronistic, ultra-conservative, anti-social system). So if DS remains single sex, it is making no statement about anything except its own destiny and modus operandi. It is different & expensive.

4. So why am I (and some others) opposed? (Speak only for myself). It is not doctrine or social theory, or AL Nunn's writings or history. I believe we all agree on that. It is for intensely practical considerations

arising out of D.S.'s mode of operation. The prominent elements are small size and isolation / location. Convolution of the three. and what those are intended to produce.

5. Small size. But first one consequence

First and almost trivial is the maintenance of a ratio. Spokesmen for coed have usually assumed nearly equal populations (say 8-12 to 12-8). This is very unlikely without grossly complicating admission / selection process. I once gave a lecture on the Bernoulli (binomial) dist. that went over like a lead balloon. Maybe Brandt or Peter or Jim Schaal can take over. But that is only the beginning, if you use 0.5 a priori prob. Must consider that can be 5-15, either way, as chance takes over. I do not envy you the endless arguments about whether to take the next most promising student on the list or take the student who would even the sex ratio.

6. But my main problem is the intensity of the DS experience — the isolation, the interpersonal tensions, the effect of small number & the circumstance that there is no place to hide. One must continue to live with one's associates no matter what happens, or leave the valley with a sense of failure. Even outside DS it is a period of tension in one's life — suicides second only to auto accidents in your age group. Not making any dire predictions — only pointing out the rapid change, tension,

7. Part of the DS idea is to take a key period in one's life & be remote from complicating elements of all kinds, supported by a relatively homogeneous community of purpose, to think deeply about one's relation to society, one's career choices, to make personal commitments — rarely ever spoken — to make the world a little better. This place has a unique purpose — it does not seek to be like every other place. It is an extremely expensive emancipation from everyday complication — $20,000/yr.

To me, preserving & enhancing this freedom from complications of all kinds is so close to the core of the DS idea that I don't want to tamper with it, Has been monotonically approaching "college," role of faculty — it's increasing in non-student population — isolation less effectively by those abilities.

8. Pressures always to make DS like other insts — if it does become close enough to others, no point in it.

9. I've said nothing about alumni/contribs. My guess is that if you put names down with realistic $, negative, but only a guess and not the central point. I want no part of destroying life of DS.

10. Br. tirally, no such thing as an "experiment" or a "trial" — no turning back. Not quite as firm as Sec. Law of TD (which says same thing), but once go in the direction of broadening & becoming like others, rarely can an institution re-focus its activity into something different from others. Thus must look at it as making a substantial change, with great risk to the contribution DS can make to society & with no realistic possibility of turning back if it is a mistake.

(insert over)

11. This is the way I look at it; of course I could be wrong. I doubt if there is any other Board member who, even if he agreed with conclusion, ultimate way of voting, views it just like this. As I have said, I respect the motives of all, but I urge you to think long and hard about losing the distinctive features of DS and its own way of accomplishing its purpose.

12. ~~Close with two other ... of~~ (wares / notes)

Anecdote: W. L. Beriach, Treas. wrong, not vicious, not bright, kindly. Maint. Inst. Pers. None of us had seen the word before. Didn't look it up. (Tota ...). "Personality" H.L. Mencken & Amer. newspapers. "adsorb" "causal".

Well, institutions do have personalities. D.S.'s started about as different as you can get. "Relaxing toward the mean."

→ examples (upper right)?

Much writing on universities speaks of "integrity" – threatened by govt, by crazies, by fragmentation & so on. I've never liked. Rock

disintegrity. Eric Ashby (Lord Ashby of Leeds), ~~thoughtful~~ writer, introduced a better term. "Inner logic." I believe this is the most sensible way of looking at D.S. It has its own inner logic, the practices, doctrines (if you will) that distinguish it from others that enable it to do its work. Small size, isolation, location, traditions, LL's writings, are all part of it. As I have said, these ~~add to a~~ implement the purpose of altering promising young people's lives toward ~~a~~ preparation for a dedication to lives of service. A certain amount of mutatis mutandis can be accommodated without threatening the inner logic of an institution, but the complications of coed. in my view threaten the inner logic of D.S.

Insert, top of p. 5

G M Remarks 6 April 86

"A Research Educator's Viewpoint on Science, Technology, & the Modern Industrial Corp."

R. Feynman, Geneva, 1958 "27 particles in 25 minutes; let's get on with it."

Four points:

I Science is not on a "growth curve," approaching some kind of saturation. ∫. Tired old field like optics (Maxwell's Equations): along comes phase conjugation. Q. Hall Effect. Biology may just be entering its flowering phase.
But special role of US science may. i.e., fraction of world total's discoveries that are US may ∫. (Germany, UK, France, ~~and~~ up until 1930).

II Selection of fields then becomes critical. If can't do all, do the pregnant ones well without skimping. Dan Koshland, Science 4 April "The greatest discoveries often (sic) come from the unplanned. The transistor, the laser, and recombinant DNA were not foreseen by either bureaucrats in Washington or scientific seers."
Yes, but once Stern & Gerlach, then Rabi, Townes' maser, clear that an field of enormous promise. Similarly, Schottky, wartime Si & Ge rectifiers, Shockley ~~saw~~ Bardeen, others saw that narrow band-gap semiconductors gave promise of solid-state amplifiers & switches. Avery, McCarty linking genetic transformations to DNA.

III Univ. - Ind relns. developing more variety, and that is good.
- Scare of Cambridge - Calif group on genetic engineering; 20th Cent Fund
- Settled down now to a variety of benign & productive relations, characterized by openness on all but $, which aren't interesting.
- Meanwhile more than ever need for univ. consultants into corps, especially small ones, & indust. scientists & especially engineers in universities. Culture of grad. educ. still can be developed in engineering toward joint work. Harvard vs Georgia Tech. (Geo. Tech is getting better; Harvard then needs to get better connected to compete)
- Industrial corp. fellowships & scholarships excell, espec. when connected with summer jobs - GM
- Shortage of engineers in 90's - demographics almost always wrong.

IV Finally, 3 levels of GRH: 1.) GRH 2.) Deficit 3.) High consumption society, providing defense umbrella for Western World & still a little help (gloves-on competition) to Third World.
Must face as graceful as possible a transition to < #1.
Problems concealed, delayed by prosperity, low inflation, low oil prices
When it hits, all the more rigorous because of delay.
Beginning to hit universities, science, engineering. But res. univs. still great places. A grad. student can see just how far he can go with his taste in selection of problems, his imagination, and his standards of when a work is finished. He may never in his life get a similar opportunity. Many doubtless call this a "luxury," but I hope it is one of the last luxuries to disappear.

THE FUSS ABOUT SUPERCONDUCTIVITY

Robert L. Sproull

October 1987

The press and television, so ordinarily unconcerned about applied physics and always total innocents about basic physics, have featured it repeatedly. It has been called the greatest technological development since the invention of the transistor. The American Physical Society staged a technical session about it in March that was mobbed and continued into the small hours of the morning; the meeting, which has been called "the Woodstock of physics," officially ended at 3:15 AM but the discussion terminated only when the hotel management cleared the room at 6:00 AM. The manuscript for the basic technical publication about it contained a crucial error (ytterbium was substituted for yttrium) which the authors corrected in proof, which they claimed was a secretary's error, but which everyone else believes was intentional, based on their conviction that the APS publication process leaked like the Pentagon (and they were right!). Thousands turned up for a White House conference about it in July. Already at least eight bills have been introduced in Congress about it. And so on and on.

What is "it"? "It" is usually referred to as "high temperature superconductors." I shall try tonight to put some perspective into this development and what it means in practice, to guess where it is going, and to ask you to face some issues.

First, of course, I need to review what superconductivity is. "Super" is used so uncritically and even casually now that prefixing it to "conductivity" fails to capture the truly remarkable phenomenon. Kamerlingh Onnes in Leiden discovered it in 1911; he cooled a sample of lead below 7°K and found that its electrical resistance vanished! The resistance did not just get dramatically lower, but it disappeared. If an electrical current was established in a ring of lead, the current persisted for as long as the ring was kept cold (as one could verify by measuring the magnetic field produced by the current, which could be observed without touching the ring).

The Leiden group quickly discovered many other superconducting metals and alloys, but curiously the metals that are especially good conductors at room temperature (silver, copper, and gold) are not superconductors even at the lowest temperatures that have been reached (a tiny fraction of 1°K). The vanishing electrical resistance is perhaps the most obvious feature of the superconducting state, but that state has a fascinating array of other differences with respect to ordinary matter (if we have time, we can discuss the "Meissner Effect"). The superconducting state of solids and the superfluid state of liquid helium II are so differentfrom everyday matter that they have appropriately been called a "fourth state of matter" (after solids, liquids, and gases). But we must leave all these interesting branches of our subject and concentrate on zero electrical resistance.

Now you can easily appreciate the appeal of zero electrical resistance and how an inventive mind could harvest it for many economically important applications. But for forty years after 1911 superconductivity remained (except for physicists) a curiositywith no connection to our real world. To understand how this happened, one needs only to understand that no material was found that was superconducting above about 20°K and to understand the cost and complexity of apparatus to produce and maintain temperatures like this.

To explain the latter a little more I need to recall your high-school chemistry (or physics). You will remember that the Kelvin scale (or "physical scale") of temperature has its zero at the absolute zero, where all random motion of atoms, electrons, or anything else ceases, and its degrees are the same magnitude as Celsius degrees. (Both Celsius and Fahrenheit scales are arbitrary and "biological" in that they feature the temperature region of human experience and especially that above the freezing point of water.) Zero °C is at 273°K, so 0°K is at -273°C or -460°F. Now a household refrigerator and freezer cools from about 80°F to about 10°F. This seems like a lot to us but it is only 70° out of the 510° temperature difference required to cool to the operating range of metallic superconductors, and even to do that 70° requires three times the amount of electricity that would have been required to produce the same temperature change in the opposite direction (electric heating). But that is far from the worst problem. For fundamental thermodynamic reasons, the efficiency of cooling engines is roughly proportional to the absolute temperature, which circumstance drives up the size and energy consumption of cooling machines rapidly as the temperature drops. And then there are additional complications such as the freezing of contaminants, notably ordinary oxygen and nitrogen from the air, which not only block the flow of the working fluid refrigerant (helium gas) but score pistons, cylinders, bearings, and valves as effectively as would sand.

Thus as long as the highest temperature at which a superconductor could exist remained in the range of a few degrees Kelvin, superconductors remained in the laboratory and out of popular view.

In about the year 1950alloys of niobium (Nb_3Sn and NbTi) were discovered to be superconductors to about 20°K, albeit of a type more complicated than metals. Over a period of fifteen years they were developed to the point where high-strength magnets were manufactured using NbTi coils, magnets with magnetic fields about ten times that possible with iron poles and copper coil windings, and of course without the enormous energy consumption and cooling problems of large conventional magnets. Superconducting magnets became very useful in the accelerators and detectors of the particle physicists and the Magnetic Resonance Imaging of the radiologists. But despite long and expensive effort, applications such as electric generators, motors, and transmission lines were simply out of the question because of the immense cost of maintaining temperatures in the range of 20°K; running trains or elevators on magnetically levitated guideways was even more remote.

This was the situation until about a year ago when it was discovered that some complicated oxides of copper, barium, and lanthanum showed superconducting properties. Oxides are ordinarily excellent insulators, but oxides of particular combinations of these three metals were found to be metallic conductors at room temperatures and superconductors in the 20°K to 30°K range. That in itself was interesting enough, but the real excitement started because these compounds were quite different in many ways from the known superconducting metals and alloys, on which research had pushed so long and hard that it had essentially reaching a dead end. Many groups started working furiously on this new tack. The oxide discovery was made at the IBM Laboratory in Zurich, primarily by a team of a Swiss, a Japanese, and an American. The follow-up occurred there and at least at the following: The University of Tokyo (two separate teams), Academia Sinica at Beijing, AT&T Bell Laboratories, IBM at Yorktown Heights, and many U. S. university teams. By February 1987 the lead had proved to be remarkably fruitful. Teams led by Paul C. W. Chu at the University of Houston and M. K. Wu at the University of Alabama at Huntsville (both Taiwanese) submitted technical articles describing how they substituted yttrium for lanthanum and achieved superconductivity at the astounding temperature of 90°K. Announcements came from China and Japan, without specifying the composition, of similar successes in the next few weeks while the Chu and Wu papers were being published.

Now 90°K may not seem to be all that much warmer than 20°K, and it is still very cold (-297°F). So why the excitement? There are two principal reasons: First, 90°K permits many economically sound applications that 20°K does not. Nitrogen boils at 77°K and oxygen at 90°K. Refrigeration to 90°K is simpler because these contaminants are no threat and can even be helpful, and it is much cheaper for the fundamental thermodynamic reasons mentioned earlier. High performance motors and generators and even transmission lines are probably possible, and many more applications in computers, communication, and transportation will exploit more subtle properties of these new superconductors than their electrical resistance.

The second reason is that this new class of superconductors clearly works by a process different from the superconductivity known before 1986. This fact opens the possibility of even higher temperatures, perhaps as high as room temperature (or at least outdoor Rochester in the wintertime!). Instead of butting against the hard upper limit of a mature technology, one now enjoys the open-endedness of an infant technology.

And so the excitement and fuss are thoroughly justifiable. But there is so much that needs to be done before these new materials can reach the stage of engineering exploitation that large issues are created for industry and governments. Before addressing them, however, I note that universities have a simpler decision problem: Graduate training and research will appropriately expand sharply in superconducting science and related fields of physics, chemistry, metallurgy, and electrical, chemical, and mechanical engineering. (Only the decision process is "simpler"; getting the resources or dropping other work may be difficult and complicated.) The understanding of phenomena in the new materials is still very murky, and advancing this understanding is a clear target for university research. Both the training of people and the advancement of understanding strengthen the infrastructure, the underpinning of the Nation's capacity to exploit the new materials. (It was this infrasructure, created in large part by the Federal support of solid-state research and graduate training in universities, that enabled the U.S. to profit so quickly and effectively from the invention of the transistor, obtaining as we did for twenty-five years a competitive edge in all solid-state devices and the computer and communication industries based on them.)

A corporation's problem in deciding what to do and when is much more difficult, in large part because of timing; the problem of governments, Federal and state, in deciding what kind and rate of development to encourage and sponsor, is similar. After I provide some more information about why the path ahead is clouded, I will ask you to consider what industry and governments should do.

To see the kind of development required and to get a feeling for the time required, let us consider the example of an application of superconductivity that is already common (albeit expensive), namely the use of superconducting coils to create the very strong fields in devices like the Magnetic Resonance Imaging machines that are being introduced at considerable expense into the medical care picture and like the synchrotrons (high energy accelerators) of the particle physicists. These magnets are now wound with NbTi. The macroscopic geometry, namely of a string wound round and round in a coil, is apparently like a copper wire winding, which you have seen in electric motors and in electromagnets for opening valves and many other purposes. But the actual structure is much more intricate. Each element (which looks at first sight to be a wire perhaps 1/32" in diameter) is in fact a bundle of tiny filaments, each separated from the others. If the conductor is not broken up into these slim multiple paths, the maximum current it can sustain and the maximum magnetic field it can produce are severely limited; instead of fields ten times or more the strength of iron magnets, the maximum fields would be only 1/100 or so of an iron magnet. I cannot go into the physical reasons for this (involving "flux pinning") but only state that it seems reasonably certain that such treatment will be required for all practical superconductors.

A second complication is that an enormous amount of electrical energy is stored in a high-field superconducting magnet, the energy increasing as the square of the field strength. Once the coil is energized by pushing a current through it, that energy remains in the coil until it is purposefully removed or until the temperature rises above the superconducting transition temperature. An accident that lets the temperature rise can thus release an immense amount of energy which would destroy the magnet unless suitable precautions were taken. The usual precautions are to surround each superconducting filament by a copper cladding, so that the copper carries away the electrical energy.

A third complication is the great mechanical force on the filaments in a strong magnet. You can begin to appreciate this from having felt the "pull" of permanent magnets or the torque of powerful electric motors. Such forces also increase as the square of the field and require "potting" and sophisticated mechanical design.

You can now better understand why the path to commercial applications of the brittle, crystalline materials that constitute the new superconductors may be long and tortuous. The design, materials processing, and manufacturing problems are doubtless greater for these materials than for an alloy of two metals and therefore may take years to resolve. On the other hand, the commercial potential is so great that one's competitors--other companies and other nations--may put out such an expedited effort that caution will cause a lost market.

The NbTi superconductors took about fifteen years to go from a laboratory curiosity to a salable product. The problems are even greater with the new oxides, but the stakes are much higher and therefore more effort can be expected. Should a company borrow money at (say) 10% to create a research and development effort with this delayed and uncertain pay-off? Should a company merely maintain a "window" on the work of others through a modest laboratory activity? Should it rather concentrate on its own products and expect to buy not only the materials but also the technology to use them? Should the Federal Government seriously intensify its sponsorship of superconductor research, which up until now has been very modest? Should state governments attempt to secure an industrial future by sponsoring superconductivity institutes?

I leave these questions with you as I close by reminding you of the obvious: It is a truly remarkable world that can produce a surprise as intriguing and potentially important as these new superconductors.

STRIKING THE BALANCE BETWEEN PUBLIC AND PRIVATE INTERESTS AND RESPONSIBILITIES: SITUATION IN THE UNITED STATES

Robert L. Sproull, University of Rochester

NOVEMBER, 1987

The balancing act in the U.S. is an intricate multidimensional process that has caught the public eye. In a few minutes I shall spread out the tensions along their many axes and describe how we are responding in America, but first I wish to address two questions: What's old? and What's new?

What's old? There is a long and mostly happy history of university-industry relations in the U.S. A key event, although not the beginning, was the Morrill Act of 1862 which provided grants of land to establish institutions in each of the states to foster "agriculture and the mechanic arts." The state universities plus Cornell and MIT became these "land grant" institutions and initiated the tradition of working closely with "industry" -- originally farmers -- through on-campus research and development and extension services which took the fruits to the field. Great universities like the University of Wisconsin created and nourished research establishments that contributed to human welfare through products sold by industrial companies.

This tradition was later extended to all applied fields, especially engineering and medicine, and to private as well as public universities. The Stanford College of Engineering under Frederick Terman's leadership spun off, and remained in collaboration with, remarkably imaginative and powerful industries invented by such now famous names as Hansen, the Varians, Hewlett, and Packard. Terman was in effect the grandfather of "Silicon Valley," Santa Clara County. On the East Coast, MIT which, though a land grant institution, had remained aloof from the agricultural scene, picked up the "mechanic arts" with a unique competence, partly flowing from the success of the Radiation Laboratory during the War (which in turn owed its success to the Stanford Klystron and the British cavity magnetron). The Route 128 miracle began with names on the doors of MIT offices like "High Voltage Engineering" and "Edgerton, Germeshausen, and Grier."

It is a long, and almost entirely benign, history.

What's new? The new, and to many, disturbing, features of university industry relations are: 1.) Universities are now especially hungry, having been stimulated to expand and now facing non-expanding Federal support and a decline in the numbers of 18-year-olds each year. The danger is, of course, that a hungry man may cut corners in his rush to nourishment and he may be taken advantage of in negotiations. 2.) The explosive growth of the applications of molecular biology has created what appear to be spectacular opportunities to make money. The smell of money pervades the field, and temptations abound; access to experienced and successful faculty and to promising graduate students can quickly move a company years ahead. Unlike products based on the physical sciences, biotechnological products can usually be made in off-the-shelf production equipment, and so the time from invention to sale can be short. 3.) Many small, unseasoned companies are emerging, without traditions of healthy working relations with universities. 4.) The rapid rise of the venture capital route of financing gives "start-up" opportunities to groups with little experience.

These circumstances have turned the bright light of public concern upon the whole spectrum of university-industry relations, and we should probably be grateful for that since there are some dark corners. In the recent Twentieth Century Fund's exercise in this field, the background paper, ably conceived and brilliantly written by Nicholas Wade, recounted current horror stories and analyzed the dangers of thoughtless or greedy associations between universities and corporations. It served very well the role of the "two-by-four" beating the mule, namely to get our attention. Yet when the Task Force, made up of a highly diverse group, met and listened to testimony from all quarters, the Task Force's own report said, in effect: It is well that these concerns have been raised, but there are more opportunities than problems here, and society as well as its institutions will benefit if the opportunities are recognized and approached properly.

Prompted by this new attention, let us consider where the problems and tensions lie, in the U.S. but in all fields, not just biotechnology. There is much at stake: Public respect for universities and their faculties is a most important element. Most writers in America say the "integrity" of universities is at stake. I agree, but I prefer to use different language. "Integrity" carries the unfortunate connotation of immobility, and the public often views as self-indulgence the elements, like academic freedom, that compose it. (Harvard might well still be the divinity school it was in 1636 if "integrity" had been worshiped.) I prefer Eric Ashby's phrase: "Inner logic." The inner logic, the processes and principles that a university requires to be a unique servant of society, must be preserved; these include academic freedom, respect for the rights of students, tolerance of outrageous views, and continual attention to raising the level of discourse.

The dangers and tensions are so well known that I shall take your time here only to list the axes of tensions: 1.) Basic vs applied research. 2.) The professor vs his department. 3.) The electrical engineering department (for example) vs the history department. 4.) The electrical engineering department vs the University. 5.) the university vs the state and Federal governments. 6.) The professor vs his students. 7.) The funding of the corporation's own laboratories vs funding by it of university research.

I do wish to comment, however, on a pervasive component of these tensions, namely the concept of "intellectual property" (words that one hears often in the U.S. these days). When I began consulting for industry, my value to a company (for which it paid me) was created by the investment in me and "my" laboratory by the Federal Government (research support) and by my university (salary, space, tenure, access to students). What are my rights and obligations with respect to this debt?

The question becomes even more perplexing when it refers to the institutional capability. The facilities, instrumentation, libraries, technician competence, and access to bright young people have been achieved by the university over time through financial contributions by the Federal Government, by a state government or endowment income, by other industrial companies, and by private gifts (many of which could have been employed for, say, the humanities departments). Essential contributions other than financial have been made by generations of faculty, students, and even administrators. How and at what price is this institutional capability to be marketed to the company that wishes to pay only marginal costs?

How are we approaching these and the other problems and questions raised by university-industry cooperative research relations? How are we balancing the conflicting interests and values? I acknowledge at the beginning of my answers that they will unfortunately but necessarily convolve the "is" with the "should be." They will constitute what I, and not surprisingly the Twentieth Century Fund's Task Force, consider to be the best practice. I am happy to be able to report that the motion throughout the U.S. is quite generally toward this best practice, although in many cases it is taking protracted negotiations to reach it.

1.) The first essential response is that there should be acknowledgement that cooperative university-industry research arrangements are <u>not a zero-sum</u> game. Both "sides" win. There is no necessity to give up or compromise institutional values to achieve strong positive benefits from joint relations. These benefits far exceed the monetary ones of sharing pooled costs, supporting students, buying equipment, or replacing diminishing Federal funds. Association with industry flavors graduate study in healthy ways; in Harvey Brooks' phrase, it gives them "respect for applied problems." Association provides access to experimental processes or materials; it expands the "invisible college" of the research community. It helps to keep the innovative faculty on the campus, in contact with the developing generation, instead of losing them to industry (often their own "start-up" companies). It can result in products and processes from research reaching consumers' hands more quickly; Federally sponsored research (except in the aerospace industry, where the Feds were the customer) has usually been an orphan with no champion to exploit it for human benefit. Since it is a positive-sum game, one should not be too fussy about the precise sharing of costs and benefits, a precision always sought by lawyers but attained only at the expense of less research and less imaginative and more constrained development. It is worth a great deal of effort to get nearly universal acknowledgement of the benefits on each campus, since most faculty will not wish to take the time to study individual relations and, as the Medieval cartographers said, we all "Where knowledge fails, place horrors."

2.) The second is that cooperative arrangements should be designed; they should not just happen because someone smells money or someone has an urgent problem. Although (as will be apparent later) there should be central participation by the people who are going to direct the program, there ought to be participation, too, by vice-presidental level people to assure that these principles are obeyed. The individual faculty member frequently takes the attitude that his research is "total war" and he can be insensitive to the kind of public concerns about intellectual property that I have already described.

3.) Agreements should be open and public. It is not necessary or even desirable that one provide financial details or descriptions of just what is expected to be accomplished (that speculation will almost certainly be wrong anyway). But everyone in the university and company communities should know who is participating and what the general terms of the agreements are. These agreements should be a source of pride, with no reason to be secretive or even coy about them.

4.) Publication of important results should be assured. Graduate students cannot properly be integrated into the work if publication of their theses could be stopped or long delayed, and without graduate students most of the benefits of the cooperation vanish. Delay by periods like 90 days for filing patents and exploring ramifications and commercial implications is acceptable, and in fact that is what happens now with Federal support or no external support. But there is a serious question whether an institution upon which public policy bestows the privilege of exemption from most taxes should be involved in any activity that cannot be published. Of course, if through the success of the work, avenues are opened for proprietary or even militarily classified development, these can be taken off campus and exploited.

5.) Concern for the interests of students should be built in from the beginning. They should share appropriately in publication credit and patent royalties and should be well aware of the commercial interests and affiliations of their professors.

6.) No single department, and certainly not the whole university, should become attached to only a single company. It is essential that students and faculty be able to talk with (and have summer jobs, career jobs, and consultancies) with any companies of their choice and not be constrained by the impression that their department has been captured by another company. The university should not take an equity position (of the sort Harvard flirted with but quickly abandoned) in a company in which some of its faculty participate.

7.) Aside from the foregoing points, which are so basic that they probably deserve the label "principles," <u>doctrine</u> should be avoided. The hallmark of American higher education is the <u>diversity</u> of our colleges and universities. The opportunities, problems, specialties, affiliations, and management styles differ widely among institutions. Any attempt to force all to respond in the same way to opportunities for industrial cooperation would only generate noisy faculty meetings and interfere with productive relations.

Beyond these seven points, there are additional considerations which I present in the form of "red flags" and "green flags." The spirit here is that one should not insist on any one green flag (favorable signs) or on all green flying; if a particular program exhibits enough green, a red flag or two would be tolerated.

Green flags

a.) Field of activity described broadly and generically, as basic as possible.

b.) More than one company in the program. *I acknowl. difficulties [illegible] of money.*

c.) Relation developed out of respect for a faculty member who consulted.

d.) Individual faculty member or members commit careers to success of program. *Local champion*

e.) Faculty involved have reputation of institutional orientation and success in teaching.

f.) Program interacts positively with teaching. *easy in medicine, engineering*

g.) Easy, simple procedures for *approving* release for publication.

h.) Summer jobs for students, in industry (preferred) or in program.

i.) Company people seconded to university.

j.) Program pays fair share of indirect costs.

k.) Financial rewards explicitly shared to some degree with those departments that are unlikely to attract industrial cooperators. Prof. Moses' pt of view

l.) University owns the patents.

m.) Royalties and other revenues shared in proportion to input support; some consideration for individual inventor.

n.) Well-staffed university patent department.

o.) No exclusive licenses of patents.

p.) (Less vividly green, but still green) Exclusive licenses possible up to five years with "march-in" rights if not aggressively exploited.

q.) Convenient, easy ways for all to be honest and for full disclosure of affiliations and interests.

r.) Long-term agreements; experience fed back to make modifications.

s.) Accounts audited by university's public auditors.

t.) Company has track record of unrestricted or fellowship support and of open-ended, generous agreements.

u.) Process of approval through established, seasoned governance channels of university.

Red flags

a.) Development of affiliation began by president-to-president discussions.

b.) Narrowly specified field, close to marketplace.

c.) Product and process development emphasized, even specified.

d.) A single company attached to a major fraction of a department.

e.) Expectations of early rewards.

f.) Proprietary information used on campus.

g.) Company has practice of using proprietary rather than patent route to protect its discoveries and developments.

h.) Company is unseasoned, facing many problems for first time, under-capitalized.

i.) University people who are part of governance structure of the university are operating officers of the company.

j.) Reporting requirements are specific and elaborate.

k.) Project is segregated geographically.

l.) A department chairman is a principal investigator.

m.) Short-term agreement with short period remaining at time of renewal.

Finally there is a flag that is neither red nor green but colored like the flag of the company's country. There is very little jingoism or "America First" spirit in American universities; if anything the spirit is more like Gilbert and Sullivan's "~~Every century~~ All centuries but this, and every country but his own." But the expensive science and engineering facilities at our universities have been provided in large part by state and Federal funds, and the arguments for these that have been used to justify them to the taxpayers have frequently been that such expenditures were necessary if the U.S. was to compete in world markets. There is thus a special sensitivity if a foreign corporation enters, pays only marginal costs, and secures the benefit of this investment. Yet outlawing such cooperation might ultimately put blinders on our institutions and certainly is not within the spirit of American universities. The situation is unresolved, but most institutions are reluctant to affiliate with foreign corporations unless the corporation has a substantial, bona fide manufacturing operation in the U.S.

Extensive as these lists are, they should not be viewed as comprehensive. The process of weighing the positives with the negatives appears to be underway at most American universities (and even at colleges, where the concerns are especially serious). The agreements that have been announced generally follow the "principles" I have listed and exhibit much more green than red among the detailed attributes. This result is to be expected, since industrial companies, especially large experienced companies, have no interest whatsoever in reducing the independence or destroying the inner logic of universities. The horror stories have usually been of one of two kinds: 1.) A hastily conceived agreement hits the press while it still has unacceptable features; it later is refined appropriately but with less public attention; in a very few cases the public attention may have been necessary to achieve the proper refinement. 2.) A single faculty member seeks substantial personal wealth and is casual or even negligent about his obligations and constraints.

The "situation" in the United States is definitely moving away from horrors toward a promising set of relations that honors the public interest and preserves respect for universities. The balancing act is receiving careful and sensitive attention, and the conclusions at individual institutions seem to fit well their traditions and prospects.

In one respect, however, the balance is lacking: University-industry relations cannot realistically be expected to provide enough for basic research, although the best of these call for some. It is not useful to probe what the balance should be, according to me, since with all of us working as hard as we can to produce more support of basic research, the balance will still be overweighted on the side of applied work. The Federal Government must be relied upon to provide the basic research support that gives a core to be exploited later by cooperative and other applied programs and to provide a vehicle for bringing along a new generation of scientists and engineers. Industrial support is too small (and likely to remain so) and too tied to prompt application (with the corporation's necessity for internal rates of return like 20%) to conclude otherwise.

But with continued effort to maintain Federal support of basic research, with industrial participation adequately attending to the dangers and safeguards I have described, and with the growing experience of weaving this complicated fabric, I am highly optimistic about "The Situation in the United States."

—

My notes for extending my talk

if time permits

1.) Negotiations
   - Freq a faculty member wants it badly, a highly respected fac. member, who may leave [dark side, Prof. Moses problem]
   - this puts univ at a disadvantage. One who cares most loses.
   - attempt to specify every contingency in advance, impossible
   - true that it would be nice to have tidy agreements, but when R&D involved cannot
   - X exper: generic work in theor & appl med. All right until lawyers. 5. Next week, 5 (2 diff) + 1 of ours. I called CEO. Concl: U. would hold patents. corp. & U. would share in proceeds in prop to input; if corp dissatisfied, could cut unrestricted support.
   - negot. more complic. if more than one company, yet advantages in that. Color of money. Periods of approvals, renewals, etc. Finance manual [Airline personnel analogue.]

2.) Govt
   NY Times & Wash Post leading Press in Amer: Govt. support of univs benign, industry wicked.
   ONR tradition: Univ people in ONR, ONR in univ. Minimal paper, steady support, year-to-year funding but warning of cease commensurate with length of assoc. Unclassified. Use of invest. to help Navy.
Sec 203
   Clouds developing in future. Perle - de Lauer controversy. ITAR / EAR
   Corson report of NAS. Last summer, battle was won - ~~fought~~ war was won, but last week Opt. Inst. Eng. papers stopped.
   Reminds us that the NYT/Wash Post view is wrong

Systematic compilation in 1980 – NSF study, NYU people.

Govt. benign, industry wicked – press in US

GUIR

Classification – "sensitive"

Negotiations
- Lawyers want to specify in advance

Govt
See 503 Mansfield "Relevance"

Local champion

New developments in "classification" "sensitive"

multiple sources of support – different colors of money.

UNIVERSITY-INDUSTRY COOPERATIVE RESEARCH RELATIONS

Robert L. Sproull, University of Rochester

A nation needs a strong and stable government. It needs banks, schools, and effective transportation and communication systems. It needs parks, orchestras, and art galleries. Because people differ so widely in their views of what constitutes the quality of life, it needs a variety of other activities; some people have even seen the tablets of jade on which letters of gold proclaim that a nation needs professional sports; I have not seen these tablets but I will not quarrel with those who have.

But to survive and serve its citizens well in the modern world, a nation above all needs aggressive technically based industries and research universities and technical schools. In America, these institutions--industrial companies and universities--have developed independently and largely isolated from one another. But in the last few decades imaginative leaders have created interesting and potentially powerful cooperative relations between the two. The cooperation has entailed a balancing between public and private interests and responsibilities, an intricate multi-dimensional process that has caught the public eye.

There are two reasons why an explanation of university-industry cooperation research relations may be of interest to this audience: 1.) This explanation may help you to understand the various events and signals emanating from the United States, which must be rather puzzling after filtering through the low-pass filter of the American press; 2.) There may be some exportable wisdom from the U.S. experience that would help in the design of cooperative research relations in the Republic of China. I express this second possibility with great diffidence since the ROC is already ahead of the U.S. in important aspects and since translation of the U.S. experience into a different culture is necessarily questionable and possibly unwise. To describe the U.S. experience, in a few minutes I shall spread out the tensions along their many axes and describe how we are responding in America, but first I wish to address two questions: What's old? and What's new?

<u>What's old?</u> There is a long and mostly happy history of university-industry relations in the U.S. A key event, although not the beginning, was the Morrill Act of 1862 which provided grants of land to establish institutions in each of the states to foster "agriculture and the mechanic arts." The state universities plus Cornell and MIT became these "land grant" institutions and initiated the tradition of working closely with "industry" -- originally

farmers --through on-campus research and development and extension services which took the fruits to the field. Great universities like the University of Wisconsin created and nourished research establishments that contributed to human welfare through products sold by industrial companies. This tradition was later extended to all applied fields, especially engineering and medicine, and to private as well as public universities. The Stanford College of Engineering under Frederick Terman's leadership spun off, and remained in collaboration with, remarkably imaginative and powerful industries invented by such now famous names as Hansen, the Varians, Hewlett, and Packard. Terman was in effect the grandfather of "Silicon Valley," Santa Clara County. On the East Coast, MIT which, though a land grant institution, had remained aloof from the agricultural scene, picked up the "mechanic arts" with a unique competence, partly flowing from the success of the Radiation Laboratory during the War (which in turn owed its success to the Stanford Klystron and the British cavity magnetron). The Route 128 miracle began with names on the doors of MIT offices like "High Voltage Engineering" and "Edgerton, Germeshausen, and Grier."

It is a long, and almost entirely benign, history.

What's new? The new, and to many, disturbing, features of university industry relations are: 1.) Universities are now especially hungry, having been stimulated to expand and now facing non-expanding Federal support and a decline in the numbers of 18-year-olds each year. The danger is, of course, that a hungry man may cut corners in his rush to nourishment and he may be taken advantage of in negotiations. 2.) The explosive growth of the applications of molecular biology has created what appear to be spectacular opportunities to make money. The smell of money pervades the field, and temptations abound; access to experienced and successful faculty and to promising graduate students can quickly move a company years ahead. Unlike products based on the physical sciences, biotechnological products can usually be made in off-the-shelf production equipment, and so the time from invention to sale can be short. 3.) Many small, unseasoned companies are emerging, without traditions of healthy working relations with universities. 4.) The rapid rise of the venture capital route of financing gives "start-up" opportunities to groups with little experience.

These circumstances have turned the bright light of public concern upon the whole spectrum of university-industry relations, and we should probably be grateful for that since there _are_ some dark corners. In the recent Twentieth Century Fund's exercise in this field, the background paper, ably conceived and brilliantly written by Nicholas Wade, recounted current horror stories and analyzed the dangers of thoughtless or greedy associations between universities and corporations. It served very well the role of the "two-by-four" beating the mule, namely to get our attention. Yet when the Task Force, made up of a highly diverse group, met and listened to testimony from all quarters, the Task Force's own report said, in effect: It is well that these concerns have been raised, but there are more opportunities than problems here, and society as well as its institutions will benefit if the opportunities are recognized and approached properly.

Prompted by this new attention, let us consider where the problems and tensions lie, in the U.S. but in all fields, not just biotechnology. There is much at stake: Public respect for universities and their faculties is a most important element. Most writers in America say the "integrity" of universities is at stake. I agree, but I prefer to use different language. "Integrity" carries the unfortunate connotation of immobility, and the public often views as self-indulgence the elements, like academic freedom, that compose it. (Harvard might well still be the divinity school it was in 1636 if "integrity" had been worshiped.) I prefer Eric Ashby's phrase: "Inner logic." The inner logic, the processes and principles that a university requires to be a unique servant of society, must be preserved; these include academic freedom, respect for the rights of students, tolerance of outrageous views, testing the soundness of research by open publication, and continual attention to raising the level of discourse.

The dangers and tensions are so well known that I shall take your time here only to list the major axes of tensions: 1.) Basic vs applied research. 2.) The professor vs his department. 3.) The electrical engineering department (for example) vs the history department. 4.) The electrical engineering department vs the university. 5.) the university vs the state and Federal governments. 6.) The professor vs his students. 7.) The funding of the corporation's own laboratories vs funding by it of university research.

I do wish to comment, however, on a pervasive component of these tensions, namely the concept of "intellectual property" (words that one hears often in the U.S. these days). When I began consulting for industry, my value to a company (for which it paid me) was created by the investment in me and "my" laboratory by the Federal Government (research support) and by my university (salary, space, tenure, access to students). What are my rights and obligations with respect to this debt? That question deserves an answer, and yet the question is rarely articulated, and almost never answered.

The question becomes even more perplexing when it refers to the institutional capability. The facilities, instrumentation, libraries, technician competence, and access to bright young people have been achieved by the university over time through financial contributions by the Federal Government, by a state government or endowment income, by other industrial companies, and by private gifts (many of which could have been employed for, say, the humanities departments). Essential contributions other than financial have been made by generations of faculty, students, and even administrators. How and at what price is this institutional capability to be marketed to the company that wishes to pay only marginal costs?

How are we approaching these and the other problems and questions raised by university-industry cooperative research relations? How are we balancing the conflicting interests and values? I acknowledge at the beginning of my answers that they will unfortunately but necessarily convolve the "is" with the "should be." They will constitute what I, and not surprisingly the Twentieth Century Fund's Task Force, consider to be the best practice. I am happy to be able to report that the motion throughout the U.S. is quite generally toward this best practice, although in many cases it is taking protracted negotiations to reach it.

1.) The first essential response is that there should be acknowledgement that cooperative university-industry research arrangements are not a zero-sum game. Both "sides" win. There is no necessity to give up or compromise institutional values to achieve strong positive benefits from joint relations. These benefits far exceed the monetary ones of sharing pooled costs, supporting students, buying equipment, or replacing diminishing Federal funds. Association with industry flavors graduate study in healthy ways; in Harvey Brooks' phrase, it gives them "respect for applied problems." Association provides access to experimental processes or materials; it expands the "invisible college" of the research community. It helps to keep the innovative faculty on the campus, in contact with the developing generation, instead of losing them to industry (often their own "start-up" companies). It can result in products and processes from research reaching consumers' hands more quickly; Federally sponsored research (except in the aerospace industry, where the Federal Government was the customer) has usually been an orphan with no champion to exploit it for human benefit. Since it is a positive-sum game, one should not be too fussy about the precise sharing of costs and benefits, a precision always sought by lawyers but attained only at the expense of less research and less imaginative and more constrained development. It is worth a great deal of effort to get on each campus nearly universal acknowledgement of the benefits generally accruing to cooperative research, since most faculty will not wish to take the time to study individual relations and, as the Medieval cartographers said, we all "Where knowledge fails, place horrors."

2.) The second is that cooperative arrangements should be designed; they should not just happen because someone smells money or someone has an urgent problem. Although (as will be apparent later) there should be central participation by the people who are going to direct the program, there ought to be participation, too, by vice-presidental level people to assure that these principles are obeyed. The individual faculty member frequently takes the attitude that his research is "total war" and he can be insensitive to the kind of public concerns about intellectual property that I have already described.

3.) Agreements should be open and public. It is not necessary or even desirable that one provide financial details or descriptions of just what is expected to be accomplished (that speculation will almost certainly be wrong anyway). But everyone in the university and company communities should know who is participating and what the general terms of the agreements are. These agreements should be a source of pride, with no reason to be secretive or even coy about them.

4.) Publication of important results should be assured. Graduate students cannot properly be integrated into the work if publication of their theses could be stopped or long delayed, and without graduate students most of the benefits of the cooperation vanish. Delay by periods like 90 days for filing patents and exploring ramifications and commercial implications is acceptable, and in fact that is what happens now with Federal support or no external support. But there is a serious question whether an institution upon which public policy bestows the privilege of exemption from most taxes should be involved in any activity that cannot be published. Of course, if through the success of the work, avenues are opened for proprietary or even militarily classified development, these can be taken off campus and exploited.

5.) Concern for the interests of students should be built in from the beginning. They should share appropriately in publication credit and patent royalties and should be well aware of the commercial interests and affiliations of their professors.

6.) No single department, and certainly not the whole university, should become attached to only a single company. It is essential that students and faculty be able to talk with (and have summer jobs, career jobs, and consultancies with) any companies of their choice and not be constrained by the impression that their department has been captured by another company. The university should not take an equity position (of the sort Harvard flirted with but quickly abandoned) in a company in which some of its faculty participate.

7.) Aside from the foregoing points, which are so basic that they probably deserve the label "principles," doctrine should be avoided. The hallmark of American higher education is the diversity of our colleges and universities. The opportunities, problems, specialties, affiliations, and management styles differ widely among institutions. Any attempt to force all to respond in the same way to opportunities for industrial cooperation would only generate noisy faculty meetings and interfere with productive relations.

Beyond these seven points, there are additional considerations which I present in the form of "green flags" (favorable signs) and "red flags" (danger signs). The spirit here is that one should not insist on any one green flag or on all green flying; if a particular program exhibits enough green, a red flag or two would be tolerated.

Green flags

a.) Field of activity described broadly and generically, as basic as possible.

b.) More than one company in the program.

c.) Relation developed out of respect for a faculty member who consulted.

d.) Individual faculty member or members commit careers to success of program.

e.) Faculty involved have reputation of institutional orientation and success in teaching.

f.) Program interacts positively with teaching.

g.) Easy, simple procedures for approving release for publication.

h.) Summer jobs for students, in industry (preferred) or in program.

i.) Company people seconded to university.

j.) Program pays fair share of indirect costs.

k.) Financial rewards explicitly shared to some degree with those departments that are unlikely to attract industrial cooperators.

l.) University owns the patents.

m.) Royalties and other revenues shared in proportion to input support; some consideration for individual inventor.

n.) Well-staffed university patent department.

o.) No exclusive licenses of patents.

p.) (Less vividly green, but still green) Exclusive licenses possible up to five years with "march-in" rights if not aggressively exploited.

q.) Convenient, easy ways for all to be honest and for full disclosure of affiliations and interests.

r.) Long-term agreements; experience fed back to make modifications.

s.) Accounts audited by university's public auditors.

t.) Company has track record of unrestricted or fellowship support and of open-ended, generous agreements.

u.) Process of approval through established, seasoned governance channels of university.

Red flags

a.) Development of affiliation began by president-to-president discussions.

b.) Narrowly specified field, close to marketplace.

c.) Product and process development emphasized, even specified.

d.) A single company attached to a major fraction of a department.

e.) Expectations of early rewards.

f.) Proprietary information used on campus.

g.) Company has practice of using proprietary rather than patent route to protect its discoveries and developments.

h.) Company is unseasoned, facing many problems for first time, under-capitalized.

i.) University people who are part of governance structure of the university are operating officers of the company.

j.) Reporting requirements are specific and elaborate.

k.) Project is segregated geographically.

l.) A department chairman is a principal investigator.

m.) Short-term agreement with short period remaining at time of renewal.

Finally there is a flag that is neither red nor green but colored like the flag of the company's country. There is very little jingoism or "America First" spirit in American universities; if anything the spirit is more like Gilbert and Sullivan's "All centuries but this, and every country but his own." But the expensive science and engineering facilities at our universities have been provided in large part by state and Federal funds, and the arguments for these that have been used to justify them to the taxpayers have frequently been that such expenditures were necessary if the U.S. was to compete in world markets. There is thus a special sensitivity if a foreign corporation enters, pays only marginal costs, and secures the benefit of this investment. Yet outlawing such cooperation might ultimately put blinders on our institutions and certainly is not within the spirit of American universities. The problem is further complicated by the possibility that a cooperating American corporation might exploit the fruits of research through manufacturing overseas, which necessarily makes American labor nervous. The situation is unresolved, but most institutions are reluctant to affiliate with foreign corporations unless the corporation has a substantial, bona fide manufacturing operation in the U.S.

Parenthetically, I should note here that there are ominous signs appearing in the Federal support of research and graduate training in American universities, quite independent of any connections with industry. As Federal agencies are more and more using the argument of "competitiveness in world markets" as they seek expanded funding from Congress, university faculty members and administrators are tailoring their rhetoric accordingly when applying for Federal support. But Congress does not understand how training graduate students from overseas helps the U.S., just as it has never understood how supporting unclassified research strengthens the National defense. Congress and the Federal agencies are then tempted to restrict access to Federally supported research projects. The openness of American universities, which along with their diversity is one of their most precious assets, and the infusion of highly talented young people from abroad, many of whom stay here and all of whom contribute in some way to the Nation's strength, are thereby threatened. The Congress should be concerned instead about the encouragement throughout our educational system for bright young people to seek careers in engineering and science and about the quality and quantity of jobs in technical industries and research establishments; it is this infrastructure that will be our competitive strength.

To return to the green and red flags, extensive as these lists are, they should not be viewed as comprehensive. The process of weighing the positives with the negatives appears to be underway at most American universities (and even at colleges, where the concerns are especially serious). The agreements that have been announced generally follow the "principles" I have listed and exhibit much more green than red among the detailed attributes. This result is to be expected, since industrial companies, especially large experienced companies, have no interest whatsoever in reducing the independence or destroying the inner logic of universities. The horror stories have usually been of one of two kinds: 1.) A hastily conceived agreement hits the press while it still has unacceptable features; it later is refined appropriately but then attracts little or no public attention. In a very few cases the public attention may have been necessary to achieve the proper refinement, but damage is done nonetheless even if the cause of the initial fright is reversed; there is a principle, well-known in the environmental quality field, that "it is easier to scare people than to 'unscare' them." 2.) A single faculty member seeks substantial personal wealth and is casual or even negligent about his obligations and constraints.

The experience in the United States is definitely moving away from horrors toward a promising set of relations that honors the public interest and preserves respect for universities. The balancing act is receiving careful and sensitive attention, and the conclusions at individual institutions seem to fit well their traditions and prospects.

In one respect, however, the balance is lacking: University-industry relations cannot realistically be expected to provide enough for basic research, although the best of these call for some. It is not useful to probe what the balance should be, according to me, since with all of us working as hard as we can to produce more support of basic research, the balance will still be overweighted on the side of applied work. The Federal Government must be relied upon to provide the basic research support that gives a core to be exploited later by cooperative and other applied programs and to provide a vehicle for bringing along a new generation of scientists and engineers. Industrial support is too small (and likely to remain so) and too tied to prompt application (with the corporation's necessity for internal rates of return like 20%) to conclude otherwise.

But with continued effort to maintain Federal support of basic research, with industrial participation adequately attending to the dangers and safeguards I have described, and with the growing experience of weaving this complicated fabric, I am highly optimistic about the future of university-industry cooperative research relations.

MULTIDISCIPLINARY RESEARCH AND EDUCATION

Robert L. Sproull
Harold H. Hall

INTRODUCTION

Renewed interest in multidisciplinary programs in universities has prompted this paper. Working Group II of the Government-University-Industry Research Roundtable (1) chose such programs as one of its principal interests in 1985, approaching the topic through a series of discussions and consultations. The authors of this paper, as members of Group II, have revised their original working paper to incorporate ideas from those interactions. The purpose of the paper is to assemble, in a necessarily personal fashion, perspectives from past multidisciplinary programs as an aid to designers and supporters of future programs. The authors express their gratitude for the inputs received, but remain responsible for the limitations and shortcomings of the final expression.

Multidisciplinary (or interdisciplinary, adjectives which are deliberately used interchangeably throughout this paper) programs have a venerable history, but most of the early history was either at big university equipment projects (like the Berkeley

---

Robert L. Sproull is President Emeritus and Professor of Physics, University of Rochester, Rochester, N. Y. 14627. Harold H. Hall is Vice President, Corporate Research Group Staff, Xerox Corporation, Stamford, Ct. 06904.

cyclotrons which were heavily dependent on innovative electrical engineering, even if performed by "physicists" and directed toward physics output) or at first-rate industrial laboratories. There has always been a small amount of interdisciplinarity, such as the physicist's or chemist's cadging crystal specimens from the geologist. But only in the post World-War II period have substantial multidisciplinary projects flourished in American universities.

It is worth noting at the outset that differences within an academic discipline can be as consequential as those between disciplines. The experimental solid-state physicist has less reason to interact in his research with the particle physicist than he does with the analytical or inorganic chemist. Many scientists and even a few engineers have been highly productive by brilliant contributions in a narrow field. But some scientists and almost all engineers flourish by avoiding building a fence around what they call their "field"; some have done this spectacularly, like Peter J. W. Debye, who was an electrical engineer, a mathematician, a physicist, and a chemist. Whether within an academic discipline or across disciplines, the absence of fences has much to recommend it. But whereas the spreading of one's wings to other fields in his discipline is easy and natural, the crossing of disciplinary boundaries involves institutional friction and restraints, and these are addressed in this paper.

The context for approaching multidisciplinary work is that of a university program supported in part by the federal government. If industrial companies are involved, so much the better, but the questions indigenous to university-industry contracts (patents, publication, foreign corporations, and such) are not addressed here; similarly, state participation can be a major plus but is not addressed. The policies, practices and culture of both universities and government agencies are important to this paper, but the principal obstacles (along with the creative strengths) will be found in the universities.

The taxonomy of multidisciplinary programs, however rich and rewarding in its own right, is not a focus of this paper. The focus is on a university program supported in part by the federal government through a formally organized continuing institutional body. Current interest in technological and international competitiveness drives some of the renewed interest in the subject. Federal and state governments, industry and universities all promote multidisciplinary programs as a means to improve competitiveness. No implication is intended that interdisciplinary programs are either a necessary or a sufficient condition for improving competitiveness. The intent is to describe some of the problems, to characterize some of the circumstances where a multidisciplinary approach may yield benefits not other wise accrued, and to list key principles contributing to success of such efforts.

OPPORTUNITIES

The expanding opportunities in multidisciplinary approaches in science and engineering are well recognized. Problem- and product-oriented research are necessarily multidisciplinary, and so is the creation of a technology base. There is considerable evidence that the part of scientific progress that will be ultimately applicable in some form to human needs will be heavily multidisciplinary. Murray Gell-Mann (2) has commented:

> "It is usually said that ours is an age of specialization, and that is true. But....new subjects, highly interdisciplinary in traditional terms, are emerging and represent in many cases the frontier of research. These interdisciplinary subjects do not link

together the whole of one traditional discipline with another; particular subfields are joined together to make a new subject. The pattern is a varied one and constantly changing."

These remarks are not to take anything away from traditional single discipline areas like particle physics (with implications for cosmology and the "ultimate" structure of matter) but only recognize the obvious power and promise of heavily hyphenated research. This power is nowhere as evident as in biology, where the macroscopic manifestations (development, pathology, and creation of new chemical and biological agents) of molecular structures demand ever more sophisticated combinations of physics, chemistry, biology, applied biology (the art and science of medicine), and engineering.

There is a special opportunity in educating graduate students in an interdisciplinary environment. A graduate student should (in addition to amassing facts and acquiring tools) have the opportunity to see just how far his imagination can carry him in a research project, and the heart of this thesis research is usually firmly in a discipline. But if he is immersed in a multidisciplinary setting while doing this, if he is encouraged by his environment to bring other disciplines to bear on his work and to explore the implications of his results for other disciplines, he can acquire a firm base for richer contributions to science and technology in his subsequent career.

THE ACADEMIC DEPARTMENT

In the structure of the modern American university, the prominence and power of the academic department are both its greatest strength and (in our present context) a serious handicap. The department is absolutely necessary to sponsor, to promote, and to organize teaching and to serve as a vehicle for the faculty's participation in college and university affairs. It plays the key role in the quality control of staff and students; its close relation to its discipline on a worldwide scale is essential. The discipline (e.g. chemistry) is characterized by internal standards for teaching and for research, the latter depending on refereed journals and national and international societies. Although in the interdisciplinary world it is common practice to castigate departments and disciplines, it must be emphasized that they are absolutely vital not only to the university but, paradoxically, to multidisciplinary programs. The important trick is to build on the disciplines and not to let the departmental structure compartmentalize research and graduate education. Recruiting, promotion and tenure are dominated by the department, and this circumstance poses well known and severe problems for those in multidisciplinary programs, especially young people. This is not the place to argue for tenure, although an excellent case can be made for it especially in the humanities and social sciences; perhaps in the natural sciences and engineering instead of lifetime tenure only the tenure characteristic of the better industrial laboratories would be more appropriate. But, in any case, a serious decision about "permanent" employment based on an individual's promise must be made early in his career, and key elements of that must be his concern and competence as a teacher and his soundness, imagination, and productivity in research in his discipline; the department is pre-eminently involved in both areas. This role is justifiable since multidisciplinary programs cannot be counted on to survive for 30 or 35 years, the typical period of a tenure commitment. Yet the interdisciplinary research, teaching, and leadership must also be included in this process (else the young investigator cannot risk interdisciplinary participation), and there must be spokesmen for this "output" and competence just as there are for his work in the department and discipline. More of this later.

Much of what has been said above about the academic department bears repeating at the next higher level of university organization, the college administrative level. Great difficulty attends multidisciplinary research which crosses college levels if the deans do not bless and nourish the interactions.

Sometimes a multidisciplinary program evolves into a discipline, and this is a key element of scientific progress. Psychology in the nineteenth century and computer science in the twentieth are examples. This evolution is completely healthy, but it is also healthy for a multidisciplinary program to remain a program (like "space science") or to disappear after a few years. It is because of this required flexibility that one must insist on a disciplinary home-base for investigators to whom a university makes a lifetime commitment.

Some of Washington (parts of NSF and NIH) is also organized on disciplinary lines. The long established relations of some, say, chemists in universitities with chemists in NSF can complicate the creation of multidisciplinary programs, but this effect is minor compared to the university structure problem.

PRINCIPLES AND PRACTICE

In this section appear those principles and practices that make a multidisciplinary program at a university succeed. The first four or five (the fifth is implied by the first) are essential, and the program is virtually guaranteed to fail if they are not present in a substantial way. The remaining elements are not essential, but their presence will materially help the program, make it more readily defended, and improve the lots of the participants.

1. Incremental support. It is essential that there be resources that the program adds that would not otherwise be available, that these continue year after year, and that they remain under the control of the multidisciplinary institution, not the participating departments. Only by this process can the program be perceived as non-threatening by the relevant departments and can the local director and his advisers have enough influence to make the program flourish. To the extent that this "umbrella" contract provides multi-year funding, it will give the government more for its money since longevity of funding leads to efficiency and enhances the director's ability to accomplish creative research that could not have been accomplished by routine support of principal investigators within the disciplines. The umbrella, especially if long-lived, encourages the university to commit tenure-track appointments and prime space in the heart of the campus. The contract can be the sole research support of new investigators, but established people should, in addition, continue to get support from other agencies. This means that when the director says "no" it is only "no with the umbrella dollars," which makes for a much happier program, and it enables other federal agencies to take pride in the program. Incidentally, this principle of additional resources is the time-honored way foundations have made changes and opened new avenues in universities.

2. Promising field. The program must produce research at the cutting edge of fields recognized by the departments as important. No department should justifiably conclude that its people are being exploited to aid in research of exclusive interest

to other departments. The program must address important scientific and technological questions, be open-ended technically, and be rich in connections with other research and (if possible) eventual applications. It should be a field where a modest effort can make a real difference and yet not be just a problem that once solved, will go away. It wou[illegible] be advantageous if the scale of research projects within the program were of "human scale" (such that a graduate student can have an individual thesis project rather than a piece of a large joint project) and if the research were such that the institution's administration and even Congress can understand and take pride in it.

3. Departmental affiliation. All but the most junior (research associates) investigators should be members of regular academic departments. They should be evaluated on their strengths in their disciplines. Nothing destroys a multidisciplinary program faster than the knowledge, or even the perception that the participants have joined it because they were not able enough "to make it" in their disciplines. An exception may be required for the relatively few senior people managing large and complex special apparatus or central research facilities.

4. Strong departments. Although strong departments make the program director's negotiating difficult, they are absolutely essential for the success of the program. The danger here precisely parallels that in the preceding principle. On the positive side, participation in the program by a department that is nationally recognized as strong and productive greatly enhances respect for the program.

5. Local champion. The support required by the first principle is unlikely unless some respected scientist or engineer commits a major fraction of his career to the program. This individual, the director of the program, must have unexceptionable

credentials in his own discipline, must be respected by the relevant faculty, must be alert to Washington considerations, and must be adept at defending and promoting the program. He will continually be negotiating with his university administration, his federal agency, and his department chairmen to secure resources and to provide an auspicious setting for research and education. His principal aids are the umbrella contract and the "output" of the program. One of his principal problems is to make sure that participation in the program, especially on the part of young people, does not interfere with promotion, salary advancement, and tenure. He will usually enjoy the buttressing and support of an executive committee or other group of respected faculty and associate deans who can speak for their Colleges. In order to attract the success-prone, it is necessary that the champion be given a high degree of delegated freedom to succeed (or fail) without perpetual interference from above.

6. Strong institution. The strength of the college or university and of its traditions is a major factor in the success of the program. It must be willing to take risks in committing faculty positions and space, and it will frequently have to borrow against uncertain repayment promises. It must not be frightened by innovations in contracting or easily be distracted by protests, the most benign of which will be envy by those who think the program has been unfairly favored. It, of course, should have good traditions in computing, library, and other facilities.

7. Shared evaluation for hiring, promotion and tenure. It is a great help if the institution already has a tradition of bringing in faculty from intellectually contiguous departments to evaluate cases for promotion or tenure. This process can then be used to factor into key decisions the individual's interdisciplinary research and education, along with the more "regular" teaching and discipline evaluation. Nor should such shared evaluations start only when some one is proposed for tenure or

promotion in his department; such considerations must be incorporated into the original hiring decisions for the multididsciplinary organization if its staff is not to be led down garden paths.

8. Arenas for interdisciplinary interaction. Much of the stimulating interaction among disciplines will be planned by the director and others, but much will be unplanned and informal. Centrally shared research facilities, libraries, computing centers, conference rooms, and lounges are important elements of a multidisciplinary envirionment. (A good case could be made that the faculty club is as important as the president's office in advancing the work of an interdisciplinary program, but there is no chance of convincing HHS or DCAA auditors of that.) The colocation of people from different departments is of enormous help; their departmental affiliations are secure, and geography can be used to establish the working affiliation with those from other departments.

9. Two-dimensional matrix. An individual has his primary interactions at the intersection of a column (his department) and a row (his interdisciplinary program). In the column, he relates to his department chairman and department apparatus like committees. In the row, he relates to the program director and his associates and advisory apparatus. This degree of complexity seems to be tractable and tolerable by almost everyone. It is unwise to ask the same individual to take a substantial role in two interdisciplinary programs, in addition to his department role. The reason is that in the real world, there are higher order demands (such as the college and the university) which render more than two primary interactions unworkable. More than one departmental affiliation does not usually create a problem, and can frequently provide benefits.

10. Refereed journals. In many interdisciplinary areas, archival, refereed journals are appearing. They are helpful in many ways, especially in evaluating investigators since an article in such a journal cannot be withdrawn if one's standards improve, and each article reveals a great deal about the individual's taste and judgment in choosing research problems and his standards of what he considers a completed piece of research.

11. Industry involvement. If the area is rich in potential commercial applications, it is a considerable advantage to get industrial companies (preferably more than one) involved. Among the many advantages are the realism this contributes and the acculturation of graduate students to the industrial environment. If commercial applications are not plausible, it may be better to avoid industrial involvement.

12. Federal program manager. The program manager in the supporting federal agency is a key element in the program. If he has high aspirations for the program, defends it effectively in long-range terms, and commands the respect both of his superiors and of his university directors, the whole enterprise is auspiciously managed. He should be quickly informed of all interesting developments in "his" universities and should justifiably take pride in them.

CONCLUDING REMARKS

Multidisciplinary programs should not be static but should be constantly evolving, as progress is made and as the outside world changes. Some may evolve, like computer science, into new disciplines. Others may disappear into the existing departments or expire for lack of funds; if the umbrella contract terminates, the program should terminate unless truly remarkable industrial or state support replaces it on appropriate terms. It is easier to drum up support for abandoned children than for abandoned multidisciplinary programs. Parts of a program may be spun off as individual projects or even as nuclei of new interdisciplinary programs.

Partly because of this evolution and also because as Aristotle said, "the unexamined life is not worth living," there should be an external review body for each multidisciplinary program. Although it might be convened annually for interim "readings," its real value would come from a more secular review, say once every five years.

Measurement of the degree of success of multidisciplinary programs is especially difficult, and little is known of dispassionate measurements with the possible exception of the NSF reviews of the Materials Research Laboratories in 1972 and a review by MITRE in 1978 (3). Like educational experiments, these programs are almost invariably reviewed by their promoters (the standard approach for proving the theorem "all educational experiments succeed"). There is, however, good agreement as to how they should be measured. A knowledgeable, unbiased uninvolved group should compare the "output" of research and educated people over a period of (say) 10 years with what they think the same resources would have produced if applied in the routine, individual-principal-investigator way. Such a comparison, of course, presents formidable difficulties.

In closing, two cautionary notes and an expression of faith are offered. Lord Kenneth Clark has said that creative periods in history occur only in periods of great confidence. The present period is characterized by a conflicting combination of confidence and doubt. It might therefore appear inauspicious for new, imaginative, creative enterprises like multidisciplinary programs. This observation need not be discouraging but only suggests caution, and care in designing the programs.

The second cautionary note is a derivative of principle 1 above. The thrust of that item was that a program would not be given even a fighting chance on a campus if resources for it were evidently obtained at the expense of existing disciplinary research support. To a lesser, but still appreciable extent, this principle applies also on the national scale. If new interdisciplinary programs are established during a budget retrenchment period, evidently at the expense of other research support, the national program would be perceived as parasitic. This is less of a problem than if the zero-sum game was played out locally, but it is still a problem. Again, this is not an argument for no new multidisciplinary initiatives, but only a caution that they should be conceived and designed very carefully for specific opportunities.

The expression of faith is that the opportunities for multidisciplinary programs are great and growing, and even in unusually tight budget circumstances, a few carefully chosen programs would be an excellent use of federal funds.

We wish to thank our colleagues in Working Group II for their criticism and comment, and Linda S. Wilson and William C. Kelly of the University of Michigan for helpful suggestions.

REFERENCES AND NOTES

1. Colin Norman, *Science*, 24 May 18, 1984.
2. Murray Gell-Mann, Emerging Syntheses In Science: Proceedings of the Founding Workshops of the Santa Fe Institute, edited by David Pines, 1985, Santa Fe Institute, Santa Fe, New Mexico.
3. James G. Ling and Mary Ann Hand, Science, 209, 1203 (1940).

UNIVERSITY OF ROCHESTER

ROBERT L. SPROULL
PRESIDENT EMERITUS
AND PROFESSOR OF PHYSICS

201 BAUSCH AND LOMB HALL

10 June 1987

Editor

Science

AAAS, 1333 H Street, NW
Washington, D.C., 20005

Dear Sir:

The enclosed paper is submitted for possible publication in Science.

Respectfully submitted,

Robert L Sproull

ROCHESTER, NEW YORK 14627 · TELEPHONE (716) 275-2797

National Energy Strategy

Robert L. Sproull
November, 1990

About fifteen years ago I gave a talk labeled "Fission, Fusion, and Fuss'n'." The burden of it was that we should be exploiting nuclear fission for energy currently and developing fusion for energy in the next century, but all we were doing was fussing. To some extent my talk this evening is an updating of that talk, but, as you will see, my central message remains unchanged.

The structure of my remarks this evening is, first, to remind you of the key position of energy supply for human welfare; second, to sort the alternatives; third, to present the fusion option; and finally, to suggest a national energy strategy. Perhaps I should explain that I wrote this talk when oil was at about $14 per barrel, before the invasion of Kuwait. I have not thought it was necessary to change anything because of recent events; indeed, if oil went to $50 or $100 per barrel, I would sing the same song.

Importance of Energy

It is a mistake to think of energy solely in terms of heating, cooling, lighting, and power for industry and transportation. These are all important, of course. But in addition, once you have abundant energy you can operate the whole manufacturing economy from fertilizers to jet aircraft. Although energy may be used in many forms, producing electricity and hydrogen will doubtless be the basic form, and if hydrogen is not produced directly it can always be generated by electrolysis if you have abundant electricity. Food and the carbon content of plastics will probably be provided more cheaply from field crops for many generations, and mining will continue. But abundant electricity can do all the rest, including supplying unlimited fresh water by the desalination of sea water.

Energy supply and population control are the dominant long-range world problems. Of course, freedom, the quality of life, the preservation and enhancement of Eastern and Western culture, reasonable political stability, and a certain civility in human relations are all **more important**, but these will all **fail** if energy becomes scarce and expensive or if the world population continues to grow unchecked. And in the near term, when there is still an abundance of energy but some have and some have not, the tensions are dangerous and damaging.

Alternatives for Energy

There is enormous confusion about the alternatives from which residents of the 21st Century will choose. Consider solar power, for example. People say: "Doesn't it work? Why can't we use it?" Of course "it works." The navigation lights on the Intracoastal Waterway in Florida are a typical example. Storage batteries at each light are necessary anyway, and the labor cost of exchanging charged for discharged batteries is high. The high cost of solar cells is thus justified, and the intermittent nature of sunlight is no problem. Solar energy for warming swimming pools in Arizona is certainly practical, and there are many other specialized applications. But for electrical **base load,** the electricity supply maintained at all times ready for you to turn on a light switch, solar is still much more expensive than its competitors and outside of the sunbelt is fabulously prodigal in its use of real estate. Doubtless development of cheaper cells and cheaper storage will occur, but they have a long, long way to go.

Other renewable sources continue to make headlines and frequently mislead the unwary. Tides, waves, wind, ocean thermal gradients, thermal gradients on land ("geothermal")--all can produce energy. Geothermal is useful in a highly limited number of locations. Wind is practical in remote installations, as any farmer can tell you, and small contributions from wind machines are common in the American West; but wind electric generators are still too expensive and require constant maintenance. But as **systems** for base load, including storage where necessary and transmission of electricity, all renewable sources are prohibitively expensive. The arguments against them are always **quantitative** arguments, and quantitative arguments are virtually impossible to transmit through the low-pass filter of the American newsmedia. The public is left--or perhaps encouraged--to suspect some vast conspiracy to keep these energy sources off the market.

In any sensible National energy strategy, solar, wind, and geothermal will all be used up to their capacities in specialized applications and locations. But they will have almost no effect on the energy supply.

Waterpower will continue to supply a few percent of the base load, but the net development of waterpower will soon stop, since the remaining opportunities for dams face opposition for the large loss of real estate in the impounded lakes, and the silting of existing reservoirs will reduce the fraction of the year that electricity can be generated at almost all of the power plants.

This leaves coal, oil, gas, biomass, nuclear fission, and ultimately nuclear fusion as the major candidates in a National Energy Strategy.

Coal reserves in the U. S. are adequate for a couple of centuries, but with increasing sulfur content, and coal is by far the most dangerous energy source. Hundreds die every year in mine and transportation accidents. If a single person dies in a mine, the word does not get beyond the county; only if 5 or 10 people are imprisoned in a mine fire, flooding, or cave-in and die an ugly death is any National interest attracted. It is unfortunately a class question, and in America we do not like to talk about classes; Rochester and Boston enjoy an upperclass sit-in at Seabrook but care nothing about the poor workers in Harlan County, Kentucky.

Coal-fired plants indirectly consume a great deal of space, not so much for local storage as for the railroads required. Conversion of oil- and gas-fired plants to coal will be difficult in most places because of lack of railroads. Mine-mouth electricity generation will help if the protesters do not succeed in outlawing high-voltage power lines and if the electrical grid can tolerate the outages due to strikes and accidents.

But the largest difficulty with coal is the oxides of sulfur and other poisons released to the air when coal burns. Scrubbers to remove SO_2 from flue gases add considerably to the cost of electricity, and we are only beginning to worry about disposal of the scrubbed sulfur, usually in the form of $CaSO_4$, in quantities which will soon far exceed quantities of waste from other energy sources.

Oil and gas are premium fuels. Any sensible global energy policy would prohibit their use in any application other than dispersed (home heating and cooling), mobile (cars, trucks, ships, and trains), or as a starting material for the chemical industry. It takes little imagination to predict what the history books of the 22nd Century will say about our generation that burns these fuels in large power plants.

Burning of coal, oil, gas, and biomass necessarily produces CO_2; there is no possibility of "scrubbing" that out. There is great uncertainty now about the "greenhouse effect"--not about the fundamental processes but only about the amount of global warming to be expected if we continue to burn fossil fuels and to clear tropical forests (a "sink" for CO_2). Global climate models are still in their infancy, but it is certain that there will be winners and losers: The annual rainfall in the U.S. Middlewest may well decline; the grain production in Manitoba may increase: arctic regions are likely to warm appreciably and the temperate zone may actually cool; the sea will probably rise.

How "bad" is all this? It is hard to tell. The "good news" is that it cannot go on forever: The supply of fossil fuels will be consumed. In the shorter run, the North-South tensions and the environmental treaty obligations may be more restrictive on our use of fuels and more damaging to our quality of life in the U. S. than any military tensions or treaties.

We are left with nuclear fission as the only safe, non-polluting source for the heart of our energy requirement, namely electrical base load. No one has been killed by a power reactor in the U.S., and the deaths at Chernobyl would not have occurred had that reactor been designed solely for power (it was a combination design to produce power and plutonium for nuclear weapons). No noxious fumes leave a nuclear power plant. The much publicized waste disposal problems are only political, not fundamental or even technical. The fuel supply with breeding will last for centuries.

But the fission reactor industry in the U. S. is virtually dead. Our world-wide lead, created at great expense to the American taxpayer and ratepayer, has been thrown away. Meanwhile the French have copied and extended our technology, and they now export both power (to Germany and Italy) and power plants; they are the only developed nation with a grasp of their energy future. Although four new designs of passively safe nuclear reactors have recently been developed in the U. S., their future is bleak. This situation makes no sense, but it is hard to see how it can be changed. The New York Times reversed itself sixteen months ago: Having helped to kill the Shoreham power plant, as soon as Shoreham was dead they wrote an editorial which called the opponents of Shoreham "nuclear vandals." But that was an isolated piece of parochial New York City nonsense, and the drum-roll against nuclear energy continues. An energy strategy certainly cannot count on nuclear as a key element.

The Fusion Option

Nuclear fusion is the long-term answer for the core of an energy strategy. The fuel is virtually inexhaustible; although cheaper sources will be used, it can be obtained from sea water, and nearly all countries have access, directly or indirectly, to the sea. There are no noxious or radioactive products or by-products, and the induced radioactivity in the structural materials of a fusion reactor is far less than in fission reactors.

But even after 40 years of research fusion is not yet a proven technology available for application to a power plant. Why is this? And why is it so intrinsically safe? It turns out that the circumstances that make it difficult are the same as those that make it safe, and I turn now to explaining a little about the fusion process.

Nuclear fusion is the process in which the nuclei of two light elements combine to form a heavier nucleus. The most promising pair is the rarer isotopes deuterium and tritium of the common element hydrogen. When the nuclei get close enough, the strong nuclear force takes over. The protons and neutrons now have more neighbors, and the nuclear force acts to hold pairs tightly together. The resulting nucleus is now more tightly bound, which means that energy was given off (energy would have to be supplied to break it apart).

That sounds easy enough, but the difficulty is that both nuclei are positively charged and therefore repel each other mightily. Heating a gas of such nuclei to temperatures of the order of hundreds of thousands of degrees is necessary to give the nuclei enough energy to bang against each other close enough to combine. And then the problem is: How do you contain or confine the hot gas, hotter than the melting point of any conceivable material? In our sun, the enormous gravitational force of the sun's mass, which is 330,000 times the mass of the earth, provides containment. There is no possibility on earth of using the gravitational force. Fusion is accomplished on earth in the hydrogen bomb, in which the nuclei are initially compressed and heated by radiation from a fission bomb and react so quickly that most combine before the material can fly apart.

There are two possibilities for containment in a fusion reactor to produce electrical power: First is to emulate the hydrogen bomb, but on a tiny scale; this is called "inertial confinement fission." The name comes about, of course, because the inertia of the fuel elements is the important factor in preventing disassembly before the reaction can take place. In this approach, fuel pellets containing deuterium and tritium are injected into a reactor and compressed and heated by a laser beam or charged particle beam. To stop the reaction, one simply stops injecting fuel or stops the beam. The difficulties of this approach, and hence the reason it is not already supplying energy to the power grid, are all **quantitative:** getting enough energy into the fuel pellet in a short enough time. The numbers are pretty forbidding: The energy required is about that of 100, 1000 watt heaters (the large burner on your electric stove) running for one minute--that is a lot of energy; and the time is just a few billionths of a second. Getting this much energy into this tiny a target in this short a time is difficult enough, but there are even more demanding requirements that I do not want to take your time to go into now.

The second possibility is to use a powerful magnetic field. Since the nuclei are charged and moving, the magnetic field exerts a force on them. By clever design one can create in effect a "magnetic bottle." But the "magnetic confinement fusion" device must be very large, and the magnetic field must be so powerful that only large and expensive superconducting magnets can provide it. An even greater problem is that the moving charged particles create their own magnetic fields, which distort the externally applied field and can destroy the containment. (It is like an unruly schoolroom in which the teacher tries to impose order but the student-student interactions produce chaos.)

I digress momentarily to compare the fusion and the fission reactors. The repulsion of the fuel nuclei and therefore the necessity for elaborate containment schemes in the fusion reactor are quite different from the process in the nuclear fission reactor. In the fission reaction, a neutron (electrically neutral) enters a heavy nucleus, adding its nuclear force and thereby exciting the nucleus, which was on the verge of breaking up into fragments including neutrons, and releasing energy. The neutron is not repelled, and the neutrons from one reaction can create other reactions, without being hot or fast. Thus the reaction can continue, with an abundance of fuel in the reactor, even after we want it to stop, and we rely on external controls to consume some of the returning neutrons and slowly stop the reaction. The fission chain reaction is like a wildfire.

In contrast, the fusion reaction is like burning small sticks, one at a time, and needing to apply a new match to light each stick. One must continually manage to drive nuclei against one another at high velocities in order to make the reaction go. Thus, as I have said, what makes fusion so difficult also makes it so safe. To shut down the reactor one simply stops injecting fuel or stops the precise stimulating of each fuel pellet.

Fusion is currently at an interesting point: Magnetic confinement, studied for 40 years, has finally almost reached the point where as much energy is produced as is required to contain and heat the fuel. Inertial confinement has produced much more energy out than in by using a tiny fraction of the energy from an underground fission explosion, and it is on the point of producing net energy in the laboratory. Both approaches now need large experimental devices to take the next logical steps to make this technology useful. Both suffer as they attack the Federal purse from the fact that their proponents have oversold their prospects in the past and predicted unrealistically earlier harvesting of the research and development. Nevertheless, pathways to a viable energy alternative early in the next century now appear to be secure, needing only commitment and support.

Energy Strategy for the U. S.

A year ago President Bush called on the Secretary of Energy, Admiral Watkins, to propose a National Energy Strategy. Last March Admiral Watkins negotiated a softening of this charge. He now is sending a set of **energy options** to the White House. After analysis and comment by other Federal departments, the President expects to propose a National Energy Strategy in March of 1991.

It is hard to predict how forward-looking, sensible, and courageous this Strategy will be. On the one hand, the history of energy policies so far has been only softening, delaying, and improvising, and the change in charge last March could be considered a continuation of this tradition. On the other hand, delaying until after the Congressional elections next month may mean that the administration intends to take a more courageous stand. The Gulf crisis has created an additional risk: The Strategy may be dominated by short-range considerations and be as wasteful as the frantic moves by the Ford and Carter administrations after the last crisis.

Courage will be needed in at least four areas of tension:

1.) Putting the miscellaneous sources and conservation in their proper place, namely to use as much as appropriate but not to subsidize or to rely on as if they would solve the basic energy needs; each has its aficionados and pressure groups. The "cream has been skimmed" from conservation; although much more can be done, the remaining opportunities are more capital-consuming and have less spectacular savings.

2.) Promoting fission for the immediate future; a complete reversal of media mythology will be needed here.

3.) Taxing the precious sources, oil and gas.

4.) Funding development of the long-term option, fusion, in a deficit environment.

All this is immensely complicated, of course, by events in the Middle East. No matter what happens there in the short term, we can expect uncertainty and instability there as long as the oil lasts.

One final plug for the fusion option: Fusion development, especially inertial confinement fusion, can now move from being a sandbox for the National Laboratories like Los Alamos and Livermore, to being a development project with goals and schedules to be met. As a large project, it is distinctly different from programs like the Superconducting Super Collider and the Space Station: For those projects, we are borrowing money from our grandchildren to have the fun in our generation of learning more about cosmology and the stars. We are deficit-financing self-indulgence.

In contrast, fusion development is to some extent an investment for our grandchildren. It is not, of course, pure investment: The payoff occurs so far in the future that one cannot borrow money on the open market to pursue it. But in the spirit that government should do for the people only those things they cannot do for themselves, this development is a proper task of government.

Furthermore, fusion development can now be done in cooperation with at least the USSR, Japan, and the European Community, thus sharing the costs and emphasizing that there are no "winners and losers" when fusion comes on line: Everyone wins. International cooperation will mean, however, that Congress will have to be more self-restrained in carving up the program each year; once the treaties and funding schedules are established, we shall have to live up to them.

Well, we shall see what happens in March. If the present administration does not adopt a real energy policy, there will be other administrations. But the longer we wait, the harder it will be, and there is mounting impatience in the rest of the world. If we do not care about our children and grandchildren, there is no use for universities or any other forward-looking institutions. If we do care, we should get on with it.

THE WHITE HOUSE

WASHINGTON

December 20, 1990

Dear Bob:

Many thanks for sending me a copy of your paper entitled **National Energy Strategy.** This really is an excellent piece of work and came at a very appropriate time because we are, as you know, just in the midst of finalizing the National Energy Strategy on which Jim Watkins and his colleagues have been working for the past year and more.

My only quibble with your piece is your statement that "an energy strategy certainly cannot count on nuclear as a key element." Although I would not argue with you, in the near future I think that any rational energy strategy must, in fact, take into account the fact that nuclear fission sources are the only ones that produce large blocks of electrical energy and thus replace significant blocks of imported petroleum. More and more environmental pressures are moving in that direction and I have been considerably heartened to find some of my most rabid environmentalist friends now saying that nuclear is clearly our only hope for saving the environment.

I am fully convinced, however, that we must come up with an entirely new generation of modular nuclear reactors that can be certified once and for all and then mass produced before we really can hope to get any kind of public acceptance. I was very much impressed, for example, by the German test about a year ago of an advanced high temperature gas-cooled reactor in which the helium was simply released, the water cooling shut off, and the electrical power removed simultaneously. What happened was that the temperature of the core went up a few degrees and then dropped exponentially precisely according to the design expectations. We need to have more systems that have this demonstrated safety behavior before we can expect our citizens to really accept the nuclear option and, of course, present utilities that own standard PWR or BWR units are very much afraid to even consider the idea of intrinsically safe nuclear reactors because this immediately raises the specter in the minds of their shareholders that the reactors that they now have are in fact not intrinsically safe. Fortunately, they are an enormous amount safer than most other things with which our citizens come into contact and we need an enormous amount of educational activity in the area of risk assessment.

I am currently having a group take a look at this whole question of risk assessment across the entire spectrum of Federal Government activities in the hope of arriving at a set of consensus, assumptions, and principles that would at least provide some degree of uniformity in the way we treat risk in all the different agencies.

You can be proud of what the Rochester group has accomplished with its Omega laser. I was very impressed when I last visited the facility to see how effectively Bob McCrory had used high power laser technologies in a wide variety of fields quite apart from inertially confined fusion. This seems to me exactly the right course of action for university laboratory, and he has demonstrated the very wide range of technological options that are available when very powerful laser energies can be brought to bear. Unfortunately, he is requesting the $47 million upgrade of the facility at a very difficult budget time, and, although I am confident that the upgrade will eventually occur, it may take somewhat longer than Bob had hoped. At the moment, in fact, as you know, the Congress has not dealt well with DOE's total fusion program in part because there is a perception that DOE has not come through with the promised plan of action on the schedule that Congress had requested.

I hope that you are enjoying life in Florida, and would take this occasion to wish you the happiest of Christmases and the best of New Years.

Sincerely yours,

Allan

D. Allan Bromley
Assistant to the President
for
Science and Technology

Professor Robert L. Sproull
E402, 16910 Bay Street
Jupiter, Florida 33477

The Future Comes Quantized

Robert L. Sproull
21 April 1990

I have a great privilege this evening. I have the opportunity to tell a lot about James A Krumhansl. I could, for example, tell how the name Jasper Franhandle originated, I could tell how to fill fountain pen with cigarette ash, and I could tell how JAK's imitation of Karl K Darrow would send shivers up your spine. These, and other, manifestations of Jim's characteristic *informality* could fill an evening.

By contrast, the only *formal* behavior I can remember was his sending a telegram to the then Vice President John Burton all the way from Rockefeller Hall to Day Hall. (I have never again seen a man as angry as Burton was when he received it!)

I contrast Jim's pleasant and effective informality sharply with the stuffy formality of another character from the time when Clark Hall was being built, Julius Weinhold, the Cornell Director of Buildings and Properties. Weinhold was so authoritarian and mistrustful of his own organization that he insisted on personally opening every piece of mail addressed to B. and P. The Cornell Trustees are not noted for their sense of humor, but they did, probably unintentionally, produce a laughable when Weinhold retired: They named a chilled water plant for him!

But I won't tell any of these things.

Tonight, instead, I want to speak of three quanta of time, and the quantum is the 50 years of the Krumhansl period.

1890-1940 was a period of discovery in physics and chemistry that was unexampled in the entire history of science, and it may just possibly remain so. It was a period in which our understanding of the basic laws in the world around us, the touchable world, were discovered and developed magnificently.

1940-90 produced a rich harvest from the seeds sown in the earlier quantum. I do not say that no important new discoveries were made in this period: There were many such in the touchable world but the most famous were primarily applicable to the untouchable world of cosmology or even philosophy. Meanwhile physics and chemistry of eventual applicability on the human scale flourished as never before in history. It was as if pure and applied physics were competing as to which could nourish the other most abundantly. Of course there was never enough money, but no promising new area was neglected.

A revealing event occurred a third of the way into this period. Congress in 1965 held a reprise of the 1946 hearings which had established in the minds of Congressmen the legitimacy of using public funds to build big accelerators. In 1965 two dozen or so physicists testified with compelling arguments why high energy physics ought to be supported, a question which was not at issue, but only two even mentioned the question of the *rate* at which it should be supported, and then only tangentially. That rate was *the* issue for Congress. As the time quantum proceeded, the rate at which science was done became more and more the central issue, although most physicists continued to believe that it was an immoral question.

I cannot, of course, predict the discoveries of the 1990-2040 period or even whether they will upset the applecart of the 1890-1940 understanding or only further extend the spectacular achievements of the 1940-90 period. One can see marvelous opportunities as a continuous development from current work; one of my favorites is the artificial microstructures in solids of only a few or a few hundred atoms. With these, we should be able to learn much about the transition from dynamics to thermodynamics, from the reversible to the irreversible. Perhaps biology will be even more interesting. Our successors are not in any danger of running out of rewarding territory: If all else fails, there is always the intriguing mind-brain problem, still in almost the primitive form that it was left by Descartes. One way or another, it will be an exciting period.

But I do predict that the 1990-2040 period, the next quantum, will be characterized as the period of making *choices*. It will no longer be a sufficient justification to demonstrate that "it ought to be done." Language like "support all promising scientists" will get you nowhere. There is an old Krumhansl joke of which the punch line is: "Yes, of course. But when? And by whom?" The "when," the rate, and the "by whom," whether by the U.S. or others, will be key considerations when a physics department, a university, a Federal agency, or the U.S. Congress is considering research directions. The NSF, for example, has already decided that some fields of astronomy will be supported *only* as parts of international consortia.

Making the physics establishment larger was an appropriate move in much of the 1940-1990 period and made choosing less urgent, but it will not work in 1990-2040. The Federal agencies, especially the NSF, are worried about the demographics which show a serious "shortage" of scientists and engineers early in the coming quantum. But these projections assume that the aerospace and defense industries will be unaffected by the Revolution of 1989 and that the Federal deficit will not further constrain the Federal agencies supporting research. I find both assumptions hard to believe. General Motors used to have an equation that said "large size equals high quality." About 20 years too late, Japanese competition forced GM to abandon the equation. Physics should not self-destruct by similar blindness. Although adding two more professors always seems attractive and solves some problem or rises to some opportunity, that way lies madness in the period ahead.

The problem that makes expansion unwise or even wicked is, of course, that the U.S. is in trouble. We are making the transition from a great, perhaps the greatest, nation to just one of several. The transition is inevitable and to be welcomed; our big-boy-on-the-block behavior and image are nothing to be proud of. But we are making it gracelessly, propelled by self-indulgence, spending beyond our means, a macho defense umbrella for the friends who compete with us, drugs, and illiteracy (both in the schools and in the press). It is a shame that the nation that produced the Marshall Plan and that has played a key part in the education of many of the world's leaders (including Gorbachev) cannot make the transition more gracefully. The remnants of "America First" persist in the SSC, the Space Station, and the Genome Project.

As the diminishing economic and political position of the U.S. forces choices, who is going to make them? Since we now have a Democratic Congress and a Republican Executive Branch, it is popular on American campuses to castigate the administration and to applaud the Congress (perhaps with the clapping of only one hand). But in the long run the Congress is a poor instrument for making science choices. It suffers from constituency obligations, a multiplicity of committees with overlapping jurisdictions, a short time horizon, and staffs whose knowledge and experience frequently do not match their enormous influence. The Congress has been writing more and more restrictive language into their appropriations, weaving a cocoon around the Executive Branch and managing whole programs themselves. There is a school of thought that says that the greatest disaster for Washington was the invention of air conditioning: Without air conditioning, Congressional staff would not remain in Washington the year around to attempt to run the Government.

I urge scientists themselves to serve in Washington, preferably in the Executive Branch, to help make the hard choices. I know the conventional academic reluctance, enhanced by the circumstance of a Republican administration. But you have only one President at a time, and you must not use political antagonism as an excuse for non-participation. You may not live long enough to get the administration of your choice, and then it will disappoint you after being in office for a while.

I make this plea even though it is not as comfortable or rewarding as it used to be to serve the Government. The new law put into effect last July seriously deters officials who might be involved with contract decisions. The Federal advisory committee apparatus is being strangled with rules and the "purity Potlatch." But the key choices are going to be made, and everyone will benefit if they are made by competent people.

The buzzword for exercising choice is now "competitiveness." For industry, of course, this is an appropriate and necessary focus. We must export enough in order to import oil and raw materials, to say nothing of Asian Basin electronics.

But for academic science to march under this banner is dangerous and little short of fraudulent. An invention or idea is not a product, and a product is not an industry. There is a complicated route from basic research through applied research, invention, development, scale-up, and production, and the process is not usually linear. But the rate-limiting step is almost invariably *investment*, first in scale-up and then in production and marketing facilities. We are now over and over again in the frustrating and debilitating position of inventing and developing a promising product only to have it turn out that American industry cannot afford the investment to produce it. Because of our excessive spending and *negative* saving (when we take the Federal deficit into account) the cost of money in the U.S. is 2.5 times that in Japan. U.S. industry must have about a 20% internal rate of return on any investment; a Japanese company can live with a payback in nine years whereas an American company needs to have it in four.

I am not saying that we should not undertake and take pride in research that is helpful to industry. I specifically applaud research that with time and luck is applicable to the touchable world. I am not saying that academic research does not help the country, but only that it is of very limited direct value in industrial competition with other countries, and that its principal value is indirect, in preparing a new generation. Providing a new generation with respect for applied problems and experience in areas like condensed matter physics and chemistry will certainly be helpful. But to emphasize what we can do for competitiveness in proposals for Government grants risks distorting graduate education and research in unhealthy directions.

In the choices involved with big programs the argument is often made that the "fallout" for civilian industry is an important positive attribute. NASA is famous for this claim. But the effect is slight; a dollar on a NASA project, much of which goes up in smoke, is nowhere near as effective in promoting civilian industry as a dollar devoted directly to that industry would be. The absurdity of the claim can perhaps best be seen by an example from another country: Soviet television last February showed spectacular pictures of spacemen flying outside the Mir space station. As reported in *Aviation Week*, "a TV anchorman bemoaned the fact that several Soviet plants turn out 'splendid satellites' but the country has only two telephone factories. He pressed the point with the deputy head of Mir operations, V. N.

Branets. 'Why can the USSR get excellent pictures from space, 'while hundreds of Soviet areas have no telephone systems?' he asked. Branets agreed, 'Every family needs a telephone.' Then another Soviet space official chimed in 'And space technology will make it possible to solve this lagging behind in the supply of telephones.'"

Another kind of choice faces all of us: the choice between supporting the individual (me) and my institution. I urge you to consider that although universities are resilient they are not necessarily immortal, or at least not immortal with the characteristics we know and enjoy. In the next quantum of time universities are going to be increasingly buffeted by populist antagonism, particularly against the great research universities, and by Government intrusion. I do not fret very much about the current silly Department of Justice inquiries about financial aids, since I think that exercise will run its course and fade away in a few years (after, however, intimidating the weaker administrations). But the Federal intrusion will increase, I believe, inexorably.

There is a recent development that has as yet attracted very little attention but that I find especially frightening. It is a derivative from the legal maneuvering in the corporate takeover field. What is the duty of a member of a board of directors? The courts have established that he or she has the duty *not* to have a conflict of interest, *not* to indulge in self-dealing, and so on, but also the duty of *loyalty*. That last is fine for a corporate director, and it is almost as simple as that the loyalty is to the shareholders (not quite exclusively only because of employee and community interests).

The trouble comes when this language is applied, as I am sure it will be, to boards of trustees of colleges and universities. To whom is the duty of loyalty? I believe the courts will answer: to the students. That is not a bad answer for a college, if broadly interpreted to include former and future students as well as current ones. But it will be a very damaging answer if, as I fear it will be, it is applied to research universities, and I fear the courts will consider only current undergraduates as the targets for the required loyalty, since their representatives will be bringing the suits. Who will speak for the duty of loyalty to the next century? You may need to be very active in support of your institutions and their needs to emphasize graduate education and research. An unsupported administration or a weak or political board of trustees can bring disaster.

This brings me to my concluding section, about graduate education in physics at a place like Cornell. The great unwashed with their cliche of research *versus* education do not understand the way Ph.D. education occurs, especially in the *interactive* mode that is characteristic both of Cornell and of Jim Krumhansl.

As you doubtless know, it used to be that all computing was done in the "batch mode," in which you would deliver a stack of cards to the computer, walk away, and come back later to learn that some programing glitch kept your work from running. We now take for granted the "interactive mode" in which we enjoy a (usually friendly) give-and-take with the computer and avoid such massive inefficiencies.

Long before it appeared in computers, the interactive mode was a feature of Ph.D. education at Cornell. The working together of students and faculty in the laboratory and the theory professor's office is a most effective process, and the *informal* interaction that, as I have already mentioned, is characteristic of Jim Krumhansl's work is especially to be admired. That interactive mode is in the Cornell tradition; even Cornell synchrotrons were built that way. Unfortunately that mode is not universal; batch processing is still often practiced in the humanities, where a student can get a powerful batch of professorial direction and go off for several months to process it.

Informal interaction between faculty and Ph.D. students is a marvelously effective way of bringing along a new generation. The research that comes out, interesting as it is, is less important than the understanding the student gains about his or her own abilities and limitations, a revealing self-appraisal. When students publish their theses in the archival literature they tell themselves and the world the *standards* they have set for their work, and that is a uniquely educational process.

In the search for better ways of organizing industry and competing with the Japanese, many writers have called for a "flatter" organization of American companies. By that they mean less vertically organized, less hierarchical, less formal, more participatory, and more interactive. Well, that is fine but hardly new, since physics graduate education, and in particular that supervised by Jim Krumhansl, has exhibited that feature for half a century.

Look around the country: What does the U. S. sell in world markets such that we have competitive mastery? What do we sell that is so superior that we can set the price without considering foreign competition? The answer *used to be* automobiles, and then communications gear and computers, but we are now outclassed in almost all of these.

The only product I know of that fits this description is post-baccalaureate education in science and engineering. We still attract students from all over the world and are the clear quality leader. We set the price and define the product without thought of competition. (Paradoxically, unlike other units that dominate their markets, we discount the price far below cost, but that is another issue.) We should maintain this quality and the joy and excitement that goes with it at all costs, and not let expansion or talk of competitiveness compromise our product.

And so tonight, as we recall fifty years of James A. Krumhansl, I ask you to celebrate the participation in informal, interactive graduate education that has characterized his career.

I. INTRODUCTION

This document constitutes the "house rules" for writing and typing letters, reports, and any other material emanating from the President's Office. It is a somewhat more general document than simply a style manual. A style manual, as the title is usually used, is a series of statements of only those items where reasonable people might differ, as in whether to put a comma before the "and" when one has a string of three or more objects. This document will include all of the elements of a style manual, but it will also include some statements of policy and practice with respect to grammar, punctuation rules, and spelling. The boundary between these and "style" is in any case rather arbitrary, and in this document no attempt will be made to distinguish these.

It is important to note at the beginning, however, that the rules set down here are in no sense "far out" or provincial; it should be possible for a person to work in the President's Office and then to work in another office with minimum readjustment. [In contrast, a person working in some University offices would be compelled "to change gears" sharply with respect to capitalization before working in another office.]

This document does not attempt to be a complete grammar textbook. Most adults seem to be so "turned off" by grammar in school that they find it impossible even to glance at a grammar textbook. Nevertheless, anyone working in the President's Office would be well advised to scan a grammar textbook at least as frequently as once a year, in order to be reminded of grammatical rules and common grammatical errors. (This remark applies more to authors than to typists, of course.)

Perhaps an analogue will help the reader understand the status of a style manual and of an established style. In Western societies, people shake hands with their right hands. There have been guesses as to how this practice came about, but in modern society it is really completely arbitrary, and even left-handed people do it. One could say "I'm left-handed, and I ought to have the right to shake with my left hand." Such a statement would not be high-principled and independent, it would be merely silly. Thus it is with punctuation rules and those parts of grammatical rules that are not based on logic. On the other hand, if one's right hand is in a plaster cast, he will shake with his left. Similarly, even well-established style may be (rarely) violated, but only for a good and explicit reason applicable to that particular situation.

II. BASIC STANCE AND USAGE

No written material containing spelling errors or grammatical errors will leave the President's Office. This is no mere arbitrary rule, since there is a great deal at stake here. Remember the special situation of a university and the central role of correct usage of language in teaching and learning. Remember also the special situation of the President's Office, or of any other "high-ranking" office in the central administration; any piece of material emanating from our office may at any time be put on the bulletin board of one of our departments or a bulletin board at another university as an example of the "illiteracy of the administration." The risk of damage to our ability to serve the University is simply too great to tolerate any deviations from this basic rule.

Now what about typographical errors? We should do everything we can to see that no documents go out with typographical errors, in part to set a high standard for the University and in part because everyone likes to take pride in good workmanship. On the other hand, occasionally the pressure of work in the office becomes very great, and therefore when necessary material will be allowed to go out with obvious typographical errors. Wherever possible, these should be corrected in heavy black ink, in order that any copies subsequently made by us or by recipients when the document goes beyond our control will also include the correction. Authors will try to be as considerate as possible in allowing time for typists to retype if the typist wishes to do so (because her own reputation is at stake); this consideration may not always be possible, however. "Obvious typographical errors" means such things as omissions (for example, "introdution") and transpositions (for example, "teh"); it does not mean omissions like "ocasionally" or transpositions like "concievably"; those are simply misspellings and no one who receives a document or letter with such misspellings will give the sender the benefit of the doubt.

There are three words that are misspelled almost as often as they are spelled correctly in University of Rochester written material. These are consensus, commitment, and judgment ("judgement" is simply incorrect, not a "fielder's choice"; it is the British spelling and is no more nearly correct than "honour" or "draught," where "honor" and "draft" are the correct American spellings). It is good practice for each typist to have her own list of the correct spelling of words that are commonly misspelled.

Two words are now used so commonly and uncritically that they have lost all meaning. These are "relevant" and "meaningful." They creep stealthily into written text the way "I mean" and "you know" have crept into spoken language. The best practice is never to use them, even in places where you could critically defend their use.

"Like" can be an adverb but never a conjunction: "He talked like her" is correct (although infelicitous), since "like her" is an adverbial phrase modifying "talked." But "He talked like she does" is illiterate, since "like" cannot serve as a conjunction joining two clauses. [If at this point the reader quotes back to the writer a statement from the eleventh edition of a dictionary which seeks to make the tenth edition obsolete by embracing contemporary vulgarisms, it is a good indication that the reader has missed the point of this Style Manual.]

"Oral" is the opposite of "written." "Verbal" means communication by use of words (whether oral or written), in contrast with communication by use of graphs, computer printouts, music, or other means. [Most dictionaries allow the use of "verbal" to mean "oral," but only "by confusion," and we try not to specialize in confusion in this office.]

"Less than" applies to bulk, not number. "There were less than fifty students at the 'Giant Rally'" is incorrect; we say "fewer than" for any items that can be counted. [This error is not so egregious as some others mentioned here, but careful writers (and we hope "careful" describes us!) preserve the distinction between "less than" and "fewer than."]

Almost every use of the word "hopefully" in modern speech or writing is illiterate. Look for the adjective, adverb, or verb that "hopefully" is alleged to modify, and you will almost always discover that the sentence makes no sense at all with this adverb attached to any word in it. It does, of course, make good sense to say that "The candidate listened hopefully to the election returns" ("hopefully" is an adverb modifying "listened"), but it is simply illiterate to say "Hopefully, the new draft will be ready by Thanksgiving." This misuse of "hopefully" in the sense of "we hope" or "I hope" is a terribly insidious development of the last few years, and we will have no part of it.

The position of "only" in a sentence is frequently incorrect. Sentences with different positions of "only" have different meanings: "I only discussed rabbits" means I did not touch them, kill them, eat them, ...," whereas "I discussed only rabbits" means "I did not discuss chickens, peacocks, gerbils, ..." Make sure that "only" is in juxtaposition to the word to which it is intended to apply.

Infinitives ordinarily should not be split; remember that grammatically speaking "to be" is a single word (and, in fact, in almost all languages other than English it is a single word). Occasionally in informal writing, to avoid an apparently stuffy sound, it is permissible to split an infinitive. But it must never be splattered;

it is simply illiterate to write material like the following: "The purpose of this regulation is to 1.) control..., 2.) inform..., 3.) delay..."

One of the most frequent errors in reports, and less frequent but still ubiquitous in letters, is lack of parallelism. Any list must be of objects of the same kind and must be expressed in the same parts of speech. For example, the following is wrong (as well as graceless): "The purpose of this program is to attract better students and for cost reduction." The error is obvious here, but it can be much subtler. All elements of such a list must be parallel in every respect. For example, if each is a verb, each verb should be in the same tense and mood. Attention to parallelism is especially important in long lists employing numbering of elements. Non-parallelism is perhaps the most troublesome error in draft material submitted to our office for editing, since to make it parallel can require a great deal of rewriting (and marking the draft "Make parallel!" and returning it to the original author frequently does not work since he does not know what the instruction means!).

These are only a few comments on spelling, grammar, and usage. Obviously they cannot substitute for consultation and a good text in grammar or a secretary's deskbook. Note, however, that deskbooks and even dictionaries must serve a wide variety of offices, many of which cultivate what we must consider an excessively informal or "popular" style; where conflict occurs, this Style Manual prevails in our office. For additional subleties (or to win arguments), consult Fowler or Margaret Nicholson's American version of Fowler.

STYLE SHEET

1. Indentation.

Our practice is to indent paragraphs. This practice is not arbitrary. Many of our letters and almost all of our documents cover more than a single page. If one does not indent paragraphs, there is frequently ambiguity as to whether the first sentence on a new page starts a new paragraph.

2. Commas.

Most errors in the use of commas come from failure to read the sentence and to look at its structure. The subject and its verb, for example, can be separated by two commas or none, but not by a single comma. Two independent clauses are separated by a comma unless one is so short and the rest of the sentence is so long and complicated that it makes it easier to read if one "draws together" the two independent clauses by omitting the comma. Our practice is to use a comma before the "and" or "or" in a string of three or more objects.

3. Quotation Marks.

The position of quotation marks with respect to commas and periods has been established for reasons of appearance rather than logic. We write: He said he loved "rock 'n roll," whatever that is. Logically, the comma would follow the quotation marks, but it (or a period) would look so lonely out there that usage violates logic for the sake of appearance. All other usage is logical, and the other punctuation appears inside or outside the quotation marks depending on whether it is a part or not a part of the material quoted. For example: How can one love "rock 'n roll"? Another example: He asked "Who's there?"

4. Colons.

The colon is always followed by a double space. If the material after the colon is a complete sentence, the first letter is capitalized. If it is not, the first letter is not capitalized unless specific instructions are given to do so; such instructions may occur sometimes when numbered lists of phrases or even (rarely) single words follow the colon.

5. Capitalization.

Capitalization probably causes more agony than any other element of style. The reason for correct capitalization is to convey more information than could be conveyed if one did not use capitalization; to see the force of this statement, you should read any document where the same word is used both capitalized and not capitalized and see how correct capitalization helps in understanding. The rule on capitalization is that unique nouns are capitalized. The application of this rule is not always self-evident, but if in doubt on capitalization one should always ask: Is this word designating something or somebody that is unique? For example, "people" is not capitalized, but "Frank Smith" is. Frank Smith is a unique individual; the fact that there could be a number of people named Frank Smith does not interfere with our usage of the words Frank Smith to designate a single, unique individual. This example, however, also illustrates the way that the difficulties and the (rare) exceptions come in, which many people use as a way of throwing up their hands and saying they will never understand capitalization. Of course it is possible that in some specialized writing one might have a phrase such as "If all of the Frank Smiths in New York City were assembled in this room it would be a crowded room, indeed." Obviously the capitalization of the Frank Smiths in that (highly unusual) sentence was simply to copy the usage in the much commoner sentences where only a single individual was involved.

There are two clues that are very helpful, even if, used alone, they may not produce a definitive answer. These are: 1.) It is extremely rare that the indefinite article appears before a capitalized word; the chief exceptions are when the capitalized word is derived from a unique word, e.g., "An American," which is obviously a shorthand expression derived from "a resident or citizen of the United States of America." 2.) It is rare for a capitalized word to be used in the plural; again the exceptions are derivatives like "Americans." It is, in fact, this derivative relation in capitalization that seems to cause the most trouble. For example, if we are talking about chemistry departments at various universities in the world, none of these words becomes capitalized. When we speak of the Department of Chemistry at the University of Rochester, they are of course capitalized. A chemist, or chemistry as a profession, no more warrants capitalization than a bus driver or bus driving. Thus, it is perfectly appropriate to have a sentence like the following: "We have sent brochures to chemists at universities throughout the country in order that the University might attract the best possible graduate students to Chemistry," in which "Chemistry" is shorthand for "the Department of Chemistry at the University of Rochester." Note that if the final word had been uncapitalized ("chemistry"), the meaning would have been different; it would have meant "to do our part in attracting students to the profession of chemistry."

All of these remarks conform to practice elsewhere; they are not a special style for our office. For example, the distinction about departments made in the preceding paragraph is quite parallel to the distinction made in saying that "we will drive west" as distinguished from "we will drive to the West." In the former "west" means "in a westerly direction" and is no more to be capitalized than other adverbs like "slowly." In the latter, "West" is shorthand for "the western part of the United States of America," and its capitalization follows from its uniqueness.

One of the most difficult capitalization problems has to do with the ranks of professors; when we are writing formal language in which the distinction between ranks is important, we capitalize "Assistant Professor," "Associate Professor," or "Professor." This usage is to some extent arbitrary, but it is in part reasonable because what is unique is the rank and privileges pertaining thereto. Usually, however, we are not referring specifically to ranks, and then we say "the professors at the University constitute our greatest asset," or "a professor is a man who thinks otherwise."

There is another problem that frequently arises in our writing, and that has to do with the word "administration." This is a much overworked and vaguely used word; for example, the students are always writing in the Campus Times about the "administration," when that word means different things from sentence to sentence and from day to day. The word "administration" should never be capitalized if it is being distinguished from uncapitalized groups; for example, one would not say "the faculty thinks so-and-so but the Administration thinks otherwise." In fact, the situations in which "administration" should be capitalized, even when it means "the upper-level central administration at the University of Rochester," are probably very rare.

Another way problems can arise in capitalization is if the same word is used in slightly varying connotations. For example, one might talk about the "National Parks Service" and later in the same paper talk about "Glacier Park" and "Yosemite Park." Referring to these two together, one might well call them "these Parks" (rather than "these parks") in order to avoid the unsightly juxtaposition of Glacier Park, Yosemite Park, and parks (in which the upper- and lower-case p's clash.) But he might also use "Parks" as shorthand for "the National Parks Service." If this kind of trouble is encountered, it usually means that the writing is sloppy and ought to be edited.

One final remark on capitalization is that inexperienced people usually over-capitalize. If one never has encountered the word "paleontologist" before, there seems to be a tendency to treat it as a proper noun. [Of course in some circumstances it could be, as for example in "Chief Paleontologist of the U.S. Bureau of Mines," but there it is the title of a very specific and unique individual, not the name given to a member of the profession of paleontology.]

6. Hyphenation.

Hyphenation will frequently have to be dictated by the author. There are occasions when hyphens will be used to draw together strings of adjectives or noun modifiers in order to make the meaning clear. To be sure, good writers avoid using long sequences of these, especially noun modifiers, but sometimes they appear in technical language, however infelicitously. You should be warned that it is quite possible to have the same sequence of words both with and without a hyphen in the same letter or paper. For example, most publishers' styles will use "physics of the solid state" but "solid-state physics." This usage of a hyphen in the second instance is, of course, to avoid an unintended and incorrect linkage in the reader's mind; if the reader associates the words as if they were written "solid state-physics," he gets the wrong idea.

7. Apostrophes.

Apostrophes are frequently misused, even though the rules are extremely simple, and there is only a single exception. The exception is: When one writes in casual English or conversational English "it is" in shortened form, it comes out "it's." Possibly because of this preemptive usage, but quite possibly for more fundamental reasons (as in the relation of "his" to "he"), the possessive form of "it" is "its." There is no choice permitted here, and frequently the usage is incorrect.

Another frequent error has to do with the possessive case of proper names. One can say "I am going to dinner at the Smiths'" since that is short for "I am going to dinner at the Smiths' home." But to say "I am going out to dinner with the Smiths'" is illiterate since "Smiths" here is <u>not</u> the possessive.

It is our style to <u>avoid</u> the extra "s" after words ending in s'; that is, we say "Mr. Loomis' daughter," <u>not</u> "Mr. Loomis's daughter."

8. <u>Numerals</u>.

We do not start a sentence with an Arabic numeral; either it is spelled out or the sentence is recast. Numbers smaller than one hundred are ordinarily written out. The exception to this rule is if one is "doing arithmetic" with them. "Arithmetic" means a sentence something like the following: "Since of the 15 members of the committee, 5 are from the Medical School and 3 are from the Eastman School of Music, it would be difficult to have more than 4 or 5 from the College of Arts and Science." The idea here is that if the reader is being asked implicitly to do subtractions, it is easier to do these when the numerals are given in Arabic numeral form.

Rounded numbers (like "about three hundred") are ordinarily written out.

9. <u>Diacritical Marks</u>.

Occasionally French, German, or Spanish words will be used. The diacritical marks associated with some letters in these languages are not just optional "topping on the cake"; each is an essential part of the letter to which it is attached. It is as incorrect to write "e" in place of "é" (acute accent) as it is to write "a" in place of "e."

These marks would always be dictated as in "Pétain, that is, capital P, e acute accent, t, a, i, n." Other commonly used diacritical marks are the "grave" (pronounced 'grahv') accent ` , the circumflex accent ^, the cedilla ¸ , the tilde ~, and the umlaut ¨. [This is called "umlaut" only in slang. It is the diaeresis found also in some English words; strictly speaking, the meaning of "umlaut" is the process of changing the vowel sound rather than the two little dots, but in practice it is handy to refer to the dots as an umlaut. Although the only correct procedure is to put in the dots by pen and ink, sometimes it is better to use the " mark on the typewriter if there is risk of forgetting to do it right.]

10. Carbons and Peripheral Marks.

Unless you are instructed otherwise, make four carbons of all typing: one yellow carbon (file copy), one pink carbon (chronological file copy), one white carbon (for the President's Office circulation folder).

When you are instructed to make a "blind copy," note "bc: Dean XYZ" on the four master carbons as well as on the blind copy itself, but not on the original document and not on copies ("blind" or "open") you might be instructed to make for other persons. So far as the person receiving a blind copy knows, he is the only recipient of a blind copy, but of course his copy (like all the others) shows the recipients of open copies. However, all four office copies should, unless you are otherwise instructed, show all recipients of blind copies.

Fill in formal names for copies, even if the manuscript says "KEC" or "Bill Jones".

The initials of the person who produced the material to be typed should be in the lower left-hand corner of the last page and in upper case.

A colon and the typist's initials follow these. For example, if Robert L. Sproull dictated and Annelise Falzer transcribed, she would type "RLS:af."

If the date of typing is different from the date on the heading of the material or if the heading has no date, put a date under the initials, as "RLS:af
14 Nov. 1973"

If only the typist's initials appear, the material was produced by more than one person, or the typist also wrote the material and signed either his or her own name or signed "In Mr. So-and-so's absence."

When two or more people create a piece of material to be typed, try to keep a record of who participated and if possible a record of who did what.

11. Dictation Tapes.

Do not erase any dictation belt until your transcribed material has been returned to you by the dictator. It is our practice to give the author (unless he specifically instructs otherwise) the transcribed material together with any enclosures, envelopes, or other supporting material in a correspondence folder and to place the dictation binder with the matching belt (or belts) in the same folder.

12. Speeches.

Speeches are typed on the large-type typewriter available in the President's Office. The instructions may vary as to whether the speeches are to be typed on cards or on full-sized sheets of paper. In any case, a word should never be divided by hyphenation and a sentence should never be divided, part on one card or sheet and part on another. Thus the typist should look ahead and, if necessary, leave considerable blank space.

13. Drafts.

There is no such thing as a single-spaced draft. Drafts should either be double- or triple-spaced and should have wide margins. All drafts should be dated. Often a "first draft" will be so labeled, and all subsequent drafts should be labeled "second," etc. In moving from a draft to a later draft or to the final manuscript, the most essential aspect of proofreading is to avoid dropping a whole line or a whole phrase of material. This is especially easy to do if the same word or especially the same combination of words appears in quick succession.

14. Editing Drafts.

It is convenient to have a communication language for correcting and editing drafts. Proofreaders' language can be used, but it is rather complicated, and if one does not use it much it may involve a great deal of looking up of symbols. Furthermore, proofreading practice puts the basic instructions in the margins; this is awkward and requires practice and concentration, but it is the only way for printers since printed text has no space between the lines. The following abbreviated set of symbols and instructions is very convenient, and we use it to the virtual exclusion of others:

Instruction	Example: Edited	Example: Final
∿ , transpose	What have I to say	What I have to say
To remove a letter, it is blacked out; to remove several letters or words, a horizontal line is drawn. Space should not be left where the removal occurs unless instructed.	Theřm Now is the ~~is the~~ time	Them Now is the time
∧ , caret. Material should be inserted here. Usually the material will be tied to the caret, but sometimes a "balloon" will be tied to the caret, referring to the reverse side of the page ("over") or to a separate page.	com∧unicate (m) the time. ∧ When he (Insert over)	communicate (Material from back of page inserted)
______ , underline. This means to draw a typed line under the word or words (if it is to be printed, this word will come out in Italics).	Now is the time	<u>Now</u> is the time
, remove underline	Now is the time	Now is the time
≡ , capitalize	now is the time Now is the time	Now is the time NOW is the time
/ , make lower case	Now is the time NOW is the time	now is the time Now is the time
⁀‿ , close up (horizontally)	N ow is the time	Now is the time
( ) , close up (vertically)	Now is the time for all good men	Now is the time for all good men
# , insert space (horiz.)	Nowis the time	Now is the time
—# , insert space (vert.)	Now is the time # for all good men	Now is the time for all good men
, delete. Remove any material to which this symbol is attached.	Furthermore, he meant it. Now is the time for all good men	Now is the time for all good men

Instruction	Example: Edited	Example: Final
] , move right, indent	]Now is the time for all good men	Now is the time for all good men
..... , stet, leave as it was	Now is ~~the~~ time	Now is the time
"run on"	[A form of "closing up" which may bring material together that had been widely separated or pasted up irregularly; see example below.]	

The use of these marks will be illustrated in the following text, first in its "marked up" form and then as it would be typed:

]The purpose of this sam~~pel~~ of text is to ilustrate the sub set of Proofreaders' marks that we use in the President's office. obviously any team of author and typist whos ~~want~~ wish to use the full set, found in good dictionaries, may do so.

Unlikethe actual marking for the print er used on printer's proof, ~~it is much clearer~~ we find it and more convenient to place these marks in the text at the "point of infraction" rather than in the margin.

The purpose of this sample of text is to illustrate the sub-set of proofreaders' marks that we use in the President's Office. Obviously any team of author and typist who wish to use the full set, found in good dictionaries, may do so. Unlike the actual marking for the printer's use on printer's proof, we find it clearer and more convenient to place these marks in the text at the "point of infraction" rather than in the margin.

RLS:jeb
18 June 1974
24 Nov. 1981

Skiing, Gold, and AC-DC

Robert L. Sproull
1999

The three words in my title are about as disparate as words in the same language can be. There will be, therefore, a certain miscellaneous character to this talk. My talk will be even broader than my title, since along the way we shall encounter a great entrepreneur, a question of why the universe is as it is, and even a *ménage à trois.* As is usual in my talks, I shall try to draw some lessons from my account, such as that executives should listen to their troops and that the serious entrepreneur may have to risk everything. These are, of course, not new, but you may be interested in seeing them in this unusual setting.

What unites the three words is the little town of **Telluride** in southwestern Colorado. The town was originally called "Columbia," but as it was a mining town it was confused with the more famous Columbia, California, in the Mother Lode region of the Sierra Nevada, which had become a major mining center in the gold rush days after 1848. A mining engineer proposed the name "Telluride" as a name not likely to be used anywhere else. He was correct: I know of no other "Telluride" anywhere.

Before dealing with that name, however, I will dispose of "skiing," of which I know little. Very few people had heard about Telluride until about 30 years ago, when Aspen, Vail, and

Breckenridge became crowded and pricey, and skiing promoters developed the mountains surrounding the Telluride Basin as alternate ski resorts. I understand that Telluride is now a prosperous rival to the others. That is all that I will say about skiing.

Now to "gold": In the Americas almost all gold is found in the elemental form, shiny yellow flakes (usually) or nuggets (rarely). The typical history of a mining region is that first someone agitated a shallow pan of water, sand, and gravel scooped up from a river and noted small specks or even nuggets of gold in the bottom—gold with a specific gravity of 19 is easily separated from the other materials, mostly silicate rocks and sand with a specific gravity near 3. He then worked upstream, at any fork taking the stream that "panned out" most promisingly. Any diminution of yield meant that he had passed the region adjacent to the stream that was supplying the gold. He then looked in the nearby hill for veins of quartz rock that had specks of gold in them, staked out his claim, and began mining. In dry regions where this pattern could not be followed, often gold would be visible in quartz rock veins that came to the surface.

Gold is sometimes found in gold compounds. Two minerals, sylvanite and calaverite, are compounds of gold, silver, and tellurium. Tellurium is an element similar to oxygen, sulfur, and selenium, but not so plentiful. Like "oxides" or "sulfides," compounds of tellurium are called "tellurides." Along with elemental gold found in the San Miguel River, some sylvanite and calaverite were found in the basin where Columbia had been, and thus when a substitute for "Columbia" was required, the name "Telluride" seemed appropriate and was chosen.

But the economically recoverable gold in the Telluride Basin was flecks of gold in quartz rock, often found at high elevations. The rock was mined and then inserted into a stamp mill, where it was pounded into a fine dust. The dust was then mixed with mercury to amalgamate with the gold; nothing else in the powder amalgamated. The amalgam was easily separated from the dust, and the mercury was boiled off in a retort (and of course recycled). Liquid gold was poured from the retort into ingots. To minimize the expense of transporting tons of ore, often with very little gold content, the stamp mill was located as close a possible to the mine mouth. But the stamp mill required lots of power and so was frequently located at a source of waterpower, even if considerable transport of ore was required. The alternative was steam power, which was not prohibitively expensive if the mine was below timberline but which became very expensive if coal or wood had to be brought in.

At this point, our story is in transit between "Gold" and "AC-DC.." The story now starts in the mining town of Leadville, Colorado, in 1880. Those of you who heard my talk several years ago about the little work-study school Deep Springs will have encountered **L.L. Nunn** when he was in his sixties and when he was active as an educator and philanthropist. You may even remember from that talk, which I titled "A Unique Institution," that Nunn founded Deep Springs after two decades of experience in training young men in the electric power industry.

Tonight we encounter him as an energetic little man (five feet one inch) who had been born and raised in Oberlin, Ohio, traveled and studied in Europe, read law at Harvard, and now had arrived in Leadville in 1880 at the age of 27. I say "energetic," but the adjective fails to capture the flavor of many anecdotes telling of his prodigious energy and tenacity. He built and operated a restaurant in Leadville, then in Durango, and then earned enough money as a carpenter in the goldrush town of Telluride to open a law firm. As you know, entrance to the practice of law was a pretty casual affair in those days, and it was probably especially informal in a rough and bustling mining town. Most of his clients were miners and mining companies, and by 1890 he had become the owner of several mines and the manager of the largest,, the Gold King mine.

By 1890 many mines in the Telluride region had already shut down as veins petered out. The Gold King was still running ore that assayed $40 per ton, but the vein was running into poorer ore and the costs were rising. The principal shafts were above timberline on the ridge between Telluride and Ophir. In order to provide hundreds of horsepower for the stamp mill, coal had to be packed in by mule at a cost of $40 per ton; the climb from the railhead at 9261 feet elevation was to mine mouths above 13,000 feet. As the ore that could be reached deteriorated, the mine faced certain failure.

Nunn had built a small hydroelectric plant below Ophir on the San Miguel River; there was adequate water flow for great expansion, but it was three miles over rugged mountains from the River to the stamp mills. Carrying the power by belts, circulating oil, and compressed air had been investigated, but all seemed impractical. He wrote to his younger brother, **P. N. Nunn**, a school principal in Massachusetts, on 21 May 1890: ". . . I wish you would investigate the subject of transmission of power by electricity. I have surveyors now at work laying out a line over the mountains for a cable for the transmission of about 175 horsepower from a water power [*sic*] to three mills distant from two and a half to six miles. I am not sure of putting in the plant, but if I do I want you to take charge of the construction, and not let any one know that you are not an old hand at the work. Post yourself thoroughly and know whom to send for as an assistant if necessary. The mills have cost over $100,000, and of course the power to run them must be sure. It now costs upwards of $2,500 per month for power, and I believe it can be furnished when the plant is put in for $500."

At this point I must turn aside and remind you of the history of electric power. The French physicist André Marie Ampère had discovered in 1820 the mechanical force on a wire carrying an electric current when it was in a magnetic field. In 1831 Joseph Henry in the U.S. and Michael Faraday in England had discovered electromagnetic induction, the creation of a current in a wire when the wire was moved through a magnetic field (or in a wire when the magnetic field was moved through it). Experimentation followed rapidly, and in 1875 two physics professors at Cornell, William A. Anthony and George S. Moler, built the first dynamo, a machine for converting mechanical force to electric current. As a demonstration they powered arc lights on the Cornell Campus through an underground cable. [Parenthetically, I might note that when I arrived at Cornell in 1938, the north end of Rockefeller Hall was still labeled the "Dynamo Laboratory"; Rockefeller had been constructed in 1908 but the original equipment and its progeny had been moved to it from Franklin Hall.]

By the 1880's a number of small installations of electricity, mostly street lighting, were created. The Edison Electric Company had developed the Cornell Dynamo and produced its brother, the electric motor. Thomas A. Edison was the dominant figure of anything electrical, and his company sold systems for lighting and even a small street railway. All of these used **direct current,** D.C. Of the two wires going from dynamo to lamp. one always remained positive and the other negative, the system that is still used wherever batteries are involved, as in an automobile. **Alternating current,** A.C., had been proposed, and Frederick Bedell, another Cornell Professor, and Nikola Tesla, a Croatian-born physicist who had immigrated in 1884, had worked out a solid theoretical framework for the design of systems. Here the two wires exchange polarity many times per second (this is the universal practice in residences and industry now, and in the U. S. the frequency of exchange is 60 times per second).

Edison and his associates conducted a powerful smear campaign in the press against alternating current. They were heavily committed to D.C. and wanted no competitor. They exploited public ignorance by intentionally confusing the effects of A.C. (which are wholly benign) and high voltage (which can be very dangerous). Both A.C. and D.C. must of course be handled carefully and with adequate safeguards when at high voltages. But at the same voltage, there is little difference in danger of A.C. and D.C., and D.C. is actually somewhat more dangerous. There were, of course, occasional accidents, and in the newspapers the D.C. accidents were suppressed and the A.C. accidents were made the subjects of panic stories. The stories usually quoted Edison, the high priest in the U. S. of anything electrical. (In England, the high priest was William.Thomson, later to be named Lord Kelvin, who was quoted making exaggerated warnings against the "dangers" of A.C..) The situation was closely parallel to the scares you are familiar with in the 1980's about the effects on people of microwaves and of the magnetic fields from power lines.

But the Westinghouse Union Switch and Signal Company had nevertheless invested in some young engineers who had developed low voltage A.C. equipment. In 1886 the Chief Engineer, William Stanley, had persuaded George Westinghouse to buy the patent of a **transformer** from Lucien Gaulard (French) and John Dixon Gibbs (English) for $50,000. Now the transformer, which gets very little fame and credit, is the marvelous device which permits transmission of electricity at high voltage—and therefore low current and low loss— over long distances, but then converting to low voltages (e.g., 120 volts) for safe use around the house. You see a transformer, a steel-encased pot the size of a large wastebasket or small garbage can, on the pole at the rear of your house and take it for granted, but it actually is one of most important inventions of all time.

So, here was the situation in 1890: 1.) Direct current generators had been built and had powered experimental street railways. 2.) Low voltage (100 or 200 volts) generators powered lighting systems in commercial buildings, using the incandescent lamp that had recently been invented by Thomas Edison, but transmission over more than a few hundred yards was prohibitively inefficient at these voltages. 3.) The Edison group dominated the newspapers and used the reverence in which Edison was held to convince the public that **A.C.,** not just high voltage, was dangerous. 4.) Westinghouse engineers had developed an A.C. lighting system of 1000 volt lines over appreciable distances, then transformers to "step down" the voltage to 100 or 200 volts with distribution within buildings for lighting;. Despite the smear campaign by the Edison group, this system was becoming so popular that the Westinghouse Company could not make transformers fast enough to satisfy the demand. 5.) Nicola Tesla, who had immigrated from Croatia to work for the Westinghouse Company, had invented the induction motor, which was to become the work horse of electric power. 6.) George Westinghouse was still pushing compressed air as the means for transmitting power and recommended it for the Colorado mines.

When P. N. reported that Edison had told him that A. C. electrical power was impossible, Westinghouse said "If Edison says it won't work, that's good enough for me." Like many chief executive officers in the 1980's and 1990's, he listened more to other chief executive officers and newspapers than to his own engineers for wisdom in the A.C.—D.C, conflict. (I could give you several examples of executives in the 1980's and 90's who made the same mistake, with grave consequences.)

This was the scene as P. N. Nunn traveled around the East in the summer of 1890. He reported to his brother: 1.) The Edison group was hopeless; they were wedded to D.C. which was a dead end. 2.) For power transmission, A.C. was the way to go; there was no difference in danger between A.C. and D. C. at the same voltage, and the transformer permitted efficient transmission at high voltages but harvesting at safe low voltages. 3.) A.C. was limited to a few thousand volts by available insulation materials, but that technology was rapidly improving. 4.)The Westinghouse engineers could develop a 100 horsepower generation and transmission system and a 100 horsepower induction motor, but George Westinghouse was so skeptical and preoccupied with other business that P. N. could not persuade the Company to contract for it. Westinghouse's position was quite reasonable, since the Nunn brothers had no experience in electrical engineering, they were over 2000 miles away, and their business (of all irresponsible things!) was gold mining.

But L. L. Nunn was a feisty little entrepreneur who was facing failure if he was limited to current technology. He arranged a second hearing by Westinghouse and his engineers, put $100,000 in gold coin (the entire output of the Gold King mine for a year) on the table, and said "I am ready to wager $100,000 in gold on the success of our proposed venture into alternating-current power development if you will wager an equal amount in the time and experience of your staff in manufacturing the needed equipment." Under the urging of his staff, Westinghouse reluctantly agreed.

I will not dwell on the difficulties of constructing the system under conditions both dangerous and primitive. The rugged high country, the weather, the remoteness from Westinghouse engineers and their manufacturing plants in Great Barrington,Massachusetts, and Pittsburgh, and the lack of trained manpower were only some of the problems. But construction progressed on schedule, and the system **worked**! At first the Eastern establishment was incredulous of reports that 100 horsepower was being transmitted over three miles with less than 5% loss. But by 1892 the system had established a reputation for reliability and low cost that captured the attention of the East and then of Europe. Even William Thomson (Lord Kelvin) was convinced. In the next few years the equipment was repeatedly upgraded to higher voltage and higher power, as the Westinghouse Company learned how to make ever higher voltage transformers, and the system was extended to other mines.

In 1895 the Nunns began to build power installations near Telluride, than elsewhere in Colorado, and by 1898 in Idaho, Utah, and Montana as well, always under the name of the "Telluride Power Company." L.L. quickly learned that graduates of Eastern engineering schools were not rugged enough to build power lines in Montana in the winter, and he turned to on-the-job training of local youths. I have already reported in the earlier talk to this group how L. L. Nunn's interest in developing the engineering competence of his employees broadened and deepened into the Telluride Association and Deep Springs. You can now see why L. L. sent his employee-students to Cornell for advanced education and established the Telluride Association and Telluride House there.

Westinghouse developed ever larger and higher voltage equipment. Nunn and the young engineers had judged correctly the open-ended promise of A, C. P. N. Nunn became the Chief Engineer of the Niagara Falls power plant which began operation in 1895; you can see his name on the bronze plaque in the large generating station under the Canadian viewing park. The electrical revolution was on its way.

As you know, in the 1890's electric power developed explosively. Electric motors began to dominate manufacturing, replacing the cumbersome and dangerous huge shafts driven by water power or large steam engines and distributing power by belts to machines or processes. Electricity, always A.C., in the 1890's was reaching into every activity, like the Internet of the **1990**'s. The Nunns' pioneering surely advanced this movement by many years. P. N.'s self-taught engineering competence was important, but of even greater importance was L. L.'s identifying the technology that was open-ended and especially his acceptance of great risk.

A few minutes ago I celebrated the transformer as a vital part of the electrical technology that is such an important part of modern civilization. Now I want to raise with you a question. The transformer, alternator, generator, and motor all depend on the use of iron or iron alloys. These metals or alloys are **ferromagnetic,** that is, they can concentrate the magnetic fields that do work or produce currents; without materials that are ferromagnetic, electrical machinery would be prohibitively large and inefficient. The property of ferromagnetism, present only in iron and a very few other elements, depends on a precise combination of three atomic constants, the charge e of the electron, the mass m of the electron, and Planck's constant h. If any of these three were somewhat different, there would be no ferromagnetism and no electrical power. The question is: Is this accidental? One would think that the universe could very well have been created otherwise.

Finally, the story of the Nunns and the birth of the electric power industry must seem like ancient history to most of you, but it is more vivid and recent for me. This is because I knew P.N. Nunn well, He was Chairman of the Board of Trustees and intimately involved in the management of Deep Springs when I went there as a student in 1935; his brother L. L. had died in 1925. In the fall of ~~1938~~, 1937 when I was 19, I served as what would now be called the "system" integrator" for a new central heating system for the entire school. The plans and specifications had to have P.N.'s approval, and in January, 1938, to obtain that approval I spent four days as a guest in the Nunn *ménage à trois* in San Diego. We went over every detail in agonizing concentration. At breakfast each morning, Mrs. Nunn, P. N., and his adopted "daughter" Mrs. Julia Hamilton would each have a stack of pills and a newspaper. Conversation began from different starting points in the newspapers, but the final comment was always the same: "Oh, well, what can you expect with a crazy man in the White House?"

@5 I Sat There, Trembling

First I must tell my route to that seat, trembling. I had grown up in the Middle West. My first two years of high school were at one of the best schools in the Nation, but my last two were in a small town with teachers who tried hard but were unable to provide much intellectual content. After graduation I spent three years at a tiny work-study college in the Inyo Mountains of California (how and why are another story). I transferred to Cornell in September of 1938.

At Cornell I launched a physics major, and its requirements plus the college's "distribution" requirements called for five courses instead of the usual four. In addition, I had read much of the work of the great Carl Becker, and I was determined to take his course in modern European history even though that made six in the spring semester.

It was then the practice in that course (as in most at Cornell) to set an hour-long examination about half way through the semester. The crowd that arrived for the exam was about twice or three times the size of the class of students who attended the lectures; I was astounded at the size of the group who had chosen to miss one of the greatest lecturers they could experience.

The examination room was the main lecture theater in Boardman Hall, a building that was originally the Law School and that was later (in1960) torn down. A distinguishing feature was the presence of massive columns, required to support the upper floors but restricting viewing from seat to seat.

The proctor, evidently a graduate student, gave each of us a "blue book," the blank book for our answers, and a sheet of questions. I quickly read the two questions, each for half of the score. The first question was: "In what way were the circumstances at the founding of the Third French Republic more auspicious than at the founding of the First?"

I sat there, trembling. With an open-ended question like this, how was I going to compete with these students from Eastern high schools and prep schools, with two or three years of Cornell experience, and with confidence so great that they had not felt it necessary to attend the lectures? My own credentials looked good on paper, but they were from a miserable high school and a tiny, strange, and little known junior college. I was so frightened that I could not put pen to paper. Then a student must have raised his hand (he was behind a pillar and I could not see him) and asked "Sir, what does 'auspicious' mean?"

I was off and running, with instant return of confidence. I remain indebted to that student, whom I never saw, who quite possibly saved my fledgling academic career.

Robert L. Sproull
January 2004

March 9, 1996
Robert L Sproull

"DS"

A Unique Institution

Do you share my obstinate objection to "very unique," "more unique," and other barbaric misuses of the word "unique"? If you do, you probably already have your forces in place, armed to the teeth to contest that word in my title. May we have a twenty-minute cease-fire? I ask you to listen to my brief account of the institution, and then you can decide for yourself whether the word is appropriate or hype.

Deep Springs College is a school for 22 to 24 young men; *not* 22 to 24 per year, but 22 to 24 *total*. It is situated on a cattle ranch in Deep Springs Valley at an elevation of 5000 feet, 240 miles east of San Francisco, 230 miles north of Los Angeles, and ten miles from the Nevada border. Sage brush and rabbit bush cover the valley floor since the climate is "high desert." It is about 40 miles east of the Sierra Nevada crest, which squeezes out most of the moisture from the Westerly air masses. The Sierras are barely visible from the ranch, since they are obscured by the intervening Inyo and White Mountains, the latter cresting just 250 feet shy of the highest mountain in the 48 contiguous states. The pastel colors of vegetation and mountains like these are not splashy enough to stimulate Continental painters but have been the subject of remarkable paintings beginning with Charles Russell and Frederick Remington and continuing to Walker Stone and Christian Midjo in contemporary times.

Deep Springs does not charge for tuition or room and board. This circumstance plus the small size enables it to take its choice of applicants. It usually has a freshman class with the highest SAT scores in the Nation, although from time to time CalTech edges it out on mathematics SAT's.

The faculty typically numbers four or five, and visiting faculty for short periods are common. Occasionally a professor from a major university will spend a sabbatic semester at Deep Springs. A student spends two years at the College taking more specialized--less broad, survey--courses than he would at a university or liberal arts college. When he transfers to a university after two years his course mixture is a little chaotic. But the intensity of the small classes (four is a "large class") more than compensates for the lack of tidiness in his course record. This intensity extends beyond the classroom to the informal contact with faculty and visitors in a variety of settings, especially meals; a student has no place to hide and can be questioned at any time by a faculty member. This intensity plus the selectivity of the admission process combine to achieve a track record of graduates such that major universities admit Deep Springers and give full credit for Deep Springs courses. Almost all graduates transfer and get bachelor degrees; three fourths get advanced degrees.

But the academic program is only one part, and probably not the most important part, of the Deep Springs experience. An even more consequential part is the work program. Students work half-time on all the jobs of a school and ranch: laundry, kitchen, dairy, vegetable plots, irrigation, constructing new facilities, and working cattle. An elected student is the Labor Commissioner, and he assigns students to jobs after consulting with the professional ranch manager.

And there is a third part, student body self-government. The Deed of Trust, which is in effect the charter of the college, gives specific rights to the Deep Springs Student Body, especially the control of the conduct of its members. The DSSB adjudges any alleged violation of rules, such as those against smoking and drugs. It plays a major role in the admissions process; it elects one of its members to be a member of the Board of Trustees; it has considerable influence on all aspects of management and planning. The DSSB is quite different from the student associations at conventional colleges and universities, which are typically playgrounds for academic politicians who wish to make mischief and sandboxes for students who wish to get into law schools.

It has been said that at Deep Springs "you study half-time, you work the ranch half-time, and you manage student self government half-time." This impossible arithmetic becomes acutely personal if you have a term paper due, you are one of the dairy boys and have a cow with an inflamed udder, and you must report for the Admissions Committee at the Student Body meeting this evening.

How was this institution created? It was founded by one man, L. L. Nunn, in 1918. He and his brother P. N. Nunn were pioneers in the industrial use of electricity and the first users, in 1891, of alternating current. Their story may be the subject for another talk; it is intimately convolved with the story of the competition between A.C. and D. C., the fight between Westinghouse and G.E. , and the conflict between Edison and science. Here, I have time only to explain that the Nunn brothers became power company originators in Telluride, Colorado, and rapidly expanded their interests throughout much of the Northwest.

During the next 15 years L. L. Nunn built waterpowered electrical generating stations in Colorado, Utah, Idaho, and Montana and built distribution networks with each. At first he attempted to recruit young men with engineering training from Eastern universities. But few of them could survive the rugged conditions of building power lines in mountainous country with primitive living conditions. He began building collegiate level schools associated with each generating station and recruiting local young men whom he and his more experienced associates trained in electrical engineering. The student employees would work at increasingly responsible jobs during the daytime and go to school at night. He delegated the management of the dormitories and most of the aspects of student life to local self-governing associations.

By 1905 Nunn had become more interested in education than in power. He built a large brick dormitory and classroom building at his Olmsted plant in Provo Canyon, Utah. He sponsored the ablest employees to continue their education at Cornell, one of the first engineering colleges and the pioneering electrical engineering department. He built on the Cornell campus a large brick replica of the Olmsted building ; many of you will have known it as Telluride House, just down the hill from Willard Straight Hall and the Law School. In 1908 he institutionalized the self-government of the residents of Telluride House by founding the Telluride Association and providing it with an endowment of about $3 million. The members of the Association, few of whom are over 30 years old, are at once the trustees and the beneficiaries of the endowment which they manage.

By 1917 Nunn concluded that he had made a mistake in giving so much authority to young people; He had urged the Association to employ an especially inept crony of his, but the young men had refused. He determined to try again to institutionalize his educational ideas, but this time with a conventional Board of Trustees possessing the ultimate power. After a false start in the East, he bought a cattle ranch in Deep Springs Valley and opened the school in 1918.

The Deed of Trust places the ultimate responsibility squarely upon a Board of Trustees. Nunn chose his power-company managers for the initial Board, including the inept crony I have already mentioned, and their terms were for life. But in the formative years, until his death in 1925, he was prominently in the background, visiting the ranch, talking with students, and writing letters which became the canon of the institution.

Unfortunately, after L. L. Nunn died, this Board did not have the educational zeal and sense of purpose of the founder. Furthermore, they did not make a clear distinction between their private businesses and funds and the business and funds of the Trust. Although they were never held accountable for actual fraud, they managed to run the endowment into the ground. Like most of us, they became even more "conservative" as they grew older. In the 1950's they appointed a local Director who was described by a hard-right Professor of Agricultural Economics at Cornell as "having fallen off the right side of the road." This appointment plus the dwindling of the endowment nearly killed Deep Springs. It was saved by the heroic service of two former students: One is a noted neurosurgeon at the University of California and the other was a prominent New York City lawyer; both had been students at Deep Springs, and the neurosurgeon had known the founder. Under their leadership, a number of alumni regained their interest . A new Board, with eight-year terms, composed mostly of alumni, has piloted the institution since then. Annual contributions by alumni have substituted for the missing endowment, and gifts have even re-established, to some extent, that endowment. But Deep Springs finances are a struggle, and I will return to them in a few minutes.

The founder was not bashful about declaring a purpose for his creation, and this purpose has survived essentially intact. Briefly stated, the purpose is to educate young men to commit careers to service to society. Almost any occupation is suitable, and there are no religious connotations or limitations. But in return for two years of an intense and expensive experience at no cost to the student, he is expected to repay society over his subsequent lifetime by the choices he makes. These could be large choices of career; many alumni have chosen careers in the not-for-profit sphere (including the executive head of the Red Cross and a Catholic priest). But they could also be the many small choices that everyone, especially a professional, makes as his career develops. Substantial volunteer service is nearly universal among Deep Springs graduates.

There are two key elements in the way Deep Springs advances this purpose. The first is to provide an isolated community, 28 miles over a mountain pass to the nearest town, where adults and students interact in every setting--seminars, work, pack trips, eating, and leisure (what there is of it). Even the shyest student is involved in the deepest discussions, and shyness is not a hallmark of Deep Springers. There are essentially no distractions, and you have probably already noted the similarity--in technique, not in purpose--to a classical monastery.

The other key element in the Deep Springs technique is forcing the acceptance of responsibility at a much younger age than is customary nowadays, but was universal on the American farm in the last century. Complicated and even dangerous tasks are entrusted to 18- or 19-year-olds. The sanctions against a student who fails to do the ranch job he was assigned or the bookkeeping he was responsible for as office boy are enormous, though largely invisible. For example, I do not think that an animal has ever been mistreated, yet I do not believe there is a written word on the subject.

One of the last exercises of the President's Science Advisory Committee before it was abolished two decades ago was the "Second Coleman Report" on late K-12 education; I believe the title was "Youth: Transition to Adulthood." It backed away from the busing that was the most famous feature of the First Coleman report some years earlier. Perhaps because of this or because it was essentially posthumous, it is rarely quoted now. But it made a number of profound findings and recommendations. A brief summary (my words, not Professor James Coleman's) is: Schools are a mistake. The report especially deplored separation of students according to age and suppression of the adult-youth interaction. It also strongly deplored the lack of acceptance of responsibility, including the responsibility of behaving properly toward others. In our schools, not only is one youth's success not dependent on any of his fellows' but he actually makes out better the worse they perform.

Although Deep Springs was founded a half century earlier and is for the first two years of college rather than for high school, its approach is almost exactly that recommended, albeit wistfully, in the Coleman Report. The mixing of ages--from the ranch manager and his wife and the Director and his wife to their very young children--tempers and flavors education in a healthy way. Even more in keeping with the Coleman Report is the work program: If a student leaves the hay bailer at the end of the day without explaining its deficiencies to his replacement the next day, it is not only irresponsible but can be downright dangerous.

Deep Springs is now operating effectively, despite its financial troubles. You might well ask why the ranch income, based to a considerable extent on the practical work by students, does not support the school. Four or five generations of accountants and economists have been unable to determine even if the ranch is a net producer of income. The farm, ranch, and school are too tightly convolved to permit sensible cost accounting. I think the best way to get perspective on this question is to consider a 200-acre ranch in the same area, 28 miles from the nearest town (and that only a couple of filling stations and three motels). Typically, such a ranch would be owned and operated by a family, with a hired man as irrigator or cowboy. There are many such operations in the region, and the families are usually not poor, but even with very hard and confining work they are not rich. The ranch operation at Deep Springs is therefore likely to be at best a break-even enterprise, with no income left over for faculty, administration, library and other collegiate expenses.

The future, even if Deep Springs becomes financially secure, will always be in doubt. The school is just too *different* to be secure. One cannot expect the director (now called, with the inflation of academic titles, the "President") to stay more than a few years. The faculty have almost all been products of a conventional educational system. The forces of political correctness add to the natural action of increasing entropy to push Deep Springs into more conventional directions. The isolation has substantially decreased because of better roads and vehicles. The balance between academics and practical work has slowly shifted toward more academics. The faculty, who were not even mentioned in the founder's documents, have taken a stronger role, gaining power at the expense of the Student Body's self-government.

In recent years, the focus of much of the thrust toward making Deep Springs into just another junior college has been toward making it co-educational. Much of the discussion has been at the level of "Why not?" (why not let women profit by the Deep Springs experience?). Creating a similar institution for women would doubtless be a good idea, and I recommend it to anyone with a spare ten million dollars. But adding women to the existing Deep Springs would threaten its purpose and method of operation. The almost monastic concentration would surely be lost. Personal relations would surely be complex in this tiny student body. Although establishing two-somes would occur naturally enough (at some loss to those not selected), *dis*establishng them in this little community would be much more complicated; there is no place to hide, all relations, no matter how personal, are open to public view. Although the Trustees have resisted this thrust until now, it seems highly likely that in a few years they will give in.

Meanwhile, Deep Springs continues, much as Nunn conceived it. The Student Body is able, lively, and devoted to the institution. But still it is appropriate to ask: Is Deep Springs worthwhile? It is probably the most expensive undergraduate education anywhere, and a great deal of alumni and Trustee effort is expended in addition to dollars. Only a dozen students a year graduate, Although there has been a Congressman, several ambassadors, and others of note, none has yet been a U. S. President, if that is any measure.

You will have your own opinion of whether Deep Springs is worthwhile. You have probably already guessed that I believe it is. I adduce only two items: First, it does not subtract in any way from the American scene, and thus any positive contribution is a net plus; taking a dozen students a year from the pool of 18-year-olds will not drive any other institution out of business. Second, scores of alumni believe its influence on them was such that they are willing to work hard to sustain the institution for the benefit of another generation.

Now, in conclusion, I leave with you the question: Am I justified in calling Deep Springs "unique"?

Sybron Corporation
An Illustration of Corporate Governance

The Sybron Corporation was a conglomerate of mostly Rochester companies: Taylor (instruments), Castle (hospital equipment), Ritter (dental equipment), Pfaudler (industrial tanks), Nalge (plastics), and Sybron Chemical (industrial chemicals). I became a member of the Board of Directors in about 1971 at the request of Mercer Brugler, then Chairman of Sybron and Chairman of the University Board of Trustees. At about 1980 Bill Stolze (another Board member) and I began to warn the Board that in many places (notably in Taylor) the technology applied was weak and the products were obsolete. Donald Gaudion (who had become CEO) became angry and defensive and asked us to visit Taylor. We did that, and we reported that the situation at Taylor was even worse than we had thought: Foxboro and Leeds and Northrup were already in production on modern, solid-state electronic sensors and controls, whereas Taylor was only beginning to consider the possibility in the laboratory, and the laboratory was fully occupied in designing cosmetic changes to obsolete (D'Arsonval) instrumentation. The troops defended themselves, and Gaudion took no action. Stolze resigned. I attempted to do so, but Gaudion made it clear that if I did, thereby raising questions in public about the

Board's confidence in the management, it would hurt the University; both Sybron Corporation and many of its executives were major donors.

In the period February to May of 1985 the President and CEO, Herbert Jarvis, with the concurrence of the Board, retained the investment banking firm of Lehman Brothers to advise on corporate strategy. This followed a period of about three years when we had been buying and selling divisions and companies in an attempt to make a more coherent and profitable corporation out of a bag of disconnected units. Lehman reported to the Board on May 18 and said that we were just right, that we should not do any more buying and selling and should not try to merge the corporation into another or organize a leveraged buyout. The Board accepted the report and began reducing the corporate staff that had been added during the acquisition and sale period, a process that continued through the summer at two Board meetings.

On the Saturday before Labor Day I was in Toronto chairing a meeting of the Loran Commission. A telephone call from Jarvis informed me that he and four other executives had organized a leveraged buyout. They had evidently been working in secret all summer retaining, with corporate funds, Sybron's counsel and Lehman Brothers. They were offering to buy all of the stock at (I believe) something like $19 a share (it had been trading around $15).

They gave the Board only five days to decide whether it would accept the offer and put it to a shareholder vote, threatening shareholder suits if we did not comply. The Board was, of course, furious at this attempt to steal the company, by the CEO, the Chief Financial Officer, the Chief Operating Officer, the Chairman of the Board, and the Vice President and Corporate Counsel.

The immediate problem was to determine what the corporation was worth. If it was worth less than $19 a share and the Board turned down the offer, both the Corporation and we as individuals would be subject to suits. But our investment bankers (Lehman) to whom we would ordinarily go for an answer, were part of the cabal. At a Board meeting on Wednesday we decided that if a bunch of pirates including professional evaluators offered $19 it was probably worth more and we had the courage to turn down the offer.

Now what do we do? (Discuss)

What we did do was to give full powers to the three Directors living in Rochester, constituting what we called the "Fairness Committee" of the Board. For practical reasons I, the least qualified of the three, became Chairman. Our first concern was to run the company, to prevent disintegration. Then we decided that we had to sell it, since the stock control was by then dominated by short-term speculators; it was "in play," in the Wall Street slang. Furthermore, we could not know whether the second and third people in each of the various functions (e.g., finance) were loyal to the shareholders and Board or to the cabal. We retained Salomon Brothers (a rival of Lehman) to help us market the company and (I believe) White and Case (a major New York City law firm) to monitor every action (or inaction). In three months we put together a financial description of the company and went out for bids. There were three bids, including one by the original cabal (who had been tapping our phones). We accepted the highest, a $26 per share cash bid by KKR, a firm specializing in takeovers of this kind.

Was this a success? Hardly. The long-term investor got $26 per share when the pirates had offered $19, but was forced to sell her shares and pay capital gain tax even if in real terms she lost. KKR ran the company for a few years as a private entity, milked it of cash, and sold it again as an IPO . The suits and threatened suits gradually subsided, with no damage except the payment of lawyers' fees.

Is there any way to prevent this kind of chicanery? I know of none. The recent Sarbanes-Oxley legislation would not have helped. What are your suggestions? (Remember that you must also run the company in more ordinary times, with fierce international competition.)

A Quick Romp Through the Quantum Century

I am sensitive to the Ciub's abhorence of talking shop and I know I will be skirting the boundaries. But what I will say is properly called "intellectual history," ordinarily peddled from the shop of the historian.

In the title of this talk I have called theTwentieth Century the "Quantum Century." Most of the talk will be concerned with the story of the physical world as revealed by quantum physics and quantum chemistry in this Century, which opened spectacularly exactly 101 years ago. It was celebrated last year with, I am afraid, some rather romantic portrayals of Albert Einstein and some examples of the scientific illiteracy of major media. Now, this year, we can approach it more calmly, and it still emerges as a noteworthy Century.

- But first I will follow the development of physical science from the 17th Century forward, slopping on paint with a very broad brush. I will follow development into the 20th Century, which I have called the "Quantum Century" because of the flourishing of quantum mechanics.

In addition to telling what I hope is an interesting tale, I have three objectives: 1.) To de-mystify and to defend quantum physics and quantum chemistry; I claim that quantum mechanics is no more mysterious than Newtonian mechanics. 2.) To prepare you for defense against a plethora of books that purport to explain the universe.

To do that I need to begin by reminding you of another remarkable Century, the Seventeenth. At the opening of that Century the observations of the Dutch astronomer Tycho Brahe had been sufficiently accurate to enable the German physicist Kepler to show that the earth moves in an ellipse, not a circle, around the sun and to find two other laws of planetary orbits. Thus accurate *observation*, not the speculative thought of the Greeks, was beginning to guide the understanding of the physical world.

Even more consequential was the role of *experiments* by the Dutch carriage maker Simon Stevinius and the Italian physicist Galileo Galilei. Theirs were not the first experiments (recall the Greeks Diogenes, Archimedes, and others), but Galileo's work was far-reaching; his analysis of the pendulum was especially impressive and incidentally led to clocks, an early example of the application of science to practical devices.

Such was the basis for Isaac Newton's famous remark: "If I have seen a little farther than others, it is because I was standing on the shoulders of giants." Newton invented the differential calculus to deal with mechanics experiments and prove Kepler's laws of planetary motion. He invented the integral calculus to deal with gravitation and the orbit of the moon. As you know, Newton's laws of mechanics, especially the Third Law, $F = ma$, form the basis for all design and operation of devices and equipment even to this day (with only a slight modification to m). Newton's laws of mechanics and universal gravitation were spectacular theoretical developments that began the tradition of interaction between theory and experiment that has characterized science ever since.

Newton's universal gravitation law stated that two masses m and m′ attract each other by a force proportional to the product of the two masses divided by the separation distance squared. It posed a fascinating question: How does one mass m know the other mass m' is there? Or if it has moved to a different value of the distance between them? What is, as the question was put at the time, the *machinery* of the interaction?

This mystery of "action-at-a-distance" remains today. It has been complicated by theories and discoveries of new particles in big accelerators and the vacuum appears to be a busy place, but there is still no understanding of the "machinery" that ties our moon to us.

It is fortunate that, starting with Newton, able scientists, although troubled by the mystery and subjected to endless argument, were not paralyzed by it; they respected the philosophical weakness of the foundation for their calculations, but they tried not to think of it while successfully calculating orbits and building machines.

Another development in this great Century was the beginning of popular attention to science. Although Newton's revolutionary work was published in Latin, an English version was soon available. Voltaire, a refugee in England from repression on the Continent, was astounded to observe the great crowds at Newton's funeral, "as if he were king." But Voltaire also wrote: "Few people read Newton because one must be learned to understand him, but everybody talks about him."

To recapitulate the developments of the Seventeenth Century. it produced:

1.) The birth, or at least rebirth, of experimental science.

2.) The working together of theoretical and experimental science.

3.) Accurate calculations and predictions despite the basis in a profound mystery, action-at-a-distance.

4.) The application of science to make useful devices and processes.

5.) Public attention to and reading of science.

II

Fast forward now, and I will pass quickly over the 18th and 19th centuries. Early in the 19th Faraday and Henry discovered electromagnetic induction, upon which the vast electrical industry was based. But it brought in a new mystery, the creation of an electric potential by a magnet moving through "empty" space, a new version of action-at-a-distance. Toward the end of the century James Clerk Maxwell formulated the laws of electromagnetism elegantly in six equations, papering over the mystery by introducing the concept of a "field." Calculations based on this concept guided the production of motors, generators, transformers and other devices and predicted radio waves.

Meanwhile chemists and some physicists had almost convinced themselves that matter was made up of atoms and molecules, and the art and science of chemistry had been prolific.

I digress to remind you of the stunt the late Senator William Proxmire pulled off to give publicity to his antagonism to the National Science Foundation. He held a mock contest monthly for his "Golden Fleece Award," alleging a waste of money by publicizing research project titles which sounded crazy.

Well, in Germany in 1895, a research investigation occurred that would have won a Proxmire award; it could have been titled: "Some Observations on the Anomalous Behavior of Barium Platinocyanide Crystals in the Neighborhood of Crookes Tubes." Silly? It was Röntgen's discovery of X-rays.

At nearly the same time, Becquerel and Marie Curie discovered radioactivity.

To recapitulate: At the end of the the 19th Century, Newton's Laws prevailed. But with electromagnetism and the discovery of radioactivity and x-rays, new mysteries were developing. It was increasingly becoming apparent that matter was composed of atoms, but no understanding had been created of the structure and properties of atoms, molecules, and solids. Chemistry was developing rapidly but more as an art than as a science.

III

Then, to start the Twentieth Century with a bang, in 1905 *four* articles appeared in the *Annalen der Physik* by a young patent examiner Albert Einstein, any one of which would have made him famous. One paper, on Brownian motion, resolved the doubts that were still current about the existence of atoms; not only did it produce telling evidence for atoms, it showed how their sizes and masses could be measured. Although a powerful contribution, it was overshadowed by the other three.

The second paper was conceptually revolutionary and introduced the theory that has come to be called the Special Theory of Relativity. Although it built on the theories and speculation of H. A. Lorentz (Dutch) and George Fitzgerald (Irish), it was as bold and unprecedented as Copernicus' claim that the earth circled the sun. It started from the problem posed by a body moving at or faster than the speed of light, a possibility not ruled out by the then current laws of physics. In order to keep the laws (including the speed of light) the same for all observers, Einstein found that time, mass, and length, up to then considered absolutes, had to vary with the speed of the observer relative to the body.

The variation of mass with velocity was soon verified experimentally. Furthermore, it is only a sophomore exercise to show that the mass variation leads to the now famous expression for the equivalence of mass and energy, $E = m\ c^2$, and Einstein published this in a brief fifth paper.

Einstein followed this path to the General Theory of Relativity in 1917. It was even more revolutionary than the Special Theory, requiring us to view time as the fourth dimension, weaving in the effect of gravitation on light, and forming the base for all theories of cosmology for the rest of the Century. It was adequately verified by astronomical observations in the eclipse of 1919. The philosophical implications of the cosmologies based on it are profound, and astrophysicists make good use of it in dealing with the "big bang" and the subsequent development of the universe. Few people understand it, and I am not now one of them. (Recall Voltaire's remark about Newton, which applies with equal force to Einstein.) Unlike Newton's work, there are as yet no applications to the "touchable world."

Not so for the Special Theory and the mass-energy equivalence. The notorious application of this equivalence was the "atomic" bomb of 1945, but more peaceful applications, such as to the medical use of radioactive isotopes, abounded. Whenever a nucleus of an atom emits radiation or a particle, its mass decreases by an amount calculated from the energy emitted. Calculations are exact and useful, despite the lack of a comprehensive theory of nuclear forces. It is yet another example of proceeding with the application while leaving a mystery aside.

IV

The fourth article was the real shocker, and the theory, experiment, and applications based on it are the reasons I call the Twentieth Century the "Quantum Century. It dealt with the photoelectric effect that had been discovered by Heinrich Hertz, the same Hertz who had invented wireless, then called "radio." Hertz found that electrons were emitted when light was incident on a metal in a vacuum. Nothing odd about that, it was just one more indication that matter was basically electrical. But Hertz measured the energies of the emitted electrons and found that, strangely, they did not change as the light intensity was drastically increased. They did increase, however, if shorter wavelength light was incident.

Einstein took the courageous step of treating the light as a stream of particles, each with the energy $h\nu$ wherein ν is the frequency of the light and h is a constant. This approach nicely accommodated the observed behavior. Furthermore, this constant was just what Max Planck (a very distinguished German) needed in his surmise about hot-body radiation; it is now called the Planck constant and the packet of energy is called a quantum of light or a photon. Einstein, the lowly clerk, helped Planck, the *Herr Geheimrat*, out of what had become a dead end.

What made Einstein's paper revolutionary, of course, was that "everybody knew" that light consisted of waves. Indeed, the light in Hertz's experiment had passed through devices that depended for their functioning on the wave nature of light.

Within a few years Rutherford, a New Zealander at Cambridge, showed that the atom consists of a tiny, heavy core surrounded by light electrons, and de Broglie (French) suggested that an electron is diffracted like light with a wavelength equal to h divided by its momentum, a suggestion soon experimentally confirmed by Davisson and Germer (Americans).

In the late 1920's all of these facts, both contradictory and paradoxical, were put together by Schrodinger and Heisenberg, Germans, into a theory called *quantum mechanics*. Like any good theory, it also made predictions that could be compared with observations. Indeed, it is still copiously producing such.

Its first success was the hydrogen atom, the simplest of all atoms, consisting of a single light electron and a heavy proton. I am sure you have seen "pictures" of a little ball orbiting around a big ball, a sign of some "nuclear" product or installation or part of the logo of a company. Such are quite misleading. One cannot follow the electron around the proton; there is no way of measuring its position or velocity. What one can do, and quantum mechanics does that, is to describe the atom as a *situation*, with a precise energy. If excited, as are the atoms in a fluorescent lamp, it may be in a higher energy state, and it may radiate energy in the form of photons, light quanta; quantum mechanics correctly predicts the wavelengths of the radiation. This is just one example of the general situation: For anything that can be measured, quantum mechanics has a calculation that can be tested. Furthermore, as we will see, quantum mechanics leads to a marvelous array of applications and inventions. What more can you ask?

Quantum mechanics provides a quantitative understanding of the photoelectric effect, for example, including the motion of the light to the emitter, the motion of electrons in the emitter, the emission process itself, and the subsequent motion of the emitted electron. In the process, sometimes one is using the mathematics of waves and sometimes of particles, but always as part of the same theory.

But, you may ask, is the electron a wave or a particle? Why do you ask? What would you do with the answer if there was one? Quantum mechanics lets you calculate anything you can observe, and you can function famously after exorcizing the ghostly words "wave" and "particle" from your vocabulary.

V

And yet, despite its achievements, quantum mechanics has a bad press. One of the reasons for this is that many of the predictions of quantum mechanics are in the form of *probabilities*. For example, one can calculate the probability per unit time of particle emission from a radioactive nucleus, or of radiation from an excited atom. Or one can calculate the probability that an electron will be within a small range at a particular distance from a nucleus; this can then be used in calculating the binding that atom will have to another atom to form a molecule or the binding into a solid, tasks that were impossible with Newtonian mechanics.

What is probability for a single particle becomes essentially certainty when one deals, as we always do, with large (on the atomic scale) assemblies of particles; the Law of Large Numbers, familiar in the study of statistics, yields predictable macroscopic behavior from uncertain microscopic behavior.

Yet the probability aspect leaves one somehow dissatisfied. Even Einstein felt it was inadequate. He famously said: "I do not believe that He, the Old One, throws dice," and he argued for years with Nils Bohr (a Dane); but he eventually came around. And there are other subtleties, some of which seem paradoxical, such as correlations of behavior over long intervals in space and time.

Again, as with Newton, physicists have sidestepped. Along the way an expression has entered the literature: "Shut up and calculate." This has often been attributed to Richard Feynman, who had a spectacular career of calculations, and this blunt, even crude language is characteristic of Feynman. But I doubt if he said it. Although a great calculator, he also was the master of the subtleties of quantum theory and the creator of a whole new physics of " Feynman diagrams," for which he got the Nobel Prize. The advice was good for me, but not for Feynman.

A second reason quantum mechanics has a bad reputation is the Uncertainty Principle. Just as people think that they have captured the essence of Einstein's insight by saying "everything is relative," people think that quantum theory means that "everything is uncertain." For example, the *New York Times* principal story last year commemorating Einstein's papers was titled: "One Hundred Years of Uncertainty." But the Principle, which is a part of quantum mechanics and not a separate piece of physics, is much more limited. It applies only to *pairs* of quantities, such as energy and time, or position and momentum. It says that the more accurately one of the pair is determined, the less accurately can the other be determined. For example, the energy (and therefore the wavelength) of a spectral line in radiation from a gas of atoms (as in a fluorescent lamp) can be determined as accurately as one wishes, provided only that one foregoes determining exactly the lifetime of the radiating state. What would you do with the answer if there were one? Quantum mechanics gives accurate predictions in any situation where accuracy is useful. Indeed, the world standard timepiece is a quantum clock with a possible error of much less than a second per century, much more accurate than timekeeping based on the rotation of the earth.

A third reason quantum theory has a bad press is that everyone is comfortable with the thoroughly verified Newtonian mechanics, and to the extent quantum mechanics is different, it must be wrong. But quantum mechanics gives the *same* answers as Newtonian mechanics in the regime to which Newtonian mechanics applies; we can have both, with no conflict. One can, for example, produce a quantum theory of Galileo's pendulum, giving exactly the same result; it is pretty cumbersome, but it works.

My main defense of quantum theory, however, is to emphasize its *utility,* the same attribute that justified Newtonian mechanics. It is hard to know where to begin, since. our whole approach to the physical world is now quantum theory. It provides answers to such basic questions as: What holds matter together? What holds matter apart? How do electrons move in solids? And also very detailed questions such as: Why is iron ferromgnetic? eedntail for moodrn ccivilizion, Tllurid dedmontrte. But why is iron ferromagnetic? How is our sun heated?

Perhaps the application with the most significance for our daily lives was the prediction and calculation of energy bands for electrons in solids. This led to understanding semiconductors such as silicon, and it led to inventions such as the transistor. The transistor led into the integrated circuit, the "chip," without which modern life, from moon walk to cell phones, would be quite different.

Another quantum device is the laser, which can trace its lineage back to one of the early quantum experiments, the Stern-Gerlach experiment in 1922. Also, all theoretical chemistry and much metallurgy are based on quantum theory. The neglected heroes of the Quantum Century are chemistry and metallurgy. To cite just two down-to-earth examples, lubricants and alloys that permit an automobile engine to last for 100,000 miles were unheard of at the start and commonplace at the end of the Century.

Insert Bethe here

V

(I return now to the main talk.)

I do not claim that the development and application of quantum mechanics are more consequential than the great engineering, social, and political achievements in the Twentieth Century. But its revolutionary view of the physical world, replete with applications, emboldens me to call the Twentieth the "Quantum Century." And it is even expected to form a base for spectacular progress in understanding the biological world in the Twenty-first.

But, in an exact parallel to the 17th Century's Newton, there are mysterious subtleties in quantum theory that I have ignored ("Shut up and calculate"), and the probability aspect, although practically satisfactory, is philosophically uncomfortable. Over-all, the mysteries in quantum mechanics are no more itroubling tthan the mysteries in Newtonian mechanics,

Above all there is still a mystery about forces. There seem to be just four in nature: the electrostatic force, the gravitational force (each of which by itself is now well understood), and the "weak" and "strong" nuclear forces (which are not well understood, at least by me). Many, perhaps most, physicists believe that there will eventually be some grand theory that encompasses all four.

Many popular books have been written, mostly by eminent British physicists and mathematicians, that speak to the "theory of everything," a theory that (if it existed) would encompass all the known forces and interactions. These books appear to be designed to impress rather than to inform. Somehow they get to be bestsellers. After giving a momentary technical gloss to book clubs, they must lie unread on the coffee tables of gullible intellectuals. I strongly suggest you shun them. Any real progress in this difficult area is much more likely to appear first in the technical literature.

Three-fourths of a century of trying has not produced a theory that includes the four forces. A glimpse of the difficulty can be seen if one compares the magnitudes of the electrostatic force and the gravitational force between two particles: The electrostatic force is about 4×10^{42} times the gravitational force! Yet both forces would have to play prominent roles in equations of a grand theory. One simply cannot accomodate such disparate quantities in equations, and so the mathematics must be more complicated and possibly new. The other two forces also bring in serious problems. It may be that we must give up the whole concept of force as a function of position, just as we de-emphasize position in quantum mechanics.

VII

How will the Quantum Century be remembered? Will it be remembered as just another century or as an equal to the Seventeenth in advancing the understanding of the physical world and in the application of science to the quality of life?

I believe the answer will depend on whether any of the experiments and partial theories of the Quantum Century provide a foundation on which to stand to clear up the vast mysteries, some of which I have described. At the moment and to this observer, it does not seem likely. The insights coming from accelerator physics and astrophysics are fascinating but *fluid;* one is not tempted to put much confidence in the theory *du jour*. (The currently popular one is called "string theory," marketed, but not explained, on Public Television.) Yet any new understanding will have to be consistent with the accelerator experiments.

There is a story about a young theoretical physicist who finally got a hearing with Werner Heisenberg for his new theory of the fundamental particles. The great man's report was: "Any theory that will make sense of this complicated situation will have to look very crazy to us; your theory is not crazy enough." Any rationalization or replacement of the four forces will probably look very crazy. It will probably need to look as foreign and revolutionary to us as the Einstein papers did to the readers of 1905.

If and when a new revolutionary understanding comes, it will probably be revealed, as were Newton's and Einstein's, in the technical literature, suitably augmented by electronic publication, rather than in cocktail-table books. Meanwhile we continue in a more pedestrian way to enjoy the fruits of the work in the Quantum Century by those who heeded the advice to "shut up and calculate."

Robert L. Sproull
September 2006

Hans Albrecht Bethe

I digress to bring quantum theory home to Western New York by noting the career of Hans Albrecht Bethe, whose life virtually coincided with the Quantum Century, who was one of the greatest contributors to that Century, and who spent 70 years of it at Cornell.

Bethe was born in Strassburg in 1906 and died last March in Ithaca.

In his 'teens he became a student of the renowned Arnold Sommerfeld in Munich. A fellow student was Lloyd P. Smith, a postdoctoral fellow at Cornell, temporarily at Munich on a Rockefeller Fellowship. Both Sommerfeld, the *Herr Geheimrat,* and Smith, the student, identified Bethe as the ablest physicist they had ever known. He was a half generation younger than the original giants of quantum theory (Einstein, Planck, Bohr, de Broglie, Schrodinger, Heisenberg), but he had mastered and began extending and applying quantum theory before he was 20. While still in his twenties he wrote Volume 24 of the *Handbuch der Physik,* the canonical quantum theory of atoms, molecules, and solids, still in use today.

In 1933 Hitler canceled the academic appointments of all who had a trace of Jewish ancestry. Sommerfeld arranged temporary appointments for Bethe in England at Bristol and Manchester. The depression was forcing Cornell to "downsize " and creating a new position was virtually impossible, but Lloyd Smith, by then a professor, worked hard and ultimately successfully to create a position for Bethe

.

Within three years Bethe had worked out the "carbon cycle" whereby a chain of nuclear fusion reactions heats our sun, an achievement that gained him the Nobel Prize. Additional contributions poured out, both applications and fundamental extensions of quantum mechanics. His teaching and aid to students soon became legendary.

During the War he was briefly at the MIT Radiation Laboratory, contributing to microwave radar and then became head of the Theory Division at Los Alamos. Theory was especially important on the "atomic bomb" project because vital measurements were lacking. For example, one had to infer from theory important properties of plutonium before any plutonium was available for experimental purposes.

After the War, despite many other attractive paths, Bethe chose to return to Cornell, and many of the ablest Los Alamos physicists followed;
two of the most notable were Freeman Dyson and Richard Feynman. Bethe continued to turn out great quantities of remarkable papers and students. At age 93 he gave a series of popular lectures on quantum theory at the retirement village where he and his wife Rose lived until his death last March at 98. His career of important calculations, nearly coterminous with the Quantum Century, probably set a record for quality, quantity, and relevance.

Robert L. Sproull
18 October 2005

NUCLEAR TEST DETECTION

Robert L. Sproull

I have chosen for my example a complex fabric of science interwoven into policy, covering 35 years. I am substituting at virtually the last minute in this program, and I apologize for the lack of quantitative information in many places, such as magnitudes of tests; I have not had access to any records in the few days allowed me for my preparation.

In the period from 1945 to 1963, concerns gradually increased about the testing of nuclear weapons. The weapons laboratories (Sandia, Los Alamos, and Livermore) insisted that new and more advanced weapons had to be developed and tested, "to stay ahead of the Soviets." The advances were indeed striking, particularly in safety (avoiding accidental triggering), in efficiency (hydrogen bombs and tritium boosting), and reducing the size (even to the size of an artillery shell). But the rest of us had increasing worries about the expanding scale of nuclear testing by both the U. S. and the SU and the implications for public health and East-West tensions.

By the summer of 1963 John Kennedy had been President for two and a half years and the negotiations he had started for limiting nuclear weapons testing were approaching a climax. There were two objectives of a possible treaty: 1.) Symbolic. A step in reducing tensions, in demonstrating that there was an alternative to the monotonic increase in weaponry. (Here, unlike my practice elsewhere, I do not use the label "symbolic" pejoratively.) 2.) Real. A limit to the power of new weapons. A reduction in fallout, of which the world-wide build-up of Strontium-90 was especially dangerous.

An intense harvesting of all the available science applicable to the detection of nuclear weapons tests and the collection of intelligence from them occurred in the summer of 1963. The science applicable to tests in space, in the atmosphere, and in the oceans was adequate to design and field systems to detect cheating in any of these three environments. Some of this science had been supported by the Defense Department with a possible treaty in view, but most was just the body of scientific and engineering knowledge extant. Note that open, unclassified knowledge was particularly important, since the SU, too, had to be confident that they could detect cheating. Much of our capability was to be satellite photography, which we called "national technical means" as a favor to the SU military who did not wish to admit to their paranoid citizens that they did not shoot down our satellites.

Thus the capability of verification of a treaty outlawing testing in these three environments was assured. Clearly the President would not sign a treaty, and the more hawkish Senate would not ratify it, unless verification was built into it.

But the fourth environment, underground, was not in such good shape. The only possible means of detection with detectors outside the country cheating would be seismic detectors, and there was a serious problem in distinguishing explosions from earthquakes; the problem was labeled "discrimination." Small earthquakes occur almost everywhere, almost all the time; the earth is rumbling. A small enough explosion at a great distance ("teleseismic") could be masked by earthquakes.

An intense effort by a technical study team was fielded in the summer of 1963; it included people from State, Defense, the Arms Control and Disarmament Agency, and the White House. The policy questions were: Were "on-site" (in country) inspections necessary? If so, how many each year? These questions were marvelously convoluted with the questions about the purposes of possible SU tests, the magnitudes and frequencies of useful tests, and the discrimination capability and its possible improvement. [If time permits, material here on venting, the kinds of seismic waves, and the geometry of teleseismic observations.] Although there were substantial differences in outlook (hawks *vs* doves) among the members of this technical working group, there was nearly perfect agreement on the conclusions reported to the U. S. negotiators; good science was being applied by capable scientists to the policy decisions.

The U. S. position based on these conclusions from science was that about 10 on-site inspections per year must be provided in a test-ban treaty if it was comprehensive (four environment) and that nearly free access to Soviet territory must be permitted. The Soviet position was that no on-site inspections should be permitted. Much negotiating occurred, with back-and-forth motion on the "numbers game" and restrictions on access. But the Soviet negotiators clearly did not want to permit access, and the U.S. negotiators (basing their stand on the harvesting of science as of 1963) were adamant that some in-country observation must be permitted.

The negotiators gave up on the underground environment, and the President signed a three-environment treaty. The Senate ratified the treaty subject to three provisos: 1.) The DoD would guarantee that any cheating would be observed. 2.) U. S. underground testing would be actively conducted. 3.) The vitality of the weapons laboratories would be sustained.

The U. S. policy was still that a *total* test ban should be pursued. Clearly the key to adopting a comprehensive treaty was to elevate the science of discrimination from its primitive state.

The Advanced Research Projects Agency had been designated by the Secretary of Defense as the agency for nuclear test detection research. ARPA had been created in the post-Sputnik period as a quick-response, advanced science and technology unit, active especially in areas where more than one (or none) of the armed services was active. ARPA, in part through JASON and IDA, had provided the critical survey of science used in the 1963 negotiations. It was this work that had concluded that cheating by approximately 100KT explosions could not be distinguished from earthquakes at the (primitive) stage in which discrimination science was in 1963. ARPA therefore undertook advancement of that science in support of the U. S. policy position.

There were three, more or less separate ARPA programs. I will describe each and than explain how the results of each were harvested for policy decisions.

The first program to be described was a "head-on" attack at the detection of cheating by simulating as closely as possible a likely clandestine underground SU test. A site for such that would be attractive to the SU would be the Kurile-Kamchatka area, much of which is wilderness and all of which is extremely seismic. ARPA conducted an experiment in which an underground nuclear explosion occurred in Amchitka, in the western Aleutian Islands. The geology in Amchitka closely resembles that in the Kurile-Kamchatka region, and Amchitka is at teleseismic distances from major U.S. seismic detectors. Most notable among these was the new seismic array covering many square miles in Montana. The explosion was announced in advance, and seismologists in many countries examined the signature at their stations, noting how the various surface and volume waves differed from an earthquake. The implication for policy was that now we knew the magnitude of a test in a likely region of the SU that could be confidently distinguished from an earthquake.

Another simulation experiment was carried out in a cavity in a salt dome near Hattiesburg, Mississippi. Many had suggested that if a nuclear explosion were made at the center of a large cavity underground, the energy would be only weakly coupled to the surrounding rock and the explosion would go undetected. The implication for policy was that, although some decoupling occurred, SU tests of useful magnitude could not be concealed in this way. I leave to your judgment whether this was wasted money: Powerful people in Congress believed they invented the idea of decoupling and promised continued opposition not only to a comprehensive treaty but to research validating such a treaty until the experiment was tried. The underlying science was in good shape before the experiment, which produced the expected results.

The second ARPA program was a long-range program of advancing the science of seismology and discrimination and developing more capable seismology instrumentation. Seismology had been virtually the fief of Jesuit colleges, and the only seismographs were archaic instruments recording on drums individually carbon coated by exposure to smoke. The ARPA program developed institutions as well as people. [It was helped by the arrival of plate tectonics on the geophysical scene.] It advanced the understanding of acoustic waves in the earth's crust and interior and the different effects at long distances of an earthquake tensor source from an explosion scalar source. The implication for policy was that each year one could lower the magnitude of a cheating explosion that could be confidently distinguished from an earthquake.

The third ARPA program was the creation of a world-wide network of 125 seismic stations, mostly in the third world. ARPA provided the equipment and a data and information exchange, but the stations were operated by the local hosts. There were two motivations for this program: First, to strengthen seismology and geophysics world-wide; a stronger profession should provide more sensible advice to governments and more scholarly responses to crises or wild claims. Second, to provide detection of tests from closer stations and by a large spectrum of friends, enemies, and neutrals, expected to be an advantage when a "he said, she said" controversy ensued from a SU test that the SU disavowed.

Although the first of these programs was by far the most expensive, the consequences of the second and third were far greater. The magnitude of the test that could be observed and confidently distinguished from an earthquake was steadily pushed down. Digital seismometers were developed, with greatly enhanced dynamic range and recording capabilities. The science of using frequency, polarization, arrival time, and amplitude decay to separate explosions from earthquakes was developed.and this science, more than the "head-on" experiments, permitted signing the "threshold treaty," without demanding on-site inspections. This treaty outlawed tests underground above a threshold magnitude; although it was never ratified, both sides have been observing its provisions, and it has contributed to the reduction in East-West tensions.

The 125-seismometer network has meanwhile flourished and become the heart of the PASCAL network of the Incorporated Research Institutions for Seismology, IRIS, the consortium of all the American universities that have any research in seismology. IRIS is now negotiating for establishing a world center of the network at Borovoye in northern Kazakhstan.

The break-up of the Soviet Union in 1990-91 has rendered moot the early policy barrier of on-site inspections for U.S.-SU treaties. At least for the time being, treaty verifiers could have all necessary access on-site in the FSU republics. This happy situation could, of course, deteriorate rapidly if some of the politically and economically disaffected elements were to reverse the progress of the last three years, especially in the Russian Federation.

But now there is a new arena where science is needed to support policy: The U.S. policy makers are vitally concerned about the proliferation of weapons of mass destruction. The research underlying detection of nuclear tests in all four environments is now vital to treaty verification and to isolating non-signers of the Nonproliferation Treaty. The science developed over these years has thus permitted 1994 policy makers to negotiate and monitor, with full confidence that cheating would be detected and without the messy questions of sovereignty that would arise from demands for on-site inspections.

Thus by continuing the longer range scientific programs, the U.S. has obtained a major tool for deciding policy and supporting policy makers in the new and challenging environment of proliferation.

The major lesson I take from all this is that if one is supporting good science in fields relevant to important policy decisions, it is wise to continue the research even if the policy decisions for which the science was initiated have been made. Of course this puts a major burden on program managers and defenders to *know* what is relevant and to have courage. Courage is required to defend the program when the urgency has passed and to sort and discard when either quality or relevance deteriorates.

I see also some more detailed lessons:

1.) In this case study the principal actor was ARPA, and the characteristics of ARPA that led to success were its relative independence of the policy makers and its ability to plan and manage over a number of years. No "guidance" from the White House influenced the research (although White House approval was almost withheld from the Amchitka shot). The reputation of the Agency carried its budgets through Congress without micromanagement (although of course the over-all appropriation rarely equalled the request). The only outside control of the program within the over-all budget was by two people in the Department of Defense each of whom had at least as much vision and technical competence as the managers within ARPA.

2.) ARPA enlisted the service of the JASON group of young university scientists who spent most of their summers on projects like this one. Their fresh, independent competence and varied backgrounds injected great resilience, strength in depth, to the program. They did not manage the program, but the program managers exposed the issues and choices to them and then listened carefully to their analyses and recommendations. ARPA also enlisted people at the Institute for Defense Analyses (IDA) to perform studies and help when a quick response was required.

3.) Continuity of program and persistence of vision were essential. ARPA by its nature had to have mostly "short-timers" as Directors and Associate Directors, but replacements conscientiously continued the directions of their predecessors.

4.) The whole program was open and unclassified. The international part was generous to the foreign participants, and that generosity almost certainly secured more for the U. S. dollar than hard-nosed contracting would have secured.

Outline of presentation by Robert L. Sproull

NUCLEAR TEST DETECTION

1958-63 — Science to detect underground explosions and to distinguish them from earthquakes.

July 1963 — Harvesting of science for policy decisions by U.S. side in nuclear test ban talks.

October 1963 — Harvesting of science for U. S. Senate hearings on ratification of three-environment nuclear test ban. Disagreements on risks and sizes of useful explosions, not on science.

Nov. 1963+ — Design of science for complying with obligations undertaken at Senate hearings and to make possible a comprehensive test ban:

1. "Head-on" experiments: Longshot and Dribble
2. Instrumentation, institutionalzation
3. World-wide network

1965+ — Results, conclusions, new basis for decisions.

1968+ — Change in policy-makers' objectives; threshold test ban.

1990-91 — Break-up of Soviet Union. New policy concentration on non-proliferation.

1994+ — Harvesting of instrumentation and world-wide network for new policy. IRIS. Potential violators with limited real estate.

Lessons

1. Nature of ARPA. Decoupling of science from policy decision makers.
2. JASON and IDA
3. Continuity, persistence, vision of program managers; insulation from Congress.
4. Open, unclassified, generous programs to underpin international cooperation.

FAX COVER SHEET

To: Charles M. Knapp
Technical Resources Inc.
712 Fifth St., Suite G
Davis, CA 95616

From: Robert L. Sproull
E402, 16910 Bay St.
Jupiter, FL 33477

To FAX machine 916 756 4835

Total number of pages including this cover sheet: 2

Date: 2 March 1994

Your 26 February FAX asked for my address; it is above. My title is President and Professor of Physics, Emeritus, University of Rochester.

The accompanying FAX was sent to Mittelman at the same time as this.

FAX COVER SHEET

To: Abe Mittelman
Technical Resources Inc.
3202 Tower Oaks Blve.
Rockville, MD 20852

From: Robert L. Sproull
E402, 16910 Bay St.
Jupiter, FL 33477

To FAX machine 301 231 6377

Total number of pages including this cover sheet: 2

Date: 2 March 1994

Mel Knapp asked me to FAX this to you. If necessary to call me, my number is 407 747 4396. In an emergency I can be reached by FAX to 407 746 3933.

Knapp also asked for a vitae but suggested I bring this with me to the meeting on Wednesday.

The Palomares Incident

Robert L Sproull October 2002

The Cold War incident I am about to describe occurred in 1966, but I believe it is worthy of your attention even now because it has an important lesson for education, because it resulted in profound changes in that War, and because it was instrumental in creating the field of ocean engineering,.

At 9:26 in the morning of January 17, 1966, the people of the little town of Palomares on the southern coast of Spain heard a loud explosion and saw a huge fireball in the otherwise perfectly blue sky. Out of the fireball emerged hundreds of objects, many burning brilliantly. The instantaneous explanation was, of course, that it was a miracle associated with the fact that it was the day of Saint Anton the Abbot. More credible explanations slowly developed. Within a matter of minutes, the objects rained down over a remarkably wide area. In a few hours, the Spanish Guardia arrived with a force of one hundred, but the Guardia is everywhere, all the time, and this did not worry the townspeople. What really worried them was the arrival within a dozen hours of the first of an ever increasing squad of U. S. Air Force officers and men, followed in a few days by the U. S. Navy in force. What was it all about?

Six hours earlier a U. S. Air Force B52 had taken off from an Air Force base in North Carolina on a routine training mission. It crossed the Atlantic on schedule to rendezvous over southern Spain with a KC135 which had taken off from the local air base at Moron. Such operations had been performed routinely thousands of times as part of creating a "credible deterrent." Without such exercises, no one on either side of the Iron Curtain would believe that the B52 force would actually be effective. Furthermore, the Air Force insisted that having actual weapons aboard was essential to the training. This B52, like all the others, therefore had four hydrogen bombs in its belly.

The B52 was to be routinely refueled from a KC135 tanker. What **should** have happened is shown on the chart. The B52 approached from underneath with a closing speed of about six knots. What actually happened can be only imperfectly inferred from the oral testimony of the survivors and the mute testimony of the fragments of the B52. It seems likely that there was an abrupt onset of violent clear-air turbulence and that the refueling boom of the KC135 penetrated the B52 like a dagger to its heart. One of the main longitudinal spars was severed and the aircraft started to break up. The reaction back on the KC135 caused an explosion fueled by hundreds of thousands of pounds of jet fuel that the tanker had been about to deliver to the B52. The four crewmen of the tanker were killed instantly, and only small flaming pieces of it emerged from the fireball. The breakup of the B52 was much slower since it was a stronger airframe designed for more maneuvering and gust loading and since it had little fuel aboard. Three of the bomber's crew were killed or trapped and died in the wreckage. Four of the B52 crew survived, each by a different route.

Captain Buchanan, the radio operator, was the first to leave. He ejected, tied to his seat. He was hospitalized for burns but was not seriously injured. His parachute operated normally, but he could give very little information about other objects in the sky.

Second to leave was Major Messinger, who was worried about his chute and pulled the cord at an uncertain altitude. He saw two survival chutes like his own. Since his oxygen bottle did not work he was lucky to be alive, but he survived without serious injury.

Captain Wendorf had even greater problems. He was subjected to extreme accelerations, he broke his arm in ejecting, and his oxygen mask was never found. His chute opened at 14,000 feet and caught fire when a burning fragment of the KC135 hit it. He was the first to land, on shore but near the water. He believed he saw an object that looked like two parachutes close together.

Airman Rooney, the navigator, was subject to severe accelerations but managed to crawl to a hole and tumble out. He was lucky to survive since he had no oxygen. We shall come back to the observations of the survivors later but meanwhile note only that it is remarkable that they observed anything at all.

By four o'clock that afternoon the Guardia had found the first of the four hydrogen bombs. It was near the coast, southeast of Palomares, in perfect condition except for the loss of its fins.

Now I must digress to talk about hydrogen bombs. You probably have never had the occasion to think about it, but if you did, you would immediately realize that these bombs must be provided with parachutes. The bomber could not separate himself from the exploding bomb by the many miles of lethal radius of the bomb unless he could delay the explosion by deploying the bomb with one of several parachutes. Thus you can well imagine that a bomb "package" would include a variety of parachutes, timers, and altitude sensors. It is this fact that enormously complicated the Palomares incident.

Bomb number one was preserved intact by one of its parachutes. But why did it not "go off"? Here we get into another feature of bomb technology. Most people seem to think of an atomic bomb or a hydrogen bomb as if it were a large pot of nitroglycerine that had to be handled very delicately to keep it from exploding. This is a long way from the truth. To be sure, one of these bombs carries considerable **chemical** high explosive; when detonated this causes the implosion that compresses the fissionable material to a critical density so that a **nuclear** explosion occurs. This high explosive must be set off in an extremely **precise** way, with each of many charges nicely timed, or the nuclear fission explosion simply will not occur. The same process occurs in the hydrogen bomb, since the fission explosion heats and compresses the fusion reactants. Several accidents have occurred and in **no** case has there been a **nuclear** explosion. All of these accidents have served to reinforce confidence in the basic "safety" of hydrogen bombs, that is, the carefully created feature that they cannot explode accidentally.

Bombs number two and three were found on the second day, the 18th. None of the parachutes of bomb two deployed. It hit the earth in the hilly region about two miles west of Palomares. Much of the high explosive had detonated upon impact, and pieces were found thousands of feet away. Bomb three was found in the outskirts of Palomares with a partially deployed, partially burned parachute. Some of its high explosive had exploded.

But bomb number **four**? There was no trace of it, and that is the rest of this story.

But first let's look at the activities of the next few days. You can well believe that an intensive search was going on for bomb four, but the most immediate problem was to take care of the population. Had any of them been exposed to any radioactivity? Radioactivity there was, in large quantities, mostly from bomb two. Many of the parts of the B52, which had rained down over tens of square miles, were contaminated. Parts of that aircraft were put aboard Navy ships for further study and attempted reconstruction.

Cleaning up the ground around the impact points of bombs two and three was harder. Every square foot of ground was surveyed with hand-held radiation-detecting instruments. Over a thousand cubic yards of soil were removed, packed into fifty-five gallon drums, shipped to the U. S., and disposed of as radioactive waste. Four hundred acres were not turned back to the owners until March.

You can well imagine the distress of the local population. Their principal crop is tomatoes, and disaster struck during their harvesting season. The Air Force moved in without red tape and gave cash compensation t o every farmer on the spot. Radiation is a very frightening thing. Since it makes no noise and is invisible it has a scary quality that is hard to appreciate unless you have been faced with it. In this respect, it turned out to be extremely fortunate that a number of Spanish scientists had participated in international conferences on radiation protection and were familiar with the instrumentation and procedures used for "sweeping" an area. Thus they could monitor the activities of the Air Force, and their testimony was, of course, much more respected than that of any U., S. spokesperson.

By the middle of March, thousands of cubic yards of new soil had been brought in, the farmers had planted their new crop, and the Province of Almeria was approaching its normal state, with one terrible exception: The fourth bomb had not been found. Here you see the great complexity introduced by the parachutes: Deployment of even a small chute separated the landing points of bombs one and two by three miles. Even if bomb four had become free from the bomb bay at the same time as the others, which was by no means certain, it could still be many miles away if its largest chute had deployed successfully. Thus it happened that the total area searched encompassed over a hundred square miles and included areas with elevations to 1500 feet on land and to depths below 2000 feet at sea.

On land, the situation was bad enough. Although h it was not too hard to cover the tilled tomato ground, the hills behind Palomares were very difficult indeed. When one thought he had just about every complication known to man, another cropped up: The hills are riddled with lead mines, many dating from Roman times. Since lead and uranium are usually found together, the tailings of these mines are moderately radioactive. But by the middle of March, the search on land, which was very expensive and highly demoralizing to the villagers, was coming to an end. There was simply no more territory to search. An experiment performed back in the U.S. had shown that even if an intact bomb without a parachute a had been dropped into the softest soil near Palomares, the crater would have been obvious in aerial photographs. The Air Force had already spent nearly a million dollars, in addition to personnel costs and damage claims. To continued the search would be expensive, damaging to relations with the villagers, and a lightning rod for international complications.

Nevertheless, the bulk of the physical evidence, which I have not detailed here, pointed to the likelihood that the bomb dropped on land. Against this almost overwhelming stack of evidence there were only three pieces of evidence that led one to look for the bomb in the water. First was, of course, the circumstance that a thorough search had failed to find the bomb on land. Second was the testimony of Captain Wendorf, who thought he saw an object that could have been the bomb. But you will recall that he had a broken arm, no oxygen, and a parachute that caught fire.

The third piece of evidence was discounted by all at the beginning. A fisherman, Francesco Simo, captain of the small fishing boat Manuela Orts, reported seeing a very large parachute with an object the size of a large man under it drop into the water. He had in fact thought it was a man, but when he went to the position where he thought the chute had landed, there was nothing to be found, man or parachute.

It was on the basis of these three pieces of evidence, however fragile, that a huge search had been underway in the Mediterranean. The Navy had sixteen ships at the peak, and in mid-March fourteen were still operating. Navy divers had picked up hundreds of pieces of light wreckage, aluminum panels that had floated down like leaves and drifted out to sea. These objects were highly dispersed, their positions depending on how heavy they were compared to their aerodynamic drag. The Navy was spending about a quarter of a million dollars per week on special deep-diving submarines and sonar gear, all real incremental costs and most paid to contractors. In addition, the Navy was charging to the project six hundred thousand dollars per week for ships. One got the impression that since it was an Air Force accident, the Navy was fattening the charges with everything back to the amortization of the invention of the screw propeller!

But of course in addition to these costs of about a million dollars per week, there were the tremendous costs that could not be measured in dollars. The Spanish were concerned about their tourist industry, estimated for the occasion at a billion dollars per year, and the reluctance of tourists to come if they thought that a hydrogen bomb might go off or that the water might be radioactive. Early information released by the U. S. had said nothing about a missing bomb but told only about the clean-up of radioactivity. But everyone knew about it, and the newspapers and magazines had published quite openly reasonably accurate accounts of the conditions of the three bombs that had been found. The fact that public announcements had obviously not been candid of course contributed to the uneasiness, fear, and suspicion. Eventually, on 2 March, a U.S.. press release did say that a bomb was missing' and spoke in detail about the whole operation, but by this time suspicion was so deep and widespread that even the full story was not much help.

By the middle of March the U.S. embarrassment was extreme. It was bad enough to drop flaming and radioactive objects on one's friends, even if no one other than U.S. nationals was hurt. But to lose a hydrogen bomb! What if the Soviet Union found it? The Soviet trawler that had been prowling around the submarine base at Rota had spent two weeks between 16 February and 2 March dodging about the Navy ships, presumably taking photos and having all sorts of fun.

There were, of course, wholly predictable developments of a serious or not-so-serious sort. The villagers, having recovered from their initial fright, eventually became organized and of course demanded paved roads, a central water supply, and telephone service. The Spanish government had asked for and had immediately been granted suspension of all U.S. flights carrying bombs over Spanish soil. The Spanish Minister of Public Affairs and the U.S. Ambassador, Angier Biddle Duke, had accumulated a bunch of press photographers and gone for a much publicized swim in the ocean off Palomares to demonstrate their confidence in the safety of the water. Many people had suggested that the Navy should drop a bomb into the water some night and "discover" it the next day. Art Buchwald, as usual the prime beneficiary of crucial embarrassment, wrote a column in which a group of California hippie surfers, having dredged up the bomb, were demanding a seat with veto on the UN Security Council. And finally, the hit of the week on the Spanish radio was a new song, "La Bomba, Yeh, Yeh."

This, then, was the situation in the middle of March when a systematic stock-taking occurred to decide whether to continue the searches and, if so, how. If not, if the searches were to be abandoned without finding bomb four, what should be left in the area to monitor possible radioactivity and possible Soviet or third-party search attempts? And how should one prepare for the expected Congressional investigation?

Attention now focused more and more on Francesco Simo. He was far from the stereotype of a sleepy, warm-country peasant. He was a highly respected fisherman, owning several boats. Like all the other villagers, he had been helpful to the harassed Americans. He was interrogated by Captain Joe Ramirez, a Spanish-speaking Air Force lawyer who had been selected because he did not know what bomb parachutes would look like. Simo's description was excellent, including two facts that would have been hard for him to fabricate: First, he saw a solid-color chute which is correct for the bomb's chute whereas personnel chutes were red-and-white striped. Second, he sketched a chute with its widest point some distance above its bottom, which is also correct for the bomb's chute. American officers accompanied Simo in his boat to the position about five miles offshore where Simo said the parachute went down Unknown to Simo, the position of the boat was monitored with a Decca navigation system, accurate to a few feet. The experiment was repeated on the next day, and Simo went to the same place within a few hundred feet. He was an impressive man with an impressive performance. Almost solely on the basis of his testimony, the sea search was centered at the point to which he went.

But it was not easy. Although the water was shallow near shore, at the search position it was 2000 feet deep, and the bottom was extremely rugged, with sharp rocky peaks, mud slides, and turbidity currents. It was much too deep for divers, and therefore all exploration was indirect. It was difficult and dangerous for the *Alvin* and the *Aluminaut*, recently developed experimental deep-submergence craft, to work on the ocean bottom. The *U.S. Mizar* was constantly getting its towed sled and sonar gear entangled in rocks, cliffs, and mud slides.

The search and preparations for decision making were dramatically interrupted on the morning of 16 March. The Navy announced a contact that was likely to be the bomb. Then came two and a half weeks of terribly frustrating activity. The bomb was entangled in its parachute and shrouds which in turn were caught in rocks. Twice the bomb was lifted part way, only to have the shrouds by which it was being lifted snap. Once the bomb slid an additional twenty feet down a seventy degree slope and threatened to slide into water a thousand feet deeper. Finally, *Alvin* managed to attach a heavy cable to the bomb, and with much excitement it was pulled on deck. It was unharmed, intact, with no trace of radioactivity or damage. The Navy, having pulled the Air Force's chestnuts out of the fire, made the most of it. A *New York Times* reporter asked Admiral Guest what he would have done if he had not found it; with typical public confidence, he replied that of course he would find the bomb, the Navy would keep trying until it was successful.

I think it is instructive to examine the fields of intellectual activity, what we call "disciplines" in the University, that were involved in the follow-up of this incident. The variety of expertise that had to be brought to bear carries a message about **education**. Any one individual, say an oceanographer or a metallurgist, could contribute a certain amount just from his own specialty. But the ability to understand other people's specialties and to work with them was important even for the lowest-ranking participants. And, of course, those who had to make the decisions had to be at least moderately familiar with **all** the specialties. Differing standards of proof and precision and differing ways of expressing confidence in conclusions had to be factored into basic decisions about where and how long to search and under what circumstances to stop searching. For example, the metallurgist could tell within a few percent the acceleration forces that tore pieces of the bomb bay apart, but how does one assess confidence in the testimony of Francesco Simo and evaluate the possibility that he had overheard Air Force officers talking about parachutes and manufactured testimony that he thought we wanted to hear?

Here are some of the disciplines:

(1) First are the disciplines of **aeronautical** and **mechanical engineering.** The analysis of the structures of the two aircraft, the probable scenario for the accident, the sequence with which various objects became detached, and the significance of the places where each part was found--all of these required a detailed knowledge of materials, modes of failure, and the aerodynamic forces on parts as they were being torn out and falling.

(2) **Metallurgy** entered in understanding the break-up of the B52 and in creating believable scenarios for the history and likely current state of bomb number four.

(3) **Nuclear engineering** was involved in the integrity of the bombs themselves and the specification of the possible hazards.

(4) **Nuclear physics** entered very heavily in connection with the instrumentation used for the search and clean-up operations. The maintenance of field meters became the most serious problem after a couple of weeks.

(5) **Chemistry** became important when one had to consider the possibility that a bomb might have to be left in sea water: What would the likely corrosion history of this bomb be?

(6) **Meteorology** played a large role. The winds at 31,000 feet were reported to be 65 knots by controllers working that area, whereas the crews of the two airplanes were using 110 knots and 130 knots as the winds aloft, determined from their own navigation and readings of drift. The wind at the ground was reported by some to be as high as 30 knots and at right angles to the wind aloft, but was probably much less. The whole question of the wind profile as a function of altitude was clearly of the essence in determining where a parachute-supported object would land.

(7) **Agricultural science**, including soil chemistry and physics, became of considerable importance. Could, for example, the planting of tomatoes be delayed a week or two without damage to the crop? To what depth should one remove soil if the surface was contaminated? If soil was to be brought in, did it have to be sterilized to prevent bringing parasites or diseases, like phylloxera into the vinifera grapes of France.

(8) As another part of agricultural science, bordering on biology, was **animal husbandry.** Pigs were raised in quantities and the pigs' livers concentrated plutonium. Again, the question of safety to consumers of the pigs was involved, and questions of how to adjust diet and and the patterns of slaughter, sale, and consumption became important.

(9) In **agricultural economics,** an important problem was the compensation scale for tomato farmers and the patterns of selling their product. To what extent could time, demonstrations, and even advertising offset the fear of radiation?

(10) Obviously the sub-fields of medicine called **radiation biology** and **radiation toxicology** were of great importance. The screening of the local population and the measurements on the surviving crew called f or basic

understanding of the effects on people of ingesting plutonium. Incidentally, a great deal of the basic work on aerosol transport and ingestion was done in the early days of this discipline when it was invented at the University of Rochester.

(11) **Geology** was a gratuitous player. With all the other complications, it seemed wholly unfair to have to contend with the background of uranium in the tailings of lead mines, but that was the way the country was and therefore one had to understand much of its geology.

(12) **Photographic science** entered in several ways. Photogrammetry was indispensable to make excellent maps. Photo-interpretation of the survey photographs was vital, and, as you know, photo-interpretation is a science in itself. Photography also entered in the undersea explorations that were eventually successful.

(13) The whole broad areas of **law** and **political science** were encountered at several points. There were the rights of the survivors, to be respected in questioning them. There was always the possibility of courts-martial and conversely the possibility of exaggerated claims for damage from radiation exposure. There were the very delicate relations with the villagers, the Spanish people, and the Spanish Government and the questions of the safety of beaches and the survival of the tourist trade. At home there were the relations with Congress and the whole fuzzy questions of the interactions among the logistics operations (which entailed the most cost), the cost, and the eventual Congressional investigation, which everyone expected if bomb number four was not found.

(14) **Psychology** turned out to be a key element of the solution. The visual perception by the fishermen, particularly by Francesco Simo, was especially important. How accurate was his perception of distances under different sea and sky conditions? Quite different perceptions by the villagers would have to be considered if the search had to be abandoned. The wholly practical applied psychology of returning them to their lives of farming and

fishing after a period of upheaval, complicated by substantial indemnity payments, would also have to be developed.

Well, this is quite a list, and it still is not comprehensive. But it seems to me that it highlights the way that even though breadth of education may not be immediately applicable by a young person, breadth of education and of experience comes into its own in a problem like the Palomares incident, and under circumstances where there is no time to go back to develop adequate expertise in each of the fields where it is needed. Breadth of education may not be required in one's first job, where training may be more important, but real education in all its breadth and depth comes into its own in later crunches. You may argue that there are not many crunches as complicated as Palomares, and I would agree, but I know of many in industry and in universities that approach its complexity.

At the beginning of this talk I mentioned this conclusion about education. The second outcome I cited was a change in the Cold War. Palomares made it obvious that it is not good policy to let the health of the Western Alliance depend on whether there is clear-air turbulence at 31,000 feet over Spain. More importantly, the Palomares incident prompted a penetrating analysis of our relations with our allies and a major decrease in our arrogance.

The third outcome I cited was the creation of the field of **ocean engineering.** The success of the first-generation undersea vehicles *Alvin* and *Aluminaut* launched new generations of manned and unmanned devices and systems, such as robotic cable-laying systems. Now, the forbidding undersea environment is being mastered by a wide variety of vehicles and systems, and the field has become promising and respectable for the training and employment of engineers.

I have four final comments. First, in deciding what operations can be mounted and what cannot, it is well to remember the principle known as "Murphy's Law": "Anything bad that can happen, will." Remembering this is good leavening to the decision process, to be included along with probabilistic and statistical models and sophisticated mathematics.

Second, Hollywood's imagination is nothing compared to what a real-life situation can create. Here we have the Air Force and the Navy, with many opportunities for colorful ceremonies and romance. For humor, there are many Shallows and Dogberrys available; if Airman Rooney as an accelerometer is not exactly amusing, Art Buchwald's surfers could be injected. And we have the real hero, Francesco Simo, who saves the Western Alliance and, for all I know, may have married Admiral Guest's daughter.

Third, here, as in most peace-time incidents, there was an effectively infinite force of trucks, jeeps, aircraft, ships, and people, but there was, as always, a **shortage** of **knowledge** and **instrumentation**, the two things one never has enough of.

Finally, if only one of the fishermen had had a camera!

CONFIDENTIAL DRAFT (retyped Jan. 23, 1973)
(talk to Public Affairs Administrative staff Oct. 17, 1972)

University of Rochester - Goals and Aspirations

by Robert L. Sproull

An objective measure of the University's goals is difficult; however, one means of measurement is by comparison with other similar institutions. By the end of the century, Rochester may be somewhere between M.I.T. and Cal Tech (probably closer to Cal Tech because of our size) in the sciences and engineering. We should be as good as or better than Chicago and Harvard in social sciences, as good as or better than Harvard or Stanford in medicine, and possibly where Harvard, Princeton, and Yale are in the humanities. These are moving targets so that in order to achieve these goals -- to be where these institutions will be in 2000 -- we shall need to develop at a rate considerably faster than theirs.

One factor which will determine to a great extent whether we reach these goals, particularly in the humanities, is how libraries will be utilized. At present, Rochester has the largest research library in upstate New York, with about 1,300,000 volumes, but it is a small library, far behind Harvard, for example. Our library is "exploited" by Monroe Community College, R.I.T., and other institutions which do not contribute toward maintaining or expanding this facility. With the refining of technology, particularly in the use of computer systems, the libraries of Harvard, Yale, Princeton, etc., will be accessible to the University of Rochester community, and we shall be able to exploit these facilities. Unless there are major changes in using libraries, we are unlikely to overtake Harvard, Yale, and Princeton in the humanities (except music).

One fact which will always be with us is our small size. We will try harder, work harder, and devote more resources to being successful and famous as a small institution. The plan is that we will not be much larger than we now are in undergraduate areas and only moderately larger in graduate programs, medicine, and new programs. We may add a new law school. The College of

Arts and Science should number 3,600 students in the next 5-10 years and will probably not grow much beyond that point by the end of the century.

The greatest problem we face is that we pay for a quality faculty but are not attracting a comparable quality student body. Our faculty is capable of teaching better students than we are getting. In many areas the faculty is far ahead of the students, although we are already attracting top flight students in some areas such as graduate work in economics, psychology, and most work in medicine and in music. The most pressing short term goal is to get better students for the faculty and facilities we have... to make the University of Rochester better understood and thereby attract the best students.

The greatest cost that we must bear in reaching our goals is in completing facilities. The renovation of the Eastman Quadrangle will cost more than we had planned, and the Eastman School of Music physical plant must be renovated or replaced. The renovation of the "old" Strong Memorial Hospital is not wholly funded, but will be completed. With proper facilities and an excellent faculty, we should be able to attract a better quality of student than is now the case.

A law school is the one additional unit needed to enable us to reach our goals by the end of the century. The law school would resemble the Medical School in that, just as our Medical School was one of the first influenced by the Flexner Report -- that is, based not upon the trade-school approach but upon science -- so the new law school will be based upon the social sciences. The law school will relate law to the social sciences as the Medical School relates medicine to the biological and physical sciences.

We have at our disposal a number of weapons which should aid us in reaching our goals and which set us apart in the public eye.

- more -

A particularly strong weapon is our endowment, endowment management, and our 5% spending policy. Although our endowment management and spending policy have been very successful to date, we have no guarantee that they will continue to be so. Investing may not be as effective over the next thirty years as it has been over the last thirty. We should be able, however, to do at least as well as other institutions. Moreover, our spending policy will allow for inflation; that is, the real purchasing power of the endowment will continue to grow in spite of some use of principal.

Our second weapon is our planning, which is as intensive and realistic as in any other private university. State universities, of course, carry on extensive planning, but this type of planning is externally produced while UR's planning springs from interior imperatives. We are not at the mercy of some governmental body; in addition, we are not heavily dependent upon foundation money. Although our plans must be revised to meet change, we believe strongly in planning as a process.

A final weapon is a strong central administration. At Rochester, we have a total, overall administration made up of administrators who take responsibility. We involve the trustees in this administration by having them take on a measure of responsibility not allocated in most institutions.

We are faced then with the job of taking the tools and aspirations of the University and reaching those goals we desire. To do this we will need new funds and new people, and we must work hard and competitively. We must also be aware of the gravest threat to our self-view, the threat of homogenization. There is external pressure growing in the form of governmental and foundation control. We shall be extremely pressed to preserve the special style of the University of Rochester.

Style is another way of saying "the way we operate," and one of the ways we operate is through a graduate/undergraduate mixture which allows those students with drive and intellectual ability to develop as rapidly as they wish.

We achieve this mixture through a number of strategies. Graduate and undergraduate students all have access to the same faculty; there is no separate faculty for graduate students. Moreover, even the medical faculty is actively involved in the teaching of undergraduates. The <u>university</u> experience -- access to facilities and faculties working at the frontiers of scholarship and research -- is a much more stimulating and "open-ended" experience for the undergraduate than the small-college experience. To be sure, the university experience is not suitable for everyone, and small colleges have their advantages for many, particularly in the first two years. But the able students in many colleges may outstrip faculty members in their junior or senior year and perhaps, in boredom, turn to such things as drugs or drop out completely. At Rochester the undergraduate student receives the opportunities and benefits of a complete university, not a college.

Another facet of our style is our stress on undergraduate education: This is a good place for undergraduates who have the ability to profit from a graduate faculty. Tenure and promotions for the academic ranks are very heavily based upon the individual's undergraduate teaching. We also make much less use of teaching assistants than most other major universities.

As staff members, you should understand the importance of this emphasis upon undergraduate education and be willing to listen to all students. This concern is already seen in the academic ranks where professors are genuinely interested in the intellectual growth of their students. Rochester is, although students may complain, a warm place where both staff and professors should continue trying to develop additional ways for people to progress at their own pace.

Another part of our style is that we are striving toward having fewer rules and thereby avoiding the dangers of homogenization. We have just revised our calendar, making it a 4-4-X type which allows for maximum personal involvement. We have no "core" curriculum. We allow students to cut across traditional discipline lines. We allow students to finish their undergraduate work in as few semesters as they are able rather than tying them to the traditional eight-semester program. In addition we do not charge for extra classes. Our style, then, is to attract the best possible student, provide him with an excellent faculty and facilities, and get out of his way.

Still another component of Rochester's style is that it is a completely integrated university in that each of its professional schools is a part of the total University. For example, the School of Medicine and Dentistry is not a separate entity, but, rather, a vital unit of the University teaching engineers, economists, and chemists as well as doctors. Nor is it simply providing care for the sick in its hospitals and clinics; it is, for example, teaching the economics of health care and community health.

One area of difficulty which we face in reaching our goals is that we will probably never be able to explain to the local community what we do for that community. Our most visible contributions are probably through our professional schools such as the University College of Liberal and Applied Studies, the Graduate School of Management, the College of Engineering and Applied Science, and above all the School of Medicine and Dentistry, which provide educational facilities for the whole community and expertise in solving community problems. However, we must draw a line since our primary function is educational. We should undertake only those projects which interact in a primary and healthy way with our main job of producing new generations of well educated people who go out into the community to work. There is, of course, a great irony in the criticism leveled at Rochester by the local community. We, for example, are a part of and subject to review by the CERC, a committee formed by members of different organizations in the city for reviewing applications for federal and state funding. Such institutions as R.I.T. and MCC do not have their applications reviewed since they are not in the city. The University of Rochester is the only institution of higher learning still in the City of Rochester.

Another important service role which staff people fill is in the forming of a positive relationship between the student and the University. It is important that you provide feedback from the people served -- our students. This is sometimes difficult -- it requires forebearance to serve the young -- but it is our job to serve these young people. We should try to solve students' problems by referring them to the person who can handle their complaints. We must make the students feel that the system works and that it is for their benefit. At times this sort of consideration is very difficult for the staff, for they must be content in a university where faculty has academic freedom, and the staff must put up with their short-comings.

A few final words about the management of the University. (I suggest along these lines that you read the Wynd Committee report.) The University of Rochester is fortunate in two respects: We have administrators who really administer, and we have trustees who accept their roles -- to guarantee that University resources are used for the purpose for which they are given -- and who take a very active part in our affairs.

As far as the management of universities is concerned, we have a reputation of being an authoritarian rather than a communitarian university. These ideas, however, change over the years. If you read some of the recent literature on the subject, such as "End of a Movement" in Change (April, 1972), you will see that the shift has already begun. When an institution becomes complex, as is the University of Rochester, it is difficult for it to become communitarian. The "community" is often its own worse enemy since it tends to become preoccupied with governance; there also tend to be conflicting goals and priorities among communities. At Rochester the concept of community can be attained most effectively through smaller groups, such as our medieval house and drama house, which are not involved in governance. Certainly we must listen to students on educational matters in which they are experts in terms of their experience.

One final word: Although we employ many of the techniques of business in such areas as planning and investment policy, we differ in that the educational process depends not just upon the concerns of the educated but depends wholly upon the active participation of the educated. We are in our job for the benefit of people being educated at all levels, and maintaining their participation; if possible with enthusiasm, is part of our job.

cc Mrs Brown
cc Mr Angle.

Taiwan, Self Indulgence, and Education

Robert L. Sproull
17 October 1989

I realize that there is an unwritten rule in this Club prohibiting travelogues, and I shall honor that. No Kodachromes, no maps, no account of food and drink. I obey the rule out of respect for the traditions of the Club, but also because I know that you all have vast arsenals of armament with which to retaliate.

But I do need to explain a little how I got into the business of this talk. Several years ago I was invited to join a group called the Science and Technology Advisory Group ("STAG," a title one could not use in the U. S.!) which advises the Premier of the Republic of China. STAG is composed of five Americans and one person each from Germany, France, and Italy, and recently two Japanese have been added. It is now eleven years old and has been a major factor in the development of technical industry in Taiwan, in beginning anti-pollution activities, and in modernizing power and communication systems. One of its members was the key figure in first controlling and then eradicating in Taiwan Hepatitis B, a deadly virus to which Asiatics are peculiarly susceptible. The Science-Based Industrial Park in Hsin-Chu was initiated by STAG; it is a smaller copy of Route 128 around Boston or Page Mill Road in Palo Alto, but with interesting overtones that we might get into during the question period.

Many of my friends believed that I should not participate in any help to the Republic of China, that it was a rightist, repressive government under martial law, that the "real Chinese" were the Peoples Republic of China. Anyway, they said, all the ROC made was junk, and exporting that junk denied jobs to Americans. Well, I firmly believe that the U. S. should be as friendly as possible with the PRC, simply because there are so many of them; no one knows just *how* many, but certainly more than one billion and probably more than 1.5 billion; in contrast, there are only 20 million people in the ROC, and the population is growing very slowly, almost not at all. The rather romantic U.S. interest in the PRC that has developed in the last five to seven years, in which well-seasoned travelers just *had* to have been to the mainland, has now been dampened considerably by the events of last May and the killings of 4 June.

Meanwhile, in Taiwan the hold of the Kuo Min Tang, the conservative party of the late Chang Kai Shek which maintains the mythological position that someday they will govern "both Chinas," ~~is rapidly eroding~~. The new President ~~(as of~~ two years ago) was born in Taiwan and is a Cornell Ph.D. in Agricultural Economics. The KMT stalwart who has been Premier for many years resigned three months ago in anticipation of new elections to be held next month. Younger people, born in Taiwan, mostly educated in the U.S., are rapidly rising in power. There *could* be upheavals and repression, but all the signs now point to a relatively rapid transition to a Western style democracy with liberal goals and traditions.

An interesting statistic indicative of stability and the future of political democracy is the following: In Taiwan the average income of the wealthiest 20% of the population is only 4.3 times the average income of the poorest 20%; by contrast with this 4.3 ratio, the ratio in South Korea is 7.9 and in Brazil is 33.

Let me give you a metaphor illustrating the change in Taiwan in the last decade. Mary and I spent two weeks in Taiwan in 1978. While there we had a couple of dinners and excursions with Teh Chang Koo, with whom I roomed for a year when we were students at Cornell. Koo in 1978 ran a small trading company that exported window shades to the U. S. and marketed them in the U.S. Window shades, as you know, are a "commodity" type of product, "low-tech," with a high labor content using semi-skilled or unskilled labor. The low cost of manufacture of this near-junk was its only redeeming feature. When we returned in 1987, Koo was still running his trading firm, but no more window shades (they are now made principally in South Korea and Singapore). Now he is selling investment castings and lost-wax castings of high-tech alloys, including some from the Science-Based Industrial Park.

Now I do not contend that this transition has occurred everywhere; there are still a lot of toys and junk being made in Taiwan. But the anecdote emphasizes how rapidly the transition is occurring and the determination of the ROC to improve the quality and the reputation of its exports. The ROC balance of trade per capita is now a *positive* amount just a little more than the U.S. *negative* balance of trade per capita (about $800 U.S. per year).

This remarkably fast transition has been accomplished with the help of Japanese and Western (mostly U.S.) corporations in joint ventures and by U. S. engineers and U.S.-trained Chinese engineers. But indigenous engineering is developing rapidly and impressively. One of the restrictions on a company's participation in the Science-Based Industrial Park is that its work force must have at least 25% engineers. and the young chinese engineers in this activity are a formidable bunch.

The transition to higher quality, higher technology, higher skilled manufacturing, rapid though it has been, is now accelerating by the return of engineers from abroad. Many extremely talented Taiwanese have been educated in American colleges and universities. Although some returned immediately to Taiwan, many stayed in the U.S. and worked in major U.S. corporations. As Taiwan industry has developed, there are now attractive jobs for engineers, and there are now many expatriates returning from the overseas stockpile. Not only do they have an excellent U.S. education, many to the level of the Ph.D., but they have the even more valuable experience of five or ten years in an American corporation. Taiwan universities, too, are now much stronger, again by help from abroad, and they are turning out first-rate engineers, including to the Ph.D. level.

All this makes for tough competition. The bad news is that while we train more lawyers than engineers, the lawyers (in addition to more useful, even vital, contributions) add to the overhead of American industry. The bad news also continues in that the Taiwanese *family* is still a strong influence, the Taiwanese work much harder and somewhat longer than we do , and their children are not on drugs. The only good news is that there are only 20 million of them.

To some extent, the same competitive edge and threat to the U.S. are shared by the other three of the "Four Tigers of the Pacific": Hong Kong, South Korea, and Singapore. But Hong Kong has an uncertain future, especially questionable after the events in Tienamen Square. South Korea has political and sociological problems which breed instability. And Singapore is small in area and in population. Nevertheless, the four together, led by Taiwan, are formidable competition for the U.S., comparable to, although still somewhat behind, Japan.

How is the U.S. going to compete? In Washington nowadays almost every document has the buzzword "competitiveness" prominently displayed, but speech making, legislation, or import restrictions are not likely to solve anything.

Into the competition we carry two giant handicaps: First, we provide the defense umbrella for Japan and the Four Tigers, as well as for much of Western Europe. Much of our engineering and research and development is drained off from the civilian economy to provide military capability. The taxation to support the defense umbrella adds heavily to the cost of U.S. industry and must be reflected in the selling price of its products, it detracts from the ability to provide new capital for manufacturing, and it siphons off private savings into (as far as the competition is concerned) unproductive routes. Although there is a little "fall-out" from military R & D to the civilian competition, it is nothing like as effective as it would be if the same dollars were applied directly to civilian industrial opportunities.

The second great handicap is that we are the most self-indulgent society of any substantial nation. Perhaps our self-indulgence of lax rules in the work place, of union protection of inefficiency, and of excessive fringe benefits is not as great as Australia or West Germany. But our self-indulgence of consumption is the largest of any major nation. We are a bunch of fat cats in an entertainment-oriented society. We value our individual freedom so highly that the family counts for little, especially in cities--everyone does "his own thing." Similarly the work ethic means less and less; what little work we do will soon be for other countries, as our trade and Federal deficits transfer ownership of our industry to foreigners.

While we relax and enjoy, our competitors in the Asian basin are working hard and saving. Their labor acts directly to increase competitiveness, and their savings permit industry to borrow money or float equity participation at low enough rates that it can afford to look ahead, to invest in R & D, to be relatively protected from the American disease of concentrating on the quarterly reported "bottom line." It is especially painful to witness an American invention or technical development that reaches the point where "scale-up" or pilot plant operation is indicated, only to observe that the Asian cost of capital is only 1/2 or 1/3 of the U.S. cost, and our industry cannot afford to produce the product but Japan or Taiwan can.

It is easy to identify the self-indulgence that the *other* guy ought to forego in order to prevent or ameliorate this impending disaster. *My* list would include not only the 40-hour week (of which perhaps 20 hours are really work), the drug culture, rock-and-roll, and teen-age pregnancies, but also (horrors !) professional sports and two-martini lunches. It will be interesting to learn *your* lists!

But the Detroit shift worker has a different list: *He* values highly his 4000 pound automobile, his UAW wages and benefits, movies of sex and violence, professional sports, and beery weekends. *He* looks on my helping the Taiwanese and to our welcoming Taiwanese students into our colleges and universities as *our* self-indulgence. And he is right!

Yet we have a long and proud tradition of helping, as individual advisors, less fortunate nations; we are proud and happy to be the nation of the Marshall Plan and the Peace Corps. We have a long and proud tradition of opening our colleges and universities to overseas students; we are happy to associate with the impressive array of talent, usually the cream of the crop, other nations send us. Our faculty are delighted to have these able young people in their laboratories, especially since the numbers of American nationals who go on for advanced engineering study are still declining.

Nearly a decade ago, British universities began to charge overseas students much higher tuition than domestic students. Yet this practice is not even being discussed in the U. S. We discount heavily, especially in state-supported institutions, the price an overseas student pays for a U.S. education. The *cost* to U.S. donors and taxpayers to educate our competition is enormous; if education is the key to industrial success, as we claim it is, why do we almost give it away to the people who are beating us in the competition? I hope we do not have to give up this piece of self-indulgence, since it is one of our redeeming qualities as a nation, but as part of adaptation to an intensely competitive world, we may have to do so. After all, if we fail to survive as a reasonably prosperous nation, we cannot continue to provide the highest quality technical education, to say nothing of being in position to be generous if something like a Marshall Plan is needed again.

Adaptation to changing circumstances is the only way to stay prosperous and free. To emphasize this, I note that the Chinese in Taiwan are beginning to give up one of their most cherished and respected traditions, the all-or-nothing sorting of individuals according to performance on *written* examinations, a major feature of Chinese society since Confucius' time. They are adapting, to compete.

To look at the foreign student question from strictly the point of view of economics, let me ask you first what products we shall be able to sell in the next century in world markets that will be the best in the world, that will be such that we can set premium prices and be the market leader? For a time, we could do that with automobiles. Later, we could do it with communication equipment and then computers. All of that position has disappeared. One of the very few areas remaining is selling higher education, especially post-graduate education in science and engineering. From the point of view of economics, does it make sense to *discount* ;the price of one of the very few items in which we are the market leader?

So, in conclusion I leave you this question: How do we as a nation adapt to the competitive challenge? And in particular, is our situation so serious that we should give up our bargain sale of higher education to our competitors?

Dexter Perkins

Robert L. Sproull
October 20, 1992

My "giant" is Dexter Perkins. He had by far the longest tenure (56 years, 1924-1980) of any member of The Club, a record not likely to be challenged soon. Whenever he was host he was also the "reader," although he never actually *read* any of his 48 papers (also a hard-to-beat record). The intellectual treat of a Perkins paper was complemented by excellent dinners provided by Wilma Lord Perkins, as was to be expected since she repeatedly augmented and revised the Fannie Farmer Cookbook, originally conceived and written by Dexter's aunt. Few current members of The Club knew Dexter, and that is my excuse for this admittedly self-indulgent talk tonight, But George, Mac, and Bernard could do better. Indeed, I will append to the text of this talk (for the record) the citation that Bernard read, and I am sure wrote, for an honorary degree for Dexter from the University of Rochester in 1955.

To try to acquaint you with Dexter I start with the bare bones of his career. He was born in 1889 in Boston, with a certified Brahmin ancestry. He suffered the Prince School and enjoyed the famous Boston Latin School before entering Harvard in 1906. He and the Harvard of the early Twentieth Century were made for each other. He continued most happily as a graduate student in history and came under the influence of Archibald Cory Coolidge. Coolidge suggested that Dexter look into the diplomatic correspondence relevant to the Monroe Doctrine. Dexter embarked on that project with great skill and energy, first in Cambridge and then in Paris and elsewhere in Europe. He pursued it with such depth and breadth that it provided the grist for not only a Ph.D. thesis but also four books and many papers and parts of books.

After a year at the University of Cincinnati he was recruited personally by Rush Rhees to come to the University of Rochester. He remained, turning down many offers. He was Chairman of the History Department for 28 years and retired after 39 years and 20 books in 1953 to become the first John L. Senior Professor of American Institutions at Cornell. Along the way he was the first Pitt Professor at Cambridge, the President of the Salzburg Seminar, and President of the American Historical Association. After six years at Cornell he returned to Rochester and continued lecturing under the auspices of the University's evening division, the University College (the History Department in the College of Arts and Sciences ~~was~~ had been unsympathetic; it had been infiltrated by sociology, journalism, and popular causes, and far from having one chairman for 28 years was about to chew up chairmen at the rate of five in six years).

Dexter entered deeply into the community life of Rochester. Of the many services and interactions I will mention only two: He chaired a committee to examine the school system in Rochester and continued this interest for the rest of his life. He was appointed City Historian in 1934 and claims to have gradually turned over (starting in 1936) the responsibility to Blake McKelvey; he wrote "I have no hesitation in saying that, owing to his [McKelvey's] labors, Rochester is most favorably known among students of urban history."

To attempt to give a portrait of Dexter I must start with Dexter as scholar. His twenty-one books were concerned first with the Monroe Doctrine and then with diplomatic history more generally. He wrote extensively on American presidents and the presidency. Foreign policy and the peace movement also became central interests, both in his writings and in his talks to The Club. A good example is his 22 April 1969 talk on the Cuban missile crisis, and we are indebted to Tom Hawks, Bernard Schilling, and Karl Kabelac for preserving a tape of that. In it, among other fascinating insights, he states ". . . we learned that nationalism is a more powerful force than communism" and that ". . . the Russian empire is dissolving." Well worth listening to, twenty years before it became apparent to the rest of us!

From being primarily a scholar: studying and writing, Dexter gradually transited to being primarily a lecturer: studying, writing, and above all *speaking*. He had a high-pitched, almost squeaky, voice and some mannerisms ("Don't you know?") that turned off many listeners until after a few sentences the Perkins charm and brilliance captured them.

In the summer of 1915 he took instruction in elocution from an instructress "with the warmth of a codfish." "Nothing I ever did was more useless. I soon decided that what I gained in diction was more than balanced by what I lost in freedom from self-consciousness." In beginning his professorial career at Cincinnati he had written out and read to his class the first ten lectures, to a ho-hum response from his students. His eleventh, from notes, caused the class to sit up. After that, Dexter rarely wrote out a speech, and when he did (as in his inaugural address as President of the American Historical Association) he referred to the text only obliquely in his presentation. We are, unfortunately, the poorer for this pattern, since no texts of his Club talks exist; but through the work of Tom, Bernard, and Karl Kabelac we do have several tapes and most of Dexter's longhand notes.

Dexter was a consummate lecturer. His scholarship underlying the talk, the crafting of his presentation, his enthusiasm and informality, and his wit more than offset the high-pitched voice. Above all, his audiences enjoyed his lectures because it was so apparent that Dexter enjoyed lecturing. He quotes his younger son (Dexter, Jr.) who reported that his first lecture at Columbia went off "pretty well": "I think I have some of the family ham in me." Dexter admired Carl Becker, the great scholar of modern European history at Cornell, but he considered him "not a conspicuously good lecturer." As an undergraduate at Cornell who had read some of Becker's books, I turned elaborate handstands with my schedule in order to attend Becker's lectures, and I thought them superb. But I know what Dexter meant: Becker was a *formal* lecturer, reading from beautifully constructed text, a strong contrast with Dexter's style. Dexter made a similar point in writing of how he "diverged" from Arthur Schlesinger, Sr.: "He had a tremendous faith in the power of the written word . . . I, on the contrary, feel that direct contact with people, and the wisdom that ought to emanate from history in the classroom, is of the highest importance."

Dexter was, in my view, a deeply religious man, but in what most would view as an unconventional fashion. By ancestry, upbringing, geography, and analysis he was a Unitarian, but just barely. When the prominent Unitarian William Channing Gannett asked him what he believed in, he replied: "I believe in the dignity of human effort." Nevertheless, in 1952 he became Moderator of the Unitarian Churches of the United States and Canada, a post Bernard called "the Unitarian papacy." Typically, Dexter wrote that "Presiding over the Unitarians was great fun."

Dexter had enormous respect, approaching reverence, for the university as an institution. He repeatedly expressed his gratitude for the setting that the institution, and even specifically the University of Rochester, provided for a scholar and teacher. He was impatient with those who did not take full advantage of the setting: "History professors at universities of repute have a lot of time to devote to literary pursuits--long vacations, sabbatic leaves, and special grants from foundations. Under such circumstances, the wonder is that so many of them produce so little."

He was also a student of and commentator on university leadership and specifically presidents of the University of Rochester. He was distinctly not a fan of Martin Brewer Anderson, the first president of the University and one of the founders of The Club. Anderson offended Dexter's scholarly, skeptical sense: "He was more confident of everything than I am of anything ." (1500th Club meeting). Dexter *was* a great fan of Rush Rhees, a member of both The Club and Fortnightly. His warm appreciation of Rhees shines out in his paper at the 1500th meeting of the Club, more than half of which is devoted to Rhees. [I share Dexter's view, and in speeches I have said that even if the University of Rochester lasts 1000 years, it will have had only *one* president, Rush Rhees.] Dexter described Rhees as "a conservative" and added: "I have never been able, in contradistinction to some of my academic friends, to see why this is a sin in a college president.

I am glad that Dexter is spared viewing the contemporary National university scene, but I would dearly love to hear a Perkins lecture on it. I think he would applaud the steady increase in quality and concern for students, but it would be a memorable occasion to listen to his outrage at what has come to be called political correctness. I can just hear him castigating moral relativism and cultural egalitarianism!

Finally, there is *politics*, and without discussing politics any portrait of Dexter would be grossly lacking. He was a lifelong Democrat, but a *conservative* Democrat. He believed Theodore Roosevelt was a great president but had only moderate enthusiasm for John F. Kennedy. He voted for Herbert Hoover in 1928 but not in 1932. His views of other presidents were incisive and insightful. His analyses of the intersections of American foreign policy with domestic politics deserve close attention. A good example is his talk on the Cuban missile crisis, at the 1494th Club meeting on 22 April 1969.

Dexter wrote It was not easy to be a Democrat in Rochester. I remember the first meetings of The Club I attended after joining in 1969. I was usually late, since I had become almost from the day I arrived in 1968 the chief operations officer of the

University, and we all spent an inordinate amount of time keeping the peace, maintaining an academic setting when groups wished to capture the University and enlist it in their causes. As I would enter the host's house, Dexter would invariably be at the far corner of the living room. He knew from our association at Cornell that I was a fellow Democrat, and each evening he had been waiting in a room full of Republicans for a kindred spirit. He would blurt out in his loud, squeaky voice something like: "Bob, have you seen what those idiots have done now?" The "idiots" were, of course, the Republicans in Washington, and I perhaps should have been more embarrassed than I was. But I reflected that I was no radical, and the *average* of Allen Wallis and me was still well to the right of center.!

Dexter's political positions always developed from analysis. You may have disagreed with them, and he would still have admired you if yours, too, proceeded from analysis, but you could not successfully accuse him of having his positions drop out automatically from strong bias. If he had a bias, it was toward action and strong measures: "There are too many pallid personalities in this world." In the conclusion of his book *The American Way* he states the core of his political philosophy; he suggests that the fundamental strengths of the American way are "the pragmatic spirit, the libertarian spirit--that is, the unwillingness to submit to centralized authority in both the political and economic spheres--and finally and supremely important, the faith in education.

Dexter's last days were far from happy. Both he and Wilma had rooms in the Brightonian Nursing Home on Elmwood Avenue. I visited them there many times, and for a while I looked forward to the discussions. But then Wilma died, which was not according to Dexter's game plan, and then Dexter's eyesight deteriorated to the point that he could no longer read the large-print edition of the *New York Times*. He was still interested in events in the country and in the world, but the other residents of the Brightonian were interested only in what was going to be the menu for dinner and the indignities they had suffered from the nurses.

But Dexter remained an optimist to the end. I close this talk with the final paragraph from his autobiography *Yield of the Years*, the source of almost all of the quotations I have used tonight:

> Do I end in pessimism? In despair? I do not. In youth one finds simple answers to complex questions. In old age one knows better. In youth one trusts; in old age one knows that power, no less than good will, that emotion no less than intelligence, rules the destinies of nations. But I repeat the phrase which came to my lips forty years ago--I believe in the dignity of human effort. I believe that for each of us there is a way to serve. The results for each of us will be modest. But the mass effect of wisdom and good will is

significant in human affairs. If it cannot redeem society, and bring about Utopia, it can do something to make the world better. The effort must be made.[11]

THE INTERDISCIPLINARY MATERIALS LABORATORIES - AN APPRECIATION

ADDRESS BY

DR. R. L. SPROULL, DIRECTOR, ARPA
Department of Defense

BEFORE THE MEETING OF

THE PITTSBURGH PHYSICAL SOCIETY

8 APRIL 1965

8:30 pm

PITTSBURGH, PENNSYLVANIA

THE INTERDISCIPLINARY MATERIALS LABORATORIES - AN APPRECIATION

Introductory Remarks

It has been nearly five years since the first of the interdisciplinary materials laboratories ("IDL's") was created. It has been three years since the program was completely established in the form it exhibits today. On the other hand, most of these laboratories have had to construct buildings before the anticipated connections between disciplines could flourish, and the clock runs slowly when degrees, tenured appointments, deans, and such academic paraphernalia are involved. Thus it is still too early for an evaluation of the program, an answer to the key question of stewardship: Is public money better spent on this than on some other conceivable program with similar aims?

Thus the subtitle of this talk promises something short of an evaluation. It also removes the last possibility of surprise (not much of a possibility in any case) since it suggests that I think the program is worthwhile.

I nevertheless approach this audience (the Pittsburgh Physical Society) with some diffidence. The Pittsburgh area has distinguished itself without benefit of any such program.

Physicists have been skeptical from the first - many of them (of us) have invented varieties of boojums to be afraid of. (I know one otherwise reasonable physicist who refused to have any of his graduate students supported by a program with the name "materials" in it; "materials" does have a grubby ring to it.)

As a final remark in this introduction, though this program is frequently referred to as an "ARPA program," AEC and NASA participate, too. About one-fourth of the program thus far has been supported by AEC, and a smaller part by NASA.

Origins

There was no single origin of this program. I know of three independent impetuses, and there probably were others.

One powerful stimulus was certainly the AEC and particularly John Von Neuman. I never heard him speak on this subject, but the reports that others gave sound so characteristically like him that they must be completely accurate. He was simply fed up with hearing over and over again that he couldn't do what he wanted to do because of the limitations of materials. Certainly the integrity of reactor fuel elements at high temperatures in contact with such uncooperative neighbors as alkali metals or molten fluorides was one of the critical problems, and one can easily list many others. When told that only slow progress was being made on such materials-limited areas because of lack of people, he is reported to have drawn the obvious conclusion. But he did more: He promoted a program, the IDL's to entice more and abler people into materials research.

Another origin was a National Academy of Sciences study for the Air Force in 1957-58. This group for a while promoted the idea of a national materials laboratory, to make possible materials research and development with a focus on technology but without being tied to specific end-items. Nothing came of this enterprise. It was explained to me that there were "only" three kinds of people opposed to it: People from industry, people from universities, and people from government! Each group felt that the proposed laboratory constituted a threat, largely because of the competition for people.

Another origin was a panel on solid state sponsored by the ONR. This panel documented the leveling-off of Federal support of solid-state science in the period 1956-57. It noted that development funds were still growing but that basic science and engineering support in solids was not keeping pace, a conclusion that became even more demonstrable in the aftereffects of the first artificial earth satellite in October, 1957. Again, the shortage of both applicable science and fundamentally trained people who might participate in applied work was noted.

Two threads ran through these origins: 1) There simply were not enough good people being trained in materials; the cause was identified as partly lack of support and partly the attraction of competitors such as big-machine physics. 2) There was a need for additional people who had a broader outlook than traditional for a science or engineering Ph.D., a need for metallurgists who understood quantum mechanics and theoretical physicists who were excited about grain boundaries.

All of these impetuses came together in action by the Federal Council on Science and Technology in the spring of 1959. The Federal Council endorsed the need for additional university research and graduate training in materials, accepted the recommendation of its Materials Committee that interdisciplinary laboratories be established at universities, and recommended that Federal agencies implement a program of dimensions such that a 75% increase in Ph.D's each year in materials (defined broadly) would ensue. The Department of Defense undertook the major role and assigned it to ARPA. The AEC and NASA came in later with smaller programs.

Concurrently with these activities, teams had been visiting universities to learn what kinds of and how much support would be required to produce the desired results. There was a remarkable unanimity among universities that they would need support for buildings, support for substantial infusions of research equipment, and multi-year funding. With these aids, the universities could take the risks of expanding their academic staffs faster and of filling the pipeline with additional graduate students.

The competition was intense; 45 universities submitted proposals. The unsuccessful competitors have not been shy about their unsuccess. Several points should be noted in defense of the selection of the 12 ARPA-supported universities (and I believe the same considerations apply to the others): 1) Selection was not a prize for past performance but was acknowledgement that the selected institution was especially promising as a place to build for the future. 2) Universities that had already taken a serious commitment toward expanding and generalizing materials research and training competed favorably. 3) Institutions that demonstrated a willingness to experiment, to bestir themselves from the comfortable seat of tradition were favored. 4) Finally, as in any such selection, luck played a large role, since in some cases the transfer of a single leading professor or the happenstance of existing building plans at just the right time could make the deciding difference between close competitors.

Institutions that were not selected were obviously uneasy, but several have managed to have some of the fruits of the program anyway. Leaders among professorial groups at several institutions have used the failure to get an IDL and the anticipated competition with IDL's for staff and students to pry increased support from their administrations. Other institutions, perhaps not so seriously interested in materials, have chosen to apply their limited resources in other directions (e.g., molecular biology). Both responses are appropriate.

The Program

As we use the term, "materials research" means essentially all of solid-state physics and physical metallurgy and much of chemistry and process metallurgy. Some biology, binding-energy-range nuclear physics, mathematics, atomic and molecular physics, and other disciplines also enter on appropriate occasions. I have already alluded to the grubby sound of the word. But "materials" has an advantage in its vagueness. Unlike "metallurgy" or "ceramics", titles of disciplines that are rather severely restrictive, "materials" (like "physics") invites expansion of horizons. The field includes any activity that is conceivably useful in extending and deepening the understanding of solids and liquids and in exploiting that understanding.

I shall discuss the program under three headings: Expansion of the field, special features of support, and the experimental aspects of interaction between disciplines. I shall speak only of the ARPA part, but I believe my remarks would apply as well to the AEC and NASA parts of the program.

A Expansion

The desired expansion of graduate research and training in materials is definitely occurring. It is difficult to give meaningful statistics because of the multiplicity of support of graduate students, the "pipeline capacitance," and the question of what the expansion would have been in the absence of the IDL contracts. One piece of information is the growth in numbers of graduate students in materials at the IDL institutions from about 1100 to 1900 in the 3-5 years of the program. The rate of output of students with advanced degrees has risen sharply but not yet doubled, because of course this output cannot grow as fast as the input or the total number in graduate study and because in many schools new construction was required before more experimental thesis work could be undertaken.

There was a great deal of concern at the beginning of the program that its first effect would be a substantial decrease in the numbers of people doing materials work in industry and government, because presumably many such people would be pirated away to university positions. There were a few such transfers, but the build-up at all schools was deliberate, and the program was never a net consumer of people. The clamor seems to have died down.

Clearly much of this expansion could have been accomplished by other ways. But it would certainly have required Federal support. Most of it would have required Federal support for increased research space, a difficult activity under ordinary contracts. In this connection, it is worth noting that as soon as a school knows its space problem will be solved in a technical area like materials, it can begin accepting larger numbers of graduate students to "charge the pipeline." Thus some progress was made even before the contracts were signed.

B Special Features of Support

I have already noted that at the beginning of this program, teams from several Federal agencies learned by visiting universities what they would need to make a substantial expansion along lines planned by the Federal Council. Those that needed buildings were asked to share the cost of the buildings and to expect that the Federal share would come to them, not as an immediate lump sum, but only at the rate of 10% per year after a building was occupied and as long as the contract remained in force. Clearly considerable risk-taking was required of the universities. On the other hand, one must presume that a university would not have applied unless it was seriously interested in expanding in this particular direction "anyway."

Some of the buildings have been occupied; most are now just being occupied. There have been the usual vicissitudes characteristic of campus building programs, of committing the university share of funds (frequently obtained from a donor), and of appeasing neighboring and rival academic departments. But no problems of principle and no unusual difficulties have arisen.

The 4-year contracts are renewed every year by adding an additional year, so that 3-4 year funding is always in hand.

The advantages to the universities of this longer-term funding are obvious, and much of their willingness to take the risks involved comes from this feature. It makes problems for the sponsoring agency, however. Unless the program is continuously and obviously productive and is energetically interpreted (and defended if necessary), the long-term funding could easily be whittled away when other programs seem more urgent.

The substantial additions of capital equipment have made a spectacular difference in the quality of graduate work. I can remember some years ago putting together support from five different non-Federal sources to buy a $14,000 spectrophotometer. No one group could afford it alone, yet when it was installed it revolutionized the research of half a dozen theses within the first few weeks. This instrument became the core of a central facility and was used night and day.

The IDL contracts have permitted and encouraged this type of equipping on a much larger scale. Individual research contracts would have to be much larger than they commonly are to afford an electron microscope or a mass spectrometer. Furthermore the tendency in such contracts is always toward supporting one additional graduate student, with attendant deterioration of the graduate training because of inability to buy modern equipment. Pooling needs and resources in the larger quantum of the IDL contracts gives effective relief, provided the university leaders of the programs have the courage to insist on a proper balance between operating expenses and equipment.

The presence of such buildings and equipment permits competing for the ablest students on more nearly equal terms with high-energy physics, a study which, if it is present at all, has the newest buildings and the most modern equipment at most universities.

The large-quantum and long-lived (4-year) contracts also permit equipping and supporting the new young assistant professor just added to the staff. Without such support, the new staff member typically had to get by on completely inadequate university support until he could establish himself as an independent investigator. With such support, the new staff member is immediately productive, at an especially productive time of his life.

The large quantum also permits the established staff member to make a substantial change in research direction, to turn a sharp corner if his imagination leads him into a new field. Again, this can be done without an enthusiasm-sapping induction period.

Visiting staff can be readily supported under the large contract, but can be supported only with great foresight or good luck on the typical small contract. On many occasions the money can be "used twice": A visiting distinguished research worker can be invited for a few months or a year because the large contract is available as a source of support long in advance. Frequently other sources of support, such as industrial or overseas fellowships, can be found before the visitor actually arises, and the contract money - though essential for the invitation - does not need to be used.

Despite all these advantages of the large "umbrella" contract, I believe there would be serious disadvantages if this were the only source of research support available to the scientist or engineer at an IDL. The individual contract, though small, gives the individual a degree of independence that makes his participation in the large contract more palatable. It surprised some of us to learn that many professors accustomed to the restrained Federal contracting (in the "ONR tradition") were much more worried about control by an organization on their campus than about control by Washington! The core of support by the large contract plus the individual contracts seems to be a happy combination. Incidentally, it means that half the credit for the success - I firmly believe it is "success" - of an IDL goes to the Federal agencies that support these individual contracts.

C Interdisciplinary Aspects

To me the most interesting and promising part of the program is its experimental approach to bringing the traditional disciplines together. This aspect is also the slowest to develop and the hardest to evaluate.

I have to admit that there is nothing intrinsically new in this idea. For example, Bell Telephone Laboratories' management brought physics, chemistry, and metallurgy together with resounding success in semiconductor science and technology. But the successes of universities - and there are many, of course, - have generally had a different flavor; they have almost invariably been individual efforts or efforts of a team within a single department. [Even the big-machine physics, with an insatiable appetite for engineering support, nearly always buys its engineering off-campus.] The academic department is the strong unit, and interdepartmental barriers are massive. Party this isolation is because management of the industrial laboratory sort is anathema to most professors, partly it is a matter of tradition and patterns of what research goes with what teaching, partly it is a matter of pecking order, and partly it is the advantages that go with a small and totally independent research group.

But the disadvantages are profound. Much solid-state physics is done on needlessly impure or imperfectly chemically analyzed specimens. Many investigations of the physical properties of newly discovered chemical compounds are limited to the unsophisticated approaches afforded by the apparatus and knowledge available in a single room of a chemical laboratory. Both physicists and chemists frequently fail to use the knowledge of the distribution of chemical and physical imperfections that is common knowledge to metallurgists.

The basic idea of the IDL's is to make it possible and attractive to bring sophisticated approaches from all these - and even other - disciplines to bear when appropriate. By no means every thesis calls for serious crossing of traditional boundaries. Furthermore management - either local or Washington - would produce an unproductive and perhaps violent reaction if attempts were made to force unnatural association or cooperation.

The most prevalent and successful techniques of promoting this larger, interdisciplinary view at the IDL's are co-location of workers from various disciplines and central technical facilities.

If physicists and chemists are next door to one another in the same building, borrow each other's equipment, and have coffee together, there is a strong presumption that they will be useful to one another in their research. Even if the professors aren't, the graduate students will be. There is no danger that the physicist (for example) will lose contact with other physicists; his departmental ties inevitably remain strong because his salary, his promotion, and his teaching menu flow from the department. Thus physical proximity is exploited to reach out into adjacent fields while organizational proximity maintains depth in a man's "own" field.

The central technical facilities can be a marvelously effective mixing ground, especially for thesis students. An X-ray laboratory, for example, draws students from all materials disciplines, since X-ray investigations are almost always helpful at some stage in an experimental thesis in inorganic chemistry of solids, solid-state physics, or physical metallurgy. Facilities such as crystal growing, analytical chemistry, irradiation, electron microscopy, high magnetic fields, high temperatures, low temperatures, and computing are also of wide applicability. Not the least of the contributions of such facilities is the chance chemists and metallurgists are given to make electronics their slave rather than their master.

I know of many situations where this interdisciplinary technique is already working out in practice at IDL's. For example, an analytical chemist and an experimental physicist consulted at every stage (including the beginning!) on the preparation and analysis of a highly purified crystal. The need was expressed by the physicist, and very interesting indeed experiments could be and were performed once the crystal was prepared. But there were important consequences for analytical chemistry as well, since the physical tests for perfection proved out the new and ultra-sensitive chemical analytical techniques. I know of another example in a related field where the metallurgical concept of the distribution of impurities between grain boundaries and solution in the single crystal grains was the essential ingredient added to a piece of solid-state thesis research.

Other techniques are used to promote interdisciplinary cooperation. Joint seminars and curriculum adjustments are common. Many metallurgists are learning that Hume-Rothery quantum mechanics is just the beginning (even though an interesting beginning) of the quantum mechanics that can be useful to them. Many physicists have come to terms with the intricacies of solidification in multi-component systems.

Of course any of these developments could have been done without the IDL contracts, and many schools have been doing some of them for years. But it would be very hard to do all or even a major fraction of them without this impetus and support.

What we in ARPA ask of the IDL's is that they energetically experiment with interdisciplinary techniques. We do not require any particular format or any fraction of students that must be in some sense crossers of traditional boundaries. What is possible, promising, and natural for one school may be catastropic at another. Both we and the institution would have failed, however, if the latter takes the contract money and simply divides it among its professors without any innovation or experimentation. The acceptance of an IDL contract implies a concern on the part of the university for materials research and training and a willingness to try new approaches appropriate to that university. Not all of the people or all of the contract dollars need be involved in this experimentation. But some should be, and there should be a strategy of approach and a rationale of how much of the effort can go into interdisciplinary experimentation.

How is this working out in practice? Co-location and creation of central facilities have to wait in most cases upon the occupation of new buildings. Nevertheless, the ideal I have described is already being approached at most of the IDL institutions. There was usually a small group on each campus who "really had the message" and who were moving toward the goals outlined before the contracts started. There is ample evidence of the growth of the scope and effectiveness of this spirit under the contracts. The contracts merely put additional tools into the hands of those already sympathetic to these ideas.

In closing this section I should like to digress from materials to discuss two examples of the interdisciplinary approach <u>outside</u> the field of materials. Perhaps these examples from (I hope) noncontroversial areas will illustrate the power and promise of cross-disciplinary approaches. E. A. Hylleraas has speculated* that <u>if</u> the mathematicians of the 1880's and 1890's had been more interested in the physical world, they would have applied the tools of Bessel, Hermite, and Laguerre to <u>infinite</u> vibration problems (instead of vibrations of plates and the like.) Further, they would have transformed sets of infinite numbers of eigenvalues in different ways and thus would have presented spectroscopists with mathematical systems suited for the classification of spectral lines. A notable acceleration of understanding of atomic physics could have resulted.

*<u>Reviews of Modern Physics</u> <u>35,</u> 421-431 (1963)

The second example is the relatively recent application of wind-tunnel instrumentation to the dynamics of automobile tires. It is hard to believe, but true, that only within the last dozen years have studies of tire dynamics been undertaken with the 6-component (3 forces, 3 torques) instrumentation used for several decades in aeronautical engineering. Once the boundary between fields was crossed, progress in tire design was impressive.

Looking from the outside at examples such as these, one is perhaps entitled to a slight feeling of impatience that the people in the field have what appears to be such a narrow outlook. Do we in, for example, solid-state physics look similarly narrow to outsiders?

The IDL and the Graduate Student

There was some apprehension at the beginning of this program that "interdisciplinary" meant "undisciplinary". The fear was that the depth in research competence in a single field would be lost in the attempt to broaden some students' training. Personally, I did not share this worry since I was certain that the power of the academic departments would remain unimpaired (it has survived in the face of far greater disturbances!).

Furthermore, in the last analysis, the "thesis advisor" or "major professor" (or chief pilot of a student's academic career by any other name) is the man who sets the extent of depth for the degree. I could not and cannot imagine that the professor in that position would change his standards appreciably in response to any external stimuli.

In practice at the IDL's, a Ph.D. is still a Ph.D. in chemistry, or physics, or some other discipline. The depth is no less than before. It is true that there are courses with general sounding names such as "Materials Science," and at some schools a major subject for the Ph.D. can have that name. But in fact such a "Materials Science" major typically has at least as much depth as the subject it most frequently competes with ("Metallurgy") but is often not as restricted in outlook.

What has happened is that a Ph.D. in, for example solid-state physics, can have just as much depth and much more breadth. He can in his research have actual experience - not just exposure - in two or three neighboring disciplines. This is possible without extending his already extensive stay in graduate school because of the modern facilities and technician assistance. In his course work in the more traditional courses, it is probably done at some sacrifice to detail, detail that can rapidly become out of date not because it is wrong but because it is irrelevant.

Graduate students in these programs have many of the advantages of students in the big-machine programs: modern equipment, appropriate space in usually modern buildings, and technicians' assistance. Yet they avoid the managed research, low entropy, logistical planning characteristic of so much of the work of the "Indians" subject to big-machine scheduling. The research in the materials programs still moves in the direction the individual professor or individual student sets according to his own imagination and discovery.

A corollary of this degree of independence is that the IDL universities are not being counted on to solve directly the nation's urgent materials problems. Of course, everyone expects that they will increase understanding of materials, develop new approaches to understanding, and from time to time develop new materials. There are two ways of getting over a mountain, both useful: Build a road or invent an airplane. We must always place some of our bets on approaches contiguous to current technology (road building) and some on approaches remote from current technology (airplane inventing). The IDL's should be more nearly in the second category, but with the additional task of providing a training ground for imaginative road builders.

Even though we are not counting on the IDL's to solve Defense materials problems, we are trying to expose the IDL's to some of the more challenging of these problems. Direct interest in working on actual problems, consultantships, and other connections occasionally develop from such exposure. We have sponsored extended visits at the IDL's by young engineers from Defense laboratories. We hold an annual meeting of IDL directors and representatives at a DoD laboratory (one will be at Natick next month).

Speaking personally again, I hope that there will be two features of many of the students turned out with advanced degrees, in addition to the increased breadth without sacrifice in depth. First, I would hope that they would have learned more of the "permanently pregnant" subjects such as probability theory and quantum mechanics. Such subjects will enhance their capabilities for the rest of their lives, regardless of the kind of technological changes that are causing such problems for experienced engineers. (Compare the articles by Carl Frey of the Engineers Joint Council and the demands for "retreading" courses.) Second, I would hope that many of the students would (in Harvey Brooks' words) acquire a respect for applied problems. They need not work on applied problems while in graduate school to do this. But if the tone set by their professors is warm and friendly to such problems and if they are exposed to intellectually intriguing examples of them, they may lead more productive careers.

Concluding Remarks

This, then, is my "appreciation" of the IDL's. You will note that I look at them in large part through the graduate student's eyes. It is these students in their later careers on whom we really rely to solve materials problems. To the extent that better students are attracted and get better training, the Nation's materials problems are in better hands.

I should like to remind you that I think it is still too early for a proper evaluation of this program. But in a sense others are already evaluating the approach. It is interesting to note the extent to which elements of the IDL activity are being copied. Universities such as Lehigh and McMaster are attempting almost literal copies. In England, the Materials Science Club has been formed to promote just the kind of interdisciplinary approach we have been discussing.

Finally, I should like to note that at each of the sixteen IDL's there is a local director and at least a few like-minded supporters of the ideals of the program. These are the people who are responsible for the success of the program. It is their imagination and energy that make it go. One of the reasons I am confident in the continued success of the program is that these able people are determined that the public funds spent in this way will be well spent.

HOUSE ARMED SERVICES COMMITTEE

July 8, 1987

Robert L. Sproull

University of Rochester

I appreciate the opportunity to appear before you today to discuss the future of ballistic missile defense. I applaud the Committee's interest in probing deeply, beyond the year-by-year decisions on offensive forces and the Strategic Defense Initiative, into the future security of the United States and the character of the world our children will inherit.

I appear before you today as a private citizen, even though (for reasons that will soon become apparent) I work as hard as I can for the Strategic Defense Initiative Organization. Although I have thought about the questions we shall discuss today since 1945 and worked hard on them in the nineteen sixties, I do not claim the scholarly base of the scores of thoughtful writers and analysts who are currently spending full time on them.

We have become so accustomed to the balance of terror that most of us hardly think of it at all. The threat of massive retaliation against a nuclear attack (as we see it) or the ability to fight and win a nuclear war (as the Soviet Union appears to see it) has existed now for a quarter century after the Soviet Union acquired substantial parity, and none of the stimuli to conflict (such as Cuba, Afghanistan, or various Middle East crises) has been serious enough for either side to contemplate seriously nuclear war. I, and many others, had no confidence in 1945 that nuclear weapons would not be used for at least 42 years. I was wrong in my predictions; the first requirement on anyone who wishes to engage in science is the acknowledgement of the capacity for error, as you may wish to remind me as we proceed today. The variety of stimuli to conflict will surely increase as time goes on, and I have no confidence that deterrence of a nuclear exchange can be maintained. Deterrence is after all a state of mind, and a poorly understood state of mind. If it fails, two arsenals each with tens of thousands of hydrogen-bomb warheads are arrayed against each other, and no quantitative analysis is required to conclude that the consequences are completely intolerable from any sensible point of view.

If you believe that the balance of terror will continue to prevent nuclear war, if you believe that the problems created by the growing Soviet Union air defense and ballistic missile defense can be effectively solved, and if you are willing to pay the cost, then we can continue as we are without the complications of ballistic missile defense. But, as I have said, I believe the stability produced by massive offensive forces is fragile and precarious, and the cost, which is difficult to aggregate since it is divided into many lines of many budgets, is an increasingly heavy burden. The burden is becoming especially onerous as Japan (and to lesser extent West Germany) can put more of its resources into civilian research and development because the U.S. provides the "nuclear umbrella," and the prices of U.S. products in world markets must reflect the cost of taxation for maintaining the umbrella.

WHAT KIND OF WORLD DO WE WANT?

Clearly we would like a world in which nuclear weapons were impossible. Just as clearly, we cannot have such a world. Physics cannot give any fundamental reason (and I doubt if philosophy or religion can) why a nuclear chain reaction is possible; the world *might* have been constructed such that each nuclear fission produced *fewer* than one neutron (instead of more than two), and in that case the fission bomb and the fission "match" to ignite the fusion bomb would have been impossible. But in the world as it is, nuclear weapons are either here to stay, or if they are somehow obliterated, the capacity to make new ones is here to stay, and that capacity is now widely distributed around the world.

There is a good deal of literature (see A. M. Weinberg, Strategic Defenses and Arms Control, Paragon House, November 1987, and many others) that demonstrates that a nuclearly disarmed world would be a dangerous world, not just for the "superpowers" but for everyone. Many countries would have the capability to go from a standing start to (say) ten hydrogen bombs rapidly and in better secrecy than the U.S. can maintain. If our national laboratories and weapons factories had stood down, cheating by another superpower or by any of n countries would imperil the whole world.

WHAT BETTER WORLD DO WE HAVE SOME CHANCE OF ATTAINING?

I submit that a good world would be a defense-dominant world characterized by three features: 1) Each of the two superpowers limited by treaty to 500 nuclear warheads. 2) Each superpower (and other countries if they wished and could afford it) fielding a defense in which an effective ballistic missile defense complemented defenses against aircraft, cruise missiles, ships, and submarines.

3) Protection of warning and surveillance satellites, whether as part of the BMD or by treaty.

Let me discuss briefly the first and third of these, and then the rest of my testimony will be concerned with the second.

Why do I suggest "500 warheads"? Of course there is no magic in that number, but it seems to me that limiting the major power arsenals to numbers of the order of 300 to 1000 has enormous advantages. These numbers are small enough that an effective defense (including all delivery modes except the clandestine "suitcase bomb") will be attainable at affordable cost in a generation. It is possible that a ballistic missile defense against 20,000 warheads could be developed and its deployment financially supported sometime in the first half of the 21st Century, but surely an effective defense would be possible decades earlier against 500. Of course a barrage of 200 or 300 ballistic missile warheads can be expected to be accompanied (by the end of this century) by thousands of other objects (booster fragments, bus parts, chaff, and decoys), and it is this ensemble that the defense must cope with; some of the most impressive accomplishments of the SDI program are the optical and radar science and engineering developments for discrimination, but discrimination and battle management against hundreds of thousands or millions of objects will take far more time and money. The warhead numbers are small enough that n-th countries should be much less apprehensive; the fallout from a barrage of (say) 200 nuclear warheads from such an arsenal would be serious, but the world would not be held hostage to the devastating effects of 10,000, nor would we have to be regarded as an arrogant, "macho" superpower insensitive to the needs and wishes of small countries.

These numbers are <u>large</u> enough to discourage n-th country adventurism or cheating by the superpowers. There is a substantial and growing literature that convinces me that a two-power dominated world can be stable, whereas a world of n powers with comparable strengths is unstable. If each superpower retained 500 warheads, any other country that set about to have a strength of (say) 100 nuclear warheads and related delivery vehicles would surely be observed in time to be effectively discouraged or prevented from reaching this objective. Cheating or treaty breakout by a superpower might develop somewhat more rapidly, depending on the way the treaties spoke to production capacity and verification, but making a substantial change in a 500-warhead arsenal and delivery apparatus would be observable in time to enhance deterrence.

What about the United Kingdom, Canada, Australia, and France? Our English-speaking allies would certainly be allowed a few hundred warheads if they chose, but after a decade or two they would probably question the usefulness, especially if they too had substantial defenses. Our semi-ally France would doubtless wish to keep her nuclear arsenal. As a result, the Soviet Union might have a somewhat "thicker" deployment of active defense components, but that would pose no threat to us. A greater danger is that France might sell weapons and delivery vehicles to n-th countries; all countries, including France, should have their production capacities and inventories limited by treaty in such a way that no country could achieve a rapid build-up of several hundred warheads.

Clearly it will be very difficult and take considerable time to secure the required treaties. The optimism of fifteen years ago that treaties could quickly and substantially reduce the inventories of warheads has largely dissipated, but there are encouraging signs again. The Soviet Union has traditionally been more defense-oriented than the U.S. Their massive air defense system and substantial (and partially illegal) ballistic missile defense system attest to their continued defense interest.

I believe we still use the euphemism "national technical means" in treaties to refer to photographic satellites, at least in large part so that Soviet leaders do not have to admit to their defense-oriented people that they allow U.S. satellites to fly over their country. Approaching arms control negotiations with the goal of a defense-dominant world might well be more acceptable to Soviet negotiators and has a real prospect of developing a popular consensus behind it in the U.S.

The Congress might believe that it should tie the rate of the authorized program and appropriations for ballistic missile defense tightly to progress in arms control treaties. Such a tie is superficially appealing, but I believe it would be a mistake. There is a principle in negotiations that the side that cares the most loses. Making the orderly development of the U.S. R&D program contingent on specific achievements in arms reduction would give too much of an advantage to the S.U. negotiators.

I have spoken in terms of numbers of nuclear warheads rather than in terms of numbers of missiles or launchers or of bomber or cruise missile aircraft. The magnitude and complexity of the defense task are measured primarily by the numbers of warheads, and all delivery systems need to be considered. But treaty language would more likely be in terms of numbers and characteristics of missiles and launchers and of long-range aircraft, since verification of these numbers is more feasible. Thus reload capabilities and manufacturing facilities and inventories would enter the negotiation process.

The third feature of the defense-dominant world is the protection of warning and surveillance satellites. Without such satellites, "worst case" fears dominate in times of crisis and defenses of all kinds become much more expensive and their effectiveness more questionable. Warning of launches, especially of submarine launched ballistic missiles (SLBM's), is essential to the defense. The capability of attacking orbiting satellites needs to be prevented by treaty or by the development of protective devices or weapons on the satellites themselves or co-orbiting friendly satellites. The development and testing of antisatellite weapons ("ASAT's") should be outlawed by treaty; verification should present no serious problem. Satellites could also be protected by the space-based interceptors ("SBI's", formerly "KKV's") being developed as part of the first phase of the SDI program. Ground-based lasers, such as may eventually be part of a powerful weapon against missiles in the boost phase, present a formidable blinding threat against optical systems on satellites, but even here there is considerable hope, both by the treaty and the device routes.

Preserving the system of satellite surveillance is, of course, even more important to us than to the Soviet Union, since they find it relatively easy in our open society to learn the details of our weapons and systems. But information on each other's capabilities is a stabilizing element; it is not a zero-sum game, and both sides gain by satellite surveillance and warning.

There would be much to be gained by making satellite photography routinely available to the American press. The resolution and contrast could be degraded somewhat (by an undisclosed amount), and very recent or very lucky photographs would remain secret. Obviously the layman could not learn as much as the photo-interpreter. But it would be helpful if the American public could <u>see</u> the outlines of the Soviet arms build-up, including the Moscow defenses and the Krasnyarsk radar, rather than being told about them by military people suspected of exaggeration.

I have now described the first and third features of a defense-dominant world; it remains to attend to the heart of this testimony, the progress toward an active ballistic missile defense system.

THE STRATEGIC DEFENSE INITIATIVE

For about 25 years before President Reagan's speech in March of 1983, there had been programs addressing ballistic missile defense in the armed services and defense agencies, especially in the Army, the Air Force, and ARPA (now DARPA). After that speech and the Fletcher study, the Strategic Defense Initiative program was created under the direction of the SDI Organization. It is important that we understand the differences between the SDI program and its antecedents, differences which I assemble under five headings.

First, the expenditure rate was substantially increased; the SDI program is spending money at several times the total rate of the earlier programs.

Second, the program is integrated and centrally managed. There was, of course, communication and cooperation among the performers before SDIO, but the centrifugal tendencies were also strong and the program really came together only awkwardly and partially, and only on Capitol Hill.

Third, it has developed major efforts and achievements in system architecture and battle management.

These central attributes of a ballistic missile defense could not be adequately addressed when (for example) the Army was working terminal defense radar, the Air Force was working satellite surveillance, and ARPA was working advanced technologies. Analysis of candidate architectures became an early SDI priority. Battle management and command, control, and communication ("BM/C^3") began to receive the attention they deserved and are being integrated with architecture in a National Test Facility. Thereby any technical gaps that would be rate-limiting in the development of a system could be addressed.

Fourth, it has begun the process through the Defense Acquisition Board of the DoD that brings together the technology, systems architecture, hardware and software availability, the problem to be solved, and the service organization to operate a ballistic missile defense system. In this process the Joint Chiefs of Staff become involved and ballistic missile defense can be evaluated in cost and effectiveness in comparison with other military missions.

Finally, SDI differs from its antecedents in that it seeks a decision to deploy at a definite time. It is not "just an R&D program that goes on forever." It recognizes that a defense will <u>evolve</u>, and that it is not just a new weapons system.

The President's speech, Congressional actions, the people's money, and the SDIO Management have thus begun the process which, together with warhead reduction by treaty and satellite protection, will lead to a world better than the "balance of terror." But it is a long process and the program has deep troubles.

A PERSONAL ASSESSMENT OF SDI AND ITS TROUBLES

SDI has been fortunate from the beginning in the excellence and dedication of its top managers. In the field, the performance of the DoE's national laboratories and the technical components of some major aerospace industries have been especially impressive; service laboratories and a few universities have also been very helpful. Technical progress has been rapid, particularly on the hardest parts of the program, such as directed energy weapons, advanced sensors, and the preparation for the simulation of battle management. SDI is attacking comprehensively all of the problems of ballistic missile defense (most particularly countermeasures) including the reservations and vulnerabilities raised by its most skeptical critics.

A great deal of effort was put on the architecture of ballistic missile defense systems. This was expensive, and criticism has been expressed that buying competitive architectures was wasteful, but it was necessary to make sure that no promising approach was overlooked. The conclusions of the Fletcher study were verified, and the concept of a three- or four-layer defense was found to be basically sound. Promising components were identified and their characteristics outlined. The ways in which a system might evolve from an initial architecture have been explored. Evolution of a system is the way one must approach ballistic missile defense, since we are dealing here with a creation more like that of a navy than of a single weapons system like an F-18; the initial capability is of much less importance than the evolutionary path.

Much antagonism to SDI occurred two or three years ago on the grounds that its computer-controlled battle management, and especially its software, "would not work." The attacks said that software was notoriously unreliable, that tens of millions of lines of perfect software code would be needed, that this could not be verified and maintained, that the battle management could not be tested, that it would "have to work the first time," that "nothing works the first time," that "a software error will start a nuclear war," that vital links in the C^3 network would be shot down, and so on. All of these reservations had some substance, but the critics "proved" too much: Their conclusions would also "prove" that many systems such as Minuteman would not work. The architecture and battle management are being developed together in SDI in a resilient manner; if one part fails, another picks up and sustains the stress. It is assumed that many warheads will leak through the first layers of the defense (and even the last layer in early phases of deployment), that many elements of a distributed BM/C^3 system will be destroyed, and that there will be software errors. Large systems like the AT&T electronic switching system are studied to emulate their response to stresses beyond the designed stress. Furthermore, whereas a mistake in the battle management of our current nuclear-tipped retaliatory system could conceivably make a nuclear explosion in the Soviet Union and start a nuclear war (or even an explosion in a friendly or "neutral" country), no mistakes in a BMD system could have such disastrous consequences. About the worst mistake we could make would be to shoot down (without using a nuclear weapon) the booster of a Soviet manned-space mission, an unlikely mistake because our doctrine would probably not call for boost-phase intercepts until more than one booster had gone off from a known missile launch area. If our response to the Soviet mistake of shooting down Korean Air 007 is any indication, this booster mistake would be a long way from precipitating a war.

Another criticism of SDI is that it does not provide a defense against the delivery of nuclear weapons by bombers, cruise missiles, or other means (submarines and suitcases). Clandestine delivery presents a real, but quantitatively limited, problem. The (unlikely) use by the S.U. is the only case where a massive retaliation world might be better than a defense-dominant world; use by other countries would threaten or invoke use of our 500-warhead arsenal. Air defense would certainly become a part of our total nuclear defense, but it can most efficiently come later. The development path is likely to be very much shorter than for BMD, and the character, size, and management of the air defense can be designed much more cheaply and with greater confidence when progress in arms limitation treaties has been achieved and the threat is better known. It would be very expensive, wasteful, and useless (since ICBM's and SLBM's could make end-runs) to deploy an air defense system like SAGE now.

The troubles with the program started before the program. The first was the pejorative "Star Wars," which the media attached immediately after the President's speech, and the associated media opposition, especially in the

Northeast. "Star Wars" carries the connotations of science fiction, of "zapping" people with laser guns, of space ships with nuclear explosions, and (more seriously) of extending the arms race to space. By use of this term, people (if they choose) can avoid the hard analysis of deterrence and hard choices of alternate worlds and say "You can't be in favor of Star Wars!" The media rarely make it clear whether they include surveillance satellites, which we already have but need continuously to improve, under this sobriquet; these seem to me to be a vital component of a peaceful world. It would be possible to develop an SDI without any weapons in space, but it would probably take longer and be more costly, and treaty protection would be needed for surveillance satellites; thus "Star Wars" is quite misleading, possibly intentionally.

The second trouble on my list is money. "Why do you come along now with an initiative when the deficit is so alarming?" This trouble will increase as more costly experiments and simulation facilities are needed and as the deficit will not go away. I eschew the popular practice of comparing missile defense expenditures with what I want least in the Federal budget, but I come close to this practice as I suggest that the comparison should be made between investment for future security and a better world on the one hand and current and probably ephemeral capabilities (like recommissioning, maintaining, and manning battleships) on the other.

The third trouble is more of the nature of growing pains and should decrease somewhat in time. SDIO chose "to hit the deck running" and immediately incorporated existing programs from the services and defense agencies. SDIO has much too small a staff to create and manage an integrated program, especially with today's complications of defense contracting, and almost all operations must be contracted through the services. They in turn have dedicated and conscientious people, greatly exceeding SDIO in numbers, but the central direction, coherence, and leadership are threatened.

The next trouble is that SDIO has not been able to get the studies and analyses institution that would make its management of the program more efficient, that would get more for the people's money. Its proposal of a Strategic Defense Initiative Institute for this purpose has been "put on hold" by the Congress. The SDIO needs the same kind of studies that the Institute for Defense Analyses provides for the Department of Defense or that the Center for Naval Analyses provides for the Navy. Industrial contractors and "for profit" study companies must naturally be thinking about future business and therefore cannot be expected to provide unbiased analyses of alternatives. The SDIO Government officials must, of course, manage the program and decide among competing contractors with millions of dollars at stake, but they are much too occupied with current demands to provide for themselves the thoughtful analyses insulated from the daily crises. Meanwhile IDA and other Federal not-for-profits are of great help, but it would be too great a distortion of staff and diversion from other vital DoD tasks for one of them to substitute for an SDII.

Another trouble is that continued adherence to the 1972 Treaty restricting anti-ballistic missile development will soon raise the costs of the program (more of this later).

Another trouble is that although SDI enjoys broad support throughout the country and in Congress, many of its supporters make conflicting requirements. Some support only if the program is cooperative with the Soviet Union; some support only if there is no cooperation. Some want definite dates to be set for decisions and deployment; some want no dates. Some support only if there are no weapons in space; some want the maximum effectiveness at the lowest cost. And so on.

Another trouble is that considerable opposition has developed on university campuses and within the technical personnel of non-defense industry. This has many origins, some of which are deep scholarly studies that worry me greatly since they may be right. But there are also less honorable origins. The very circumstance that the program was initiated pursuant to a President's speech, rather than by push from the scientific community, raised hackles at the start. The symbolism of apparent retrogression in needing eventually to withdraw from the ABM treaty, when treaties are so hard to come by, is very damaging. Some of the opposition is on the level that "my research needs the money." On some campuses the discourse has descended to the bumper-sticker level, when an important goal of the university as an institution is to raise the level of discourse. This antagonism has made it more difficult to get independent advice and service from senior faculty, which is unfortunate especially in the absence of an SDII. The eventual consequences are probably worse, since universities should be the source of the young people and many of the new ideas that will make the program succeed in the long run.

Our allies, especially those in Western Europe, are increasingly nervous about U.S. intentions and plans. A recent Gallup poll showed that more Britons think that we would start a nuclear war than think that the S.U. would. The West Germans are not at all sure that we would defend them by "pushing the nuclear button" if the Red Army invaded their territory. The doctrine of massive retaliation is revealing its weaknesses. SDI can improve both the security and the ease of mind of our allies in the long run, but in the near term the confusion and controversy in the U.S. add to their uneasiness.

The last of this list and the most damaging trouble is the ambiguity of goals and timetables, a trouble which originated outside SDIO. The original goal was a defense that would make nuclear weapons obsolete and a deployment decision time in the mid-nineties. The time seems to have been held constant and (as experience has accumulated) the goal has been allowed to vary. Further, there does not appear to be agreement within the Executive Branch on what the goal should be and on intermediate objectives on the road to the final goal. Keeping the time constant under these circumstances seems to me to be something between unwise and nonsense. The SDIO, the Secretary of Defense, the White House, and the Congress share the ultimate goal but face serious dilemmas in advancing development of an evolving technology and system when there is so little popular or technically competent consensus on the time it will take and on the effects of early phases (enhanced deterrence? partial protection? stimulation of increase in S.U. offensive forces? treaty abrogation?). My own view is that the ultimate goal should be a defense-dominant world, that the SDI program should flourish to make an essential part (along with warhead reduction and satellite protection) of that world, and that although SDI must develop evolving systems, the intermediate objectives should be enhanced deterrence and a firmer base for later systems (not partial protection of military assets).

THE 1972 ABM TREATY

An important complication on our way to a defense-dominant world is the ABM Treaty. It was a useful agreement fifteen years ago. It was intended to be part of a series of even more important treaties limiting the expansion of, and ultimately reducing, offensive forces (Article XI stated: "The Parties undertake to continue active negotiations for limitations on strategic offensive arms"). Ultimately, it stood alone as the only serious success of the negotiation process since the 1963 Partial Nuclear Test Ban. This circumstance has led to near reverence for the treaty and the view that withdrawing or a legalistically sharp interpretation of it would be a drastic retrogression on the road to peace.

Although the Treaty was to remain in force indefinitely, the negotiators and signers recognized that the world would change and made this recognition explicit in two unusual ways: The treaty calls for a review every five years (1987 is one of the years) and the creation of a Standing Consultative Commission to consider interpretations and protocols. Withdrawal on six-months notice is also provided for under "extraordinary" cimcumstances; the Soviet Union could hardly call SU cheating "ordinary," and since we seem to be convinced that the Krasnoyarsk radar and probably other SU installations violate the treaty, it would appear that we could withdraw.

Meanwhile, the SDI program is advancing to the point where experiments should be done that if done most efficiently, combining sensors and other elements that might be considered as "components" of an ABM system, might violate the treaty. I say "might" because there is a deep controversy within the U.S. about the interpretation of the Treaty. This controversy is creating so much confusion and antagonism that it seems preferable to proceed inefficiently with the experiments. It is wasteful of the people's money to proceed this way, but it is wasteful of the goodwill and mutual respect between the Administration and Congress to proceed otherwise and that goodwill and respect are even more valuable.

Although the Soviet Union publicly treats the Treaty with religious respect (either because they are sincere or because that makes maximum mischief in the open U.S. society), it is not at all excluded that the S.U. will withdraw from the treaty or violate it so brazenly that no responsible elements of U.S. society can defend it. If neither happens and we continue to progress toward a defense-dominant world, we must withdraw. We need not do that soon, but promising now to adhere for n years would be risky, since we cannot know how fast technical progress and arms reduction progress can be made. Clearly our withdrawal would be more acceptable world-wide if it came at the same time as substantial reduction in offensive forces.

I should like to add a footnote concerning statements in treaties about technology limitations. Although the negotiators have access to the best technologists in the world, they find it very difficult to allow for the development of science and technology, since that occurs in largely unpredictable ways. Insofar as a treaty tries to specify restrictions on technology it will become dated rapidly; if the parties still want the treaty earnestly enough, they will live with the ambiguities, but if the treaty is not vital to them there will be endless arguments about what the old language means in the new situation. In contrast the 1963 Partial Nuclear Test Ban provides an interesting example of the will to continue:

The treaty prohibits any nuclear explosion that results in "radioactive debris" outside the country of origin. Scores of nuclear tests by both the S.U. and the U.S. have resulted in the detection by the other side of radioactive debris outside the testing country, but both sides continue to adhere to the treaty without substantial complaint.

The 1972 treaty was written in the context of technology appropriate to a terminal defense. It was inevitable that its ambiguities would lead to arguments and ultimately vitiate the treaty as the context of defense was enlarged to include boost, post-boost, and midcourse intercepts and as the technology palette became more colorful.

THE TRANSITION

A good deal has been written about the transition from a world in which stability is provided by the threat to make it uninhabitable to a defense-dominant world. Some have the view that even if the latter is possible and better, one could not go from one to the other without war. If so, there would be no point in developing a defense since one would doubtless not choose to make the transition. To decide whether or not to develop defense it is not necessary to decide now how would be the best way of making the transition, it is only necessary to be confident that there is at least one peaceful way. Some, including myself some years ago, believe that a transition as rapid as possible during a period of relative calm in international tensions would be the best way, the least likely to lead to war. I still believe this would be possible and would be easier following a period of arms reduction and preferably other cooperation with the S.U.

In the last years, an intriguing proposal for the transition has been made by Barkenbus and Weinberg based on Charles Osgood's idea of a gradual reduction in tension ("GRIT"). They call it Defense Protected Builddown. It consists of a unilateral reduction of offensive forces as defenses are deployed in such a way that deterrence remains invariant; it encourages the adversary to respond with a similar builddown in offense and buildup in defense. The big appeal, of course, is that no treaty is required.

Whether DPB or some other transition is now thought best, ultimately the decision would be made within the context at the time, presumably in the 21st Century. For the present, the likelihood of one or another peaceful transitions seems to me to be great enough to justify proceding toward a defense-dominant world. Meanwhile, at every stage in the research, development, and evolution of a ballistic missile defense it must be possible to show that our activities are not reducing deterrence.

WHAT CAN CONGRESS DO?

Congress must be experiencing more than the usual frustration in its interaction with SDI. Most of the American people are attracted to the President's vision, which has great popular appeal, but individuals in the Executive Branch differ in their statements of the goals and schedules of predicted accomplishments of the program; of course some of this difference can be explained by lack of knowledge of what Congressional actions will be, especially on appropriations. In the country, some of the strongest proponents and opponents of SDI know least about it. Congress cannot really control the program, only the money. And money is now even more than usual a tension-raising question. The military establishment has not yet taken a clear stand that the program is necessary for the security of the country. The ABM Treaty, conflicting interpretations of it, and Gorbachev's manipulation of the American media with respect to it add complications. Arms control negotiations are not programmable by Congress, and success in arms control is not programmable even by the Executive Branch. It is difficult for Congress to get independent technical advice and to test the validity of the important technical numbers in the program. Although it is appropriately largely an engineering program, the most noise about it is made by scientists. And, I am sure, there are important frustrations of which I am unaware.

What can Congress do? As you will realize from all of the above, I hope Congress will take a longer and broader view than just reacting to SDI and that it will espouse the concept of a defense-dominant world. Of course it must make decisions year-by-year, but I urge the kind of long-term commitment that permitted the development of the hydrogen bomb, ICBM's and Polaris, and the Apollo program.

The Olin Amendment (HR1748) stated that it was "the sense of Congress that (1) vigorous research and technology development that could lead to the creation of a strategic defense system...should continue to be supported by the Congress..." This paragraph was an important declaration and a strongly positive step. I hope that after suitable hearings the Congress can make statements that go beyond this in two important ways: 1) that it is an established long-term goal to negotiate treaties with the Soviet Union that limit the numbers of nuclear warheads deployed by each of the superpowers to the neighborhood of 500; and 2) that a ballistic missile defense system be developed that can evolve into a defense capability (complementing other defenses against cruise missiles, aircraft, ships and submarines) fully effective against the agreed attack force. Also, as part of either (1) or (2) or both, the Congress should work toward the protection of satellites, especially surveillance satellites.

Congress should, according to me, not demand a time of deployment or a time of decision to deploy. As the ballistic missile defense program proceeds and progress occurs in warhead reduction, the times should be allowed to vary in order to keep the goals of the program constant. Invariance of these goals, which threaten no one and augur well for a better world, would provide a program that I believe would be backed by a powerful American consensus and would eventually be welcomed by adversary, allies, and other countries alike.

What should Congress do about the dollars? Four or five billion dollars a year in a program that has its future reasonably secure (as assured as, say, the future of carrier task forces) can make as much progress toward 21st Century capability as a six- or seven-billion dollar start-stop program that may be killed in the next election. In a few years, the annual rate might appropriately go up or down slowly in response to technological developments and to steady success in arms reduction treaties; it should not change sharply in response to a single success or failure in negotiations.

Only if unexpectedly quick success occurs in treaty reduction of warheads is there any hurry in the program. If such success occurs and the S.U. cordially embraces a defense-dominant world, the program could be accelerated toward deployment, but this contingency seems unlikely to happen soon.

What about the S.U. missile defense build-up? The short-term reaction to this can most effectively be the addition of more sophisticated penetration aids to our offensive missiles, rather than an acceleration of SDI.

In reviewing the progress in the program, Congress should evaluate not the near term capability but the technology and systems integration development leading to a totally capable system against the expected threat. (If that threat is several barrages from a 500-warhead arsenal, the deployment can be early; if against 20,000 with the expected penetration aids to counter our defense, it will be much later.) I can understand why Congress does not wish to sponsor a weapons program that consists of never-ending R&D, but as I have said this program is more like the development of a navy than of an F-18, and that takes long-term development. Along the way, it is important to plan and to evaluate progress in terms of an evolving system, with appropriate attention to massive simulation and emulation, BM/C^3, the National Test Bed, and the National Test Facility; but that is distinct from deployment. The first deployment should be evaluated in terms of what it accomplishes for deterrence (e.g., breaking up a structured attack) and where it leads, how it helps toward the required capability; the protection it will provide is likely to be only to military targets and the civilian population cannot be expected to be very happy about the consequences.

Congress should permit space-based interceptors, at least in the near term program, as attractive components for boost-phase interception and for protection of themselves and our surveillance, warning, and battle management satellites. If later on an effective ASAT treat can be negotiated and compliance verified, it should be possible to forego any "weapons in space," a consequence that would have a strong positive symbolic appeal throughout the country and the world. (On the other hand, for reasons of cost they might be retained.)

Congress should encourage cooperation with the Soviet Union in R&D on defense. This is a delicate matter and will take much time and design, but it has a high payoff. We are dealing here not with a zero-sum game, but with a positive-sum game where we both gain.

Congress should support and encourage the establishment of a Strategic Defense Initiative Institute (or preferably the same thing under a different name - see below). This SDII has been attacked and put on hold by the Congress on the argument that it was intended to be a kind of Brookings Institution, to undertake studies to see whether SDI was a good idea, and that having it under a board of directors who saw promise for peace in SDI was like having the fox guard the chickens. SDII was intended no more for this purpose than was the Institute for Defense Analyses intended to study whether we should have a Defense Department, or the Center for Naval Analyses whether we should have a Navy. The purpose of SDII and its studies and analyses is to support SDIO in making more efficient and effective the funds Congress appropriates. It is not a policy-study body or a decision-making body. The program is now suffering and the progress accomplished under Congressional appropriations is now being reduced by lack of the SDII studies and analyses.

Congress should acknowledge that we shall eventually withdraw from the 1972 ABM Treaty. In the shorter run, I doubt if it is the public interest to have a confrontation between the Executive and Legislative Branches over the interpretation of the Treaty. If I could speak for the Executive Branch (which of course I cannot), I would give up the attempt at a "broad" interpretation and ask Congress for the extra money required to conduct experiments under the Soviet interpretation. If I were the Congress, I would give SDI the money. Note that I take no sides on who is "right" or "wrong," since I do not know or even know whether there is a right and wrong.

More generally than just in connection with missile defense, the Congress should do everything possible to shorten the development cycle for new weapons systems. These periods are now so long that although the processes may get a weapon a little more cheaply (and everyone stays out of jail), the response to a change either in the threat or in useful technology is so long delayed as to partially vitiate the effectiveness of the new weapon. Although S.U. periods are comparable, the period from the time we first learn of a S.U. development (e.g. notably quieter submarines) until their deployment is substantially shorter than the period between the time they first learn (e.g. in Aviation Week) of one of our developments and its deployment. This long development cycle will continually plague us as we develop ballistic missile defense.

Finally, if a substantial change can be made in the program, especially in the steadiness of its goals and the association with arms control, I believe it would be wise to change its name.

CONCLUDING REMARKS

The most important accomplishment we could make would be to develop a national consensus for a steady, long-term development toward a defense-dominant world. Clearly this will not be easy or quick, and may be impossible (in which case we might have to return to the rather miscellaneous programs anteceding SDI). I have noted some of the costs we sustain in the absence of such a consensus: The waste of a start-stop program, our position as hostages to S.U. mischief-making, lack of breadth of participation (especially by young people in universities), uneasiness by the Armed Services, and signalling confusion to our allies.

It seems to me to be most necessary that we keep our goals in clear, public view at all times and that we change them as little as possible and only for compelling reasons. I assume we can all agree that our most basic goals are to preserve our freedoms (which could be lost by intimidation) and to avoid nuclear war. Other goals are surely to be a responsible, dependable ally and to be taken seriously in world affairs; these goals are now complicated by the circumstance that some of the allies under our "nuclear umbrella" are competing effectively with us with the advantage that their industries do not have to sustain taxation for a nuclear force. These goals can best be furthered, I contend, by a program of massive reduction by treaty in numbers of warheads coupled with ballistic missile defense, aimed to ensure (in Glenn Kent's words) "that our survival (at least with respect to ballistic missiles) is under our control."

Thank you for your consideration.

BIOGRAPHICAL NOTE

Robert L. Sproull was born at Lacon, Illinois, in 1918. He attended Deep Springs College and Cornell University, receiving a Ph.D. in experimental physics. During World war II he worked on Navy microwave radar at RCA Laboratories. He was a member of the physics faculty at Cornell from 1946 to 1968, except for leaves at Oak Ridge National Laboratory, at European Research Associates in Brussels (and lecturing for NATO), and as Director of the Advanced Research Projects Agency (now DARPA) from 1963 to 1965. At Cornell he was also successively Director of the Laboratory of Atomic and Solid State Physics, Director of the Materials Science Center, and Vice President for Academic Affairs. He went to the University of Rochester as Provost in 1968, became President in 1970, and President and Chief Executive Officer in 1974. He retired in 1984 and is now President Emeritus and Professor of Physics at Rochester. He is a member of five corporate boards of directors. He has performed volunteer service for the Federal Government for forty years, especially for ONR, DoE, and the National Academy of Sciences. He was Chairman of the Defense Science Board from 1968 to 1970 and received the Secretary of Defense's Meritorious Civilian Service Medal in 1970.

THE UNIVERSITY OF ROCHESTER
ROCHESTER, NEW YORK 14627

UNIVERSITY VICE PRESIDENT
AND PROVOST

13 January 1969

To: Steering Committee of Faculty Senate

From: Provost Robert L. Sproull

As you know, I have been uneasy about many of the discussions of increased faculty participation in the administration of the University, but up to this point I have not set down my views on paper. I guess I was rather hoping that you would invent appropriate vehicles without further intervention from me, or that an ad hoc committee would propose them. But perhaps a written analysis of the way I look at the whole question would be helpful at this stage. My uneasiness has to do only with the nature of the discussions; I am cordially interested in increased faculty participation. In fact, President Wallis and I easily agreed on this in discussions last May, before I "signed up."

One of the sources of my uneasiness is the frequent centering of discussion on the past. Now I am certainly handicapped in that I understand imperfectly or not at all the incidents and the questions from the past at the U. of R. But perhaps I am also emancipated in a sense by lack of intimate knowledge of these events and by lack of personal accountability for them (even though I of course accept responsibility for carrying on within the structure and the setting created by my predecessors).

One of the sources of my diffidence to break into print has been that I'm trying to avoid inappropriate imports from my Cornell experience. After the extended discussions of the last months and more thought, however, I'm convinced that some key elements of that experience are worthy of importation here.

Some Notes on University Decision Making

Before proceeding to make a definite set of proposals, I'd like to note here the way I look at the decision-making process in a university. You are well aware, but I believe others are not, of the complexity of this process and the way it is diffused in time and among people. Many students and faculty act as if they were convinced that there is a room like a TV station control room or a synchrotron control room, the walls papered with meters and recorders, the desks replete with knobs which control the vital processes of the university. There just isn't such a room. Since it doesn't exist, it is futile to argue over who has the keys to that room.

The actual process is exquisitely intricate and capable of infinite variation as the issues or the casts of characters change. Important decisions (like starting a new college or moving or non-moving the Eastman School) evolve over a period of time, frequently with broad discussion. The interests, attitudes, and desiderata of individuals or groups may be canvassed explicitly, or they may be well known from recent experience on similar issues. In the end, there is a certain amount of personal risk-taking by the dean, the President, or the Board Chairman, of taking the responsibility for the act or for the act of in-action. Feedback from the whole university community on the nth issue shapes the approach and the response not only to the (n+1)th but also to the (n+i)th issue. Voting with one's pen or his mouth is sometimes a part of this process, but voting with one's feet (by faculty, trustees, or even administrators) is a much more important part.

What are the goals of faculty-administration interaction in the decision-making process? I believe the prime goal should be to arrive at the "best" decision. Of course reasonable men will differ on what is best, and even on the criteria for evaluating what is best. Educational policy, resource allocation, and one's Weltanschauung and view of the future are convolved in every major decision. Individuals and groups in the university community may have different views in all of these respects. Major decisions are likely to be better if these views have been heard and if decisions are based on thoughtful, unbiased study.

Let me put in a plug here for university administrators, a be-leaguered lot, even though you may consider it self-serving. In the two-dimensional spectrum of policy elements and of constituency elements identified in the preceding paragraph, one interesting "cut" is to examine the policy element of resource allocation and look at the way the various constituency elements view time. Some would like to throw all the resources of the university into the problems of the present generation. Others believe that is important to be able "to rise and fight again" and to pass on to our successors at least as much ability to maintain an educational environment as we enjoy. Administrators are generally to be found among those with the longest view, even though the self-denial required by that view makes lots of problems for them. An interesting example is the unspoken issue looming behind much of the Steering Committee's (and others') discussions at the U. of R., namely the issue of investment policy and of how much current income to extract and to spend. In approaching this issue, investment expertise is obviously involved, but so are the great imponderables of the futures of inflation, of faculty salaries, of government support, and of private support. Different elements of the University community can make different contributions to studying this policy, but because of the widely different degrees of expertness and the widely different time-views, this area seems to me one of central concern by the administration, adequately and cordially informed by the other elements of the community. [Incidentally, one extreme but evidently not unpopular position in the community approaches this problem with only the sophistication of multiplying 6% by $400 million and saying that obviously the Trustees should put all the endowment into bonds; if we wish to continue to call ourselves an educational institution we ought to be able to teach even this group the implications for the present strength of the institution had our predecessors adopted this policy 10 years ago!]

Another footnote: Resource allocation is not a zero-sum game, but it can easily be made so if one is thoughtless or insensitive or if his hands are tied. Universities are invariably (and fortunately) resource-limited rather than idea-limited. One of the criteria for the "best" decision must surely be to enhance and to enlarge the educational program as much as possible and at the same time to bequeath to our successors at least as much opportunity for development in quality and quantity as we enjoy. If the last dollar that the university will ever receive had already been given, the decision process would have the simplicity of a zero-sum game, but at the expense of much less and much less effective education.

Another goal, important but not so central as getting the best decision, is to make the decision-making process add to, or at least not subtract from, the educational process. This point becomes especially important when considering student participation: The participation of a handful of students is surely "educational" for them and may well help to get the "best" decision; but if the introduction of student "power" leads to less than optimal decisions, by (frequent) small compromises or (rare) swaying of the decisions into unfortunate directions, the negative effect on the education of tens of thousands of students will quickly dominate. The algebraic balance is a little different in the case of faculty participation. I see no special worry about warping decisions; after all, major administrators are ordinarily faculty or ex-faculty, and moreover just the kind of faculty who are characteristically elected or appointed by faculties to influential posts. "We're on the same side." But there is surely a strong, if unmeasurable, distracting effect on faculty members who are sharing administrative responsibility, who are worrying about non-scholarly problems. This distraction is exacerbated by the tendency in all of us (as I have stated elsewhere) to "relax into action."

Another goal, and an important one, is to increase the resilience of the institution by broad participation in decisions, participation which at its best carries with it broad understanding for and support of the resulting decisions. The attainment of this goal does not automatically follow from enlarging the scope of participation, however. Skill and self-restraint are required by all concerned, and self-restraint (by definition) cannot be legislated into the system. For example, administrators should not excuse, by blaming faculty opposition, their failure to go the way that later proves to have been the wise way, or hide behind the committee consultation apparatus. Faculty should not attempt to replace the full-time administration with a part-time administration concentrating on "hot" issues. (In fact, the frictional heat on these issues may best be absorbed by administrators, partly to relieve tension in the community.) Clearly traditions of mutual support leading to institutional strength in depth can develop, and the key aim should be to set up the most promising arrangements for the natural development of these traditions. (One of the reasons why such traditions develop slowly is that the whole process receives its testing and trimming-up in the aftermath of decisions that become unpopular or apparently unwise; paradoxically, then, the better the process, the slower its improvement.)

Department chairmen and deans are faculty, too, and their initiatives are absolutely indispensable elements of institutional strength and development. Their institutional loyalties are in a way a subset of the over-all loyalty: A department chairman cannot be as single-minded in urging a new specialty as a faculty member, but he is just as single-minded about the development of his department. A dean cannot be as single-minded about the development of a department, but he is just as single-minded about the development of his college. Deans' and department chairmen's jobs are difficult and crucial ones, and the satisfactions appear only slowly and accompanied by many up-and-down fluctuations. Any new faculty-administration consultative organization or apparatus should not in any way impede the chairmen and deans or reduce their accomplishments.

These considerations, while far from the only ones, are the principal considerations in my mind as I propose the following committees.

Proposed Committees

There are two faculty committees that would, according to me, be major aids to the functioning and direction of the University of Rochester. I shall try to describe below how these would operate in the "steady-state," which is to me the important regime, but first I must say a word about the "starting transient." I think that the productive steady state, the traditions of mutual help, respect, and self-restraint, will be very slow in attainment if such committees must start by working their way through accumulated dissatisfactions and disappointments. The debilitating search for "who shot John" will surely get these off on the wrong foot.

Another general point: Should these committees work with the President, the Provost, the Associate Provost, the Director of the Office of Research and Project Administration, or others? Consultation at all of these levels is useful, and the character of the consultation can change appropriately with change of level. My own view of the most appropriate level (and I am tacitly assuming a limited number of committees) is tempered by my fright at the volume of activity funnelling through my office on the one hand and my desire for a stronger interactive role with the faculty on the other hand.

I propose two committees, each of five (or at most six) faculty members, one on Academic Affairs and one on Research Policy. Each committee and its chairman would be appointed by the President with the consent of the Steering Committee. This procedure should continue to provide committees that can work with the administrators and that are in no sense "stacked." Why not elected? One can obviously get the same people either way, and elaborate election rules could be established to get the most effective "mix" of colleges, disciplines, and ages. The principal objection I have to election is that it would create an expectation of wielding power by voting, of a narrow sense of responsibility to an electorate that had not studied the issues but expected its elected representatives to effect its will. I think you will see as I continue with the description that my concept of these committees is at variance with such an expectation.

The committees would report from time to time to the Senate or to the Steering Committee as they felt the need, but their principal "output" would be interaction with the administration, not reports, and their major accomplishments might never become widely known. Minutes and secretarial apparatus would be an unnecessary and possibly damaging diversion from the important work. I cannot now imagine situations in which the committees could be effective by voting. I should think that the usual "wind-up" of an invariably free-wheeling discussion would be that we would "go around the table" noting everyone's analysis, conclusion, or recommendation.

In proposing five or six on each committee, I am mindful that the complexity of the University is such that no number short of the Senate itself can be really "representative," and even the Senate is in many ways too small. But these faculty would serve as well-informed, well-connected, thoughtful individuals; they could co-opt other individual faculty members for particular questions beyond their own experience. The principal reason for keeping the group small is not so much to avoid unnecessary expense and diversion from scholarly work as it is to avoid the dilution of the feeling of responsibility that occurs when a group becomes larger. "What is everybody's business is nobody's business."

I have reluctantly concluded that I should probably be the member of the administration who works with these committees. If this recommendation is accepted, the chairman of each committee and I would work out the agenda and the frequency and duration of meetings, with the ground rule that any topic would be discussed if either of us thought it was sufficiently urgent or important. I can guarantee that my behavior and my part in University decisions would be strongly influenced by these committees. Sometimes I could say after thorough discussion "I'll take your advice and do....." Other times I should say "I cannot (or will not) take your advice because..." At other times I might have to say that I should push in a particular direction but might be unable to succeed in producing motion. If the members did not feel that influence wielded in this way was worth the time they spent, of course the system would collapse. My view is that their influence would be greater through this interaction than any other way, certainly greater than by some kind of "review" role. There would also be powerful advantages to me. My experience with similar committees tells me that frequently a statement like the following would be made: "You can do it that way if you like, but you ought to know that if you do..." Part of the compensation for the substantial time I would have to spend in this process would certainly be that the committees would help to keep me from making any more of an ass of myself than was congenitally required.

The more important of these committees would be called the "Academic Affairs Committee." Since everything around the University is in some sense an academic affair, even planting trees (or at least that part of planting trees that can be influenced by the Provost), this committee would have a very broad scope. I should discuss with them everything I was involved in above a certain threshold of importance, and I'd be happy to let them decide what was important. As an illustration of the "grist" for this proposed "mill," if the committee were starting tomorrow, I should like to discuss: 1.) How to approach a stiffening and intensifying of review of academic appointments, especially tenure appointments. 2.) ROTC. 3.) Programs for black students. 4.) How I should approach departmental strategies. 5.) Whether and how to establish graduate student quotas. 6.) What issues are University and what are college issues, and the development of new inter-college relations. 7.) Long-range budget planning. 8.) Implications of giving up the baccalaureate degree in Business. There are many more, but I'd like at least to start discussion of these. The committee would, of course, have its own list.

The other committee would probably be called the "Research Policy Committee." I should like to have Mr. McBride join me in meeting regularly with the committee. Among the immediate topics would be: 1.) Review of operations resulting from the NSF expenditure limitation, and planning for next year. 2.) Revision of contract research policies and procedures, including possible expansion of the role of the department chairman. 3.) Guidelines on academic-year salary recovery. 4.) Making U. of R. needs and experience known and influential in national policy deliberations. 5.) The Mansfield amendment and its legacy.

You and others have proposed a long-range planning committee. I have not yet become convinced of the necessity or wisdom of the creation of such a committee. Faculty influence on budget planning and developmental strategies should be strong and benign through the Academic Affairs Committee. I just don't believe that a faculty group (or any other group) can prevent all the "goofs" in designing new buildings. A faculty committee can, however, strongly influence priorities and procedures, even in the construction area, and this too can occur through the Academic Affairs Committee.

One concluding remark: One of the troubles with a memorandum as long as this is that it may give the impression that I have written down everything that I consider important on this topic. I make no such claim; failure to mention some point here may mean that I don't consider it important, but it also may mean only that I forgot it or that I was diverted by some other point. But I've tried.

It goes without saying that I'd be happy to discuss, comment, augment, or defend any of this.

RLS/ac

W. Allen Wallis Memorial

Robert L. Sproull
30 October 1998

I can speak with any authority only about the period 1962 to 1982, and especially 1968 to 1974. These remarks will thus capture only a small part of Allen Wallis' contributions, but that part has profound consequences for the University of Rochester.

The famous epitaph of Christopher Wren in St. Paul's Cathedral is precisely appropriate: If you require a monument, look around you. [Si monumentum requiris, circumspice.] The shape, texture, and size of the University are the product of Allen's vision and leadership. I refer to the buildings, including this one, but mainly I refer to the flavor and quality of the faculty and of key departments. In more detail, it was the vision and leadership of Allen Wallis and Joseph Wilson and the product of their mutual respect. If you could not observe, as I did, the warmth and depth of the interaction between these two giants you missed a phenomenon that is not likely to be repeated.

Although considerable building and rebuilding of the University had occurred between the end of the War and 1962, the sights had not been set high. Allen raised the goals from being an acceptable regional university to being national in scope and among the best in any activity we chose to undertake. Allen's reputation and participation in the network of leading scholars gave him access to the critical information essential to the recruitment of top faculty.

This process was well under way by the late sixties, when the disturbances Allen called the "great campus craze" created a substantial diversion of effort. Although the draft and the Viet Nam war were the obvious and publicized stimuli, the "craze" was clearly more complicated since the worst student riots were in Paris. There were two canonical manifestations on U. S. campuses: One was the occupation of campus buildings. The other was the stealing of meetings: A meeting assembled for some educational purpose or to hear a guest speaker would be broken up or exploited by a group promoting some cause.

Allen never wavered in his defense of an open university, where every position and point of view could get a respectful hearing, where teaching and research could be protected from disruption. He was, perhaps more than any other university president, willing to call the police, and that well-known willingness probably was the reason the police never had to be called here. Unlike almost all other presidents, Allen never put a foot on the slippery slope of compromise.

In reacting to the "craze" and in every other activity Allen Wallis was a rare man of principle. One principle, for example, was that the University must not take an official position in controversial situations; if it did, it would diminish the freedom of faculty, students, and others to take an opposing position. Most of us are a little uncomfortable in interacting with a person of principle, and I believe that is one reason that Allen seemed to many to be austere and remote.

Allen was a determined and persistent enemy of hype and exaggeration and of shoddy thinking. His antagonism to the moves by lesser persons to become popular was so firm that it almost seemed that he tried to be <u>un</u>popular himself. I remember one occasion when a representative of an important group responded to a proposal by saying "I'd have to think about how that would make me look." I thoughtAllen would explode!

Yet even Allen had his enthusiasms, although I believe one would call them "muffled." Among others, they were statistics, George Stigler, large automobiles, and chocolate ice cream. When he experimented with one of the first Texas Instruments hand-held calculators he devised a method of calculating means and standard deviations that was shorter than the method in the instruction book. Satisfaction with this success produced the wry smile that all of us remember— Mary Sproull calls it his "Mona Lisa smile."

Finally, association with Allen Wallis was not a soft and cuddly affair. Friendship was often demanding, since every view or conclusion was subjected to rigorous scrutiny. But if you could rise to the challenge, it was eminently rewarding. With Allen's passing we have lost a truly remarkable friend.

House Science and Technology Committee
8 February 1989

Testimony by Robert L. Sproull, Chairman,
Basic Energy Sciences Advisory Committee of the
Department of Energy

I appreciate the opportunity to appear before you to represent the Basic Energy Sciences Advisory Committee. Your Committee was instrumental in creating our Committee, and we owe you a candid statement of our findings and recommendations.

It is impossible to do justice to these in a short presentation. The body of our 1988 Report is only 15 pages, and I commend it to you as worth your reading. Recommendations are highlighted, and reading it should be an easy, but not happy, experience. The Department has already responded, and I am pleased to report that it is taking our "output" seriously, but of course its ability to respond is limited by the budget.

The reason the reading experience will probably not be a happy one is that it is apparent on every page that the BES program is being dangerously constrained by budget pressures. I acknowledge that this must be an all-too-familiar refrain in this room, but I wish to point out some of the reasons Basic Energy Sciences is especially threatened.

The largest single problem is that BES has the responsibility for the development and maintenance of major National technical facilities that serve both the Department of Energy and other Government departments and industry. About half of the science and technology research in these facilities is for users other than DOE. These facilities are a major and often central part of research and development throughout the Nation; scientists and engineers "take them for granted" and assume that they will always be there, will be well nourished and kept up to date, and will be the the equal of any in the world. The reactors, electron microscopes, chemical engineering facilities, isotope production, and synchrotron radiation sources, and the laboratory support surrounding them, are a vital national asset without which research would not be able to stay at the frontier. The creation of these facilities has been much envied and imitated throughout the world, and facilities in Europe and Japan are beginning to exceed ours in competence. The BES facilities must be at the frontiers in materials science, chemistry, biology, and engineering and must have the highest neutron fluxes, radiation intensities, and similar characteristics of any anywhere. In order to have the extreme fluxes, intensities, and other properties that enable U. S. scientists and engineers to compete world-wide, they must necessarily be designed for extreme conditions, without the benefit of decades of experience. Thus shutdowns, modifications, and expensive new instrumentation are to be expected. but rarely has money for these been budgeted in advance.

Up until now, money for these reasonable and necessary purposes has had to be abstracted from the BES research budget, even though other parts of DOE, NSF, DOD, NIH, and others benefit at least as much. The basic research in materials, chemistry, biology, engineering, mathematics, and geosciences has accordingly suffered. Although the DOE and the Nation can go on this way for a while without noticing it, the thinning of the research base underlying energy problems and opportunities and the inability to exploit research opportunities will eventually greatly weaken the Department of Energy and the country. We have recommended that funds be made available for the continued maintenance, development, and dealing with crises in these facilities without damaging the research program.

We have recommended that the Congress and the DOE not retreat from their responsibility to stimulate and facilitate advanced research through use of these facilities. The Advanced Neutron Source should be designed and constructed without delay; the synchrotron radiation sources at Argonne and Berkeley should proceed on plan; the upgrade of the

Brookhaven HFBR should be supported, in part to develop instrumentation for the ANS; the High Flux Isotope Reactor should be restarted quickly. All these will cost heavily, but the Congress when it considers these costs should be thinking of them as costs of a <u>National</u> program, not just a BES or DOE program.

We have made other recommendations about the accounting and reporting of these facilities.

We were challenged to analyze priorities for research within a constant budget. There is almost no wisdom that we can provide in response to this challenge. The research program is well balanced, well managed, and underfundunded. The only problem is that there is not enough money to follow many very promising research routes. When the next energy crisis occurs it will become apparent that the program was quite inadequate.

We were also asked to consider priorities within a program that "recognizes the need to capitalize on new opportunities." In response, we first recommended that funding of the major facilities be put on a more realistic and secure basis to let them operate more efficiently and effectively and to allow for emergencies. Next we recommended five specific research areas in which increased funding would have substantial "payoffs" for the DOE's missions. Finally, we recommended that across-the-board funding expand to respond to Department needs and to opportunities arising from new science and engineering ideas.

Unlike some other areas (e.g., particle physics), Basic Energy Sciences does not have a coherent, nearly single-minded constituency arguing for it; the BESAC is attempting to serve as a (necessarily inadequate) substitute. BES is a wide-ranging,diverse, complex program consisting of some applied and much basic research. Unlike many other areas of science, <u>all</u> of the basic research is in <u>applicable</u> areas; that is, a research success will connect with the real world and provide products and services needed by the Nation in its difficult task of remaining great and strong.

I shall be glad to answer questions.

Remarks of Robert L. Sproull
The Future of the U. S. Academic Research Enterprise
December 9, 1991

The Working Group, like its parent the GUIR, is constrained: We are not supposed to make recommendations. This could be a convenient "cop-out": As James Thurber wrote at the conclusion of one of his fables, "It is easier to ask some of the questions than to give all of the answers."

But as individuals we are not so constrained, and in fact I have been asked to give my own views of the issues raised in the Discussion Paper in the hope that they will help to stimulate the discussion this morning, study in the sessions this afternoon, and recommendations of this Conference tomorrow morning.

I start with the assumption that research is, and will be into the indefinite future, severely resource-limited. All of us in this room have spent almost all our lives in resource-limited settings. Although this is frustrating, it is nothing like as frustrating as an idea-limited setting. There is no possibility of that: Every field is witnessing important advances and is replete with opportunities. Somehow one must pick and choose, and the fiscal constraints are binding more and more tightly. On page 61 the Paper therefore appropriately starts with "Priority-Setting."

In setting priorities there are two big questions: What criteria do you use? and Where do you get the muscle?

The Working Group's approach to criteria is first that the *quality* of the research enterprise must be preserved and even enhanced, and that that will not happen without attention, effort, and even imaginative change; in the absence of substantial priority setting, there will be a general leveling down and even decay. Without quality, the various groups that seek to mobilize the research establishment for their own purposes will have nothing worth exploiting.

Relevance comes next, but *not* relevance as that term is ordinarily used in this town, namely immediate applicability (as in the infamous Mansfield Amendment, Sec. 203). Relevance must be in the long term, and to constituencies that are not yet born.

Setting priorities is looked upon as wicked by the humanists and yet it is always done.

The *individual* does it in large in deciding the focus of his or her career and in small every time a new research problem is tackled. That is part of the reason that selecting individuals is the key to quality. But individual priority setting doesn't come close to solving the problem we have before us.

The *academic department* sets priorities, but only rarely does its leadership have the courage to abandon whole sections of a field, as it must do now.

The *university* sets priorities. It is constrained by the need to preserve enough breadth to serve undergraduates, but often this is used as a specious argument to support political decisions and minimize complaints. A university *can* decide, for example, to have a world-class chemistry department at the expense of a so-so physics department and minimal geology and astronomy departments. I believe the essence of university management in this era is to be "anti-Isaiah," just the opposite of his famous prophecy that "Every valley shall be exalted, and every mountain and hill shall be made low." Not many faculties will support a strong enough university administration to act like this.

The *Federal Government* sets priorities. With rare exceptions its priority-setting is reactive, responding to committee reports, professional societies and other interest groups, and politics. It has managed to increase the support of science and engineering without cutting out any whole fields, but it has also managed to leave everyone unhappy. It simply must find ways to abandon the *macho*, America-first-and-best attitude toward *every* field.

The Paper cautiously advocates increased internationalization, and accomplishing this in selected fields would enable us safely to place a lower priority on Federal expenditures in those fields. This cannot, however, be done as we have tried to do it, by first fixing the technical characteristics and site of a major project and then asking other nations to join in its support.

On page 15 the Working Group urges that the mix of criteria be made explicit. Although this sounds only fair and honest, it flies in the face of the success vagueness and ambiguity have on the Potomac. (How otherwise could the Department of Energy support particle physics?) For a Federal agency to make explicit the mix of excellence, originality, relevance, and equity it intended to follow would very probably attract so many enemies as to seriously cut its budget. Even worse, overburdened officials would likely distort the remaining budget heavily in the direction of "equity," with a score or more definitions of equity.

Toward the bottom of page 36 the Discussion Paper notes the role *time* plays in priority setting. I have already noted the conflict in time scales, when discussing relevance. This conflict is at the heart of our problem. Once when I was suggesting a next-year action to a President's Science Adviser, he replied: "You're asking me to look to next year; I can't even see into next week."

Unfortunately there is no calculus to determine how much to invest in research. I once tried to start such a basis by suggesting that the investment should be the sum of three terms: 1.) Science for science' sake. This would be just like history or philosophy, just as justifiable as one of the great achievements of civilization, and just as hard to quantify. 2.) Science for teaching's sake. This would be the relatively small amount necessary to vitalize undergraduate instruction and is the only one of the three that is really quantifiable. 3.) Science and engineering for technology's sake. Only areas of science that have some chance of being eventually applicable in the real world would qualify here. This is the big term, but the magnitude of this term is now more limited because American industry cannot afford, at the high internal rate of return it requires to maintain independence, to exploit as much science as foreign industry can.

In the absence of a calculus, decisions tend to consist of adding (or subtracting) so many percent each year. I do not know of a plausible argument that the present Federal expenditure for research is a "correct" (or "incorrect") base to which to apply these increments. I have, of course, seen many arguments that it needs to be increased and comparisons with other countries' numbers, equally without basis. The situation is simply not intellectually respectable. It is similar to, but not quite so egregious, as the strategic deterrent: There is no way that we could have decided to create the size and complexity of this system as a deterrent to what once was the Soviet Union but is now altogether different.

We are no longer a rich country in our ability to invest, although that fact is obscured by our spectacular consumption and self-indulgence. The *negative* investment arising because meagre personal and corporate saving are dwarfed by huge Federal, state, and local government indebtedness inhibits a positive view toward research as an investment.

A largely irrelevant argument goes on in the media about whether we are going into or out of a recession or depression, coupled with the usual calls for short-term Federal action. There seem to be good reasons for thinking that the current sluggish economy and near zero real growth may be the new steady-state, brought on by lack of productivity increases, government spending dominating personal saving, and a consumption rate unrivaled in the world. Is this too apocalyptic for the holiday season? I believe it is dangerous to ignore this possibility.

Let me put this ugly thought together with another one. Harold Brown about 25 years ago noted that everyone thought they were guaranteed about a 15% per year growth in Federal support of research, although he could never find this assurance in the Constitution. The growth nevertheless has continued, albeit not at that rate. Dale Corson and others have pointed out the good reasons *why* science expects an exponential increase. We all know that this exponential increase must some day change to a *growth curve,* at least as a fraction of the Gross Domestic Product. But tacitly we usually assume that the transition will occur on someone else's watch. Well, I believe there are good reasons to conclude that the transition will begin *now* and will be well underway before the end of the Century.

This belief is what persuaded me to go along with that rather flamboyant word "Fateful" in the Paper's title. New choices must be made or the research enterprise in the U. S. will be nibbled away across the board into a general valley of mediocrity.

I close these remarks by addressing the final two pages of the Discussion Paper. I accept all of the words on page 70 and almost all on 71, but there is one thought missing on each page.

On page 70 I would add that universities (and Government laboratories even more urgently) "must" agree to give up some things. I would not ask them to give up integrity (or "inner logic" in the phrase of Eric Ashby's that I prefer) but only a little of their autonomy. As a starting example, I would ask that any Federal support of facilities be conditioned on non-expansion of the research enterprise, both in square feet and in numbers of principal investigators.

On page 71, I would emphasize the first sentence on that page and the self-restraint required by faculty in order that the required leadership have some maneuvering room. Someone must take the *responsibility* for the unpopular decisions that will be required if we are not further to mortgage our future. Finally the word "articulate" in the third and fourth sentences is too generous, a word of art. "University leaders" must first *have determined* "the goals, purposes, and priorities of their institutions." And "Government leaders must first *have determined* somehow "the Government's goals, priorities, and policies in supporting academic research. Once those are done, I count on the eloquence of university presidents and the studied vagueness of Government leaders to articulate appropriately.

As you see, I believe our basic choice is quality *or* size, and I believe size must become the dependent variable. You will recall Chekhov's play *The Cherry Orchard:* The family's famous cherry orchard is the largest in the region. As the family descends into deep financial trouble, a young man's proposal to cut down some of the trees is viewed as wicked by the traditional members of the family The choice is between the well-being of the family, including the orchard, and the size of the orchard. The proposer of reduction is castigated as a philistine, but quality of life (in our case, quality of the research enterprise) must be preserved at the expense of size.

Report of the Long Range Planning Committee

November, 1980

I. Preamble

This is the report of a Committee that never met. It is not a plan. It is a presentation of some of the issues facing the Board of Trustees as they make their decisions shaping the institution and its futures. Many have said that they have confidence in planning but not in plans. In that spirit we have tried to help the Board by analyzing and presenting most of the ingredients for their planning.

We regret that we cannot advise the Board on what is perhaps the most fundamental element of Deep Springs planning: How many years can Deep Springs operate if current practices of the Board (budgets, scope of operations, fund-raising activities, etc.) prevail? We do (or at least the writer of this section does) recommend that the Board address this question. To do that will require knowing four basic elements: 1.) What is the budget now for operations plus the average annual additions of capital items? 2.) What is the assumed inflation rate? 3.) What is the realistic evaluation of the average earnings from endowment and ranch operations and their inflation rates? 4.) What is the realistic estimate of capital gains and losses? 5.) What is the realistic estimate of average annual gifts? If the reader says: "Those are the questions your Committee was supposed to answer," our reply is "Don't be silly!" (See also Appendix D.)

We believe these questions can be answered and are being answered by annual financial decisions and budgets. We add only the observation that if a "Net Deficit" of anything like $77,000 occurs with any frequency, the life of the institution will be very short indeed.

As we said before, this is the report of a Committee that could never meet. Therefore there can hardly be any "conclusions" or even recommendations. There are some suggestions, however, and we hope the Board and the Deep Springs community will take them seriously.

The "Introduction" section was written by the Chairman, after whatever consultation he was able to achieve. The "Academic Program" was written by Dale R. Corson after strong interaction with the Sub-committee for this purpose at Deep Springs. The section "Facilities" was written by Robert F. Gatje with considerable help from students and others at Deep Springs. "Financial Analysis and Planning" was written by James R. Withrow, Jr., with help from Frederic S. Laise.

The reader should not be misled by the structure of this report, which seems to imply at first glance that there is a single "output" (academic) supported by two "inputs," facilities and finances. We strongly reiterate that the Deep Springs experience, its contribution to the growth of a new generation, is a tripod of academics, practical work, and Student Body self-governance. We especially emphasize that the Board should give strong effect in their plans and decisions to the central importance of developing a sense of responsibility in the individual students and by the Student Body as a corporate entity. The work program is a key part, but only part, of this. (The effect of acceptance and discharge of responsibility on education is alluded to in Appendix B.) "Responsibility" covers a broad array, including responsibility for the well-being and safety of others, for the care and proper use of ranch and school equipment and facilities, and for introspection and analysis of one's own values and choice of careers.

II. Introduction

> "It is, sir, as I have said, a small college, and yet there are those who love it."
> Daniel Webster[1]

> ". . . want of forecast is the nation's crime."
> Daniel Defoe

Burton Clark[2] in writing about Reed, Antioch, and Swarthmore makes the point that the distinctive college is likely to have distinctive strains, tensions, and risks. Deep Springs is probably the most distinctive college in contemporary society, and so it should come as no surprise that it is better endowed with strains, tensions, and risks than with what is left of L. L. Nunn's foregone self-indulgence.

Any attempt at long range planning brings these tensions out of the woodwork. Long range planning at Deep Springs is necessarily as unconventional as the institution itself.

---

[1]The Committee's counsel says that Webster never said it. Perhaps God never said "Let there be light," but a lot of people think he did, and it turns out that there is light.

[2] Burton R. Clark, The Distinctive College, Aldine, Chicago, 1970.

Conventional planning for a private academic institution proceeds from a basic statement of how the imperatives of the present are to be balanced against the imperatives of the future. A common statement runs like this: We should spend and plan in such a way that our successors will have at least the same ability to support educational programs as our predecessors bequeathed to us. This deceptively simple statement can usually be agreed to by all. But when one begins to use this statement in planning he must insert numbers, and that is where the tension develops. One must agree on an inflation rate, which is much more difficult now than five years ago. He must predict a rate of growth of endowment assets and of cash flow from the endowment. He must predict government support and negative government support (compliance costs). For most institutions, he must have good estimates of how his competitive position will change as the numbers of 18-year-olds decline. And so on.

But one tacit assumption is always made in conventional colleges: The lifetime of the institution must be infinite. Planning is complicated enough with this assumption and becomes almost nebulous without it. Yet Deep Springs planning cannot make this assumption.

The basic tenet of Deep Springs planning is the decision by the Board to operate Deep Springs at annual budget levels that sustain the type and quality of program characteristic of Deep Springs' history and traditions. The Board has from time to time noted that to do that with existing and currently anticipated resources implies a finite lifetime. A search of Trustee minutes since that resolution has failed to reveal an explicit statement of this basic tenet.[3] In the present planning exercise we take this as given doctrine. We do not quarrel with it; indeed, we suggest that the Board make it explicit with any additional language with which it wishes to surround the basic statement. One such supplemental statement might be: It is nevertheless the determination of the Board to work hard to try to make the lifetime of the institution infinite, by energetic fund-raising combined with frugal management. See Appendix A.

---

[3] Will Masters has found a statement attached to the Board Minutes of April 27, 1973 that reveals the spirit of the Board's discussions: "I don't think any of us, or at least anybody in my hearing, has suggested that we change the basic strategy which we all agree to: namely, that we run a first class operation or we stop." This comes from a dubious source, however, and hardly has the blessing of official Board action.

In conventional planning, the number of students is a most important variable. This has from time to time been a controversial issue at Deep Springs, rivaling smoking and coeducation in popularity. We began our work with the Student Body size considered as a variable and looked at the new facilities that would be required to increase the Student Body by (for example) a factor of two (there did not seem to be any sense in exploring a decrease by a factor of two). This could perhaps be accomplished best by converting the present Main Building to classrooms, offices, and (existing) library, miscellaneous renovations and minor expansions elsewhere, and building a new dormitory (probably designed so as not to pre-judge the coeducation issue). It quickly developed that with existing and realistically predictable resources, such remodeling and new construction would be finished just in time to be dedicated on the day the Trustees were forced to close the institution. We therefore in all our subsequent work assumed the present Student Body size would prevail, but (see below) this cannot be considered to be a Committee recommendation or conclusion. Further, we suggest that the Trustees discuss the following resolution: Any substantial and unexpected resources provided to Deep Springs should be applied to extending its lifetime rather than to expanding its size.

We did not consider the questions of charging tuition or of coeducation.

There is a basic split within the Committee concerning what faculty size should be used in planning. No amount of wallpapering-over will conceal this crack without destroying whatever usefulness this report might have. No ringing changes on words like "ideal," "adequate," "minimum," or "optimum" will help. There is basic disagreement. One section believes that 5 1/2 faculty (full-time equivalents) are about right; one section believes that 5 1/2 are too many. The cleft is wider than "just finance." One group believes that missing 5 1/2 by substantial amounts imperils attracting a suitable Student Body, students' abilities to transfer or otherwise pursue studies and careers, and the quality of the Deep Springs experience. The other group believes that a number as large as 5 1/2 threatens the sense of initiative and responsibility of the Student Body and limits Deep Springs' effectiveness by making it more conventional.

There seems to be considerable correlation between the number and nature ("regular" or visiting) of faculty when an individual experienced Deep Springs and the number and nature he considers to be best. All of us are prisoners of our own experience, and the provincialism (in both space and time) of Deep Springs is legendary. It would be easy to be cynical and speak of Alexander Pope, Dr. Pangloss, and tunnel vision. We prefer to be more optimistic and view this correlation as yet another manifestation of the deep affection and loyalty Deep Springs commands.

Anyone who has worked for Deep Springs by seeking donations or by speaking with prospective students or their advisers knows that its unique character is often regarded with suspicion. Those whose only experience is with conventional schooling and whose imagination and depth of analysis are not unlimited often regard Deep Springs as kooky. It is time to counterattack. There is a deep and growing dissatisfaction in American society with schools and colleges. The most scholarly and authoritative embodiment of that point of view was provided in the "second Coleman report," a report of the Panel on Youth of the President's Science Advisory Committee.[4] Some excerpts from that report are given in Appendix B, but it is no exaggeration to say that there are two simple statements that distill most of the (not inconsiderable) wisdom of that report: Schools are a mistake. Deep Springs is the way to go.

Deep Springs' financial difficulties are deep and inexorable. But it would be a tragedy to let it disappear just as the Deep Springs way is beginning to be recognized as the way to go.

---

[4] James S. Coleman, Youth: Transition to Adulthood, Report of the Panel on Youth of the President's Science Advisory Committee, June 1973.

III. The Academic Program

The academic program portion of this long range planning report derives from two sources: 1.) extended discussion by the Academic Program Subcommittee, primarily in deliberation at Deep Springs on the occasion of two Board of Trustees' meetings and secondly through correspondence among members of the Subcommittee, with subsequent revision and editing of drafts of the report; and 2.) deliberation of part of the parent Long Range Planning Committee. The body of the report, through the "Conclusion" section on page 18, represents the work of the Subcommittee. The addendum following the above conclusion section represents the work of the Eastern part of the parent Committee.

The Long Range Planning Committee has approached its work with the intent of identifying those policies which will permit the continued operation of Deep Springs College with the distinctive purpose and with the provision of the first-class "Deep Springs experience" which have characterized the College from the beginning. We assume that the Board of Trustees will authorize expenditure of whatever funds are necessary to implement these policies. It may be the inevitable conclusion, however, that in adopting such an approach we are automatically letting the College's lifetime be the variable in the consideration.

We hope that adequate resources will be available to maintain the College, along with the policies here recommended, indefinitely, but we believe that the future is apt to be a difficult one. Consequently, constant attention and imaginative thinking will be essential. In particular, we think it may be impossible to extrapolate the status quo operation into the indefinite, and likely austere, future. In our recommended policies we distinguish between an ideal program and one which may be less than ideal but which can still provide the distinctive educational experience we cherish.

A. Basic Objective of the Academic Program

The fundamental objective of the Deep Springs academic program is to give students an insight into the conditions bearing on human existence, to instill in them the desire to contribute to the betterment of mankind, to give them an understanding of the physical world, and to promote the development of skills which will permit pursuit of these goals through study in depth.

A specific objective of the curricular program is to permit easy transfer, for those students who so wish, to a quality four-year college or university after two or three years of study at Deep Springs.

The Deep Springs experience should include study beyond the first-year level in some academic subjects. It may not be possible to provide faculty at Deep Springs to afford this opportunity as widely as would be useful. However, whatever it is possible to provide at Deep Springs can be enriched through the use of visitors, alumni, orother friends of Deep Springs.

B. Curriculum Specification

The Deep Springs curriculum should embrace a core of subject matter in the general area of the humanities, the natural sciences, and the social sciences. The most important humanities areas are English and literature, languages and philosophy. In addition, public speaking has always been a feature of the Deep Springs experience and should continue so.

The important natural sciences are mathematics, physics, chemistry, biology, and geology. Geology has a particular role in the curriculum because of the Deep Springs setting.

Among the social sciences, in addition to history and government (including political theory and international relations), students should have over a two-year period some opportunity to study economics, psychology, and sociology or anthropology.

There should be curricular programs in areas peripheral to the core areas. The most important of these is the arts, with instruction provided in a relevant discipline for at least one term every year. In the core areas the formal courses can be limited to one-year introductory courses, with study beyond the introductory level through directed reading or informal study.

In all Deep Springs curricular programs emphasis on interrelationships among subject areas is important. With broadly qualified faculty it may be possible to offer courses with thematic overlap, as with science and philosophy or literature and history.

An important feature of the Deep Springs experience is breadth of study, and the Long Range Planning Committee believes it important to continue this emphasis. It comes naturally, however, in the Deep Springs setting, and we believe that no fixed distribution requirements are necessary to insure breadth.

C. Nature of the Faculty

To have faculty qualified at the Ph.D. level is a common, but not always essential, requirement.

A faculty complement of 5 1/2 FTE salaries is adequate, and probably represents the ideal situation, providing a good academic program, especially when the faculty members are broadly qualified, but in terms of an austere future this number may be unrealistic. It may be necessary to provide comparable coverage at less cost.

While such a situation would be undesirable, we believe that there are ways to mitigate this, provided sufficient imagination and cooperation are available. We believe that there are many Deep Springs friends and alumni, particularly in the academic world, who would be willing to serve as short-term, visiting faculty, at little or no cost to the college. Such arrangements have been successful in the past. Further, opportunities exist to appoint husband and wife teams with qualifications in appropriate fields within the specified curricular needs. Such teams can sometimes fill two FTE teaching positions at 1 1/2 FTE salaries, providing curricular coverage at reduced cost.

The academic program can also be enriched by friends or alumni who could direct study through reading courses in their fields of competence.

In addition, the Deep Springs student body sometimes includes students who are expert in particular areas of study. These students can gain valuable experience and can provide valuable instruction if they are used as student teachers to a limited degree and under the active supervision of a faculty member.

These enrichments are intended as a supplement to and not a substitute for in-residence teaching.

An increasing library of video tapes on matters of interest is available. Instructional opportunities exist in this direction.

No matter what the number of FTE faculty members, we believe study at Deep Springs could be enriched in these ways. Experimentation with curricular programs should be encouraged.

As a general practice, short-term visiting faculty can be used to provide breadth of coverage within the core areas and coverage in the peripheral areas, while faculty members with extended appointments can provide the continuity required in core areas.

Since a faculty of such limited size cannot teach all the subject matter specified, areas of specialization must be selected carefully with emphasis on the core areas. Every effort should be made to hire faculty with broad academic competence.

In the use of short-term faculty, attention to scheduling is required so that each student can have the maximum opportunity to study widely within a two-year or three-year stay at Deep Springs.

Opportunities exist to appoint husband and wife teams with qualifications in appropriate fields within the specified curricular areas. This opportunity is particularly great at times such as the present when the academic job market is depressed. Such husband and wife teams sometimes seek to share an academic appointment or perhaps to share less than two full salaries. Such appointments can provide advantageous curricular capability within the 5 1/2 (or fewer) FTE salary budget.

Maximum use should be made of visiting lecturers to provide both depth and breadth of coverage in specialized areas.

D. Features of Faculty Appointments

In most cases it is unwise for a faculty member to remain at Deep Springs longer than five years since career opportunities for young faculty members are lost if residence at Deep Springs is longer. Normal appointment at Deep Springs might be for an initial one-year appointment with two subsequent appointments of two years each. Whatever the particular arrangement, limited tenure and flexible contractual arrangements should characterize faculty appointments.

Faculty career development should be promoted during residence at Deep Springs. Leaves, with financial assistance if feasible, for professional development would be useful, both for the College and for the individual. The Long Range Planning Committee believes, however, that such financial assistance is likely to be minimal in the near future.

Attendance at professional society meetings is an important element in professional development. Some travel assistance, particularly for West Coast meetings, may be feasible. A useful step would be provision of a lifetime travel budget for each faculty member, leaving the decision to the faculty member himself on how to use the budget. Such a budget would have to be administered so as not to provide disproportionate benefits for short-term faculty members.

Faculty exchanges with other institutions are useful for everyone concerned, provided satisfactory financial and scheduling arrangements are possible. The Committee recognizes, however, that satisfactory financial arrangements are difficult to devise.

E. The Students

High quality students will always be important to Deep Springs and selection standards as they have been established in the past must continue. These standards include capacity for high quality intellectual and physical work, leadership potential, emotional stability, and an adventuresome spirit.

The Committee believes that recruiting techniques must be refined to maintain an adequate applicant pool. In particular, it is vital that alumni cooperate in the identification and referral of likely candidates.

F. Teaching Facilities

Facilities at Deep Springs are minimally adequate. Classroom space is adequate but it needs rehabilitation. Laboratories are minimally adequate for first-year instruction in physics, chemistry, and biology. Provision of new and modern laboratory equipment would promote better instruction but is is not the highest facility priority.

The library is adequate but every opportunity should be seized to expand the holdings. It may be possible to compile lists of desirable books with circulation of the lists among alumni who might make gifts from their personal libraries.

Simple and reliable computing equipment must be available. It must be programmable and capable of computational sophistication. Hand-held programmable calculators have reached a degree of sophistication which can meet this need at present, provided a printer is available for the calculator.

With the growing availability of a good library of video tapes it is important for Deep Springs to possess a video tape cassette machine.

G. Term Structure

The present structure, with 6 seven-week terms, is satisfactory, although the Committee believes that it could be modified without serious loss if there were a need for modification.

H. Conclusion

In summary, the Long Range Planning Committee believes that the academic program as it has evolved is a good one and that continued provision of this program into the future, with some adjustment and modification, can insure a quality educational program at Deep Springs. On the other hand, the Committee believes that financial exigencies may well require a degree of retrenchment and that new approaches with new instructional methods may become a necessary feature of Deep Springs life. The Committee believes that the quality of the Deep Springs experience need not be jeopardized should such moves become essential.

I. Addendum

In deliberations of the long range planning report, the Eastern part of the parent Committee has strong reservations concerning some features of the academic program portion of the report as developed by the Academic Program Subcommittee. A majority of the parent Committee holds a substantially different view from that expressed in the Subcommittee's report. The final report necessarily represents the views of the split parent Committee.

The Subcommittee's report represents a slightly, but not significantly, modified version of the status quo operation at Deep Springs and implies that the standards pursued in the current operation are necessary to an effective "Deep Springs experience." The majority of the parent Committee disagrees with this assumption and believes that the Subcommittee's thrust is to provide no more than an adequate substitute for two years at a top college or university. The Subcommittee has failed to emphasize the essential importance and interaction of the work program and the Student Body government program with the academic phase of the Deep Springs experience. The majority of the parent

Committee believes that Deep Springs students of the quality the College is accustomed to attracting can have a significant intellectual experience without a broad array of formal courses or even of visiting lecturers. The accomplishment of alumni who experienced Deep Springs when the faculty resources were significantly fewer than those now available is testimony to the soundness of this point of view.

In the views of this portion of the Long Rang Planning Committee, the basic objectives of the academic program are the following:

1. A sufficiently broad program to attract the caliber of students we want;

2. A program adequate to enable students to transfer as a junior to leading colleges and universities;

3. To provide the opportunity for a student to have an intellectual challenge in areas not covered by formal curriculum;

4. Through short-term lecturers or outstanding one-term professors to challenge the students to the problems of the real world—as distinguished from the academic world of most college campuses.

Turning to the mechanics of the program, the majority of the parent Committee believes that the concept of an "ideal" situation is flawed and that substitution of the concept of an "effective" situation is important. With this point of view in mind, the following points are important.

1. The specific number of FTEs is not important. The College has operated satisfactorily in the past with as few as three FTEs and often with four. If necessary, future operation can be on the same basis.

2. The Ph.D. level is not sine qua non for good and inspiring teachers.

3. "Areas of specialization" are unnecessary or indeed undesirable for students at Deep Springs. There is plenty of time to specialize later.

4. Five years is too long for most, if not all, faculty to remain. It is usually bad for the teachers in their academic or other careers, and it is also bad for Deep Springs since professors tend to go stale in the secluded atmosphere and lack of constant contact with their peers.

5. One-year appointments with a limit of three years, unless there is specific Trustee approval for each additional year, might be a good policy. Circumstances could dictate easy deviation from such a policy.

6. If the length of stay of individual faculty members is reduced, the faculty career development plan proposed by

the Subcommittee would be excessively costly. Certainly it is difficult to conclude that the longevity of Deep Springs should be reduced by such expenditures.

The thrust of the addendum by the parent Committee is to remove consideration of the academic program from the context of the current operation and to place it in a longer range perspective where the future of the College may well be extended through modification of the program in the direction which characterized Deep Springs in its earlier years. In fact, there is evidence to support the view that by giving more weight to the work program and the student government program the total Deep Springs educational experience can be enhanced.

The future academic program at Deep Springs is apt to be dictated by circumstances as they develop rather than by pursuit of a detailed long range plan. Consequently, the thrust of the addendum comments is to suggest that should circumstances require substantial deviations from current standards and from standards specified in the report of the Subcommittee the future of the College need not be jeopardized and, in fact, in some respects might even be enriched.

IV. Facilities

We had assumed that there were three separate and distinct possible courses of action by the Trustees of Deep Springs with respect to their treatment of the physical plant. (Excepted is our response to such catastrophes as fire, flood, and earthquake.)

1.0 In brief, these are:

1.1 Emergency maintenance only in response to breakdown and collapse.

1.2 A systematic program of preventive maintenance in addition to 1.1 above.

1.3 All of the above plus a gradual program of capital improvement in order to improve the current standard of living and teaching and to reduce operating costs. Part of this option can be thought of as "catch-up" maintenance, returning the school to a former state of neatness that may be imagined but may never actually have existed.

2.0 We will examine each of these possibilities in greater detail but would first like to establish some facts and ground rules in order to facilitate the evaluation and comparison of these alternatives.

2.1 Throughout what follows, we will attempt to use one currency, in so far as it is possible to establish this with reasonable accuracy, namely: <u>replacement value</u> in mid 1980 in Inyo County, California.

2.1.1 Student labor, if likely to be utilized, is counted at \$3.30 per hour so as to remain comparative to projects necessarily executed with outside labor. One student working five (5) days a week half-time comes to an annual labor contribution of about \$3,500.

2.1.2 Staff employed on maintenance are somewhat arbitrarily evaluated at \$10,000 per year including room and board.

2.1.3 New construction of institutional space is evaluated at \$55/sf.

2.1.4 New construction of residential space: \$40/sf.

2.1.5 New construction of unheated utility or storage space: \$25/sf.

2.2 At the present moment, the School and Ranch are made up of 24 principal buildings, subdivided and measuring as follows:

		Institutional	Residential	Utility or Storage	Condition
2.2.1	Main Building	10,500 sf			good to bad
2.2.2	Museum	2,000 sf			fair
2.2.3	Boarding House	5,300 sf			good
2.2.4	Upper Faculty Cottage		1,600 sf		very good
2.2.5	Lower Faculty Cottage		1,200 sf		good
2.2.6	Aird Cottage		1,300 sf		excellent
2.2.7	Guest Cottage		750 sf		excellent
2.2.8	Faculty Duplex		3,600 sf		very good
2.2.9	Mechanic's House		1,000 sf		fair
2.2.10	Ranch Manager's House		2,000 sf		good

2.2 continued	Institutional	Residential	Utility or Storage	Condition
2.2.11 Modular House		1,000 sf		excellent
2.2.12 Cowboy's House		1,000 sf		bad
2.2.13 Irrigator's Shack		450 sf		(needle does not register)
2.2.14 Green Shed			4,000 sf	fair
2.2.15 Garage			2,500 sf	excellent
2.2.16 Dairy Barn			3,000 sf	good
2.2.17 Chicken House			3,000 sf	good
2.2.18 Horse Barn			2,500 sf	good
2.2.19 Pig Shelter			525 sf	bad
2.2.20 Slaughter House			400 sf	good
2.2.21 Tin Shed			1,400 sf	good
2.2.22 Red Shed			250 sf	fair
2.2.23 Blockhouse			160 sf	excellent
2.2.24 BH Boiler Shed			250 sf	excellent
	17,800 sf	13,900 sf	17,985 sf	

2.3 The approximate replacement value of our buildings (excluding contents) therefore would be 17,800 sf x $55/sf = $ 979,000

13,900 sf x $40/sf = $ 556,000

17,985 sf x $25/sf = $ 449,625

$1,984,625

(This total is quite close to White & White's $1,784,524 when one considers our different styles of calculation.)

2.4 The contents of our buildings are somewhat more difficult to evaluate. Rules of thumb come up with values between 10% and 20% of the buildings that house them; excluding the unheated Utility and Storage space, this yields a spread of between \$158,000 to \$307,000. Since this includes library books, it is probably conservative to err on the high side.

2.5 Vehicles and farm equipment were recently valued by Ed Cronk and Tom Payne at about \$100,000.

3.0 We have attempted a first, very rough evaluation of our three alternatives as follows:

3.1 Emergency Maintenance. On the assumption that this probably approximates what we are doing now, we tried to extract from the last three financial reports and next year's budget those costs that can be reasonably assigned to the care and maintenance of the physical plant, excluding crisis projects such as the rebuilding of the garage. (000's)

	77-78	78-79	79-80	80-81	+"Special"
3.1.1 Maintenance & Repair of Buildings & Grounds					
- Material	8.3	16.6	15.5	17.5	+ 17.2
- Labor (est)	5.0	6.0	8.0	10.0	
Total (est)	13.3	22.6	23.5	27.5	+ 17.2
3.1.2 Repair & Replacement of Contents					
- Material including lab and library	5.2	6.2	6.6	7.0	

		77-78	78-79	79-80	80-81	+"Special"
3.1.3	Repair & Replacement of Vehicles					
	- Materials	0.8	0.2	0.2	1.3	+ 34.0
	- Labor (est)	10.0	12.0	13.0	15.0	
	Total (est)	10.8	12.2	13.2	16.3	+ 34.0
3.1.4	Estimated Value of Total Maintenance Effort	29.3	41.0	43.3	50.8	+ 51.2

3.2 Preventive Maintenance. There has been a lot of work done in this field by such organizations as APPA (Association of Physical Plant Administrators of Universities and Colleges) and state university systems. Cornell has adopted a "steady-state" maintenance program which is adapted from pioneering work elsewhere. Simply put, they are budgeting a certain percentage of the replacement value of their physical plant each year. Cornell uses 1.1% of buildings only and leaves equipment to departmental budgets. Grounds are also budgeted separately. APPA says a set of buildings of wood and masonry should be maintained at 1.3%.

3.2.1 If we follow Cornell's lead, 1.1% of $1,984,625 = $21,830.

3.2.2 APPA's figure would be 1.3% of $1,984,625 = $25,800.

3.2.3 Since we lump buildings and grounds together, we are probably not too far off national averages and have, unknowingly, been following a program of preventive maintenance rather than purely responding to emergencies.

3.2.4 Repair and replacement of furniture, books, and lab equipment is probably unbudgetable except on a year-by-year, need-perceived basis.

3.2.5 Our vehicle fleet must be maintained at something like the present 1.5% of replacement cost but we should also factor in replacement at something like once every 10 years, say $10,000/year if we ever get off our "one horse shay" timing.

3.3 Capital Improvements. We have heard discussion of the following:

3.3.1 A new science lab building: 2,000 sf @ $55 = $110,000.

3.3.2 Installation of new showers, general repainting of the student quarters: $50,000.

3.3.3 Rebuild Cowboy's and Irrigator's residences: $60,000.

3.3.4 Convert some fuel oil burning heating plants to LP gas on the assumption of at least short term savings: $1,000± per conversion.

3.3.5 Rehabilitate the museum following construction of a new lab building, creating a new Fine Arts facility for ceramics, music, etc.: $30,000.

V. Financial Analysis and Planning

It is impossible to devise a meaningful five-year financial plan for Deep Springs for two basic reasons:

1. We do not know what the institution will spend either for
    a. operations (including necessary repairs), or
    b. prudent maintenance; and,
2. We do not know what will be realized in the form of income from its various sources, including donations.

Under these circumstances, the most that can be done at this time is to point out some of the probable long range financial problems and considerations which the Trustees face at every meeting of the Board. In the fall the primary emphasis involves setting the outside limit on academic salaries and in the spring the entire remaining budget is in issue.

In preparation for this portion of the report, both the operating financial statements and the audits for the past eight years have been studied. We conclude that under the present economic climate and the special circumstances surrounding Deep Springs operations, past history forms little basis even for five-year approximations. [Attached in Appendix C are three exhibits which may be helpful as background as to the past, but see also Appendix D.]

A. <u>Basic Financial Policy</u>

Any financial plan such as we are charged with undertaking rests upon policy decisions of the Board of Trustees. We recommend that the Board of Trustees, at the November 1980 meeting, restate as its general policy the following:

> "The Board of Trustees pledges that it will continue the operation of Deep Springs College in a manner and style so that the 'Deep Springs experience' is not impaired."

Such a policy means that when necessary (as it has often been in the past) the Trustees will borrow from its endowment and expend capital assets. All reasonable efforts should be made to replenish the Trust Fund in those years when there are substantial cattle/hay profits or large gifts and bequests. In other words, a year where there is a cash surplus, <u>i.e.</u>, when receipts from all sources exceed all disbursements, the amount should be credited to the Capital Account.

The difficulty with this policy is that Trustees, students, the administration at Deep Springs, and faculty members will certainly tend to differ as to when the "Deep Springs experience" is impaired. A glance at the past may well be of assistance on this point. Everyone knows that Deep Springs has periodically had hard times in the past. Somehow it has survived. Some of the current members of the Board and of this Committee found the "Deep Springs experience" meaningful and worthwhile even when the academic and maintenance programs were quite spartan compared with the budgeted program for 1980-81. Indeed, some may reasonably conclude that operating on a very tight budget has certain positive educational benefits.

B. Projections as to Income

Projecting income for the next five years is an impossible task, as it involves four important variables:

1. Income from securities;
2. Income from the Ranch;
3. Gifts for current operations;
4. Contributions to endowment.

Item one can most readily be estimated and it has been done. (See footnote 2.) Item two has wide swings, indeed beef on the hoof in July 1979 was $1.09 and a year later a mere $.81. (See Section C.) Items three and four depend primarily on alumni support.

C. The Importance of Ranch Income

In the past two or three years, income from cattle and hay have made substantial contributions to the financial picture at Deep Springs. The increase in gross income from cattle and hay has been little short of miraculous. The average over the past decade was probably less than $50,000 a year. The estimated income for 1980-81 is $81,000. However, the wide swings in gross income from these sources range from a low of about $19,000 to a high of $114,750. These figures put us on notice that Deep Springs, even with the best of ranch management, is at the mercy of local rainfall and the price of beef.

D. The Importance of Alumni Support

During the past five or six years Deep Springs has been the recipient of substantial gifts from two or three older alumni, i.e., pre-World War II Deep Springers. As their donations dwindle, as they must because of retirement or death, Deep Springs will not receive as much unless younger alumni make up the short fall.

No matter what seems probable in the future, Deep Springs must have strong alumni support to survive. Contributions by friends and foundations are welcome windfalls and should be avidly pursued, but they are too problematical to be relied upon as a major source of financial help.

While Deep Springs has a fairly broad base of alumni support, it should be close to 100%. An alumnus who profited from a stay at Deep Springs at no cost to himself ought to try to repay this in later years by helping Deep Springs so that this experience is available for young students in the future.

E. The Current Budget

A 1980-81 Budget, which provides for a deficit of $76,800 after assuming Ranch income of $81,200 and security and gift income of $205,000, clearly demonstrates that Deep Springs has a pretty finite life. This budgeted deficit comes after two years of apparent net incomes due largely to Ranch income and somewhat to the accrual basis of accounting. The swing from a net of plus $90,844 to a minus $76,800 is a bit staggering.

Perhaps a more significant figure is that the Trust starts this fiscal year with an accumulated net deficit of $693,960, down from an all time high in recent years of $857,409 on May 31, 1978. Had the Trust been intact, one might have been able to achieve a balanced budget for 1980-81.

F. The May 1980 Deep Springs Expenditure Forecast for 1981-82 through 1985-86

Meetings at Deep Springs during the May 1980 spring Trustees meeting and estimates by former Director Cronk produced figures which should be reported and analyzed. The basic assumptions were that normal operating expenses would increase at a compound rate of 10% and capital replacements and major maintenance at a reduced rate.

The 1980-81 Budget seems to show:

	Normal operations	$313,000	
	Capital replacement, maintenance, etc.	50,000	
	Total expenditures		$363,000[1]
Less:	Ranch income	81,200	
	Gifts, securities, income	205,000	286,200
	Net deficit		($ 76,800)

The projected income from fund raising was made at the rate of $150,000 per year for the next five years. No projection was received as to gross income from cattle and hay or from securities. These figures would then be as follows:

	1981-82	82-83	83-84	84-85	85-86
Normal operations	$344,000	379,000	417,000	458,000	504,000
Capital replacement & maintenance	50,000	50,000	25,000	30,000	30,000
	$394,000	429,000	442,000	488,000	534,000
Gifts	150,000	150,000	150,000	150,000	150,000
Income (non-ranch)	80,000[2]	85,000	90,000	95,000	100,000
	$230,000	235,000	240,000	245,000	250,000

---

[1] One set of figures received from Deep Springs divided this total as follows:

Normal expenditures	$330,200
Capital replacement, etc.	32,800
	$363,000

[2] No estimates were received for non-ranch income, but starting with $80,000, an increase of $5,000 per year would seem possible. The amount is a percentage increase between the Common Fund and S&P figures.

Based upon the figures just sited the deficit to be made up from gross ranch income or, in the last resort, from capital funds are as follows:

1981-82	82-83	83-84	84-85	85-86
$164,000	195,000	202,000	243,000	284,000

If we assume the following three alternatives:

a. average gross ranch income (cattle, hay, etc.) of $53,000;

b. the $81,000 gross projected for 1980-81; and,

c. the maximum gross ever received $118,800;

the deficits would be as follows:

	1981-82	82-83	83-84	84-85	85-86
a.	$110,000	142,000	149,000	190,000	231,000
b.	$ 83,000	114,000	121,000	162,000	203,000
c.	$ 45,200	76,000	83,000	124,000	165,000

With these indicated deficits, it is clear that a critical review of the entire operation is in order, including expenditures and fund-raising efforts. In making estimates as to future costs a national average of 10% inflation was utilized.[3] One can realistically question the validity of applying a national figure to Deep Springs since cost of housing, food,

[3] Experts disagree as to the precise percentage, but recently a number have postulated a 9% annual rate; others are considerably higher.

and fuel are probably at substantial variance from national averages.[4]

Recent years indicate that the average source of funds to cover expenditures is about as follows:

Contributions	37%
Investments, etc.	21%
Cattle and hay	20%
Capital	14%.

If the 1980-81 Budget were included, the percentages would be substantially the same.[5]

G. Conclusion

Absent some new miracle at Deep Springs, one could easily conclude that the College has only about ten more years to live. This conclusion has been reached several times in the past, but renewed efforts have averted such a catastrophe.

A very lean operation at Deep Springs is not the complete solution to its College's financial problems. It seems clear from the efforts of the past dozen years that Deep Springs must enhance its efforts to expand the

---

[4] In the recent past, Deep Springs seems to have operated on a basis considerably smaller than the national inflation rate.

[5]

Contributions	36.6%
Investments	28.1%
Ranch	21.1%
Capital	14.1%

support of its alumni. Efforts must be made to have recent alumni begin support by modest annual donations. The habit of giving to Deep Springs must be cultivated. Efforts to expand the number of friends who contribute should be actively pursued. A program of seeking bequests must also be pursued.

Over the years, Deep Springs has hoped that it would receive substantial financial support from foundations or corporations. While these efforts should be continued, we can hardly rely on this avenue to save Deep Springs or even to lengthen, in a substantial way, the life of the College.

The longevity of Deep Springs unfortunately will not depend very much on the acumen of the investors of our securities or the profits from Ranch operations. Of course, these are important; successful results do help. What will be determinative is first the extent to which Deep Springs runs a tight ship, sticking to the essentials, and second the extent to which Deep Springs convinces its alumni and friends that keeping Deep Springs is so worthwhile that it will command their substantial support.

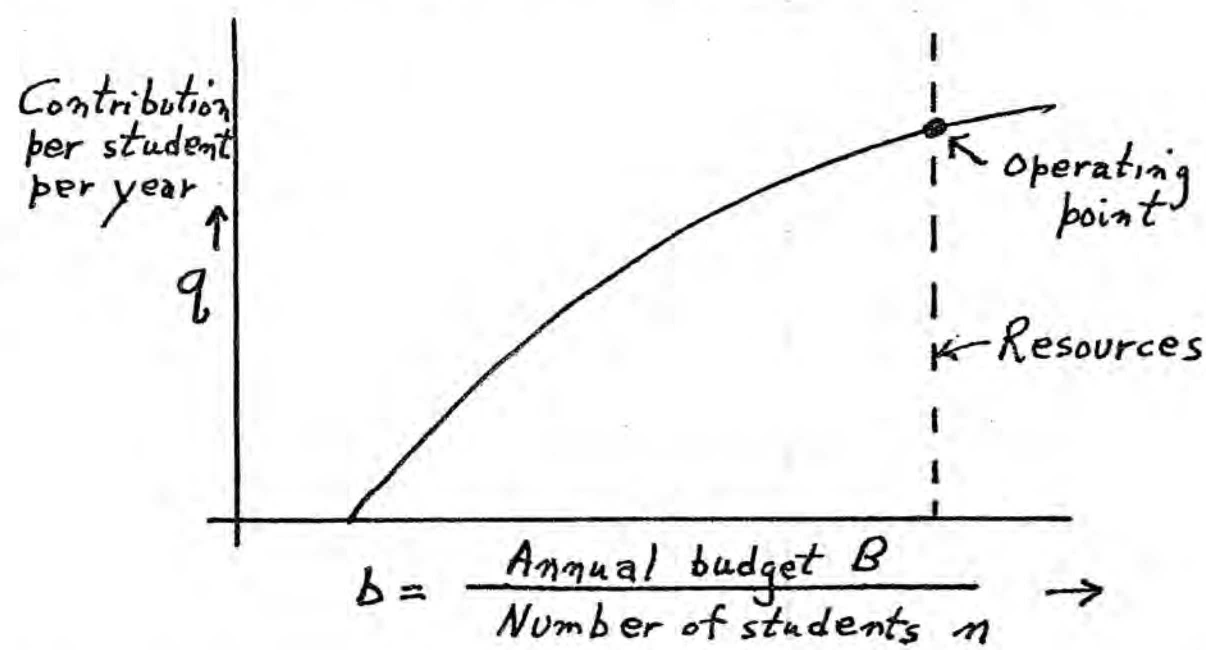
Appendix A

This Appendix is intended for those who like to view problems graphically. It is not necessary to buttress any part of the report, and therefore if it is difficult to understand, alleged to be misleading, or just plain confusing, the reader can safely ignore it.

A. The Conventional Institution

The simplest form of planning at the ordinary (not Deep Springs!) institution is represented in Figure 1. By "contribution per student" we

Fig. 1

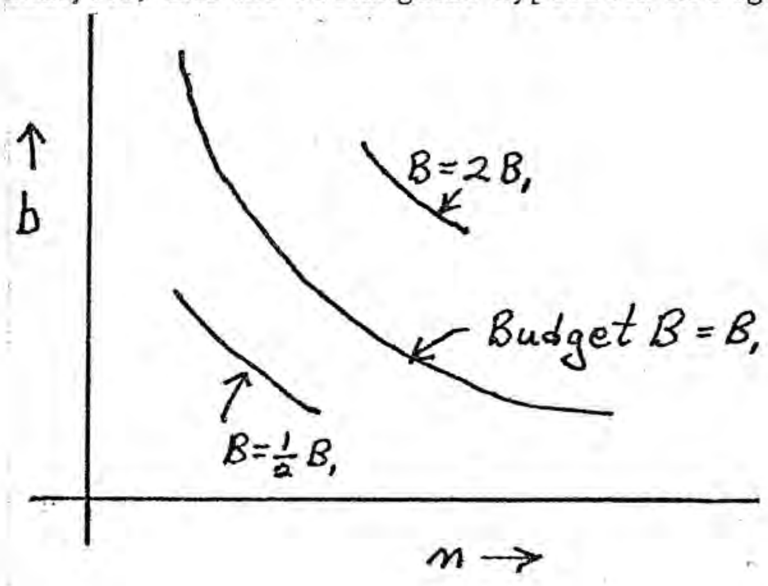


mean the educational accomplishment per student. For constant application of student time and constant aptitude of the Student Body, it is a measure of the quality of the institution. (The analogue in industry is the "manufacturing value added.") The curve has an intercept on the abscissa because until the budget exceeds a certain level, there is no "output" at all; all the dollars are going into "overhead" items like repairing roofs and paying lawyers. The curve is concave downward because there is a limit to what money can buy; presumably the curve eventually approaches a horizontal asymptote, but no real-life institution explores that region of the abscissa.

In the simplest form of planning, the number of students n is fixed, the available resources are fixed, the budget B is fixed equal to the available resources, and the institution operates at the point indicated. Educational management then consists of moving the curve up as far as possible year after year by aggressive recruiting of students and faculty, efficient use of plant, sharing overhead by outside-funded enterprises, and so on.

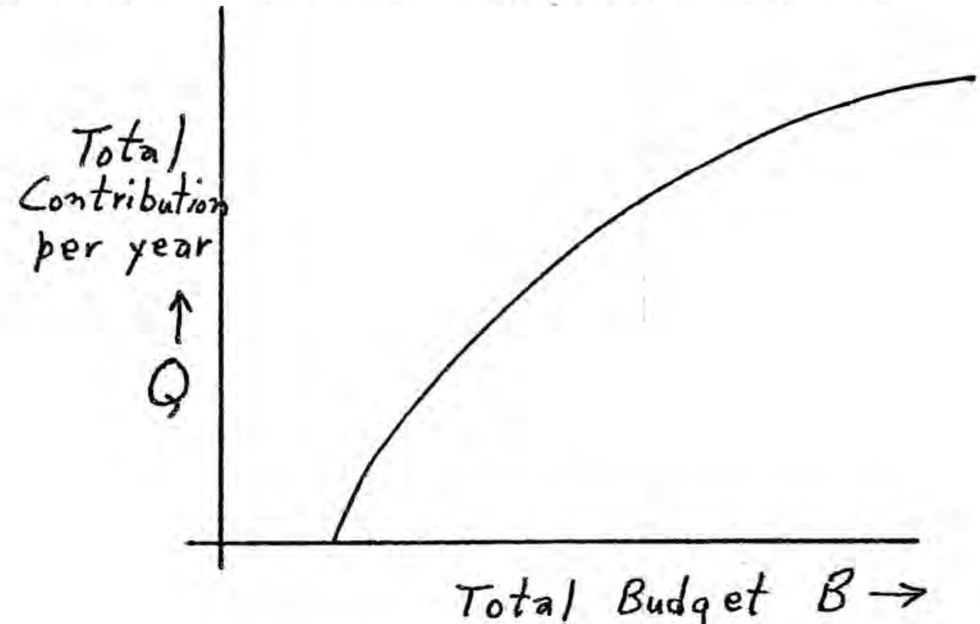
This simplicity is usually violated first by the removal of the assumption that n is fixed. The relation among B, b, and n is the only simple one in this analysis, and the rectangular hyperbola in Figure 2

Fig. 2



is the only curve in this Appendix which is not schematic. Of course at most institutions, B is not constant as n changes: Tuition income in private colleges and state appropriations in public grow with n. Thus the next curve, the <u>total</u> contribution of the institution to society (the product Q of the contribution q per student and the number n of students) is not easily derived from the foregoing. But the general

Fig. 3



shape is only quantitatively different. At the far right, for state institutions and a handful of large private institutions, the curve may turn down (it would do so if the q <u>vs</u> b curve in a 30,000-student institution were less than 1/2 its value in a 15,000-student institution, for example), but that need not concern us; it does make politicians roar, however.

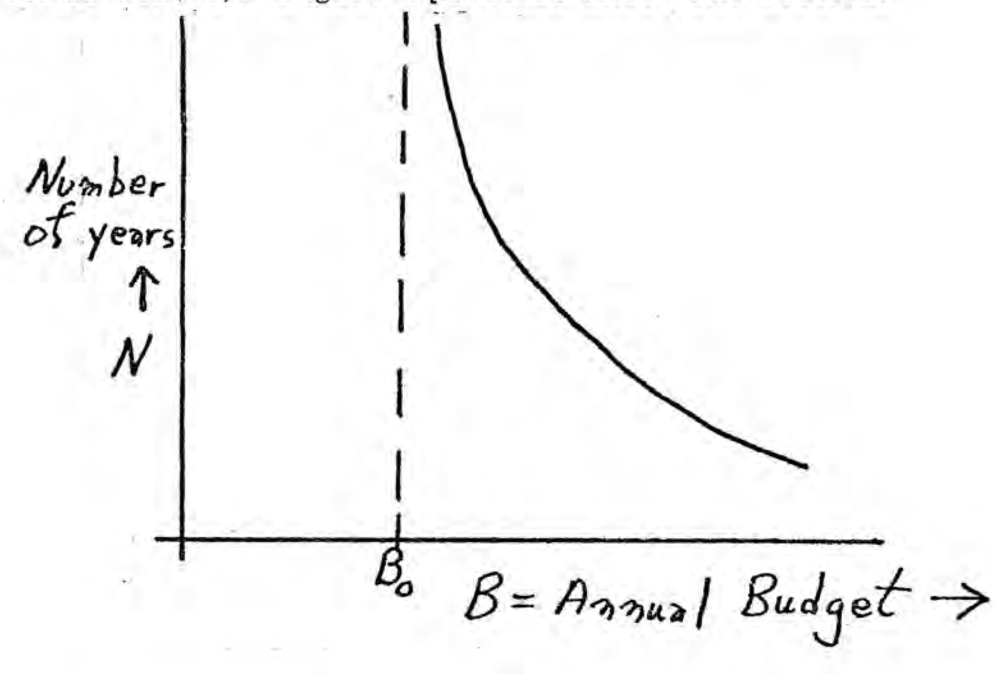
B. Deep Springs

Up to a point the analysis is similar at Deep Springs. A curve like Figure 1 applies. We should emphasize that lower and higher curves of similar shape would apply if n were changed. But we are not considering changing the Student Body size, and therefore we need not go on to Figures 2 and 3.

What introduces the complexity in Deep Springs planning is that by policy the annual budget does not have to equal the annual resources. This introduces a new variable, the lifetime N years of the institution. Before, we spoke of the total contribution of the institution per year and tacitly assumed that it would go on forever. Now, the significant quantity is the product Nnq. Since we are treating n as a constant, this product is proportional to Nq.

We next must look at the relation between N and b (or B; since n is constant, either will do). Figure 4 provides this. The vertical

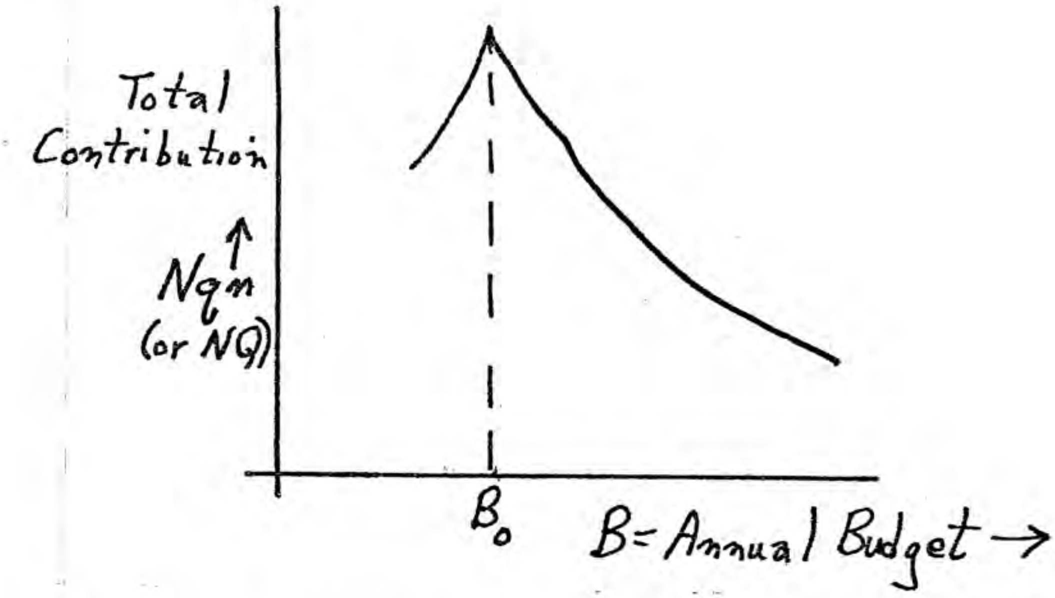
Fig. 4



asymptote at B_o is the budget that would not require spending any capital; ranch income, cash flow from the endowment, capital gains, and regular annual giving would just offset expenditures, including the growth in expenditures by inflation. We can assume that the Trustees will push this curve as high as possible by sound investment management and aggressive fund raising. However, there is a limit to how high it can go. The curve will also depend on policy: One curve would occur if policy were to hold on to the ranch after educational operations ceased in the expectation that capital could be accumulated and some decades later the institution could begin functioning again. A different curve (higher) would occur if the ranch were mortgaged (the school buildings and most of the faculty cottages could probably not be mortgaged) in order to continue as long as possible but with no hope of starting again.

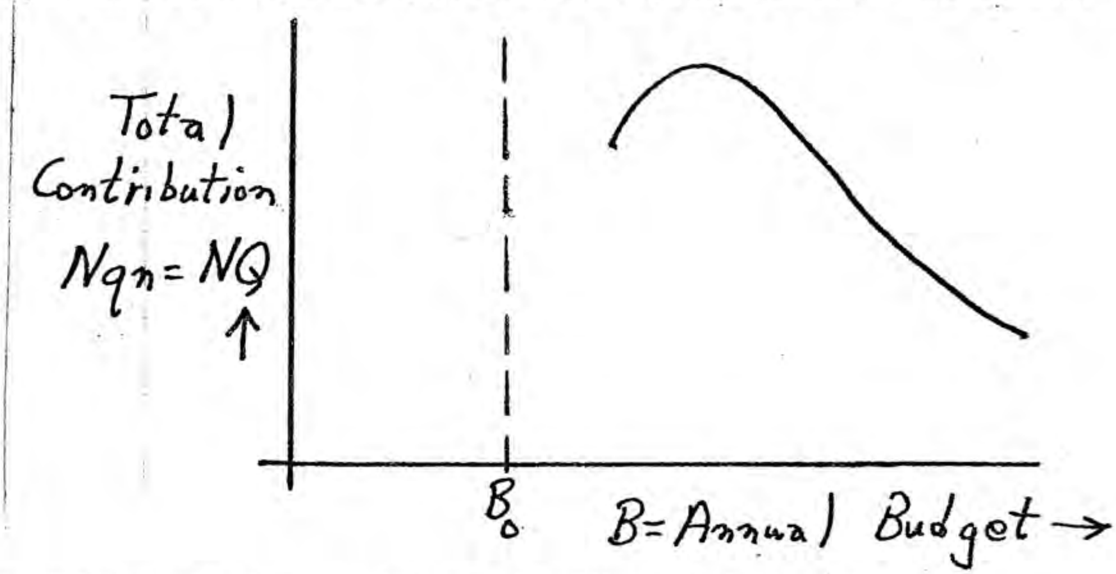
Now the final curve is the total contribution—not per year but for the life of the institution. There are several possible shapes for this curve. Figure 5 shows what the Trustees have decided is <u>not</u> the case. Here the loss of quality as the budget is reduced to B_o (the budget required for infinite life) is not too great, and the optimum point is B_o.

Fig. 5



The shape of the curve that is tacitly assumed in the Trustee policy is shown in Figure 6. The assumption here is that an annual budget significantly in excess of B_o produces the greatest total contribution;

Fig. 6

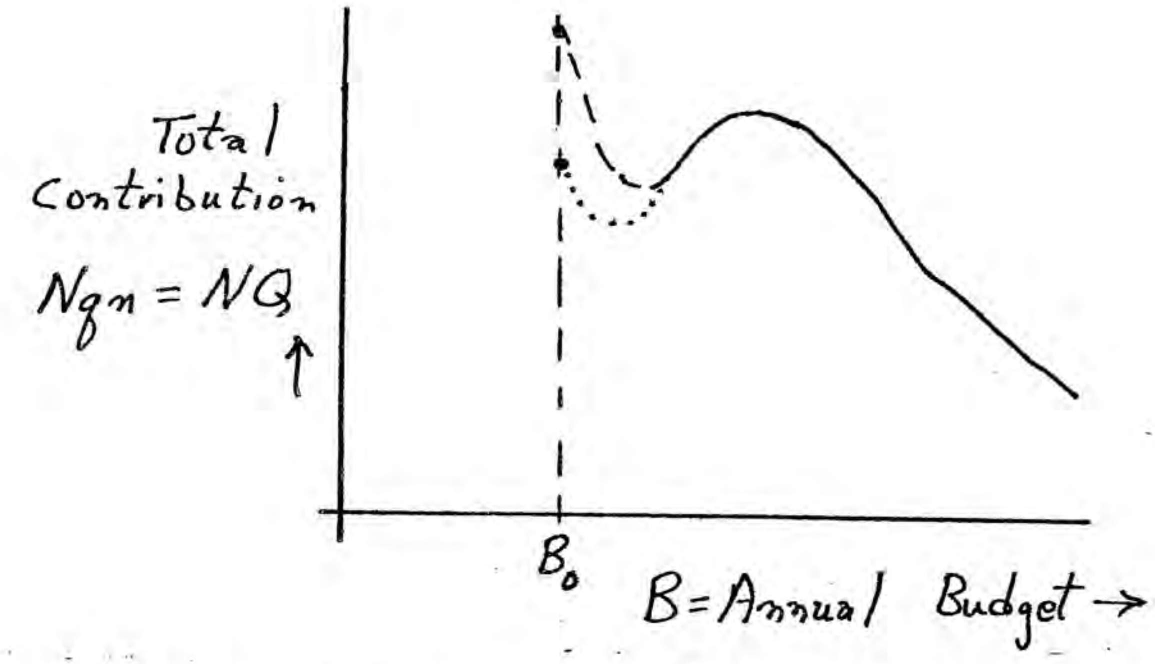


although the lifetime is shortened for each additional dollar of annual budget, it is assumed that for a while as the budget is increased the quality improves so much faster that the total contribution climbs. Of course, eventually it must decrease, and therefore there is a maximum. The effect of Figure 4 on Figure 6 is to multiply larger budget values by smaller numbers, thus providing a strong incentive to keep expenditures down, much stronger than in a conventional institution with infinite life.

We suspect that most would agree to the preceding paragraph. The disagreement comes in the quantitative playing out of consequences, and all depends on the precise shape of the curve in Figure 6, particularly on where the maximum lies. Each person will have a different Figure 1, since he will evaluate the elements of quality differently. There is less room for differences about Figure 4 since it is dominated by the Mr. Macawber realities of spending beyond one's income; differences can still occur, however, because one person may estimate gifts conservatively and another may count on being bailed out by some spectacular windfall. The maximum in Figure 6 (the product of Figures 1 and 4) thus occurs at different values of B for different people. It is important to realize that if X puts his maximum to the left of Y's, that does not mean that X is more of a philistine or less devoted to a quality institution than is Y. It is also important to note that if Deep Springs operates at the annual budget to yield the maximum in Figure 6, everyone will be disappointed, since everyone's Figure 1 will reveal that a higher quality program could be obtained if B were increased.

The curve has not been drawn in Figure 6 near B_o because there are several possibilities. Something interesting is <u>certain</u> to happen at $B=B_o$, because (Figure 4) N goes to infinity at that value and infinities always provide some kind of excitement. The curve will almost certainly turn <u>upward</u> as $B \to B_o$ from the right to the left. Suppose, for example, one thinks of the quality (contribution per student) of an institution with $B=1.20\ B_o$, that is, a budget 20% beyond real income. Although <u>cutting</u> 20% can be a traumatic affair, especially if done in a single year, <u>adding</u> 20% is "no big deal"; it follows that if it meant enough to the institution (e.g. if it could convert a finite-life to an infinite-life institution), one would certainly find a way of moving from $B=1.20\ B_o$ to $1.00\ B_o$ (either by budget-cutting or fund raising or both). At $B=1.20\ B_o$ and a 10% yield on capital the lifetime of the institution is 18 years; at $B=B_o$ it is infinity. This argument shows the curve almost certainly ("almost" because someone could invent his own Figure 1 with some infinities in it, but that would be reaching hard) turns upward as in Figure 7. This leads to the fascinating possibility that the new local maximum in the curve (at $B=B_o$) might be higher than the first. We leave the speculation on that possibility to the reader.

Fig. 7



If the reader has learned his economics at the Universities of Chicago or Rochester, he will object that the infinity so important to Figure 7 goes away when one converts everything to "present values." But this leads us into a deep philosophical jungle, since these economists also believe that man lives by bread alone and since their institutions would not exist if their predecessors had been of their persuasion. It should also be noted that this infinity is <u>not</u> as nebulous as the one in Bacon's "Infinite Conjecture." To prove that, one simply substitutes some large finite value (e.g. 100 years) for the infinity, and the analysis proceeds almost unchanged.

Appendix B

The following are some excerpts from Youth: Transition to Adulthood, Report of the Panel on Youth of the President's Science Advisory Committee, Executive Office of the President, June, 1973 (Superintendent of Documents, Washington, D.C. 20402, Stock Number 4106-00037, $1.60). Of course no excerpting can substitute for the full report. But the quotations here, which are all from the "Summary," represent fairly the burden of the report; indeed, the first excerpt is almost a summary of the whole report.

"The dominant institutions for youth at present are high school and college, replacing work settings in which youth was spent in the past. Thus it is useful to say that society has passed through two phases in its treatment of youth. In the first, which might be characterized as the work phase, young persons were brought as quickly as physical maturity would allow into economic productivity, to aid the economy of the family. In the second phase, which may be described as the schooling phase, young persons are being kept as long as possible in school and out of economic productivity, to increase their opportunity.

"We believe it is now time for a third phase in society's treatment of its young, including schooling but neither defined by nor limited to it.

In this third phase, the environments within which the transition to adulthood take place have broader objectives than those of school. The objectives are in two classes: self-centered objectives of acquiring skills and knowledge; and objectives relating to responsibilities affecting other persons. Schools have traditionally limited themselves to the first of these classes, and within it, to cognitive skills. The report argues that environments for youth should encompass all the objectives. Further, it argues that the necessary modifications of youth environments are in the very institutional structure: that widely different objectives require different institutions, and the school is not adequate as a pervasive environment for all these objectives.

"The objectives are:

In the first class of self-centered objectives—

1. Cognitive and non-cognitive skills necessary for economic independence and for occupational opportunities.
2. Capability of effective management of one's own affairs.
3. Capabilities as a consumer, not only of goods, but more significantly, of the cultural riches of civilization.
4. Capabilities for engaging in intense concentrated involvement in an activity.

In the second class of objectives involving responsibilities affecting others—

1. Experience with persons different from himself, not only in social class and subculture, but also in age.
2. The experience of having others dependent on one's actions.
3. Interdependent activities directed toward collective goals.

And finally an objective that is not wholly in either class, the development of a sense of identity and self-esteem."

(pp. xv and xvi)

". . . The first of these issues is segregation from adults vs. integration with adults, an issue that has been resolved in the former direction as a by-product of the growth of schooling. This segregation facilitates the specialization of activities in society, but it inhibits the experience of youth in incidental activities that form everyday life, and thus the learning that accompanies those activities. Balancing the benefits of segregation and integration, we feel that the benefits of integration of youth and adults far outweigh those of segregation, and that new institutions should move in the direction of such integration."

(p. xx)

"A fourth and very important issue concerns the patterning in time of self-development or learning activities and productive activities.

Through the existence and prevalence of formal schooling, this issue has been resolved in a different direction from its resolution in the past societies, where there was an intimate intermixture of learning and productivity during youth. In modern American society the issue is resolved in the direction of continuous attention to self-development truncated by a shift at the end of schooling into productive activity. There is increasing evidence that this pattern is not the best one for all youth, and perhaps is best for none."

(p. xxi)

Appendix C

The basis of the three exhibits are the audits which Deep Springs has received from its independent CPA's. The only exception is for the fiscal year ending May 31, 1980. Some of the figures used for that year were those supplied by Director Cronk in June 1980 which do not differ substantially from the final audit.

Fiscal 1973-74 was chosen as a starting point since in that year expenditures and income were in balance. These exhibits are as follows:

Exhibit I - total disbursements with all ranch income netted out;

Exhibit II - source of money for expenditures expressed in percentages; and,

Exhibit III - net worth.

The audit as of May 31, 1979 showed an operating deficit of (\$784,804). This figure was reduced by a significant surplus for fiscal 1979-80 to (\$693,960).

Exhibit I
Total Disbursements
(Ranch income has been netted out.)

1974*	$124,605 (Cattle receipts $35,523)	(surplus $232)
1975	$155,294 (Cattle receipts $27,414)	(deficit $1,041)
1976	$142,376 (Cattle receipts $19,357)	(deficit $19,419)
1977	$198,738 (Cattle receipts not separately stated, probably $42,000)	(deficit $61,868)
1978	$199,627 (Cattle receipts not separately stated, probably $33,000)	(deficit $68,335)
1979	$152,268 (Cattle receipts not separately stated, probably $90,000)	(surplus $64,534)
1980	$168,442 (Ranch gross income $118,800)	(surplus $90,844)

---

* Fiscal year ending as of May 31.

Exhibit II

Source of Money for Expenditures

Expenditures	1974	1975	1976	1977	1978	1979	1980
Cattle	18.5%	15.0%	11.8%	17.4%	14.2%	42.5%*	26.6%
Contributions	55.3%	45.3%	42.5%	34.4%	31.8%	22.0%	26.7%
Investments	26.2%	24.2%	29.0%	20.1%	23.3%	35.5%	46.7%
Capital	-0-	15.5%	16.7%	29.1%	30.7%	-0-	-0-
	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

* Surpluses in 1979 and 1980 have been credited so as to reduce the accumulated operating deficit. In the above table surpluses were assumed to be from ranch income.

Exhibit III
Net Worth*
(at close of fiscal year)

1974	$1,194,312	(inc.-equipment & plant $444,000)
1975	1,192,411	(" $444,000)
1976	1,269,599	(" $444,000)
1977	1,476,635	(" $444,000)
1978	1,531,134	(" $444,000)
1979	1,662,191	(" $447,652)
1980	1,772,507	(" $451,801)

---

* The audits have the following footnotes:

Note 1 - Inventories

No adjustment has been made to reflect May 31, 1979 inventories of livestock, from products, or supplies.

Note 2 - Property, Plant, and Equipment

Land, buildings, and equipment are stated at the valuation designated in the Deed of Trust dated November 5, 1923 with certain subsequent additions at cost. No provision is made for depreciation, and all normal renewals and replacements have been charged directly to current operating expenses.

Note 3 - Investments

Investments are stated at cost, if purchased, and at fair market value on date of acquisition, if donated, increased by realizable earnings to date of May 31, 1979.

Appendix D

There are many ways of dividing the whole field of accounting, but for our purposes the following division is most revealing.

First, there is accounting for the purpose of proving that no chicanery is going on. This kind also can be used to prove to outsiders that the stewards of resources are husbanding them properly and that gifts are being applied for the intended purposes. It also can discourage for some time and to some extent would-be cheaters and thieves. It must be accurate to the penny, but the classifications, purposes, and descriptions of the individual accounts are not always important. Deep Springs has always had this kind of accounting, and for all we know it is perfectly adequate for its intended purposes.

Second, there is accounting for the purpose of management. The numbers here need not be accurate—for small items $\pm$ 10% may be perfectly acceptable—but the descriptions of accounts, the proper aggregation of costs and assignment to functions, the accrual of obligations and incomes in the proper periods, and the separation of capital and expense are vital. Deep Springs has never had this kind of accounting. At various times people have tried to create it from the first kind, using the backs of envelopes, but different people get different answers (especially when cattle and hay are involved) and after the immediate problem goes away the exercise is forgotten.

Third, there is accounting for <u>planning</u>. This is just like the second kind but has the additional feature that the labels, definitions, semi-arbitrary decisions, and aggregations remain substantially constant over periods of five or ten years. Deep Springs does not even suspect the existence of this kind of accounting.

Before any useful long range planning can be done, accounting of the third kind must be in place and seriously conducted for at least three years.

The Computer Science Department

Robert L. Sproull
13 October 2000

The Computer Science Department was born on 22 May 1974. It had been proposed to the Executive Committee of the Board of Trustees in 1970 and woven into the Five-year Plan submitted to the Board a few months later and into all subsequent plans. But the Department was going to be created not by Trustee Minutes but by the genius of the founding Chairman. Trustee blessing was appropriately delayed until the leadership of the new Department was secured.

I now proceed on the assumption that a short account of the pre-history of the Department is of interest to you, although some of you know more about it than I do.

In the early 1960's several Rochester faculty began agitation for a department. It was already late. In 1964 I learned by visiting MIT with J. C. R. Licklider of fascinating developments there and at other campuses, notably Stanford and Carnegie Mellon. (Incidentally, Lick deserves great credit for extending the computer in many ways beyond the massive batch-processing typical of 1964; his imaginative contributions seem to be a well kept secret, as is the fact that he earned a Ph.D. in Psychology at the University of Rochester.) These earlier departments were all different--some derived from mathematics departments, some from engineering schools, and some independent.

At Rochester the promotion was not only late but was complicated and weakened by the differing views of what a department should be; many wanted merely augmentation of the modest Computing Center and assistance in the design and support of departmental computing. Many doubted that computer science was a legitimate field or discipline. The excessive promotion of the non-field of "space science" made others nervous. There was no champion who would bet his career on a new department. And, of course, there was a general reluctance to have one more mouth to feed within increasingly severe budget constraints. No dean wanted it badly enough to provide for it. The central administration was pre-occupied with keeping the peace and maintaining an open University.

Nevertheless the Advisory Committee for the Computing Center in 1964 recommended creation of a graduate program in computer science. But the fourteen signers ended their report with the statement: " . . . we suggest that this matter be referred to the cognizant individuals and faculties of the University for study and recommendations." I believe all of you are familiar enough with the academic ballet to predict that a proposal with this concluding sentence was not going anywhere!

Interest revived in 1969 primarily through the initiative of two champions, Hugh Flynn and Herbert Voelcker. Others joined. Deans Loewy and Meckling, albeit with somewhat different concepts of a department, urged the Provost to appoint a committee; he did so immediately and provided resources for a committee chaired by Flynn and with Voelcker as a prime mover and shaker. On 1 November 1969 an all-day meeting featured talks by "outside" consultants Bob Ashenhurst, Jerry Haddad, Juris Hartmanis, Tony Oettinger, Bill Miller, and Saul Rosen. They testified to and illustrated the remarkable richness and open-endedness of computer science and cut out the ground from under the skeptics. The committee's report of 6 January 1970 became the tentative charter of the Department and it was the document presented to the Trustees in February 1970 and May 1974.

The proposal to the Board said "We very early decided that we would demand energetic, capable leadership for the Department before we set it up, and we knew competition was keen in this rapidly developing field." Recruiting was conducted vigorously. In 1971 the Provost responded to an inquiry by writing "Herb Voelcker is, more than any other single individual, carrying the ball for the new Computer Science Department ." In January 1974 Flynn wrote that there was a ". . . possibility that Jerome Feldman of Stanford might be interested. . . .He has been rated very highly by our consultants." Jerry had been in Israel in 1970-71. Although one faculty member (not named in this talk) had volunteered to interview him in Israel (if first-class travel was sponsored!), Jerry's name could not be on the list of those to be immediately explored, and the systematic working through of that list took several years. In the end, no harm was done, by May 1974 Jerry had agreed to come, and the delay enabled him to bring with him the nucleus of a strong Department.

This ends the pre-history of the Department. I should like to add two notes.

The rapid development of the department was powerfully facilitated by grants from the Sloan Foundation and others. Now foundation money is especially **precious** when adding a unit to the University since envious competitors for budget dollars do not perceive it as stolen from them. Jerry became highly respected by the Sloan Foundation, "paying his dues" by helping them with important programs, and by the end of 1974 he already was granted $400,000 for the development of the Department.

One final note to tie together the pre-history and the future: In 1964 John R. Pierce was chairman of a White House committee on computing appointed by the President's Science Advisory Committee. His report opened with the following prescient sentence: "After two decades of unprecedented development, the computer is approaching its infancy." Now, in the year 2000, we can see not only how accurate that gutsy prediction was but also how this field, no longer infant but by no means tired and old, holds great promise for the future of the Department of Computer Science.

THE STRANGE WORLD OF NUCLEAR DETERRENCE

Robert L. Sproull

Wednesday Evening at the University Talk

18 April, 1984

Many German writers employ the structure of what they call a "Rahmspiel" in telling stories. They encase the story itself, the "Spiel," in a frame, the "Rahm." I need to follow that structure tonight, partly in order to get my message across with more confidence and partly to avoid being stoned. I shall therefore take a few minutes of your time at the beginning and the end to set my major message in a protective frame.

My own special interest in the avoidance of nuclear war began, as it probably began for all of you of my generation in the audience, in the announcement of Hiroshima in the summer of 1945. Unlike many of my colleagues, I had not "known sin" (in J. Robert Oppenheimer's words) by being involved with the atomic bomb project during the war. I did become a founding member, however, and minor official in the Federation of Atomic Scientists which started out by working to secure the civilian control of atomic energy in the face of a strong lobby for military control. I was deeply involved in avoiding nuclear war in connection with the partial nuclear test ban of 1963 and my service in E-Ring of the Pentagon and later as Chairman of the Defense Science Board.

I first surfaced the ideas I am going to talk about tonight at a technical conference of the Pugwash group in 1970. To say that my presentation received a cold reception is the understatement of the year; most of my remarks did not even survive into the printed record of the meeting. In the fall and winter of 1981-82 I became so concerned about the MX controversy and the way it was diverting attention from the real problem that I gave a talk very much like the one I am giving tonight to two of the discussion groups in Rochester.

I report all this not in any sense to establish a priority but only because I want it to be apparent that the burden of this argument antedates President Reagan's speech in March of 1983. You will find a good deal of similarity in what follows this evening to that speech, but not to the way the speech was reported in the newspapers or to the subsequent interpretations by critics and actions by the Pentagon.

The next piece of the frame is some comment on the literature of this subject. Avoidance of nuclear war is easily the most complex question of our times, a question which is very hard to decompose into individual pieces in the classic approach that has been so successful in science. It is thus no accident that the television tube is completely inadequate (in the idiom of the engineers it would be called a poor "impedance match"), and it is virtually impossible for that medium to contribute any understanding. The newspapers are only a little better, but of course in print one can go back and reread things. But the complexity is such, and the need for illustrations and even a calculation or two is so great, that the newspapers and national news magazines are only a little better than the tube.

There is, however, a burgeoning literature in the more elegant national and international magazines, and for the most part it has been honest and helpful. My own favorite source is the Scientific American which permits its authors the use of diagrams, graphs, and photographs and yet is still accessible to any educated person. The Scientific American has been careful not to become the prisoner of "hawks" or "doves," and in general it has been very balanced. On the other hand, the breadth and complexity of the subject are so great that each Scientific American article can be, like all others, only a one-dimensional approach, for example, an article on the sea-based deterrent. The sum of all of its articles and of other articles does, however, create a very large and important literature.

In the four issues of the New Yorker magazine which appeared in the month of February were four long articles by Freeman Dyson. These set a new and much higher standard in all of this literature. In my experience, this is the first broad gauge, multi-dimensional approach to the whole question. These articles have now appeared in book form, and I strongly urge everyone interested in the survival of the planet to read them. I will go farther: If you are so pressed for time that it becomes a choice between staying here this evening for another 40 minutes or so and listen to me and investing 40 minutes in beginning to read Dyson, by all means do the latter.

There are two final pieces of the frame: First, I must acknowledge that it is dangerous to embark on a talk like this. It has been my experience that when you discuss these things, the listeners frequently conclude that somehow or other you tolerate nuclear war. I do not know exactly how this comes about, but I think that you will agree that the effect is real. In a way it is what Hermann Kahn was saying in the title of his book Thinking About the Unthinkable. It is somehow considered more moral and less callous not to think about it. I do not suppose it will help, but let me say anyway: Any nuclear war at all would be an unmitigated disaster. A speaker who gives the ritual recital of horrors somehow stakes out a high moral position, but you can recite them as well as I and I shall not take your time to do so. But I believe them all. Simply stated, there is no point in universities, children, or any other future-oriented activity if there is a substantial possibility of nuclear war.

For the final piece of the frame, let me deal with that word "strange" in my title. If you do not believe that the world of nuclear deterrence is about as strange as it can be, let me cite two examples from the not too distant past. When the Peoples Republic of China was unfriendly, there was a serious proposal to give them a few ballistic missiles with nuclear armament. The idea was that the Chinese would be less dangerous if they had the possibility of annihilating a few American cities. They would not have to feel as frightened of the United States, and time could be gained to work out relations. Another proposal at about the same time was equally strange and equally serious. The suggestion was that one "mine" the major cities in both the United States and the Soviet Union. Nuclear weapons would be installed under these cities, and crews from the opposing nation would be implaced with the weapons. Upon command from their parent country, they would blow up the city in which they resided, and themselves of course. This would accomplish exactly the same thing as a nuclear exchange, including killing the exchangers, but without the expense and uncertainty of rockets, aircraft, and submarines.

That is the end of the "Rahm," and here begins the "Spiel."

What I should like to do tonight are essentially three things: I should like to remind you of the doctrine of mutual assured destruction and what it involves. Some of this will include numbers, but not numbers of warheads or the complicated arithmetic of SALT. Next I should like to explain the problem of a defense against missiles with nuclear armament. Finally, I should like to suggest what seems to me must be done in the long run if there is to be any optimism at all. I shall stay away from such questions as to whether the MX is needed, how it should be based, and other controversies of the day. I am trying to take a considerably longer view than such questions.

What stability we have against a nuclear holocaust has been provided for more than thirty years by the concept of mutually assured destruction (I have already prepared you for the fact that its initials are MAD). Any of the nuclear powers must face the likelihood that if it uses nuclear weapons, its cities will be quickly devastated by a "secure second strike," that is, a barrage of nuclear weapons that have survived the initial (aggressive) strike. This deterrence so created, which is fundamentally a state of mind, has a host of technical and non-technical problems which we shall explore shortly, but meanwhile I note the intrinsic fragility of such a concept, vulnerable as it must be to fluidity in the internal structures of countries and even to the mental balance of a single person. A further difficulty with it is that the Soviet Union enjoys the capacity for assured destruction of the West but uses different rhetoric to justify its nuclear force.

The secure second strike capability for both the United States and the Soviet Union consists of a triad of nearly independent forces. I shall describe the

United States forces, concentrating on their vulnerabilities, the reasons why they may not be able to accomplish their missions, since it is talk of these vulnerabilities that saturates the newspapers and worries over them that impel programs with price tags in the hundred billion dollar range. The Soviet Union forces are similar enough that I shall not describe them separately, although the differences are fascinating.

The land-based part of the triad is primarily Minuteman III, based in underground silos in the North Central U.S. For many years we have been confident that these missiles could survive a massive Soviet attack and be ready when their blast covers were opened after such an attack to inflict unacceptable damage on the Soviet Union. In recent years, two causes for doubt have crept in: 1.) The accuracy of Soviet missiles has steadily improved, and a single Soviet Union warhead now seems capable of immobilizing a Minuteman with something like a 90% probability. 2.) Soviet missiles have 6 or 10 separately targeted warheads and are capable of handling 20; a few dozen Soviet missiles could, with very high probability, wipe out the Minuteman force. (MX was conceived as a less vulnerable force in specific response to this threat.)

The air-based leg of the triad has been primarily the fleet of B-52's, large subsonic bombers each typically carrying four nuclear bombs. As air defenses have become much more capable, the confidence that B-52's could successfully penetrate the Soviet Union has declined. A variety of responses has been suggested of which the two most substantial current parts are developing and deploying the

B-1 bomber, which is faster and more agile in every way, and arming B-52's and other aircraft with cruise missiles, so that the manned aircraft could "stand off" hundreds of miles from land-based air defenses and fire long-range missiles that would penetrate the Soviet Union at low altitude (to be hard for radar to detect) and with less risk to aircrews. In addition, there is now a good deal of talk about a "stealth" bomber that would have especially low radar returns and infrared emissions and therefore might be able to penetrate the Soviet Union with less chance of detection.

The sea-based leg consists of a fleet of nuclear-powered submarines armed with Poseidon or Trident missiles. Two-thirds of these can be at sea at one time and can remain submerged for weeks if necessary. They are difficult to find, especially since the long range of the Trident missile means they could be almost anywhere in the Northern Hemisphere (and many places in the Southern) and still threaten Soviet cities. Here, too, there are new vulnerabilities developing, of which the most imminent may be the enormous expansion of the Soviet Union submarine fleet, a good part of which seems intended for antisubmarine warfare.

Each leg of the triad has some fascinating technical problems. In assessing the vulnerability of Minuteman, some of the questions result from the problem of firing an attacking missile at a Minuteman silo from 5,000 miles away and guiding it to within about 100 yards of the aim point. Questions arise about the mass distribution of the earth (not really a sphere), the accuracy of satellite photographic mapping, atmospheric density variations and winds, accelerometer and gyro accuracies, and many more. Geologic questions concern the coupling of a nuclear blast to the soil and rock to damage a silo. If retaliation is to be quick, questions arise of the effects on our launch of the electromagnetic pulse and the cloud of debris generated by the attacking warhead. And many more.

The air-based deterrent has a similar array of technical questions. Many of these are of the operations-analysis type. In a crisis a large fraction of the B-52's plus aerial refueling tankers would be airborne at all times. Dealing with various levels of crises, dealing with feints and false alarms, and managing the whole enterprise constitute an extremely complicated operation. Unlike launching the Space Shuttle, all of these must "work" the first time!

The technical problems with the sea-based deterrent are perhaps the most interesting of all. Secure communication with a submerged submarine is an enormous challenge; sea water is electrically conducting which limits use of radio to extremely low frequencies, and it scatters and absorbs light. Although fancier methods are conceivable or even possible, submarines are usually detected by their magnetic anomalies (the change in the magnetic field of the earth when a few thousand tons of iron are present) and by sound detection (sonar and listening to the submarine's machinery). If the ocean did not have variations in temperature and salinity, both of which refract sound waves, an array of acoustic receivers could locate all the submarines in (say) the North Atlantic accurately enough to annihilate them by a first nuclear strike (or possibly even by "conventional" weapons). The study of these variations and the performance of acoustic arrays thus has tremendous importance.

Questions like these have prompted the proposal of a new, very expensive missile system, the MX, and dominate the arguments over how to base it. But there are two kinds of questions that seem to me to be far more consequential than whether MX is "needed," which boils down in the end to whether the two legs (air- and sea-based) are sufficient if Minuteman cannot survive (the remaining two legs are, after all, still a "belt and suspenders" approach). The first of these is the complicated set of questions surrounding on both the Soviet Union and United States sides the "pushing of the nuclear button." These are the structure and stability of the governments, the states of minds of the participating officials, the ambitions and alternate lives of these, the popular wills and communication (if any) with the peoples, the reliability of military orders under unprecedented conditions, and similar questions.

The second is the gap between the actual capability for inflicting unacceptable damage on the other country and the capability that country perceives; even our own perception of our own capability may be wrong.

In the face of these enormous uncertainties, the creation of a new weapon like MX seems relatively unimportant. One remains surprised that deterrence has worked at all. One can only surmise that the variety of different scenarios, personalities, and crises in the last thirty years was such that we did not go over the brink. As the decades and centuries roll on, this variety is virtually certain to increase, and the fragile "balance of terror" seems certain to become even more precarious. The MX seems to me to be not so much wicked as irrelevant; like recommissioning the New Jersey, it is a noisy way of spending money and appearing to "do something" about National security.

Similarly, I believe all of the talk about a nuclear "freeze" is irrelevant. However nice it would make us feel to be "doing something" to halt the expansion of the weapons of terror, it hardly matters whether the superpowers have 10,000 or 20,000 warheads poised to throw at each other. Like the MX and other expansion of weapons systems, the freeze (if one can use that metaphor for a boiling pot) owes more to our intense frustration than to thought.

Creation of a new weapons system like the MX or establishment of a nuclear freeze would give a signal of the way our country is turning. This has been a large part of the argument by the proponents of both causes. But I believe far more than such symbolic moves is called for, and I have never had much confidence in symbolism when the future of civilization is at stake.

If we are overwhelmed with frustration and hopelessness, why not develop a ballistic missile defense so that the defense has the upper hand? Why is the defense against nuclear weapons so difficult? Why is it so hard to conceive of a defense that would dominate the offense, so that each nation could feel secure within its own borders, without worrying whether potential opponents increased their offensive capability?

The problem of defense, though marvellously ramified and rich in technical questions, centers on time. From launch in the Soviet Union until impact in the United States, a missile requires just 30 minutes. It is helpful to divide this short period into three phases, and I shall do that with the help of the first slide. (Slide 1)

First, the "boost phase," when the missile is being accelerated upward through the atmosphere from its launch silo: The missile, containing the warheads, is extremely vulnerable during this phase. It is large since it is still attached to its "tanks," the large propellant containers; the Soviet SS-18 and the MX are both about 8 feet in diameter and as tall as a 7-story building. As it comes up through the clouds it and especially its plume of burning gases are easily seen and recognized by satellites. But the launch could be of a benign earth satellite or an unarmed test, and even if armed, the missile might be intended for a country other than the United States.

But if a United States-Soviet Union crisis is underway and especially if dozens of launches occur almost simultaneously, our satellites would give a pretty unambiguous message that these missiles should be shot down. But how? There is not time to intercept them in the few minutes of boost phase by sending a missile from the United States or even a cruise missile from a stand-off bomber (if we somehow could manage to have such bombers ringing the Soviet Union at the time of launch). A laser weapon on a satellite, permanently stationed over launch sites in the Soviet Union is a future possibility. The power requirements for such a device are horrendous; if it is to attack a large barrage, the requirements become virtually impossible, as explained in the December, 1981, Scientific American. But there are some highly promising possibilities, including ground-based lasers sending beams to large, sophisticated reflectors on satellites. Of course the satellite would be vulnerable to Soviet attack and would have to be effectively protected. But the boost phase is probably the last chance to intercept the missile without using a nuclear-tipped interceptor; if we made a mistake, we might be able to apologize and walk away from it, and that is an unlikely outcome if we initiated a nuclear explosion.

The second phase is "mid course," the region above the atmosphere when the missile is on its way across the polar regions between the Soviet Union and the United States. The missile reaches the top of its flight path in about 20 or 25 minutes. I say "the missile" but in actuality it is a collection of objects: the warhead or warheads (the only dangerous objects, each about the size of a large wastebasket), "tankage" (the huge containers for propellant, now spent), a "chaff cloud," an extensive blizzard of tiny metal foil pieces which reflect radar waves strongly and make it difficult or impossible for radars to "see" the warheads, and a variety of decoys, light objects designed to look to radars as if they were warheads. This assortment of objects is moving through an almost perfect vacuum at a few thousand miles per hour. Since all of the early process of acceleration and guidance has by this time been accomplished, the warheads are just "coasting;" they are heavy, compact objects not easily damaged. Even a nuclear interceptor creates only a moderate blast in the vacuum, and although γ-rays, X-rays, and energetic neutrons can also "kill" a warhead, the chaff cloud may prevent aiming the interceptor close enough to destroy warheads. Incidentally, Greenland and Canada are the only promising places to base the interceptors and the radars to control them, and these bases (if created) would be the obvious first targets for a Soviet missile barrage.

Finally, there is the "terminal phase." As this medley of objects re-enters the atmosphere, the chaff particles stop almost immediately, the tankage burns up, and the warheads emerge. They are probably still accompanied by decoys, however, and some of these may be highly sophisticated foolers. There now remain only three or four minutes to discriminate (by sophisticated use of radar) between warheads and decoys and to fire interceptors, which probably must themselves have nuclear warheads. This whole process gets pretty complicated if hundreds of warheads are coming at us at once; interceptor exhaustion, computer saturation, dust, electromagnetic pulse, and interceptor fratricide are some of the problems. It is not, however, out of the question to gain some protection of Minuteman silos in this way. Protecting cities is much harder, I believe virtually impossible against present arsenals of weapons, partly because of the large areas to be protected and partly because of the fallout from exploding interceptors and "salvage-fuzed" attackers.

The time scale is forbidding enough - a half-hour total - for the land-based Soviet missiles, but it becomes even worse for submarine-launched ballistic missiles ("SLBM's"). For these the total time from launch to impact might be only four or five minutes. Furthermore, the threat to a city like Atlanta could come from any direction from northeast through east and south to southwest, which makes siting radars and interceptors more difficult.

At present we have a treaty with the Soviet Union which prohibits deployment of a ballistic missile defense beyond the very minimal capability existing in 1972. The purpose of this treaty was to avoid the pressure for increased numbers of attacking warheads, to be confident of inflicting unacceptable second-strike damage, that would exist if a powerful ABM (anti-ballistic-missile) system were created. I believe the treaty was a proper and helpful move at that time. I believe that deployment of an ABM system now would also be a bad mistake.

Where does all this leave us? We have tried to agree with the Soviet Union to limit numbers and sizes of offensive nuclear weapons, and SALT-I did produce such limitations. That treaty, SALT-II which became stalled by the Soviet Union's Afghanistan adventure, and the conceptual SALT-III and START talks all have limited objectives: They serve to limit only the costs of strategic forces. The balance of terror remains, and with respect to the exasperation and fear by "third countries," it hardly matters whether the superpowers have 5,000 or 10,000 nuclear warheads ready to throw at each other (or at a third country).

We shall presumably have to go on fixing up, at enormous expense, our strategic deterrent whenever a new threat to it develops, such as the huge Soviet submarine building program or the increased accuracy of their SS-18 missiles. We shall presumably have to go on counting on enough stability and good sense in the Kremlin (and in the White House, too, for that matter) that the fragile and intricate deterrence works. Sooner or later, one would think, some accident, presumably of the human frailty variety, will occur that precipitates nuclear war.

Is there any hope for an alternative? I shall leave to others the assessment of the likelihood of world government or other non-technical means. Short of that, I see only one avenue of hope, and it is long, tortuous, and by no means certain of success. It has one enormous advantage: The moves we and the Soviet Union would be making to follow this path are moves we probably should make anyway and do not interfere with the (temporary, I fear) stability that the doctrine and hardware of mutual assured destruction provide.

What I have in mind is a combination of two moves, and neither should be undertaken without the other: 1.) A vigorous and wide-ranging program of research and advanced development (but not engineering development or deployment) of a ballistic missile defense system and an advanced air defense system. 2.) Renewed negotiations of the SALT variety, but in addition including airborne refueling tankers. Let me develop each of these two a little and explain how the combination would lead to a safer world than the balance of terror.

Deployment of a ballistic missile defense system now is quite properly outlawed by treaty. It would be ineffective in protecting cities and could serve only to stimulate deployment of more attacking missiles. But research and development on promising ideas, such as airborne (for terminal defense) or satellite-borne (for boost-phase defense) laser interception, are not forbidden and could very well ultimately substantially increase the defense capability. Incidentally, as I proposed in 1970, it would help greatly in air defense if we could achieve a treaty outlawing flight faster than sound, which would be an easy treaty to verify and would have the considerable incidental advantage of reducing everyone's cost of armaments.

Simultaneously, continuation of SALT-like talks keeps the dialogue going and gives both sides experience in the verification of forces. Of course, this depends (we hope) mainly on the Soviet Union, since cheating or repeated Afghanistans could stop the whole process; if it stops, we have not lost anything by trying but we are then left where we are, with little hope for improving safety against nuclear war.

Let us now put these two paths together at some point in the future, at least ten years away and probably twenty or thirty years away. Let us assume that sufficient progress in defense research has occurred that one could deploy a missile and air defense system that would have a high probability of protecting against a few dozen attacking missiles and nuclear-armed aircraft. (It is highly unlikely that one would be more successful, and invent a system that would work against thousands or tens of thousands.) Let us assume that sufficient progress in strategic arms limitation talks and sufficient confidence in verification of such treaties has occurred that a treaty could be established limiting the number of warheads either of the superpowers could deploy to (say) two hundred. At that time, it might be possible to make a sharp change in the SALT provisions, to permit deployment of defense and to outlaw an offensive force that was more than (say) twenty missile silos and twenty long-range (refueled) aircraft. [There is no magic, of course, in these precise numbers; the object is to have large enough forces to impress "third countries" but small enough to make ballistic missile defense possible.]

Admittedly any such scenario would involve a risky period of transition. Even if it were undertaken at an unusually calm period in international relations, it would provide an opportunity for adventurism by an unscrupulous power.

Let me attempt to illustrate this with some diagrams. I admit at the outset that I am here behaving like some economists (not ours, of course) : They draw curves that are concave upward, or concave downward, or have only x-intercepts, or only y-intercepts and frequently never tell us why they draw the curves like this; they then draw conclusions that depend on these properties of the curves.

First let me show the kind of diagram that chemists or physicists frequently draw when discussing the stability of a system. (See the second slide.) You can think of this as a ball free to roll on this surface, with the only force on it the force of gravity. If these "walls" go up infinitely high, this is a stable system; no matter how much you excite the ball, it stays in the "well." (Slide 2)

Next consider a conditionally stable well. Now if the ball is sufficiently excited it may bang about with such energy that it gets out of the well. Clearly we could deal with more than the single dimension illustrated here, but I cannot draw more dimensions on a flat piece of paper.

The stability of nuclear deterrence is of this second kind: It seems to work with the degree of excitation of international anger that we have so far experienced, but we are all pretty sure that with sufficient provocation, we would go "over the brink" into the nuclear holocaust.

A defense-dominated security would almost certainly be a deeper, more stable well, even though it still would presumably be only conditionally stable. The transition can be graphically illustrated as follows (see the third slide): We are in the well on the left, and if we can provide sufficient tranquility for a limited time in international tensions we may be able to make the transition to the deeper well on the right. Of course I must acknowledge that the "transition" wall might be higher than the wall at the left, and that would lead to disaster if an attempt was made to carry out the transition anyway. This transition will not be easy, but with another couple of decades of SALT experience it seems to me to be possible; I remind you, however, that unlike these schematic graphs, the real situation has many more than one dimension (many more than one route to chaos). (Slide 3)

How would we be left if we can do that? In brief, the world would be a much safer place. The superpowers could feel relatively safe within their national boundaries. They would still have enough weapons of mass destruction to impress third countries and to appease their national prides and possibly even their hawks. Third countries could feel somewhat less threatened by being caught up in a war of the titans, and I believe there would be a good chance that one after another of the smaller members of the "nuclear club" would give up nuclear weapons.

Why not "go all the way" and ask even the superpowers to agree to give up all nuclear weapons. Perhaps in ten or twenty years that could occur, but I think it is highly unlikely until many years after the limited, defense-dominated partial nuclear disarmament has been established and seasoned. The variety of countries and the flux of political figures and of governments are just too great to satisfy the superpowers that they would not become hostages to adventurism on the part of <u>some</u> country.

Before concluding I should like to acknowledge that there are many questions I could not address in a short talk. The interaction with third countries, the special worries of Europe, the implications for "conventional wars," and many others had to be left out. One complication, the possibility of the use of "tactical nuclear weapons" must be mentioned, however. In short, I believe the superpowers will have to come to the realization that there are no such weapons, that a strategic nuclear exchange (i.e., disaster) is certain to follow soon upon any use of nuclear weapons.

Also before concluding I should like to comment on President Reagan's speech of a year ago and the subsequent talk of "star wars." You will appreciate by now that I agree with his proposal of research and development leading to ballistic missile defense that would dominate the offense. You will also understand that I believe this will be impossible without success in strategic arms reductions, and that both will take decades (not just a few years) to accomplish. The press response to the President's speech implied that he was thinking of just a few years, but a careful reading of the speech shows that these criticisms were unfounded.

What has happened since last year is that research and development have quite properly been begun. When I worked up this talk a few weeks ago I wrote in a prediction that soon there would be demands for quick deployment of an ABM system. That was an easy prediction, and it has already come true. For example, both the lead editorial and another piece on the same page of the Wall Street Journal of 27 March 1984 (by a WSJ editorial writer) urge early deployment, even though they acknowledge that it would be an ineffective system. This kind of impatience is terribly dangerous and I want to be certain that you do not identify me with it.

In conclusion, let me summarize what I have tried to say:

1.) New weapons systems like MX and its cost and its basing get the headlines, but systems like MX are really a small increment in a very large problem. Similarly, movements like the "nuclear freeze" do not get anywhere near the heart of the problem. Both kinds of motion are interesting only as symbols of the direction of popular thought and will.

2.) The doctrine of mutually assured destruction has worked, in the sense that there has been no nuclear war, for three decades.

3.) But it is such a fragile doctrine that it seems highly likely that the fluctuations in governments, in people, and in nationalistic adventures will project us into a devastating nuclear war, perhaps not in another thirty years, but in a hundred, or a thousand.

4.) A transition from an offense-dominated to a defense-dominated mutual security would be safer in the long run and more tolerable to the "third countries" which will probably grow in power.

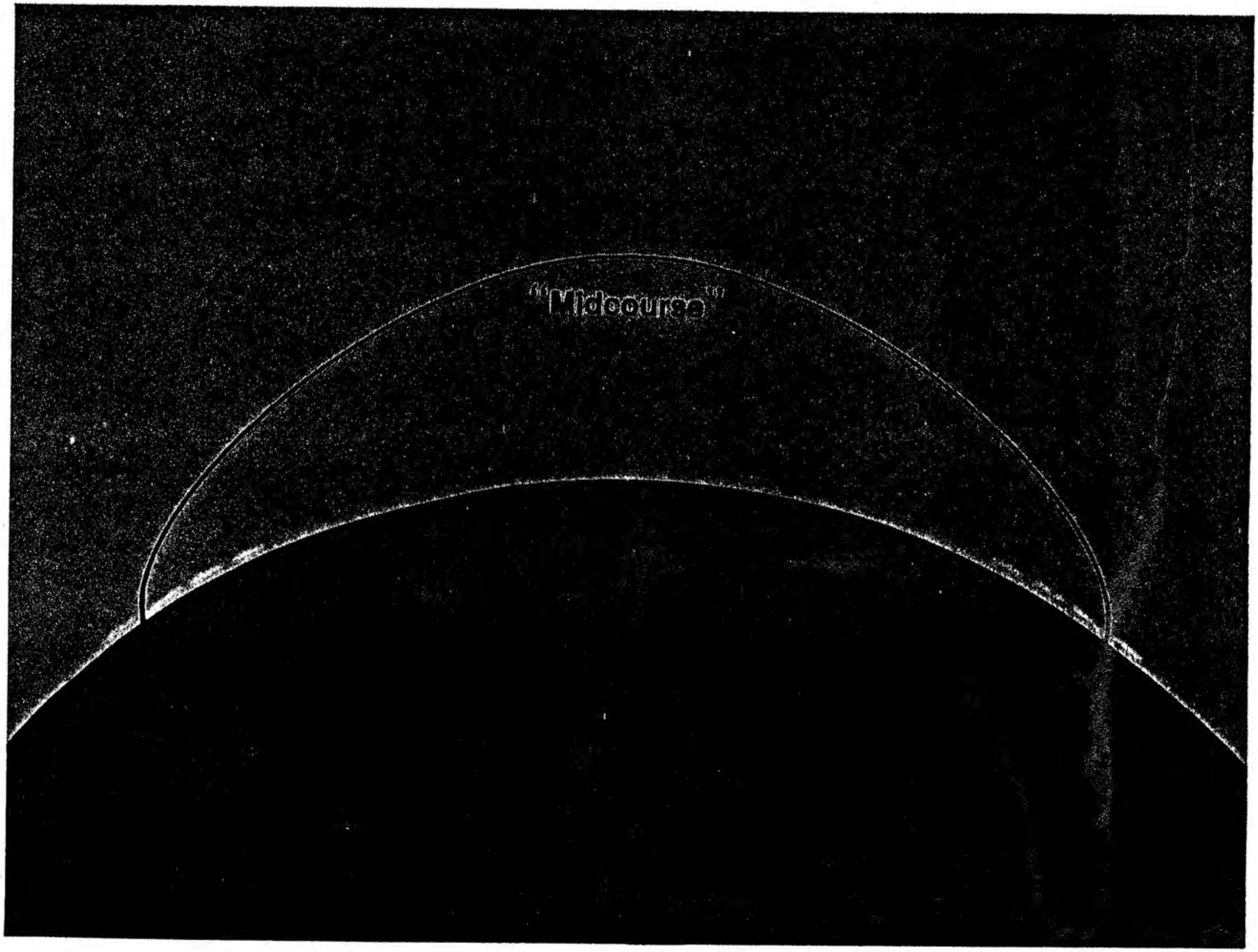
5.) Such a transition is not possible now, and any attempt to deploy a ballistic missile defense system before a drastic reduction of numbers of warheads has been accomplished would be dangerously destabilizing.

6.) But the research and advanced development of missile and aircraft defense systems coupled with intense effort on arms control are appropriate and promising moves and do not interfere with whatever security we now have. Furthermore, the atmosphere of such research and development, possibly even cooperative between the U. S. and the U.S.S.R., coupled with arms control talks would be a far healthier atmosphere in the decades before the transition could be made.

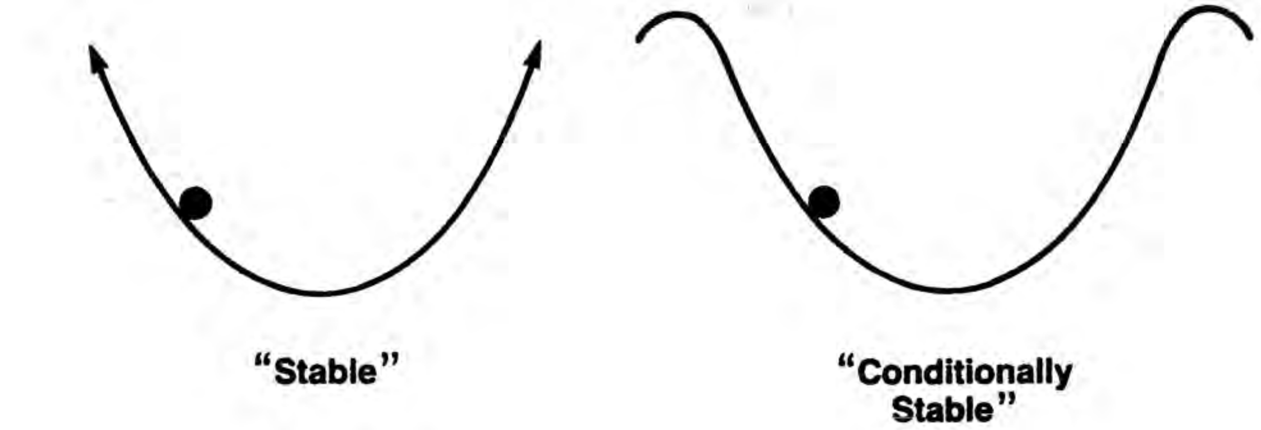
Finally (to retreat back into the frame, the "Rahm"), if you were to say that I have built up an elaborate, slow, and questionably effective program in order to have some hope that there will be a future, I guess I should have to agree with you. In defense, I respond that there is no hope in the path we are now following, and hope is essential to the human spirit.

RLS:jrc

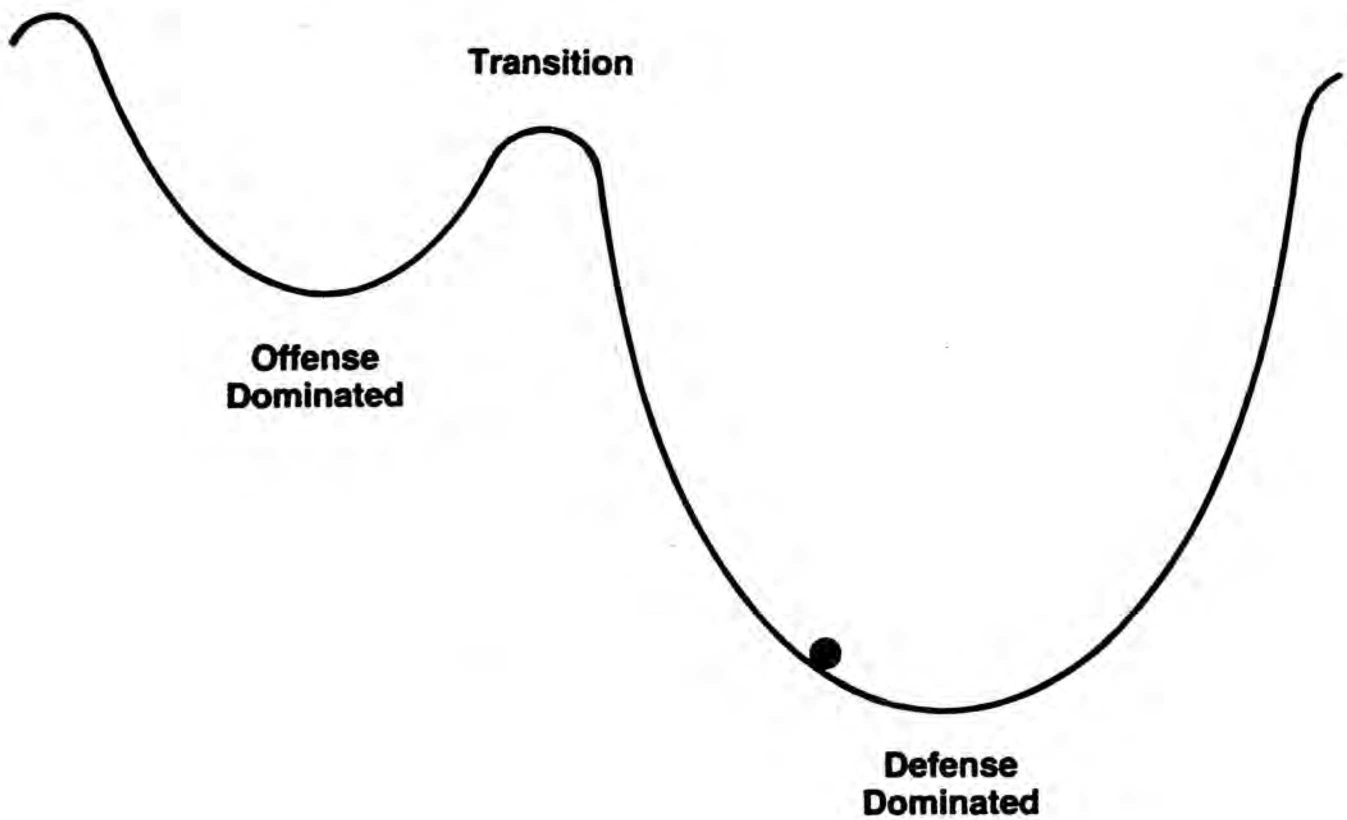
Revised: 19 April 1984



SLIDE 1



SLIDE 2



SLIDE 3

Some Principles of Bureaucracy

Robert L. Sproull
1979

1.) There is no such thing as "the Federal Government." There are, however, a lot of people housed in Government offices.

2.) It is a government of men (and women), not of laws. Everything is prohibited, and whether you are in difficulty depends on whether you are selected for enforcement.

3.) Everything that is not prohibited is required. Some things are both required and prohibited.

4.) No agency in writing a letter (laying on some requirement) recognizes that any other agency might have laid on other requirements. If reminded, it infuriates them.

5.) No agency in going to Congress or the Office of Management and Budget even knows what costs it is laying on the people, much less provides for them.

6.) Fear, of exposure by Jack Anderson or John Dingle, motivates agencies, not the drive to get something done.

7.) There is no collective memory. There are portraits on the wall of former agency heads, but no one knows who they are or what happened while they were there.

8,) There is no collective responsibility. "All our predecessors were schlemiels, we began to deal responsibly with the real problems." (Compare the State Department story of the last cable from the "old" ambassador and the first cable from the "new.")

9.) Never close the file on a complaint. If you hear from the complainant again, you can say that it is "still under investigation."

10.) Never acknowledge a mistake. Only citizens, who run up large bills for copying and postage when the wrong key is pushed in Washington, make mistakes.

11.) "Where knowledge fails place horrors" (the motto of Medieval cartographers). You may have a high school or junior college education, and anything foreign to that tunnel is suspicious.

12.) GS-13's must have something to do *next* year.

13.) If nothing happens, it has been a good day.

THE COMMONWEALTH FUND
Meeting of the Board of Directors
Tuesday, February 9, 1988
8:30 a.m. - 4:00 p.m.
The River Club

NOTES BY ROBERT L. SPROULL, Ph.D.
DISCUSSION OF THE FUTURE

I was invited to speak on "...the broad topic of the future for the U.S., and the trends that may have powerful implications for philanthropy and thus for the Fund; trends that appear to be important to bear in mind as we look ahead to the next five to ten years."

I will take a somewhat longer view; my rationale (excuse?) is that we will have to put programs in place in five or ten years to affect outcomes in 2000 or 2010.

My starting point is the end of World War II. Enormous buoyancy - "Can do anything." The expected depression did not occur. Overseas competitors were war torn and war weary. We had a powerful industrial plant and trained work force and had not lost a substantial fraction of our youth.

We built on that a commanding international position of military and commercial power. Agriculture experts sustaining, and manufacturing goods quickly powered the engine of our international trade. The invention of the transistor started a whole universe of solid state devices that made the computer practical, permitted artificial satellites and space exploration, and revolutionized communications. Almost as great a change in the years 1955-75 occurred as in the first 100 years of the industrial revolution. The "high tech" elements fueled the export machine.

This commanding position was used (here I am creating a kind of tacit bookkeeping of income and expense) for an unprecedented array of "outputs."

First was the Marshall Plan, of which we should all be proud and which we should keep reminding ourselves of what this country can do.

Second was a series of social developments that most of us would consider to have been long overdue:

1. The slow but steady ending of racial discrimination (left over from long before the Civil War - the

national posture was set by the Civil War, but little effect occurred).

2. The ending of sex discrimination in the work place.

3. Removing unnecessary burdens to the handicapped.

4. Cleaning up the environment - both current pollution and the integrated past practice of chemical dumps (incidentally, the solid waste problem now is a good example of the interplay among weak leadership, limited technical solutions and the possible solution by negative entropy contributions by individuals).

At the same time, we were coming to terms with nuclear weapons, the Berlin blockade, the Korean War, the Vietnam War and the cold war arms build-up - all costly in ways greater than dollars.

We can be proud of all of the social progress - perhaps the greatest in a generation in history - and the containing of the Soviet Union was probably necessary.

But, a great deal of the "output" was in increased consumption, in self-indulgence that did not produce goods or services or enhance the quality of life.

And there were costs other than dollars: the decline of the family and the virtual disappearance of the family farm; the emergence of the "Scarsdale brats" and the "Culture of Narcissism;" television as a substitute for reading, conversation or group activity.

Now to Look Ahead

as we become thoroughly and appropriately frightened by the disastrous course of the balance of trade and of payments, and the weak-kneed approach to the astounding Federal deficit, it is useful to consider the baggage we carry in the international commercial footrace.

We still have some unfinished business in all of the social programs I mentioned.

We have especially to solve medical care for the poor, the chronically ill, the aged and the ignorant, especially long-term care. Social Security, Medicare and Medicaid are a bureaucratic nightmare and many are uncovered. Enormous costs are faced as the population ages and medical technology becomes ever more capable and expensive.

We provide the nuclear umbrella and the support of surface armies and navies for the Western Alliance. One-half of our research and development dollars go to protecting ourselves and others, including Japan, and U.S. industry pays for it in taxes when it could be spending on R and D. Japan's R and D is entirely available for the purpose of burying us commercially.

We continue a consumptive binge, with self-indulgence and low savings.

We do not work as hard or as long or with as tight management as workers in the Asian basin.

We are preoccupied with the Soviet Union, which is itself losing ground with respect to Europe and Asia. Watching the Soviet Union and trying to "stay ahead" is a poor test, since other nations are rising rapidly.

We are inadvertently warping corporate strategies: Big funds (like us) require strong quarterly or at least annual performance by our managers, and they in turn look to current earnings and prices; a company being raided quickly has no one interested in its long-term flourishing.

The Federal deficit, the balance of trade and balance of payments performance and the weakness of the dollar are the symptoms that show that we have not faced reality. There is no sign that we will soon.

So, with these impediments, how are we likely to fare?

First, we must face the fact that we will not be a first-rate power. This probably will not become apparent in this century, but very early in the next.

There probably will be no "first-rate powers" (candidates are mainland China and the Soviet Union); there will be a dozen or so countries, each with special advantages and disadvantages and areas where they excel.

We will not be able to control or even exert powerful influence on our international environment.

At home, it is improbable that we can maintain >2.5% real growth in the economy, the secular trend since 1800. This will be a critical test of political democracy, since politicians have to be perceived as giving something, and without real growth there is nothing to give.

Optimistic Scenario

We will make a gracious transition to becoming just one of several major countries.

We will work a little harder in the factories and the schools - spending a little less and saving a little more.

We will use our dwindling technical edge to create defenses that keep us from being a pawn in other countries' conflicts.

We will not try to run the world and will not become "fortress America," "America first."

Our political leaders will modulate their appetite for causes and for single-interest groups as constituencies; they will control the rate of new crusades and not try to do, in a few years, social changes that require generations (Cumberland, ORNL, TVA).

We will export a positive world influence by example by:

1. Maintaining freedom, democracy and free enterprise.

2. Caring for each other, especially the ill, the poor and the aged.

3. Preserving fairness under the rule of law.

4. Being world class in arts, literature, advanced education, with leaders throughout the world seeking a year or two in the United States as part of their social and intellectual development.

Pessimistic Scenario

We will continue to be the world's greatest in consumption and self-indulgence, and continue lack of saving, sloppy performance in the workplace and in the schools.

We will continue the deterioration of the balance of payments, as ownership of our factories and real estate passes to foreign hands and we pay foreigners interest and dividends.

We will continue the massive Federal deficit and the intergenerational transfer that penalizes our grandchildren. The Social Security costs will make a war between the young and the old.

We will discredit democracy, freedom and free enterprise (the latter well underway with takeover mania).

We will witness an ever more shrill pleading of single-interest groups serving as constituencies for ambitious politicians.

We may be subjected to a "man on horseback" - probably not a military man - could be as probably a man in the pulpit - who would appeal to our disappointments and frustrations.

We may - or he may - understate international adventurism to direct attention from domestic troubles.

Implications for The Commonwealth Fund

I have purposely put these two futures in sharp contrast, and of course we are likely to muddle around somewhere between the two.

These are not impossible or even improbable scenarios.

This points up the enormous leverage that is available for foundation activity if it can lean on the action in a positive way. Even with a 50,000 ton ship, the direction can be changed with a few pounds.

We must not say, gee, we can't do anything about this gigantic problem. A few people, a few million, can do far more per person or per dollars than others. When the history is written, that is what counts.

Private, philanthropic foundations occupy a unique position with an opportunity for leadership. They do not have to be afraid of the devil himself. When I was growing up, doctors and lawyers were leaders in our little town; if a town had a college or university, the president of it was an opinion former, coalitions of corporate and political operatives made things happen, as in health care in Rochester. Now these groups are largely discredited. For example, college presidents have all the influence formerly held by prep school headmasters. Probably - I hope - not because they are weaker, but because they must constantly be pleading for resources and cannot offend any important group or individual.

Foundations, with this powerful position, must tackle the hardest, most charged and sensitive, and riskiest questions and undertake work that will make a real difference - not just expand the literature. Their efforts must not be wasted in projects that are not on the critical path.

Foundation dollars are the most precious of all kinds of dollars. They lubricate change in institutions and governments in a healthy way, since if a study is supported by a foundation, it does not compete for internal dollars and thus gets a much better launching pad. Foundation dollars, thus, should not be wasted on any program that could be supported by less precious dollars (e.g., the Federal government).

It follows that amplification of these dollars should constantly be sought. The most obvious way is getting other foundations to share. I strongly support this practice but it does not expand the supply of precious dollars; it does get the total of all foundation dollars more appropriately mobilized on programs initiated by the most innovative, thoughtful, professionally managed foundations and is therefore good.

But amplification should come primarily by the direction a modest foundation effort gives to more massive local, state or Federal government dollars, or to corporation support.

Amplification of a less ambitious, but still important, variety occurs when a program is carried out by an institution, so that work of the kind the Fund wishes to stimulate continues after the grant is exhausted.

A lofty goal for the Fund is that our programs should be so successful, and visible, that family foundations and individuals turn over their endowments to the Fund. But getting this visibility must not be at the expense of sacrificing the principles (of hand...etc.)!

With these observations in mind, the Fund should proceed to generate programs that will exert the maximum leverage toward the optimistic scenario for the early 21st Century. A modest-sized foundation obviously cannot take this task head-on (as if we ran Maggie for President!) and we cannot indulge in political activity. But we can work effectively on individual opportunities to bring us closer to the optimistic scenario.

Let me close with some examples:

1. The Commission on Elderly People Living Alone is an excellent program. It can be developed beyond health care to the total well-being of elderly - safety, security, nutrition, employment on a part-time, at-home basis through use of electronic mail and computers - perhaps to give personalized

service where otherwise only form letters provide non-communication.

2. Humanities - or more precisely humanism - in medical education is a long-standing interest of the Fund. It is still true that the caricature of an M.D. applied to a forbidding fraction of the profession (though none in this room), the caricature that when they were young they read comic books, a little later they read Gray's Anatomy, then they read the Wall Street Journal. And they read nothing else in between!

3. We could seek out projects in the social sciences that would have the maximum promise of a positive impact on our future. Without prejudging such a study or claiming any competence in the field, I suggest as an example that the single most important such project would be to develop quantitative, reliable measures of motivation applicable to 10-16 year olds. If such measures could be developed, they would release enormous corporate support, especially for the poor and disadvantaged minorities.

###

"... the broad topic of the future for the U.S., and the trends that may have powerful implications for philanthropy and thus for the Fund, trends that appear to be important to bear in mind as we look ahead to the next 5 to 10 years."
– I will take a somewhat longer view; my rationale (excuse?) is that we will have to put programs in place in 5 or 10 years to affect outcomes in 2000, or 2010.

My starting point is the end of World War II. Enormous buoyancy. "Can do anything." The expected depression did not occur. Overseas competitors war torn + war weary. We had a powerful industrial plant and trained work force, had not lost substantial fraction of our youth.

We built on that a commanding international position of military & commercial power. Agriculture exports sustaining, and mfg. goods quickly powered the engine of our international trade. The invention of the transistor, which started a whole universe of solid state devices that made the computer practical, permitted artificial satellites + space exploration, + revolutionized communications. Almost as great a change in the years 1955-75 as in the first 100 years of the industrial revolution. These "high tech" elements fueled the export machine.

This commanding position was used (here I am creating a kind of tacit bookkeeping of income and expense) for an unprecedented array of "outputs."

- First was the Marshall Plan, of which we should all be proud & which we should keep reminding ourselves of what this country can do.
- Second was a series of social developments that most of us would consider to have been long overdue:
  - The slow but steady ending of racial discrimination (left over from long before the Civil War – the national posture was set by the Civil War, but little effect occurred).

- Ending sex discrimination in the workplace
- Removing unnecessary burdens to the handicapped
- Cleaning up the environment - both current pollution and the integrated past practice of chemical dumps (Incidentally the solid waste problem now is a good example of the interplay ~~the~~ among weak leadership, ~~no~~ limited technical solutions, and the possible solution by negative entropy contributions by individuals)
- At the same time we were coming to terms with nuclear weapons, the Berlin blockade, the Korean War, the Viet Nam war, and the cold war arms build-up - all costly in ways greater than $.
- We can be proud of all of the social progress - perhaps the greatest in a generation in history - and the containing of the S.U. was probably necessary.
- But a great deal of the "output" was in increased consumption, in self-indulgence that did not produce goods or services or enhance the quality of life.
- And there were costs other than $: the decline of the family & the virtual disappearance of the family farm; the emergence of the "Scarsdale brats" and the "Culture of Narcissm"; television as a substitute for reading, conversation, or group activity.

Now to look ahead

As we become thoroughly and appropriately frightened by the disastrous course of the balance of trade & of payments and the weak-kneed approach to the astounding Federal deficit, it is useful to consider the baggage we carry in the international ~~competition~~ commercial footrace.

- We still have some unfinished business in all of the social programs I mentioned.
- We have especially to solve medical care for the poor, the chronically ill, the aged, and the ignorant, especially long-term care. (SS, Medicare, Medicaid are a bureaucratic nightmare, Enormous costs are faced as population ages & med. tech. becomes ever more capable & expensive. and many are uncovered.)
- We provide the nuclear umbrella and the support of surface armies & navies for the Western Alliance. 1/2 of our R&D dollars go to protecting ourselves and others, including Japan & US industry pays for it in taxes when it could be spending on R&D. Japan's R&D is entirely, available for the purpose of burying us commercially.
- We continue a consumption binge, self-indulgence and low saving
- We do not work as hard or as long or with as tight management as workers in the Asian basin.
- We are pre-occupied with the Soviet Union, which is itself losing ground with respect to Europe & Asia. Watching the S.U. & trying to "stay ahead" is a poor test, since other nations are rising rapi
- We are inadvertently warping corporate strategies: Big funds (like us) require strong quarterly or at least annual performance by our managers, & they in turn look to current earnings & prices; a company being raided quickly has no one interested in its long-term flourishing.
- The Federal deficit, and the balance of trade & balance of payments performance, and the weakness of the dollar are the symptoms that show that we have not faced reality, and there is no sign that we will soon.

So, with these impediments, how are we likely to fare?
- First we must face the fact that we will not be a first-rate power. This probably will not become apparent in this century, but very early in the next.
- There probably will be no "first-rate powers" (candidates are mainland China and S.U.); there will be a dozen or so countries, each with special

advantages and disadvantages & areas where they excel).
- We will not be able to control or even exert powerful influence on our international environment.
- At home, it is improbable that we can maintain ≥ 2.5% real growth in the economy, the secular trend since 1800. This will be a critical test of political democracy, since politicians have to be perceived as giving something, and without real growth there is nothing to give.

Optimistic Scenario
- We will make a gracious transition to becoming just one of several major countries
- We will work a little harder in the factories and the schools, we will spend a little less and save a little more
- We will use our dwindling technical edge to create defenses that keep us from being a pawn in other countries' conflicts.
- We will not try to run the world and will not become "fortress America," "America first."
- Our political leaders will modulate their appetite for causes and for single-interest groups as constituencies; they will control the rate of new crusades and not try to do in a few years ~~that~~ social changes that require generations. (Cumberland, ORNL, TVA)
- We will export a positive world influence by example by:
  - ~~[illegible]~~ Maintaining freedom, democracy, free enterprise
  - Caring for each other, especially the ill, the poor, and the aged
  - Preserving fairness under the rule of law
  - Being world class in arts, literature, advanced education, with leaders throughout the world seeking a year or two in the U.S. as part of their social & intellectual development.

Pessimistic Scenario

- We will continue to be the world's greatest in consumption and self indulgence, and continue lack of saving, sloppy performance in the workplace and in the schools
- We will continue the deterioration of the balance of ~~trade~~ payments, as ownership of our factories and real estate passes into foreign hands and we pay foreigners interest and dividends
- We will continue the massive Federal deficit and the intergenerational transfer that paralyzes our grandchildren. The SS costs will move [to] a war between the young + the old
- We will discredit democracy, freedom, + free enterprise (the latter well underway with takeover mania)
- We will witness an ever more shrill pleading of single-interest groups serving as constituencies for ambitious politicians
- We may be subjected to a "man on horseback" – probably not a military man – could be as probably a man in the pulpit – who would appeal to our disappointments & frustrations
- We may – or he may – undertake international adventurism to divert attention from domestic troubles

Implications for Commonwealth:

- I have purposely put these two futures in sharp contrast, & of course we are likely to muddle around somewhere between the two
- But these are not impossible or even improbable scenarios
- And this points up the enormous leverage that is available for foundation activity if it can lean on the action in a positive way.
- 50,000 ton ship, direction can be moved with a few pounds
Must not say gee, we can't do anything about this gigantic problem. [illegible]
[illegible]

- Private, philanthropic foundations occupy a unique position with an opportunity for leadership. Do not have to be afraid of the devil himself.
[When I was growing up, doctors, lawyers were leaders in our little town; if a town had a college or university, the pres. of it was an opinion former, coalitions of corporate & political operatives made things happen, as in health care in Rochester. Now these groups largely discredited. For example, college presidents have all the influence formerly held by prep school headmasters. Probably – I hope – not because they are weaker but because they must constantly be pleading for resources and cannot offend any important group or individual.]

~~Foundation leadership~~

- Foundations, with this powerful position, must tackle the hardest, most charged and sensitive, and riskiest questions & undertake work that will make a real difference, not just expand the literature. Their efforts must not be wasted along in projects that are not on the critical path.

Foundation dollars are the most precious of all kinds of dollars. They lubricate change in institutions and governments in a healthy way, since if a study is supported by a foundation, it does not compete for internal dollars & thus gets a much better launching pad. Found. dollars thus should not be wasted on any program that could be supported by less precious dollars (e.g. Fed. Govt).

- It follows that amplification of these dollars should constantly be sought.
  - The most obvious way is getting other foundations to share. I strongly support this practice but it does not expand the supply of precious dollars; it does get the total of all foundation dollars more appropriately mobilized on programs initiated by the most innovative, ~~thoughtful~~, professionally managed foundations & is therefore good.

- But amplification should come primarily by the direction a modest Found. effort gives to more massive local, state, or Fed. govt. dollars, or to corporation support.
- Amplification of a less ambitious, but still important, variety occurs when a program is carried out by an institution, so that work of the kind the Fund wishes to ~~stimulate~~ continues after the grant is exhausted.
- A lofty goal for the Fund is that our programs should be so successful, & visible, that family foundations and individuals turn over their endowments to the Fund. But ~~the pro~~ getting this visibility must not be at the expense of sacrificing the principles (of hard... etc.)!
- With these observations in mind, the Fund should proceed to generate programs that will exert the maximum leverage toward the optimistic scenario for the early 21st century. A modest-sized foundation obviously cannot take ~~the~~ that head on (as if we ran Maggie for President!) and we cannot indulge in political activity. But we can work effectively ~~in parts of the~~ on individual opportunities to bring us closer to the optimistic scenario.
- Let me close with some examples:
  1. Elderly living alone an excellent program. It can be developed beyond health care to the total well being of elderly — safety, security, nutrition, ~~use of~~ employment on a part-time, at-home basis through use of electronic mail & computers — perhaps to give personalized service where otherwise only form letters provide non-communication.
  2. Humanities — or more precisely humanism — in medical education — a long-standing interest of the fund. It is still true that the caricature of an M.D. applies to a forbidding fraction of the profession (~~though~~ none in this room), the caricature that when they were young they read comic books, a little later they read Gray's Anatomy.

then they read the Wall Street Journal, And they read nothing else in between!

3. We could seek out projects in the social sciences that would have the maximum promise of a positive impact on our future. Without prejudging such a study or claiming any competence in the field, I suggest as an <u>example</u> that the single most important such project would be to develop quantitative, reliable measures of motivation applicable to 10-16 yr olds. If such measures could be developed, they would release enormous corporate support especially for the poor and disadvantaged minorities.

Remarks at dedication of Clark Hall, 20 Oct 65

Early History of Clark Hall

Robert L. Sproull

We are celebrating today the arrival on the Cornell campus of a marvelously ambitious and complex building.

Clark Hall, like many other major structures and institutions, was created by the confluence of many currents. In fact, the flowing together of national and local currents was not unlike that which created Cornell University itself.

One of the principal national currents originated in the Atomic Energy Commission with the concern for expanding research and training in the field of materials--concern by John von Neuman, D. K. Stevens, and others.

This current merged through the Federal Council on Science and Technology with a similar current in the Defense Department, beginning with Herbert York and General Cyrus Betts and continuing with Jack Ruina, Julius Harwood, John Kincaid, Wilbur Bolton, Charles Yost, and others.

In both of these currents there was a determination to put new tools into the hands of selected universities--tools for outstanding research and for experimentation with new patterns of training additional graduate students in the science and technology of materials.

One of the principal Cornell currents was the desperate need for new space for solid-state physics.

I am afraid most will quickly forget the impossible conditions in Rockefeller Hall now that graduate student research has moved to Clark. But students and staff ~~were~~ had been striving in Rockefeller to create new science in the face of unbelievable crowding, cinders in the ultra-clean experiments, and incredible ingenuity of the basic plumbing and wiring, which kept developing new and mysterious forms of failure.

Another local current was the eagerness of many physicists, chemists, and metallurgists to establish connections among their research programs and to experiment with joint training of Ph. D. students.

4)

An important tributary was the desire to establish sophisticated technical facilities, such as X-rays or electron microscopy, as mixing grounds for graduate students from several academic disciplines.

Another current was the commitment to graduate teaching and the quality of Ph.D.'s in the relevant departments. For example, a tabulation was made in 1959 of the 45 Ph.D.'s in solid-state physics in the Physics and Engineering Physics Departments who had left Cornell between 1949 and 1959. All 45 were at work in demanding positions in solid-state research--not one had dropped off into work that did not use his intensive graduate training.

Other deserving academic units, astronomy and space research, should also be cited.

Two opportunities evident in 1960 deserve special mention: First, the library. The idea of establishing an open-stack library in a connection between physics, chemistry, and astronomy was most appealing--a convenient, habit-forming attraction for undergraduate and graduate students alike.

Second, the physics advanced teaching laboratory, for seniors and early graduate students. Years ago Cornell had been a pioneer in replacing the strictly organized, "do this, and then do this" type of laboratory by an individualized student experience.

Each student was given some apparatus and some references and encouraged *personally* and *individually* to trick Nature out of as many secrets as he could. (The modern un-word for this process is "engagement.") But, as so often happens, the Cornell innovators--Collins, Parratt, Hartman, others--were upstaged by later, more affluent laboratories in modern buildings at other universities. Planning a new building provided the opportunity to return the Cornell teaching laboratory to center stage.

Of course all these needs and opportunities do not produce a building on this scale without massive financial support.

7)

The support at first seemed impossible, and then possible but complicated. But the support is basically simple.

First there was the Advanced Research Projects Agency. ARPA is a fast-moving, far-sighted, flexible agency of the Office of the Secretary of Defense. ARPA broke new ground as the principal agency sponsoring the major new program in materials research and training, the program I have had time only to hint at here. In order to make that program go, ARPA offered to reimburse Cornell at the rate of 10% per year, instead of the usual Federal 2% per year, for the cost of space used for it.

By this time, the Cornell part of this program had become the responsibility of the Materials Science Center, and Dale Corson and Henri Sack were instrumental in negotiating for this support.

Next Cornell had to decide whether to build just the ARPA-supported building--even that would involve borrowing--or a much larger building to satisfy other long-standing needs and exploit opportunities such as the library and teaching laboratory I have mentioned. The Cornell Trustees chose the larger building, a decision that required particular courage since every cent had to be borrowed. The borrowing was expertly negotiated by John E. Burton with the New York State Dormitory Authority, which thereby became the second key element in the support.

Finally, and most importantly, Mr. and Mrs. Van Alan Clark made the outstanding gift of the Centennial Campaign for this building. Their generous gift not only complemented the ARPA use payments and financed the part of the building that produces no "income." Their gift also, perhaps even more consequentially, recharged the Cornell courage to deal effectively with current problems even when the support is not in sight.

So, the support is not really very complicated, and is made up of three absolutely essential parts.

Finally, a few words on the design of Clark Hall.

10)

The program of space use was exceedingly complicated, since it incorporated parts of four academic departments in two colleges, the Laboratory of Atomic and Solid State Physics, the Materials Science Center, the Center for Radiophysics and Space Research, and part of the University Libraries. Furthermore, many kinds of functions such as clean rooms and irradiation facilities were planned, with many novel features of equipment and services. Also, the building was to be connected to two existing buildings and was to be an attractive addition to the campus when viewed from any direction--there is no "back door."

All these became the problems of the engineering firm Office of J. Fruchtbaum, especially Jack Fruchtbaum and Harvey Anderson, and of the architectural firm of Warner, Burns, Toan, and Lunde, especially Charles Warner and Melvin Aminoff. You see and will see the dramatic results.

The users of the building were represented during the programming and design by Paul Leurgans, James Krumhansl, Donald Holcomb, John Rogers, and Stanley Albro. This group represented not only the immediate users but strove to assure the usefulness of the building through many generations of users.

As a result of the design attention lavished on it, Clark Hall has a flexibility as a teaching and research building that is rare and perhaps unexampled on a university campus, and features of it will be widely copied.

In these few minutes I could scarcely do justice to the early history of Clark Hall, and I could do no more than list the principal ingredients that have created this handsome building. These were the excellence of the Laboratory of Atomic and Solid State Physics and other academic units, the farsightedness and administrative flexibility of ARPA and the Dormitory Authority, the generosity of the Van Alan Clarks, the courage of the Board of Trustees and the imagination of the designers.

Clark Hall will be, I freely predict, a famous building.

From Blue Books to Bandwidth: The Future of Higher Education
Robert L. Sproull
13 October 2000

It is hard to imagine what American society would be without the contributions of higher education over the last 150 years. The whole **texture** and the **quality** of life and thought would be totally different. Also, the **input** to higher education profoundly changed during that period, most notably by the entrance of **women**, the creation of **public** institutions, and the great expansion of **numbers** of students.

But I have been challenged to identify the **most significant single contribution.** I do so with some diffidence, since my choice is easily attacked as elitist and self-serving. I believe that the contribution is **advanced instruction and research in science and engineering.**

In the first 100 years after 1850 the **electrical revolution** transformed American life. Expansion and development, including the accommodation of waves of European immigrants, required a replacement for the **physical** frontier that was coming to an end, and the industry based on or facilitated by electrical machinery supplied this **internal** frontier.

In the later 50 years and continuing now, the same is true of the **information revolution**.

It can be argued that most of the inventions were not made in colleges or universities. But an invention is not a product and and a product is not an industry. Higher education produced highly educated people who transformed inventions into industries. The transistor and the integrated circuit illustrate this process: Both were **invented** in industrial laboratories, but the U.S. lead in **exploiting** them was produced by the undergraduate and graduate "output" of American colleges and universities. And continuing into the future, graduate technical education in the U.S. is the envy of the world.

In addition, American higher education has enabled the U.S. to be an important player in the world scientific scene. The science underlying the electrical revolution began with Joseph Henry at the Albany Academy and Princeton University.

American universities have kept the U. S. in the act during the first half of our period and secured **leadership** in the last half.

II

Now to Question II. The rather grubby challenge of **money** is the general answer, but it includes many pieces. I select one here, and Question III addresses another.

The greatest stand-alone challenge is to the **diversity** of American institutions. Foreigners continually envy the wide range of American colleges and universities, which provide an institution suited to **every** undergraduate or graduate student, however special may be his or her needs, ability, financial circumstances, or ambitions. Although one often hears marketing claims that a single institution can accommodate all comers, this diversity is the true meaning of "open admissions,"

Diversity is threatened by the rapid growth in both size and quality of public institutions. For a few years, the strengthening of tax-supported schools will continue to strengthen American society. But when it drives out schools that contribute to the diversity and available choices, eventually the impact will be highly negative. Small independent residential colleges are probably especially vulnerable.

Tax-supported schools are moving aggressively to exploit all the revenue sources, including most actively pursuing **private giving**, traditionally the heartland of financing independent colleges and universities. Since the taxpayer supports faculty salaries and most construction, a state school can offer a great bargain to a prospective donor; he or she can name a professorship or a building with a gift at a large discount, say $200,000 for a professorship that should cost $2 million.

To fight back, independent institutions must maintain and enhance their degrees of difference and develop their individual niches. Tuition revenue is, of course vital. For over 30 years I have been responding to alumni and parent complaints that our tuition is "outrageous" by claiming that tuition plus room and board has been **constant** since 1930, namely equal to a well-equipped new Chevrolet. This equality has been maintained by private giving, which has grown as much as General Motors' reduction of costs by increased productivity and use of new materials and computers.

The Chevrolet equation is vulnerable when the unprecedented prosperity we are enjoying comes to an end and schools will be unable to raise tuition to match costs. The competition with tax-supported schools will also severely limit the ability of independent colleges and universities to raise undergraduate tuition, and consequently many schools, especially small liberal arts colleges, will disappear or be homogenized into state systems. Diversity will suffer.

III

Now to "bandwidth," which I take to be a metaphor for distance learning and the application of technology to enhance or substitute for the classroom.

First I need to remind you that the undergraduate experience is in part **training** and in part **education**.

In "training" I include the imparting of **facts** and the development of **skills** that enable the student to contribute to a useful enterprise and thus to be employed. The computer, the Internet, and the associated technologies all are superb additions to the training process. We have seen only the **beginning** of their use to transfer to the student that part of knowledge that can be reduced to facts. The development of training aids in 25 or 50 years can hardly be imagined, but it will be immense. For a starter, I expect the creation of highly sophisticated **simulation** programs in which the student learns by hands-on interaction with a simulated **system** as complicated as the human body or an entire industry.

The technology-enhanced, fact-transfer training is the entire content of some junior colleges and technical schools experiences, and therefore cheap or free training through technology. threatens these institutions. The better ones will survive, at least for a while, because their service in **sorting** and **credentialling** undergraduates is a valuable and appreciated function that can only slowly be created by distance learning establishments.

But **education,** as we all know, is **far more** than the imparting of Dickens' Mr. Gradgrind's facts, and the surviving colleges and universities will emphasize it. Education proceeds by student-student, student-faculty, and student-library interaction, enhanced somewhat by technology but not replaced by it. Students during an important window in their lives explore one or more fields of thought in depth and especially explore their own capabilities, interests, imagination, and relation to society.

My own ideal of the education process is the afternoon tea in a professor's laboratory, where graduate students, post-docs, and one or more faculty members interact in an unplanned and most informal way. The discussions are far-ranging and are colleague-to-colleague, not master-to-apprentice. I believe the same kind of interaction occurs in graduate seminars in the humanities and social sciences. To the extent that this type of interaction can be carried into undergraduate education, to that extent colleges and universities will successfully resist displacement by distance learning and other technology and will survive.

I envisage a higher education scene in which every student graduates with both a **competence** and a **major**. The competence is largely quantitative **training** and makes the graduate immediately useful in society and therefore employable. It is accomplished cheaply and takes little student time because of the strong application of technology. The major is **education** in which the student explores an intellectual field in depth and explores himself or herself in the process.

Whether undergraduate programs in colleges and universities can approach this ideal closely enough to be worthy of survival and in fact to survive will depend primarily on the **faculty**, not on deans and presidents. The faculty must enjoy one-on-one interaction and be willing to spend time on it. Shy faculty who use the classroom as a cheap substitute for personal interaction will be displaced by "bandwidth." The attitude and behavior of individual faculty members will be the key to the survival and flourishing of undergraduate education in colleges and universities.

Corrected
16 Nov 70

DEFENCE, SCIENCE, AND DEFENCE SCIENCE

Robert L. Sproull

President, University of Rochester

Speech delivered
in Ottawa, Canada
on 17 Nov 1970

It is a distinct privilege to talk about defence science to a Canadian audience. I have lived most of my adult life just across the border in upper New York State, a region which competes successfully with Ontario for the worst weather in North America. I have enjoyed working with Canadian students at Cornell and Rochester, students who on the average bring more seriousness and stimulation to the classroom and the laboratory than the home-grown assortment.

I especially enjoyed interaction with the Canadian Defence Research Staff the years I was in Washington. Your people in Washington repeatedly opened our eyes not only to Canadian developments but also quite frequently to U.S. developments that we were ignorant of or had ignored.

And the mitigation of Pentagon blindness was not the only or the most important contribution of interacting with the Canadian Defence Research Staff. These discussions forced us to take a longer and broader view of defence, and especially a view less distorted by the passionate swings of U. S. policy and budgets.

But my viewpoint now is again the university, rather than the U.S. Defense Department. I cannot avoid concentration on the rôle of universities in defence and defence science. The university standpoint, although obviously restricted and provincial, may have some advantages: Universities, since they work with the coming generation, necessarily take a long view of time. You don't work hard to develop a university unless you are confident there will be a twenty-first century. Thus my credentials are a little background in defence, considerable background in universities, and a basic optimism.

But I must admit that the current setting is so sobering that it is hard to be anything but a pessimist. The alienation of the young, a common enough development in every generation, penetrates and poisons education at all levels. Many of the young claim high moral ground for their alienated position, a position frequently compounded of equal parts of laziness and hedonism. Though the future belongs to the young, we may well be tempted to say "Let them have their selfish approach; why should we work to preserve a future for them?" Of course we then realize that most young people are unselfishly concerned about the future, and most of even those who aren't will later appreciate the work of earlier generations. But tension and pessimism prevail.

Confidence has diminished in man's institutions and perhaps even in man himself. The press and television amplify almost without limit every inadequacy, every slip from grace, every failure to solve the problems of the age by a week from Thursday. The prevalent pessimism has been thoughtfully noted by Edmund Bacon in The Design of Cities: "We are in danger of losing one of the most important concepts of mankind, that the future is what we make it."

Defence

Perhaps the pessimism is deepest in defence. Although I am certainly no expert on national security and world-wide stability, I need to spend a little time on this subject as a prelude to talking about defence science.

The twenty-fifth anniversary of the United Nations resembled a wake more than a birthday party, doubtless in large part because of the current quagmire in the Middle East but also because, despite its vast good works, there is little confidence that the U.N. could deal with a big war. The twentieth Pugwash meeting was also pessimistic; like the U.N., this informal group has accomplished a great deal, usually with little fanfare, but it now is immersed in gloom laced with indecision. Yet either the well-known organization (the U.N.) or the little known non-organization (Pugwash) may eventually provide the key to maintaining world stability while decreasing the fraction of human effort that goes into arms.

But meanwhile, a return to an apelike chaos has been prevented in a way that few of us would have credited with any chance in 1945. By the time the sacrifice and suffering of World War II had become dangerously less vivid, both East and West were becoming confident of their ability to inflict unacceptable damage by a secure second strike. Somehow, this improbable "balance of terror" has worked. It would be interesting to know what odds you could have drawn in a group like this in 1945 for the proposition that there would be no nuclear warfare of any kind in the ensuing 25 years. I know I wouldn't have bet on the nuclear détente!

Avoidance of nuclear war has been a truly remarkable achievement and one in which science and technology have played an essential role. If historians a century or two later look back at this period, I am confident they will rank it among the most remarkable achievements of all time, comparable perhaps to the narrow squeak by which writings of classical Greece survived through the middle ages. Yet as far as impressing young people is concerned, this achievement and twenty cents will get you a cup of coffee. And why should it impress them? So we have preserved, more or less, the 1945 world at enormous cost and considerable risk? What's the big deal? If we didn't think we could preserve the world, why did we bring them into it? Of course impressing the young is a chancy occupation in any generation, and the more substantial problem is: How do we continue to prevent nuclear war?

And there are certainly plenty of problems there. Let me list a few:

1.) The emergence of China as a world power with a technological base.

2.) The circumstance that deterrence requires continuous attention and expenditure, whereas the U.S. Congress tends to expect that after they have put $X billion into the strategic deterrent "it will do the job."

3.) The vulnerability of bombers.

4.) The apparent capability of MIRV and precision guidance to render land-based missiles unacceptably vulnerable (David Hoag has provided some speculations, based on Apollo guidance, that inertial guidance ought to be capable of a 30-meter CEP.)

5.) The possibility of rendering the sea-based deterrent vulnerable by advances in low-frequency sound systems.

6.) The likelihood that familiarity will eventually lead to tiny islands of carelessness and casualness, which could lead to accidents with far-reaching consequences.

7.) The circumstance that deterrence is, after all, a state of mind, and a very poorly understood state of mind.

It would be pleasant indeed if there were as notable achievements in limited war. Every country has had its failures; we in the U.S. went through an open switch in 1965 and are just beginning to get the train back on the rails again. It looks as if we may soon follow the embarrassment of failure with the embarrassment of isolationism. But some good may come out of South East Asia: Perhaps the best result of the Viet Nam adventure is to lay to rest forever the "never lost a war" argument, a most pernicious slogan to have lying around when tangling with complicated problems.

But there remains the serious question of whether a modern, open society can engage in limited operations. When the massive retaliation doctrine was complemented by the controlled response doctrine, a vital flaw remained in the latter: There was no answer to the question "How do you maintain control when the casualty lists mount and when the media that work largely through monosyllables and simplistic arguments demand victory or withdrawal?" It seems to me that before we invest in additional men and materiel for limited war, we must have a much clearer doctrine of controlled response and we must have thought through how an open society in which even our thoughts are on public view can disengage, as well as engage, and can maintain a flexible response as events unroll. It is one of the great disappointments of the time (according to me) that science, including social science, has been able to offer little help here.

Science

Before tackling defence science I should like to talk a little about science in general. The buoyancy attending discussions of science up until a few years ago has vanished. Meetings of scientists used to be happy affairs where we all shared the excitement of the latest discoveries and shared the fun of toppling the high priests by new data. Not so now; the air is opaque with gloom at such meetings. Instead of being the darlings of the public, we the unselfish, underpaid professors and researchers, we are now alleged to be responsible for bad tasting water and traffic jams, and we are overpaid "fat cats" who are self-indulgently subverting youth!

Science is being battered by two kinds of rising costs: "Ordinary" inflation (pretty extra-ordinary of late!) and "scientific" inflation; the latter is the increase in costs per man year caused by use of computers and other aids more sophisticated than formerly. These inflations have been accompanied by a transfer of public and governmental interest from scientific and high-technology areas to low-technology and direct action programs, which transfer has further reduced the support of science.

The reaction of scientists has been unbelieving shock. Harold Brown has said "A great many scientists think that ample government support is an inalienable right, despite the lack of its mention in the Declaration of Independence." There has been a general reluctance to make any adjustment of behavior when faced with the news that there is no such right.

This is a classical problem in human affairs: What do you carry from the burning house? On what basis do you select what to preserve when resources dwindle? Probably the best-known expression in literature of this puzzle is in Chekhov's Cherry Orchard. The anti-hero Lopakhin is explaining that the Ranevsky family can keep going only by selling at least part of their property:

Lopakhin: You'll have to pull down all the old buildings... and cut down the cherry orchard —

Mme. Ranevsky: Cut it down? My dear man, forgive me, you don't know what you're talking about. If there's one interesting, in fact quite remarkable thing in the whole country, it's our cherry orchard.

Lopakhin: The only remarkable thing about that orchard is its size. It only gives a crop every other year and then no one knows what to do with the cherries. Nobody wants to buy them.

Scientists are trying much too hard to hang on to quantity during this support squeeze, by trying to keep numbers of graduate students and postdoctoral fellows invariant, by regarding any decrease in laboratory size as a major catastrophe. I don't want to push the Chekhov quotation too far, but it's not much of an exaggeration to say "no one knows what to do with the Ph.D's. Nobody wants to buy them."

Pre-occupation with quantity in a cost-support crunch can only result in sapping the vitality from science. Raymond Bowers has recently noted in this connection that one's taste for innovation declines sharply as a rope tightens around his neck.

This brings us to the two problems that are always with us in science policy: rate and distribution. By "rate" I mean the number of dollars or man-years going into science each year. There has never been a satisfactory theory, or even rationalization, of this rate. If we were starting afresh today, I have no confidence that the annual support of science would approximate what it is, probably not even within a factor of two. Yet we regard a 10% or 20% slump as a catastrophe!

In the past, science has been oversold. Our arguments, never very quantitative or compelling, have lost credibility as the Bureau of the Budget (now OMB), the Congress, and the public have become more knowledgeable and suspicious.

Support of science has been defended under three headings: Science for science's sake, science for teaching's sake, and science for technology's sake. The first provides no yardstick at all for the rate. Certainly some science should be supported just as the arts are supported, as a continuing creative activity helping to separate man from the apes.

Science for teaching's sake is an argument necessarily tied to the number of students. Now in the U. S. the number of people between 18 and 21 years old has been steadily growing but will start to decrease in 1982. By 1986 the number will be back to its value now. Thus the number of teachers of science and of teachers of teachers of science need not grow as it has been doing. The need for Ph. D.'s has been flamboyantly exaggerated in the "Committee of Fifteen" study in the mid-fifties, and later the Gilliland study for the President's Science Advisory Committee, though limited to science and engineering and more restrained, still exaggerated it. Almost at the same time as the latter study, however, Berelson gave predictions of much lower needs, predictions that have (thus far) weathered rather well. Allan Cartter in 1966 predicted an over-supply of Ph. D.'s in the mid-seventies, although he explicitly noted that some of his assumptions depended on how hungry industry was. Long after the Berelson and Cartter studies, the behavior of graduate departments was as if the studies had never been made.

Science-for-teaching's sake, as a quantitative guide to size, thus leads inevitably to a decrease in rate of support of science, at least for a few years.

Science-for-technology's sake should also be subject to quantitative analysis. But how fast, for example, do we want medical technology and medical science to develop? For the child who died of polio the year before polio was conquered, the rate wasn't fast enough. How much of our fungible resources should be used, say, in developing an artificial heart? Also, the connection among science, technology, and economic growth is a tangled one. If it is understood, it is not understood by me. Our arguments that more science should be supported for the sake of growth have a hollow ring to them now. Britain has excellent science, the envy of most of the world, but an unenviable record of economic growth. Japan has only a modest to small science effort, but a growth unequaled among major nations.

In 1965 the Joint Committee on Atomic Energy held hearings on the need for new accelerators in high energy physics. The basic questions were: How big a step in energy should be supported? At what rate should high energy physics be supported? A distinguished parade of physicists contributed testimony, but almost none of it bore on these questions, particularly the second question. The witnesses defended the notion of doing high energy physics, which was not under attack, but gave no defense of the rate. They did hint at great practical things that would be forthcoming from further support. Dr. Robert Frosch, later Assistant Secretary of the Navy for Research and Development, was at that time working on seismology and nuclear test detection. After a visit to the hearings, he remarked that if seismologists behaved the way the witnesses had, they would not only guarantee precise prediction of earthquakes but would promise harvesting energy from them for electrical power!

The trouble was, however, that the committee staff had looked up the testimony of two decades earlier, when hints of new nuclear power sources were rather bold. In the end, of course, further accelerators were supported — as they should have been — since the JCAE scarcely wished to diminish its own dominion. But incidents like this have cut into the credibility of the defence of the rate.

The second problem in the support of science is the distribution of support, among geographical regions, among kinds of performers, and among subject areas. Here, too, there is no guiding theory or body of knowledge, and of course this distribution problem is convolved with the rate arguments.

About geographical distribution, full of overtones of politics and the "pork barrel," there is nothing of value I can say.

As for distribution among government laboratories, Federal contract research centers, industry, and universities, there is only one new issue, the appropriateness of defence-supported science on the campus, and I shall return to this issue a little later.

Distribution among subjects has always produced bitter arguments. Each scientist believes his field is the most exciting, most refractory, and most significant. Anyone who would like to rank fields for purposes of distributing support should first go through the sobering recent histories of all the major fields. For example, the path from the Stern-Gerlach experiment of 1922 to the laser should temper the confidence of anyone who thinks he can identify the most important fields at any one time. A much healthier and wiser view of this distribution is given in the closing paragraph of an article by Steven Weinberg, part of the 1965 defence of high energy physics that I alluded to earlier: "Instead of feuding with one another for public favor, it would be fitting for scientists to think of themselves as members of an expedition sent to explore an unfamiliar but civilized commonwealth whose laws and customs were dimly understood. However exciting and profitable it may be to establish themselves in the rich coastal areas of biochemistry and solid state physics, it would be tragic to cut off support to the parties already working their way up river, past the portages of particle physics and cosmology, toward the mysterious inland capital where the laws are made."

Despite the difficulties about determining how much science to support, there are two extremely reassuring aspects of the contemporary science scene.

The first of these is that science is a truly international activity, and not every nation is copying the swings of the U. S. The scientists of different countries do not all climb onto the same bandwagon. The diversity of approaches and of views of what fields are pregnant will preserve the vitality of science even if a large country like the U. S. stumbles badly. I once worked for a year in a laboratory in Brussels where there were chemists, physicists, mathematicians, and metallurgists from fourteen countries. Some cooperative operations were slow, and every operation was complicated by the cacaphony of languages, but there certainly was no narrowness, no conformity!

The second reassuring aspect is the open-endedness of science itself. The biosciences are leaping spectacularly. Perhaps the physical sciences by comparison are not so dashing, but they show no signs of saturation. One has only to mention quarks, pulsars, superconducting tunneling, and any of dozens of intricate and pregnant phenomena in nearly perfect crystals to remind us that physics shows no sign of stagnation. Mathematics, applied mathematics, and (especially) computer science continue to attract the imagination of the brightest of the young people.

This sense of opportunity and excitement extends to science's companion, technology. Just one device, the laser, is opening up a vast area in communications, measurement, materials processing, and information handling, and laser-stimulated plasmas may solve the fusion energy problem and produce propulsion and medical devices. To name just one other area, the cost and device size per information bit stored or per arithmetic operation performed continue to decrease, with no limit in sight; more and more powerful and versatile control and data processing systems flow from these developments.

So why all the doom and gloom? Why the loss of confidence in science as an element of civilization? Is a 10% or 20% cut in manpower so catastrophic? If the cost per man-year for (say) X-ray crystallographic determinations is driven upward by computerization, why not cut the man-years per year by (say) 25% and be reassured that even with this cut, one will be turning out many more and more reliable results because of computerization? Is it such a disaster if an occasional Ph. D. in physics becomes a high school principal or a computer sales-engineer? After all, Ph. D.'s in English and history have been accustomed to such shifts for many years.

I submit that very modest decreases in size, executed as soon as possible since delay exacerbates the cherry orchard problem, will restore effectiveness and confidence in most scientific operations. The only reason that this obvious route has not been followed in timely fashion everywhere is that a quarter century of almost monotonic expansion has accustomed us to use expansion as a substitute for management.

Defence Science

The gloom in defence science is probably deeper than in science generally. In addition to suffering cuts in rate of effort and loss of confidence in justification of that rate, defence science in the U.S. has been battered mercilessly by Congress, by the administration, and by universities.

I'd like to talk about the state of defence science under three headings, each in the form of a question. The first question is, it seems to me, the question that ought to be asked first whenever this subject is discussed: What is happening to the ONR tradition? The pattern set by the Office of Naval Research in 1946 was, as you know, a pattern of largely unclassified contracts, largely with universities, and operated by program managers in Washington who were in background and temperament indistinguishable from the people they supported. The ONR tradition gave leadership and precedents not only to the later research agencies of the Department of Defense but also to all the other agencies that grew up after World War II (notably AEC, NSF, and NASA).

Part of that tradition was that DoD supported its share of basic research, so that the total Federal support added to a sensible national policy. Part of the tradition was the support of applicable basic research and applied research in fields of special interest to the sponsoring agency—such fields as materials, propulsion, and electronics in the case of the ONR. Part of the tradition was the exchange of people between universities and the Government agency, an especially effective way of developing a two-way window.

Congress, sometimes with the support of the administration, has been challenging this tradition. I suppose it was inevitable; we were really rather complacent about the ONR tradition, we knew it "worked" in some sense but we did little to create a body of knowledge that showed how well it worked or compared it with alternatives. Congressional committees went along with statements like "research pays in the national defence" without examining the meaning carefully. Defending research became a "ceremony in code," like a wedding ceremony, where the minister or priest does not spell out explicitly what happens next, or what the alternatives are, or how little one knows about the process by which marriage leads to happiness.

Congress has never really understood how unclassified work supported in universities benefits the U.S. and friendly nations more than it benefits potential adversaries. How could we convince Congress when we were not all that confident ourselves? To be sure, we had a few conspicuous examples like the way the spread of unclassified solid-state research permitted the rapid exploitation of the invention of the transistor. But our arguments were heuristic rather than logically compelling. Now heuristic reasoning is great when confident expansion is looking for opportunities. But it is no substitute for logically compelling reasoning when confidence vanishes, retrenchment sets in, and the "name of the game" is the distribution of shortages.

Furthermore, the loss of confidence in the military produced by the frustrating stumbling in Viet Nam brought Congressmen out of the woodwork who had never before spoken up on defence affairs. They showed little reluctance to take apart a delicate mechanism before they knew how it worked or whether they could re-assemble the pieces.

Section 203 of the 1970 Department of Defense Authorization Act (PL91-121) is the outstanding example of dangerous meddling. Senators Fulbright and Mansfield and Congressman Rivers are all very bright, very powerful, very experienced legislators. Mr. Rivers' motivation was clearly different from the others' in most respects, but all three were happy to kick the universities once kicking them had become second only to baseball as the national sport. You doubtless know what this section said: "None of the funds authorized to be appropriated by this Act may be used to carry out any research project or study unless such project or study has a direct and apparent relationship to a specific military function or operation."

This little section threw out a large and essential part of the ONR tradition. It ignored completely the role of time, the fact that research to be applicable at all must be accomplished long before many "military functions or operations" are conceived. It could have been written by the Students for a Democratic Society (SDS), if they had been informed, alert, and imaginative enough, since it promised to generate noisy confrontations on campuses. Imagine the following, shouted, and garnished with obscenities: "You claim you are doing basic low-temperature physics experiments, but in order to get that war tainted money it must be that you are actually working on a specific military function or operation, like killing women and children in Viet Nam."

To the best of my knowledge, the Department of Defense did not substantially oppose this section. I suppose part of the reason was that the DoD was (and is) on a huge relevance kick and would just as soon shuck off broader research. Part also was doubtless that they had their hands full with Viet Nam and the ABM, and as everyone knows in Washington, "the urgent drives out the important." I hope part was not any loss of courage, since surely a nation ought to be able to rely on the maintenance of courage by its defence establishment.

Incidentally an additional, even worse section (Sec. 402) had been in the version of the Authorization Act passed by the House (HR 14000). This section was clearly designed to intimidate program managers in research agencies. It required that each proposed contract or grant to a university or college be submitted to Congress at least 60 days before execution, along with lots of data including how "well" that institution had behaved "with regard to cooperation on military matters such as the Reserve Officer Training Corps and military recruiting on its campus." The DoD actively opposed this section, the Senate bill did not contain it, and the Senate-House Conference removed it from the final Act.

Nevertheless, this whole incident shows how fragile the traditions of defence research are, how poorly understood they are by Congress and the public, and what attractive whipping boys the universities are.

Fortunately, the "system" is so big and clumsy that it takes it a year or two to respond, even to a stimulus as strong as Sec. 203. By last spring there was some evidence that even the three principal authors were beginning to realize that in attempting to rap knuckles they may have broken an arm. The report (24 April 1970) of Mr. Rivers' Armed Services Committee on the FY 1971 Authorization Act has the following paragraph commenting on Sec. 203: "This seemingly innocuous provision now appears to be fraught with danger, for it adversely affects research efforts involving the security of the nation 5 to 10 years from now. For example, it might be most difficult to associate chemical laser studies directly with a military function or operation at this time. Yet this new light source may, in the years ahead, prove to be the most efficient method of communication yet devised by man. Much of the basic research on a project such as this would be denied the use of defense research and development funds under the limitations contained in the existing law." It is hard for me to see how anyone could improve on that paragraph. The law this year replaces the old Sec. 203 with a new Sec. 204 with the words "a potential relationship to a military function or operation." A vast improvement!

But the basic weaknesses have not gone away, and this same report goes on to mobilize a data collection effort on campus activities and to warn that the Committee will consider restrictive legislation next year.

The answer to the first question ("What is happening to the ONR tradition?") is thus that the tradition is alive and well but fighting off lethal viruses. It needs all the help it can get to restore Congressional and public confidence in it and to educate new generations in Washington to its success and continued promise. There is no theorem that the ONR tradition is immortal.

My second question is: What should one preserve when the budget is cut? Here we are at the cherry orchard again, but this time the trees belong to the DoD. If one tries to keep current operations (size of forces, procurement rates) nearly constant, the leverage on future capability (via research and exploratory development) becomes enormous, since that is a smaller part of the budget to begin with. The continued (on a time scale measured in decades) effectiveness of the strategic deterrent and the bequeathing to our successors in the twenty-first century of the ability to be taken seriously in international affairs seem more important to me than the maximum size of the current force structure.

When Harold Brown as Director of Defense Research and Engineering cut the ARPA budget he would say, with a twinkle in his eye, that we would accomplish the same results by better management of the reduced budget. My eye didn't twinkle so well at those times, because I wasn't so sure what good management was. One thing I was pretty sure of then was that it was easier to manage if my budget wasn't cut. One thing I am pretty sure of now is that insisting on more "relevance" is not good management of research funds. The current relevance fad uses the word "relevance" as synonymous with "immediate applicability." (This is what the students mean by "relevance", too!) I would have no quarrel if the term (and the fad) were interpreted as "of long-term significance and future applicability."

Have you ever read a 10-or 20-year-old statement of relevance? an analysis say, of the relevance of nuclear physics in the 30's? of molecular beams in the 40's? of solid-state physics in the 30's and early 40's? Such statements generally make bad reading a decade or two later, even when discussed under broad headings like these examples; more specific analyses of smaller compartments of science make even worse reading.

How, then, does one decide what to encourage and support? It seems to me that ideas, concepts, and instruments useful in defence will come even from fields like cosmology, astrophysics, gravity research, and the far reaches of biochemistry. DoD should be sure that their support plus the support of other Federal agencies adds to a sensible Federal program, even in such esoteric areas. I see nothing to be gained and much to be lost if DoD is prevented from having contact with such fields.

Of course the larger part of DoD support of research should go into areas closer to probable activities a decade or two hence, areas like electron motions in solids, dislocations in metals, coherent light propagation, and plasma diagnostics. Real relevance comes from excellent people working in broadly defined areas where the phenomena are not yet well understood but where a theoretical framework is emerging. If such work can advance areas where we can see now a potential connection to defence devices and systems, so much the better. If such work can help interest bright young people in high-technology areas, so much the better.

But how will practical results come from such work? Some years ago Dr. Morris Tanenbaum led a Materials Advisory Board study on how really new materials developments came about. Case studies showed that planned harvesting of research and connecting a need with an invention can be accomplished by innovative managers and promoters, expecially in the later stages of development. But the study, and the case study on high field superconductors that Dr. Tanenbaum contributed himself to the set, showed also that just the mixing and stirring of engineers and scientists from different backgrounds and disciplines and connected to different needs was extremely effective. The stochastic lunch table group was often as effective as the planned conference table group.

We need this kind of mixing of people with a variety of backgrounds now more than before. During World War II the general upheaval produced probably more variety of interactions than we could use. Even earlier, refugees from the Continent contributed much stimulation, challenging of conventional ideas and approaches, and hybrid vigour to North America. In the absence of these artificial modes of stirring ideas and people, we should do everything we can to preserve and enhance the stirring that comes naturally in the best large laboratories and in universities.

This brings me to the third question: Should universities be involved in defence research? You've probably already guessed that my answer is "yes." To cut off defence needs from the universities would remove a large source of diversity of ideas and people. Protection from possible surprise developments originating in closed societies requires the maximum provision of such diversity. To cut off defence research from the universities would isolate DoD from the new generation. To be sure, the Selective Service System connects many of the young with the DoD, but <u>not</u> in a way likely to bring them back as consultants and contributors in later life! The supply of people like myself who started in defence research during World War II is becoming exhausted. The new generation will inherit our world--not nearly as bad a world as some of them contend--and it is for them that defence exists. It is essential to interest a new generation educated in science and high-technology engineering in international stability and how to preserve it.

To be sure, there are problems on the campuses. I have already referred to a small subset of them, the problems that would have been created by Sec. 203. But the new Sec. 204 is more restrained, less of a confrontation. An individual principal investigator ought to have the right to choose his own research and his own connections into the real world of applications and support. In several places, such as State University of New York at Stony Brook, faculty votes have "crowded" the faculty members with DoD grants, in essence pushing them off the campus. But Stony Brook has now reversed itself and no longer discriminates against the faculty member who is willing or even eager to connect himself to defence agencies. There is a dreadful trend toward homogenizing higher education in America. I see no difficulty if a few or even many universities outlaw DoD grants, but I think it would be disastrous if <u>all</u>, or even all of the best, universities would do so. Surely as each quite properly seeks to preserve its integrity while serving society (especially future generations of society), some will find DoD work no threat to that integrity.

There is so much to do in defence research and exploratory development! Let me cite just a few areas demanding more attention:

1.) Defence in an arms control setting. Suppose, for example, that all flight faster than sound were outlawed. Would this be stabilizing or destabilizing? Research and development would doubtless be needed to convince ministers and Congressmen that cheating could be detected.

2.) Opening the skies by improved reconnaissance. Perhaps science and technology can do what the U.N. and Pugwash have not yet been able to do, attain world-wide warning of arms buildups or preparations for surprise attacks. (I have always credited the U.S. ability to make a measured response during the Cuban crisis of 1962 to what must have been reliable knowledge of what faced us; in the absence of such knowledge, "worst case analysis" would presumably have had a dangerous field day.)

3.) Approaches to testing ever more complicated systems, like simulating an ABM engagement.

4.) Accident prevention when ever more complicated systems prevail. The Palomares accident produced unbelievable problems, like looking for a hydrogen bomb simultaneously at 2000 ft. altitude on land and at 2000 ft. depth in water. More and more imaginative simulation and study of unlikely scenarios are called for.

5.) The theory of deterrence, including behavioral science aspects.

These are only a few of the many areas where more and different defence research is needed.

In concluding this section let me recapitulate. Defence science should continue to be performed in as broad an assortment of institutions as possible, certainly including the universities. The ONR tradition is alive and well, but exposed to dangerous illness. A minimum program for defence science distribution by subject should be: 1.) Support of basic science and high technology areas, without any relevance test, largely in universities, all unclassified. 2.) Support of fields of technology related to military applications, some in universities but most in in-house laboratories and industry, all unclassified. 3.) Support of applications to specific devices and systems, all in in-house laboratories and industry, some classified.

Concluding Remarks

You may have noted two recurrent themes running through my talk: diversity and confidence.

I have in these remarks endorsed diversity and variety in several places: variety in research programs, including areas not immediately applicable to defence; variety in performers of defence research, including diverse universities; variety in testing and accident prevention scenarios and research associated with them; variety in bringing people from various disciplines, countries, and generations to bear on harvesting research. I believe we must constantly work for diversity since the natural forces seem to have a thrust toward a dull and unpromising homogenization. My favorite tribute to variety was the mock examination given by second-year students (under the leadership, I am told, of Jean-Paul Sartre) to first-year students at the Sorbonne. That year the examination consisted of just two questions: 1.) Describe the universe. 2.) Give two examples.

The other theme, confidence, entered in a parallel fashion, since I decried the lack of it: The U. S. Department of Defense lost confidence in itself in the Sec. 203 squabble. The public has lost confidence in the beauty and usefulness of science and in the worth and integrity of universities. Confidence has never been high that we were setting a proper rate in supporting science, or that there was a known relation between science and economic growth. Much of the U. S. public has lost confidence in the Department of Defense because of Viet Nam, and because they contend it takes dollars from the "new priorities."

Yet it is only because of <u>success</u> in avoiding nuclear war that it is possible to be concerned about pollution and population and to be contemplating the twenty-first century. No one would be worrying about over-population if a nuclear exchange were imminent! Thus confidence is a tricky commodity. It probably will not return because of actions <u>within</u> the defence community, but some actions there might help; for example, I believe that if the DoD, and especially the Office of the Director of Defense Research and Engineering, began orienting their actions and their public statements toward the real <u>future</u>, rather than the present or very near future, it would help. But the loss of confidence is so pervasive in current society that confidence will probably return only slowly and possibly by totally unexpected means. As part of this recovery, our generation must regain confidence in the young, and the young must regain confidence in institutions and the future.

I have made so much of confidence partly because I am impressed by Lord Clark's central generalization in his <u>Civilisation</u> films that enduring accomplishments occur only in periods of high confidence. This should serve as an urgent warning to us to recover confidence or have our period go down in history as a destructive age.

There are a few signs of the return of confidence and rationality. In the U. S. the election is over, and somehow we survived again. The DoD did get its courage up to deal with Congress on Secs. 203 and 204. The war in Viet Nam is winding down. There is some evidence of the return of non-conformity after dreadful conformity, especially among the "non-conforming" young. The job market is being realistically studied by college students, especially by graduate students who no longer think emulating their professors is the only or the highest form of life. The relevance kick can't go on forever. Events in the Middle East have caused some second-thinking among the young, many of whom had thought that it was written somewhere in letters of gold on tablets of jade that all power is evil. And internal developments in science continue to demonstrate its richness and beauty, without apparent end.

But I warned you at the beginning that I am an incurable optimist! It's possible that I am simply punch-drunk from the good year we are having at our own university, a year in which we have been able to return to working on improving the quality of education. We have so far survived unscathed the alarums and excursions. And as Winston Churchill wrote 70 years ago, "Nothing in life is so exhilarating as to be shot at without result."

RLS:
12 November 1970

Introduction to Thermal Conductivity 2 Session

Robert L. Sproull
University of Rochester

It is a distinct pleasure and privilege to introduce Dr. Robert Berman.

The reason this Conference in 1992 is at Cornell is that Robert Berman listened to Sir Francis Simon at Oxford in 1948. Let me explain.

Dr. Berman in his 1976 book reports that F. E. Simon, then the high priest of the Clarendon Laboratory's distinguished low-temperature group, suggested that thermal conductivity measurements could be used to investigate lattice defects in non-metals. By 1950 Dr. Berman was already publishing the results of measurements on neutron-irradiated quartz. That and other seminal and suggestive work by Dr. Berman was reported in an invited paper by Dr. W. W. Tyler at the American Physical Society meeting in Columbus in the spring of 1952.

Now I and others of my generation had been weaned on the attitude that phonons *interfered* with really interesting processes, like electronic conduction in metals or the shapes of optical absorption lines in insulators. Dr. Berman's work and Dr. Tyler's paper, along with the nearly simultaneous measurements of the isotope effect in superconductors, convinced me that *phonons could be friends.*

At Cornell in 1952 we *thought* we had some capability for introducing known defects into synthetic non-metallic crystals, although that capability now looks primitive by today's standards. The natural approach to follow up Dr. Berman's ideas was thus to study the thermal conductivity of systems in which the defects were quantitatively specified. Dr. Glen Slack's thesis launched this program auspiciously in 1954, but only when Dr. Robert Pohl arrived in 1958 did it attain the richness and content which brings this Conference to Cornell. Dr. Pohl and his students developed a real capability for introducing known defects and conducted far-reaching and imaginative experiments on thermal conductivity and other phonon processes.

Meanwhile Dr. Berman's central position in phonon processes in non-metals was maintained and enhanced. His experimental program flourished, and his publications also exhibited an important theoretical content, harvesting and accommodating the theoretical contributions of Debye, Peierls, Pomeranchuk, Ziman, Krumhansl, Callaway, and others. His 1953 *Advances in Physics* review effectively established the field under its title "Thermal Conductivity of Dielectric Solids at Low Temperatures," reinforced by his 1976 book *Thermal Conduction in Solids*. His classic 1965 papers on the isotope effect in LiF and solid He, along with his earlier experiments on diamond, are again in the news with his 1991 comment on the thermal conductivity of isotopically enriched diamonds.

On behalf of the organizers of this Conference, it is my privilege to present to Dr. Berman this plaque: "The Seventh International Conference on Phonon Scattering in Condensed Matter honors Robert Berman for his pioneering contributions to the physics of phonon scattering. Presented on August 5, 1992, Cornell University, Ithaca, New York."

In order that this will not be a "dry promotion," the organizers are also presenting this Steuben bowl with their congratulations.

Dr. Berman's title is "It's Quite Normal Not to (um)Klapp."

Robert L. Sproull

Mr. Sproull is President Emeritus and Professor of Physics Emeritus at the University of Rochester. He attended Deep Springs College and received a B.A. and a Ph.D. from Cornell 'University.

During World War II he worked on Navy radar at RCA Laboratories and taught at Princeton University and the University of Pennsylvania. Returning to Cornell in 1946, he was successively Assistant Professor, Associate Professor, and Professor of Physics. He was the founding Director of the Laboratory for Atomic and Solid State Physics and founding Director of the Materials Science Center. On leaves from Cornell he served at Oak Ridge National Laboratories, European Research Associates (Brussels), and as Director of the Advanced Research Projects Agency, U. S. Department of Defense (1963-65). He was Vice President for Academic Affairs at Cornell from 1965 to 1968 and became Provost at Rochester in 1968. He became President in 1970 and Chief Executive Officer in 1974, a post from which he retired in 1984.

Mr.Sproull has served on the Board of Trustees of Deep Springs, Cornell, and Rochester. He has served on the Board of Directors of the Commonwealth Fund, Xerox, United Technologies, John Wiley & Sons, Bausch and Lomb, Sybron, and Charles River Laboratories.

Mr. Sproull has served on many advisory committees, mostly for the Department of Defense and for the Department of Energy. He was the founding Chairman of DoE's Basic Energy Science Advisory Committee. He was a member of the Naval Research Advisory Committee and Chairman of the Defense Science Board. He chaired a committee for the National Academy of Sciences to reorganize the Academy of Medicine .He organized and chaired the Loran Commission for the Harvard Community Health Plan. From 1973 to 1980 he was Chairman of the General Motors Science Advisory Committee. He was a Fellow at the Center for Advanced Study in the Behavioral Sciences.

After "retirement" in 1984, he continued many of these activities and in addition became a member of the Board of Trustees and of the Executive Committee of the Institute for Defense Analyses. He served on missions to Kazakhstan and the Republic of Georgia for the International Executive Service Corps. He was the founding President of the Environmental Literacy Council. He has served as a member of the Science and Technology Advisory Group for the Premier of the Republic of China. He is a Trustee of the China Foundation for the Promotion of Education and Culture.

Among his honors are an Honorary Doctor of Laws from Nazareth College of Rochester, an Honorary Doctor of Music from the New England Conservatory, and the Meritorious Civilian Service Medal of the Secretary of Defense. He is a Fellow of the American Physical Society and a Fellow of the American Academy of Arts and Sciences.

He is married to the former Mary Louise Knickerbocker, and they have two grown children.

Robert L. Sproull

I was born in 1918, weathered the Great Depression in the Middle West, went to a work-study school on a cattle ranch in California, transferred to Cornell University, and received a Ph.D. there in 1943. During the War (for a person my age there was only one war!) I worked on microwave radar. I returned to Cornell after the war as a member of the Physics Department, where my students and I did experimental research in solid-state physics. I was the first Director of the Laboratory of Atomic and Solid-State Physics and of the Materials Science Center at Cornell. I spent sabbatics at Oak Ridge and at a Union Carbide laboratory in Brussels. In 1963-65 I was the Director of the Advanced Research Projects Agency of the Department of Defense. I went to the University of Rochester in 1968 as Provost, soon became President, and retired from the Presidency in 1984. I am now President and Professor of Physics, Emeritus.

During my Cornell years I consulted for Bendix Aviation, Union Carbide, and Xerox. I became a Director of John Wiley and Sons, United Technologies Corporation, Sybron Corporation, Xerox Corporation, and Bausch and Lomb, but have retired from all them. I am still a Director of the Charles River Laboratories. I have been a trustee of Cornell University, Deep Springs College, Wilkes University, The Commonwealth Fund and The Institute for Defense Analyses. During the last seven years I have spent 20% to 30% of my time on volunteer work for the Federal Government; most of that has come to an end.

Mary and I are Florida residents, but we spend nearly half the year in Pittsford, a suburb of Rochester, New York, and at our cottage on Cayuga Lake near Ithaca. We sail, swim, and take care of eight acres of hardwoods. I still have an office at the University, where I help in fund raising and provide a nearly dry shoulder for frustrated young scientists. At Jonathan's Landing, E402, we walk, swim, play tennis, and exercise our little lobster boat, Penelope III from Slip 24.

CURRICULUM VITAE

Dr. Robert L. Sproull

Born: August 16, 1918
Lacon, Illinois

EDUCATION:

1935-1938	Deep Springs College, Deep Springs, California
1940	B.A., Cornell University
1943	Ph.D., Cornell University; major: experimental physics

ACADEMIC:

1943-1945	Taught physics part-time at Princeton University and the University of Pennsylvania
1946-1948	Assistant Professor of Physics, Cornell University
1948-1956	Associate Professor of Physics, Cornell University
1956-1968	Professor of Physics, Cornell University
1959-1960	Director, Laboratory of Atomic and Solid State Physics, Cornell University
1960-1963	Director, Materials Science Center, Cornell University
1965-1968	Vice President for Academic Affairs, Cornell University
1968-1970	University Vice President and Provost, University of Rochester
1968-84	Professor of Physics, University of Rochester
1970-1975	President, University of Rochester
1973-1974	Fellow, Center for Advanced Study in the Behavioral Sciences
1975-1984	President and Chief Executive Officer, University of Rochester
1983	Honorary Doctor of Laws, Nazareth College
1997	Honorary Doctor of Music, NEW ENGLAND CONSERVATORY

RESEARCH:

1942-1943	Thermionic electron emission
1943-1945	Microwave radar
1945-1958	Experimental solid state physics; imperfections in non-metallic crystals, especially in barium oxide
1952-1963	Experimental solid state physics; low temperature physics; phonon scattering

CORPORATE:

1943-1946	Physicist, RCA Laboratories, Princeton, New Jersey
1958-1959	Physicist, European Research Associates, Brussels, Belgium (sabbatical leave from Cornell)
1965-88	Member, Board of Directors of John Wiley & Sons, Inc., Publishers, New York, New York
1966-1973	Consultant, Xerox Corporation, Rochester, New York
1970-	Member, Board of Directors, Security Trust Company, Rochester, New York
1971-1980	Member, General Motors Science Advisory Committee, (1973-1980, Chairman)
1972-89	Member, Board of Directors, United Technologies Corporation
1972-85	Member, Board of Directors, Sybron Corporation
1976-89	Member, Board of Directors, Xerox Corporation
1982-89	Member, Research Advisory Committee, United Technologies Research Center
1982-89	Member, Board of Directors, Bausch & Lomb

GOVERNMENT:

1950-1968	Member, Solid State Sciences Advisory Panel (Office of Naval Research and later National Academy of Sciences)
1952	Physicist, Oak Ridge National Laboratory
1958-1959	Lecturer for NATO in Europe
1959-1963	Member, Materials Advisory Board of the National Academy of Sciences
1963-1965	Director, Advanced Research Projects Agency
1965-1975	Member, Laboratory Management Council of Oak Ridge National Laboratory, Oak Ridge, Tennessee (1971-1973, Chairman)
1966-1971	Member, Statutory Visiting Committee of the National Bureau of Standards (1968-1971, Chairman)
1966-1970	Member, Defense Science Board (1968-1970, Chairman)
1970-1974	Member, Advisory Committee for Planning and Institutional Relations, National Science Foundation
1974-1977	Member, Naval Research Advisory Committee

VOLUNTEER SERVICE AND HONORS:

	Sigma Xi
	Phi Beta Kappa
	Phi Kappa Phi
1945-1947	President, Telluride Association
1954-1957	Editor, Journal of Applied Physics
1962-1963	Trustee, Associated Universities, Inc.
	Fellow, American Physical Society (1954-1955, Chairman, Division of Electron Physics)
	American Association of Physics Teachers
	American Association for the Advancement of Science
1967-74, 83-87	Trustee, Deep Springs College, Deep Springs, California
1967-1973	Member, Visiting Committee for Engineering and Applied Physics, Harvard
1968-1970	Member, Board of Advisors, Center for Educational Enquiry, New York, New York
1969-1971	Trustee, Columbia School, Rochester, New York
1970	Secretary of Defense's Meritorious Civilian Service Medal
1971-	Fellow, American Academy of Arts & Sciences
1972-1977	Alumni Trustee, Cornell University, Ithaca, New York
1974-84	Trustee, University of Rochester (Hon. T., 1984-)
1975-1976	Chairman, Consortium on Financing Higher Education
1975-1978	Member, Board of Directors, United Community Chest of Greater Rochester
1975-1979	Member, Corporate Body, United Community Chest of Greater Rochester
1976-80	Member, Committee on Corporate Associates, American Institute of Physics
1977-	Fellow, American Association for the Advancement of Science
1977-1979	Member, Sloan Commission on Government and Higher Education
1977-1982	Member, Advisory Council, Electric Power Research Institute (Vice-Chairman, 1980-81, Chairman, 1981-82)
1979-1984	Member, Governor's Advisory Council on High Technology Opportunities, State of New York
1979-89	Member, Board of Directors, Commonwealth Fund
1979 - 80	Member, Independent Review Panel, New York Power Pool
1980 - 1984	Member, Advisory Council on Postsecondary Education, New York State Education Department
1980	Rapporteur, The Ditchley Foundation Conference on Mid-Life Education in the 1980s and 1990s, Ditchley Park, England.
1981-82	Member, Fund Raising Implications Subcommittee of United Way of America
1983-84	Chairman, The Twentieth Century Fund Task Force on Conflicts in the Commercialization of Scientific Research
1983	Member, Committee on Science, Engineering, and Public Policy, Amer. Assocn. for the Advancement of Science.

VOLUNTEER SERVICE AND HONORS: (continued)

1983–84 Vice-Chairman, Council of Presidents of Universities Research Assoc.

1984–89 Member, Roundtable Council, National Academy of Sciences
" , Exec. Comm. of ↑

1984 Chairman, Study of the Institute of Medicine Committee, National Academy of Sciences

June 1983
Feb. 1984

Addendum, 1984—

The three preceding pages are essentially complete through July, 1984. After that, I had no paid employment (other than corporate boards) but served in a variety of volunteer activities. I have no systematic list of these. Some of them are the following:

Continuation of participation in GUIR.

Chairman, Loran Commission, Harvard Community Health Plan.

Advisory committees for the U. S. Department of Defense, Department of Energy, National Research Council, and National Academy of Sciences.

Science and Technology Advisory Group, advisory to the Premier, Republic of China.

Trustee, China Foundation for the Promotion of Education and Culture.

Mission to Republic of Kazakhstan, 1993, for the International Executive Service Corps, to modernize and connect to the West the Kazakh Academy of Sciences.

Mission to Republic of Georgia, 1994, for IESC to establish a "think tank" for economic, marketing, and engineering studies for the rehabilitation of Georgia.

Chairman, Independent Commission on Environmental Education.

Chairman, Environmental Literacy Council

Publications

This list is not appended. It contains the usual articles and chapters of books, both research output and articles on education and universities. It also contains two more consequential books: *Modern Physics* (John Wiley & Sons, New York, 1956, 1963, and 1980); in three editions, in five language s. *A Scientist's Tools for Business, (University of Rochester Press, 1997).*

Robert L. Sproull
13 February 2000

• = copy of talk in duplicate speed file box

BOX 1

• 25	April	1956	RPI Talk, Optical Investigation of Crystal Perfection
•	October	1958	Imperfections in Solids, NATO, Oslo
•	Oct-Nov	1959	Talk to Engin Council
•	February	1960	Swarthmore
• 14	April	1960	Syracuse, AICHE Imperfections in Solids
• 24	April	1960	Cornell Engineer
• 4	January	1961	Cleveland
•	March	1961	Cornell Close-up
• 20	March	1961	I. B. M.
• 7	May	1961	Arts College Council
•	June	1961	The Science of Materials (Talk for Cornell Alumni)
• 15	June	1961	Schenectady, NY, Semi-conducting Comp. Conference
•	August	1961	Iowa
•	November	1961	Advanced Lab Seminars
•	February	1962	Juniata College
• 12	March	1962	Centennial Planning Comm.
•	May	1962	Alfred University
•• 5	October	1962	Trustees & Cornell Council
• 25	February	1963	Viewpoints of Material Science
	March	1963	M. E. Seniors
•	May	1963	"Theory of Radiation Grinnel"
• 6	November	1963	Two Kinds of Defense ITT Fishbowl
12	November	1963	Laser Symposium Introduction - San Diego

•	27	February	1964	"Problems in Materials -- The Government Viewpoint" Dedication of Grosvenor Metallurgical Lab, Drexel I of T
•	22	June	1964	Seminar on Nat'l Materials Problems, D.C. - ARPA
missing	18	August	1964	Space Club
•		September	1964	Defense Science Board Luncheon
•	9	October	1964	NSF Talk
•	21	October	1964	AIME Manpower Symposium, Philadelphia
	22	January	1964	AIME Washington "The Government Program Deriving From Materials Sciences"
•	12	November	1964	Xerox Dedication
•	29	January	1965	Dedication of Military Research & Development Center Thailand
•	8	April	1965	Pittsburgh Physical Society, "The Interdisciplinary Materials Laboratory"
missing	13	May	1965	AAAS College, Alaska
•	13	May	1965	Wesleyan University
•	25	May	1965	IDL Laboratory Directors, Talk to
•	2	June	1965	MAB Talk
•		June	1965	Cleveland Carbon Conference
•	20	October	1965	Clark Hall Dedication, Cornell re Early History of
•	9	December	1965	John Wiley Talk
•	17	January	1966	ORNL, "Who Sets Priorities in Science?"
•		Fall	1966	Cornell Freshman Talk
•	20-21	March	1967	OECD - Paris
•	24	March	1967	IEEE Symposium - "New Horizons in Physics"

29-31	March	1967	Conference on Regional Development Planning Barranquitas, Puerto Rico
• 7	April	1967	Dedication of JILA Building, University of Colorado
• 10	May	1967	Harris Committee Report
• 27	December	1967	Symposium on Secrecy, Privacy and Public Information "Is Secrecy in Science Ever Justified?"
•		1967 - 1968	High Frequency Applications
• 15	February	1968	"Education at Cornell" Talks to Phi Delta Zappa and Pi Lambda Theta
•	June	1968	Providence Talk
•	Summer	1968	General Electric: National Defense; Stimulating Material Development
• 3	October	1968	Talk to Trustee Council, U of R
• 7	October	1968	Industrial Research Institute, "The Industrial - Academic Interface: From the Academic Viewpoint"
• 2	November	1968	"Aspirations, Rhetoric, and Action"
• 24	November	1968	Phi Beta Kappa, "Love Letters to the University"
• 13	January	1969	Faculty Decision Making
• 14	February	1969	Forbes Luncheon, "Anti-University"
• 19	February	1969	Alumni Lunch Talk
31	March	1969	Conference on Technology Utilization
•	March - April	1969	Sigma XI Talk
• 8	April	1969	Deferred Giving Committee "Yes, There is a Pilot"
• 5	May	1969	Corporate Open House, "The Anti university"
6	June	1969	Harley School Commencement

• 9 - 11	June	1969	Proceedings of the Third Research Administration Workshop sponsored by the Engineering College Research Council of the American Society for Engineering Education: "A University Viewpoint"
• 24	June	1969	Irondequoit High School Commencement
• 14	July	1969	ARPA Materials Group
• 21	July	1969	Naval War College - Talk to New Admirals
	Fall	1969	Rochester Review: The Public and the Universities A Campus Dialogue
• 15	September	1969	Wing R N Dedication
• 23	September	1969	Medical College Faculty Council
25	September	1969	Talk to Alumni
• 2	October	1969	New Trustee Orientation
3	October	1969	Talk to Trustees
4	October	1969	University Day
28	October	1969	Harrison Howe Lecture
28	October	1969	Introduction of Prof. Martin Schwarzchild
•	November	1969	Review of Campus 1980
• 9	January	1970	University of Hawaii, "Can We Make Science and Technology Work for Mankind?"
• 19	January	1970	"The University and the Community"
• 30	January	1970	"Illiteracy from A to B," talk to Trustees
6	February	1970	RCA Colloquium
24	April	1970	Introduction of Rutherford D. Rogers at Dedication of Library
• 7	April	1970	Pundit Talk "Dislocations and Man"
• 13	May	1970	Administrators Anonymous re "The Nature of the University"

8	June	1970	Pugwash
	September	1970	Freshman Week Talk, "A Personal View of the University"
	September	1970	NYS Commerce and Nuclear Structure Laboratory
13	October	1970	Fortnightly Talk "Vest-pocket Nuclear Fusion"
14	October	1970	Introduction of Clifford Truesdell
4	November	1970	National Academy of Science, Solid State Physics

End of BOX 1

BOX 2

17	November	1970	Defense Research Board, Ottawa, "Defense, Science, and Defense Science."
18	February	1971	John Wiley Speech
31	March	1971	Conference on Psychiatric Education
20	May	1971	Presbyterian Workshop
27	May	1971	Cleveland Alumni Talk
	July	1971	Berkeley Internal Pricing Conference
25	August	1971	UMSCHAU Article
15	September	1971	A & S Chairmen Talk
29	September	1971	Talk to New Trustees
1	October	1971	Board of Trustees Presentation
8	October	1971	AAU Council on Federal Rel.
8 - 10	October	1971	ORNL Report
14	October	1971	Alumni Symposium
19	October	1971	Administrators Anon.
19	October	1971	Pundit Club, "Bombs, Earthquakes, . . Sea Otters"

29	October	1971	First Rochester Conference on Superconductivity
• 13	December	1971	Two Planning Issues for the NSF
• 21	January	1972	AAUP Talk
3	February	1972	Trustees' Council
•	March	1972	Cornell Trustee nominee
4	March	1972	Eastman Kodak Concert
8	March	1972	Detroit Alumni
• 11	April	1972	Fortnightly Club, "Bombs, . . and Sea Otters"
2	June	1972	Trustees' meeting
7	June	1972	5th Int'l Conference on Environmental Toxicity
• 19	July	1972	Chautauqua Institute
•	September	1972	NSF Draft Advisory Com. for Planning
• 5	October	1972	Trustees' Council Talk
• 17	October	1972	Angle's Staff Talk
6	November	1972	Senate Meeting Notes
• 12	November	1972	Manhatten Project
• 1	December	1972	Department Chairmen, re: recruiting, career development
• 30	January	1973	Young Businessmen
• 2	February	1973	Board of Trustees
22	February	1973	UR - AAUP Talk
• 23 - 25	February	1973	"Cogito, etc. Campus Times Article and other articles by other people

29	April	1973	Deep Springs Board of Trustess Meeting
7	May	1973	Senate 10th Anniversary Dinner Talk
14	May	1973	Tri-Cities Project
22	May	1973	Cornell Tower Club
2	June	1973	Chancellor's Luncheon
4	June	1973	Deans' Meeting re: Faculty Promotions
19	October	1973	Cornell Club - San Francisco
11	November	1973	Deep Springs: "Fission, Fusion, and Fussin'"
12	November	1973	Stanford "Ford Fellows"
17	November	1973	Toronto Trustees
3	December	1973	Seminar at Stanford
17	December	1973	Sumn er
18	January	1974	Cornell Talk in NYC
12	February	1974	Fortnightly Club, "A Different Dove"
19	March	1974	Chemical Engineering III
26	March	1974	Pundit Club, "A Different Dove"
20	April	1974	Parents' Advisory Council, 10-year Plan
23	June	1974	Interview by John Czarnecki, Democrat & Chronicle
	August	1974	EOP Students, talk to
11	September	1974	Review of Seitz and Nichols book in American Scientist
30	September	1974	Hartford Alumni
7	October	1974	Corporate Open House

• 11	October	1974	Psychology Building Dedication
• 15	October	1974	"The Years Ahead"
16	October	1974	X-Club, "A Different Dove"
• 17	November	1974	Edmac Talk
• 20	November	1974	Buffalo Alumni
• 8	January	1975	Voelcker Conference
• 18	January	1975	Cornell Class Officers
21	January	1975	Interview by John Winerip of Times-Union
23	January	1975	Interview by Warren Doremus of Channel 10
• 1	February	1975	Inaugural Speech
• 3-5	February	1975	Town Meeting Talks
• 4	March	1975	Chem Engineering III talk
• 7	March	1975	U of R Associates NYC
• 8	March	1975	Alumni, Parents, and Prospective Students, talk to
• 20	March	1975	President's Leadership Council Breakfast
• 22	March	1975	President's Leadership Council
• 15	April	1975	Trustees Development Committee
• 22	April	1975	AAU, Durham, N. C.
• 9	May	1975	History Department, Commen.
• 10	May	1975	NROTC Commissioning
•	May	1975	Med School Commence.
• 10	June	1975	Philadelphia Alumni
• 14	June	1975	Cornell Alumni Symposium
• 9	July	1975	Washington Alumni

• 13	July	1975	Summer Orientation Program Picnic
• 4	September	1975	ESM Convocation
• 10	September	1975	Trustees' Orientation
• 16	September	1975	Nursing Convocation
• 19	September	1975	Trustees
21	September	1975	Dorris Carlson Professorship
• 24	September	1975	Statement to Carey
• 1	October	1975	Whipple Society, "Jaws"
• 3	October	1975	Col. Engin. Applied Science "Energy in the 90's, a Look Ahead"
• 3	October	1975	50 - year plus alumni
• 3	October	1975	50th Anniversary Banq. School of Medicine & Nursing
• 4	October	1975	SMH Rededication
• 4	October	1975	Alumni Annual Report
• 18	October	1975	Parents' Advisory Council
• 22	November	1975	Dangerous Lack of Soviet Education, Times-Union
	August	1975	Russian Trip
• 28	October	1975	Fortnightly Club, How the Botanists..
29 • 1	October– November	1975	President's Leadership Council Symposium
• 17	November	1975	San Francisco Alumni
• 10	December	1975	Sybron Board Talk
• 10	December	1975	Torch Club "A Different Dove"

•	11	December	1975	Administrators Anon. New Paradise: Soviet Union
	7	January	1976	Associates Program Associates Medal: W.J. Maxion
•	22	January	1976	Trustees' Council re Rochester Plan
	30	January	1976	Miami Alumni
•	22	February	1976	Bicentennial Concert
•	23	February	1976	Bernstein Conference
•	8	March	1976	Awards Dinner
•	13	March	1976	Boston Alumni
•	1	April	1976	Frontiers of Medicine
	2	April	1976	Laser Lab Corner. Ceremonies, Intro of Wm. Carey
	4	April	1976	Associates Program. Intro of C. McCollister Evarts
	6	April	1976	Ruth Merrill Center Dedication
	7	April	1976	Stackel Room Dedication
	30	April	1976	2nd Rochester Conference on Superconductivity no notes
•	1	May	1976	Gannett Emergency Room Dedication
	4	May	1976	School of Medicine & Denistry Convocation
•	6	May	1976	Trustees' Council re 5% Plan
	6	May	1976	Headliner's Dinner
	7	May	1976	George Graham Smith Plaza Dedication
•	7	May	1976	Trustees' Dinner
•	9	May	1976	River Campus Commencement
	10	May	1976	Capital Campaign, Major Gifts Phase
•	10	May	1976	Commoner
•	11	May	1976	Interview by US News & World Report
•	11	May	1976	USIA Taping

	• 13	May	1976	President's Society Talk
missing	17	May	1976	Capital Campaign Greater Rochester Phase
	• 23	May	1976	Medical School Commencement
missing	8	June	1976	Dedication Eisenhart House
	• 22	June	1976	Letter to new Students (not published)
	• 9	August	1976	National Junior Classical League
	• 2	September	1976	Freshman Welcome Assembly, Part of SOP
	• 10	September	1976	Arts & Science Fac.
	• 12	September	1976	Fall Convocation - Yellowjacket Day
	• 15	September	1976	Capital Campaign Gannett Foundation Gift
	• 15	September	1976	Old Friends' Group: "Fission, Fusion, & Fussin'"
	• 17	September	1976	ACUSNY Panel Talk
	• 21	September	1976	UR Alumni lunch - "Fission, Fusion, & Fussin'"
	• 22	September	1976	Intro of George Ford Wed. Evening Lecture Series
	• 27-28	Sept	1976	"Assessing Doctoral Programs in NY/Conf.
	1	October	1976	U of R Rush Rhees Cup Trustees/Trustees' Council
	• 5	October	1976	Senate: Faculty Compen.
	• 6	October	1976	Wilson Day Remarks
	• 28	October	1976	Hanson Birthday Con.
	• 5	November	1976	Detroit Alumni
	• 6	November	1976	Kilbourn Hall, EMT
	? 25	May	1976	"University Today & Tomorrow" Conference at Leeds Castle.
	18	November	1976	President's Leadership Council, Breakfast

	• 21	November	1976	Interview by Bruno Sniders, D&C
	• 5	December	1976	Dedication of May Room in Wilson Commons
	• 11	December	1976	Dedication of Speegle Pool
	• 13	December	1976	Jewish Comm. Federation Dinner/M. Goldberg
End of Box 2	• 14	December	1976	Dedication of Shumway Dental Clinic
Box 3	• 6	January	1977	ERDA group
	• 30	March	1977	Intro of Hecht at Wed. Evening Lecture Series
	• 13	April	1977	Intro of H. Brown
	• 23	April	1977	Dedication of Jean Ann Brown Room in Wilson Com.
	• 6	May	1977	Dedication of Gowen Room, Wilson Commons
	• 6	May	1977	Trustees' Meeting Remarks
	• 6	May	1977	Trustees' Dinner
	• 8	May	1977	R. C. Commencement
	• 15	May	1977	Seminar on Research in Educational Administration
	18	May	1977	CC of Rochester, Old Friends, intro of T. Penn
	• 22	May	1977	Med. School Commencement
	• 26	May	1977	Alumni/Community Phase Orientation: Cap. Camp
	• 8	June	1977	"Quantum Optics" 4th Conf on Coherence & QO
	13	June	1977	President Society Dinner
	•	June	1977	Farewell Speech to Cornell Board of Trustees
	•	July	1977	"Hail & Farewell" re Dale Corson Retirement/Cornell
	• 21	July	1977	Capital Campaign, Alumni/Community Orientation
	• 24	August	1977	Talk to R.A.'s
	• 29	August	1977	PBS Program on Brown's Ferry
	• 1	September	1977	Freshman Orientation
	• 5	September	1977	CT Article to Freshmen
	• 9	September	1977	Nursing Convocation

•	11	September	1977	Yellowjacket Day
•		September	1977	Trustees, Talk to
•	26	September	1977	Capital Campaign, Alumni/Community, Prism
missing	11	October	1977	RLS' remarks re University '77 and 27 October 77 article in Brighton-Pittsford Post
•	24	October	1977	Greenbrier
•	29	October	1977	Parents' Council
•	29	October	1977	Delta Zeta Installation
•	8	November	1977	Fortnightly Club: Sumner Line or How to invent...
•	2	February	1978	Trustees' Council
•	3	February	1978	Talk to R.A.'s
	3	February	1978	Trustees' Meeting
	7	February	1978	Pundit Club, Sumner Line or how to invest just enough
	8	February	1978	Intro of Prof. J. W. Johnson at Wed. Evenings
	1	March	1978	University Women's Club, RLS' welcome speech
•	9	March	1978	RAC Conference on Computers & Instruction
•	9	March	1978	Capital Campaign, Alumni/Community Phase Victory Celebration
•	12	March	1978	San Diego Alumni
•	15	March	1978	Strengthening the Humanities, Mellon Foundation
•	10	April	1978	Spike Garnish Dinner
	12	April	1978	CT article on RLS' speech at Garnish Dinner
•	12	April	1978	AAU testimony before Senate Committee on Appropriations for Department of Defense
	21	April	1978	Prism Concert Speech
	5	May	1978	RC Commencement Rehearsal Speech
•	7	May	1978	RC Commencement
•	9	May	1978	Buffalo Alumni Talk

•	12	May	1978	Wallis Day
•	21	May	1978	Medical School Commencement Speech
	16	June	1978	President's Leadership Council Talk
•	6	September	1978	NYC Kick Off Dinner
•	9	September	1978	Governance of Higher Education Today, Seminar Series
•	10	September	1978	Yellowjacket Day
•	19	September	1978	Boston Kick-off dinner
•	22	September	1978	Sam Havens Award
	28	September	1978	Buffalo kick-off dinner
•	29	September	1978	Fred Dennis Plaque
•	30	September	1978	Corporate/Political Open House
	2	October	1978	Syracuse kick-off dinner
•	4	October	1978	Intro of Charles Townes
	9	October	1978	D. C. Kick-off Dinner
	10	October	1978	Philadelphia Kick-off
•	17	October	1978	LLE Dedication
•	20	October	1978	50th Class Reunion
•	20	October	1978	Alumni Dinner
•	25	October	1978	Trustee Orientation
•	26	October	1978	Eastman Dental Cnt. Dedication
•	28	October	1978	Parents Council Talk
•	3	November	1978	Fred Gowen Memorial
•	12	November	1978	"Annie" Reception
•	7	December	1978	DoE Meeting, Reston
	10	January	1979	Capital Campaign, Alumni Nat. Phase, Los Angeles

11	January	1979	San Francisco
12	January	1979	Chicago
9	February	1979	Remarks to UR Board of Trustees
13	February	1979	Intro Harry Gove at Wednesday Evening
22	February	1979	Statement to Gov. Carey re Laser Laboratory
2	March	1979	Energy Research and Production Subcommittee of the House Committee on Science & Technology testimony
29	March.	1979	DoE meeting notes
5	April	1979	Energy & Water Deve. Com. of the House Appr. Com. Test.
11	April	1979	Energy & Water Deve. Subcom. of the Senate Appro. Committee testimony
12	April	1979	Kant Conference
19	April	1979	Sons of the American Revolution Speech; Const. for the Twenty-first Century
20	April	1979	Executive Development Program GSM Speech:Outside Director
23	April	1979	American Physical Society: Research and the Public Interest
8	May	1979	Women's Club Talk: State of the University
11	May	1979	Remarks to Seniors at RC Commencement Rehearsal
11	May	1979	Remarks to the Board of Trustees Dinner
13	May	1979	River Campus Commencement
15	May	1979	Welcome at Gradient Index Optics Conference
27	May	1979	SMD Commencement
31	May	1979	Remarks at GM Sci Adv. Com. 50th meeting
4	June	1979	Intro of Harry Gray at President's Society Dinner

8	June	1979	Remarks at George Engel Retirement
28	June	1979	Intro of Senator P. Moynihan at S. B. Anthony Day
	July	1979	Bureaucrats and Brainpower: Government Regulation of Universities: Federal Regulation and the Natural Sciences
24	July	1979	Testimony before US House of Representatives Com. on Ways and Means for AAU
4	September	1979	Talk to Retired Faculty
7	September	1979	Nursing Convocation
9	September	1979	Fall Convocation, Yellow Jacket Day
19	September	1979	University Convocation and Young Inauguration
20	September	1979	Intro of Dr. Donald A. Henderson at Joseph C. Wilson Lect.
26	September	1979	Intro of Paul Warnke
8	October	1979	Optics Dinner
19	October	1979	Alumni Banquet
20	October	1979	Prism Concert, Livingston Biddle
20	November	1979	Fortnightly; The Legacy of TMI
8	December	1979	Fairbank Alumni Center Dedication
12	January	1980	Associates' Dinner of the Eighties (Medal to Eleanor McQuilkin)
4	March	1980	Eastman Kodak Centennial, Eastman Exhibit
6	March	1980	Gates Lecture, Intro of Albert Rees
20	March	1980	New Conference, Sports Center
23	March	1980	Interfaith Chapel's Tenth Anniversary
25	March	1980	Introduction of Ambassador Eilts, Wilson Award Winner
3	April	1980	Interview w/Ron Robitaille, RG &E

• 9	April	1980	Introduction of Robert J. Barro, Wednesday Evening Lect.
14	April	1980	Introduction of John Romano, Roch. Plan Early Selection Bq.
• 15	April	1980	Committee on Corp. Support of Private Univ.
• 15	April	1980	Statement by RLS, Faculty Senate
9	May	1980	Introductions, Morning Session, Trustees Mtg.
• 9	May	1980	Remarks at Commencement Rehearsal
• 9	May	1980	Remarks at Trustees Dinner
• 11	May	1980	Commencement Greetings
21	May	1980	Trustees Orientation
• 25	May	1980	SMD Commencement Ceremonies
• 23, 26-27	June	1980	Science Policy Workshop, Maryland
• 24	June	1980	Remarks at the Associates Picnic
• 15	July	1980	Wilmot Bldg. Naming
• 15	July	1980	Dedication of Colonade, Eastman Kodak Centenl.
15	July	1980	Kodak Centennial Dinner
• 16	July	1980	Investment Seminar for Deans
• 3	September	1980	Year of the Aging Community Luncheon
• 5	September	1980	Remarks at the Alumni Admissions Workshop Sam Havens Award to L.G. Rigby
• 7	September	1980	Yellowjacket Day
• 12	September	1980	Nursing Convocation
• 18	September	1980	Remarks at the Presidents Society
• 19	September	1980	Tribute to James Wilmot, Trustees Meeting
19	September	1980	Introduction Remarks at Trustees Mt. Riker & Niemi

	• 19	September	1980	Presentation of Rush Rees Cup, Trustees Lunch
End of Box 3	• 19	September	1980	NRC Comm. on Natl. Educ. Policies in Sci & Engr.
Box 4	3	October	1980	50th Anniversary Luncheon
	3	October	1980	25th Class Gift Acceptance
	• 3	October	1980	Alumni Banquet Remarks; Slide & Film Presents.
	• 6	October	1980	Goldberg Career Library Dedication
	• 9	October	1980	Garms Inauguration
	9	October	1980	Introduction Sen. Pell at Garms Inauguration
	• 15	October	1980	Campaign Dinner Remarks
	• 27	October	1980	Alumni Luncheon, Rochester area
	29	October	1980	Introduction of Thomas Clarkson, Wed. Eve. Lec.
	• 14	November	1980	Remarks Concert 15th Anniversary National End. for the Arts
	• 1	December	1980	Remarks Concert Capital Campaign Edn Celebration
	• 3	December	1980	SA Sponsored Africa Talk, Hartnett Gallery WC
	• 9	December	1980	AAU Meeting NYC Century Club
	• 10	December	1980	DOD Symposium
	• 8	January	1981	Neuman Memorial
	16	January	1981	GSM Open House
	16	January	1981	Ingrl. Todd Audtrm.
	• 22	January	1981	Lilly Foundation Fellows Talk
	• 24	January	1981	Remarks at Rochester Symposium for Physics Students
	• 27	January	1981	Corporate Support of Private Higher Education Conference Board, Palm Beach

6	February	1981	Introductions Board of Trustees Meeting
• 20	February	1981	Lubin Farewell
26	February	1981	A Modest Proposal for more Effcnt. Chtbl. Gvg.
• 3	March	1981	Remarks at Howard Hanson Memorial
• 4	March	1981	Wall Street Journal Article: A Modest Proposal for Efficient Chrtbl. Gvg.
• 10	March	1981	AAU-ACE Patent Seminar, Atlanta. Un. Research and the Public Intr.
25	March	1981	Introduction of Lewis Thomas, first Friedrich Dessauer Lt.
• 2	April	1981	Investiture and Welcome.HUnter Inaug.
• 3	April	1981	Testimony on Univ. & Defense Prep. befr. Comm. on Armed Services, House of Rep., 97th Cong. on behalf AAU, ACE and NASULGC
• 9	April	1981	Intro of Dr. Robert J. Haggerty Poison Control Conf. 25 years celebration
8	May	1981	Intro. Morning Program Bd. of Trustees
• 8	May	1981	Commencement Rehearsal Remarks
8	May	1981	Unveiling of Eisenhart Portrait ESM
8	May	1981	Remarks after dinner. Bd. of Trustees
• 10	May	1981	Commencement Greetings
• 12	May	1981	Nigeria Talk University Dinner Club
• 24	May	1981	SMD Commencement Ceremonies
• 29	May	1981	SMD Seminar: Relationship of the Univ. to the Medical Center
4	June	1981	Introduction of Pro. Barro. CNA Dinner
• 19	June	1981	Commencement Address General Motors Inst. Flint, Mich.
• 29	July	1981	Testimony before Subcommittee of House Armed Services Committee re; Doe/NRL Relationships with Industry & Unv.

• 10	August	1981	Answers to Congress John P. Murtha's Questions Testimony before Subcomm. of House Armed Service
• 8	September	1981	Lilly Fellows Talk
• 10	September	1981	Welcome Kodak Plaque Unveiling UR Cancer Center
• 13	September	1981	Remarks Yellowjacket Day
• 16	September	1981	Remarks Wilson Day Hubbell Aud. & at Eastman concert.
18	September	1981	Introductions Bd. of Trustees Meeting
• 18	September	1981	Remarks Presidents Society Dinner MAG
• 23	October	1981	Remarks 50th Reunion Luncheon
• 24	October	1981	Remarks Salomone Celebration
24	October	1981	Welcome Alumni Reunion Acad. Fair
• 25	October	1981	Remarks Installation of Minister First Baptist Church
• 28	October	1981	Remarks Howard Hanson Mem. Conceert
• 29	October	1981	Remarks Wilson Award Luncheon
• 31	October	1981	Parents Welcome
3	November	1981	Introd. of Prof. Ronald Breslow, Kodak-UR Chem. Conf.
12	November	1981	Welcome Dean's Lecture
• 17	November	1981	Fortnightly speech "The Strange World of Nuclear Deterrence"
18	November	1981	Introd. of R. Gordon Douglas, Jr. Wed. Evening Lect.
• 9	January	1982	Remarks Associates Dinner
• 11	January	1982	Remarks Ded. Zornow Sports Center
• 15	January	1982	Remarks Washington Alumni Dinner
• 16	January	1982	Remarks Washington Alumni luncheon Lessburg Xeroz Trng. Ct.

• 19	January	1982	Pundit talk "The Strange World of Nuclear Deterrence
5	February	1982	Introduction of David B. Kassing Bd. of Trustees Meeting
• 16	February	1982	Talk to the Faculty Senate
• 24	February	1982	Talk to Rochester Engineering Sox. "Engineers & Potholes"
•	March	1982	RLS article Harvard Bs. Review "Making Charitable Giving more Efficient"
• 4	March	1982	Ideas for WRUR
• 10	March	1982	Talk to Faculty
• 7	April	1982	Talk at NSF Workshop on Coop. and Shrng. Among Microele. Res. Cntrs. Dulles Marriott, Wash. D.C.
• 20	April	1982	Talk at AAU mtg.
1	May	1982	Remarks at Jerald Graue Memorial Service
• 3	May	1982	Remarks at Ground-breaking Cer. Med. Cntr. Psyc.
7	May	1982	Introduction of John Reeves morning session Bd. of Trustees
7	May	1982	Commencement Rehearsal
• 7	May	1982	Remarks at Board of Trustees Dinner
• 9	May	1982	Commencement Greetings
• 21	May	1982	Talk to NY Alumni Group at Cornell Club
• 23	May	1982	SMD Commencement Ceremonies
• 24	May	1982	Remington Lobby Dedication, Zornow Sports Center
14	June	1982	Introd. of Hugh Van Horn at CNA Dinner
• 16	June	1982	Talk to OFG at Country Club, "A Fair Deal for Donors"
• 6	July	1982	Keynote address to Wiley International Group Conf. at Niagara-on-the Lake
• 8	July	1982	Talk at NACUA Conference, New York City

15	July	1982	Remarks Summer University Dinner
22	July	1982	Introduction Schmitt Organ Recital Hall Dedication
• 26	August	1982	Remarks at Dedication of Keating Bldg.
• 7	September	1982	Talk to New Faculty and Lilly Fellows
• 12	September	1982	Remarks at Yellowjacket Day
17	September	1982	Introductions Board of Trustees Meeting
• 21	September	1982	Talk to Med. Cntr. Admin. Group, "What is ahead for the university"
• 23	September	1982	Remarks at the 2 Millionth Book Celeb. University Library
• 27	September	1982	Remarks at the Hylan Building Dedication
• 30	September	1982	Remarks at the Eastman Theatre Sixtieth Anniv. Celebration (Kodak night)
1	October	1982	Award Honorary Degree Enid Botsford Orcutt Eastman Theatre, Sixtieth Anniversary Celebration
• 6	October	1982	Introduction of Robert Hughes, Wilson Day
• 13	October	1982	Xerox Talk on Recruiting at Xerox Square
• 13	October	1982	Remarks at Trustees Orientation Dinner
• 15	October	1982	Remarks at Trustees Orientation
• 15	October	1982	Remarks at 50th Class Reunion Luncheon
• 16	October	1982	Remarks at Alumni Academic Fair, Breakfast with the President
• 21	October	1982	Talk to Young Faculty
• 27	October	1982	Remarks at Wilson Award Lunch
11	November	1982	Remarks at Waasdorp Dedication SM&D Neurology
• 16	November	1982	Talk at University-Industry Relations Conference at Madison, Wisc. "Protectionism and the Universities"
• 18	November	1982	Remarks at LLE Sponsors management meeting

	• 24	November	1982	Remarks at Cameros Lecture
	1	December	1982	Introduction of Sir Nevill Mott at Dexter Lecture
	• 6	January	1983	Talk in San Diego "Fusion R&D as a Cooperative Government-Industry University Venture"
	• 14	January	1983	Science, Vol. 219, Num. 4581 article "Protectionism and the Universities"
	• 15	January	1983	Remarks at Kennedy Center Recp. Eastman-Stargell Concert
	3	February	1983	Introduction of Kende & Hayes to Bd. of Trustees
	• 10	February	1983	Talk to NYS Legislators at R.I.T.
	• 1	March	1983	Statement to House Budget Comm., talk Task Force on Energy and Tech. for AAU, NASULGC, and ACE
	• 11	March	1983	Talk to Pediatric Fellows Seminar on Admin.
	• 25	March	1983	Talk at CCSPU Seminar, Stamford, Conn. "Private Res. Unv.)"
	• 30	March	1983	Talk to Faculty Council College of Arts & Science
	• 5	April	1983	Talk to Meridian Society
	• 6	April	1983	Talk to Medical Center Exc. Comm. "Strengths & Weaknesses of the Unives."
	• 14	April	1983	Talk to Trustees' Selection Committee
	• 16	April	1983	Talk to Campaign for Admissions, Bill Brown's Committee
	• 20	April	1983	Talk to University Management Forum
End of Box 4	• 25	April	1983	Remarks at Senate 20th Anniversary
Box 5	6	May	1983	Introductions at Bd. of Trustees Morning session
	• 6	May	1983	Remarks at Commencement Rehearsal
	6	May	1983	Remarks at Board of Trustees Dinner
	• 8	May	1983	Commencement Greetings
	8	May	1983	Interview, Channel 21 Commencement
	10	May	1983	University welcome Opening ceremony department of PSych.

• 22	May	1983	SMD Commencement Ceremony
• 25	May	1983	Remarks at Presidents Society Life Members Dinner
8	June	1983	Remarks at Experimental Therapeutics Dv. opening Cancer Ctr.
10	June	1983	Remarks at Meckling Dinner
• 12	June	1983	Welcome at GSM Commencement
• 13	June	1983	Talk to Quantum Optics Meeting
20	June	1983	Remarks at Bartlett Dinner, Genesee Valley Club
• 10	July	1983	Welcorning remarks Summer university group
1	September	1983	Remarks at Lander Aud. dedication (Huch. H.)
6	September	1983	Remarks at Presidents Society Life Members' Dinner
• 11	September	1983	Yellowjacket Day Remarks
• 14	September	1983	Welcoming remarks at Robert L. Berg, M. D. Symposium
16	September	1983	Introduction for morning program. Bd. of Trustees' meeting
• 18	September	1983	Talk to Steuben Society "Language and the University"

23 September 1983	CNA "All Hands" Talks
• 30 September 1983	Remarks at Alumni 50th Reunion Lunch
• 14 October 1983	Remarks at ESM Cecile Genhart Memorial Concert
• 15 October 1983	Talk to American Association of Physics Teachers, Hoyt Hall
15 October 1983	Introductions, Threshold's Tenth Anniversary Gala
• 17 October 1983	Welcoming Remarks at Cameros Lecture, President Jimmy Carter
19 October 1983	Introduction of Horace Barlow, Wilson Day
• 7 November 1983	Talk at 25th Annual Meeting of National Council of Research Administrators, Washington, D.C., "Research, Universities, and the Public Interest"
11 Nov. 1983	Welcome, MacAvoy lunch.
• 11 November 1983	Inaugural Convocation, GSM Dean Paul MacAvoy
• 22 November 1983	Pundit talk, "Of Time and the Sea, or Compensation <u>vs.</u> Isolation"
• 22 Nov. 1983	Remarks at Faculty Senate, 5% Plan
• 1 Dec. 1983	Talk to Young Faculty
• Jan 1984	*Currents*, Case Publication
3 Feb. 1984	Board Meeting, introductions
• 3 Feb. 1984	Tribute to George Graham Smith
• 3 Feb. 1984	Talk to Board, afternoon session
7 Feb. 1984	Fortnightly. "Of Time and the Sea, or Compensation VS. Isolation"
2 March 1984	Welcome, Cancer Center Symposium/
• 6 March 1984	Remarks, 28th Annual CICU mtg. Albany.
• 27 March 1984	Talk, 25th LASSP Anniversary, Detroit.
• 3 April 1984	Testimony before Senate Judiciary Committee's Constitution Subcommittee, Washington.
• 7 April 1984	Talk, Undergraduate Engineering Council banquet.
• 11 April 1984	Talk to Presidents of Hamilton, Colgate, Hobart, Ithaca, Syracuse, Nazareth, Cornell, in Ithaca, NY.

18 April 1984	Wednesday Evening Lecture at UR, "The Strange World of Nuclear Deterrence."
4 May 1984	Remarks at Board of Trustees' Dinner
6 May 1984	Commencement Greetings
8 May 1984	Remarks at Wiley ~~Board~~ Annual Meeting, NYC
19 May 1984	Remarks at Dexter Perkins Memorial
20 May 1984	SM&D Commencement Greetings
10 June 1984	GSM Commencement

Carnassa Park

Robert L. Sproull
7 October 2003

Mary and I were among the founders of Carnassa Park. We participated in all of the, seemingly interminable, meetings and did much of the amateur pre-surveying. Planning and negotiating occupied many of our evenings and weekends from 1944 until we left in April 1946.

But it was all so long ago. In addition, and more seriously, I admit susceptibility to Mr. Dooley's famous difficulty: "It ain't what I doesn't know that hurts me. It's what I does know that ain't so." You have been warned.

One thing I can be clear about, however is the "impetus" for the creation of Carnassa Park. RCA Laboratories was totally occupied with war work and thus was entitled to priorities for construction. The Lab was opened (I believe) in the summer of 1942. If that had been in peacetime, developers would have been building apartments and houses simultaneously with the construction of the Lab. But of course there were no priorities for such. This situation was exacerbated by the social structure of Princeton: At the top were the big estates. Next were the New York City commuters to Wall Street. Then were the Princeton University faculty and staff. Finally, at the bottom, were the new rabble working at Penns Neck, RCA plus Hayden Chemical Company.

The resulting acute shortage of rentable housing was especially bad for uis, since we did not arrive until April of 1943. We lived first in a room on Witherspoon Street, then in an attic over a garage on Rosedale Road, then in the guest house (former slave's quarters) on C. J. Young's rental of an estate on Princeton-Pennington Road, and finally in a third floor made from an attic in Penns Neck. The situation was helped somewhat by the circumstance that we were almost never at home. I taught physics in Palmer Lab two nights a week and later taught a classified course in microwaves at Camden; Mary worked for the League of Nations at the Institute for Advanced Study.

Not only was the housing in short supply but the vacant space on which to build near the village of Princeton was almost zero. Many of us at RCA had explored and found we would have to commute a fair distance to RCA and to Princeton shops and sarvices if we built on available lots.

Thus the lunch table discussions at RCA in late 1944 and 1945 were dominated by questions of housing after the war. I do not remember who discovered a farm that could be acquired. It seemed like a major piece of luck, since there was little property close to town that could conceivably be developed. Once that possibility was raised, organizing a group of interested potential participants went rapidly. I do not remember any selection of participants; I believe everyone who was interested was accommodated in the initial group, but people who wished to join later could not do so. Since the over-all size was fixed, the average lot size was fixed.

There were many discussions about roads. The only thing I remember is that there was some kind of contest for naming roads, and I suggested the name "Random Road." I wanted a two- syllable, English language word to make it easy to give orally to others, and I liked the sound of the alliteration.

The only firm memory I have of conflicts was the problem of restrictions on sale to others. There was a group who proposed a tight and narrow racial restriction. The group was not numerous but was determined; I remember only the spokesman. But a counter group of two or three of us led by Don North secured the votes and a strategy. At the next meeting of the participants, Don introduced a resolution that listed so many specific kinds of purchasers who would be excluded that the whole idea of racial restrictions was laughed out of consideration.

It was that affair plus a number of less consequential Carnassa Park arguments that led me to discover a law of human behavior: Real estate brings out the worst in people. I have observed over 50 years many proofs of this law. Since Carnassa Park seems to be flourishing, I guess the conclusion is that the worst in the cheerful gang who participated in it was and is not very bad.

ROBERT SPROULL INTERVIEW Part One of two

Edited for spelling and clarity, questions condensed, reviewed and corrected by R. Sproull
Copies filed at Telluride House, Ithaca; Deep Springs College Archives; R. Sproull; B. Edmondson

Digital audio at Deep Springs Archives and with Brad Edmondson, (607) 272-1832, brade@lightlink.com.

BRAD EDMONDSON: Okay, it is Saturday, September 14th, 2002, and I am here with Robert Sproull, Deep Springs Class of 1935. Bob, why don't you start off by telling me how you first learned about Deep Springs? Where were you and what was your first knowledge of it?

ROBERT SPROULL: My father worked for a power company, which was all around Chicago, but not in Chicago. Chicago was the Commonwealth Edison Company, and he was Public Service Company of Northern Illinois, and he moved us every two years. My first two years of high school were in what was then called Deerfield Shields Township High School, now called Highland Park High School, one of the very best high schools in the nation. Of course, I didn't know that. It was just a high school. But there a couple of things happened. I was a pretty good student, and my homeroom teacher was already thinking about what's going to happen to students after we graduated even though I was only a freshman and a sophomore at Deerfield Shields. He put me in touch with two people, John Burchard and Jack DeBeers, both of whom were Deep Springers and had gone to Deerfield Shields, and so I talked with them. My mother was sort of an expediter of all of this. She was already looking ahead to see what was going to happen. You realize this was the bottom of the depression, and so you didn't just casually think that you might go on to college. You started wondering how you were going to do it. Burchard and DeBeers, especially DeBeers, got me excited about Deep Springs, but it seemed like a terrible long shot, so few people.

At the end of my sophomore year we moved to a little town called Morris southwest of Chicago because my father had been promoted and had become the manager there. That's one of the worst high schools in the nation -- not because the teachers don't try, but I remember my physics teacher didn't have a clue as to what physics was all about. I was very proud of myself then arguing with him about things, but I became ashamed of myself much later. I was proud of myself because he quit and became a farm implement salesman.

BE: You drove him from the field.

RS: I drove him out of it. Later on, ten, twenty, thirty years later I really am kind of ashamed of myself. That's not a good thing to do for an inadequate teacher. I guess he was trying. But anyway, it was a terrible high school, but nevertheless I'd had the seeds of this business of Deep Springs, and also from Deerfield Shields I knew what universities were like to some extent, and I thought that I -- I applied for Telluride House in the spring of '35.

BE: Had you learned about that from--

RS: Yeah, from Burchard and DeBeers both of them were Telluriders and I came, in fact visited the house that spring and at that time was interviewed for Deep Springs here in Ithaca, but I took the--. I don't think it was called the College Aptitude Test in those days. It was administered by the College Board, and it was an aptitude test. I was thinking of an eastern university [but] didn't know how I was going to pay for it. Also I wanted to be an electrical engineer. So I started looking at various entrance requirements, and everybody required trigonometry. Of course, trigonometry wasn't offered in my high school. But I started it by myself with the books from the previous College Board exams, which were published by Ginn and Company, maybe still are. I don't know. Anyway, I took the exam in trigonometry and the aptitude test, and then curiously enough I got a letter from the dean of Deep Springs, Dean Crawford. I think it was Walter Crawford, if I remember correctly, inviting me to come to Deep Springs. Now that was a particularly--

BE: When did you fill out the Deep Springs application?

RS: Oh I don't know. In the winter some time, probably January or February before I came to Telluride House to be interviewed.

BE: Were there a lot of essays on the Deep Springs application?

RS: It was an essay as I recall, just one.

BE: Just one.

RS: I think.

BE: Now there's eight or nine.

RS: Oh great. I'm all for it. I'm pushing writing ever since including my Ph.D. students.

BE: Another question I had was about your trip to Ithaca to be interviewed, did you take a train?

RS: Train and bus, mostly bus.

BE: By yourself.

RS: Yes.

BE: Was it your first trip to Ithaca?

RS: Oh yes, it was my first trip probably outside of--no, I had gone to Kansas where my grandparents lived. First trip of any sort, really.

BE: It was your first trip certainly by yourself.

RS: Yes. Yes.

BE: So it must've been a fairly momentous thing for you.

RS: Oh I don't know. I didn't scare easily. I still don't. It was -- yeah, it took some doing. It took some planning. The problem with my family, both my mother and father but especially my mother, were interested in my brother and me getting an education. The problem was we didn't have any money.

You've probably never heard of Samuel Insull, have you? He was a great friend of Edison's and a great hard righter in the 1930s. He was almost single-handedly responsible for the Securities and Exchange Commission. He died in Leavenworth Prison.

BE: What did he do?

RS: What he did was to buy up the good stock in operating electric companies all around the northern middle west and create a thing called Middlewest Utilities and issue stock, which was almost a hundred percent water. He hung on to the good stock of the operating companies. If you worked for one of the operating companies--you probably don't believe a word I'm saying. It happens to be true.

BE: Oh, I believe you.

RS: But if you worked for one of the operating companies and you wanted to keep your job, you turned in all of your Public Service Company of Northern Illinois Stock and exchanged it for Middlewest Utility stock. I've given shares of it to my kids to light fires with since. I don't think I still have any shares. I wish I did. But yeah, it was absolutely criminal. I think they finally got him on tax evasion because there really weren't any laws against this kind of stealing. You could cheat your stockholders out of their life savings.

BE: That sounds so much like Enron.

RS: Yeah, well.

BE: That's really striking.

RS: People think the SEC was there forever. It wasn't. It was created by Roosevelt in the 1930s in response to the Insull scandal primarily, but obviously other things came out of the woodwork because as soon as they started investigating that one--

BE: So did your family lose a lot?

RS: Everything. But my brother was older. A little bit of cash savings and so on helped him to go to the University of Illinois, but I was two years behind. Sorry, I was only one year behind. I was two years in age, but only one year in school. But, the coffer was empty. I had a Harvard College fellowship, but the chances of getting a job in Cambridge then were just slight in 1935. I'd have to have a job to go there, or I wouldn't have anything to eat.

So I worked in the cornfields in the summertime. I might have gone on working in a farm for the rest of my life for all I know, but this letter came along from Crawford. Of course, I immediately accepted, and then the problem was how to get there. But that turned out to be solved. Bill Spalding who was a second year man, a returning student, had a car. So a group of us, Spalding and I and two others, drove out and with practically no expenses, eating hamburgers and that's about all.

BE: That's interesting. Twenty years ago, I talked to the men who were in the original classes of Deep Springs, and they did not have a group of alumni and students to get advice and counsel from. Almost all of their Deep Springs contacts started with a personal approach from Nunn or one of his inner circle. But it seems like the alumni recruitment and student support, which is such an important part of the college now, had already been established by the time you got there.

RS: Well, it was established by Johnny Johnson. E.M. Johnson, did you ever know Johnson?

BE: No, I've certainly heard a lot about him but I didn't get a chance to meet him. Was he already at Telluride by the time you were there?

RS: Oh yes. He was sort of a failed English instructor at Cornell in the early twenties or the mid twenties, and he was hired by Telluride Association after they fired Fannie Noon -- Frank Noon. That was when L.L. sort of lost interest in the Telluride Association. He thought that the boys made a terrible mistake by not keeping Frank Noon as the chancellor of the Telluride Association. There was a gap of several years during the war, and shortly after the war they hired Johnny. For all of his failings -- like me, he talked -- but for all of his failings, he did an enormous job of recruiting for Deep

Springs, interviewing for Deep Springs, getting people interested in the Telluride Association. But it was a function that people I don't think really appreciated.

He was a little bit naïve in many respects. Any time some newspaper article would say that people were going to the moon, Johnny would say that people are going to the moon. They eventually did, of course, but long after he died. But Johnny carried on correspondence, and I suppose I must have met him when I was here in April at Telluride House. He was the guy who managed the paperwork for interviews and things like that.

BE: Describe him physically. Was he a tall man?

RS: Oh no. He was short and rather hunched over and limp. He was lame.

BE: One leg or two legs?

RS: One leg, I think. Not terribly lame, but such that you had to slow down as he walked. It was costing him a lot of effort to walk, to keep up with you.

BE: How would you describe his voice?

RS: Oh that's a fascinating question. He had very well enunciated language. In fact he taught speech at Deep Springs from time to time.

BE: Public speaking or elocution?

RS: Well, both. From time to time somebody would go on a kick and say that we ought to teach it formally. It would last about two weeks. But he also taught drama, and in fact I'm getting ahead of myself, but in my first year at Deep Springs he came for about two months in the winter time and taught drama. I've been interested in it ever since. We put on the play *East of Eden*, and we also did *Richard III*.

BE: Tell me about your trip west with these guys that you'd never met before. You jumped in a car and off you went.

RS: Well, everybody was feeling out the other guy, of course.

BE: Who was in the car?

RS: Bill Spaulding is the only one I remember. Unfortunately, I had a book of black and white prints, but I've given that to Deep Springs for archives several years ago. If I had it, perhaps I could remember names.

BE: Well, maybe I can get the pictures digitized and sent to you.

RS: I remember there was one big hang-up when we were going across New Mexico. Bill was a great sort of amateur anthropologist. He wanted to go to the Zuni Indian Reservation, and the rest of us didn't. But he owned the car. So I've forgotten how it came out. All I remember -- that was the only brouhaha on the trip. But the car held up, and we held up, and we got there on time. We had an interesting tour through New Mexico, the Grand Canyon.

BE: Did you sleep out? Did you have bedrolls?

RS: No, we slept in tourist camps. It didn't cost much in those days. A dollar a night was a lot.

BE: Had you had much experience with camping, sleeping out, riding horses, that sort of thing?

RS: I had never ridden horses except on my grandfather's farm, just working horses. Camping, just a little. I had gone to a Camp Edwards, I think it was called, up in Michigan some place, which was a camp associated with our church. I wasn't much of a churchgoer, but my parents were. I don't remember any overnight camping on my own or with a group of people. I was a Boy Scout. So I must've done that. But I changed when we went to Highland Park -- I became a Sea Scout.

BE: Had you seen mountains?

RS: No, I had never seen a mountain. I had seen lots of pictures, but I had never seen a mountain. I'll always remember the preacher at our church, a guy by the name of Wenzel, a marvelous guy. After I had been at Deep Springs for some time, my parents drove west to visit to see what it looked like. After she came back, my mother said something to Mr. Wenzel about the lake they saw in the mountains. This reverend said, "You must be wrong. There can't be any lakes in mountains. The water would all flow down out of them. There are no lakes in mountains." You could get away with that if you were a preacher in north Illinois, just because nobody had ever been outside of town.

Well, anyway, the answer to your question is kind of a qualified no. But I had worked on farms. So I had gotten used to that sort of thing.

BE: So the labor wasn't unfamiliar to you.

RS: Labor was not. I worked ten hours a day in Illinois corn fields.

BE: Yeah, farm labor.

RS: In July and August it gets pretty hot.

BE: So you were ready for whatever--

RS: I was not a strong character, but I was not delicate. I didn't get sick much or anything like that, but I also didn't have much strength. In a group of people, I would not be the person who carried the biggest load even if I tried hard. One summer in the cornfield, I was saved by working on the inbreds. The man who owned the farm, Chester Hunt, was trying to develop a white hybrid. There were yellow hybrids then. It was a great thing in hybrid corn. Anyway, working inbreds meant reaching up about so high, being a midwife, actually not the midwife but the pimp.

BE: You were pollinating.

RS: Pollinating. Well, that was nice because it was not as hard a job as de-tassling way up over your head with a machete, which is very hard work.

BE: Do you recollect your first sight of Deep Springs, or your first hours there? What did you think of the place? Did you have the same feeling that many do, that you had gotten into something you weren't sure you wanted to stay with?

RS: Not at all. No sir. I guess I was partly getting adjusted with three other Deep Springers going out there, so I didn't have to do it all at once. So I knew their first names and where they lived and what their idiosyncrasies were, et cetera. The only thing I was scared of was whether I could make the grade academically. Because I knew if I had stayed on at Deerfield Shields and had gotten the grades that I had, I'd have no problem, but I didn't. I got nothing but "A"s at Morris High School, but what the hell. "In the valley of the blind, the one eyed man is king." So that's the only thing that frightened me. But of course, those chickens didn't come home to roost until the end of the semester. By that time I had decided I could deal with it.

BE: So you felt at home and comfortable from the beginning.

RS: Not really at home, but not really worried. It was like a farm, and I knew what a farm was like. It settled into a comfort, I guess -- well, not really comfort, but an understanding of what I had to do, and thinking that I could do it.

BE: Was there much of a distinction between second year and first year men?

RS: A great, great distinction. Our first year class had twelve students in it, which was unusual. Remember it was a three-year school then. So you would expect to get something like seven or eight [per class] or something like that, or maybe twenty or twenty-one students all together. Well, we had twelve because a lot of the third year class, what should've been the third year class, had resigned or been fired. I'm not sure which, never was sure which.

BE: That scandal had played out before you arrived.

RS: That's right. It really wasn't a scandal -- it just was a wart on the distribution. So there were twelve of us in the first year class. So we were distinct. It was upper classes versus the first year class, and there was a real difference. Of course, they had all the elected offices, and we just behaved as kind of the junior members of the community.

BE: Were you treated as an equal or were you not?

RS: Well, we were treated as equals in the student body. Of course, we voted like everybody else. But as I say we were essentially separated from the student offices because they had the voting power and the seniority, et cetera.

BE: Did first year students sit at dining tables with second year--

RS: No, no. There wasn't any segregation. The tables were determined by when you got off work or what you were doing or whether you had something you had to discuss with people.

BE: So that sort of free interchange, where you could casually walk up to the president of the college and talk about whatever you wanted, that was there.

RS: Oh, sure. The faculty -- Larry Kimpton was a fresh caught Ph.D. student from Cornell. He had just gotten his Ph.D. in philosophy the year before he came to Deep Springs. Walter Crawford was the dean, and he was not so much an anti-intellectual as a non-intellectual. We all thought he was a fairy. I'm not sure. I don't think anybody had any evidence or at least I never had any, but he just behaved that way. Incidentally, that's the only suspicion I ever had of homosexuality in connection with Deep Springs. Once when I gave a swan song, Deep Springers accused me of being terribly naïve. They said there must've been a lot of that, and you just didn't know about it. I said, "No, I don't think so."

BE: I had the same experience. I had absolutely no inkling of anything.

RS: When we get around to it, I'll tell you about some bisexuality. There was a fair amount of it.

BE: Well, was P.N. Nunn at the college?

RS: P.N. was the chairman of the board of trustees, and he may have been nominally the president, but we didn't really have a president. It was the dean who ran the thing, and that was Crawford.

BE: Was P.N. on campus very much?

RS: No, but he was there at the beginning of the year. I remember, of course the geometry is different now, and I haven't been there since the main building has been renovated. So my geometry goes two generations back.

The main building, the room as you walked down the door toward the library on the left there, was a dean's office in the post-war period. It was also a conference room or it could be a seminar room and so on. The first day that school was in session, P.N. got the whole twelve of us around that table in that conference room, and he had a big battery box like this for his hearing aid. Hearing aids didn't come in little tiny transistorized things then. The transistor was still years away. So there was the battery box in the center and his little earphone and so on. I remember as if it were yesterday. He sat us all down, and then he had a long stage pause and he said, "Young gentleman--" and then another pause --"you are here to be exploited." Then he went on. You probably remember one of L.L.'s letters or in the gray book or some place, where he says that you essentially have to serve now. You have to pay back to Deep Springs. There are various ways you can do this and so on. P.N. enlarged on that theme by saying "you're to be exploited." We were all scared to death.

BE: What did he mean by that?

RS: Well, it was just an exaggeration of L.L.'s point of view.

BE: I see. You're raw material, and we're going to shape you.

RS: Not so much that as the fact that you're raw material. We're going to educate you, and then you're going to have to pay back to society for your education. That's the exploitation.

BE: I see. I see.

RS: You be P.N. over there, okay.

BE: Okay.

RS: No, no. Sit down. You're there at the end of the table. I'm over here, okay. And over there, in my line of sight, would be Bob Henderson, and here are the other members, the other ten members. So you [P.N.] start lashing out to me. Let's imagine that I said something you didn't like. Now, Henderson is over there--

BE: And he's making reindeer antlers on his head with his fingers.

RS: To try to throw me off so I couldn't look seriously at P.N.

BE: Oh no!

RS: Well, Henderson and I settled down to be chums right away.

BE: He was still a mischievous guy when I was a student and he was a trustee.

RS: Well, I'll tell you some of those things. You ought to get some anecdotal fun in here sooner or later. But meanwhile, what I want to say is that the gain control on his (P.N.'s) hearing aid was on that box. If he didn't like what I said or what somebody else said, he went over and ostentatiously turned down the gain.

BE: So he couldn't hear you anymore. That's great.

RS: I remember thinking, gee whiz. I can't wait until I get hard of hearing! I'm going to do the same thing. But you don't have that kind of opportunity any more.

BE: So he was--

RS: P.N. was there and everybody was scared to death of him, but he didn't stay more than a week or two in the fall. He didn't like the climate. He was set up in San Diego then with a menage a trois.

BE: The famous menage a trois.

RS: Well, I can tell you about that later because I stayed with them for several nights once.

BE: Tell me a little bit about that now. Do you think that it was because he had lived in Utah for a long time?

RS: I don't think that had anything to do with it. I think he was just a captain of industry and he decided he could have a mistress. That's all. . . He was just a guy who was accustomed to getting whatever he wanted. He was accustomed to having much more money than other people who were around.

The woman's name was Julia, or something like that. He called her Juliet. Her name was Hamilton, Mrs. Hamilton. She turned out to be an aunt of Harry Scott. I don't know if you ever knew Harry Scott.

BE: I've heard that name.

RS: Deep Springer, San Francisco lawyer, now dead unfortunately. He went to Oxford once on the Cornell track team.

BE: So everyone knew about it, and nobody said anything about it. Mrs. Nunn seemed fine with it.

RS: Oh, she was great. Well, let me tell you another anecdote about P.N. because you're not talking to many people who knew either one of the Nunns.

BE: That's right. P.N. is a big figure in the story, and the portrait of him needs to be fleshed out.

RS: In my senior year, we were completely rebuilding the heating system. It was an absolute disaster. It was woodfired, and it was a lot of work cutting wood, particularly in the fall, to go through the winter season. We were going over to oil, and we did this without a contractor. What we did was to act as our own contractor and go directly to vendors to get the engineering of individual pieces. Then we put the thing together with student labor. I was the--what would it be called now--the I've forgotten what the military's term for it is. The integration contractor, or something like that. I just did the engineering design of putting the pieces together, each one of which was thoroughly designed by the vendors. For example the furnace itself, the registers, the sizing, the piping and so on. I didn't have the competence to do more.

BE: Was there a mechanic?

RS: Yep. The mechanic was named Perry, and I've forgotten his last name. The mechanic kind of supervised the actual plumbing that went into it in the end. But most of the plumbing was already there. So it was mainly a case of replacing the furnaces and the controls, and the controls for the most part I did. I mean, I actually did the wiring of them. What modification of plumbing that wasn't done by the vendor of the furnaces, I think Perry did. But anyway, I put the plans of the thing together, but it had to have P.N.'s approval. He was the chairman of the board of trustees, and he called himself an engineer because he had the title of engineering. He didn't have any engineering training. He was a schoolmaster, as you know. But anyway, it had to have his approval. So it fell to me to take all of the plans and the vendor's materials down to San Diego and stay with the Nunns for three days, which I did in January, 1938. I was a third year man. It was an interesting third year. I'll get to that eventually.

P.N. was signing by hand a bunch of Telluride Power Company bonds. He was making a big deal out of signing P.N. Nunn, P.N. Nunn. Nobody in his right mind would do that, even then. He didn't have anything else to do. But we'd have a session several hours each day on going over the plans, and I was explaining things to him. He was sharp. He understood -- he tried to zero in on things like capacity and so on, and what would happen if this happened, and so on. But in general it was fairly benign. He didn't have any real objections to anything. He was more pro forma than anything else.

I have always remembered having breakfast there. I stayed with them in their house, a beautiful big house in San Diego, and each morning we would all sit down. There were four of us at breakfast -- P.N., Juliet as he called her, and Mrs. Nunn, and as I remember, he always called her Mrs. Nunn. But I'm not sure, I'm not completely sure of that. I was just trying to think the other day, and I can't really dredge up anything else. I don't think he ever used her Christian name, and I don't know what that was. Anyway, everybody had their pills in front of him, a whole bunch of pills, half a dozen different kinds. And each one had a newspaper--

BE: The same paper?

RS: I'm not sure. That's an interesting question. I think it was the same paper because they were comparing stories.

BE: Oh, sure. It wasn't possible to get a *Wall Street Journal*.

RS: Oh, heavens no. Nobody in San Diego at that time had ever heard or the *Wall Street Journal* or the *New York Times*. Anyway, the conversation then would start. Somebody would start with something about something crazy that had happened. These terrible things, mostly in Washington, et cetera, et cetera. Remember this is 1938 now. It's well into the second Roosevelt administration. The conversation would start in different ways, depending on which article people had read. But it would always end with the same expression by one of the three, and this happened three days running. One of them would say, "Oh well. What can you expect with a crazy man in the White House?" Every single morning for three days, this conversation ended in the same way.

BE: And then all was silent.

RS: Oh boy. But meanwhile, I wish I had had a tape recorder with me. P.N. was talking about his days in the power business, et cetera, talking about this that and the other thing as if I had memorized each one of the Telluride locations.

BE: Did he tell you about Niagara Falls?

RS: He talked about that, yeah. Have you ever seen the plaque there?

BE: Yes.

RS: The one that says P.N. Nunn, chief engineer?

BE: Yes. I guess that the Nunns' power house was just recently decommissioned, just last year. It ran until last year.

What was your knowledge of or what was your use for L.L. Nunn? How did L.L. Nunn affect your life at Deep Springs?

RS: Well, the canon was L.L. Nunn.

BE: It had been collected into a book that you were given.

RS: Oh yes, the gray book, so-called.

BE: There was a gray book.

RS: Yeah. Yeah, of all of his letters.-

BE: Were you tested on it?

RS: No. The students that I talked with later, when I was a trustee, had a different view of [The Gray Book]. but maybe just in my hearing. They took kind of an Olympian view -- "oh well, that stuff." It was just like the Bible to them. Nobody really believed in it. It was just kind of like mythology. But they read it because they had to, and they didn't want to really spit on it because they liked what was coming out of it. That wasn't our attitude, really. We were, I think, much more inclined to think that it was worth reading. We didn't throw bricks at it. We didn't quote from it to win arguments or anything like that.

BE: Was there anybody at the college who had known L.L. Nunn?

RS: Oh sure. Well, not in residence, but there were people. Dean Thornhill, for example, was a frequent visitor. All of the trustees had known him, and they had their meetings. One meeting a year at Deep Springs, and one meeting a year in Los Angeles. So we got to know those people. Then there were people like O.B. Suhr. He was a frequent visitor. He was an engineer who had done a lot of things. As a matter of fact, it was really rather strange that I defended the oil heating system to P.N., because it ordinarily would've been Suhr. But I guess it was a big enough job that P.N. thought he ought to be in the act to bless it.

BE: Let me just ask you one more question about Nunn. I recently heard a story about a meeting that happened shortly after Michael Eisner became the CEO of the Walt Disney Company. He was newly in the job, and he was at a lot of meetings with old time Disney employees. He would suggest something, and people would go, "Oh Walt wouldn't like that. Walt wouldn't do it that way." Eventually he got irritated and said, "Until Walt walks through that door, I'm in charge."

Was there a lot of "L.L. wouldn't like that? L.L. wanted it this way?"

RS: No. No, but--

BE: There was no cult of L.L.

RS: There was not a cult of L.L., but there was a feeling that these guys, particularly the trustees, were somehow blessed because they knew and were chosen by L.L., and you didn't and weren't. There was definitely that feeling.

BE: So you were encouraged not to talk back.

RS: That's right. The feeling was that they had a special route to God for whatever they said. But they didn't quote him. We sort of got the impression that they didn't read! [laughter] Oh, the trustees were an absolute mess. Why don't we go on with my experience? I do want to come to the trustees and the various things about them, and so on.

[I want to talk about] my first job, Henderson and I were the laundry boys, which was the second worst job. The orderly was the worst job. I don't know how it was in your day.

BE: Yeah. It was certainly the lowest status job.

RS: But Henderson, being what he was, , wouldn't have changed that assignment for any other job at Deep Springs. We had a hell of a good time. You talked about whether the first year people had any problem with second and third year men. Well, from time to time we would make a mistake. For example, a dark sock might get in with the white shirts. We did have white shirts in those days. We had to dress up for Sunday dinner and for other things.

BE: Not for weekday dinners.

RS: No.

BE: So you would wear your work clothes to that.

RS: No, but you wouldn't wear a white shirt.

BE: Oh I see.

RS: But you would wear casual clothes, sort of like what we're wearing now.

BE: You would change clothes for dinner.

RS: Oh sure. Oh God, yes. We had a mud room, and we used it.

Anyway, if for example we got one of the dark socks with a white shirt and it dyed it, one of the students who had it might complain. The next week, the complainer would probably find that he had starch in the armpits of the shirt or in the crotch of his undershorts. They didn't complain after that.

Henderson was always playing jokes on somebody or other. So we managed with our own vest pocket freshman class, and even though we were at the bottom of the pecking order, we had a great time. The other thing was that since we were at the bottom the first semester, we kind of were owed something. So we got the best job the second semester -- namely the garage, the mechanics shop. There were two of us, one in the morning, one in the afternoon.

BE: Working with cattle was not the most highly sought after job?

RS: No, because there was very little to do with cattle during the winter and spring. The most respected job from the standpoint of prestige was dairy, because in effect that was the place where a bad mistake would be devastating, either a sick cow or a sick Deep Springer. But the most sought after job was the mechanic's job. We learned everything. We learned to take apart engines and put them back together. Perry -- wish I could remember his last name -- was a great smoker and beer drinker. He would sit off at the edge of the big room there in the garage, giving orders and suggestions -- just sitting down on the floor smoking with gasoline around everywhere. I forgot what he called us -- not pinheads, but something, some derogatory name like that telling us what to do.

BE: Was he drinking beer?

RS: No, not seriously on the job, but off the job he did. He was smoking on the job. We took it in stride. We all rather liked the idea that that gave us more responsibility. On the spring trip that year, which was down to Death Valley, we didn't go very far. We had an old international truck, which was dated back I think to 1924, but I'm not quite sure.

BE: I've seen many pictures of it.

RS: Yeah anyway, we started down Death Valley Wash, and the front wheels went like this [splayed outward]. The whole bar that connected the two had given out, and so Henderson and I went up to Death Valley Scotty's Castle. We came back with a little pick up truck or something like that with welding equipment. We welded the thing back together again and went on with our trip.

BE: How did you jack it up?

RS: Oh we had equipment, and we may have borrowed that too. We borrowed anything we needed from Scotty's Castle, which was a pretty modern sort of place up there.

BE: Was that your only experience with Scotty?

RS: Yeah, he came down to us that night. We camped out at Grapevine Springs, and he came riding up on his Palomino, and he was using this characteristic language. We told him we were going up to the castle the next day. He started in telling us all about it, that Mrs. Johnson was still presiding over it. I don't know what her name was. He called her Mabel because she reminded him of a bareback rider he once knew. So he called her Mabel, and he, he gave us all sorts of yarns. You've been there have you?

BE: Sure.

RS: He talked about the big organ. "Yeah, Mabel wanted an organ, so we had to put the organ in," he said. "It's a hell of thing because it costs thirty-five dollars a month just to blow smoke up its ass." [laughter] That's the way Scotty was. But a bit paunchy, just a great big belly.

BE: Doing the cowboy thing to the hilt.

RS: Trying to act like a cowboy.

BE: Yeah.

RS: Well, anyway. Back at the ranch--

BE: Meanwhile, back at the ranch--you're the mechanic. Finishing your first year. Talk about the classes.

RS: Okay. I've already told you that for a couple of months, Johnny Johnson taught drama. Peterson, who was a young guy, was very serious but a really scholarly character. I must say I owe a great deal to him. He taught political science and literature and was kind of the central academic figure. I don't know where he had been before he came to Deep Springs. He had been there one year before, so this was his second year as I recall. Then there was Larry Kimpton who taught philosophy and German, I think. He as I said, he had just gotten his Ph.D. just the spring before at Cornell. I don't think Crawford taught anything, but maybe he did.

The first year there was no science or mathematics, but I read a lot of symbolic logic, and I guess it was with Kimpton. I guess he liked to learn symbolic logic because I was fascinated by it. I guess maybe Kimpton got me started on it. I started reading it and fell in love with it. So I took drama and history and political science with Peterson, and the symbolic logic was not a course at all. I just read it, and I learned more than if I had had a course.

BE: I was going to ask you about that. How much of the learning took place outside of classrooms? You were tutored in symbolic logic informally. When the class time ended, did you frequently keep talking about it over meals and amongst yourselves?

RS: Yes, there was quite a lot of that. With two young guys, Peterson and Kimpton, there was an awful lot, particularly because neither one of them was much interested in the ranch or for that matter the mountains. They were not hikers or climbers or anything like that, so the conversation at table tended to be more "intellectual" than it otherwise would be. In later years, people like geologists and chemists and so on were among the faculty, and people were very excited about camping and mountain climbing and so on. I think the table conversation and the out of class conversation then was not so dominated by what you might call raw intellectual stuff. I don't know.

BE: What did Crawford bring to the table?

RS: Nothing.

BE: He was kind of inert--

RS: An incredibly weak, vapid sort of guy.

BE: That's so common at Deep Springs -- that the president of the college is almost a vestigial figure.

RS: Well, he had been around I don't know how many years. He succeeded Thornhill. As I say, I think he was a homosexual, but I don't think he ever -- he certainly never tried anything on me, and I don't think he tried anything with the other students. We may be dead wrong. It may be just that he seemed so sort of sheepish and womanly.

I remember one incident that sort of catapulted us into the second year, involving David Alan Makepeace McConnaughy. Have you ever heard of McConnaughy? Within a day after he had arrived, Henderson started calling him the Senator, and he's been Senator ever since. All the time at Telluride House, he was Senator. His wife even called him Senator. But anyway, David McConnaughey, the Senator, came to Sunday evenings. We used to have Sunday evening meetings which were not really church services, but which frequently had some music or somebody would give a talk or have a visiting professor or somebody else talk on Sunday evening. Anyway, I remember vividly McConnaughey seeing Crawford there for the first time milling around before that Sunday evening talk. Sen was very shy, and I do mean very shy.

BE: Why was he called the Senator?

RS: Well, you almost had to know him. It was his mode of speaking.

BE: He was senatorial.

RS: He had a formality to everything he did. It was senatorial.

BE: Oh I see.

RS: It wasn't the shy, non-shy axis as you suggest, but a kind of a cognitive dissonance. The axis of formality/informality is what got him the nickname. He shouted Virgil to the mules, for example. But that was after he had been called the Senator for some time. But it worked. It was just a tone of voice. If you really mean it and are serious about it, the mules get the idea.

Anyway, back to this incident. There's the Senator standing seeing Crawford for the first time and just turning this way and that and not looking at Crawford at all. He was apologizing over and over again for the fact that he had promised Crawford when he was interviewed--. He had interviewed in Southern California, which is where he lived -- that he'd promised him that he would play the hymns on Sunday nights, and he had to come clean. He had not practiced.

BE: So you're a second year man now. Is the first year class that comes in after you a normal size? Does the size of your class mean that the class after yours is smaller?

RS: Well, yes. Well, our class was attenuated pretty seriously. It was not at all expected that you would be automatically reinvited. We went through a big invitation process.

I just realized that I loused up that completely that McConnaughey thing.

BE: Well, that's the beauty of editing. Why don't you try it again.

RS: I'll go back at it. Let me first of all finish off. I've forgotten now exactly how many, but not more than nine of us, maybe only eight of us survived into the second and third years.

BE: Were those decisions made completely by the grownups, or were the students involved?

RS: By the grownups. The students had almost exactly zero to do with it.

Let me straighten out the McConnaughey story. I don't know how I got so screwed up on that trying to--I guess I was going back and forth in time. It didn't have anything to do with Crawford at all. It had to do with Kimpton. It was during the second year. The Senator had promised Crawford, who had interviewed him during my first year. Crawford interviewed for the people that would then become the new students coming in my second year. He'd interviewed McConnaughey, and McConnaughey had promised that then if he was accepted, he would play the hymns. But by the time this incident occurred it was the first meeting of the fall of my second year. McConnaughey, a new first-year man, was apologizing to Kimpton, who was now the new dean and director. Kimpton could care less about hymn playing on Sunday night. He may have once been to church himself, but he just didn't want to have anything to do with it. Anyway, I got one year ahead of myself.

BE: So Crawford left after your first year.

RS: Crawford was fired--

BE: By?

RS: By the intrigue that Kimpton built up with the trustees. From the day he arrived at Deep Springs, Kimpton started angling to throw Crawford out and become dean himself. He was a consummate politician. He just was ambitious for the job, and he started buttering up the trustees right away. He knew how to do it, and he had the stomach for it. He could say things that would never get out of my mouth -- sucking up, as the students used to say, to the board.

Back to your question -- reinvitation was entirely a trustee and faculty task. Since the trustees by the spring of 1936 had determined they were going to fire Crawford, Crawford had nothing to do with the reinvitations. Kimpton, who was the rising dean, the one who was going to become dean and director, was the guy that had the most to do with it. So if anybody had been trying to look good with Crawford to try to become reinvited, he had made a terrible mistake. I don't know if it had anything to do with the selection process or not. All I know was that everybody was worried about it. I wanted very badly to stay. There were some people who resigned and didn't want to stay. Everybody completed the year. The business of not completing the year happened much later. In our case, nobody ever quit during the middle of the year. I don't think somehow or another that you would have lost standing with your fellow students -- it just never even occurred to anyone to do that. But there were a lot of men that quit at the end of the year. Some of them quit at the end of the year, and some of them were essentially fired at the end of the year.

Back to the out of class environment. One of the things that happened to me is that I read a lot in the library, which was very spotty then -- much more spotty than it is now. But I started reading electrical engineering, and of course because of Nunn's things, there was a lot of that there. Then I got interested in sort of the first chapter of those electrical engineering books, which were concerned with physics. I had never even heard of physics as a profession.

BE: Is that right?

RS: But I got interested in it. By the time I transferred to Cornell, I was a physics major, not an engineer. That was specifically because of the out of class reading at Deep Springs. There was nobody on the faculty that had a prayer as to what physics was, but it was all in the library.

BE: So you had no one to talk with about physics.

RS: That's correct. Later on in my senior year we had a physics course, but it was taught by a mathematician named Mersman. The guy really didn't know any physics. Again it was Henderson and me. We had a spectroscope. I don't know if you ever worked with a spectrometer. Ours had been dropped and manhandled. The whole museum then had been full of rats. The equipment was just terrible. We tried the spectroscopy experiment that Mersman put before us. We tried. We couldn't for the life of us find the major lines that we were supposed to find. Presumably the calibration of the thing was off by whole degrees, not just minutes. So we said well, what the hell. We took the numbers *from The Handbook of Chemistry and Physics,* which are out to seven significant figures, and we put those into our report, knowing that Mersman would see them as absolutely impossible. He never suspected a thing.

BE: I have sympathy for him. I taught a class at Deep Springs once, and I had the sensation of being pursued by hungry dogs. The students were so bright and so hungry for more material, more reading, that if you didn't stay ahead of them, you would just be consumed.

RS: I taught too. When we get to the third year, I'll tell you about that. If we ever get to the third year. Are you game?

BE: Sure.

RS: Well, as you see, I talk. We're back in the first year. Let's stay in the first year for a little while. I already have given you one anecdote from the spring trip, but we didn't get very far. I think we got only as far as Death Valley. I don't think we got as far as Boulder Dam. How shall I say this -- the luxury/frugality axis was way over on the side of frugality. We didn't have a decent truck, and we didn't spend money on trips et cetera, but we did have a trip up Big Pine Canyon one weekend, and a number of us we went.

BE: I know from looking at the records that in 1935 to 1938, the finances of the college were absolutely scraping. A lot of the faculty were going without pay, and the college was as close to the bone as it had ever been. Did you ever experience basic hardship, like lack of food?

RS: No. No.

BE: Lack of heat?

RS: No. No. The heat was all wood-fired, and we had plenty of wood. It was just labor. Fuel was gasoline for the tractor, and the belt on the tractor drove the saw. No, we were not hungry.

BE: So as far as cash outlay goes, probably faculty labor was the major budget item.

RS: That's right, and the faculty didn't have a lot of choices. A fresh caught philosophy Ph.D. in 1935 didn't have very many jobs to choose from. I don't know what kind of recommendations Larry had. He was no scholar; so he probably didn't even have good recommendations from people like Ned Burtt, the philosophy professor here at Cornell.

We weren't hungry, but we felt that school was very likely to go down the tubes almost any year. We gradually learned things about the board, partly by being student trustee and partly by just talking with people like Harold Waldo, who was really the only decent person on the board. Later on Parker Monroe was added, but he was a New York City financier and he really wasn't all that interested. But Harold had been an associate of L.L.'s. He was a Provo lawyer and was very interested in Deep Springs partly because he had two sons that he wanted to send there and eventually they did go -- both Charles and what was the other one's name? All I remember was that Charles had very big ears. The expression was that "even the Waldos have ears."

BE: Was Waldo a Mormon?

RS: Yes. Anyway, we all knew about the Telluride Motor Company, which the trustees had their own personal investments in.

BE: This is different than Telluride Power.

RS: Oh yes. It was the Ford dealer in Provo. They had put a lot of their own funds into it and they owned it, and then it didn't do very well because people weren't buying cars. So they put the Trustees of Deep Springs funds into it, and then it did even worse.

BE: [disbelief] The Ford dealership in Provo?

RS: Yeah.

BE: I had no idea.

RS: Yeah, and a lot of money went down the tubes. If it had been reinvested in 1935, 36, or 37, you could have made a fortune. Instead it went into the Telluride Motor Company and pretty much went down the tubes. I don't guess it actually failed, but it just didn't produce any cash to speak of to help out. Then there was some investments that were regular stocks. I don't know what, and I don't know how much.

BE: It's really interesting. I'm still in Telluride Association, and I'm currently the chair of the custodians, and the difference between how Deep Springs has managed its money and how Telluride Association has managed its money is really interesting to me.

RS: Tell me about it!

BE: Deep Springs has done such a bad job over the years.

RS: Well, it has done worse than a neutrally bad job. It has done an actively bad job because the trustees got their personal funds mixed up with the trustees of Deep Springs funds. I think it's actionable, but nobody ever took them to court.

I was a Telluride Association custodian too. In fact, I was a custodian before I was twenty-one. When I left Deep Springs in 1938, I immediately became a member of Telluride Association, and later I was made secretary of Telluride Association, and at that time the secretary was automatically a custodian. That was changed later. But I became a custodian by being secretary when I was, I think, twenty.

BE: Did the custodians at that time really have free rein?

RS: Completely.

BE: So John Johnson was not looking over your shoulder.

RS: Johnny had nothing to do with the custodians. Not a thing.

BE: And there was no oversight.

RS: There was a quasi-- how shall I say-- a quasi professionalism that came from Sid Walcott. Have you ever heard of him? He was a Buffalo stock broker, and he was the only professional.

BE: An alumnus.

RS: Yeah, an alumnus, and we had a kind of an obligation from time to time to ask for his advice. Of course the only time I can remember we took his advice, it was disastrous. It was the Crow's Nest Pass Coal Company, but let's not get on to that.

The thing that saved us and made the difference that you just cited was that the Telluride Custodians had a kind of know-nothing approach. We decided that we didn't know anything. But curiously, as a custodian you were supposed to read and almost memorize Graham and Dodd, the thing that Warren Buffett claims made his fortune for him. *[Ed note: Benjamin Graham and David Dodd, authors of the classic investing book* Security Analysis*]*. But anyway, that's I think the thing that saved the Telluride Association custodians. We didn't think we knew anything, so we played very conservatively, and then there was the fifteen percent blowback provision. It was the combination of feeling that you weren't very smart, and that you were certainly not trying to outsmart the market, and that you had read Graham and Dodd and were trying to do the best you could with what feeble information you could get. We couldn't get much -- the information flow was nothing like what it is now.

Let's finish up my freshman year. I should say something about what was going on around the ranch. MacKenzie was ranch manager. I've forgotten his first name because everybody called him Mac. He was Scottish-- it was not Robert or John. It was more complicated than that. But anyway, I do remember Mrs. MacKenzie's name. She was Evangeline. She was a tall rather stately woman, and an excellent horseback rider who loved to ride. MacKenzie himself was short, of course, and also an excellent rider. I think, he knew the ranching business very well. The problem was, as later was found out, he was running his own cattle up in Long Valley and mixing them up with the trustees cattle. This was not established until after I left Deep Springs, but it was pretty well understood by us.

I don't know whether I knew this in my first year, but probably by my second or third year I realized that the sleeping arrangements were mixed, to say the least. Larry Kimpton was sleeping with Evangeline MacKenzie. Charles Collingwood, a student, was sleeping with Mrs. Kimpton.

BE: I didn't know about Collingwood and Mrs. Kimpton, but I did know about Kimpton and MacKenzie. That blew up in the trustee correspondence. It was pretty openly discussed and was the reason why MacKenzie left.

RS: Yeah, right. But he also left because they discovered he was running cattle of his own and charging the immunization shots and so on to the trustees. The expenses were all on the board, and the profit was all to Mac.

BE: So was there any other hanky-panky you were aware of?

RS: No, isn't that enough?

BE: Yes, it's quite a bit.

RS: Stealing cattle, sleeping around. What do you want? I must say that it was more than just growing up in the middle west. It was a different world, from my standpoint.

BE: I had never heard that about Collingwood before.

RS: Oh, that was well known. When he came to Cornell, he was listed as the "champion skirt lifter on the Cornell campus."

BE: I guess Larry Kimpton didn't mind because he had his eyes on another woman anyway. So was there ever any tension between MacKenzie and Kimpton that you saw?

RS: Oh, I think there was a lot of tension, but they just worked in separate orbits. MacKenzie didn't read and wasn't involved in any of the academic programs or anything like that. Sometimes we had a farmer, and we had an irrigator. But he didn't eat in the boarding house. The farmer would sometimes eat with MacKenzie at the table right closest to the kitchen. Kimpton would never eat there. So I don't think there was any active antagonism -- they just sort of agreed to live on separate orbits, that's all.

BE: So there was a regular table where the staff would eat?

RS: No, just this part of the staff. Just the non-academic people who would regularly sit in the corner. If you come into the boarding house from the circle and turn left toward the kitchen, it's the table right there.

BE: Interesting.

RS: That was invariable. My three years it was always the same.

BE: That one table was always for the non-academic staff.

RS: Yeah. The level of cleanliness was not quite the same. It wasn't all that great anywhere, you understand, but it wasn't even ordinary at that table.

BE: Well, actually the overall level of cleanliness wasn't all that good at Deep Springs until the Newell era renovations. The new student residence has had an enormous effect on the place. I've been up several times since the main building was renovated and the professionalism in the kitchen is really striking now.

RS: I'll be darned.

BE: It's finally being run the way an institutional kitchen ought to be run.

RS: Good, great. Well, I hope it doesn't get too institutional. I worry about Deep Springs with the rise of the faculty et cetera. But that's another thing. Let's stay back on history.

BE: Sure. Second year.

RS: Second year, I was beginning to hold student offices. I don't think I was ever nominally a trustee although I attended trustees meetings. I was president and I was labor commissioner at different times. But those were third year jobs. But I don't remember much happened. Peterson left the second year. I've forgotten, my faculty didn't make all that much of an impact on me. One thing happened though. Henderson and I took a chemistry course. Henderson wanted to be a chemist, and of course there was no chemistry professor. Oh I know what the problem was. Armand Kelly. I'll have to come back to that in a minute.

I wanted to have any science. Chemistry was what we were going to have then. The institution hired Charles Coryell, who was a post doc at Cal Tech, to be the chemistry teacher, but he was at the ranch only, I think a total of four days, two days at the beginning of the semester and two days at the end. Now we had some correspondence during the course of the fall, but for the rest of the fall it was Henderson and me learning chemistry.

BE: So the ranch hired him to teach you a correspondence course.

RS: Essentially. Later on he became a great man in the Manhattan Project, a young and very brilliant chemist. Unfortunately he died very young, just a few years after the war.

[Coryell] started us out with a text and as you say a correspondence course, but he also did something which in retrospect turned out to be very clever. He had us inventory everything in the chemistry lab. It had been a chemistry lab, and there were things, there were a bunch of bottles around and so on. So he said first thing you do is you take an inventory of this and send me the inventory, and I can better tell you then some set of experiments to do, so we won't have to buy so many chemicals.

So we started doing the inventory, and we learned a lot just by inventorying the chemistry lab. We never thought of that as a way of starting to learn chemistry. But it has its points. I do remember the sort of flavor of it. Henderson would be on a step ladder. He would open a bottle and see what was in it, and there was a can up there. He was telling each time what was there and about how much. He opened the can and he said one smell of ethyl ether. Well, even from that you learn that there is more than one kind of ether. There's an ethyl ether, a methyl ether and propyl ether, et cetera. So you're well on your way to starting chemistry, and we did.

[Coryell] passed us for the course, but he gave us a final exam, which was a complicated mix of things including a carbonate. Unfortunately, we messed things up by getting too much acid in it right away, and we lost all the carbonate going out as carbon dioxide. So we didn't do very well on his final exam, but he passed us anyway because we had done things along the way that I guess he approved of. We learned our chemistry. I later had a physical chemistry minor as a graduate student, and a lot of my research work leaned heavily on chemistry. So the grounding wasn't all that bad just with that kind of a makeshift course. Armand Kelly--

BE: One question about that. Those are very crude conditions. Your chemistry lab is in disarray. You don't have a teacher present. Yet you remember that as being a pretty good course. What do you think was it about that experience that made it a good course?

RS: We were responsible for it. We had access to a library, and there was no place to hide. There was no way you could bluff anything. There it was. We were highly motivated. We wanted to learn.

BE: It was just the two of you.

RS: Just the two of us. Most people were not scientifically oriented. Mersman, as I say, was a terrible physics teacher, but he was a really good mathematics teacher, and that was the first mathematics I had, second year. I learned calculus and absolutely loved it. We'll come to that a little bit later.

Armand Kelly was sort of the associate director, the kind of administrative director. Kimpton wasn't interested in pushing paper. He was interested in his own ambition. As you know, he later became president of the University of Chicago by playing that game around there. He was just a clerk with the Manhattan Project: in fact he was so low that when he wanted to get his draft deferment, he wrote me for a recommendation. So I sent him a recommendation as one of the people recommending him as vital for the country's effort. He really hit the bottom of the barrel.

BE: Kimpton has never been described to me before as being an operator, but it makes sense.

RS: Oh God almighty. You'd better go back and look at your sources. You're painfully, woefully--

BE: I just don't have that much information about the guy.

RS: He was an operator par excellence. That's the leading term for Kimpton, an operator.

BE: Was he a person who was conscious of his appearance? How did he look?

RS: Yes, he was very suave. His appearance, his bearing, his vocabulary, everything. Everything was -- I shouldn't say contrived, but it was planned. He didn't do anything casually. Everything was part of "how is this going to help me?" or "is this going to hinder me getting where I want to be?"

BE: So he was political to the core.

RS: That's right, and an operator.

BE: Do you think anybody at the college ever got to know him on a friendship level?

RS: I don't know. Curiously enough, his closest friend among the students was Collingwood.

BE: Who was also an operator.

RS: That's right. Probably that's the way it started. Incidentally, I just remembered what Mrs. Kimpton's name was. I remembered it was almost like a regular name. It was Genevra, not Geneva, but Genevra. She was a Frick. A Kansas City socialite, et cetera, very wealthy, and in fact I think Larry just married her to get money to go through graduate school. She was very unhandsome, ands she had a terrible voice.

BE: So he was not at all jealous of Collingwood.

RS: No. I think it was a convenience for him. He was very, how shall I say, Gallic in that respect.

BE: So Collingwood and Kimpton were friends. That's funny. It kind of completes the circle doesn't it?

RS: Yeah. Collingwood, I was going to tell about being in Telluride House with Collingwood, but let's not get into that. We still are talking about Deep Springs.

My third year was really great. These people that now have only two years at Deep Springs, they don't know what they're missing. They know what they're gaining. They don't know what they're losing.

BE: I have the same feeling. My third year at Deep Springs was the best.

RS: Well, see I was president one semester, I guess the fall semester. How did this work? I don't know, maybe the spring semester, Anyway, I did the revamping of the heating plant, and with Herb Gustafson, who was a second year man, I built a culvert which is still there, down by the dairy barn. It's going across the main irrigation ditch. There's a concrete culvert. We build it. We didn't buy any rebar. I know that. I tried to get rebar from Kelly and Kimpton. They said nothing doing, we don't have any money for that sort of thing. But we had iron to do the proper reinforcing. We just had to go to the junkyard and take anything we could get. So there is angle iron and everything else in that culvert.

BE: This is the culvert where the road crosses the--

RS: If you come from the lower ranch, this culvert is just before you turn left in front of the dairy barn. The date should say 1938.

I learned a lot. I have since done a lot of concrete work, but I've always bought rebar. There were books on it in the library, and we just looked at the books.

The main thing that happened was that the guy that had been hired to come and teach mathematics reneged at the last minute. He got a better job somewhere. I don't know if there were any other science teachers or not, I don't think so. So Kimpton came to me and said, "Well, you teach calculus." So I taught calculus that year and, as you say, teaching it to Deep Springers. David Brown Spalding, the second Spalding was a member of my class, and he was always cutting up, always. There was no way I could spank him, but nevertheless I was a good teacher. They learned good calculus, and some of them even got the spirit of calculus down, which if you do that, you really have learned something.

This did me a great service because when I got to Cornell, I started taking the notes for John Curtis's advanced statistics course. He was writing a book, and so I wrote up the notes of the course, and in recompense he recommended me as a tutor of calculus. So it was very lucrative because a lot of people found they just couldn't get through calculus, and I knew how to get them through. So I did that.

I'll tell you an anecdote. The war was coming on by then. This was the fall of 1938, and within a year or so Cornell had to have engineers in order to have a football team. Only engineers had draft deferments.

BE: Why do you have to have engineers to have a football team?

RS: Because any other major was drafted. So the only people that were at the college were engineers. So you had to have engineers, and to be an engineer you had to pass calculus. Somehow or another the word got around that to pass calculus, you had to be tutored by Sproull. So the alumni association hired me to teach the football team calculus.

BE: [laughing]

RS: This is not funny at all. Everybody was playing the proper role. I needed the money. They needed the team. The team needed calculus to pass. I taught a class actually up in Schoellkopf and then did individual tutoring. To finish up the anecdote, the second year I did this, I was married. We needed money even worse, although my wife started working for Agricultural Economics. But anyway, after a couple of weeks in the fall, I hadn't been paid. The alumni association had been pretty prompt in paying me, but they hadn't paid and hadn't paid and hadn't paid. I called them about it, but they had not even returned my calls. So we went to a football game, bought a program, and I annotated the program with all of the names of the people who wouldn't be there if they hadn't taken my calculus class. I sent that to the alumni association, and I got paid the next week. That's how to get money out of a not for profit organization.

Anyway, back to my third year. I never had so much fun in my life. Everything was going fine. But then, of course, there were a lot of worries as to what would happen afterwards.

BE: What was Kimpton like as dean? Was he a good dean?

RS: He was a sensible dean. He knew the academic world and how it worked, et cetera. I think he was good for the school in the sense that he took it from sort of like a YMCA camp, which was Crawford's level of intellectual activity, up to a Cornell Ph.D.

BE: And Dean Thornhill of course was a headmaster of a preparatory school. So would you say that Kimpton was the first to elevate the academic program to the college level?

RS: Yeah, at the time there was intellectual respectability at Deep Springs. Up until that time people learned a lot and because of the selectivity of the people going in, they did well later. But it was not a defensible academic enterprise then. It just accidentally, I think, hit with Peterson and Kimpton that one year. They were the first two I think.

BE: Did Simon Whitney succeed Kimpton?

RS: Yes, and there may have been a hiatus there, but I don't know. Si, of course, was a firmly intellectual economist of no mean proportions.

BE: Right. So he kept things at a fairly high level.

RS: When Whitney was at Deep Springs, I was very busy elsewhere and had nothing to do with Deep Springs. I was doing my best as president of Telluride Association for a couple of years, and then the war was on. We had negotiations with the Navy and with Cornell about Telluride House, worrying about our investments. It was a busy period.

BE: You know I want to have a second conversation with you about Telluride Association -- probably not today, but I do want to get--

RS: It won't be as long because Telluride, how shall I say, did not have the uniqueness of the Deep Springs experience.

BE: So why else was your third year great?

RS: Well, I was labor commissioner and enjoyed that immensely. I worked with MacKenzie. In fact I would eat breakfast at his table almost every morning to work out what was happening with the general work crew as the other people, of course, were going on about their regular jobs. Then there were occasions when we had gotten outside of the valley, driving, picking up trustees at Reno or Las Vegas or Los Angeles. We did very little travel to Las Vegas in those days. The route over there was developed later actually.

BE: Las Vegas was pretty far out.

RS: Yeah.

BE: Pretty small town.

RS: Mostly Reno was the entrance path, and--

BE: Not Los Angeles.

RS: Well, from time to time, yes. Trucking hay, sometimes we were selling it, sometimes we were buying it. Trucking cattle, we were usually selling, bringing in bulls from time to time. We bought a couple of Charolais bulls because some extension bulletin said that they put on a heavier weight of calf. I don't know if that's true or not, but I do know that when the pregnancy testing was done one fall, it turned out that one of the Charolais bulls was not doing his job, and he had to have the most ignominious thing that ever happened. He was sold for hamburger.

Being labor commissioner in many ways was more key to my education at Deep Springs than anything I took in class. Working with people, getting them to do things that they didn't want to do, getting them to do things to a level of quality that they hadn't even thought about.

BE: Do you have any anecdotes about managing people who were spectacularly inept? That is a real subcategory of Deep Springs stories.

RS: I don't really have those. We had, in my freshman year, Richard Kiegley. Do you remember Joel Blftspk in the Lil' Abner cartoons? Everything bad happened to him.

BE: The most unlucky man in the world.

RS: Yeah, Kiegley was that. Everything happened to him. He was almost killed. The winter of 1938-39 was a very bitter winter. There had been very little snow cover. It got to be extremely cold, and the pipes froze from one building to another, and also going down to the little ranch. So we had no internal water for many, many days. We had to dig up the water pipes and thaw them out. Kiegley was standing in the ditch and a guy, I think it was Randall, behind him stuck a pick almost at the base of his spine. If it had moved over about an inch, it would've killed him. That was the worst thing that happened in the course of my tenure at Deep Springs.

The thing that all the members of my class remember me by, and Bob Henderson would never let me forget, had to do with a power line. Well, it was not really a power line, but the distribution line behind the main building. There is sort of a road of sorts from the machine shop back of the museum and the main building. Then just short of the substation and then down to the circle. Well, I was driving the stake truck across there, and I'd forgotten that the stakes were up. There's a thing that should not be there -- namely, a guy wire for that pole line goes across like that behind the main building. The stakes hit the side of this thing and brought down the line coming from the substation over to the machine shop, so there was no power to the machine shop. I guess we went on to the lower ranch from there as I recall. Well, who had to fix that line?

BE: The electrical engineer.

RS: Turns out there were some climbers, these things that you put on your shoes that stick into the pole. Nobody had ever used them and some other tools, and so we put in a new pole, and I learned how to do that.

BE: You knew how to turn off the substation.

RS: Oh sure. I had to find out how to do that.

BE: Right. Wow. You'd never be allowed to do that today.

RS: I suppose not. It's a different world.

BE: It is a miracle really. Everyone that I've ever talked to that has been associated with Deep Springs agrees with this. It is a miracle that the college went as long as it did without a student fatality.

RS: Well, and this student fatality. I really searched my soul.

People ask, "What is it that Deep Springs teaches you? What are they trying to do?" I explain that I think the major thing that Deep Springs tries to do is to start you thinking seriously about what your life is going to be, about what you're going to do, and what kind of impact you're going to make. Before you just casually fall into a profession or just casually fall into anything else. At least for me and the students in my class that I talked with and others, this is what seems to me the big contribution of Deep Springs. We learned a lot of things in class and a lot of things out of class. But in addition to all that, Deep Springs pushes something else that is not true perhaps of a work-study program in a university.

The thing that really rattled me [about the student fatality] was that it was an example of just the opposite of taking responsibility. Deep Springs teaches you what it means to take responsibility for your actions. If you left the corn planter in a dangerous position for the next guy who comes along and you didn't tell him about it, that's real, and you should really feel it very deeply.

Well, this guy [Pihos] did absolutely the most irresponsible act I think I've ever heard of at Deep Springs. He not only dealt with something that he had no briefings on, but as I understand it, he didn't even have permission to take the tractor. That was a second thought. He was thinking of taking a truck and it just occurred to him that the tractor would be better, and so he didn't go back and talk to the ranch manager or whoever it was that he was talking to.

BE: He paid for it, too. That's what I would add to your comment about what Deep Springs teaches you. I was talking to a student ten years younger than I am, and he said that at Deep Springs there are no pillows. At a normal college, you can do something wrong and weasel out of it. You can skip class and nothing bad will happen to you. Deep Springs teaches you that actions have consequences and that if you don't do something right, it's going to come back and bite you.

RS: As I've said in talks from time to time, at Deep Springs there's no place to hide.

BE: I think when maybe when Nunn was talking about the voice of the desert, he was talking about that reflection on the purpose of one's life.

RS: That's right. Yep. You go back to the gray book. Without quoting it, that's what we tended to kind of pick up.

BE: I think that has been absolutely consistent for eighty years. Those functions have been fulfilled regardless of how well the college was doing financially, the quality of the faculty, and even the quality of the student body. Those lessons, I think, have been consistent, which is extraordinary. It has always seemed to me like it was a physical fact of the college and the system of organization that Nunn put in place that imparted those lessons. And that all of the magnificent efforts that people made over the years to mess it up have been completely ineffectual.

RS: It's just a tragedy that [L.L. Nunn] went to his cronies when he chose the trustees. He did that, I think, with his eyes open. He realized what he was doing, but he thought that he didn't want to just make another college. So he wanted to not have just another board of trustees. He wanted to have all the people who he thought had his magic. He was wrong. They didn't. They were all mediocre. Possibly Waldo was an exception to that. But FannieNoon, O.B. Suhr, they were all lightweights. They kept their head above water because of L.L. He was such a strong person.

P.N. was a methodical person who worked hard and knew what he had to learn and learned it and so on. He didn't have the vision that L.L. had by a country mile, but he acted to hold things together. But then he died, unfortunately, even though we were all terribly in awe of him. So in one respect it was kind of helpful to find that we didn't have to be afraid of him anymore.

BE: Did he die when you were a student?

RS: No, it was after that. This anecdote I said was in January of 1938, and I left there of course in June of '38. P.N. lasted for several years after that. He was in reasonably good health despite all the pills when I visited him.

One of the things that I'm not so sure now about Deep Springs is what magic they make of trips. The trips were a great part of the socialization of Deep Springs when I was there. Both the student body main trip in the spring and some of the weekends in the fall and then trips that somehow or another that we managed in the summertime or over winter holidays or something. Bill Spaulding and I went up to Roberts' Ranch at Christmas and just camped out for almost a week, I think, skiing up in the hills above Roberts without ski tows et cetera. You had to go up methodically. Another Christmas or maybe in the same one, Ed Cronk and I borrowed Larry Kimpton's car and drove down into Southern California down into Baja for a little ways.

BE: Do you keep up with Ed? How's he doing?

RS: Well, that's an interesting question. The answer has been no, but is now a yes. A couple of years ago I invited him to dinner at the Cosmos Club in Washington. We had a very pleasant dinner. In fact we closed down the Club that night. Went on to about eleven o'clock I think. Then he retired completely from the State Department, and they built a house in a retirement village near Frederick, Maryland, a long ways from Washington. It's an hour's drive from Washington.

We stopped on our way from Florida and visited with him last spring. Ed has become a devout Christian, I don't know if you know that, some years back. He's got religion really. He never showed any of that at Deep Springs. I don't know what happened. I think Dorothy got to him. . .

RS: Skivvies.

BE: Underwear.

RS: Yeah, long woolen underwear, toe to neck underwear. You asked if we ever were cold. Well, we tended to wear that kind of stuff in the wintertime. Buildings were never heated to anything like what they are now, I suspect.

BE: I want you to go through your years as a student and talk about the end of your time at Deep Springs, and I also want to talk to you about more details about the trustees. So your, in your third year, was there at the end of that year was there anything like a ceremony? Was there any kind of a graduation ceremony?

RS: No. No. No.

BE: Nothing. Was there the custom of ringing the bell when someone left?

RS: No. No.

BE: You can't recall any kind of ceremonial ending to the--

RS: Not only can I not recall a ceremony, I'm pretty sure there was not. It's not just from failure of memory. It was not within the spirit of things to have anything. We didn't have ceremonial business. We had public speaking, which was the real assembly thing. There was an occasional Sunday evening gathering, and as I say it changed from Crawford which was more religious and hymns, to Kimpton who to say that he was an active agnostic is probably understating it. Kimpton had no interest whatsoever in organized religion.

BE: So there was sort of an anti-ceremonial bent to things.

RS: That's right. Yeah.

BE: Because there was also no graduation ceremony in the 1970s when I was a student, but now there's a fairly elaborate graduation ceremony where parents come and students put on a show and--

RS: Well, I am of two minds about that. Mind one says sure, why not. It's a good thing. Everybody enjoys it and it's something to remember, et cetera. But mind two says that it's one more way in which Deep Springs is becoming just like every place else. I just worry, worry, worry about that. With the rise of the faculty, having a dean of faculty, you have absolutely clobbered the ability to work several years ahead on getting sabbatic professors -- they're hard to ever get anymore. The reason is they've got this cocoon woven around the director or the president. You can't hire somebody for two, three years from now because he has the student committee and the faculty committee, and they just immobilize him. And the faculty start talking about tenure and retirement and so on.

BE: I didn't know there was talk of tenure.

RS: They would like to have it.

BE: That's a ridiculous idea.

RS: Yeah, but the forces of entropy assure you that things tend to become alike. So you have to keep resisting that and resisting and resisting.

BE: That's a good way of putting it.

RS: That's the only thing that worries me about the ceremonial thing.

BE: There's also, I think, more of an emphasis on the students' family and friends, seeing Deep Springs and participating in it than there once was. It was really in my experience and probably in yours too, it was a private experience that the student alone had Perhaps he had a visitor, once in a very great while.

RS: We didn't. We discouraged visitors, and I can't even remember. It probably happened from time to time, but I can't remember the family of any student ever being at Deep Springs.

BE: It was rare in my time, but now it's quite common. That's another thing that I think is drawing it to be more similar. I've noticed just in the last couple of years really since Newell has been president and since the rebuilding has taken place, the student body seems a lot more competent, a lot more serious, and a lot more well-behaved than they were, say, when Brant Kehoe was president or Buzz Anderson was president. I see that as a positive thing, and certainly the rebuilding of the college is a positive thing. My own view is that, as I said, it's the physical facts and the basic governace of the place. As long as those things don't change, the essential lessons that really make it go and make people remember it as being important will probably endure. And as long as there's no high speed Internet access--

RS: Yeah.

BE: That I think would wreck things pretty well.

RS: I hadn't thought about that, but you're certainly right.

BE: I mean the Internet access at Deep Springs is terrible, and that's great. That's about as good as it should be.

RS: Well, having it possible to reach Deep Springs by telephone is helpful. In my day it was virtually impossible. The telephone line was usually out, and when it was not out, there were three or four other ranchers on it.

BE: Did you have a crank phone with Deep Springs #2 as the phone number?

RS: Yeah.

BE: Yeah, I had that too.

RS: Now with the radio thing, it seems to be fairly reliable.

BE: Earlier you characterized the trustees as lightweights. One of the things that's really valuable when talking to people who have firsthand experience with someone who is long gone is to get physical descriptions. That's sort of why I asked you about John Johnson. What he was like.

RS: Elmer Johnson. Johnny, but Elmer was his name.

BE: When you see a picture of Frank Noon, he's usually wearing a long black wool topcoat and a black hat.

RS: Yeah. He looks like an undertaker. It would have been a fitting profession for him. If he had just been an undertaker and stayed out of my life, I would have been very happy. He was perhaps the most lightweight of the trustees.

BE: What was his background? Was he from Utah born and bred?

RS: Yes, and somehow or another he started working for L.L. His career, such as it was, was with a home loan bank. At least that's what it was by the 1930s. These things didn't get started though, didn't they, except in the Roosevelt administration.

Honest to God, I remember an anecdote that may actually illuminate two people. I don't know what the issue was. But Noon and Waldo were in a trustees meeting, and Noon gets up with his gaunt undertaker's bearing and says, "I can't go along with that. That's sounds like a deal." Harold Waldo was more round. Have you seen pictures of him? You probably didn't know him.

BE: No, actually he died during my second year. I never got to know him.

RS: He almost always had a smile on his face, and he was going on with a big smile and he said, "I LOVE deals!" As a lawyer, he was probably a dealmaker more than anything else.

BE: Was Noon physically large?

RS: He was tall. He was thin.

BE: Tall and thin.

RS: Aesthetic.

BE: Deep voice or high voice?

RS: Not especially either. Certainly not a deep voice, but not especially high either. He had kind of squeaky voice, come to think of it.

BE: Was he an especially loud speaker? When the talked, did he dominate the room?

RS: No. No. He never had anything to say.

BE: He was kind of a small town banker.

RS: That's right. A very small town banker. As you can see, I hated Fannie Noon.

BE: Well, he did something or at least the rumor is he did something very bad from the standpoint of Deep Springs history. The rumor is that he burned a lot of L.L. Nunn's correspondence after L.L. Nunn died.

RS: Really? I never heard that rumor.

BE: Well, most of the paper is now in Kroch library, and there's not very much correspondence in L.L. Nunn's hand, but we do know from letters written to him and various other accounts that he was a voluminous correspondent. I can't trace the origin of the story, but the story is that for one reason or another after L.L. Nunn died, Frank Noon gathered up a lot of his letters and burned them.

RS: It could be because it might have been some incriminating evidence against Fannie Noon in there. He certainly had no conscience about milking the trust for his own benefit.

BE: Did he participate in investing in the Provo Ford dealership?

RS: Yeah.

BE: I had always assumed that there was stuff in Nunn's letters that Noon didn't want people to know about Nunn.

RS: Could be. But if I started imagining what it is, I'll imagine it in the way that does Noon the least credit, so I'm probably not a very good imaginer for you.

BE: That's okay. Was he a sneaky guy?

RS: Yes. Very sneaky. He would take students aside and try to pump them for dirt that they could pick up and so on. Just a hack. Just an ugly character.

BE: That's the way he comes off with almost everyone who recalls him. Did you know Thornhill very much?

RS: No. Once or maybe twice I had to drive to Los Angeles to pick up Dean and Mrs. Thornhill and take them up to the ranch and back. I guess it was just once going up and once going back. I got a lot of conversation during that period, but I don't really recall what it was all about.

BE: Twenty years ago I had a very interesting conversation with his daughter, Virginia who strangely enough was living in Elmira at the time. It was a fascinating conversation because she was a fifteen-year-old girl living at Deep Springs College in the

1920s. So she had a perspective on the place that was radically different than all the other people I've talked to.

RS: Well, I don't know how to work out the numbers now. She must've been there in the '30s, early '30s too.

BE: Probably.

RS: Probably '30 to '33 or something like that, and Crawford was say '33 to '35.

BE: Until she went off to school.

RS: Because I knew her I think, but I thought of her as very much younger than that. We always called her Ducky because we had inherited a story from earlier classes, or maybe from one of the members of the board of trustees. Although that's not the kind of discussion we usually had with them. But somehow or another we had learned the story that Dean and Mrs. Thornhill had been trying for years to have a child. Somehow he read or got the idea that the way to have children was to eat duck eggs. So they had a very strong importation of duck eggs at Deep Springs over a period of months, and after a few months then Ducky was born. Anyway, that's the story we inherited. Whether it was true or not, I don't know. Unfortunately, the people that would know about it are mostly dead, I think.

BE: Well, that's okay. You just, you take these things in the spirit in which they're given to you. So when you left Deep Springs, where did you go? Did you go back home?

RS: Yes, I went back home to work in the cornfield because that would be the summer of 1938. My truth squad [wife Mary] is out there. She's about to say that it's a lie.

BE: When did you two meet?

RS: My junior year in high school. That reminds me. Sport?

Mary Sproull: Yes.

RS: Are you listening?

MS: Not really.

RS: Well, it occurred to me that I don't know whether you are willing or not, and I don't know whether I'm willing or not and I don't know whether Brad is interested in or not. But looking around down in the family room a while back I discovered my letters to you--

BE: When you were a student.

RS: When I was a student. Those are raw data.

BE: Well, I'll tell you what. Deep Springs has a pretty good archive now. The Cornell archives is very interested in the materials that illuminate daily life at Deep Springs College. They're very high on the history of Deep Springs. You don't have to make that material available now if you don't want to. But it would be great if you made sure that it ended up there.

MS: I think we'd better read it.

RS: I think we'd better read it too.

BE: Yeah, I had a steady girlfriend when I was a Deep Springs student who I later married, but I don't know if I would want those letters read during my lifetime either. But you know Herb Reich had a similar thing. He wrote home, I'm told that he wrote home to his parents every day--

RS: Every day [astonishment]!

BE: Every day.

RS: Was he homesick or--

BE: You knew Herb, right?

RS: Yeah, sure.

BE: Herb was just a very quiet methodical detail oriented person, and so these letters were saved, and so there is this record of what I did at Deep Springs from 1917 to 1920, and we've got to get those letters.

RS: Well, don't you have them?

BE: Not yet. No.

RS: Who has them?

BE: I imagine his son does. But anyway, that kind of thing is really, really valuable. I know that Jack Newell is interested in writing a history of Deep Springs College. That's the kind of stuff that a historian really could make great use of.

RS: Well, we'll look at them. Look, remember that when I started, I was only seventeen years old. I was in love with this woman--they called them girls in those days--who was only sixteen years old. So I don't know what's in those letters.

BE: I think the things that would embarrass you are probably not the things that a writer would be most interested in.

RS: Probably true.

BE: But anyway, that would be great. Make sure those are kept track of, and that they don't get lost.

RS: This is interesting. Because we're just at the burning down stage, so we're going to have to think through things like that.

BE: If I could just give you one piece of advice, it is, don't give the originals to Deep Springs.

RS: Okay.

BE: Because Deep Springs now has a very good library and an archive room.

RS: But nobody taking care of it.

BE: Right, and Deep Springs doesn't have continuity.

RS: Now who's doing this for the Kroch Library?

BE: Well, I've, one of the main reasons I got involved with the Telluride Association is that I was so shocked at the condition of the Deep Springs archives, and I've been working for years to kind of get things into better shape. The university archivist is a woman named Elaine Engst, and she and I have been working together for several years to get things together. I had the student body minutes transferred to Kroch Library several years ago. That was a huge production, and the students agreed to release them and to send the originals to Cornell on the condition that they be photocopied; so they were photocopies of all the minutes in the Deep Springs archives so the students had that resource. They can look back to see what decision was made in 1922 if they ever want to. But the originals are in the vault, and I think if you had material such as these letters, that would be a good way to go is provide Deep Springs with a copy.

So tell me about some of the others like O.B. Suhr. What do you remember about him?

RS: Very little. He wasn't around very much. He came a couple of times, no more than once or twice a year. A very quiet engineer, uneasy about anything academic. He didn't talk about that. Talked about mountains and power lines. I tried to pick his brains about the early days of Olmstead and stuff like that, but he was shy and not very interested in talking.

BE: Was Waldo the one who was easiest to talk to and most available to students?

RS: Right and also I thought he was the most friendly to the idea of Deep Springs as an institution, which although it was unique and although it was separate from the academic establishment, never the less it had to feed them, so it had to deal with it. It had to feed people into the academic establishment because very rarely could it be the final years. The only one I know of in my time is Fred Laise who didn't go on after Deep Springs. He went to Telluride Association but did not do any college or university.

BE: Is that right? Fred was a trustee when I was a student, and he was the head of the American Red Cross and was a big accomplished well-spoken guy. I just assumed he was a Tellurider and--

RS: Well, he was a Tellurider.

BE: And a Cornell grad.

RS: No. I'm pretty sure.

BE: Interesting.

RS: I don't know if he made Who's Who in America, but he might. If so, you could look up there. He started in Alex Brown and Sons, the brokers in Washington. I think he started directly from Deep Springs. Then he became a wheel in Alex Brown and around Washington, and that's what led to the Red Cross, I guess.

BE: That makes sense. Have we covered the trustees. Is there anybody else?

RS: No, except to put leaves on the tree of Waldo a little bit. I found him, I guess, I said he frequently was smiling. A uniformly friendly, able guy. He had to get along with all these other trustees, but he recognized what even though the lowliest student recognizes, namely that Deep Springs was on the track towards disaster with the board the way it was. When there was the initial move to get a decent board of trustees with Parker Monroe who was also an investment man, with TIAA-CREF. But then the second one was Jack Laylin. He was a much more powerful and much abler person than Monroe and much more interested in things. Monroe was not a Deep Springs graduate.

BE: Did either Monroe or Laylin serve on the board when you were a student?

RS: Monroe did, I think. I keep getting mixed up because there was the Telluride incarnation you see when I was on, and I knew Parker Monroe in that. I think I have it right that Parker Monroe came on the board about 1937 or something like that. I don't know how. I don't know why. He became persona non grata to the western trustees because they had preserved their little fiefdom pretty skillfully, and they did not want to have any real people in it. So I don't know quite how Parker Monroe managed it. Then I think that Laylin was part the fact that Monroe was there. I don't know. But they didn't know what they were getting with Laylin, because if Jack Laylin had only been more patient, more willing to spend time on the thing, things would've turned around a lot faster. But he was impatient. And Jim Withrow, I think, did a world of good for Deep Springs and very few people give him credit for it.

BE: I agree with you. He was the chairman of the board of trustees when I was a student, and I knew him pretty well.

RS: Robert Aird also did a world of good for Deep Springs. In fact, in a couple of talks I called him "the second founder of Deep Springs," and I still believe that.

BE: I think that's right.

RS: But unfortunately, he and Withrow didn't get a long. But how Laylin got on the Board, I don't know, but that after I ceased to be a student I think.

BE: I heard somewhere along the line that Laylin was one of L.L. Nunn's favorite students. He was one of the sort of superstars of the early student body. He may have just stayed in contact with one of the trustees or sort of stayed in the orbit.

RS: Did you have access to the Board of Trustees' minutes?

BE: Oh yeah.

RS: The Kroch library has those now.

BE: The paper record of Deep Springs is fairly good, and the student body minutes provide you with a fairly good account of what happened day to day. But so much of Deep Springs is the interplay of personalities, and that's not there in the minutes.

RS: I was very fond of Harold Waldo. I thought he was an absolutely necessary person. He put a little bit of sense and reasonableness and morality into the board. Later on when I was in the Telluride Association, Harold from time to time came to conventions -- not regularly, but maybe once every three years or so. I always thought he was a force for good.There were always terrible rumors about Deep Springs floating around Telluride House, and there were always Telluriders who for one reason or another were down on Deep Springs.

BE: Isn't that something? That is the case now too.

RS: Harold by his very presence showed that a perfectly normal healthy individual could be a trustee at Deep Springs.

There was also a phenomenon. L.L.'s nephew Carroll Whitman--

BE: Did you know him?

RS: Oh did I know him! That's the Telluride chapter, not the Deep Springs chapter. Carroll never showed up at Deep Springs because he liked to live the Dolce Vita life, and Deep Springs was much too much of a cattle ranch for him.

BE: Well, we can talk about him when we talk about the Telluride years. A lot of people have a lot of things to say about him. But for this portion when we're talking about Deep Springs, is there anything that I haven't asked you about that was particularly important or something you really wanted to say?

RS: Oh gee. I don't think so. You asked some very stimulating questions. My relations with Larry Kimpton were very interesting to me. I don't know how interesting they are to anybody else. But because I had arrived at Deep Springs almost the same day that Larry did, I was in a somewhat different position from most students and he couldn't father me the way he could the others. So we got into squabbles over one thing or another from time to time. I've forgotten what the meat of the sandwich was, but just we were on different sides.

I had a great deal of respect for Larry's ability as a scholar and his interest in scholarship and a huge respect for his ability as an operator and way of putting forward his ambition, but I had no respect at all for his telling the truth. He would lie and misquote in a terrible way, and I remember more than once I suspect only twice, but let's say twice, we came to really kind of a parting of the ways. Some student body in some way I was speaking for the student body. His answer was, "Well, Sproull, you're getting stale. Why don't you take my car and go to town?" I knew what he meant by go to town.

I'd heard that before. But that's his way of dealing with otherwise political or intellectual issues.

BE: What did he mean by go to town?

RS: Go to the local whorehouse, I presume.

BE: The Cotton Tail Ranch. For many years, that was where the Greyhound bus dropped students off, which I thought was terrible.

RS: You know Dale Corson's favorite story about that. One winter he was going for a trustee meeting, driving alone back to Las Vegas at night. I don't know the circumstances. There'd been this big snowfall, and he'd been delayed a day or so at Deep Springs. So he had as soon as the roads were open he had to go. He went through the snow and managed to get out. Over Lida Summit he saw a car down in the ditch. There was a young couple, man and woman, they had skidded off the road into the ditch and had didn't know what they were going to do next. He took them along and took them over to the junction of the Cotton Tail Ranch and went in to call the state police and so on, but they wouldn't let her in. He could go. The man could go, but they wouldn't let her in.

BE: What, she had to stay in the car and freeze?

RS: I guess she had to wait in the telephone booth because Dale had to go on to get his plane.

BE: That's Nevada for you, isn't it? Nevada is a whole other country.

RS: Anyway, back to Kimpton. I think the student body probably had more influence, and we did the things we wanted to do more. There were big major issues. One of them was the irrigation system, the business of the header canal and feeder canal was unknown. It was -- it reminds me much later I found the difference between the canal system and southeast Asia where Vietnam was all just sprinkled every way. It went this way, that way the other way. To Bangkok as opposed to Saigon, canals were all a header and feeder system so that the farmer was related to the central government. In Vietnam the farmer had never heard of the central government. In fact there wasn't really any central government. It made all the difference in the world, and that's one of the ways we made such a terrible mistake in Vietnam. But anyway, that was the difference.

BE: So you had squabbles with Kimpton over irrigation? What was the disagreement?

RS: I can't remember. The way we were to do it. One of the big things was cutting down trees. We would have to cut down some big cottonwoods to do this, and we did eventually, and the student body was split on that too. But it needed to be done. It was done.

BE: Where did you go for firewood?

RS: Up to the what's it call the flat on West Guard Pass--

BE: Cedar Flat. So you just cut the trees there or--

RS: Cut deadwood, hauled it down in the truck and sawed it off. I don't think we actually cut any bristlecone pines. It always worries me because the age of the bristle cones wasn't discovered until later.

BE: Was there much of a road up the White Mountains then?

RS: No, there was a road only a short distance. I think the only people that used it were us doing our wooding.

BE: How many times did you go home when you were a Deep Springs student? Did you go home every year?

RS: Well, I went home the first Christmas, I think, to make sure that the woman in the kitchen was still in love with me. One of the students, Hayes, had a car, and he lived in Detroit. It was a Ford Model-A with a rumble seat. So there were four of us, and two of us went through the winter in the rumble seat in the car.

BE: Over the Rockies.

RS: Over, yeah covered up with blankets on top. Came back we had a small accident just after we left Morris within a few miles of that. So we got back to the ranch late. We drove night and day because we'd already been scheduled to do that. So when we had an accident, it meant we were late getting back. The student body of course was upset. We had all sorts of goings on, but I think we got our hands, our knuckles rapped, but not very seriously by the student body--

BE: Did you get a term off every year the way students do now?

RS: No. Well, we had summers. We kept a skeleton group during the summer.

BE: But there were no classes during the summer.

RS: No classes. It was before, of course there were any Telluride summer sessions or anything like that. It was a regular calendar. The calendars have been such, I've never been able to understand it since. But the summer -- I'm glad you bring that up, because the summer was really a good part of the Deep Springs experience. There were only three or four of us there. We had to do all of the jobs. It's the only time I ever did the dairy in the summer.

BE: Three or four people did all the haying.

RS: Well, we hired some people for hay. Good point, but I don't know. The hay didn't loom all that large. We had, we didn't have the haybaler until the last year I was there. We just did it on piles on wagons.

BE: Maybe the fields weren't as large.

RS: The fields weren't as large. I ran the office one summer. I guess that was the only one summer.

BE: Then you went to Illinois for the other summers.

RS: Yeah.

BE: Did you work in the cornfields?

RS: Yep.

BE: You must've been happy to get out of those cornfields.

RS: I sure was. People think you always have a choice. Well, you don't always have a choice. But the summer I remember fondly the driving. Did you have the "Marvel" when you were a student, the stripped down Model-T Ford? We used it around the ranch to bring the cans of milk up from the dairy to the dairy room.

BE: We had a stripped down Willys Jeep.

RS: Okay, that's what came afterwards. Well, anyway we would drive this marvel, so called, very early in the morning. We took the dairy cattle down to the lake so that they could feed off the forage down there during the summertime. The early morning drive down to the lake with the sun coming up, that's really quite an experience.

BE: I remember--that's what kept me at Deep Springs three years more than anything else was the beauty of the place. I just didn't want to leave it.

RS: Then things would happen. If there were any visitors, you got the benefit of them. One time we got a telephone call saying there was a fire in Wyman Canyon, and anybody that was free and able could come up and fight the fire. But it turned out that we had two distinguished birds there at the time. There was the president of Harvard who was an organic chemist as you probably remember and his visitor, a fellow organic chemist from Oxford, Neville Sidgwick.

BE: How did they get out to Deep Springs?

RS: They were visiting there. The people at Cornell had told them about it. Probably, I don't know . We had good friends always in the chemistry department there. So I've forgotten who was there besides me at the ranch, but we drove them up and fought the fire. It wasn't much of a fire. It was just the willows down in the canyon. From our standpoint we would prefer that they burn. They just took water out of the irrigation channel, but anyway, we fought the fire, and we arranged afterwards that the forest service would send them checks. I think it was something like one dollar and twenty-seven cents or something like that. I never heard from Sidgwick but, the Harvard President Conant wrote and said that he was going to frame his check.

BE: Well, I think we'll close for now. We'll get together for a second taping later to talk about your days in Telluride Association.

END OF INTERVIEW

Transcribed by L. Altizer, November 5, 2002

11.20.02: Edited for spelling and clarity, questions condensed, sent to RS for review
2.24.03: R. Sproull corrections entered

Interview #2 with Robert Sproull, conducted 9/21/02 in Ithaca, NY by Brad Edmondson

[Off tape, Edmondson asks Sproull how he left Deep Springs College at the end of his time there. Sproull replies that he and classmates got a ride East with Dean Lawrence Kimpton and his wife.]

ROBERT SPROULL: Usually you could cook in the hotels in those days. We would cook hamburger, and the Kimptons would go out and buy round steak for their Scotty dog. This was sort of the difference between our position in society and theirs. Kimpton was coming to the [Telluride] Convention. They must have dropped us off in Morris, Illinois. Henderson and I went on together without them. I'm sure we stopped either in Chicago or ~~Kansas City~~.

BE: Did you go to convention that year right after Deep Springs?

RS: Yes. Yes. I was elected to [TA] membership and residence in the house at that convention in 1938.

BE: Did you apply to any other colleges from Deep Springs?

RS: I don't think so. I figured that I didn't know whether I would get into Telluride House or Telluride Association, but I figured I could get into Cornell because Deep Springs had a pretty good record of students when they came to Cornell. Cornell was the one place in the world that you could be assured that Deep Springs credentials meant something.

BE: Was that your first visit to Telluride House?

RS: No, in the spring of 1935 before I went to Deep Springs, I came to Ithaca to apply both to Deep Springs and to the Telluride Association as a high school senior.

BE: You applied for both things.

RS: Yeah, right.

BE: And you were accepted to Deep Springs, but not to the Association.

RS: That's right. I didn't really take that seriously. I probably didn't apply for the Association because that required papers that I don't think I'd prepared just for preferment to] the House. I went through the interviewing process there at Telluride House in April, and I didn't think I competed at all favorably for that. I mainly went there to be interviewed for Deep Springs.

BE: What was your first impression of Telluride House back in 1935?

RS: For a seventeen year old from a little town in the middle west, I was awed. All these people were going around using big words. It was the first time I had seriously visited a major university. In those days Cornell had a thing called Cornell Day, where the engineers put on a sort of a show and tell of engineering apparatus. You know how that game is played -- breaking concrete up and things like that. Cornell Day was the interviewing day. So that was part of the impressive feeling I got from Cornell. Having thought I was going to be an engineer, of course, it sort of augmented my feeling.

BE: So you got to see a concrete crushing machine, which was pretty thrilling in those days. Did you feel before you went to Deep Springs that Cornell would be a pretty good choice?

RS: Yeah. Both Burchard and DeBeers had gone on at Telluride House and Cornell, and these were the people that I talked with when I was a freshman and sophomore in high school and first learned about Deep Springs.

BE: What percentage of Deep Springs graduates would you say went on to Telluride House?

RS: In our third year class, I think all of them applied. Unfortunately, there was one semi-disaster -- Randolph Newman, who was a very, very good, kind of shy and formal person. At his age he was very formal, and I don't know why he didn't make it into Telluride Association. Telluride, of course, has its ups and downs, and there is always a certain amount of fluctuation with respect to who gets in and who doesn't. [Newman] was no special friend of mine because he was no special friend of anyone's. But he was an able student and a loyal student to Deep Springs.

He came back into the fold as a Deep Springs donor. I was always impressed with him. I wrote him a couple of times to say so because he was the administrator of a school system in California. He couldn't have made much money, but he gave quite a lot to Deep Springs. He was turned down by the Association.

Another one was Morrison Rutherford. We always called him Moppy. He was embittered by the fact that the association turned him down and took it out on Deep Springs. I tried to raise funds from him for years and never got a nickel. He became a medicine man on Long Island. But anyway, I think there were not very many third-year men. [My Deep Springs class] started out with twelve students, but there was a lot of attrition along the way, especially at the end of the first year. I think there were only four or five third-year men. There was Henderson, Newman, Rutherford, and probably one more that I can't dredge up. I think we all applied to the Association.

BE: Do you think that Harvard, for example, or Stanford would not have accepted your Deep Springs credits?

RS: I really don't know. I didn't seriously consider going any other place. It's an interesting question.

BE: See the same exact thing happened to me. I didn't consider going anywhere else. I figured Cornell was a good bet, and I was in, and I didn't apply anywhere else, and it's a nagging question at the back of my mind. What would have happened if I had applied somewhere else?

RS: I just thought of something else. Your questions are remarkably able to dredge things out of my decaying mind.

BE: That's the idea.

RS: There was one Telluride Association member, Anderson Pace, P-A-C-E, a chemist, who was not a Deep Springer. He in fact was in Telluride House as a graduate student, getting a Ph.D. in chemistry, and he was from Evanston, Illinois. I may have even talked to him before I went to Telluride House in the spring of 1935, but if not, I talked with him afterwards. He was a great spokesman for engineering at Cornell, and in 1938 I was in the process of converting from engineering to physics. He sang the praises of Lloyd Smith, the physics professor, as an advisor. So when I showed up in the fall, they had the usual sort of routine questioning - "do you have any particular person in mind for advisor?" -- and I said, "Yes." Nobody expected that. So I went with Lloyd Smith. I was the only advisee he had. He was very graduate-student oriented, and it was kind of alarming to him that somebody wanted him as an advisor, but he played the game. I was really my own advisor, and I used Telluride House as an information bank.

BE: What was Lloyd Smith like?

RS: He was an interesting character. He had been a University of Nevada undergraduate who had come to Cornell to do his graduate work. Then he got a fellowship and went to Germany in the 1920s, and of course, that's where physics was in that period. That was the early days of quantum mechanics. He went to Munich with Summerfeld, and one of the people who was also a fellow there at the same time, and practically the same age as Lloyd, was Hans Bethe, a very brilliant young man. Bethe -- you don't know about the ~~*Handbook de Physique*~~ HAND BUCH DER PHYSIK, no?

BE: No.

RS: It's not something you carry around like a date book or something like that. It's sort of a compendium of physics. Volume twenty-four ,number two was for many, many years sort of the heart and most of the soul of solid state physics. It was where it began, actually. Bethe wrote it almost entirely. It was called Bethe and Summerfeld, but it was almost entirely Bethe. Summerfeld just blessed it from time to time.

Anyway, that was Lloyd's coming of age in physics. He came back to Cornell on the staff. He was the first person to try to bring Cornell physics into the twentieth century. Up until that time it had been sleepy, traditional, competent enough but not brilliant, not anywhere on the horizon. Later on Bacher was brought in, very close to the end of my story. But Bethe was the first. It was through Lloyd that Bethe came here, because when Hitler moved in, Bethe had to leave. I think Bethe's mother was a Jew. So he got out of Germany and went to England, and there was a good cottage industry in Cambridge then of finding jobs for brilliant young Germans who had been exiled from Germany. Lloyd made a strong pitch to Ezra Day, the president of Cornell, and said that they just had to get Bethe. Of course it was a hard thing to do because Day was a business man and didn't really understand physics. I'm not sure how he stood in the German-U.S. axis, either. But anyway, Lloyd made the case and almost single-handedly brought Bethe here. And the growth and prospering of the department since the war has been almost exclusively because of Bethe.

Bethe's name is what brought other people here. It brought Bacher back when he was done with Los Alamos, very briefly because he went on the Atomic Energy Commission almost immediately, and then it brought Wilson here from Harvard and Corson here from Los Alamos. Bethe was the recruiting arm, even if he didn't lift a finger.

BE: It was just his presence.

RS: That's right.

BE: Before we get back to Telluride House, I have one more question about physics. How did you switch from studying engineering to studying physics? What was it about physics that attracted you? Did you have apprehension about going into an endeavor that was more purely intellectual?

RS: Oh, apprehension is not *le mot juste*. I was scared stiff. It was just one of many frightening things, though. Telluride Association awed me. Cornell awed me. A world war was in the offing, and it awed me. You're right, going from engineering to physics was leaving something that I knew I would feel comfortable with and going into something that was unknown.

What did it was reading. Deep Springs didn't have any physicists. Bill Mersman was a mathematician who taught the physics course, the only one we had. As I said before, Bob Henderson and I really designed and built and did the course. But it was mostly reading and playing around with the terrible apparatus at Deep Springs, which just happened to be knocking around over in the museum building. It was not systematic apparatus at all, just an occasional this and an occasional that.

There was one book, I'm trying to think of it. I think it was by Karl Darrow, who became secretary of the American Physical Society, and ran the thing. His nominal job was at Bell Laboratories, where he wrote articles to try to establish Bell Labs in the public eye as a forefront laboratory. But he also wrote this book in the early 1930s to popularize quantum mechanics, and it got into the Deep Springs library. I had read other physics books, and as I mentioned before, I had done a reading program in symbolic logic. That kind of whetted my appetite for theoretical physics, Making the equipment at Deep Springs work and so on and finding out why it worked -- it all was part of that.

BE: You also took care of the boilers at Deep Springs, and you had the story about putting together the parts for a new oil-fired boiler. -

RS: I put the parts together, especially the controls. That was simple. I didn't think about it.

BE: But then you would go and read Darrow and get absorbed in some of the ideas surrounding the thing that you were working on.

RS: Actually I was fascinated by physics, quantum mechanics, and all these Deep Springs experiences contributed to it. I was excited, but certainly not confident.

BE: Tell me about your first semester at the House. Contrast the social atmosphere at Deep Springs with the social atmosphere at Cornell branch.

RS: It wasn't as much of a contrast as I think it became in later decades. In the 1930s there was still a certain formality at Deep Springs. We always wore white shirts for Sunday dinner, et cetera. I don't think we had ties, but we had white shirts, and we had events on Sunday evenings that were very formal occasions. We entertained guests, and later on I came to blows with Telluride House because of their lack of cordiality to guests, or even down right courtesy. At Deep Springs, if somebody came to the Valley, we put ourselves out to show them a good time as well as the guts of Deep Springs. We were proud of it.

One of the ways the house was distinctly different was that at Telluride SUNDAY dinners we would invariably have two faculty people, usually with their wives. There were not any female faculty in those days, maybe one or two, but I don't remember them coming to Telluride House. One of the students, one of us would carve at the head of the table with the faculty woman on the right. "A lady on the left is not a lady," to use the nineteenth century expression. We would make conversation as best we could. They gave that up right after the war.

BE: Everyone was seated at one long table?

RS: No, three tables.

BE: The faculty wives were the only women in the room.

RS: Yes. I hadn't thought of it like that, but that's true.

BE: During the week, were there faculty guests at dinner?

RS: Yes, but irregularly and not as a social function. Only because somebody was down for a meeting, or something like that.

BE: Was there always an adult present at dinner?

RS: It was not common during the week. But at Sunday dinner we would have a lot of formal guests. Berndt Olson and Olaf Swenson, have you heard of them?

BE: Oh sure. I didn't know they were at Telluride House.

RS: What do you mean?

BE: Berndt Olson and Olaf Swenson were L.L. Nunn's valet and driver.

RS: Yeah.

BE: I didn't know they went on to Telluride.

RS: Of course. They were the heart of the place, particularly Berndt.

BE: What was his job?

RS: He ran the place. He hired the waiters to do dishwashing and so on. He did all the hiring and managing. Olaf was the cook, and a very good cook indeed. But Berndt did the supervising of all the housekeeping. We got clean sheets every week, et cetera. And he supervised anything having to do with maintenance. If a window got broken or something like that, he was the one that arranged for somebody to come and fix it.

BE: I see. Did Berndt work in the office with Johnny Johnson?

RS: No, they had separate fiefdoms. Johnny didn't have anything to do with running the building. He ran the correspondence, the applications, and recruiting, and he went to Deep Springs each winter for a month or two. But Berndt and Johnny were on good terms.

BE: Olaf's fiefdom was the kitchen.

RS: Berndt outranked Olaf, but you didn't feel that. They were friends. They ran the operation of Telluride House, whereas Johnny ran the bookkeeping and operation parts of Telluride Association.

BE: Did you ever know how Berndt and Olaf got to be a package deal?

RS: No.

BE: Back when L.L. Nunn was alive, it was Berndt and Olaf, the manservants to L.L. Nunn, the driver and the valet.

RS: Well, they were very much in evidence still. I remember when Olaf died. Olaf had had a rubber tree in the ballroom. I guess you still call it the ballroom?

BE: The room with a piano in it.

RS: Yes.

BE: We call it the striped room now because it has striped wallpaper.

RS: Anyway, there was a rubber tree there, and it was Olaf's favorite. He watered it all the time and took care of it and so on. But when he died, the question was what are we going to do with that thing? Nobody else is going to take care of it, and it's a shame to let it die. Well, Johnny Johnson suggested to us that we try to give it away to some church downtown. So we found a church that wanted it -- the First African Methodist Episcopal (AME) Church.

BE: Oh sure, on Cleveland Avenue.

RS: Yes. Still there I think. Anyway, we took the tree there. It was probably Henderson and me -- we were frequently together. We got somebody's car. Very few people had cars in those days. We took the rubber tree down, and the reverend was effusive in his praise of us and so on. As we walked out the door, he was standing up there on the steps, and he said, "The Lord will remember you for this." [laughter] That's the only sort of blessing of the Lord's that I have. I'm not sure that's enough to get to heaven.

BE: Well, there was that rubber tree.

RS: One of the features of Sunday dinner was a fruit cup that was always there -- the first thing on the table. It was Olaf's concoction and it had a little bit of wine in it. It was the only alcohol you could get at Telluride House, except when we declared a special occasion. I don't know if the special occasion thing is still in.

BE: I think it's restricted to parties now.

RS: Well, that's what a special occasion was. You had to do it in advance. You had to declare your plans for it at the student body meeting and vote on it. Then you could have alcohol. But anyway, it was not very much alcohol, but it was just a pinch. Anyway it was one of the features. Then there was always carving to be done, usually with a certain amount of awkwardness. I remember Henderson once slid the chicken. There were usually two chickens on the plate. He slid one of them off the plate and almost into the lap of the woman he was carving for. He didn't say it at the time, but at a subsequent dinner soon afterwards, he invented the expression, "I'll thank you for that chicken, madam," which became kind of the thing that you said when you committed an awkwardness with carving.

You were asking about the difference in setting between Deep Springs and Telluride. During the week, Telluride was pretty informal, particularly the lunches. Dinners were reasonably formal, but I've even forgotten what we wore. I suppose we wore suits and collared ties. I don't know. Everybody did in those days.

BE: Did you wear a suit and tie to class?

RS: Now you've got me. I don't think so, but I don't really remember.

BE: When you got up in the morning, you didn't put on a suit and tie every day.

RS: No, I don't think so. Oh yes, here's another one of the things about dinner. During the week, people's table manners were I guess you might say variable. Some of them had manners and some of them didn't. I always remember the aesthetic branch of the Telluride House. Did you ever know Bob Gorrell?

BE: No, just heard the name.

RS: He became a trustee at Deep Springs. He became a professor at the University of Nevada, an English professor. He and Christopher Morley, Jr. were kind of the, how shall I say, the aesthetic axis of Telluride House. Gorrell and Morley just picked on Bonham Campbell, who was an engineer along with Paul Swatek. Engineers were very rare then. And they just did everything they could to make Campbell angry, and occasionally he'd lose his cool. The rest of us stayed out of it. We didn't have any desire to take on those characters.

BE: Did they pick on Swatek too?

RS: No. Swatek, the little wizard, was very hard to pick on. He was much too mild mannered and gracious and would just turn the other cheek and walk away. Campbell was more courageous, and when he was irritated he wanted to do something about it. When Swatch was irritated, he just walked away.

BE: You called him the little wizard.

RS: The little wizard. That was his name at Telluride House.

BE: Because he was so bright?-

RS: Because he got all As or A plusses in engineering. He was very bright. He was the best student the engineering college ever had, I think. That was only a small part of it. When he left Deep Springs after three years, he started in as a freshman in engineering. He almost had to because of all the required courses. It gave him a chance to recoup a little bit.

But anyway back to Gorrell and Morley and Campbell. One night, as usual, we had little sauce dishes for vegetables, and we had steamed corn, canned corn and creamed corn in a little sauce dish about like this. Either Gorrell or Morley was sitting at the same table with Campbell, and they started in on him and calling him a boor, among other things.

BE: An uncouth person.

RS: Right. Finally he just got Campbell's' goat. Campbell finally said, "I'll show you what kind of a boor I am." He picked up his hand like this and spun the little dish, and creamed corn went all the way around the table.

BE: A Telluride food fight!

RS: That's the only time I ever saw that at Telluride House. Of course, Gorrell and Morley at that point realized they'd gone too far.

BE: Now, all three of these people, Gorrell, Morley and Campbell, they were all Deep Springs alumni?

RS: No. No. Gorrell and Morley were not. Campbell was.

BE: Was there a division in the House at that time between people who had gone to Deep Springs, and people who hadn't?

RS: It didn't express itself in cliques, or in voting for office or anything like that. But it was apparent, particularly because friendships established at Deep Springs persisted at Telluride House.

BE: Were those who had not gone to Deep Springs in the minority?

RS: No. I think they were the majority. Remember that there were a lot of graduate students, and most of the graduate students were non-Deep Springers. No, I think we were in the minority, but we didn't feel downtrodden the least bit. That one episode I mentioned with Campbell was because of his friction with the aesthetic crowd. But they didn't turn it on me or on Henderson or on Cronk. They weren't against Deep Springs.

BE: It was Campbell, for some reason.

RS: It was because he was fair game. He got angry and that was fun.

BE: One thing has always interested me about Telluride and Deep Springs. There seems to be this consistent theme of young people, of course before 1965 it was young men, who were extremely bright. They are trying as hard as they can to act grown up, but they will still play games and have cliques and social structures that are like gangs of boys. It's a kind of a duality that I think is always present at Deep Springs.

RS: I guess that's true. Yeah. Incidentally, while we're on sort of a side trip into Deep Springs for a minute, there's one mistake I made in the earlier interview. I didn't mention Armand Kelly. He was a Tellurider who came out there on the staff I think my second year and taught geology, very badly. He didn't know any geology. But he acted as a kind of glue between the ranch manager and Dean Kimpton, who had a distant relationship. And Bunny Kelly, Mrs. Armand Kelly, was a huge asset to Deep Springs. She ran the office, did the dean's correspondence, and so on. She was an excellent secretary and also a very bright, cheerful person. We were all pretty fond of Bunny. We could take it or leave it with Armand. He was more of a joke than anything else. I've forgotten what his field was. I think economics.

BE: How in the world did he get to teach geology?

RS: Geology needed to be taught, and he was there. He had gone to Deep Springs, and I guess he learned a little bit of the geology of the area. It was just a textbook course out of the textbook by Longwell, Knopf and Flint, one of the traditional Harvard-based geology textbooks.

BE: Pre-plate tectonics.

RS: Oh very much so. It's a stupid book. Longwell was a guy, I think he goes back to the nineteenth century, and then the book was revised from time to time. That's where Knopf came into it. It was all taxonomy, really. It wasn't geology. It was learning the names of things. I remember the chapter on mountains. It started out by defining a mountain: "A mountain is a comparatively high place with relatively limited space at the top." [laughter] It was silly.

Anyway, Bunny Kelly was a great asset, and Armand was good in the sense that he made a routine human connection between the goings on at the ranch and the goings on at the dean's office. It was kind of nice to have somebody. We all kind of made fun of him, which I don't look back on with any great relish.

BE: Of Kelly.

RS: Yeah. Strictly speaking, it was just on the basis that he just wasn't as quick as we were. He had been around and we hadn't, but you'd never know it.

BE: That's another consistent theme. It goes all the way back to 1917. Students who weren't as quick intellectually quickly fell to the bottom of the pile and got trodden on. It was particularly acute when Nunn was alive because he would pick students and place them in the Deep Springs student body with no application process. I think there was a pretty big gap in the early decades between "Nunn's boys" and those who had been picked for their abilities.

Walter Welti was an elevator boy for a building in Salt Lake City, and L.L. Nunn rode the elevator where he worked. One day L.L. Nunn gave him a silver dollar, and Welti said, "Oh thank you very much." L.L. Nunn kept riding the elevator, and one day he said to Walter, "Would you like to come to Deep Springs?" Walter said, "Sure why not?" because he had limited opportunities, and this was the best offer he had heard. He went on to be a music teacher in Utah public schools. But it was clear that throughout his time at Deep Springs, he and some others who had basically been plucked out of their circumstances by Nunn formed a lower clique, whereas people like Bob Aird --

RS: That probably helps give credence to all the rumors of homosexuality.

BE: Yeah. That is a complex issue. After years of thinking about it, I think the explanation that makes the most sense to me is that Deep Springs students were like Nunn's surrogate children, and he treated them more like children. There was probably some physicality involved too, but he had no family, and Deep Springs College was what passed for his family.

Back to Telluride. We're getting off track. You entered Telluride House as an Association member. That would be unusual today.

RS: Well, it was also very rare in my day. I was the only one that year, and I was the only one I remember in the eight or ten years I was active in the Association. Partly it was because I think in 1938, people were already worrying about Europe and trying to sort of plot out what they would do if there was a war. There was a certain hurry-up character to things. I don't know whether that affected the decision or not. I was also elected secretary in a year. I'm sure that was the first time a secretary had been elected so early. In those days, a secretary was automatically a custodian. So I became a member of Telluride Association and member of the house all at the same week.

BE: Did you transfer to Cornell as a junior?

RS: More or less. I graduated in a year and a half afterwards.

BE: It's odd. You showed up and all of a sudden you were in a position of some responsibility. You didn't really have an apprenticeship as far as Telluride Association was concerned.

RS: No, unless Deep Springs is considered that. It was heavy going, but I liked it. As I mentioned earlier, I was doing tutoring too, which in a way didn't qualify as work. It was not as hard as learning new material in physics or mathematics, which I was also doing. But to go back and teach somebody else the calculus that you know like the back of your hand was relaxing, if anything.

BE: Did anyone ever disapprove of you having a job when you were living at Telluride House?

RS: Not at all. I didn't do any of the tutoring at Telluride House. I always did it at the person's dorm or whatever place. Then the Schoellkopf stuff was done up in the fieldhouse.

BE: Did the proceeds from that help towards your tuition?

RS: Sure did. Yeah. Because I got cash from the Telluride Association only in one year. My books and all that stuff came out of tutoring.

BE: Did anyone else get tuition assistance?

RS: Oh sure. Oh it was common actually.

BE: Were there people who had a totally free ride at Cornell, courtesy of TA?

RS: Oh I don't know about a free ride. Even if you get tuition and board and room, you still have a lot of expenses.

After I graduated from Cornell in 1940, I stayed on here because I hadn't begun to use the facilities of the department and the people after only a year and a half of courses. Although I did get credit for taking elementary physics at Deep Springs. Oh boy, for a while I had holes in my understanding of elementary physics, and I had to go back and rectify.

I would think the most common situation was that a person wuld get board and room and maybe half of their tuition or maybe full tuition. That's it. I don't think anybody got any money beyond tuition. Tuition was about $400 a year then.

BE: Was it commonly known among the students what kind of financial grant had gone to which person, and did it make any difference?

RS: We knew, but I don't remember anybody making any fuss about it.

BE: So it was sort of like Deep Springs, where one person would come from a modest background and another would come from a well to do background.

RS: There wasn't any stratification based on family income.

RS: Paul Todd had a car. His family was fairly wealthy -- they ran Kalamazoo. His father had been mayor, et cetera, and Todd had a car, but he was such a gracious and healthy young kid really. I'm afraid we rather patronized him for a while. But there wasn't any economic stratification. As I said, there was an attempt at an intellectual stratification.

BE: I think that's always been the case -- that the stratification has always been intellectual, either based on ideologies or on ability. When I lived at Telluride House, it was ideology. It was in the early 1980s, and there was a very stark and nasty division between people who were fans of the Reagan administration and people who were Marxists. It was tense.

RS: Speaking of Marxists, we had an active communist -- Jim Moore. The thing that was going on in Telluride House in 1938 and 1939 was the impending war. People were choosing up sides pretty sharply.

There was a Communist cell, and Jim Moore was a member of it, and he was trying to recruit at Telluride House. He had all the Communist literature and the Communist Party line. And of course, there had been the Molotov-Ribbentrop Pact [August 1939, pledging non-aggression between Germany and the Soviet Union]. So Jim Moore's position in 1939, 1940, and 1941 was that all the rest of us were idiots. We were helping America in this terrible war, et cetera, et cetera, et cetera.

BE: Because Germany and Russia had made peace.

RS: Yeah. . . He was saying that it was a terrible capitalist war, et cetera et cetera, and we shouldn't be helping the British fight the Germans and the Russians. Then [in June 1941] he left Cornell to go out to Cleveland to organize labor there against helping the British. He wrote a letter back saying how we were fat cats. He said that as part of doing our service to mankind, and so on and so forth, we ought to throw every crock that we could into the gears of the war effort. He was doing his part by shutting down Cleveland industry, et cetera, et cetera. I posted it on the bulletin board, because between the time the letter was mailed and the time we received it, Russia switched sides and joined our side. We knew that Jim Moore had to have changed sides, but we never heard from him again.

BE: Really.

RS: We never got any kind of communication from him again. But I shouldn't give that story too much prominence, because that was really just kind of a wart on the general picture. The general picture was [Telluriders arguing about] intervention versus non-intervention.

BE: So people chose sides after Germany invaded Poland.

RS: That's right. There was some choosing up before that, but during the winter of the Phony War of 1939 and 1940, the choosing up sides became stark and out in front. The question was whether Roosevelt was keeping us out of the war or taking us into war. The question of the draft was on everyone's mind, and what to do about it. There were a couple of people, both dead now, that I admire very greatly from that period. One was Charlie Ennis. He became a lawyer up in Lyons, New York. He was an extremely able guy. He enlisted in the Canadian Army because he couldn't see standing by while Hitler was conquering Europe.

BE: Why Canadian?

RS: It was the only Army that was active. The U.S. Army wasn't in the war, but the Canadian Army was in the war. All he had to do was go across the border and enlist. They wanted him. They needed him. And he fought in Europe all during the war, and he survived it.

I always admired Charlie after the war because he went back to his little town of Lyons and became sort of a country lawyer. Unfortunately he couldn't make a nickel out there, really. The old joke is that the county is too small to accommodate and feed one lawyer, although it could easily accommodate two. But Lyons didn't have a second lawyer.

The other one was Bob Huffcutt. He became the private secretary to a guy who was a prominent Roosevelt supporter, a politician whose name unfortunately I have forgotten. SAYRE But this man became ambassador to the Philippines. Ambassador to the Philippines is a nothing job today, but back then, because of the fact that the Pacific war was rising in everybody's minds, it was central. Japan had been making all sorts of inroads both into Mainland China and into Malaysia and the islands. So it was an important job.

Huffcutt had done some kind of public service work in Washington and had gotten close to this guy. He was very serious, very concerned, very much a public servant. He went to Manila with this guy whose name I've forgotten. Huffcut was never in the house when I was there, but I got to know him in the Association and got to admire him particularly for taking this job. You may have read that a few of the highest ranking people were evacuated from Manila by submarine after the Japanese invaded. They left from Corregidor Island, the island there in Manila Bay. Huffcut was not one of those. He became one of the people in the Bataan Death march. He was shot in the back and killed by the Japanese. He was a tragic figure, but he certainly did more than his share of service.

So the argument had been going on since 1938 or 1939, since I arrived at Telluride House. When the draft came along, people had to make up their mind what to do.

BE: Wasn't it compulsory?

RS: Yeah, but you still had other things to do. I, for example, got a deferment, and I had to ask for that. Some other people went into ROTC. Nobody fled -- as people did during Vietnam, when they went to Canada. If you went to Canada then, you went like Charlie Ennis, to fight.

BE: Of course, there was another alternative, which was just to register for the draft.

RS: Mostly that's what happened. Then various crazy things happened. I remember Jim Tucker, a Deep Springer who later became a lawyer in Southern California. He was a very buoyant, happy-go-lucky character. Not a scholar at all. He was a member of the House and a member of the Association.

Anyway, Tucker had the luck of the damned. For one thing, you get a number in the draft that is either a low number or a high number. Then there were draft boards. One would go out of business and another one would come into being, and so on. One after another, he survived things. He should have been drafted, but he wasn't. He didn't want to be drafted. And he just had absolutely magic luck, but then suddenly all at once everything happened to him. He was drafted. He flunked his courses and busted out of school. He was drafted immediately into the Army and got into the military police. His luck just went over the precipice.

Jim's story was kind of an extreme case. I'm giving you various responses of various people. It was all pretty much probabilistic.

BE: So life at the house was changed dramatically. Some people started walking around in uniform.

RS: No.

BE: Didn't people in ROTC wear uniforms?

RS: Maybe they did when they left the house in the morning or something like that. But certainly not at lunch or dinner. Nobody wore a uniform at meals or public events.

BE: Was non-intervention a safe position to take? Were you considered unpatriotic?

RS: Sure you were. And on the other side, if you were for intervention, you were considered stupid. Both sides had their adjectives for the other. Both sides took the point of view that any pledge they had was being honored by their position. You know how the game is played.

BE: How hot did it get?

RS: It didn't get hot. The debate cooled off with the coming on of the real war and the liquidation of Telluride House. We turned Telluride House over to the military.

BE: Were you out of Cornell by then?

RS: Just barely. I became president of the Association in 1945. I know I was president the summer of 1945 because I went out to the Deep Springs board meeting that summer. The presidency was for two years in those days. So probably 1945 to 1947, and by then the house had been turned over to the Navy. I didn't have anything to do with that, and I don't think the people who lived at the house had much to do with it either. It was done by the Association.

BE: Which you were a part of.

RS: Yeah.

BE: Do you remember a big debate, or did somebody just say hey, let's turn the place over to the Navy?

RS: There was no debate at all. There wasn't any choice.

BE: Did the Navy ask for the house?

RS: Yes, through Cornell. I think Cornell was doing the negotiation. I know Cornell did the negotiation to give it back to us, because I did that in the fall of 1945. It almost killed me, although it wasn't intentional.

BE: How?

RS: Well, we had to drive up to Ithaca. George Sabine was I think in Philadelphia or something like that, and I was in Princeton. We drove up in my little car, and he was driving. Driving back, near Catatonk, on the road to Owego, he was driving too fast on an icy road, and oh my God. We should've been killed.

BE: You and he were the ones who negotiated with Cornell.

RS: I don't know why George was involved, but he was one of the few people who was available. He was also a physicist working for the Naval ~~Reserves~~ Research Laboratory.

BE: So at the beginning of the 1942 school year, the house was full of naval officer candidates.

RS: No, I think it was probably a year later than that. I know that 1942 was my first year of not living in the house. I got married in June of 1942.

BE: Did you visit the house very much during the time that the Navy was using it?

RS: No, not at all. Never. For lots of reasons. I was busy doing other things, and also it wasn't really our property then. We didn't have any rights or privileges. They put a vast number of people in the building. Somehow I remember 120, but anyway a huge number of people in the house. They had bunk beds in every room, and they were very hard on it. They ruined the telephone system completely.

BE: Someone told me that they painted the woodwork.

RS: Yes.

BE: As part of their work detail.

RS: I guess it was part of their work detail. But anyway, most of the woodwork got painted. I think it was because it was Navy regulation, et cetera. What was the Navy expression? If it doesn't move, paint it.

BE: Tell me about the negotiation to return the house to Telluride.

RS: Well, it was mainly a question of indemnity for all the things that they had done wrong. We took the attitude that the university, which was negotiating with the Navy, had to be our friend. Of course, the university had lots of other fish to fry with the Navy. The problem was getting their attention. It was hard to get people to come down and look at the house. I think that's what we probably accomplished that weekend when I came up with George. But it was just a question of money. There was no question about timing; the Navy didn't want the house any more, and the university didn't want it. Well, I guess the university wanted it, but they knew they couldn't get it. It was ours. Cornell was desperate for space right after the war, but I don't remember them saying that they would use it as a dormitory or anything. I don't think the university really caught on to the fact that they desperately needed space until a little later. But there was the issue of money. It was just a question of reimbursing the university, mostly for university buildings, and Telluride House was an afterthought. I don't remember how successful we were, but we did our best.

BE: Why did you ask for a draft deferral?

RS: Well, I was fascinated by the things I was doing. I worked the summer of 1941 at Bell Telephone Laboratories. I worked six days a week, and sometimes half of a seventh day. The klystron had been invented and was being made into a receiving tube by Bell laboratories people, and I was at the bottom of that ladder. But I was actually making experimental tubes and doing experimental modifications of them, and I got a couple of patents out of it. They were absolutely useless, but that's all right.

I also got three patents later when I worked at RCA, but I was all keyed up about microwave radar, and thought I could do something with it. This tube I worked on was a so-called McNally Tube. McNally was my boss. It became the receiving tube for all ten centimeter radars, almost immediately It made it possible to do [radar?], if you had a receiving set with a maximum voltage of 300 volts. Three hundred was the magic figure for many, many years, like forty years in radio. Above 300 volts, it was supposed to be dangerous. You had to have some kind of a cut off. It was pretty arbitrary, but there it was.

Anyway, the McNally tube was a 300-volt tube, whereas the tube it replaced was 2,000 volts. It made all the difference in the receiving sets that you could put on an aircraft carrier, for example, certainly for airborne equipment. Because 2,000 volt tubes would arc over and become extremely dangerous. Three hundred volts was like an ordinary receiving tube that everybody knew about.

BE: So you were involved in research that supported the war effort.

RS: Well, it was actually development, but it was called research.

My real decision came in mid to late November of 1941. We still were not in the war, but it was perfectly clear what was happening. One way or another, we were going to be in the war. It was only a matter of time and not very long, probably. I was offered a job at the Western Electric Company, which you remember was the manufacturing branch of the Bell system. I ~~was~~ would be essentially supervising the production of this tube that I'd been working on experimentally during the summer. The manufacturing plant was in downtown Manhattan. The question was whether to do that or continue on my thesis work, which clearly was going to be classified [as sensitive information by the military] and eventually was. That was the only decision I made, really, was to go back to working on my thesis. The other one, asking for a draft deferment, was almost automatic. I just thought that was the place to be. It would be more fun and more effective. Why clean latrines for the Army if you can be doing research? So it may have been mostly self-interest, but there it was.

BE: I think you were in a fairly unique position. Radar was one of the things that we had that the Germans didn't have.

RS: Sure. I believe heartily in the well-known expression that radar won the war. The atomic bomb ended it. The bomb may have shortened the war by many months, but radar was the thing that won the war. Microwave radar, in particular. The Germans never discovered that we had three-centimeter radar. They just barely discovered that we had ten-centimeter radar. Their U-boats had all these mysterious sinkings, and they never got a word from the U-boat that they had been spotted. Well, we were looking at them with microwave radar, and the Germans didn't have scanning receivers for all the frequencies that would have told them that they were being looked at by a radar.

My decision was probably wrong, because what work that I did on the thesis eventually got used, all right, but it was too late in the war to do all that much good. The work that I did at RCA on microwave radar, for three years starting in the spring of 1943, was almost all of it. Some of the work went out into the fleet through a thing called the airborne coordination group, but most of it came too late.

BE: Of course, you had no way of knowing that at the time.

RS: No. But you asked about my conscience, and so on. In the early fall of 1944, I had arranged with a group of Naval officers that we had been working hand in glove with, called the Airborne Coordination Group. I had arranged to get a billet in the Navy as a JG if I could get into the Navy. So I went to my draft board and applied to do that. But by the fall of 1944, RCA was already thinking about television and the post war years, and so they fought my application. They refused to release me to that. I'm hoping when we look for the letters and so on, which we haven't done, I will find the letter from the draft board. It was the god-damnedest letter I think I've ever received.

BE: What did it say?

RS: I think I can quote it exactly. It was a one sentence letter. It said "this registrant, Robert L. Sproull, has been deferred since such and such a date; and therefore, he cannot be released at this time. " That was the letter. It was a complete *non sequitor*. And I actually I fought it at the local selective service headquarters in Trenton, although I don't think it ever got to a court. It dragged on and on. By the time the Trenton group said that they agreed with the draft board, I probably could've just thumbed my nose at them and probably gotten away with it. I probably would've been sent to jail. But it was too late. The war was over by January of 1945. A lot of people were still being killed. It was a bitter business, but the Ardennes in December of 1944 was really the last gasp of the Hitler Reich.

BE: Your thesis was classified. What was it?

RS: It was on thermionic emission, and what happens when you take the thermionic emission from an oxide cathode for a very short time. It had been discovered by the British, or by the people at the radiation lab, that you got a lot more current from a cathode if you got it for only a microsecond or so. The question was why. It was a very important question because in microwave radar, a one micro-second pulse was typical. You used an oxide therminoic cathode, typically. So it was a great boon to get ten or fifty times as much current. But we wanted to know why, because we wanted to know how long it would last and what we could do to get even more current. My thesis was to try to explore and find out why. I never could really find out why. I found out what you had to do to get more current, however. Namely, you had to make the vacuum tubes in a much cleaner environment than they had ever thought of making them in before. It was just a question of the housekeeping and cleanliness and outgassing of parts and so on. You could get more current and that could last longer.

BE: We've kind of gotten beyond your years at Telluride House. Is there anything about the Telluride experience that we haven't talked about that you wanted to talk about?

RS: Well, we talked about public speaking I think, didn't we?

BE: Not at Telluride. What was the difference between Telluride and Deep Springs public speaking?

RS: Well, at Deep Springs we took it a lot more seriously. At Telluride it was a semi-joke. In fact it was so close to a joke that one year the Association got uptight about it and said we're going to do something about it. So they hired an instructor to teach public speaking. The Association should have known that was a bad idea. But it wasn't really an awfully bad idea. The guy's name was Mowat, and we used to call him Mo-at because his whole lesson was "vibrate your vowels." We got that translated as "bowels."

But on the other hand, I must say that the public speaking approach at Telluride probably helped me as much as anything Telluride Association did. In many years working with trustees at Cornell and then at Rochester, and working with boards of directors, I was frequently working with people older than I was, and quite frequently their hearing wasn't all that good. Public speaking alerted me to the fact that Slurvian is the *lingua franca* of Americans, not English.

BE: Slurvian?

RS: Slurvian. Yes. Particularly dropping things at the ends. Have you noticed that people on the public radio station will slide in with a sentence and end with a final vowel that will be hardly known. A lot of the phrase is carried by that final vowel.

BE: I've had exactly the same experience. Probably of all the courses that I took in college, public speaking at Deep Springs was the most rewarding. If you can do it well, it's an enormous advantage.

RS: Well, that and writing. At Telluride I never had a course in writing, but you had to write articles and letters all the time. Corresponding in those days was a lot more formal and substantial than it is now. E-mails are fast and friendly, but they get to be pretty chaotic too. Back then you just paid more attention to writing, and Telluride always was great on that. You felt that you were on view all the time. The Telluride atmosphere of self-criticism in order to avoid being criticized. It was like a fish bowl all the time, and your writing and speaking were always subject to adversarial comment.

BE: Self-criticism to avoid being criticized. You knew that what you were going to say was going to face scrutiny, so you were harder on it before it left your mouth.

RS: Right. Isn't that part of the Telluride tradition?

BE: Well, I guess that's a good way of saying it. Do you think that was also an element of Deep Springs?

RS: Oh sure. The fishbowl atmosphere is part of Deep Springs, too.

BE: You mentioned several things that you feel you got from your Deep Springs and Telluride experience, both in this conversation and in the last one we had. Now, you were clearly a very talented student. You had an interest in science before you ever got to Deep Springs. Had you gone to another university, you probably would have had an equally distinguished career in engineering or physics or whatever. But the particular educational experience that you had -- did it give you anything you regard as unique?

RS: Well, probably. The things that we have already talked about add up to quite a package. I'm not sure that putting any frosting on that cake is really appropriate. I could put my heart on my sleeve for a moment, but maybe I've already done that too much.

BE: No, I don't think so.

RS: After the war, my field became known as solid state physics, but it wasn't called that when I started in it. I had many, many opportunities to go into industry and take jobs that were pretty decent. I could've gone back to Kodak. I could've gone back to Bell Labs. I could've gone to IBM. But the Telluride mole was always working under the grass. I always felt that those kinds of jobs were not worthy of the investment that had been made in me.

Many of my best friends are captains of industry. I've done my part as a director of a number of corporations, sometimes straightening out some people that were not far from being Enron-types. But nevertheless I heeded the call to stay in education and do lots of volunteer work. Since retiring I've done nothing but volunteer work, including going on missions to the Republic of Georgia and to Kazakhstan. I think my basic choice of career reflects the difference.

That is the Deep Springs idea. What is it that's different from an ordinary education? The two most prominent aspects of it are to force a student to think about his career before he freezes it in by graduating from a college -- that fixes the choice for most people. It gets you started early, as a freshman, saying what are you really going to do? When you pull the coffin lid shut from the inside, what are you going to say to yourself? That's one part of the Deep Springs and Telluride idea. The other part is teaching people early on to take responsibility for their actions that other people count on. In fact, as I've mentioned before, that's why I felt so dreadful about the recent accident with the tractor. It was the exact opposite of that type of thing.

So I think that that the Telluride difference is in its emphasis on responsibility. A number of times I've had a choice -- do I serve on a group the routine way, or do I become chairman? Usually I became chairman. When I took the responsibility, I took it seriously. I think that's a good part of the uniqueness of the Telluride. You don't shy away from taking responsibility.

Another aspect in my own life -- I've been talking with Mary for some time about this. I don't think Telluride emphasizes it, but in the various choices I've had to make, I invariably have taken the riskier one. Coming back to Cornell after the war was a very risky decision. I had taught at Deep Springs, and I had taught nights during the war at the University of Pennsylvania, but those experiences didn't give me a sense of how teaching would go as a career. I didn't know whether I could make it or not. I don't think you probably focused on it, but everybody was predicting a very serious depression right after the war. There had been a depression after the First World War, and everybody was predicting bread lines and back to the heart of the depression. So going to Cornell and choosing a career in education, instead of staying at RCA where I would have worked on television, was a very risky decision. It was motivated also by the fact that I avidly hated the idea of television. I couldn't see spending my life working on it.

BE: Even then.

RS: Yeah, even then. See, RCA was big on television. They were banking their future on it.

BE: Why did you hate it?

RS: I had seen it. It's like the old expression, "He's the kind of guy you can't really dislike until you really get to know him." RCA had a thing called the TRK 120 -- a big thing, a big long tube. They had it standing upright with the image on the top, and you looked at it through a mirror. That was the original electronic television. RCA was going to make it work. We babysat for good friends Albert and Lillian Rose during the war, and they had one. Rose was one of the inventors. In fact, he invented the image orthicon, which is the tube that made television really possible. Up to that point, they had to use so much light in a television studio that no serious performer could really perform. The image orthicon made it possible to record an image with much less light.

When were babysitters for the Roses from time to time, we looked at their television. The only television program on was wrestling from the Saint Nicholas Arena, wherever that was, up in Brooklyn someplace. From watching that I could imagine what television was going to be like, and my imagination placed it not far from what it turned out to be.

BE: So you were sitting there in your friend's living room watching wrestling from Brooklyn.

RS: Not very often. I mean, we'd look at it for five minutes and then turn it off.

BE: This is 1943 --

RS: 1943 to '46.

BE: You sat there, and you looked at that thing, and you said, "This is no good."

RS: I said, "This is not what I want to do." I didn't say it was no good. I just said it's not what I want to be involved in. I didn't know whether it was going to be successful economically or not. I had no idea, but I thought that the idea of having that in your living room was going to tie people down in what I considered to be an unhealthy way. I didn't want to have any part of it.

BE: Well, you were dead-on correct about that.

RS: We didn't have a television set until I went to Washington. During the presidential campaign of 1964, we bought a little Sears Roebuck black and white set. I figured that I had to, with my job in Washington. It was one of the worst decisions I'd ever made. You may remember that it was the dullest convention ever. It was a runaway for Goldwater, and it was not worth looking at the television. That was our first television set, our only television set until we went to Rochester in 1968.

BE: So you made a conscious choice not to participate in the development of television.

RS: I took that risk. And then I took a risk going to Brussels in 1958 with a wife and two kids, putting the children in a French school. Nancy's teacher didn't know a word of English. Bob's teacher knew some English.

BE: Why did you go to Brussels?

RS: I was on sabbatic from Cornell and trying to work at a lab there as a European research associate. I'd also lectured for NATO around the continent, which was a way of getting my expenses paid to visit physics laboratories at various places. That was good fun. We made lot of friends and we got people coming to our labs here from there afterwards.

BE: When did you leave Cornell for Washington?

RS: 1963. I was in Washington for two years.

BE: So you were on the Cornell faculty from like 1946 to 1963.

RS: Yes and then from 1965 to 1968.

BE: Then after 1968 you went to the University of Rochester [New York].

RS: Yeah. I went there to be the Provost. I continued to teach after I became a vice president here at Cornell, but going to Rochester as Provost, I knew I couldn't teach. It would be unfair to the students and unfair to me. My predecessor as Provost had been fired over a recruiting business. I think it was for the CIA. I'm not sure. But anyway, there was no way I could teach at Rochester. So I went there as Provost, and then I became President, which didn't really mean anything. I continued to be the number two man as President.

Allen Wallis, who was President, somehow or other got the idea in 1970, two years after I'd been there, that I was going to become president of Brown. There were only two things wrong with that idea. One was that Brown wasn't going to ask me. The other was that I wasn't going to accept if they did ask me. But any rate, he had that idea, and once Allen Wallis got an idea you couldn't talk him out of it. So he decided the way to keep me from leaving was to get the trustees to appoint him as Chancellor and me as President. That way, I couldn't very well give up President of Rochester to go to Presidency of Brown. It didn't matter to me, and he thought we could hire another person as Provost then. I knew that was wrong because it wasn't going to be somebody else who would do the hard work of being provost and only be number three in the university. That just wasn't going to work. I told him so, but it didn't impress him. So anyway, I became President in 1970, which confused everybody. I didn't become the number one man until 1973.

BE: I see. I see. So when you were President initially you reported to the chancellor?

RS: Yes. But I ran the university. The Chancellor was mostly interested in writing and Washington. He had been in the Nixon White House, but I disagreed with Allen on almost nothing when it came to running the university. I disagreed heartily with him about his politics. I couldn't stand Nixon, and I was an ardent anti-Republican in those days -- not in these days, but in those days. So we just didn't talk about Nixon. I did the operation of the university and tenure appointments.

BE: I have two more questions relating to the time you were on the Cornell faculty. How did you decide to get involved in administration rather than teaching? Did you ever have a moment where you said, "I'm not going to devote myself to teaching anymore. I'm going to be an administrator?"

RS: That's a story of some interest. A lot of people ask about it. It's really very simple. At the end of the war, we started trying to do physics in Rockefeller Hall, which you know was a little wood frame building. Between the ceiling of one floor and the floor of the next, the joist region, it is all filled with cinders.

BE: Is that right?

RS: Yeah. The architect had the idea that since the building was going to burn down, you might as well dampen it with cinders. Anyway, we were trying to do ultra-clean experiments on nearly perfect crystals--

BE: In a building filled with cinders.

RS: In a building also where water would trickle down to the basement every time you had a flood in the teaching laboratory on the top floor. So in addition to working with my graduate students, I was constantly trying to find money to get that building to work. I was getting new electrical nerve systems into the building, getting a new water system, getting a new sewer system. I was also getting support for not only for my own students and their work, but also helping others, particularly theoreticians, with getting support for their students. Lloyd Smith and I together had one of the first Office of Naval Research contracts right after the war. ONR was a marvelous agency in those days, and we began to get some support. When the National Science Foundation (NSF) was born, we began to get equipment support from them. Anyway, we got some foundation support. We got nickels and dimes everywhere. But more and more, my job became seeking support.

In 1958, I brought over a young guy named Robert Pohl from Germany. I had known his father very well, but I had particularly known his thesis advisor, who had recommended him very highly. Pohl came over the first of April 1958 to help in my lab while I was gone to Europe. Between the first of April and the first of August when I left, he just absolutely bloomed in the lab. My graduate students were finding it so much easier to deal with him than with me. I had gotten the reputation of working graduate students too hard. Starting almost the second of April, I didn't have that reputation anymore. Bobby had that reputation. He worked them harder, and they stopped complaining about me. But anyway, that was the first part of the answer. He had come over and started doing my teaching job so well.

The second part was while I was gone, Paul Hartman and Dale Corson and others cooked up the idea for having a new laboratory for atomic and solid state physics. By transatlantic telephone, they talked me into being the first director of it. It was probably a mistake, but anyway the idea was to get a laboratory, so-called with a capital L, started by hiring a secretary and an office and doing fundraising, both with the deans locally and with people in Washington. So I did that. I had also done some administrative work earlier. I had been editor of the *Journal of Applied Physics* for three years and had gotten into the American Institute of Physics that way. So it was not completely foreign to me anyway. Then main thing though, was that we were competing for this big federal contract, the material science center contract. So that was really my job during that winter of 1959-60, and we won the contract. We got the biggest one and the largest of three contracts awarded, and it contained the magic thing -- support for a building. Over a ten-year period we would get, I think, four million dollars.

BE: Which turned out to be Clark Hall.

RS: It turned out to be Clark Hall. We immediately of course went to work to try to get individual support for all of the other things that were in the building other than the material science center for physics and the chemistry library. Some of the space center people were there, Engineering and Physics was there. The undergraduate physics advanced laboratory was there, and so on. So half of the building was not a material science center, and so Jim Perkins and I got the Clarks interested in that, and it became Clark Hall.

The key to getting the building was the Advanced Research Projects Agency (ARPA) contract. So I became director than of the material science center. By then the decision was essentially made. I still had a graduate student, one or two. But it was perfectly clear that the people I had brought in and helped bring in -- Dale Corson and I, and Paul Hartman and I, and later on Don Holcomb was one of the ones we brought in, but then he became one of the recruiters. Anyway, the young kids that I had brought in or helped bring in were much brighter than I was. They were going to get the good graduate students, and it was going to be no fun because I had essentially spent so much time as an administrator while still nominally just a professor of physics. I was going to compete unfavorably. So there was just no question about it. I didn't have to make a decision. It was made for me. I became an administrator.

BE: See I was going to interpret it a different way because of the earlier points you made about inevitably going toward the riskier decision and also inevitably taking responsibility for things. It seemed to me like if you were in the basement of a building that had to be clean but was full of cinders, and you were dreaming about a new building, you took responsibility for building it rather than just complaining about it.

RS: That's right. That happened.

BE: And certainly that was a much riskier course than just continuing to teach physics.

RS: That's true. And then going to Washington in 1963 was extremely risky. I didn't even know whether a major general outranked a lieutenant general. I didn't know what the insignia on the shoulder meant. I never had any experience in the military.

BE: What was your job in Washington?

RS: I was director of the Advanced Research Projects Agency (ARPA). I had something like a five hundred million dollar a year agency to run. Half of the staff were military, and so there was a lot of risk in whether I knew what I was doing or not.

BE: Were you approached and asked to take that?

RS: Yes, but it was kind of engineered. I had I decided that if I had a future it was not being a research professor of physics. I could be a teacher of physics, but it was no fun competing for graduate students and losing on the research side. So I had about decided I was probably going to be in administration of some sort. The people at Wesleyan in Middletown, Connecticut had offered me a Deanship of science there. It was an interesting job, and I liked that place. A great Cornellian, Butterfield, was president. He had done some of the wooing. Well, this was almost but not quite signed up for. I don't think I was very open about it telling anybody around here what I was up to. Anyway, Dale Corson found out about it and told Jim Perkins, who hadn't come yet. He was just coming on as Cornell President then. But he was doing the work in the spring of 1963, lining up staff. He had hired Dale as provost already. Perkins said to me, "Why do you want to do that?" And he called one of his friends in Washington, Jerry Weisner, the president's science advisor.

Weisner asked me to come down and see him in Washington to be either the associate director, the second man, at the National Science Foundation, or director of ARPA. Well, I knew ARPA. They were the people who had given us the money for the building. In effect ARPA was saying, "We've given Cornell this money, so now you give us this guy." This also happened a couple of other times with corporations when we went for money. That's how I got on boards of directors and things.

ARPA was by far the riskier operation from my standpoint, but it was also by far the most interesting one. I knew what the NSF was. I wasn't going to be surprised by anything that happened there, and I would be the number two man. It was kind of interesting to think what it would be to be number one for a change. So I went and talked to Harold Brown, who was the director of Defense Research and Engineering, who would be my boss at ARPA. I would be one of his deputies as well as being director of ARPA. It's a double hatted job. I was very much taken by him. It was risky as hell, because one ARPA director after me lasted only a month.

BE: What made it so risky? Was it the risk of managing so many creative people?

RS: That, and not knowing the system. Not knowing the inner workings of how the Pentagon operated. I knew there were a lot of ambitious people there. I knew there was a lot of cutthroat going on. I just didn't know exactly how I was going to avoid it. I didn't have a chance to meet the key people because the one key job was vacant -- director of nuclear test detection -- and I had to hire for that. I hired actually at Cornell. At the Statler Inn. I hired my director of nuclear test detection after I took the job but before I reported for work.

It was risky because it was entirely different from anything I had ever done or had any competence on. The ARPA contract with Cornell was unlike anything else that ARPA had. It was done with ARPA simply because the defense department made a decision to do it, and ARPA was the only agency that had the freedom and flexibility to do it. So the only part of ARPA I knew was the part that wasn't like any other part of ARPA.

BE: I want to ask you one question about your time at ARPA. Do you remember the first time you saw or talked about batch transmission of information between computers, what became the Internet?

RS: Oh well that's very simple. But look, that is a whole morning's conversation. That has to do with Joseph C.R. Licklider, who is one of the most unheralded geniuses of the twentieth century, and the man he hired, Bob Taylor. I didn't really hire Licklider, but I kept him when he wanted to leave. My boss expected me to not only fire Licklighter but also cancel the whole information processing section of ARPA. We needed to cancel something. We needed to get rid of fifteen to twenty million dollars of expenses, and my boss suggested information processing. But we're into this subject now, and we'd better stop because it's a whole other tape.

BE: Maybe we should meet again. It would take a while, but I think the story ought to be recorded. I'm also not prepared to ask intelligent questions about it. But I'd like to do a bit of reading and talk to you again.

RS: I tried to get the national Medal of Technology for Licklider, but by then I was working without a secretary and I had a lot of other irons in the fire, and I didn't make it. In fact they gave the medals to a bunch of light weights that year. Licklider was back at MIT by then.

BE: Let me ask you just one other thing before we close this session, and it goes back to Deep Springs and Telluride. When you were on the Cornell faculty, were you a frequent guest at Telluride Association? Did you sort of monitor the changes that were happening there?

RS: During the first couple of years after the war, I was there quite a lot. There were things still to be done in negotiating with Cornell. There was a problem of getting the telephone system rejuvenated. In fact, I went to every house meeting for several years right after the war. There was a problem, because a lot of new people were on board, and we had to try to get some kind of tradition back into the thing. Of course, I realized I was in a hopeless position. I was an old granddad and thoroughly discredited. But nevertheless I think I got some messages across to some people. I remember that most of what I said was unpopular. I remember one night, when one of the young people who was not really dry behind the ears, got haughty and said, "Sproull, why don't you stay away in droves?"

BE: [laughing] That's pretty good.

RS: I started staying away in droves.

BE: You were invited to stay away.

RS: I was busy with other things, and my influence, such as it was, had tapered off very sharply. That's fine. That's the way it should be.

BE: How long did you stay in the Association?

RS: Until 1951, maybe. I went to a Oak Ridge on sabbatic, and I thought that's a good time to get out.

BE: Did you go to all the Telluride conventions?

RS: Oh sure I did. I never missed a convention. I was a custodian during most of that period, an elected custodian after I ceased to be President. After I ceased to be secretary, I think I was elected a custodian and then became president, who was automatically a custodian. Then after that elected, I think I was still a custodian when I left the association, I'm not sure.

BE: So you were a custodian for most of 13 years. You were elected to the association in 1939 as a custodian because you were secretary.

RS: That exaggerates it. I can't have been a custodian for so long. I was just in it for a long time. Well, the records will show whatever it is.

BE: One of the things that bothers me about Telluride and Deep Springs is that so many of the people that pass through it exist in a sort of a vacuum in terms of what went before, and what went before is so interesting and so important. [deleted discussion of custodan minutes and dates of Sproull's two terms on Deep Springs Trustees]

BE: Before you were on the Deep Springs board, did you have much interaction with the college?

RS: Not at all. I made a conscious decision that I was going to be too busy to give any kind of decent service to both institutions, and I had to choose. Deep Springs obviously needed help. Telluride didn't. I think that decision has been widely misinterpreted by people like Claire Wolfowitz, who thinks somehow that I got mad at Telluride Association. I didn't at all. It was simply that I had to concentrate. Being a university president is a full-time job, and up until that time I was also doing various other things. I always had so many things on my plate that I really just had to pick and choose.

BE: But you gave money to Deep Springs.

RS: Yeah, I also donated to Telluride Association, but not much. I figured they had plenty of money. My Deep Springs donations were minimal in the 1950s and 1960s because I didn't have any money. We ended up with more money than we ever intended to have, but that's only how I was able to give anything to Deep Springs that amounted to anything.

BE: Do you have any observations or overall impressions to leave with me relating to your time on the Deep Springs board? It really wasn't the subject that brought us together, but--

RS: Of course, it was an entirely different board than the board I had witnessed as a student. The board I served on was a more routine board. The reason they talked me into going back on the board, which I still think was a mistake, is that I represented university respectability. When Deep Springs had to deal with things like faculty and library and so on, they wanted to make decisions that weren't so far out and Deep Springs indigenous. They also wanted me to try to put into Deep Springs' hands the fundraising tools that universities had. One of the things that I tried very hard to do, and I think we succeeded, was to get Deep Springs to concentrate on spouses -- because that's where the money is, to quote Willie Sutton. Deep Springs now has uniformly put Mr. and Mrs. on things, which before they had always turned their back on. I think wives were not really welcome to visit Deep Springs. I turned that around as sharply as I possibly could. It was things like that, things that are well known and standard tools of universities but were not standard tools for Deep Springs. That was sort of my role.

BE: Nobody at Deep Springs had considered that the major financial decisions in households are shared by the women.

RS: Not only that, but the widows ended up with the money. As a noted Cornell vice president once said, "When financing private higher education is concerned, where's there's death, there's hope." So it was in the widows' hands that they had to think about things.

The answer to your question is that I considered my role as not to try to make Deep Springs a routine place. I regarded the uniqueness of Deep Springs more sharply I think than anybody else on the board. So I was not about to make it just another Cornell farm team, but on the other hand, I was trying to put into Deep Springs hands the tools and wisdom, such as it was, that was common to universities and colleges, but was unknown to Deep Springs.

BE: Did you have a hand in bringing Dale Corson onto the Deep Springs Trustees?

RS: I talked him into it.

BE: Did he have any knowledge of Deep Springs at all?

RS: Not very much, just that every Cornell faculty member knows about Telluride House. I don't think he had ever visited Telluride House. And he's a sharp guy, and he said, "What does Deep Springs want out of me?" I said, "Well, look, I'm going off the board." People had asked me to stay on the board beyond the eight years. I said nothing doing. I had worked with Jim Withrow to get this business of signing your resignation when you get on the board. It was too valuable a thing, and I was not going to subvert it. So they said, you find your replacement then. That's where Dale came in. Dale was our next door neighbor, you know, on Northview Road, and I thought he'd be a fine trustee of Deep Springs. It wasn't easy to convince him. Once convinced, of course, he was a good trustee.

BE: When I was a Deep Springs student watching the trustees meetings, Dale was the one who I always felt had the situation figured out. The rest of them were sort of like a circus act.

RS: That's right. That's true. That's true.

Some of my most unpleasant hours were spent when I was elected alumni trustee at Cornell. I have no idea why I was silly enough to run. I guess I was sure I would lose, and the alumni association somehow or another wanted a candidate. Anyway, watching Dale preside at a trustees meeting -- there was all this running around stuff we were talking about going on, and he was powerless to do anything about it until it had run its course. Then he and Austin Kiplinger and two or three others stepped back to try to make some sense out of it all. But he had to suffer through all of that nonsense first.

BE: Of course when I was a student, the financial situation was fairly dire, or at least so we were told.

RS: It was.

BE: And the trustees meetings had the same structure over and over again. Merritt Holloway and Ed Cronk would come in and present the finances to the trustees, and it would be, "Oh my gosh. This is terrible. How are we going to make it? What are we going to do?" Then at the certain point in the trustees meetings, Jim Withrow would write a check. So it was kind of like a play. All the power was gravitating toward Withrow.

Well, I think I've pretty much asked the questions that I had. Is there anything else that we haven't gotten into?

RS: Oh I could think of lots of thing but you certainly asked some very stimulating questions I must say. I hadn't thought of a lot of these things.

BE: Well, thanks, I appreciate that. [Deleted discussion of 11/23/02 Custodians meeting]

RE: One thing before I forget it. If you're interested in the Internet origins et cetera, you should read a book called *The Dream Machine* by Michael Waldrop. It is a biography of Licklider, and it's a story of the origins of personal computing and networking. And one final thing about *The Dream Machine*. My ever-loving blue-eyed son, who is a fellow with Sun Microsystems and so on and the computer type from way back, says that the story in *The Dream Machine* is accurate, which is kind of interesting because there's a lot of who did what in there. The other thing I want to warn you about *The Dream Machine* is there's some part of it that's inaccurate or at least misleading. If you look in the index, you'll find five entries for Robert Sproull. Three of them refer to me, and two of them refer to my son. So it's mixed up.

BE: All right. Thanks for setting me straight. Okay, I think we can stop for today.

RS: It's been fun.

BE: It has.

END OF INTERVIEW

Transcribed by L. Altizer, November 14, 2002