Regional Oral History Office
The Bancroft Library

University of California
Berkeley, California

University History Series

A. Carl Helmholz

FACULTY GOVERNANCE AND PHYSICS AT THE UNIVERSITY OF CALIFORNIA, BERKELEY, 1937-1990

With Introductions by
Walter D. Knight
and
Henry J. Vaux

Interviews Conducted by
Graham Hale and Ann Lage
in 1975 and 1989-1990

Since 1954 the Regional Oral History Office has been interviewing leading participants in or well-placed witnesses to major events in the development of Northern California, the West, and the Nation. Oral history is a modern research technique involving an interviewee and an informed interviewer in spontaneous conversation. The taped record is transcribed, lightly edited for continuity and clarity, and reviewed by the interviewee. The resulting manuscript is typed in final form, indexed, bound with photographs and illustrative materials, and placed in The Bancroft Library at the University of California, Berkeley, and other research collections for scholarly use. Because it is primary material, oral history is not intended to present the final, verified, or complete narrative of events. It is a spoken account, offered by the interviewee in response to questioning, and as such it is reflective, partisan, deeply involved, and irreplaceable.

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A. Carl Helmholz, circa 1970.

Photograph courtesy of UC Berkeley
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Family and youth in Rochester, Minnesota; education, Harvard and Cambridge Universities, 1930s; graduate studies and research work in high energy physics at the Radiation Laboratory, UC Berkeley, 1937-1950s: recalls Ernest Lawrence, Burton Moyer, Harvey White, Emilio Segrè, Edward Teller, Manhattan Project; Department of Physics: chairmanship, 1955-1962, undergraduate and graduate curriculum, loyalty oath controversy, faculty hiring and retention; service on first governing board of faculty retirement system, 1950s; faculty social relationships: Faculty Club, Section Club, Kosmos Club; issues surrounding UC oversight of Livermore and Los Alamos Laboratories; Academic Senate service: committees on Committees, University Welfare, Academic Freedom, Educational Policy; faculty response to Free Speech Movement and ethnic studies controversies; emeriti affairs. Appended brief biography, Elizabeth L. Helmholz.

Introductions by Walter D. Knight, Professor of Physics, Emeritus, UC Berkeley; and Henry J. Vaux, Professor of Forestry, Emeritus, UC Berkeley.

Interviewed 1975 by Graham Hale for The Bancroft Library History of Science and Technology Project and 1989-1990 by Ann Lage for the Regional Oral History Office.

Donors to the A. Carl Helmholz Oral History

The Regional Oral History Office, on behalf of future researchers, wishes to express its thanks to Jean and Henry Vaux and to the Laird Norton Foundation. Their encouragement, support, and contributions have made possible this oral history memoir.

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PREFACE

When President Robert Gordon Sproul proposed that the Regents of the University of California establish a Regional Oral History Office, he was eager to have the office document both the University's history and its impact on the state. The Regents established the office in 1954, "to tape record the memoirs of persons who have contributed significantly to the history of California and the West," thus embracing President Sproul's vision and expanding its scope.

Administratively, the new program at Berkeley was placed within the library, but the budget line was direct to the Office of the President. An Academic Senate committee served as executive. In the more than three decades that followed, the program has grown in scope and personnel, and has taken its place as a division of The Bancroft Library, the University's manuscript and rare books Library. The essential purpose of the office, however, remains as it was in the beginning: to document the movers and shakers of California and the West, and to give special attention to those who have strong and often continuing links to the University of California.

The Regional Oral History Office at Berkeley is the oldest such entity within the University system, and the University History series is the Regional Oral History Office's longest established series of memoirs. That series documents the institutional history of the University. It captures the flavor of incidents, events, personalities, and details that formal records cannot reach. It traces the contributions of graduates and faculty members, officers and staff in the statewide arena, and reveals the ways the University and the community have learned to deal with each other over time.

The University History series provides background in two areas. First is the external setting, the ways the University stimulates, serves, and responds to the community through research, publication, and the education of generalists and specialists. The other is the internal history that binds together University participants from a variety of eras and specialties, and reminds them of interests in common. For faculty, staff, and alumni, the University History memoirs serve as reminders of the work of predecessors, and foster a sense of responsibility toward those who will join the University in years to come. For those who are interviewed, the memoirs present a chance to express perceptions about the University and its role, and offer one's own legacy of memories to the University itself.

The University History series over the years has enjoyed financial support from a variety of sources. These include alumni groups and individuals, members of particular industries and those involved in specific subject fields, campus departments, administrative units and

special groups, as well as grants and private gifts. Some examples follow.

Professor Walton Bean, with the aid of Verne A. Stadtman, Centennial Editor, conducted a number of significant oral history memoirs in cooperation with the University's Centennial History Project (1968). More recently, the Women's Faculty Club supported a series on the club and its members in order to preserve insights into the role of women in the faculty, in research areas, and in administrative fields. Guided by Richard Erickson, the Alumni Association has supported a variety of interviews, including those with Ida Sproul, wife of the President; athletic coaches Clint Evans and Brutus Hamilton; and alumnus Jean Carter Witter.

The California Wine Industry Series reached to the University campus by featuring Professors Maynard A. Amerine and William V. Cruess, among others. Regent Elinor Heller was interviewed in the series on California Women Political Leaders, with support from the National Endowment for the Humanities; her oral history included an extensive discussion of her years with the University through interviews funded by her family's gift to the University.

On campus, the Friends of the East Asiatic Library and the UC Berkeley Foundation supported the memoir of Elizabeth Huff, the library's founder; the Water Resources Center provided for the interviews of Professors Percy H. McGaughey, Sidney T. Harding, and Wilfred Langelier. Their own academic units and friends joined to contribute for such memoirists as Dean Ewald T. Grether, Business Administration; Professor Garff Wilson, Public Ceremonies; Regents' Secretary Marjorie Woolman; and Dean Morrough P. O'Brien, Engineering.

As the class gift on their 50th Anniversary, the Class of 1931 endowed an oral history series titled "The University of California, Source of Community Leaders." These interviews will reflect President Sproul's vision by encompassing leadership both state- and nationwide, as well as in special fields, and will include memoirists from the University's alumni, faculty members and administrators. The first oral history focused on President Sproul himself. Interviews with 34 key individuals dealt with his career from student years in the early 1900s through his term as the University's 11th President, from 1930-1958.

More recently, University President David Pierpont Gardner has shown his interest in and support for oral histories, as a result of his own views and in harmony with President Sproul's original intent. The University History memoirs continue to document the life of the University and to link its community more closely -- Regents, alumni, faculty, staff members, and students. Through these oral history interviews, the University keeps its own history alive, along with the flavor of irreplaceable personal memories, experiences, and perceptions.

A full list of completed memoirs and those in process in the series is included in this volume.

The Regional Oral History Office is under the the direction of Willa K. Baum and under the administrative supervision of The Bancroft Library.

9 November 1987
Regional Oral History Office
University of California
Berkeley, California

Harriet Nathan, Series Director
University History Series

Willa K. Baum, Division Head
Regional Oral History Office

INTRODUCTION--by Walter D. Knight

I first came to Berkeley in July 1950, arriving in the Physics Department office shortly before lunch to meet Carl Helmholz, who was giving dictation to the anchor lady, Miss Young. Carl was serving as the summer chairman, while Professor Birge was on holiday. Carl then took me to the Faculty Club, where we lunched with Luis Alvarez and Ed McMillan. It all seemed natural and easy, and made a novice faculty member feel comfortable at home in a new environment which was in fact to be home for forty-plus years. That day foreshadowed many years of observing Carl Helmholz working for the good of the Physics Department and of the University. Not long after there was a department party at Chez Helmholz, which was then on Beloit Avenue in Kensington. That was the first of many such family-type gatherings there and later at Betty and Carl's new home on Crest Road in Lafayette, all of which added importantly to the enduring cohesiveness among the members of the Department.

Carl served the Academic Senate in a variety of ways, which are doubtless discussed later in this Helmholz history. Notable among these was the Senate Committee on University and Faculty Welfare. When I first heard of this, on top of all the other committees in the structure, I wondered what or why it could be. As the years passed it became clear that such a committee had its essential place in a large institution, and Carl made sure that the committee looked after matters affecting many aspects of faculty life and working conditions. The culmination of his efforts in this respect was his contribution to the establishment of the Berkeley Emeriti Association of retired professors, of which he was the first president. The citation list of his committee service is endless, but aside from the length of the list the important fact is his perpetual willingness to serve the good of the order.

While faculty meetings tend in general toward microscopic examination of even the simplest problem, Carl's style is to come to the point in a minimum of time and words, without, however, neglecting to remind his colleagues of the relevant background. His manner is direct and positive, and his words are edged with a tone which cuts without rasping. When asked a historical question he throws his head back and slightly to one side, saying "we--ell," followed by a statement accentuated by pursed lips and a recollecting smile. His disagreement during an argument is temperate, urgent if necessary, but seldom angry, and never personal. He walks lightly and purposefully, as a tennis player approaches the baseline for a serve. He plays tennis vigorously, competitively, and properly, exclaiming, "Nice shot, Babe!" when Betty makes a point. Until recently he was one of the strongest supporters and vigorous players in the faculty-student softball game at the annual Physics picnic.

Carl's chairmanship of the Physics Department (1955-1962) fell in the relatively quiet period between the Loyalty Oath and the Free Speech Movement. This, along with Carl's calm and self-assured style, accounts for the fact that the Physics Department academic staff grew steadily and the department ran smoothly. Proposals for new appointments are traditionally initiated by the faculty and forwarded to the dean and faculty Committee on Budget and Interdepartmental Relations. The chairman gathers the evidence and makes the primary arguments in favor of the appointment. Carl settled easily into the chair, with generous and attentive assistance from Raymond Birge, the previous chairman, who had enjoyed an outer office for ordinary business and interviews, as well as an "inner sanctum" for private study, writing, and high level or confidential meetings. When Carl assumed the chair, Birge took up quarters in the "inner sanctum," where he could generally hear what went on in the new chairman's office. Although Carl has minimized the aggravations attendant on this close association of the new and old chairs, there were surely trying moments. At one of these I was sitting in Carl's office when the phone rang. Following a couple of sentences which quickly revealed the identity of the caller and his business, Birge hustled out of the inner office and whispered loudly, "No, don't tell him <u>that</u> yet!"

We see somewhat less of Carl since he retired, but he remains in touch with what goes on in the department and around campus, between sojourns in their houses in Lafayette (scene of my retirement party in May 1990), Alpine Meadows (hiking, bird watching, and great country cooking), Echo Lake (swimming, boating, and climbing), and Alexander Valley (at once the cynosure and valley lookout with the hemispherical view), where he and Betty scored again for the good of the order, hosting this year's retirement party for Kinsey Anderson, Geoff Chew, and Bob Tripp. Carl has done many important things at the University of California, but none more important than contributing in a large way to a social cohesion which wonderfully supports the scholarly activities of his colleagues.

Walter D. Knight
Professor Emeritus of Physics

May 1991
Berkeley, California

INTRODUCTION--by Henry J. Vaux

Carl Helmholz's contributions to the Department of Physics as a teacher, researcher, and administrator have been matched in both quality and extent with service to the campuswide and statewide faculties. From senior colleagues in his department such as Professors Birge and Brode, he gained at an early date a strong sense of the importance of the faculty's role in the University's governance. He understood the necessity of active faculty participation in the affairs of the Academic Senate if that role was to be maintained and strengthened. From about 1950 on he gave much time and energy to participation both in the senate and in related faculty activities.

The almost unique importance of the Berkeley faculty in academic decision making is generally recognized by many. But relatively few people have an in-depth understanding of how such governance is carried out. To anyone with an interest in these matters, Professor Helmholz's interview provides an informative source, not only on the structure and operation of the various institutions which enable the corporate faculty to exercise its responsibilities, but also on many important events of the entire post-World War II history of the faculty. In addition, there is much to be found here which illuminates the flavor of this aspect of faculty life.

Carl Helmholz's participation in faculty affairs displays two somewhat different characteristics of his own personality. One is a highly practical and essential one--the ability to find the common ground on which conflicting views on important issues within the faculty could be constructively accommodated. His always temperate and judicious remarks on the senate floor and his work on innumerable senate committees contributed to this end. In many instances the language of newly adopted senate rules, resolutions, reports, and other tangible senate actions resulted from Carl's thoughtful, imaginative, and sensitive treatment of matters on which the faculty was initially sharply divided.

Controversies over the Loyalty Oath, the Free Speech Movement, university operations at Livermore and Los Alamos, Ethnic Studies, and a number of other concerns for matters related to faculty rights, equal opportunity, and civil liberties are recounted here from the special perspective and vantage point of major senate committees such as Academic Freedom, Faculty Welfare, or Educational Policy, on which Carl served as a member or chair. His interview gives an intimate view of how these controversial matters were dealt with by the Academic Senate. He knew and interacted with most of the members of the faculty who were major participants in the debates over these issues. His judgements about the forces and influences that had to be resolved in each case are both measured and insightful.

Helmholz's experience of over forty years in campus and statewide faculty affairs covered not only the headline items such as those involving academic freedom and student concerns but also many issues which, though of little interest to the general public, provide the nuts and bolts that, for almost eighty years, have held together the system of a strong faculty voice in the governance of the University--for example, matters having to do with student admission standards, curriculum and research review, the regents' retirement system, and (particularly since his retirement) with emeriti concerns, organization of the Emeriti Association, and the naming of campus buildings. It is characteristic of him that his work on these less newsworthy but still vital projects has been as meticulous and as dedicated to faculty welfare as that on more widely noted matters.

Helmholz has an unusual capacity to make and retain friends across the whole academic spectrum. This important feature of his personality has provided part of the glue essential to holding the senate together. He has known senate members and understood their points of view to a degree well beyond what most of us ever achieved.

The Academic Senate is no exception to the generalization that institutional memories, though difficult to maintain and too often of short duration, are essential if the strength of the institution is to survive over long periods of time. Professor Helmholz's interview represents, among other things, a significant contribution to the senate's institutional memory.

Carl sometimes alludes in a humorous vein to the fact that certain issues seem to be virtually permanent items on the agendas of the senate or one of its committees. His interview shows that the reason such items never go away is evolutionary change in the problems themselves, rather than any inability to come to grips with reality. This sort of reinforcement of the institutional memory is important, not just as history, but as valuable data to illuminate and inform the on-going process of faculty decision making.

All of those concerned to maintain the strength of faculty governance in the University will find much of importance to them in the following pages. They must be grateful to Carl both for his years of dedicated service in the broad arena of faculty affairs and for the insights which his interview provides on the nature of the senate as an institution.

Henry J. Vaux
Professor Emeritus of Forestry

May 15, 1991
Berkeley, California

INTERVIEW HISTORY--A. Carl Helmholz

The idea for an oral history with A. Carl Helmholz originated during the 1987 oral history interview of Henry J. Vaux, dean emeritus of the University's School of Forestry. In discussing his own service on various faculty senate committees, Vaux noted the contributions of Helmholz, who had served on "virtually every senate committee that there is." He encouraged the Regional Oral History Office to "get on Carl's trail" and obtain the recollections of this behind-the-scenes facilitator in the Academic Senate and former chair of the Department of Physics (1955-1962).

A review of Professor Helmholz's university career indeed demonstrated an impressive record of service since his appointment as lecturer in physics during World War II. In the Academic Senate he was most active on committees on faculty welfare, educational policy, and academic freedom. In the Department of Physics, he was again an outstanding contributor to faculty governance and had an impressive record of concern with educational quality, at the high school as well as the university levels. Moreover, his involvement with the Radiation Laboratory (now the Lawrence Berkeley Laboratory) since his arrival in Berkeley in 1937 was of historical interest.

Professor Helmholz was willing to devote the time to the oral history process in part because he was interested in updating Raymond T. Birge's written history of the Physics Department, which documented the period to 1950. To facilitate his work on this history, we agreed to take a broad and detailed look at the department, including faculty hiring and promotion, undergraduate and graduate curriculum, and relationships with the labs, campus administration, and other UC campuses.

In the course of research in the University Archives, whose records for the Department of Physics cover the years 1920-1962, we discovered the existence of an extensive oral history interview undertaken with Professor Helmholz in 1975 for The Bancroft Library's History of Science and Technology Project. The interviewer, Graham Hale, was the assistant coordinator of project with a doctorate in Atomic Physics from University College, London University. The 1975 interview discussed Helmholz's family background, education, graduate studies at Berkeley, and wartime research and provided an insider's look at the Rad Lab and its personnel during those early years. It was agreed that Helmholz would re-review the transcript of this series of interviews and we would incorporate it into our oral history. It became the first four chapters of the following document.

Our interviewing began with some supplementary material on the prewar, wartime, and postwar years; covered the Department of Physics in some detail; gave an overview of the system of faculty governance at the University of California, with specific examples of faculty committee work during the turbulent years of 1960s and 1970s; and discussed Helmholz's contributions over forty years to issues of faculty welfare, including the retirement system and emeriti affairs.

One distinguishing quality of Professor Helmholz's account of his more than fifty years at Berkeley is his sensitivity to the web of social relationships that binds a university community. During a discussion of the Faculty Club and the faculty wives' Section Club, he suggested that I should meet with his wife, Betty, for a better view of the social side of the University community. An informal interview with Mrs. Helmholz on January 23, 1992, revealed a woman who, in addition to fulfilling her role as homemaker and mother of four, has a record of varied and extensive service to the university and community. A synopsis of the interview and record of Mrs. Helmholz's many activities is appended to the oral history.

The eight interviews conducted for the History of Science and Technology Project took place from June 20, 1975, to November 11, 1975. The eleven sessions conducted by this interviewer began on October 3, 1989, and were completed on January 16, 1990. We met each time in Professor Helmholz's office on the third floor of LeConte Hall. The transcript of the taped interviews was lightly edited and then reviewed by Professor Helmholz who made only minor changes. Tapes of the interviews are available in The Bancroft Library.

Ann Lage
Interviewer/editor

October 5, 1992
Regional Oral History Office
The Bancroft Library
University of California, Berkeley

Regional Oral History Office
Room 486 The Bancroft Library

University of California
Berkeley, California 94720

BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name August Carl Helmholz

Date of birth May 24, 1915 Birthplace Evanston, Illinois

Father's full name Henry Frederic Helmholz

Occupation Pediatrician Birthplace Chicago, Illinois

Mother's full name Isabel Gray Lindsay Helmholz

Occupation — Birthplace Milwaukee, Wisconsin

Your spouse Elizabeth Jane Little Helmholz

Your children Charlotte Chaffee-King, George Lindsay H., Frederic Vogel H, Edith Little RoTh

Where did you grow up? Rochester, Minnesota

Present community Lafeyette

Education Shattuck School (Faribault MN), Harvard College AB 1936 Cambridge, England 1936-37, Univ. of Calif. Berkeley PhD Physics 1940

Occupation(s) Physicist - Professor

Areas of expertise Nuclear physics, high energy physics, Physics education

Other interests or activities University affairs, grape growing and wine making, emeriti affairs.

Organizations in which you are active CUCEA, Bohemian Club, Berkeley-Albany YMCA, Council of Univ. of Calif. Emeriti Associations

I FAMILY, YOUTH, AND EDUCATION

[History of Science and Technology Program Interview 1: June 20, 1975]##[1]

Father's Medical Career, Chicago and Mayo Clinic

Hale: Professor Helmholz, you were born in Evanston, Illinois, on May the 24th, 1915, to Henry F. and Isabel G. Helmholz?

Helmholz: She was Isabel Gray Lindsay and when she married she went by Isabel Lindsay Helmholz. My father had been a medical student Johns Hopkins University and got his M.D. degree in 1906. He spent a year of internship in Boston. He and my mother were married in 1907. They went immediately to Berlin, Germany, where he studied pediatrics with what at that time were just about the only pediatricians in Germany.

He came back to the Chicago area in 1909. He was born and had grown up in Milwaukee, and he decided that he didn't want to practice in Milwaukee even though a number of his family had suggested that he do that. So he had moved to Chicago. My older brothers were born while they were living in Chicago, but sometime around 1913 they moved to Evanston. That was where I was born. I was born at home. It was the style at the time for a few years along in 1915 that children be born at home.

My father had a practice in Evanston, and also in the neighboring communities. He was involved in some work at Rush Medical College, so he would go into Chicago quite often. My father kept up his practice there toward the end of World War I. I think it was really in the flu epidemic of 1918, or 1919. He got quite ill and came close to dying. It left him with a lame knee; not terribly lame, but as kids we used to ask him which knee was his bad knee.

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[1]This symbol (##) indicates that a tape or a segment of a tape has begun or ended. For a guide to the tapes see page 379.

He decided at the advice of his physician that he'd better move away from a private practice into something which was a little less physically strenuous. So, in 1920, he accepted a position as the head of the department of pediatrics at the Mayo Clinic in Rochester, Minnesota. At Christmastime in 1920, our whole family moved to Rochester. This was the first time the Mayo Clinic had really had a pediatrics department. Previous to that time, other doctors would take care of children. He remained head of the pediatrics department until his retirement in 1947.

I was just in first grade when we moved to Rochester, and I attended school through ninth grade there. Then I went to a private school called Shattuck School in Faribault, Minnesota. It's about sixty miles away. My brother, my next older brother, had gone there before me. I was there for three years and graduated from there in 1932.

Hale: Your father was on the faculty, in fact, of Rush Medical College?

Helmholz: Yes, but he was officially what we nowadays would call a clinical professor. In other words, he didn't spend all his time there teaching medical students. He was there for the clinical training of some of the students. I never have known exactly how much time he spent there. I have an older brother who's a doctor from whom I can find that out. But he did have a fairly substantial private practice in Evanston and that area.

Hale: Your mother was from Milwaukee?

Helmholz: Yes.

Hale: What was her background?

Helmholz: Well, she came from a Scotch family by the name of Lindsay. Her father and mother lived in Milwaukee throughout all her life up to her marriage in 1907. The family lived there for many years afterwards. My grandfather was one of five brothers who ran a sort of a farm supply business called Lindsay Brothers, which still exists in Milwaukee now. And he spent essentially all his lifetime working in that business. My mother was born in 1886. Went to school in Milwaukee and then went to Smith College. She graduated from Smith College in 1907 and then was married the following Christmastime. I think Mother and Father were married on December 30th.

Hale: What generation Americans were both your father and your mother?

Helmholz: They were both born in this country. My grandfather on my father's side was born in Germany but had come to this country when he was, I think, two or three years old. My grandmother on my father's side was actually born in Milwaukee, although her father had come from Germany. Both my grandmother and my grandfather on my mother's side were born in this country. My grandfather's father had been born in Scotland, and I think one has to go one more generation back to find the person who was born in Scotland on my grandmother's side. So, my father and mother were really second or third generation Americans.

Hale: How old were you when you left Evanston?

Helmholz: We moved to Rochester, Minnesota, when I was five and three-quarters. I was born in May and we moved in January.

Hale: How much do you remember of living in Evanston?

Helmholz: Not very much. I can remember our house, and I can remember that I went to a Montessori kindergarten and started in a Montessori first grade school. I can't remember very much else. It's sort of difficult--I've heard my brothers and my mother and father talk about having lived in Evanston, and sometimes I can't remember what I remember myself and what they've told me. Oh, I do remember the sort of look of the street. It was a well-to-do neighborhood in Evanston. I can remember in a sort of vague way that I had some friends around my own age. I have a younger sister. I don't remember anything of her birth in 1917.

Family Life and Values

Hale: How would you characterize your upbringing, you know, I mean classwise, intellectually, financially?

Helmholz: I would characterize it as an upbringing typical of the professional class or the upper middle class. My mother and father were certainly well-off. When they came to Rochester, we lived in a house that they rented for about two years. But they immediately planned to build a house themselves, and they did so. They had an architect, an old sort of family friend from Milwaukee.

I remember some years later my father telling me that they built that house partly on the basis of income from the Fister and Vogel Leather Company. His grandmother was a Vogel and his

grandfather worked in the company, and they would continue to pay dividends. But about the time the house was built, they stopped paying dividends and my father had to borrow money in fairly considerable sums to pay for the house. We certainly never had financial problems at all.

My mother and father were both, I would say, intellectual people. My father was the only one in his family who took on a profession. I think, in the memory of him and my grandmother and grandfather, there had not been anybody who had been in a profession before. He really struck out on his own. He was, I think, quite an intellectually superior person relative to the other members of his own family. He had decided on medicine as a career when he went to the University of Wisconsin. I think he went in 1899, and he graduated from the University of Wisconsin in three years by taking extra courses in the summer. Then he went to Johns Hopkins for his medical training. He was one of their really distinguished pediatricians and remained a distinguished pediatrician throughout his life.

My mother came from a good Scotch family. She had a good many intellectual interests and went to Smith College. She had two sisters and one brother, and they all stayed in Milwaukee, Wisconsin. The two sisters married businessmen in Milwaukee, and her brother went into business in Milwaukee. I certainly remember my grandparents and my uncles and aunts with a great deal of pleasure. I think from the two families, my father was certainly the most distinguished of his brothers. He had two brothers and one sister. My mother was probably the most distinguished of her two sisters and one brother, so I came by what brains I have from a good source.

Hale: Was your childhood generally a happy one, pleasant one?

Helmholz: Yes. I would say very pleasant. My mother and father and my brothers and my sister always remind me of the fact that when I was growing up, I had a terrible temper. When I played tennis, when I wasn't winning, I would throw my racquet around, and I showed this temper in a great many other ways. Somehow or other, when I was about twelve or thirteen years old, my father tells me, I stopped getting so angry. At that time my anger didn't have any particular focus. I was just mad at one thing or another, and since then I think I've taken out my anger on myself, when I do get angry.

But it certainly was a very pleasant upbringing that I had. We used to go to Milwaukee always at Christmastime and had Christmas with my mother's and father's families. During the summer I often went to a place outside of Milwaukee where my

grandmother and grandfather Helmholz had a house on a lake. I would spend two, three weeks during the summer swimming and sailing, having a generally good time as kids of that age do.

My brothers and my sister were all well educated. My oldest brother, whose name was Lindsay, went in Milwaukee to a private school called the Milwaukee Country Day School for four years and then went to Cornell for two years. Perhaps his history will give you some general idea of the interests in the family. My father was always very concerned that we adopt some sort of business or profession. He didn't have any particular suggestion as to what it should be, but something that we really wanted to do. When my first brother decided after two years at Cornell that he was interested in chemistry, he wanted to get into graduate school. Johns Hopkins had a system at that time of letting undergraduates start in graduate work after two years of college. So he went to Johns Hopkins without graduating from Cornell. He pursued his graduate work in Johns Hopkins and got a Ph.D. in chemistry, I think in 1933.

My second brother went to Dartmouth College and then went to Johns Hopkins Medical School, following in my father's footsteps. My sister graduated from Rochester High School, and she spent a year at a private school outside of Philadelphia called the Baldwin School--that's also the school my wife went to--and then went to Smith for four years and graduated in 1938. My mother and father were very much interested that we should get as good an education as we possibly could. There was no emphasis on my father's part in our following in the medical profession or in any particular profession. Whatever we wanted to do he thought was fine.

Early Influences and Interest in Sports

Hale: Can you think of other people that, during your childhood, had a major effect on you other than your parents?

Helmholz: Not really. Of course, my two brothers, being older than I, had some influence. I tended to follow along with things that they did that I thought well of. Certainly my two grandfathers and my two grandmothers, I felt, were good people. I'm sure they had in a somewhat indirect way an influence on me, but nothing sort of specific that I could point to. Certainly my mother and my father, particularly my two brothers, were influential. My sister was younger; we always kind of made fun of her. I think she probably had less influence than they did.

Hale: What were your boyhood interests?

Helmholz: I had a great interest in tennis. My father was a very good tennis player; he'd won state championships in Wisconsin and Illinois. When he and my mother built a house in Rochester, Minnesota, they built a tennis court on the property. I suspect there was another tennis court in Rochester, but none that was ever played on. So, he started to introduce tennis. This knee I referred to didn't interfere very seriously with his tennis. He started us playing at a very early age. I spent a lot of time during the summers playing tennis.

When I was about twelve years old, I think I even played golf for a little bit with some friends of mine, but that lasted for about two years. I kept at the tennis for a number of years. I can remember that, I think, when I was ten years old, my father arranged that the local newspaper would have a tournament, and since our court was just about the only court in town, the whole tournament had to be played on one court. Of course, there weren't very many entries, but I won some division in that tournament. They must have had a division for eleven and under or something like that. As years went on, I played a good deal. When I was thirteen, I played in Milwaukee in some tournaments and again when I was fourteen, which was 1929. In 1930, when I was fifteen, I played in the national boys' championship, which was at Culver; it's a school there in Indiana. That was sort of the peak of my tennis career; I was the sixth-ranked in the nationals of the boys fifteen and under. There was only one really good player in the first five. He was second; that was Frankie Parker, who was quite a well-known player later on. My two brothers and my father and I used to play doubles, and we had a great time. That's certainly one of the happy memories of my tennis playing.

My next older brother, the one who became a doctor, liked to swim and swam quite well. Sort of following in his footsteps, when I got into high school, I did some swimming. When I went away to Shattuck School, I played tennis in the fall and in the spring and swam in the winter. I never was a terribly good swimmer, although I did swim on the team there. I don't recall any sort of other great interests that I had. I enjoyed all sorts of athletics, and I did, I suppose, an average amount of work around the house for a person whose parents were well-off. I would mow the lawn, and I spent more time taking care of the tennis court than anything else.

Other Boyhood Interests and Education at Shattuck School

Hale: You didn't have anything mechanical that you fiddled with? Toys of that sort?

Helmholz: I really didn't. I suspect that I got into physics more from the intellectual interest of it than from a bent in the physical or electronics direction. I've never been much on electronics. And, while I sort of enjoy working with machines, I've never made it a point to get machines or to work with machines.

Hale: You ever fiddle with automobiles, for example?

Helmholz: No, I never have. I have a son who became quite an expert about automobiles, but I never did.

Hale: Something that seems to be common among engineers and physicists is having a Meccano set when they were kids.

Helmholz: I'm sure that we had one, but I never particularly played with it. I think the toys that I played with were sort of the ordinary toys. I used to enjoy playing with toy soldiers and at one time had quite a collection, several hundred. One year I was given a little cannon that was of the appropriate size for these toy soldiers, and I got to know how that worked, all right. I wasn't terribly interested in the mechanics of it.

Hale: Never had a chemistry set?

Helmholz: I occasionally would visit my father's office. He had quite a strong interest in research and did a good deal of research along medical lines. He and an associate of his used to work at the place we called at that time the Dog Farm. It was a laboratory outside of Rochester, Minnesota, where they kept a lot of animals for research. While I was there a few times, I really don't have any strong feeling for the kind of research he was doing and never took part in that. I didn't really get interested in science except in a sort of a very general way until I went to Shattuck School. There I took chemistry in my senior year.

Hale: How old were you?

Helmholz: Sixteen or seventeen. It was the only science course I took in high school; things were rather different then. I took four years of Latin and three years of French, four years of mathematics, a year of history, four years of English.

Hale: Shattuck was a private school?

Helmholz: It's a private military school. I can't remember at all whether I thought of taking chemistry in my junior year and physics in my senior year, but I didn't do it. I was very much interested in chemistry and did well in chemistry. Then, when I went on to college, I really had in mind that I might want to go to medical school, so I started physics in my first year there and became interested in physics. By the end of my freshman year at Harvard, I decided to major in physics. I had not given up the idea of going to medical school, but I decided that a major in physics would be a good thing. I would say that my school work was rather conventional for the time. At least in high school, I was quite a good student. When I was in Shattuck School, I was one of the top two in the school for the years that I was there. The other fellow I still see often. We occasionally talk about the fact that we have our names up on a board there.

So, my schooling didn't particularly indicate that I would eventually decide on going into physics. It certainly wasn't anti-science, but it wasn't particularly pro-science. I suspect that that's partly the school system of the time; there wasn't a lot of science in school. Nor is there much more science in schools nowadays. But people in those days took Latin and languages, and mathematics and history and English, things like that.

Hale: Was there any mechanical training, woodwork and so on?

Helmholz: I never took any such mechanical training. I remember when I was in high school that some of my friends took woodshop. I have since regretted it, because I have never been terribly good with machine tools. I've learned to do the things that physicists have to do, but I always say that the only thing that I could ever machine with great ease was carbon. We had to do that during the war at times. That's soft enough so you don't have to worry about having to tool it exactly at the right angle, and so on.

Hale: Did you read boys' magazines or popular magazines?

Helmholz: Not a great deal. I think I probably did as much as most of my friends, but not particularly. When I was of the order of ten or eleven years old, I tried to make some money by selling magazines, and I tended to sell the ones that my father would recommend to me because I would try to sell them to his friends.

I certainly read Popular Mechanics and things like that. But I wasn't particularly interested in building things that

were illustrated there. Occasionally, I would build something, but that certainly wasn't a major part in my interests. I once collected stamps for a while. I learned to play bridge at a fairly early age, and I remember I had several friends who also liked to play bridge. We used to play bridge when I was about, I guess, twelve or thirteen years old. My mother liked to play bridge and was quite a good bridge player. My father would play with some objections.

Hale: Chess?

Helmholz: I played chess a little; I never got very good. I still can play a game, but not very well.

I can remember that one time there was quite a good chess player who came to the Mayo Clinic. Several of the doctors who played chess knew that he was there, and so they asked him to put on a performance one night. They were looking for people who would play and so they got me in. I forget how old I was at that time. I guess I could have been seventeen or so. He played twenty-six games or something like that, and I remember that he played some twelve moves with me, and he more or less said after the twelfth move, "Well, the game is over." I couldn't see it really. But he could.

Hale: What do you remember of your peers while you were a boy?

Helmholz: Well, I had several quite good friends who were just my age and in the same grade that I was. There were really four of us that were quite close. We would play football in the fall, we would play basketball, and a couple of them played tennis with me. One of them swam with me and was actually a good deal better swimmer than I was. They were sons of doctors. Rochester was a very heavily medical town, and I think at one time I figured there was a doctor for every fifty people in the town. They had rather similar interests in athletics. They were all fairly good students also. One of them liked to play bridge as I did. I have not seen a great deal of them in recent years. Of the four people that I would say I was fairly close to in those days, three of them became M.D.'s and one of them went into the travel business. He worked for TWA for many years and, I think, still does in New York now. I don't see a lot of them--they have moved to different places--but I sort of expected that most of my close friends would become doctors. I even thought of becoming a doctor myself for a good many years.

Hale: Were you, in fact, sort of generally gregarious, or did you prefer to be alone and study?

Helmholz: It's hard. I think I'm not gregarious in the way that I think of gregarious people. I liked to study alone. I'm not anti-social. My brothers used to accuse me of being a woman-hater when I was growing up, but I don't think I particularly was. They just made a point of this to tease me. I wouldn't say I was really gregarious, but I'm certainly not in any sense a loner in the way that we think of it nowadays.

Hale: Did you see your early education as relatively boring, or did you think you got much out of it?

Helmholz: I felt I got quite a bit out of it. I don't think I was ever bored in school. I remember that my father and my mother were anxious that all of us in the family had a chance to go to private school. They felt, with some justification, I think, that the public schools in Rochester were certainly good but were not as good as the private schools. They never, to my knowledge, thought of sending any of us back east to Exeter or Andover or one of those schools. The times were not that good. It was 1929 when I went away to school. I felt that I got a lot out of the private school. I think I always enjoyed school. I always considered it as a challenge, and I obviously enjoyed learning. I always did quite well in high school.

Teachers and Youthful Goals

Hale: Do you remember any particular teachers that had an influence on you? I mean, over the whole time?

Helmholz: Yes, I can remember a fair number of my teachers when I was in grade school. We had a very strict teacher who we kind of used to laugh at, but she was very good. The ones that I can remember best, I think, were the teachers that I had at Shattuck School. I had a teacher in Latin who was also the teacher who lived in our dormitory. I got to know him pretty well; he was a somewhat unusual man who really felt very strongly about Latin as a subject and enjoyed it, and he gave a very good course. I took Cicero and Virgil, and there were only, I think, six of us who took Cicero and only four of us who took Virgil.

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I had a very good English teacher in senior year who was also the teacher who had charge of dramatics. I took part in dramatics. A French teacher who was also the tennis coach, and so I obviously knew him quite well, since I played tennis when I

was at school. And, finally, a mathematics teacher who was, I felt, a very good mathematics teacher. He was a very particular person, as I think was appropriate to mathematics, but he did a lot to make mathematics very interesting. I've always been very fond of mathematics and mainly, I think, on his account. I suppose mathematics has always interested me. He was the one that I remember, more even than the mathematics teachers I had in college, as somebody who made the subject come alive.

Hale: He had quite an influence on you?

Helmholz: Yes, I suspect that he did.

The chemistry teacher I had was also good. He was a slightly eccentric person but was enthusiastic about chemistry, and so I got a lot of interest in the physical sciences, or a starting interest in the physical sciences, through him.

Hale: Did you get no science at all up until that time?

Helmholz: Yes, that's right. I didn't get any science. Schooling was very much different from what it is now, when you start taking general science in seventh grade and eighth grade. We just never had science like that. Of course, I was exposed to a lot of discussion of medicine at home, and so I had seen some aspects of science in that respect. My father had lots of medical books in his library, and I would occasionally take them down and look at them.

Hale: Was the possibility of becoming a doctor a strong desire?

Helmholz: No. I had a feeling that I would probably like to go into a profession of some sort. Perhaps because of my liking for mathematics, or at least arithmetic, and the fact that I was sort of Scotch with money, my father once suggested to me that I ought to be a banker. I think he said, "We could do well with a banker in the family." And I actually considered that as a possibility for a number of years. In fact, when I was in college, even when I had become very much attracted to physics, I still thought that I might go into something like banking or business.

I don't think I had, in those times before I got into college, any idea as to whether I'd ever go to graduate school or not. I did feel quite strongly always that I would go through college. I never thought of not going through college, but I didn't have any definite ideas as to what I would do after college except this recurring theme of medicine, which was natural because I grew up in that medical community.

Hale: When did you first understand about physics as a discipline?

Helmholz: I knew that there was a physics course given at this school that I went to, and I knew some of my classmates were taking the physics course, but I really didn't pay a lot of attention to it. I had lots of activities. I spent my time on my own studies and didn't really look into the studies that other students were taking, except insofar as my roommate was taking other classes.

Studying Physics at Harvard, 1932-1936

Helmholz: It was really when I got to college that I began to think about physics. In my first year at college, I decided to take a course meant for biology students and premedical students, and it was a non-calculus course in physics. It was a poor course to take, given by a man named Newton Black who, in those days, wrote textbooks.

Hale: It's like a general physics course?

Helmholz: It was a general physics course, a one-year course with laboratory. The laboratory wasn't particularly challenging, but Newton Black was an interesting person. He gave quite good lectures, although rather dry. He was a person who certainly never became a full professor; I've forgotten now whether he ever became associate professor at Harvard. He did make it interesting, and I think that I probably was just ready for physics at that time.

I would have learned a lot more physics if I had taken the more advanced course. There was an advanced course for students who had taken high school physics, which was a very good course, and I didn't even think of taking that. If I had gotten an advisor when I went to Harvard who had known about physics, I think he might have then suggested that I take this course because I had done well in chemistry. I was taking the mathematics that was appropriate to that course. But nobody ever really suggested that to me, and when I got started I didn't even realize it until about the second semester. Then I was thinking of majoring in physics so that was, I suspect, unfortunate. I would have had a difficult time in that course, but I think that I would have learned a good deal more. Anyway, during that year, I realized that physics was a discipline. Harvard had a good physics department at that time. Toward the end of that year, I went to a few of the seminars--or general

topic meetings--that they had just to try to find out a little bit about what it was like to major in physics. I talked to a number of the faculty; I got around and found that I liked the people. That year or the next year, I had quite a good lab instructor.

During that year I also took mathematics, I took an English course and a German course, and then, in the second semester, I took analytical chemistry, quantitative analysis. I did sort of look around some, but at that time one had to decide on a major at the end of the freshman year. I chose physics. I was the only one of the close friends that I had at Harvard who really was a physics major. I think there must have been fifteen or so of us who were physics majors, and I think I knew them all, but not very well. I roomed my first two years with a fellow who had been at Shattuck School with me. He left college after the first two years and didn't return. We lived in a dormitory with sort of the normal run of students around, but none of them happened to be physics majors. I never got to know the other physics majors very well.

We did have the system there of tutors who were members of the faculty with whom one would meet about once a week. I remember that my tutor in the first year was a man named Curry Street, who must be just about retiring now. He was one of the ones who was an early discoverer of the mu meson. There was an argument as to whether he and a coworker at Harvard named Stevenson--whether Stevenson and Street, or Anderson and Nedermeyer at Caltech, had been the real discoverers of the mu meson, having found them in cloud chambers.

I think he was quite influential in developing my interest in physics. At the time, for example, when I started in my second year, he and I decided that I would not take a course in mechanics, in Newtonian mechanics, because students took, in general, four courses. Then they had this tutorial work, and there wasn't time for all the regular undergraduate courses. So, he and I studied Newtonian mechanics together, and I must say I felt that I learned it pretty well. We would sit down and he'd ask me questions, and then he'd show me how to do certain kinds of proofs and so on and work certain kinds of problems. It was a good way for me; I was interested in the subject, and so I really learned quite a bit.

At that time, we also had an examination at the end of our senior year, and all students who were going to graduate in physics had to pass this examination. You had to study some in the major fields, which were mechanics, electricity and magnetism, thermodynamics--the thermodynamics was somewhat less

dealt with in this examination--and optics. Also, something about atomic and nuclear physics, although there wasn't much nuclear physics in those days. So, as I went through, in my second year I did take the course in optics which was, I think, a one-semester course and a course in electricity and magnetism. Then, in my last year, I took the atomic physics and a course that had some nuclear physics in it, some more electricity and magnetism.

Hale: Like a modern physics course?

Helmholz: Yes, something like that. In my last year, I wrote a thesis which was an attempt to illustrate or to list the methods of determining molecular structure. Partly through my brother, who was a physical chemist and who was interested in molecular structure, I got interested in it. My tutor in my last year was Kenneth Bainbridge, who was a mass spectroscopist. He was also an excellent tutor. I suggested that maybe I should write a thesis which I'm sure counted for a course or something like that.

Hale: You suggested to him?

Helmholz: I suggested to him that wouldn't it be interesting to write a thesis, and he said fine. I guess he probably would have suggested that I do something along the lines of nuclear physics or mass spectroscopy, but I had this idea about molecular structure, so he said, "Fine, go ahead." We worked out a kind of a program.

There were a number of people at the time in the department of chemistry who were interested in molecular structure, and then there was [John] Van Vleck who had written a number of books about electric and magnetic susceptibilities. I can remember a couple of times going and asking him questions. So I had a lot of good help when I was doing this work, and it was a thesis which, I suspect, I still could find in the Harvard physics library if I went back and looked. I did go back about five years later and found it was still there. It was just a discussion of the different methods of determining molecular structure and how they related, what each one would determine, so on.

Hale: It wasn't required to do a thesis?

Helmholz: No. I really have forgotten whether, if you didn't write a thesis, you were asked to take an examination over a somewhat more specialized subject. This was a written examination; I think a morning and an afternoon examination was required of all

students. It was required in just about all of what we called the major fields in those days.

Treatment for Diabetes at Mayo Clinic

Helmholz: My father used to send us all through the Mayo Clinic; he'd get us to go down and get a physical examination. When I was about to go back to my third year in college, they discovered that I had diabetes. That would have been in September 1934. My father immediately sent me to another doctor whose sons I knew quite well. He said, "Well, why don't we try something unusual in your case and take very careful care of your case of diabetes for a considerable length of time?" It was actually for the school year.

I didn't go back to Harvard that year; instead, I took some courses through the University of Minnesota, which was only eighty-five miles away in Minneapolis. I took a physics course up there--that was another kind of a course in atomic and nuclear physics, and I took a course in organic chemistry. I had credit from an extra course I had taken in my freshman year, and so I had enough credits to graduate. During that time, I spent a fair amount of time studying organic chemistry and physics. But I also spent some time working in the chemistry laboratory with the man who discovered cortisone; his name was Kendall. He was sort of chief research chemist at the Mayo Clinic at the time. He had quite a distinguished reputation, and afterwards he and a doctor there won the Nobel Prize for their discovery of cortisone.

Back in the early twenties, I think he had been involved in work with thyroxin. He had almost discovered the structure of it, but some British chemist, Harrington, I think, had found it just before he did. He was the one that really did a lot of the work on cortisone. I think he justly deserved the credit that he got. So, I worked in that laboratory. I can remember he was doing some work in which he was trying to follow the sodium and potassium levels in animals that had had the adrenal cortex removed. They were being given cortisone to try to keep them alive. I remember doing analysis for sodium and potassium in it.

And then I used to work with this doctor who was interested in my case. I would take my blood sugar every day and work in the laboratory, and so I learned to do blood sugar analysis. Then I remember that sometimes he and I would go in on the

weekend, and we would take our blood sugar four or five times during the morning just to see--he would compare his with mine to see whether I was getting along, whether my blood sugar was fairly normal.

It was an interesting year. I never had really thought about whether I would have done a lot better if I had gone back to Harvard and just taken care of my diabetes as it was then. I've been told by doctors since then that it was probably only because I was very careful that first year that my diabetes has not gotten a lot worse or that I haven't developed some of the normal complications of it. I stayed home all that year. My mother used to weigh out my food, things like that. It was a rather different year from what I would have had if I had gone back to Harvard.

More on Harvard and Development of Career Interests

Helmholz: I went back my last year and was a regular student. I had roomed with a fellow from Shattuck in my first two years; there was a third person in our suite. Rooming at Harvard was pretty good in those days. We had a sort of a living room and a bathroom, and each one of us had a separate bedroom. This third fellow was from the East who had never been out in the Middle West. His father was a Harvard graduate; my friend from Shattuck's father had been a Harvard graduate, too. He was a rather different type from us, having been born in Argentina and grown up in England and France where his father had been the representative of the Otis elevator company. Partly through him, we met a number of people in the Boston area, because he had a number of friends. We lived together the first two years. Then, during the third year when my friend from Shattuck didn't come back, he just lived alone. In my fourth year, I lived with him again. He was a very good tennis player, so we struck it off pretty well. He also played squash, which I learned to play when I was at Harvard.

Hale: Who or what directed you to attend Harvard originally?

Helmholz: Well, I suspect that it was my brother. I, of course, had known about Harvard when I began thinking about going to college. Harvard was certainly one of the possibilities. I wanted to go east to college, and since my father was able to afford it, I thought of Harvard. My second brother had gone to Dartmouth, and my older, first brother always sort of made fun of Dartmouth. I think he wanted to make fun of my other brother,

but he thought very well of Harvard as an undergraduate institution. My teachers at Shattuck also thought well of Harvard. I don't think any of them had ever been there, as undergraduates, but they all thought well of it. In Rochester, Minnesota, there was a surgeon named Cabot who came from the Cabot family of Boston, who had obviously been to Harvard. I can remember going to talk to him one time about Harvard. So I decided to go to Harvard. As a friend of mine, John Austin, later said it wasn't so terribly hard to get into Harvard. All you had to do was to have enough money to pay the bills, and you'd get in. But I certainly am glad that I went.

Hale: Were you much aware of the Depression, events in Europe, things like that?

Helmholz: I wasn't particularly aware of the Depression. I had read the newspapers, and my father's salary was cut at the time. I knew enough about it so that when I went to Harvard I applied for a scholarship. I won a small scholarship, I guess in those days it was $100 or something like that. It was a fair amount of money in those days. Rochester, Minnesota, wasn't terribly much affected by the Depression. I don't think I was nearly as severely affected by it as were lots of people who lived in bigger cities. While I was at Harvard, I would read the New York Times, and I got a lot more interested in what was happening in Europe. I think I was very well aware of what was happening in the outside world at that time. In the years '29-'32, I would say that I perhaps was less aware than lots of people of my age about the Depression. Then, from '32 on, when I was in college, I began to realize what was happening.

Hale: What were your grades like in college?

Helmholz: Oh, I did very well in college. I was a Phi Beta Kappa, and, partly because I wrote this thesis, I graduated magna cum laude. I suspect my grades were mostly A's and some B's.

Hale: And did you care much about your grades? I mean, you obviously must have been pleased with your A's.

Helmholz: Yes, quite a bit. From high school on, I paid a fair amount of attention to grades.

[Interview 2: June 27, 1975]##

Hale: Dr. Helmholz, you said that you had intended to go to medical school, even when you went up to Harvard. Was it, do you think, because of your father and your environment in Rochester? You said your father never pushed you into any particular career.

Helmholz: Yes, I think so. I was interested in medical problems. I think it did indicate that I was interested in science as a whole, but when I got to Harvard and got to studying physics, I decided that I preferred physics to medicine.

Hale: Your father was a pediatrician.

Helmholz: Yes. The Mayo Clinic was mainly run by Drs. William and Charles Mayo. They decided that there should be a department of pediatrics. And so he was asked to come as the head. I can't really remember whether there were two doctors he brought. There was a Dr. Samuel Anburg who had been associated with him in Evanston, and I suspect that when they started, they were the only two members of the department. It grew sort of slowly over the next twenty years so that I would guess there were perhaps five permanent staff members by 1940.

Hale: Did he at the same time become a member of the faculty at the University of Minnesota?

Helmholz: Yes. They had an arrangement which started, I think, sometime in the 1930s, by which the permanent staff of the Mayo Clinic were members of the faculty of the University of Minnesota. They served on examining committees, sometimes were the research directors for Ph.D. theses done in Rochester on medical subjects: physiology and anatomy and things like that. He would go to Minneapolis, as I remember, once or twice a month for one thing or another. I know that he was a close friend of a Dr. McQuary who was the head of pediatrics at the University of Minnesota medical school, and they saw each other quite often and discussed problems and things like that. I don't really know whether he ever had any Ph.D. students himself. I don't remember any, but it is possible. I'm sure he was on Ph.D. thesis committees, but I don't believe he was the research director for a Ph.D.

Hale: It was more rather in a professional capacity than in a teaching capacity or something like that.

Helmholz: Yes.

Hale: Now, did you have much contact with your father in his laboratory or office?

Helmholz: No, I really didn't. I suppose I got into his laboratory where he was doing some research work once a month or so. The year that I was trying to treat my diabetes, my third year at Harvard, I did work in the chemistry laboratory of Dr. Kendall. That was really my first real experience in a laboratory.

Hale: Your father, I gather, was a friend of Luis Alvarez's father, Dr. Walter Alvarez?

Helmholz: Yes, yes. They were both members of the staff of the Mayo Clinic, and I knew Luis Alvarez and his family. Luis is about the age of my next older brother, and they really knew each other somewhat better than I knew Luis. My sister knew Luis's younger sister quite well. Our family was not terribly close to the Alvarez family, but we certainly knew them. They lived about two and a half blocks away. We didn't see them often, or not even socially very much, but we certainly knew each other.

Hale: Did you know whether he had any interest in physics at that time?

Helmholz: No, I didn't know this until I was in college. I knew that Luis was a graduate student at Chicago, and I remember that when I came to Berkeley, I knew that Luis was out here. I forget now exactly how I learned, because I must have been in Europe. I think maybe my father found out. When I told him that I was going to go to the University of California in Berkeley, he probably mentioned this to Dr. Alvarez sometime. He knew that the whole Alvarez family had come from San Francisco. When I came out here the first time, I wrote to Luis and said that I was coming. I remember he met me at the train.

Hale: But, until then, you hadn't had that much to do with each other?

Helmholz: No, we had just known each other as friends, certainly not in much having to do with physics.

Lab Work at Harvard: Proton Diffraction

Hale: What laboratory work were you required to do at Harvard?

Helmholz: We were required to take three courses which had laboratory work. My freshman course, the introductory course, as I think I mentioned last time, was a rather low-level course and the laboratory work was low level also. In my second year, I took a two-semester course in electricity and magnetism which had the sort of standard experiments in it. Then there was one course in optics that was a one-semester course. That had a laboratory, also standard optical experiments, I really can't remember them very well. I remember a few of the electricity experiments, like the "silver voltameter," I guess they called it, and I remember we had some diffraction experiments in the

course in optics. They were, as I say, rather standard laboratory courses.

In my last year I didn't do any laboratory work. The work I was doing on my thesis on methods of determining molecular structure was purely a library work. After I graduated, while I was working on this thesis, I got the idea partly from studying electron diffraction that it would be interesting to try to diffract protons from molecules--to see whether there was anything more one could learn from proton diffraction than from electron diffraction. One of the problems about electron diffraction was that because the reflection is proportional to Z^2, one could not find much about hydrogen for which Z=1. While I don't think protons have been shown to do any better, that seemed like a possibility.

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As I mentioned, I was interested in the methods of determining molecular structure. I got interested in the possibility of using proton diffraction.

So, when I found that I was going to Cambridge the next year--I won a scholarship to Cambridge for the year after I graduated from Harvard--I started to work, partly at the suggestion of Kenneth Bainbridge, who was my tutor at Harvard, on an apparatus which might develop a beam of protons. These were low-energy protons, of the order of 100 to 200 volts, to be applied to proton diffraction. I spent about a month and a half in Cambridge, Massachusetts, after I graduated starting to build this, and then I just took it along with me.

When I got to Cambridge, I was under the tutelage of Mark [Marcus] Oliphant, and he found a place for me in the basement of the Cavendish Laboratory, in the part they used to call the "garage." I set up this apparatus, and I worked a fair amount during that next year. I was never able to get a good enough beam of protons to try this out. We were thinking of sending a beam of protons through a jet of molecules, then observing the diffraction rings on a film behind the jet. There was a man at Caltech who did succeed in diffracting protons from molecules. He succeeded in doing this--I've forgotten exactly when it was --just about the time that I was working on it.

When I came back to California, I went down to see my brother, who was at Caltech. He's a physical chemist, and he introduced me to this fellow whose name was Uri, and I talked to him some about it. So that was sort of my real beginning in

experimental physics, if you discount the time I spent in the chemistry laboratory in Rochester.

Hale: You didn't also have to have a lab, say, in your atomic and nuclear physics course?

Helmholz: There was a separate laboratory which I didn't take. It was optional, and I didn't feel I had time for that one.

Hale: Those would have been simple atomic experiments; Franck-Hertz and so on?

Helmholz: Yes, they were simple atomic experiments. I remember that one of the fellows I knew worked in that laboratory in his senior year, and he developed an experiment in which one could determine the constant in the black-body radiation formula from a kind of a black-body experiment. I've often thought in the last couple of years, since I've been teaching something like that in a course, I should go back and look that up. I think he wrote it up for one of the journals in those days.

Physics Education at Harvard

Hale: Now, what was the depth and the topical extent of the advanced courses? You know, for example, did you do any quantum mechanics?

Helmholz: No, there was no quantum mechanics in the undergraduate curriculum then. This was 1935. The atomic physics course, for example, went up through the Bohr atom, and I guess it probably mentioned quantum mechanics. But that was about all. It discussed nuclear physics up through the disintegration of the nucleus by Rutherford in 1919 and the sort of following experiments that Rutherford and his collaborators did. I'm sure that it mentioned the artificial disintegration of the nucleus as was done in 1932 and the discovery of the neutron. I wasn't particularly interested at that moment in nuclear physics. When I went to Cambridge I realized, of course, that they were doing a lot of nuclear physics there, and I heard a good deal about it from the other people who were working in the laboratory. But, since I started on this proton business, I thought I would try and continue it and finally just gave it up as being a little too hard.

Hale: But at Harvard, was there an identifiable departmental bias, say, towards either a phenomenological or a theoretical presentation in physics?

Helmholz: No, not really. There were some theorists and some experimentalists--relatively few theorists, though. Van Vleck was the chief theorist at Harvard then. There was a person named Wendell Furry who became a fairly well known theoretical physicist; my recollection is that he was just getting his degree about that time. In that era of physics, there was a greater preponderance of experimental physicists over theoretical physicists than there is now.

When I came here to Berkeley, for example, there was an older man whose name was Williams, who had been the theoretical physicist for many years until [Robert] Oppenheimer came. And he was not a terribly active theoretical physicist. When Oppenheimer came, he brought lots of students, but he was essentially the only theoretical physicist on the staff of twelve or so. All the others were experimentalists. I don't know how to classify Birge; he was concerned with the values of the physical constants. His work was sort of paperwork, at that time, but he had been a spectroscopist before that.

Hale: You still got a fairly balanced view between the phenomenological and the theoretical point of view?

Helmholz: Yes, I think I did.

Hale: You didn't mention specifically taking courses from Bainbridge, but I imagine you did.

Helmholz: Well, I did what we call tutorial work with him, which means I would go in and talk to him for an hour or thereabouts every week. And, of course, I knew about his work in mass spectroscopy. I think the year that I was a senior, Al Nier, who had made for himself a pretty good reputation in mass spectroscopy, came to work with Bainbridge.

Hale: He made the Nier source?

Helmholz: Yes. They were working mainly on the precise determination of atomic masses in the low part of the periodic table, from hydrogen up to neon or thereabouts. Nier himself was interested in the uranium question, and I think while he was at Harvard he did a lot of work determining the age of the earth by the lead content of uranium samples--the relation between U-235 and Pb-207 and U-238 and Pb-206. From each one of those, you can get a determination of the age from the time the uranium sample

was set down. You find out whether they agreed with each other. It was rather a new method.

Hale: And Bainbridge eventually recommended you to [Ernest] Lawrence, I gather?

Helmholz: Yes. I was recommended to Lawrence both by Bainbridge and by Oliphant, with whom I had been working in Cambridge. Bainbridge recommended that Berkeley was a very good school, and so did the people at Cambridge. Also, my present wife was a student at Stanford. It was a logical place to go.

Hale: Did we miss anybody else at Harvard that you might consider important?

Helmholz: No, I don't think so. I think I mentioned Curry Street and Ken Bainbridge; since they were my tutors, they were the most important. I did once or twice go and talk to Van Vleck who was, I thought, very impressive. But, it was a good physics faculty there--I can remember seeing Lyman walk around the halls.

There was a sort of an electronics expert whose name I ought to be able to remember. I remember going to a seminar that he gave once at which he described how he had measured the frequencies emitted by crickets. He was a short, stout man, and it amused everybody when he described how he carried all his equipment on his back and would crawl through the weeds listening to the crickets. It was a very, very pleasant and enjoyable time that I spent at Harvard.

Henry Fellowship to Cambridge, 1936-1937

Hale: Now, who or what directed you to Cambridge, specifically? I forget whether you mentioned that or not.

Helmholz: I don't think I mentioned that before. There was a fellowship available to Harvard and Yale students, even though it was open to other students, which was a little bit like a Rhodes Scholarship. This one was called a Henry Fellowship. It was money given by some Englishwoman named Julia Henry; it was a one-year scholarship and paid £500, whereas the Rhodes Scholarship paid only £400.

Hale: £500 was a lot.

Helmholz: £500 was a big amount.

Hale: That's as much as I had as a graduate student in the mid-sixties!

Helmholz: It was to either Cambridge or Oxford, and I suspect that they chose the Fellows. I think there were maybe three or four to go, two to Oxford and two to Cambridge. In those days, it was only something like January, I think, that you had to put in your application, and I was awarded one of the ones that year.

Hale: Considered quite an honor, I imagine?

Helmholz: Yes, I guess it was. I've met a few other people since then who have been Henry Fellows. I don't think Henry Fellows have ever become as well known as Rhodes Scholars. When I was in Cambridge, I met a few people who had come from the British side to Harvard and Yale. Every few years they would call up Dartmouth or Princeton or some place like that and say, "Have you got anybody you want to be a Henry Fellow this year?" because they weren't supposed to be limited to Harvard and Yale graduates. I remember meeting somebody a number of years later who had been a Henry Fellow from Dartmouth; he said the dean called him in one day and said, "Would you like to be a Henry Fellow at Cambridge or Oxford next year?" The dean explained what it was, and he said, "Sure, fine," so he was awarded it. Things were very much more informal in those days!

Hale: And a little more elite!

Helmholz: Yes. After I had applied for this, I don't know exactly what I would have done. When I applied for it, I was still thinking that maybe I wouldn't keep on in physics. But when I won this, I decided that I would certainly use the fellowship and study at Cambridge the next year. As I got into physics at Cambridge, I became still more interested in it and stayed with physics.

Wanderjahr

Helmholz: I stayed in Cambridge, Massachusetts, after I graduated for another month and a half. That was 1936. Then I took the boat to England. I left a suitcase there because it had the apparatus that I was going to put together again. It wasn't a very big thing, a foot and a half by two feet or something like that. Then I went on to Bremen. My father had an old friend in Bremen who met me, and I spent the day at their house. Then I went on to Berlin.

My father had a relative, a first cousin who lived in Potsdam. So I went to Potsdam and stayed with them for something of the order of a week. The Olympic Games were in Berlin then, and I went to the Olympic Games a couple of times. Betty, now my wife, was traveling through Europe with two other girls, and they came to Berlin.

Hale: You knew her before you went abroad?

Helmholz: Yes. She comes from Winona, Minnesota, which is close to Rochester. I had known her for quite a number of years before. She went to camp with my sister and things like that. When they left Berlin, they went to Munich, and I went along. I just spent those few weeks, the end of August and the first week of September, sightseeing. Then on to Paris.

I took the boat over to England and met another friend who had been at Shattuck School and Harvard with me. He was studying Greek at Cambridge, and he and I took a bicycle trip through southern England. I was in Trinity College, but I had not been in the list of the Trinity College entering students for long enough to get a room in the college, so I lived down in a place on Chesterton Road, which was across the Cam River from the town.

Hale: This is all before the term started?

Helmholz: This is all before the term started. I would guess that the term started about October first.

Hale: Normally it's quite late.

Helmholz: I think we probably spent two weeks riding through southern England. We went down to Canterbury and then sort of across the southern part of England. We arrived about the time that Cambridge started again.

Marcus Oliphant and the Cavendish Lab

Helmholz: I had a tutor in Trinity College whose name was Dudley. He was a classicist if I remember right. It's a little unfortunate, I think, that I didn't get a physicist or a chemist or something like that. After seeing my tutor in Trinity College, I went to the Cavendish Laboratory and had been recommended there to

Oliphant, who had been a friend of Bainbridge's. Oliphant sort of looked after my work that year. He had several students with whom he was working, essentially on Ph.D. theses. Mine, I think, was a little bit out of his line, which was sort of straight nuclear physics, but he was helpful and I learned quite a bit during that year.

It would have been better, I think, if I had really taken Part II of the physics tripos course. There were several other students there whom I've seen more of since I got back to this country than I did even while I was there. One of them was Charlie Kittel, who is in this department. The other was Norm Ramsay. They were both taking Part II. Part II was a sort of a natural step after graduation from an American college or university. I did go to a number of lectures. In both fall and the winter I went to Eddington's lectures on relativity, and they were terribly dull.

Hale: That's what I've heard of them.

Helmholz: I think I had read one of his books, and I expected that he might be a very good lecturer. He turned out not to be at all.

Hale: I gathered from Malcolm Henderson that he [Eddington] was more into cosmology; he talked about cosmology itself rather than teaching you relativity.

Helmholz: Yes. He gave the relativity that he was teaching at a fairly sophisticated level, which was a little more than I was able fully to appreciate. I also went to some lectures by Fowler on statistical mechanics, which were very good. Fowler was an enthusiastic and animated person. I think that I would have been a little better off if Fowler had not been lecturing on quite as advanced a level as he was, but I did enjoy it and learned a fair number of things.

Hale: They were all advanced lectures, though?

Helmholz: Yes, they were really advanced lectures, and they were not the Part II level lectures that I probably should have been going to.

Hale: C.T.R. Wilson had already retired?

Helmholz: Yes, he had retired.

Hale: Was Searle still around?

Helmholz: I don't remember him. Aston was still there. He worked in another room which was quite close by. There were quite a number of people there, and they had just built the nuclear physics laboratory building there, which was attached to the Mond Laboratory. The Mond Laboratory was originally a low-temperature laboratory, and this one had been built with a grant.

Hale: Was it the one built by Austin?

Helmholz: Yes, I think it was from Austin.

Hale: He gave a quarter of a million pounds?

Helmholz: Something like that.

Hale: He's the automobile mogul.

Helmholz: Yes. That was just getting running during the year that I was there.

Hale: It was to house a bigger accelerator?

Helmholz: Yes.

Hale: A cyclotron as well, wasn't it?

Helmholz: Well, they hadn't started a cyclotron there, but it did house the cyclotron later.

Hale: I think Rutherford had in mind that that's what he was going to do.

Helmholz: I've forgotten how many million-volt protons it was supposed to generate, but, I think at least two. That was fairly high at that time for a DC generator.

During the winter term, I found that I had the mumps. I spent two weeks in the winter term in the Cambridge isolation hospital. I had plenty of chance to read but couldn't get around to see anybody else. Then, at the winter break I went over to Potsdam and then went skiing in Switzerland. In the spring break I went down to Italy.

Hale: You really took the chance to get around, then.

Helmholz: Oh, yes. Five hundred pounds was very helpful, I think, in getting around. I never got off into the Scandinavian

countries, nor did I go, for example, to Spain or Austria. But I did see a fair amount of Germany and France and Italy.

Hale: Had you been aware of the reputation of the Cavendish, especially of Rutherford and the scientific achievements there, before you went?

Helmholz: Yes, I was. The year before I was a senior, Bainbridge had been at the Cavendish and had worked with Aston there. I remember that there was quite a controversy then between Bainbridge and Aston and the nuclear physicists as to what the true mass of carbon was. And they differed by amounts well outside the experimental errors of everybody concerned. The nuclear physicists started with oxygen and by nuclear reactions figured out the mass of carbon. I don't know whether they finally agreed with Aston or with Bainbridge. It must have been at the spring Washington meeting of the American Physical Society, somebody from Cambridge went over and presented a talk there about the mass of carbon. My recollection is now that Bainbridge was wrong, and, just about the same time, he reported that he discovered the error that he had made, and so that everybody finally agreed about this.

Hale: Before you went to Cambridge, did you feel, "Oh, I'm really going to the center of things here!"?

Helmholz: Well, no, I don't think so. I felt that it was a great opportunity. I don't think I was overcome with the prestige of the place. Harvard was a fairly prestigious university itself. I was interested to see the difference between the American colleges and universities and the British.

Hale: I was going to ask you about how the undergraduate and graduate programs compared in your mind.

Helmholz: The undergraduate programs were certainly more advanced in the British universities. In the public schools in Britain there was a lot more advanced physics, chemistry, and mathematics than in our schools. So that by the time the students got there, they knew a lot more than the entering freshmen in our universities. And in an American university, there were, at that time, requirements to make the student get a fairly broad education. In the British schools at that time, the student was supposed to get the breadth of his education either at home or in the early years of his schooling.

Hale: However, those schools you're talking about are only the grammar schools, as they call them, or the English public schools, so it really applied to a very small proportion of the whole student

population. Most of the students went to schools which were probably not as good as American high schools at that time --not as advanced. The system there has always been educationally elitist.

Helmholz: I was at Cambridge, but I did hear a little bit about the non-elitist education. I never ran into it at all, and I've talked to other students at Cambridge and found out something about their work before they came to Cambridge. They were the elite ones and so they had done a lot more specializing before they came to Cambridge than anybody, even in our best private schools. My only regret about my year at Cambridge was that I didn't take Part II instead of trying to do some experimental work.

Chadwick, Thomson, Cockcroft

Hale: Did you take the standard lab technique course that [James] Chadwick oversaw?

Helmholz: No. I'm sure the people in Part II took that.

Hale: I thought it was graduate students that took it.

Helmholz: No, I don't think so.

Hale: I remember Malcolm Henderson saying that he took it.

Helmholz: Chadwick was gone by the time I came, and nobody seemed to be giving such a course. I don't know why.

Hale: There must be something of that sort and you may have never seen it?

Helmholz: Well, the man in the shop would give a little course on how to use the lathe, but there was nothing in the way of electronics. Electronics was not terribly advanced in those days.

Hale: Did you notice if there was a sharp schism between the people that would take the math tripos and those that would take the natural science tripos?

Helmholz: I didn't notice it. I didn't know very many of the students who were taking the tripos. I knew the ones who had started their graduate work, who had taken the tripos in previous years. It was a terribly interesting time, and I enjoyed it a great deal.

Hale: Did you sense any legacy of J.J. Thomson around?

Helmholz: Well, J.J. Thomson had his eightieth birthday when I was there, and we all went to his eightieth birthday party at which he gave a short talk. I think he had just got a new or first set of false teeth, and apparently they gave him a good deal of trouble. We didn't understand his talk very well. There were things around the Cavendish that people would occasionally talk about that were connected with J.J. Thomson, but I really didn't feel that there was much of his legacy there.

Hale: He didn't wander in anymore?

Helmholz: No, I never saw him. I suspect that he did occasionally, but I never saw him.

Let's see, when I was there the professors were [Ernest] Rutherford, and Appleton had just become a professor. He was an atmospheric physicist. And, of course, [Paul] Dirac; I went to some of Dirac's lectures when I was there. And Fowler. These were the professors associated with physics. That was another thing that surprised me: how few professors there were. I came back out here and found that almost all the people who did the teaching were professors or associate professors or visiting professors; over there, there were only two professors in the experimental physics. One was Rutherford and the other was Appleton.

Hale: They obviously must have chairs there. Here, it's a rank. Lawrence was trying to get Rutherford to give a Charter Day address at Berkeley for something like '38 or '39. Rutherford died in '37. Were you aware of their "lively correspondence," as Herbert Childs puts it?

Helmholz: No, I wasn't. I really heard about it when I came out here. And, having been there, I would see Rutherford every so often.

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One person at Cambridge at that time was [John] Cockcroft to whom Bainbridge had also been a good friend. I think it was Cockcroft who had come to the United States in about April and had reported on this disagreement about the carbon masses. I remember Cockcroft had me out to his house for tea or supper one time after he had come back. He had seen Bainbridge.

Impressions of Rutherford

Helmholz: I had never been to Berkeley before, and so I didn't know anything about Berkeley except what I had read about Lawrence's cyclotron. I don't think I ever heard Rutherford say anything. I went to a few of Rutherford's lectures, and he would come down through the "garage" every so often, maybe every other week or sometimes once a week. But I really didn't see him a great deal. He would be at tea lots of times. He was really a dynamic figure and a very impressive man, I thought.

Hale: He was very bluff, wasn't he, dealing with people?

Helmholz: Yes, oh, yes. He had a big voice. I remember his telling the story about his going down to London one time during World War I when they had the first radio communication. Somebody said, "What did you need to come down here for, we could have heard you without the radio!" It's the same story they used to tell about Robert Gordon Sproul here in Sacramento--no reason for him to go up to talk to the legislature, he could do it from here.

Hale: Were you aware of what Rutherford was doing at the time you were there?

Helmholz: Yes, I suppose, in a somewhat general way, but I heard about some of the experiments they were doing with disintegrations. I've forgotten now exactly what, although I'm sure that I did know at the time what kinds of nuclear disintegrations they were studying, whether it was lithium or the beryllium problem or what.

Hale: He was actively working on it in the laboratory?

Helmholz: Well, I wouldn't say he was actively working on it, but it seemed to me he kept pretty close touch with what was going on.

Hale: He took a personal interest in sort of almost everybody?

Helmholz: Yes. I had a feeling that he knew very well what was happening with the things in nuclear disintegration. But he didn't get into the laboratory himself and do anything. You have probably read about his assistant who helped him through the work with alpha particles?

Hale: Crowe?

Helmholz: Yes. He was there. I can't really remember whether he helped Oliphant and people like that, or what he did. But he was around the laboratory and working.

Hale: I thought Oliphant and Rutherford both were working on tritium at that time?

Helmholz: That does seem somewhat familiar. There were a number of other people there. Bretscher was there. He was Swiss-trained. Cockcroft was really in charge of the Mond low-temperature laboratory. The fellow who did a lot of the low temperature work was Schoenberg. He had worked with Kapitza when Kapitza had been there. He was a younger person. I never really knew Schoenberg, but, of course, I had been introduced to him and talked to him occasionally at tea. He had been to Russia the previous summer, I think, and he would tell people about his experiences in Russia. I really don't remember what he said.

Hale: What was Rutherford's health like during that year?

Helmholz: It seemed very good. I never could see that he had any problems. If he were gone for a couple of weeks, it could have been that he was sick, and I just never heard about it or thought he'd be gone to the Continent or even to London for some meetings.

Hale: Rutherford was due to give an address in India to a science congress that was supposed to be in '37, because he was president of that Combined Science Congress. Now, I don't know whether that was scheduled before he died or what, but it was actually given by [James] Jeans.

Helmholz: I remember that when I came here to Berkeley, I started working around the cyclotron as soon as I came. One morning on the blackboard there was this statement that Rutherford had died.

Hale: That must have been just after you came?

Helmholz: Yes. In those days school started here in the end of August.

Hale: He died in October, I think. Did you know Jeans at all?

Helmholz: I never met Jeans.

Hale: Kapitza was long gone back to Russia, unfortunately, but did anything like the Kapitza Club survive?

Helmholz: I don't think I even know what you mean by the Kapitza Club. One of the people who did know Kapitza, I think is [Robert]

Brode. He had been on sabbatical leave; I think it was in Cambridge.

Hale: Did you attend the colloquia, then?

Helmholz: Oh, some of them. Occasionally I would find something that I knew something about, but I wasn't advanced enough in my studies of physics, really, to profit a great deal.

Hale: Was it sort of general that most people would attend the colloquia?

Helmholz: Oh, quite a few people would attend.

Hale: There was another club, the $\nabla^2 V$ Club.

Helmholz: I remember that there was such a club.

Hale: That's the "inner sanctum."

Helmholz: I certainly never got into that. I think somebody like Brode or Kittel might know--Kittel was a little more theoretically inclined than I was.

Hale: What do you remember about important visitors to the lab?

Helmholz: Very little.

Hale: Wasn't Bohr there while you were there?

Helmholz: I don't think so. I've often thought of that. The only time that Bohr was here before the war was '37 or '38. I happened to have been gone on summer vacation. I don't think I ever met Bohr until 1954, when I happened to be in Copenhagen.

Life in and out of the Lab at Cambridge

Hale: What was a typical day like, spent at the Cavendish?

Helmholz: I'd go to the laboratory at nine o'clock in the morning and then would work on whatever happened to be the problem that day with my apparatus. I probably went to one or sometimes two lectures in a day, in the morning. I think I usually had lunch right around the laboratory, somewhere. Then, in the afternoon, go back in the laboratory and work until teatime. Sometimes, I'd stop at teatime and go back to my rooms and do some studying.

If I had something that I thought was interesting, I'd go back to the laboratory and work there until six o'clock or so, and then go on back. Sometimes I would go directly from the laboratory to Trinity and have dinner there. Sometimes I would go back to my rooms and then come back to Trinity. After dinner, I'd probably go home and study. I think it was too bad, in a way, that I didn't have a room in Trinity, because I think I would have learned to know a good many more people in the college or would have known more students. I'm not a terribly gregarious person; that was a case in which it would have been of an advantage to me to be more gregarious. I would have met more people.

Hale: Did you get much into a sort of a social life there, or what?

Helmholz: No, I didn't. During the fall I started to play tennis. There was a tournament at which another American and I got fairly far. In the spring, I played in some of the matches that the Cambridge University team played against other clubs. I remember the match with a group of Dutch tennis players. I've even forgotten who won the matches, but it was interesting to me to see people from a foreign country who also played tennis.

Hale: They're not as rigorous about having to be an undergraduate to play for a varsity?

Helmholz: No. I suspect that the match they would play against Oxford was all undergraduates. I suspect that this match against the Dutch people was with some club in Holland. They weren't students.

Hale: Did you get a sense of sport there being more relaxed than it is here?

Helmholz: Yes. I think in the fall I went out and watched a few rugby games, but there certainly wasn't the big sport the way there was in football in this country. That was the year in which Edward the Prince of Wales abdicated. During the next spring, I went down to London to the coronation of the next king. What was his name?

Hale: George the Sixth.

Helmholz: Yes, it must have been George. I was able to see the royal carriage go by, but there were certainly a lot of people in London that day.

Hale: All great fun.

Helmholz: And then I also went down to London the time the Cambridge-Oxford crew race was rowed on the Thames. I got down to London

a fair number of times, went down to a few shows. I once went to Lohengrin at the Covent Garden Opera House. That was quite a performance. It was one that started at five o'clock in the afternoon and had a break for dinner at seven-thirty. I think they must have given it almost uncut.

Hale: What was going on at the Mond Lab? It was mainly low-temperature work?

Helmholz: Yes, but I don't think I ever found out what kind of low-temperature work.

Hale: Wasn't Cockcroft also involved with that extension to the Cavendish, right?

Helmholz: Yes, he was. I'm sure he knew everything that was happening in the Mond Laboratory, but I don't think he took an active part himself in low-temperature work.

Hale: He'd taken over a lot of administrative duties?

Helmholz: Yes, he had. I didn't see a great deal of him either. It was really Oliphant that I saw.

Hale: Didn't Cockcroft eventually recommend you to Lawrence?

Helmholz: Yes. I just applied in the usual way. Lawrence was in Rochester, Minnesota, that spring and giving a Sigma Psi lecture or something like that, and my father had met him. When I applied, there was no problem of my being admitted. Lawrence knew that I was coming, and there was plenty of opportunity to work in the laboratory. It wasn't like today where you have to have fancy recommendations, and only the top group gets picked. As a friend of mine said about our going to Harvard, if you had the money, you could get into Harvard at that time.

Hale: Was [Patrick] Blackett at the Cavendish?

Helmholz: No, Blackett was in London at Imperial College. Brode knew Blackett well. I've forgotten exactly when Blackett had left, but, of course, Blackett was a name that everybody knew. Bernal was at Cambridge. I never saw him, although I heard about him some. He was engaged in sort of X ray diffraction or crystallography or something like that.

Hale: He was working on structure of proteins. Was Bullard there?

Helmholz: He had gone, also.

Hale: Ratcliffe?

Helmholz: Ratcliffe was there, I'm pretty sure, but I don't remember him at all. I'm sure that he would come to tea.

Hale: Alex Wood was still there, wasn't he?

Helmholz: I don't remember.

Hale: Did you feel good about what you completed?

Helmholz: Well, I was disappointed that I didn't get anywhere with my proton diffraction. I think I felt I learned quite a bit and had a good experience. As I look back on it, I didn't accomplish as much as probably I would have from a straight year of graduate work here. It was certainly worth it from having seen a different sort of point of view and a different kind of graduate work. So I was and still am delighted that I had the chance to go there.

Hale: What was your general impression of British science?

Helmholz: I thought that British science was very vigorous and very good. I had got a good impression of British science from the people that I knew at Harvard, and there wasn't any feeling in my mind that British science was in any sense decaying or decadent. Particularly in nuclear physics it was a top-flight place. I think that in the years from '36 to the beginning of the war--in part because they didn't build up their facilities for nuclear physics the way that it was being done over here--I think perhaps they were falling a little behind. On the other hand there were some very good people there. What they were doing was always interesting and forward-looking.

Hale: Could you sort of compare it or contrast it with the continental science of that time?

Helmholz: Well, I couldn't except for what I heard. I was in Germany a couple of times seeing my relatives and just sightseeing. Hitler was very well entrenched, and a fair number of German scientists had left Germany. I got the impression from people in Cambridge that German science was well on its way downhill. There were some very good people there who were still working, like Otto Hahn. There was no national prestige involved in working in nuclear physics or something like that. As far as French science goes, I just didn't really hear much of anything.

Obviously, Joliet and Madame Curie's daughter were working hard and well there. After Madame Curie's original discovery the French scientists in nuclear physics were never as important as the British and the Germans were.

II GRADUATE STUDIES AND THE RAD LAB AT BERKELEY, 1937-1940

Move to Berkeley, Early Impressions of Lawrence

Hale: Why did you decide to come to Berkeley?

Helmholz: Well, as I say, partly because I had been hearing about Berkeley from the people in nuclear physics at Cambridge, and I knew that Berkeley was a good place. I think I occasionally probably talked to Bainbridge about graduate schools. Also, my present wife was at Stanford. Those were the reasons.

Hale: Had it actually appeared to you before you came as some sort of mecca for nuclear physics, or was it just that you knew it had at least a reasonable reputation?

Helmholz: I wouldn't say it was a mecca. When I thought about it, I might have gone into some <u>other</u> branch of physics, even at that time. But certainly Berkeley had a very substantial name. I think if I thought about other places, I could have gone back to Harvard or to Princeton or Columbia. They were all good names in graduate work in physics at the time. I guess I was adventurous enough so that I would like to see the other coast. I'd been out to the Canadian Rockies with my father at one time, but that was the only time I'd ever seen the Pacific Ocean. I'd never been to California before.

Hale: Well, of course it was still quite adventurous, intellectually, to come out here. Had you any idea of what you might work on before you arrived?

Helmholz: No.

Hale: You didn't bring the apparatus back here?

Helmholz: I didn't. I didn't think of working in cosmic rays or spectroscopy or discharges in gases, which were other fields that people on the faculty were working at. I was interested in

working in nuclear physics. So, as soon as I came, I started working over there and got to know everybody in the laboratory pretty well. I had to take the regular courses, pass the examinations, but all the while I was doing that, I was also working at the laboratory.

Hale: Before you came, did you have much idea of what the Rad Lab's stage of development was--its facilities and research?

Helmholz: No, I didn't. I knew about the cyclotron and the order of magnitude of the energies to which deuterons and protons and alpha particles had been accelerated. Luis Alvarez wrote me about the 37-inch, the revamping of the cyclotron from 27 inches to 37 inches. That was one of the things he had been working on.

Hale: And you had no idea of the finances of the laboratory?

Helmholz: No, I really didn't. I had enough money from my father to get along in the first year of graduate work. Actually, I think I still had some of that £500 left over. In the second semester that I was here, I was a teaching assistant, but I didn't know anything about the support of the laboratory.

Hale: Well, what did you actually find when you got here?

Helmholz: I found a very active and enthusiastic group at the laboratory. The laboratory used to be over here, where the parking lot and Latimer Hall are. Perhaps you've seen pictures of it.

Hale: The Old Radiation Lab.

Helmholz: Yes, the Old Radiation Lab. We would go in there and other people would be there, and you'd have a regular schedule at which times you'd have to be on the operating crew. If you weren't on the operating crew, you'd sit around and talk to people or you'd do something having to do with an experiment that you were involved in. Maybe you'd just help the operating crew. Don Cooksey was the one who was mainly in charge of the operation of the laboratory. Ernest Lawrence had become in the previous year so much involved in lectures and travel and so on that he wasn't there all the time. When he would come in, though, things would happen. Once I was operating the cyclotron. He'd always go up and look at the beam meter which was a 250 micro-amp meter. If the beam was about 40 micro-amps, he'd say, "Well, how's it going?" You'd say, "Well, all right," and he'd say, "Let me try." He'd sit down and he'd have us scurrying all over changing this, changing that. By the time he

left, it would be up to 100 micro-amps. He was a genius for getting the most out of that.

Hale: And also for driving people nuts and blowing the filaments once in a while!

Helmholz: Oh, yes. Sure.

Lawrence was a great tennis player, and so as soon as I came, he said, "I understand you play tennis. Well, how about this afternoon over at the Faculty Club?" I started playing tennis with him right from the start. Played a lot with him from that time on, for many, many years actually. I remember playing with Arthur Compton at one time when he was visiting here. Even played with General [Leslie R.] Groves once.

Hale: Really? So you had a talk with Lawrence when you first came? You knew you were going to be his student?

Helmholz: Yes, that's right.

Hale: Did you talk with him about what you were going to do?

Helmholz: No, he just said, "Look around the laboratory. Anything you feel is of interest and you want to do research on, go ahead."

Hale: It was assumed, though, you'd be taking your part in the cyclotron?

Helmholz: Oh, yes, that's right. We all did that.

Work as a Graduate Student with the Beta-Ray Spectrograph

Helmholz: Among the people working here was a fellow named [David] Kalbfell, whom I really have never heard of since. He got his degree at the end of 1938, and he left me a beta-ray spectrograph. He said, "If you're interested in working on some of these things, go ahead, take the spectrograph, I'm leaving." I think he went down to San Diego where he was from. So that was what I got interested in. I was interested in general problems of radioactivity, but with this beta-ray spectrograph I could measure the energies of conversion electrons.

Hale: That was not until 1938?

Helmholz: Yes. I think probably during the spring semester I began to talk to Kalbfell about the possibility of taking over the beta-ray spectrograph. It was the only beta-ray spectrograph for measuring conversion electrons. There had been a fellow named Ernie Lyman who had gone to the University of Illinois. He had been here before I came, and he had a spectrograph with a geiger counter attached to it. The one that I took over had the photographic film for recording the electrons. He had built one which he used for measuring continuous beta-ray spectra. That one sort of fell by the wayside and nobody was using it during the years that I was a graduate student. I guess nobody just was really interested in it. When Kalbfell left, I took that over.

I happened to be gone the next summer; I was vacationing in the middle west. Sid Barnes from the University of Rochester was here, and he was interested in beta-ray spectroscopy, the measurement of gamma-ray energies and conversion electrons, and so on. He decided that the spectrograph that Kalbfell had built was not a very good one, so he had designed another one and had it built. I'd been gone for six, eight weeks, and when I got back, there it was. Actually, it was a much better design. I just kept using the new spectrograph.

The next year I finished passing my exams and started to write a thesis, doing experimental work for that. The title of that was, "Energy and Multipole Order of Nuclear Gamma Rays."

Isotope Production with the 60-Inch Cyclotron

Helmholz: This was about the time that we got started with the 60-inch cyclotron. I used to be on the operating crew of the 60-inch cyclotron, but I never really had anything to do with the building of it. Thornton had come back from Washington University in St. Louis. Once it got started, I did take some part in some of the interesting experiments.

[Interview 3: July 15, 1975]##

At that time one of the radioactive elements that the biologists and the biologically interested people had found important was a radioactive isotope of strontium. In the process of making radioactive strontium by bombarding strontium with deuterons, an isotope of yttrium, Y-88, I believe it was, was formed which had very strong gamma rays. It was a K-

capture activity followed by gamma rays. The gamma rays had energies, as I remember, of the order of one and a half million electron volts. This was rather surprising that a fairly long half-life isotope--I believe it was three months--had such a large high-energy gamma ray associated with it. I spent some time looking into this radioactivity.

There was a Belgian M.D. named Pecher who was at the laboratory at the time, and he also got interested in this radioactivity. I think we published a letter in Physical Review Letters. It was suggested that this might be a good source of high-energy gamma rays for radiographic studies in material testing. I took several samples up to Mare Island naval shipyard, where an engineer was interested in testing castings and things like that. It turned out to be a reasonably successful effort, but it just wasn't worth making the radioactive yttrium for this purpose. I think something could have been done, if it had been necessary. This was about the time that the betatron was invented by Kerst at Illinois, and the betatron was used particularly during the war extensively for radiographic testing. This use of yttrium was never followed up.

The 60-inch cyclotron was a very interesting and useful instrument. It was the highest energy cyclotron in those days. I think the deuterons had energies of 16 million volts, the alpha particles of 32 million volts. A number of interesting things were discovered, such as the radioactivity of an isotope of element 85, which had been missing in the periodic table up to that time. This was done by Segrè and Corson and McKinsey. Before the vacuum chamber was installed and while the people that were building the machine were testing the magnet, professors [Emilio] Segrè and [Francis] Jenkins--who were both interested in spectroscopy--did an experiment with the quadratic Zeeman effect of sodium or potassium. To observe the effect, you need very large principal quantum numbers, a very large orbit. I think they observed principal quantum numbers up to n=35 or thereabouts. I remember also that Dr. Jenkins gave a talk about this experiment, and the title of the talk was the "Big Atoms." That just happened to be a time in the United States when the dance craze at that time was called the "Big Apple."

Comparison of Berkeley Lab to Cavendish

Hale: How did the laboratory compare in your mind with the Cavendish?

Helmholz: It was quite different. The whole work of the laboratory was centered around the cyclotron. In the Cavendish, if you happened to be in the nuclear physics part of it, there were several different apparatuses accelerating protons for different kinds of experiments. At the Cavendish there was a sort of a community spirit, perhaps best exemplified by the fact that people had tea every afternoon. At the Radiation Laboratory, there was even more of a community spirit; everybody was involved with the same machine. It's true that each person working there had some experiment in which he was particularly interested; sometimes two of the members of the laboratory would be involved in the same experiment. But because of that focus of the cyclotron, there was a rather different feeling.

There was just a difference in the old British system of doing physics. Lawrence, as he showed later, was a forerunner of, let's call it the "Big Laboratory." Particularly, now that it's happened, one could see this coming from the way in which the laboratory was organized. Another thing: the Radiation Laboratory at that time ran twenty-four hours a day. When I first came, the cyclotron was used a great deal for making radioactive phosphorus. It was then chemically purified and used in biological work. There was a strong interest in using it to try to treat leukemia. One would feed the phosphorus to a patient who had leukemia, and the phosphorus nominally went to the bone. The presence of the radioactive phosphorus would presumably kill whatever kinds of cells were producing too many white cells in the bone.

Hale: That was [Martin] Kamen and [Robert] Wilson?

Helmholz: Wilson designed the probes, and Kamen did a lot of the chemistry work in separating and purifying the radioactive phosphorus. When the 60-inch was started, Dr. Joseph Hamilton was a principal figure; he was interested in those kinds of problems. When the war started, he got interested in the problems of plutonium and so on. He was really sort of the man in charge of the 60-inch cyclotron for a good many years.

Hale: Did you have any sense of the lab possibly eclipsing the importance of the Cavendish?

Helmholz: Well, I don't think I really thought about them in relative terms. Everybody in Berkeley, everybody in physics, recognized the importance of the Cavendish, the work of Rutherford and his collaborators, the discovery of the neutron, the first artificial disintegration, and so on. On the other hand, I think most of the people in Berkeley felt that the cyclotron

would be a more important accelerator for future developments in nuclear physics than the kinds of machines that were being used at the Cavendish.

I don't know whether anybody would have foreseen that the Cavendish would go off in other directions. When Rutherford died and Bragg was appointed Professor, I think everybody realized that he probably would bring in a different kind of work and possibly the work on nuclear physics would become less important.

Lawrence's Nobel Prize and the 184-Inch Cyclotron

Hale: Did you also get a sense that Lawrence was on his way to the Nobel Prize, for example?

Helmholz: Yes, I think probably most of us felt that Lawrence would get the Nobel Prize. When, of course, was another question.

Hale: How soon did you have that sort of a feeling?

Helmholz: I would guess that when I'd been here a year, I certainly had that feeling. I wasn't particularly concerned about Nobel Prizes. Lawrence had lots of honors in those days. But when he did get the Nobel Prize, he began to think about a still bigger cyclotron than the 60-inch. He won the Nobel Prize in '39, and I happened to be out playing tennis with him when he was called to the telephone. When he came back he said he'd just been informed that he'd won the Nobel Prize. It had been rumored about. People knew that the Nobel Prize was going to be announced very soon, and it was rumored that he might well get it. Nobel Prizes were almost always announced on a Thursday at that time, and that was his regular day for playing tennis.

Hale: Knowing now the way nuclear science and big science has developed because of the war and government funding, did you honestly have much inkling then of the way things would go?

Helmholz: Well, no. I didn't have an inkling of the way things would go. One at that time might well have guessed that it would become big science. I remember when Lawrence won the Nobel Prize and began to think about a bigger cyclotron. It was the 184-inch cyclotron, and it was decided to put it up on the hill. Plans were made, but nobody thought of the laboratory on the hill as a big, entirely self-contained laboratory. People still thought,

"You bring the radioactive materials down to the campus to do the investigation of the radioactivity."

It was, as you may know, thought of as a conventional cyclotron to get 100-million-volt deuterons. Because of the relativistic change in mass, one could not accelerate the particles lots of times. I think a hundred or two hundred times was about as many turns as one could assume possible before the particles fell out of phase. Somebody like Don Cooksey could tell you a lot better than I can whether it was the first plan for putting something like a half a million volts between the dees--how big did the vacuum tank have to be and what clearance would be necessary and so on. That design went on, not rapidly, I would say, but very extensively. It was worked on through 1940, and then at the end of 1940 the laboratory building up on the hill was started. The engineer who was particularly involved in this was William Brobeck. When I first came to the laboratory, he would be in charge of the Monday nights when we would have a time that the 37-inch cyclotron would be serviced or repaired.

Hale: He instituted that, didn't he?

Helmholz: I thought when I first came that he was a physicist. We got to talking about some problem in electrostatics one night, and he wasn't taking much part in the conversation. He finally asked, "What does ∇^2 mean?" At that time, I learned he was really an engineer and not a physicist. But he is a remarkable person.

I think that was part of Lawrence's genius in running a laboratory. He realized that one needed these people from other disciplines who could contribute the knowledge that was needed. In the design of the 184-inch cyclotron, physicists might well have made mistakes or had to spend a lot more time on the design. But Brobeck knew those things. I guess the same was true of the design of the bevatron in which he was an important member.

Hale: When, in fact, was your first realization of major changes taking place in modern methods of pursuing science?

Helmholz: I don't know. I hadn't really traveled around to enough other laboratories to realize how different the cyclotron at Berkeley ran. In years following the war, I did get around to some other laboratories, and I could remember that the Cavendish wasn't organized that way. But I don't think that before the war started I had any real feeling that nuclear physics and particle physics would develop into the kind of a science that it now is, with many collaborators on papers and big machines that are run

by people whose main concern is the machine, not the physics that went into it. The war, I think, really made a big difference in that aspect of science.

I don't doubt that if the war hadn't taken place, the 184-inch cyclotron would have gradually developed into a big laboratory in the way that the present Lawrence Berkeley Laboratory is organized. Instead of being a big laboratory in the few years after the war ended, it might have been ten or fifteen years before it developed in that way.

Hale: Because there wouldn't have been the sort of crash program for the development of the bomb?

Helmholz: Yes.

Early War Work

Helmholz: It was quite a change for those of us in Berkeley before the war started. A number of the members of the laboratory had left to go to the Radiation Lab at MIT or to Washington. Alvarez and McMillan were gone. Lawrence himself was gone a lot of the time in the year and a half before the war [in the Pacific] started. Brode had gone to Washington. He was the cosmic ray physicist at the time. That's how I happened to get started teaching--because he had to leave his class and I got a chance to teach.

Nuclear physics was just dropped in a few weeks. There was nuclear physics still going on in the production of radioactive materials, and most of the biological work associated with nuclear physics had remained in the 60-inch cyclotron. But the 37-inch cyclotron was just shut off completely and became a mass spectrograph. We all changed very rapidly. Everybody knew about mass spectrographs, and everybody realized that this was an important thing to do.

Hale: In 1941?

Helmholz: My guess would have been October or November of 1941. Then, on December 7th, we were somewhat into the work, but then it became obvious that it would be even more of a crash program.

Hale: What was the major type of research going on from '37 to '40?

Helmholz: Mostly studies of radioactivity.

Hale: Induced radioactivity?

Helmholz: Yes, induced radioactivity and the nuclear physics that went with it: the measurements of the nuclear reactions and the different probabilities of the different kinds of nuclear reactions. When, for example, the 60-inch cyclotron was started, one of the interesting things was what is the relation of D,p reactions to D,n reactions. This had been studied in 1935-36 and gave rise for D,p to the name Oppenheimer-Phillips reaction. At the 60-inch cyclotron, D,2n was the most probable reaction. Then Segrè came in 1938, I believe, and he started a very active program in the measurement of different kinds of radioactivity. Chien-Shiung Wu (Mrs. Yuan) was here at the time; she was working with him.

Hale: She's the president of the APS [American Physical Society]?

Helmholz: She's now the president of the APS, yes. You should get Segrè to tell how much he disapproved of the fact that the department wouldn't hire her as a member of the faculty. Segrè always felt that Birge was anti-feminine.

Fission

Helmholz: Abelson was working on fission at the time. They didn't know it was fission, but he was working on the characteristic X rays from uranium. They are different from the products of bombarding uranium with neutrons. He was thinking they were L-X rays from the trans-uranic element when they were really the K-X rays from the fission products.

Hale: And he just missed fission.

Helmholz: And he just missed that. We read in the newspapers that Bohr had come to this country and had announced fission. I can remember that Alvarez came into the laboratory, and he said to Phil Abelson, "Now, Phil, I want you to come over here and lie down on the table. I don't want you to fall over when I tell you what's happened." Then he announced that it really was fission.

Hale: And it was immediately clear?

Helmholz: Yes, that's right. I think by the afternoon Bob Thornton had made the cloud chamber with the uranium and the foil and bombarded it with neutrons and seen a picture of fission.

Hale: It's incredible that it was so easy to check out all those major discoveries almost immediately.

Helmholz: Artificial radioactivity, I'm sure, took place on the day that they learned about it; I wasn't here then.

Hale: What was Alvarez doing? Were they all basically working on particular areas of the periodic table in artificial radioactivity?

Helmholz: There was some of that. [Glen] Seaborg and Livengood were working on radioactivity in the iron region. Alvarez did some work on radioactivity, but he had a number of other things in which he was interested. One was the magnetic moment of the neutron. He got Felix Bloch to come up from Stanford to work on that. Another one was the slow neutron experiment.

Hale: Wasn't he modulating the output of the cyclotron or something?

Helmholz: Yes, modulating the ion source. That would give you a modulated beam of slow neutrons. Then they put the neutrons through a magnetic field causing them to precess.

Hale: You were picking up neutrons that took a long time to get from cyclotron to detector.

Helmholz: Yes, you could measure the time of flight of the neutron, which would tell you then that it was a slow neutron.

Hale: This was for medical purposes?

Helmholz: No, they were just really interested in the magnetic moment of the neutron.

Anomalous Magnetic Moment of the Deuteron

Helmholz: Alvarez, I'm sure, can tell you the detail of it, but this was about the time that Rabi was doing accurate experiments on the magnetic moment of the proton and the deuteron. It was of great interest to measure the magnetic moment of the proton and the deuteron. It was of great interest to measure the magnetic moment of the neutron since it was generally assumed that the magnetic moment of the deuteron would be just the sum of the moments of the proton and neutron. They were nominally in an S state, which would just add. Rabi was able to show by very accurate measurements that there was some D state in the

deuteron. I think he did that through the magnetic moment, showing that the magnetic moment was not exactly the sum of the proton and neutron magnetic moment.

Then Alvarez and Bloch did this. I guess this was Felix Bloch's introduction to experimental physics, and he followed up very well in later years. He was at Stanford at the time and remained there, but he came up and stayed in Berkeley sometimes and was very much involved in that experiment. Alvarez and Pitzer did an experiment with the scattering of slow neutrons from ortho- and para-hydrogen. Alvarez would occasionally do something with radioactivity, but I think those two experiments took up a good deal more of his time.

I was working on conversion lines from radioactive substances, looking at a line from cadmium 107. We didn't know whether it was cadmium 107 or 109. One of Oppenheimer's students named Nelson was doing some calculations on the ratio of the K conversion line to the L conversion line. He showed that the higher the multipole order of the gamma ray, the smaller would be the ratio of K to L. The greater would be the L conversion relative to its K. It was known that nuclear isomerism was associated with an excited state in which the gamma-ray transition to the ground state is sufficiently forbidden to make it long enough to measure. This calculation of Nelson's suggested that cadmium, in which I had measured that the K and the L conversion lines were almost of the same intensity, might be highly forbidden. So we got some cadmium. Then we realized that if it was a K capture followed by a gamma ray, maybe the isobar would be isomeric. We did the easy chemical separation of putting cadmium nitrate in solution and adding a little silver carrier and some chloride, precipitating the silver and measuring the radioactivity. Sure enough, it turned out to be a 90-second or 2-minute half-life, just associated with that particular gamma ray. Alvarez was involved in that experiment, too. He was very active in a lot of different things.

The Monday Night Journal Club

Helmholz: The Monday night Journal Club was a really very stimulating time. I guess it was Lawrence who started that; then he handed it over to other people to organize. They would ask two or three people to talk about some recent paper every Monday night. We would meet at 7:30 or 7:45 and stop promptly at 9:00. Lots of times we had the window open, and when the Campanile struck

nine, everybody would have to stop talking, even any speaker. There was a fair variety of things, yet they were mainly nuclear physics and cosmic rays. If something very exciting in spectroscopy happened, it would be reported on. It was well attended, particularly during the summers when there would be visitors in Berkeley. It got to be quite exciting. Oppenheimer used to have a number of theoretical physicists visiting him during summers, and sometimes they would get four or five people like Weisskopf and Bethe and Oppenheimer and Placzeck, all at the same seminar. They really had some very exciting discussions. I don't remember the particular topics, but they would disagree with each other. It was very interesting to listen to them.

Hale: That must have been a little overawing.

Helmholz: Well, it was exciting, particularly exciting when a number of distinguished visitors would be there. Throughout the year, it was of really quite high quality. McMillan asked me to give a talk on some paper, and I spent a lot of time preparing that. It was a compliment to be asked to talk about something. People would talk about their recent work also.

Hale: Everybody got a chance to talk?

Helmholz: Yes, everybody got a chance. If you had something which you thought was exciting, then you just would go and tell whoever was in charge that you'd like to talk! The really exciting things that would come out of the Physical Review Letters or in Nature were always talked about immediately. Sometimes one would take ordinary things, but interesting things, out of the Physical Review or Zeitschrift für Physik or whatever journal it happened to appear in.

Hale: What about McMillan, for example? You mentioned that he was responsible for the Journal Club.

Helmholz: Yes, I'm sure he went through a period of being responsible for the Journal Club. McMillan spent a lot of time on the 60-inch cyclotron--I think a good deal more time than Alvarez ever did, but you'd just have to ask him.

Isomer Research##

Helmholz: When the plans for the 184-inch cyclotron began to be formulated, McMillan was very much involved. I don't remember

what experiments he was involved in during '38 and '39, but I remember that as soon as fission was announced he got involved in measuring the range of fission products. He used thin sources of uranium and cigarette paper to measure the range of the fission products. And he began the experiments in chemical analysis to show that there could be an element 93. When a neutron reacts with a uranium nucleus, it can cause fission or be captured. In the latter case, the resultant nucleus, U-239, will decay to form the element 93.

Hale: What about Laslett?

Helmholz: Laslett had left by the time I came. I think he must have left the year before. He came back once or twice, and then I can remember meeting him, but my recollection is that he went to Indiana. Lyman had left also. He used to come back occasionally. Lyman had left to go to Illinois in the summer, and I came in the fall. His wife, who got her Ph.D. in spectroscopy, was still here when I came. I think she finished up in a few months after the year started.

Hale: What was Kurie doing?

Helmholz: I think he went into war work of some kind in about '39 or '40. He had been working on beta-ray spectra, and then he did some work with the cyclotron. Von Voorhis was another person who was here. He was still doing some work in radioactivity.

Snell was particularly active in the investigation of isomerism; he'd been studying the bromine isotopes. It was known that there were two stable bromine isotopes, and when you bombard it with neutrons you get three radioactivities with slow neutrons. Snell was particularly involved in trying to unravel this. One of the radioactivities was the excited state of another one.

Segrè and Seaborg and another chemist, I think it was Halford, devised a way of determining which was the upper state by something like a bromobenzene. When the excited state decayed and gave off the conversion electrons, the bromine bond in the molecule was broken, and the bromine was set free. They were able to precipitate out the bromine, and they found that they got the pure molar state. I think there was an 18-minute and a 4½-hour half-life.

Hale: So, it's a chemical method of separating out the two? I read that they could demonstrate a sort of a "genetical relationship" between the two types of nuclei. What does that mean?

Helmholz: Let's assume that the 4½-hour radioactivity was the parent of the 18-minute radioactivity. Let's say it was bromine 80 and that one of them was the excited state, the parent of the 18-minute so-called ground state which was a beta-ray emitter and went to krypton 80. Segrè was particularly interested in isomerism, excited states with a finite half-life. Afterwards, I think he and many other people applied this method to other cases of isomerism.

Hale: You and he wrote a review article together, didn't you?

Helmholz: Yes. That was after the war. We spent a fair amount of time on that article. I had done my thesis on a subject like that, and it was sort of a natural thing for me to do. Unfortunately, that article was written just before the ideas of shell structure began to be discussed. As soon as that was done, then, one had a real way of understanding where these excited states came from.

For example, Alvarez and Nelson and I had discovered a ground state of silver where the nucleus had a spin of ½. We guessed that the excited state, which we were able to show was an electric quadrapole radiation, would probably come from a state of spin 9/2. But why there should be a state of 9/2 that was low-lying, nobody really had had a good idea. As soon as the ideas of shell structure came in, then it became apparent that there would be a fairly low-lying state of spin 9/2. It turned out really to be a state of 7/2 with the opposite parity.

The Beta-Ray Spectrometer

Hale: How soon did you choose your research project and how did you choose it? You've mentioned the beta-ray spectrometer for the conversion electrons that Sid Barnes had left. Was the fundamental reason you got into it because the apparatus was there?

Helmholz: Oh, yes, I think so. The apparatus was there, and there were obviously lots of problems. My predecessor with the beta-ray spectrograph, a fellow named Kalbfell, had done his thesis measuring a number of these gamma rays by the conversion electrons. Then, as I got going into my work measuring conversion electron energies, Nelson came along with the calculations. From the K to L ratio of the conversion electrons you could learn something about the multipole order of the gamma

ray. I just started working along that line, and that's the way my thesis developed.

Hale: Did you suggest it to Lawrence, or did he suggest it to you?

Helmholz: Lawrence never really took much part in that decision. He was interested in the subject, but I don't think he knew a great deal about it. It was mostly Alvarez and Segrè and McMillan who sort of gave me the tutelage that I needed. Lawrence was officially the chairman of my thesis committee until I got almost ready to write the thesis. Then he said, "I can't be, I'm out of town too much," and so McMillan became the chairman. I think Libby in chemistry was also on my thesis committee. There must have been a third member, and I can't now remember who it was.

Hale: So, except in so far that you were working in the Radiation Lab, it was difficult to say that you were Lawrence's student as such?

Helmholz: That's right. Lawrence knew everybody and knew quite well everybody who was working in the laboratory. By the time I got my degree in 1940, he was much involved in national affairs; the Radiation Lab at MIT, fission as a source of energy or an explosive. He didn't spend as much time in Berkeley in those years as previously. He wasn't able to direct student's research.

Hale: I'm surprised actually at how short the theses are: thirty pages, thirty-five pages. Was it because it covered such a small proportion of the requirements?

Helmholz: I think maybe that a lot of the physics theses were of that order. Sometimes when the work involved some new kind of apparatus or something like that, it might be longer. Some of the theoretical students would do several pieces of work for the thesis. I mean, they'd have two papers in the Physical Review, which would constitute a thesis, and so then the thesis would have a double name and that would be longer.

Hale: I know you published a couple of papers on yours.

Helmholz: Yes. I revised and cut this down for a paper for the Physical Review after it had been approved. I think the theses nowadays are a little longer. One reason is it's easy to get them typed up by the laboratory and then run off. I usually tell students it's one place they ought to put down a fair amount of detail of what they've done. You'd never put it in a Physical Review article because the Physical Review wouldn't take that long an

article. Say somebody wanted to know what kind of a program you used to calculate the orbits of mesons in the spectrometer, then you ought to put something about it down.

Graduate Courses and Exams

Hale: What courses did you take and how much of your time was occupied with those?

Helmholz: Well, in '37-'38 and '38-'39, I took Lawrence's electricity and magnetism and Birge's optics course and a course in thermodynamics. I missed quantum mechanics. There was an older professor named Williams who gave a course in quantum mechanics. Then Oppenheimer gave his course in quantum mechanics and all the theoretical students were in Oppenheimer's course. Williams had three or four of us who were experimentally inclined and interested in the subject. I really never learned quantum mechanics very well, or what quantum mechanics I've learned was started then. There was a course that Brode gave on the development of atomic physics. I'd say I probably took three courses in each of those two years.

We had this system of preliminary examinations first in mechanics--that's why I took the course in classical dynamics that Lenzen gave. We had an exam in that--it was an oral exam--and then an exam in optics emphasizing electromagnetic theory. Then there was another examination in electricity and magnetism which covered special relativity and, finally, an exam in thermodynamics and statistical mechanics. One had to pass all four of these oral examinations. You didn't have to pass them before you started research, but they were a requirement for a Ph.D. and a requirement for what we called being advanced to candidacy.

You could take them at almost any time because there were so few graduate students. I think there were maybe seventy or so. You could just go to Professor Birge, who was the chairman, and say, "I'd like to take the mechanics exam," and he'd get a hold of Professor Lenzen, who usually ran the mechanics exam, and say, "Well, you schedule it with Professor Lenzen and then you can take it."

There was a fairly well-organized system among the graduate students for reporting the questions you were asked. We kept a notebook down in one of the teaching assistant offices, and I remember that as soon as I finished my mechanics exam, I went

down and wrote all the questions out. When you studied for the exam, you studied these questions. I think I finished them relatively rapidly. I probably didn't finish the fourth exam until the second semester of my second year, but I took the first one, which was mechanics, soon after Christmas. Christmas was the end of the first semester then. Soon after Christmas in my first year, I took one other one and then two again the next year.

Hale: Those would be a combination of a qualifying and a comprehensive exam?

Helmholz: Yes. They were examinations that covered everything from elementary mechanics--from the simplest things you could imagine in F=ma up through Lagrangian and Hamiltonian mechanics. It was remarkable that there was no exam in quantum mechanics in those days. It wasn't until after the war that such an examination was put in.

Hale: I noticed that there wasn't any specific course in nuclear physics?

Helmholz: That's right. You learned some nuclear physics in the course that Brode gave about development of modern physics, but not much. You really had to study that on your own.

Hale: You were occupied with that from day to day if you were in the Rad Lab.

Helmholz: I don't think other students learned very much nuclear physics except as it brushed off on them from talking to other students. I was interested in spectroscopy. Jenkins gave a course in spectroscopy, and he gave a term paper to write. I wrote on the determination of nuclear spin from hyperfine structure. I never learned much about discharges of gases. When we came to the war work we had to deal with arc discharges in uranium, and I wished I'd learned something about it.

Hale: Had there been a course like that?

Helmholz: Yes, they had a course like that, and Loeb had quite a number of students who were working in that field.

Lawrence as Lecturer and Traveler

Hale: What were Lawrence's overall attitudes to the lab and the cyclotron at this time?

Helmholz: I guess I'm not sure exactly what you mean, but the lab must have been almost his complete interest.

I occasionally meet people who took courses from him. When he was first here he gave the elementary physics course for biology and premedical students. They usually say to me, "Oh, when he got on something he was interested in, he was a terribly interesting lecturer, but there were lots of times when he was not very well prepared, or he would wander off on to something which he was interested in and not cover what was in the book."

Hale: The lab really occupied his time?

Helmholz: The lab, I think, really occupied his time. That was certainly his main interest. When I took his course in electromagnetism, he came in the fall semester pretty regularly. He did miss a few lectures. Then, when the spring semester came, he said to Birge, "You'd better have somebody else give it because I'm not going to be here enough to give it." That was in '38, and I don't think he taught much from then on.

The laboratory was his really great interest, and, of course, when the war in Europe started, he was very much concerned about that; when the United States got into the war, he was very much concerned about that.

Hale: Was he most interested in improving the machine or in doing the physics?

Helmholz: Well, by the time I came, I think, a lot of his interest was in the machine. I think he had earlier been more interested in the physics, but I had the feeling that he wasn't as much interested in the physics as other people, McMillan and Alvarez, were or as I was or Snell was or some of those people. He was obviously well acquainted with the problems. When Yukawa's suggestion about the meson came out, he realized that this was something very important for nuclear physics. One of the purposes of the 184-inch cyclotron was to try to produce these mesons.

Hale: Did you ever get a sense that his very involvement with his work was hard on his wife, his family?

Helmholz: Well, yes. It was hard on them, there's no question about that. He was, I think, very good about including people from the laboratory in his social life as much as he could, and I used to see him a little more than most of the other graduate students just because I could play tennis. I think there's no question that his wife and his children did suffer from his not being

home a lot and the irregular hours he had to keep. He'd go down to the laboratory lots of times in the evening and sometimes stayed quite late at night.

My wife and I know Mrs. Lawrence quite well, and she realizes, I think, they had a very happy marriage, I do know that. I don't think I've ever really talked to his children about it, but I don't think that I've ever heard Mrs. Lawrence say that the children really suffered from this lack of attention.

For a number of years, he had a boat which he used to invite people on. For a while he kept it down here at Berkeley, and he would take this boat up to the Sacramento River, and it was a very pleasant time. I remember that my wife and I went one weekend, I think we went all the way to Sacramento that time, slept on the way and then were in Sacramento in the morning and then came back in the afternoon. The thing I think that most people recognize about him was his tremendous verve and enthusiasm for what he was doing.

Outstanding Graduate Students

Hale: In February '38 Lawrence recommended you for a University Fellowship and then a similar one for the following year, a different one. Were you well aware of his opinion of you?

Helmholz: Well, I think I knew that I stood somewhat higher than some of the other graduate students. I never knew, for example, how he felt about me relative to Robert Wilson, who, it turned out, was probably the best graduate student at the lab at the time. Chien-Shiung Wu, of course, turned out to be a very distinguished graduate student. No, I wouldn't say that I really knew. I asked him to write when I was applying for scholarships, and I knew that my last year I won the Whiting Fellowship. That was sort of a special physics department fellowship, and I think I probably thought that was just a happenstance that I won that. Other graduate students won other fellowships.

Hale: Well, apparently Lawrence did think of you as his top graduate student, unless he said that on every recommendation.

Helmholz: Sometime you should ask Robert Wilson, who knew a lot about the history of the laboratory. He's the director of the Fermi National Accelerator Laboratory in Chicago. Robert Wilson had

done a very fine piece of work investigating how the cyclotron works from the point of view of electrostatic and magnetic focusing of the ions as they go from small orbits to large orbits. I'm sure that Lawrence thought very highly of this because he, of course, was very much interested in how the machine worked.

Hale: Lawrence also thought that you were very well endowed both experimentally and theoretically. He felt that you had a good theoretical background.

Helmholz: I think I was probably a little more interested in some of the aspects of theory than some of the other graduate students, although it wasn't a remarkable difference. I never knew exactly how he felt about Chien-Shiung Wu. She was very much interested and also very well acquainted with theory.

Hale: Did Lawrence feel okay with a woman graduate student, for example, or did he think of her in different terms than men graduate students?

Helmholz: I really don't know. He certainly had no problem. There wasn't anything that any of us could see that would indicate that he was uneasy with her or anything like that. But I suspect that he never knew her as well as some of the people. She didn't work on the crew in the laboratory. She was there a fair amount of the time, but she did not go down on these Brobeck evenings and repair the cyclotron.

Hale: I had the impression that she took as much a hand in that as anybody else.

Helmholz: No, I don't think that's true. We all knew her and knew her very well, but I think I'm right in saying she wasn't a regular member of what we called the "crew." There were two people assigned to eight [o'clock] Tuesday morning to four Tuesday afternoon, and another two people for Tuesday afternoon to midnight, and another two people midnight to eight in the morning. I don't think that she was on that kind of a group.

Hale: Did she want to be or was she excluded?

Helmholz: When Segrè gets back, you might ask him. I hadn't really thought about it until you brought it up.

Hale: You probably participated in the department colloquia?

Helmholz: Yes. The so-called department meeting was on Wednesday afternoon, as it still is, and when I got my degree, 1940, I gave one of the department meeting talks.

Hale: I see. That would be at the time when most graduate students would do that?

Helmholz: Yes. Members of the department and almost all visitors would give one of those department meeting talks, and my recollection is that maybe a third or a half of the graduate students would give a department meeting talk sometime in their career.

Prewar Rad Lab Staff

Hale: Who contributed most to the running of the lab, do you think, during this period, other than Lawrence?

Helmholz: Well, certainly, as far as the running of the laboratory, Donald Cooksey. He took care of just about everything. When we needed new equipment, he would order it. We had a machinist in the lab, a fellow named Andy Harvey--I guess he'd come to the laboratory at the time of the WPA--and Cooksey would always tell him what to do and what to make and so on. When insulators broke, Cooksey would be there to see it was properly changed and repaired. Cooksey was really a big part of it. He didn't take much pride and I don't think he was particularly interested in the nuclear physics. That is, he didn't have experiments of his own that were going. As I remember, he got his Ph.D. in something like X rays at Yale; he had known Lawrence at Yale.

As far as the scientific things in the laboratory, Alvarez, McMillan, and Snell were sort of the leaders. There was Ben Voorhis, I should look up when Kurie was there, because I don't remember Kurie a lot. Then, when Segrè came in '38 he became a leader.

Hale: What major visitors do you remember from your graduate years?

Helmholz: Well, not a lot. I don't remember very much about their scientific involvement in physics. I remember more of the theorists. I think I mentioned that Biscoff and Placzeck and Bethe were here. Compton came a number of times. Sid Barnes was here. There's a fellow from Chicago named Hoag, who spent a semester or a sabbatical here. Rabi came a number of times, and Bloch from Stanford. I was trying to think if many people from Caltech came. I don't really remember them. I think I had met

the Laurintsen's who were at CalTech, perhaps at meetings. I remember people from Michigan who came. I remember Cork who had something to do with building the first Michigan cyclotron. There was a fellow named Kreuger who came a number of times.

[Interview 4: August 5, 1975]##

Hale: How many people were around the lab, say, in '37 and also then by about '40?

Helmholz: I would place it at about ten graduate students and three professors: Lawrence, Alvarez, and McMillan. Three or possibly four postdoctoral people such as Snell, Van Voorhis, and Thornton. There was Brobeck and, at the beginning, only one person in the shop. Later on, by 1939, we had three people in the shop. And then in 1939, I think, Winfield Salisbury came, so that would make fifteen to twenty.

There was always the question of whether one should include John Lawrence and Joe Hamilton and some of the other people in the medical field who were active as soon as the 60-inch opened up.

Hale: Can you remember how many there were, say, at the end of 1940?

Helmholz: By the end of 1940 that had probably increased by ten or so--that is, before the work on the separation on uranium isotopes started. There were more people working on the 60-inch, and the number of the 37-inch stayed about the same. The 60-inch was operating, I recollect, twenty-four hours a day then also. The number did increase.

Hale: Now, were there, to your knowledge, any staff lists kept at that time? Did anybody bother to keep staff lists?

Helmholz: Not that I know of. They would have been in Professor Lawrence's record, I think, or Don Cooksey might have had them. I hope that the log books are still available. On both the 37-inch and the 60-inch, there were books kept of what was being done and how the machine operated and what troubles were encountered. Usually, on each one of those entries for an eight-hour shift, there would be the names of the crew. If one looked through those things, one could get the total number of people. Of course, this would not give the number of people involved in some of the medical experiments, but one probably could count up in that way.

Hale: When were things like the crew cards introduced?

Helmholz: Well, I'm not sure. That may have been during or after the war. I remember the log books, but whether the crew cards were introduced when the 60-inch started or whether it was in the middle of 60-inch operation during the war, I just don't know. The best thing to do would be to look them up.

Hale: Did organization at the lab increase perceptibly while you were there?

Helmholz: Oh, imperceptibly, except for the fact that the 60-inch started. The 60-inch had to be organized as well as the 37-inch. After December 7th [1941], and even after the separation of uranium isotopes started, there was more organization; one could notice that. It doesn't remain in my mind that at a certain time things got terribly, terribly organized. After the war started, there were people who came around and talked to us about security and things like that. There was more of a sense of secrecy and a sense of urgency at that time.

Hale: Now, you arrived when the 37-inch conversion was going into service. Were you immediately pressed into service on the 37-inch?

Helmholz: Yes. I remember that Luis Alvarez had written to me before I came. I was back in my home in Minnesota a month and a half before I came out here. He had written to me that the conversion was complete.

Hale: Who made up the schedules?

Helmholz: Don Cooksey would have been the arbiter, but all of us who were working would meet on Monday nights after the Journal Club, and we'd make up the schedule for the next week. Each person expressed his own desires, and if they couldn't be met, he'd bargain with somebody else as to how his crew assignments would be given.

Hale: Was it Alvarez that was responsible for overseeing the 37-inch conversion?

Helmholz: No, I wouldn't say so. I think Brobeck for the mechanical things.

Hale: He came about the same time as you?

Helmholz: He was there when I came. It could have been six months after I started there, but he was around, and he would say, "Well, that pump down in the basement ought to be serviced. You ought to go down and look at the oil and see whether the oil ought to be

changed." He was really active in the mechanical operation, the engineering aspects of the operation.

Hale: He told me that the thing was essentially done before he came.

Helmholz: Oh, it was operating, but he worked himself into an important place very soon, because he knew about mechanical things. I always had the feeling that Cooksey was really the man you would go to see when something went wrong, unless you knew that it was the particular bailiwick of somebody else. I wouldn't have said that Alvarez was in charge in any sense. Obviously, Ernest Lawrence was in charge, and Cooksey was his second in command.

Improving the 37-Inch Cyclotron

Hale: What can you remember of the process of getting more beam and higher energy during the first few months of the operation of the 37-inch?

Helmholz: Well, I can't remember a great deal because the 37-inch, as I say, was operating when I came. There was always an attempt to improve. I have no idea whose idea it originally was, maybe Wilson's or Cooksey's or Ernest Lawrence's, but there was the idea of putting in magnetic shims--taking small pieces of iron and putting them around in different places in the small air gap that was available. I think the air gap must have been three-eighths of an inch or so between the pole face and the top of the vacuum chamber. We put the shim in one place and then tested the beam, and then in another place and so on. At one time, there was an improvement by putting in some pieces of iron that were shaped in circles. They were discs, with the smallest disc being right in the center and then ever-increasing diameters. The field needs to be a little bigger at the center. There were certain places that we found a little extra magnetic field would help by putting in some iron at that place.

There wasn't anything very drastic in those days. It may be that by looking at the old log books, one could find something, but it was mostly fairly straightforward: make the deflector hold voltage a little better and improve the vacuum.

Hale: How soon did the operation of the cyclotron become relatively formalized through checklists and start-up and shut-down procedures, that sort of thing?

Helmholz: I always assumed that it was mainly Brobeck who would have formally organized the checklist of things to look at--make sure all the pumps had plenty of oil in them and the belts were looking all right and so on--but that was only the sort of formal aspect of the operation.

Hale: There weren't procedures for starting up and shutting down?

Helmholz: No. Most everybody knew those. When we let down to air, we didn't try to let in air that was dry. We just opened the vacuum systems, and there would go all the air in. The 37-inch was really never run like that until after the war. During the war, the 60-inch operated with a checklist. You first turn on the filament, and then you let the hydrogen in, and then you strike the arc, and so on. My guess is also that probably it was just that there were less skilled personnel available for running the 60-inch during those days.

Hale: During the war?

Helmholz: Yes, during the war. My recollection is that people learned very quickly. They had this list of things they were supposed to do, but they never even looked at the list after the first week. They knew the order in which things were done.

Hale: What of Salisbury's attempt to stabilize the oscillator?

Helmholz: I don't remember a great deal about that. I remember Salisbury very well, but I was never much of an expert with the oscillator.

Hale: Did you have any particular bailiwick?

Helmholz: In about 1938, Ernest Lawrence got the idea that maybe we were using a lot of heavy water to make deuterium and that we were using a lot of deuterium. He suggested that we should try to recover deuterium from the gas that was pumped out. So I made up a system where the gas should be run over copper oxide. This would make the H_2 combine with the oxygen from the copper oxide to form water and then it condensed out. It must have been a year and a half I worked on that. The water was more deuterium oxide than ordinary water, but it was never enough enriched to turn around and use again. I could have electrolyzed it and made hydrogen gas for the operation of the cyclotron. That was the main thing that I spent time on; otherwise, I was just an all-around person, I guess. I suspect that Brobeck had people he knew who were very good at vacuum joints and with the repair of the oscillator and with the repair of the ion source and things like that.

Building and Running the 60-Inch Cyclotron

Hale: What do you remember of the building of the 60-inch?

Helmholz: I didn't participate in the building of the 60-inch. I remember about it because the building was already there when I came in '37. They were working constructing the magnet. One of the things I remember was the casting for the vacuum system. Until the machine got operating, I didn't take any active part. I certainly went in there and watched what was happening, and when the operation started I took part in the crew activities. I don't remember anything special about the construction other than Thornton's problems with the vacuum system.

Hale: Brobeck mentioned that there were lots of discussions and give-and-take about what the specs were going to be and how it could be built in a more dependable way. Did you participate in that?

Helmholz: I really didn't participate in them. My guess would be that Alvarez, McMillan, Brobeck, Thornton, and Cooksey would have been the chief people involved in that. Dale Corson came in at that time, just about the time the 60-inch started. And Segrè was there. But I really didn't participate.

Hale: When you were helping run the 60-inch, was it on producing some particular isotope or something like that?

Helmholz: Well, it was really running the machine. There was a schedule for twelve o'clock Sunday night until six o'clock Monday morning. You would run to make phosphorus or something like that.

Hale: Would it require continual attention, or could you just sit back and do some reading?

Helmholz: Well, there were usually, as I remember, two people there. One person could read and the other person would keep watching the beam meter; one had to adjust the magnetic field every so often. It wasn't automatically adjusted to keep the maximum beam. Then, of course, if something happened, they had to get out and do it. There were all sorts of interlocks. If a red light on the control board went off, then the machine would turn off. Maybe it was the water for the oscillators or something like that. Then you'd have to go and find out what was wrong with the water system for the oscillator. It wasn't exactly an

automatic operation, but when it was running smoothly, it didn't require any more than one person to watch it.

Hale: That pilot light is one of Brobeck's contributions, wasn't it?

Helmholz: Yes.

Hale: He said how he was very interested in doing that sort of stuff.

Helmholz: I don't know what it was like before Brobeck came.

Lawrence's Character and Prewar Politics

Hale: Lawrence has been criticized by some people for making unfair, untried claims of great medical benefits from cyclotrons in order to get the funding he wanted.

Helmholz: I would say those things are always questions of judgment. You do make claims for things that you believe will turn out. When you think radioactive phosphorus is going to be a great boon in the treatment of leukemia, you naturally want to get as much money as you can to produce radioactive phosphorus. Of course, I didn't have anything to do with the agencies from which he was trying to get the money, but I didn't feel that he was trying to pull the wool over anybody's eyes, and I didn't feel that he made too exorbitant claims.

Hale: No more claims than he truly, sincerely believed in?

Helmholz: Yes, that's right. I think he was a very honest person.

Hale: Well, that's the feeling I've gotten.

Helmholz: I'm sure some people didn't agree with him. And he certainly was very hopeful that the neutron beam from the 60-inch could be used to treat certain kinds of cancer. He sincerely hoped that it would be an effective treatment. Well, it just didn't turn out to be that way.

Hale: Many people who knew him mention how very fair he was in distribution of credit but imply, on the other hand, that he could be very quick to share blame--for example, in the neutron mass mistake.

Helmholz: I didn't have much occasion to run into this. During the war, there was a lot of pressure, and when things turned up that we

should have seen earlier--about the operations of the mass spectrograph or something like that--he came down fairly hard on people. But he was also very fair in saying, "Well, gee, why didn't I think of that, too?!" He blamed himself as much as other people. He didn't refrain from blaming people. When he felt that we hadn't done a very good job, he would say so. But people that I knew didn't really hold that against him. Usually, if they felt that his blame was unfair, they would speak up. I'm sure there were younger people who didn't speak up as much as they might have, but a good many people would speak up and say, "Well, we don't agree with you." And he accepted that pretty well.

Hale: I read an article that Marcus Oliphant wrote on Ernest Rutherford. Rutherford loved dirty stories which Lawrence, you know, would have none of. Now, if anywhere, one would curse on the tennis court. Did you ever observe that?

Helmholz: I've forgotten what words he used when he made lousy shots, but they weren't the usual profanity. He was not at all a profane person. I don't mind profanity, although I don't use a great deal of it. But I think that that was a perfectly justifiable comment of Oliphant. I never knew Rutherford well enough to know what he used in the way of profanity.

Hale: Do you think Lawrence was really a sort of a puritan?

Helmholz: That's a good question. I think he was a good member of the people who grew up in South Dakota, and they were more puritanical than most of the rest of the country. I don't think it was a great part of his character. I think if he'd grown up in Kentucky, he probably would have been a little different; he probably would have done about the same thing, but he might have sworn a little more and told a few more dirty jokes. I don't think that was an essential part of his character. It was the thing that would strike people. It was just that he'd grown up that way.

Hale: Not being able to understand people's distaste for loyalty oaths is characterized as apolitical. Would you say he was apolitical?

Helmholz: Yes, I think he was apolitical. He was conservative insofar as he had to be political.

Hale: He voted for FDR [Franklin Delano Roosevelt], didn't he? He was persuaded by Oppenheimer?

Helmholz: Yes, I wouldn't be surprised if he voted for FDR. And it wouldn't surprise me that he was convinced by Oppenheimer. I had a feeling that he tried to remain aloof from Oppenheimer in things nonscientific, but maybe this was after the beginning of the war. Maybe somebody like McMillan or Alvarez would have a better idea of that.

Hale: I have a feeling that people who are apolitical can act in an ad hoc manner about issues.

Helmholz: Up through 1941, you'd probably call him conservative, but not ultraconservative. As the war got started, I think one would say he was very pro-government--throughout the war and after the war also.

Hale: Patriotic?

Helmholz: Yes, I think so. I think he had no real understanding of why people were against the loyalty oath. Somebody could say, "This is what happened in Hitler's Germany," and he would understand that. But it was too much of a jump for him to say, "Our government is getting to be like Nazi Germany." He just couldn't understand that. He knew so many people in our government.

Hale: He was moving in very much the upper echelons of government society and the big financiers?

Helmholz: Yes. I don't remember how he felt about the argument over the Atomic Energy Commission. After the war, there was the May-Johnson bill to put atomic energy under the army, and then there was the McMahon bill, which was eventually passed, setting up the AEC. I wouldn't have been surprised if he had been in favor of putting it under the army.

Hale: Did he have moral doubts at any time in his pursuit of the bomb, like other people?

Helmholz: I have no idea about that. I don't think I ever heard him talk about it. We never talked around the laboratory about the bomb. I'm sure he talked with a few people in Berkeley about the bomb. It certainly wasn't known, except to a very few people, that the bomb was about to go off in the Alamagordo test.

At the beginning, he had said to all of us, "This is a new source of energy, and what the energy might be used for, we can't say right now." I didn't really know for sure. I mean, when you think of energy, you think of bombs, but I didn't know for sure that the bomb was in the Alamagordo state during 1945.

If you had said, "What's the situation?" I would say, "It must be getting close to something, because they've got a fair amount of uranium available."

Hale: You knew that that's where the uranium was going?

Helmholz: Yes. Sure.

If somebody had told me, "We can't use it for a bomb, but they're making some source of energy that will drive a steamship or be useful in some other way," I wouldn't have been terribly surprised.

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Hale: How would you characterize Lawrence's relationship with Oppenheimer during this period?

Helmholz: Up until the war started, they certainly did socialize some. Some of his contemporaries would be a lot better judges of that. Around the laboratory, when Oppenheimer would come in, they would always talk just about physics. They would sometimes talk about some other things. During the war, Oppenheimer was gone all the time. I'm sure that Lawrence was at Los Alamos a lot more than Oppenheimer was ever in Berkeley.

Hale: Lawrence obviously had a lot of confidence in Oppenheimer.

Helmholz: Lawrence certainly thought very highly of Oppenheimer's scientific ability. I never had any impression before Oppenheimer's fall from favor that Lawrence ever doubted his loyalty or even his good judgment. It must have come out during those war years that Lawrence knew about Oppenheimer's problems with security. I never heard him talk about it, and I have no idea of how he felt about it.

Hale: Many "Rad-Labbers" had their own income apart from the fellowships, especially early on, and many came from good families. Lawrence seemed to be a little hung up with people's pedigree. In a recommendation he made somewhere, that seemed to be a thing that impressed him more than anything. Do you think he was inordinately impressed by pedigree?

Helmholz: Not inordinately. He was a little more impressed than if he had been living today. He was reasonably straightforward about it. Perhaps he paid a little more attention than the average physicist would have. On the other hand, certainly not more than the average banker would.

Hale: His background wasn't particularly lowly. Do you think he had an ambition to get into society?

Helmholz: Oh, I suspect he did have that ambition. It's hard for me to judge whether this was something that came along with the war years. I have at least one point of view: up until the war in Europe started, he really was solely dedicated to the cyclotron and the various applications of the cyclotron, like the treatment of cancer and the treatment of leukemia with phosphorus and use of radioactive materials. Then, when the war came along, he had these other interests--radar and nuclear energy--which took him into higher governmental and social circles. It may be that at that time he was captivated by that. Undoubtedly, you would have to say that there was some predilection towards this captivation. I do think that in the later years he became less of the pure scientist and more of the governmentally-inclined scientist. He was the go-between between the government and science. He certainly retained a lot of his scientific insight and interest, but he didn't follow the meson theory. Most of the rest of us who were in teaching and doing research with graduate students did.

Hale: I wonder whether he turned away people from the Rad Lab who didn't have funds. Maybe this produced a slightly exclusive tenor in the lab.

Helmholz: I never had the feeling when I was working in the lab from '37 to '40 that it was populated by people a little better off than other graduate students. Some of the graduate students in the laboratory were paid by the laboratory. It wasn't very much, I know, but neither did the teaching assistants have very much at that time. I think that he realized the financial problems which graduate students had.

Hale: Lawrence became increasingly preoccupied with seeking funding for the lab. Do you think this had a great effect on the philosophy of "Big Science"?

Helmholz: Well, sure. One could have looked at the 60-inch or even the 37-inch in 1938 and compared it with the Cavendish or any of the other nuclear physics laboratories. You would say, "Gee, that's really different. Here are all these graduate students and research people getting together and all working on this one machine. They all get together on Monday nights and repair the machine, and they have regular crew periods throughout the week." To me, that was sort of the beginning of the big times. It was just a natural evolution from then on. You couldn't have thought of the 184-inch without realizing that it was going to

be a tremendous operation relative to anything that the world had ever seen in the way of an operating scientific equipment.

One could say that getting Brobeck into the Radiation Laboratory was really a big step. Scientists had, I'm sure, used mechanical engineers before in helping them design equipment and so on. But, outside of the army or the navy, it might well have been the first time in a physics laboratory that a mechanical engineer had been hired full-time to make the machine work. The so-called "big science" which Lawrence's name is very properly placed with was not a sudden inspiration; it was just a natural development.

Prewar Nuclear Physics

Hale: Your own work started with nuclear isomers. What was the importance of this to the nuclear physics of the time, '37 to '40?

Helmholz: Well, it was a problem in nuclear structure, really. Why are there isomers and how can one explain isomers? Oppenheimer had a couple of students named Morrison and Danoff who had done some theoretical work on the internal conversion of gamma rays. I mentioned another student named Nelson who pointed out that gamma rays from highly forbidden transitions--maybe with nuclear spin changes of three or four units--would have a high ratio of L conversion electrons to K conversion electrons. Well, these problems were current in nuclear physics at that time, and so I was interested in looking at them from the sort of experimental point of view. It was one of the things that led to the shell model. The shell model was terribly important in understanding why isomerism occurred in certain regions of the nuclear table. When I started working on isomerism, there wasn't any idea of the shell model. It certainly was the big breakthrough in understanding isomerism.

Hale: You also found new artificial radioactive elements?

Helmholz: Yes. Everybody was interested in that. When a bigger accelerator was built, you could probably find a lot of new isotopes, and so everybody did this. It was terribly interesting to see what kind of isotopes one could make and what their properties would be.

Hale: Did everybody take a little bit of the periodic table to fiddle with?

Helmholz: I don't remember that we ever sat down and divided up the periodic table. You knew if somebody was working on bromine isotopes--that was Snell when I first came--that you wouldn't pick that out as a place that you'd want to work. When I got into that little bit with radioactive yttrium, I happened to be around when somebody mentioned that there was a strong gamma ray emitter. I said, "Let me look at it."

Hale: How much of your research time would be given over to that as opposed to your thesis work?

Helmholz: I would have to look up that radioactive yttrium to remember exactly whether I started it before. I probably was doing it simultaneously with writing my thesis. It wasn't taking a tremendous amount of time; it was a straightforward, interesting problem. After I did have my thesis written, or when I was fairly nearly done with it, I did spend a little more time with that radioactive yttrium. The gamma rays were so strong--they were over a million volts--and had a fairly long lifetime, ninety days or thereabouts. One could use that as a strong source of gamma rays. Cobalt 60 is now a much better source of such gamma rays, but in those days one didn't have a lot of cobalt 60. You would ordinarily say, well, high-energy gamma rays will occur with short lifetimes, because there's so much energy difference between the initial and the final state of the nucleus. Yet this was a case in which for reasons of the nuclear structure of yttrium 88, the lifetime was fairly long, but the gamma rays were fairly high energy.

Then, somebody suggested that this fellow up in Mare Island might be interested in high-energy gamma rays, so I went up and saw him about it. There were just lots of interesting things happening in those days.

Close Associates

Hale: What were the people like to work with? Any particular friendships?

Helmholz: Segrè worked very closely with Miss Wu. They had rather similar interests. She was a student and he was at that time sort of a research associate of the laboratory. He also helped me some because he was interested in the isomer problem.

Abelson was really working with fission even though he didn't know it, and everybody knew about his work. I wouldn't have said that he was particularly associated with any one of the faculty members. There weren't any cliques of faculty or of graduate students. It is true that Loeb's students were quite separate from the students in the Rad Lab, and Loeb himself didn't participate much. He rarely came to these Monday Journal Clubs. He had his own seminar that he would conduct on Tuesdays, I think it was.

Hale: He was, together with Birge, very instrumental in getting Lawrence to come here originally. I've read or heard that he was intensely jealous of Lawrence.

Helmholz: I think he probably was. I used to play squash with him occasionally. He never spoke that way. He would mention Lawrence very little, but always complimentary when he did. He was just in a different field. I don't know whether he ever even thought of doing something in nuclear physics himself; I doubt it. He was very much interested in discharges in gases, and he just stuck to that. Brode and Jenkins and White did maintain an interest in Lawrence's kind of work and used to come to these Journal Clubs. And, of course, Oppenheimer did also.

Hale: I thought I found out from somebody that Loeb encouraged students not to go to work for Lawrence.

Helmholz: That could be true, I think, but I just don't know. You might go and talk to Art Kip sometime. He was a student in those days. You might get an outsider's view of the Rad Lab from him. He must have gotten his degree about '38 or '39, I think, and he would have been both an undergraduate and a graduate student here. He worked with Loeb. When he left here, he went to MIT; he came back in about 1950. By that time, he'd got to working in solid state physics.

III WORLD WAR II ON THE CAMPUS AND IN THE LAB

View of Events in Europe from Berkeley

Hale: Were you affected by the events in Europe during those years? People generally talk about the war starting much later here than it did in Europe.

Helmholz: I think it depended a little bit on whether you had ever been to Europe and perhaps whether your family had come recently from Europe. I was concerned about it, and I suspect more so than a good many of the people in the Radiation Lab, although you couldn't help but be concerned when you saw Ernest Lawrence spending a lot of time in the East helping to set up the radiation lab at MIT. When we saw McMillan and Alvarez off to do war work, we became a lot more concerned than most American citizens were at an early time. We were very aware of what was going on.

Hale: What was the political climate in the lab? Was there much discussion of politics?

Helmholz: Not a lot. I think probably even less than there is now. Sure, we talked about politics. Whenever a presidential election came along, we would have a great discussion about who the good candidates were, but there were no hard feelings about it at all. There were some Republicans and some Democrats around the lab. I don't have any remembrance of who voted for Willkie in 1940 and who voted for Roosevelt. I'm sure there were some on each side. I had grown up with perhaps a liberal view toward politics. Maybe some of the other members in the lab would have felt, oh, yes, they could remember that this fellow was pro-Roosevelt and this fellow was pro-Willkie. I just didn't place that much importance on it.

Hale: What were the lab's relationships with the physics department?

Helmholz: They were very good. Lawrence had his office over in LeConte Hall where the departmental offices were and where all the professors had their offices. It was really very close. It wasn't at all like the Rad Lab up on the hill now, the department down here. It was a one-and-a-half-minute's walk over to the Rad Lab. The relationships were very good. Except for the isolation of Loeb's group, we were all graduate students or the faculty was all faculty. We all went to the department meetings on Wednesday afternoon, and most people came to the Journal Clubs on Monday nights. We all took the same courses, all the graduate students. It was a good feeling.

Wartime Changes

Hale: Can you give me a general idea of how the transitions of the war years went?

Helmholz: Well, as I mentioned before, sometime before December 7, 1941, faculty members left to go into war work, McMillan and Alvarez. I can't remember whether other people from the Rad Lab left.

Hale: That's well documented.

Helmholz: I got my degree in the fall of '40, and my appointment for the year '40-'41 was at the lab. About in September, Brode got called back to Washington. I guess he went to work for the Bureau of Standards at that time.

Hale: You were then going to be paid by the lab?

Helmholz: Yes, I was going to be paid by the lab. Birge and, I guess, Lawrence were looking around for somebody to take over Brode's courses. They struck on me. I think he was teaching two courses that fall; I guess I took over one of them. And I kept teaching from then on. Brode didn't come back until the end of the war.

It was already apparent that the war was on as far as the physics department went. In the fall of '41, we started to work on the separation of uranium isotopes, and that made it change away from nuclear physics. I've really forgotten whether I did any nuclear physics after that change. I'm sure maybe I did a little, but as soon as we started to work on the uranium isotopes everything changed over to the separation of the uranium isotopes.

I would guess that some of Loeb's students stayed working on discharges in gases for a while. Brode had a few students; I can't really remember what happened to them. I think that they kept working on the cosmic rays. Powell was here then. He probably helped to direct them.

The numbers of graduate students certainly decreased, because people graduating in physics didn't go to graduate school; they went into war work of some sort. The department kept going, and there were some graduate students during the war. But a lot of the graduate students worked in war work; they had an awful time getting degrees at the end of the war because the things they wanted to use for theses were classified.

I suppose in a way I can say that the physics department was aware of the war and became involved in the war a lot sooner than the rest of the university. Maybe not sooner than the chemistry department, but certainly there was much more awareness of it in the physics department than in other parts of the University.

Hale: Did security suddenly become tighter during the time? Limitations on visitors and things?

Helmholz: Yes. Soon after December 7, we had security people coming out here and telling us what we could say and what we shouldn't. I can't really remember the security in the 37-inch, but the Donner Laboratory had recently been built. The plans began to be made for the use of the 184-inch as a mass spectrograph, and the building over there was where a lot of the engineering work was being done. It became sort of a classified building.

Genesis of the 184-Inch Cyclotron

Hale: How long was Lawrence talking about a much bigger machine than 100 MeV?

Helmholz: I would guess something like 1938, but I really don't know. I think the rest of us knew that there were thoughts along these lines, but as far as dates go, I really have no idea.

Hale: Well, I've got a feeling it was probably the beginning of 1939, anywhere from fission on. He probably got serious about it from May of that year. And by then Brobeck was putting figures down on paper.

Helmholz: The talk about a bigger machine wasn't really in any sense indicated by fission; it was all indicated by Yukawa's idea about mesons. In other words, the 184-inch was designed with the idea that if there were such things as mesons, one would be able to produce them artificially. Until Lawrence got the grant from the Rockefeller Foundation, I'm sure there was a fair amount of talk, but not much sitting down and working on it.

Hale: Who was in his confidence about his larger designs, his philosophy about that sort of thing?

Helmholz: Well, my guess would be Cooksey, and then Alvarez and McMillan and Brobeck.

Hale: Brobeck gave me a feeling that he really wasn't on the inside of Lawrence's confidence, not the same way as Cooksey was. Loomis was here about that time, right?

Helmholz: Yes, [Alfred L.] Loomis was around a fair amount, and Arthur Compton was around some. It may well be that in those things Loomis and Compton were more important. Cooksey was probably the best one to comment on this.

Hale: What part did Loomis play in encouraging Lawrence?

Helmholz: I never knew Loomis that well. I'm sure that he was encouraging to Lawrence because he felt quite strongly that Lawrence was an important scientist, and he wanted to see science develop. Exactly what he contributed to Lawrence's development, I just don't know. Certainly the financial support that he was able to give must have been very helpful.

Hale: I have a feeling that the people most responsible for designing the 184-inch, getting the designs down, after the initial broad specs were fixed, were Don Cooksey and William Brobeck. Does that sound right?

Helmholz: I think that's right. As far as the decisions as to how you would build it--undoubtedly McMillan and Alvarez and Thornton contributed a good deal. I remember, just as an example, that it seemed like an awful foolishness, but the only way to do it was to have tremendous gaps if you're going to have a million volts between the dees of the cyclotron!

Design Problems of the 184-Inch##

[Interview 5: October 14, 1975]

Helmholz: There was also the relativistic problem with protons or deuterons or alpha particles. Since this was a conventional cyclotron, one had to accelerate the particles rapidly to the final voltage which was supposed to be of the order of one hundred million electron volts. One would have perhaps thirty or forty accelerations, several million volts for each acceleration. So the problems of sparking of the dees both to each other and to ground were very serious. In the conventional cyclotron, like the 60-inch or the 37-inch, one had at most 100,000 volts between the dees. This was an order of magnitude higher than that. Consequently, the magnet gap had to be very large and the spacings had to be very large. It was just figured that this was one of the things that we would have to overcome in order to get a conventional cyclotron.

About that time, there was the suggestion of [L. H.] Thomas that one could overcome these relativistic limitations by having a non-uniform magnetic field. I don't know what they called it at that time; it's called a sector-focused cyclotron now. My recollection of it is that it wasn't something that one wanted to take a chance on in a device of this magnitude. The decision was made just to go ahead with it.

Hale: Thomas's ideas were essentially ignored for quite some time, weren't they?

Helmholz: Yes, they were. After the World War, I didn't keep up with the sort of cyclotron construction enough to remember when they were finally adopted, but apparently they worked all right. It might be that that would have been a preferable thing to do.

Hale: His idea was a specific case of a more general principle.

Helmholz: The whole thing is really academic now because McMillan and Veksler came forth with the synchrocyclotron principle at the end of the war before the original design of the machine could be implemented. One further thing that I seem to remember is that Brobeck and Segrè did the design for the 184-inch magnet. They had a design in which they chopped off the outside corners of the rectangle of the magnet and put those corners on the inside. That gave them a little more uniform field. They had a magnet model; I don't know what scale.

Hale: It was a quarter scale, wasn't it?

Helmholz: No, I don't think it was that big. I don't remember who worked on that model of the magnet. I wasn't involved in that design. I don't remember who did detailed work about the radio-frequency system. I think the magnet was put in place and then the building was built around it. The building was pretty well complete by September 7, when the decision was made to use it for electromagnetic separation rather than for the cyclotron. I don't remember when the electromagnetic separation started, something like November of 1941. Lawrence must have been communicating with the people in Washington about the desirability of starting some work. First the 37-inch cyclotron was taken apart; the 60-inch cyclotron remained a cyclotron. The magnet of the 37-inch was used as a pilot device to see what sorts of uranium compounds could be used for the ion source.

Ion Sources for Electromagnetic Separation

Helmholz: One of the early things that I did was to try and find some way in which uranium metal might be used as the source of ions as opposed to the tetrachloride. This meant very serious problems; as soon as you heat up uranium metal, it becomes an alloy with any other kind of metal you are using. So one could not heat it up; one couldn't use tantalum or molybdenum. One of the suggestions was to use something like beryllium oxide, one of the oxides with a high melting point. It could be made into any sort of container form that you wanted. This just never worked out either. It was difficult, even when you've got the uranium into the oxide container, to bombard it in a proper way to heat it up and get a steady flow of ions out of the ion source. Uranium tetrachloride was used as the source.

Hale: You were doing this in very preliminary stages?

Helmholz: That's right.

I can remember December 7, was a Sunday. I have forgotten whether we were working Sundays then or not, but Monday morning when we all came to the laboratory, everybody said, "Well, now we are going to have to start working hard." There were a number of people involved in this early work. In the initial stages of the separation, we must have worked twenty-four hours a day, just because we wanted to try and separate small amounts of uranium 235. Then, some of the first-year graduate students started to work on this.

Hale: You could actually look at the chemical properties?

Helmholz: Segrè and his group--Clyde Wiegand, Owen Chamberlain, and George Parwell--they took on the problems of analyzing the collected material to find the fraction of uranium 235.

Hale: Were they chemical methods?

Helmholz: No. They would have to separate the uranium, but then they would use fission to find out. You have to weigh the sample to find out how much total uranium there is and then find out the fission range with slow neutrons. That would give the amount of U-235 as opposed to U-238. I can't remember whether the decision to move to the 184-inch cyclotron magnet as an experimental testing facility was made before December 7, or not. That was certainly one of the strong considerations.

I remember when the 184-inch magnet started to operate. The gap was about six feet. Even without any sort of shimmying of the field that was uniform enough over 4000 Gauss. There were two tanks that were used one on top of another in the 184-inch magnet to test out different designs of the ion source of the collector and to go through sizable enough runs so that one could collect material and analyze it.

Hale: Who took a major part in the preliminary design of the 184-inch?

Helmholz: I can't remember whether Dale Corson was here then in '40-'41 or not. I know that he had gotten his degree with Bob Brode in cosmic rays in about '38 or '39, and he spent a year or two at the laboratory getting the 60-inch operating properly. That was the time that he and McKenzie and Segrè discovered the element 85, and he might have done some work on the 184-inch. I really don't remember who else might have been involved. I think Livengood had left by that time, whether he went to the rad lab at MIT or somewhere else. Those are the people that I can remember.

Hale: Were you aware of the Rad Lab getting much useful feedback from other cyclotron labs, possibly that would influence the design of the 184-inch?

Helmholz: Well, I think Ernest Lawrence and Cooksey got all the information they could want from other laboratories. Maybe they had to ask for it, but--

Hale: Was it in the form that they could use? Did they actually use stuff without really giving credit for its origin?

Helmholz: I think, probably, some of the information was spread around by word of mouth among all cyclotron groups. There weren't that

many cyclotrons around. If somebody at Illinois found that using an ion source of one kind or another was a little bit better, would give you 10 percent more beam, everybody would know about it very quickly and probably everybody would use it. It's quite probable that nobody would acknowledge it in the literature. As far as the 184-inch cyclotron goes, I think that it was a device of such size that not much information from the smaller cyclotrons could be useful. I think, in some ways, a lot of the problems were met for the first time.

Hale: In most published accounts of people that I have talked to so far, it is almost implied that everything came from the Rad Lab.

Helmholz: Thornton is the one that I can remember had been at other places and so might have seen developments and sort of seen how they were brought back to Berkeley. I don't recollect any particular thing which I knew came from Michigan or St. Louis or something like that.

Hale: As far as you know, were there any design meetings or discussions where everybody contributed?

Helmholz: I am sure there were. I wasn't involved in them. I got my degree in 1940 and took a position at the lab as a research assistant. I probably might have got into the design of the 184-inch except that just a few weeks after the semester started, Professor Brode left Berkeley to go to Washington to do some war work, and they asked me to teach his classes. I had less time for the laboratory, so the work that I did was sort of helping to keep the 37-inch running and doing some experiments myself. Brode stayed away from Berkeley until the end of the war, and I kept teaching through the war.

Financing the 184-Inch

Hale: Did you have much idea of the projected cost of the 184-inch?

Helmholz: A million and a quarter had been given by the Rockefeller Foundation, and the Regents were supposed to contribute a quarter of a million dollars. These are the figures that I remember. And, of course, the site for the building.

Hale: Did you have any idea of the operating expenses of the Rad Lab of that time?

Helmholz: No, I don't think I really did. I perhaps did by sort of counting up the salaries of a dozen or so people who were employed by the laboratory and making a guess at the power bill. The power bill for any cyclotron is a substantial cost. I don't really remember thinking about it.

Hale: What, if anything, did you know of Wallace Reynolds and his increasing part in the financial side of the Rad Lab?

Helmholz: Not much more than that what he was doing. It was apparent when you thought about a million and a half dollars that somebody was going to have to do that job, and we knew when Reynolds came that that was what he was going to do. Of course, we didn't know at that time that the war was coming.

Hale: It wasn't until the war that he became really a Rad Lab employee.

Helmholz: I guess that is right. I think that he consulted with Lawrence and with Cooksey about the 184-inch.

Hale: Did you have any sense or feeling of the size of the operation beginning to get away from you at that time?

Helmholz: No, I don't think so. The 184-inch was a tremendous construction project and a tremendous research project, and everybody was very much excited about it. It was essentially the accident of the war that the laboratory became such a tremendous operation. But it was known, for example, that the cyclotron was going to be up on the hill and there would be some laboratory up there--that some of the work with radioactivity would probably continue on campus. I do remember that there was a fair amount of discussion about how much of the experimental work that could be done away from the 184-inch would nevertheless be done up there just because it was closer. Obviously, the things like scattering experiments and so on would have to be done up there, and work with radioactivities and so on could be done on the campus.

Hale: You had a feeling that there was more than just an accident of war that made the Rad Lab become what it was?

Helmholz: If there had been no war, the 184-inch would have been built on that old design and probably it would have operated all right. But I suspect that the development of high energy physics would not have gone along nearly as rapidly as it did from 1946 on. There was not the money available; also, the synchrocyclotron idea was a tremendous step forward.

Hale: Do you think it still would have become the same size operation that it is now?

Helmholz: I'm not really sure. After the war the 184-inch was built as a synchrocyclotron. Alvarez built the proton linear accelerator and McMillan built the synchrocyclotron. So there were three pretty good-size accelerators all at the laboratory. I just have a feeling if the war hadn't come between, the 184-inch would have been built. Undoubtedly, there would have been lots of problems in the operation of it. How long it would have taken for the idea of the synchrocyclotron to come along, then the construction of the electron synchrotron and proton accelerators, it's hard to say.

Hale: A very major part is the degree to which government funding played immediately at the end of the war. The 184-inch was completed on the coattails of the Manhattan Project.

Helmholz: How government funding would have been arrived at without the war, I don't know. Things would have gone much more slowly.

Prewar Research

Hale: Can you give me a brief rundown of what you consider to be the research highlights in the lab just before the war?

Helmholz: People in the laboratory did a fair amount of work with fission --Abelson and Segrè and Miss Wu. Then there were things like the discovery of element 85 in the 60-inch cyclotron. There was a substantial program to use neutrons to treat cancer with the 60-inch cyclotron; that went on for a couple of years. There was continued work in radioactivity; Seaborg was active. Along with the fission went the discovery of neptunium and then plutonium. I think those are the major things. There was a lot of interest, partly because the 184-inch was going to be built, in the cosmic ray results of why mu mesons apparently do not have strong nuclear interaction. That raised the question of why was the mu meson like it is when it should be a strongly interacting particle. The 184-inch was projected to make pi mesons, whatever was the intermediary.

Segrè probably has the best recollection of that period. He didn't start teaching as much as I did at that time. I think he taught a course or so in 1940, maybe every other semester. He also has a good memory about research events.

Now that you remind me or ask me questions about it, I really don't remember the details. I knew all the graduate students that were involved. I would have to go into the library and look up, for example, what Bob Cornock did his thesis on. He may have been the one that found helium 3 accelerating in the 60-inch cyclotron. That was kind of an interesting thing. Hydrogen 3 must have been discovered around that time. If hydrogen 3 was radioactive, then helium 3 must be stable. As I remember it, this was in the early days of the 60-inch. Alvarez must have been the one who suggested trying to put helium in the cyclotron and set it to accelerate helium 3 and look and see whether anything came out of it.

Hale: Did you have much to do with Segrè before the war?

Helmholz: Not a lot. I was working on some radioactive [samples], particularly with isomers in them, and he was interested in it. I got a lot of good advice from him, but we didn't do experiments together. I guess we never have done experiments together. Of course, I would see him almost every day at the laboratory.

Hale: What did you expect to be his attitude by this time towards the Rad Lab and Lawrence's style?

Helmholz: Well, I think he appreciated Lawrence's abilities as an organizer and a doer. I don't think there is any great argument about it; he never felt that Lawrence was the kind of a genius that Fermi was. Segrè was always a great backer of Fermi, with good justification, but I suspect that Segrè saw that the way Lawrence was running the laboratory was the most efficient and the best way to get things done. I'm sure he differed with Lawrence on some things: what was more important to do than something else. I don't remember his saying a great deal about the trend toward biological applications of radioactivity and of the neutron work and so on. I don't think he was particularly excited about it, but I'm sure that he realized it was interesting to do. It was worthwhile in view of the fact that it had more support for the laboratory.

Joint Loyalties to Rad Lab and Department of Physics

Hale: Let's go back to talk about your becoming an instructor in physics. You were paid by the physics department?

Helmholz: No, I think I was paid either half or two-thirds by the physics department and half or one-third by the laboratory. That continued up to the time I became chairman in 1955.

There weren't a lot of people in that category because there weren't that many in the physics department who were teaching, but after the war, when nuclear physicists and high energy physicists were added to the staff, a lot of them were added on a part-time basis. I think when Alvarez and McMillan came back from the war they were one-third time in the department and two-thirds time at the Rad Lab. I think Thornton and Powell were similar. I forget now about Chamberlain. Segrè was in that category for a while. Some of the newer members like Ypsilantis and Stevenson and Crawford were also part-time department employees and part-time Rad Lab.

Hale: The joint relationship with the physics department and the Rad Lab defined how people were employed? It must have had a great effect on the whole attitude towards physics.

Helmholz: Yes. The senior members of the staff at that time were Birge and Jenkins and Brode and Loeb. I think Lenzen took a less active part in the department affairs. They all just realized that this was a way of supporting research.

Hale: I got the impression that 1/3-2/3 arrangement came into being when the war became very important.

Helmholz: I don't really remember how the salary was divided.

Hale: Did you have any divided loyalty?

Helmholz: No, I don't think so. There was a tradition. Lawrence himself had taught part-time and worked in the laboratory part-time and McMillan and Alvarez had both done that. I don't know whether McMillan and Alvarez were paid part-time by the laboratory before they left for war work at MIT or not. Lawrence was paid full-time by the department; then, as the laboratory grew, he went on part-time. I think after he got heavily involved in things he wasn't on the department payroll at all.

Wartime Research: Electromagnetic Separation on the 37-Inch

Hale: You were involved with the Alpha program from the beginning. What were the main things that you did?

Helmholz: I got involved at the beginning with the separation using the 37-inch magnet. After most of the bigger part of the work moved to the 184-inch magnet, I stayed with a group of five or six, of whom Gordon Patton was one and a fellow named Oscar Anderson was another, and there must have been a couple of graduate students. We worked with the 37-inch on special problems like this question of whether we can have a container or not made of ordinary materials and still use uranium metal as a source of ions. I think we did some different work on types of collectors or ions, but my memory fails me as to the variety of things we did. I didn't move up to the 184-inch site immediately. I must have stayed down at the 37-inch, and it was appropriate since I was teaching all the time; it was easier to get there than go up the hill and come down again to teach.

It wasn't until the British group under Massey set up a rather very strong program on studying the details of the arc processes with your uranium tetrachloride that I went up to the 184-inch and started working there. Massey used the 37-inch magnet for those studies. I think they were very productive. I don't remember really that a lot was learned that was of tremendous value in the design and construction of the ion sources for the racetracks back at Oak Ridge.

Hale: So the British group was allotted the 37-inch for most of the war period?

Helmholz: I think so. I don't know the distribution in time, but I would say that in the years '43, '44, and '45 they were using the 37-inch. Massey and Burrup are the two names that I remember. There were several other people here; I think there was a fellow named Sayre who must have worked with them also. Kern and his wife worked at the 184-inch.

Separation on the 184-Inch Cyclotron

Hale: When did you move your efforts to the 184-inch?

Helmholz: My guess is that it must have been sometime in '43. When I moved up to the 184-inch, there were two vacuum tanks that we used to call D1 and R1. I started working on the R1 and later on I shifted over to the other.

Hale: Were they different things?

Helmholz: Not really. They were both for the alpha ion source and separation process initial enrichment. They were doing somewhat different things. The two activities were coordinated tightly, and they kept working on special features. There was a model of the alpha magnet set up there and later on a model of the beta magnet. A full-scale model of the separation to hold the ion source and the collectors and so on for both processes was set up. Those were separate from the 184-inch magnet apparatus but, of course, using a lot of the ideas that had been gleaned from the 184-inch experiment.

Hale: Who was basically in charge of putting all those factors together?

Helmholz: My guess is Lawrence and Cooksey and Thornton and Brobeck. Richardson was there; Wright was there.

Hale: Did somebody have subsidiary responsibility for the organizing of research aspects?

Helmholz: I would say not really. Responsibilities in the laboratory were fairly clearly delineated. For example, Ed Strong, who was a professor of philosophy, was in charge of the building. Whenever something new had to be built or the crane had to be operated in a certain way, he's the one who took charge of seeing that it was done. Jenkins was in charge of some of the scientific aspects. He was less involved with the individual experiments but had more sort of overall view of the things that were going on. At one time, he had to do with coordinating the Berkeley and Oak Ridge activities. Then there was Donald Shane, who was an astronomer who started out doing some things with personnel; then he moved to Los Alamos. A fellow whom Ernest Lawrence met, George Iverson, not a scientist, came to the laboratory and had a good deal to do with personnel. I've forgotten now who Segrè left in charge of the work on the analysis of enrichments.

Hale: I gather that Thornton was essentially responsible for the experiment for alpha?

Helmholz: Yes, I think probably he had more to do than anybody in translation of the results of the experiments into design.

Hale: But he wasn't involved with the research problems with the 184-inch?

Helmholz: I think he was. He had to know that such and such in the experiment might give an answer to certain questions that would be involved in the design: whether to use tungsten filaments or

tantalum filaments, things like that. He knew a lot about both the experiments and the design.

Hale: When he moved to Oak Ridge, which was about the middle of 1943, was Jenkins still the liaison person between the lab and the Tennessee Eastman [Company], or did Thornton take over?

Helmholz: I'd have to ask Thornton.

Hale: Did you feel that Jenkins was greatly involved in the liaison?

Helmholz: My guess would be '44, but he could have started in '43.

Hale: Thornton wasn't here? Jenkins was here all the time?

Helmholz: Jenkins was here all the time. I think he sometimes spent a month at Oak Ridge, even a month at a time, but I don't think in any of the years he spent more of the time at Oak Ridge than he spent here.

Hale: It was also the beginning of that sort of thing between big projects and research.

Helmholz: I'm sure it was a complicated matter to set it up. I never learned a lot about it because I was here all the time. I was involved in more of the details of the experiment than of the final design.

Hale: There was a report just before the end of the war, briefly listing everybody's contribution with a view to apportionment of credit when the secrets were all out. It was very brief, and maybe it implied that some credit was attributed more to somebody than it should be. The implication is that Lawrence could claim credit for a lot of small things which added up to a lot for one person. But in fact they came from other people. I don't know if it was intentional. You are credited with the smaller radius calutrons, the Beta program. Is that where you fit?

Helmholz: That isn't quite true.

Hale: No mention is made of your involvement in the Alpha program.

Helmholz: I never really was involved with either the Alpha or the Beta pilot models. I was always involved in the experiments that were being done in the 184-inch magnet with other vacuum tanks and sources and collectors. We did some work that was applicable to the Beta program. I would have written it differently. It would have said that I spent more time on the

experiments that really related directly to the Alpha program. I can't remember exactly what the radius was of the circles that the uranium isotopes moved in in the R1 program. Maybe it was just as large as the Alpha program. The Alpha program was four feet and the Beta program was two feet. My recollection of the R1 tank was that it wasn't quite four feet. I may well be wrong about that.

Hale: After Thornton moved to Oak Ridge, Ed Lofgren, an advocate of the two-stage process, was in charge of research on the Beta program here?

Helmholz: I certainly remember that he was in charge of something. Whether it was a part of the Alpha program or the Beta program, I don't remember.

Wartime Teaching

Hale: In '43, you became the first UCB Physics Ph.D. to gain a permanent position in the physics department. What was it like teaching in the war? Was it affected by the war and the intensity of the research you were doing?

Helmholz: The teaching was quite different. One had these classes of people who were preparing for some kind of war service. Sometimes you would have an army group studying electricity and magnetism or mechanics just as a kind of a college course to prepare them for further service in the armed forces.

We kept sort of the same courses that we had been giving, but these people were in a kind of a crash undergraduate program where there were three semesters per year. There wasn't much vacation, if any, between one semester and the next. They were good students, and I think the kind of work that was done was much the same as it would have been in non-wartime. It was just more concentrated.

I was always teaching upper division courses at that time. I didn't have much to do with the freshman and sophomore courses in which there was more laboratory work in the four-semester introductory course. One of the things Jenkins had to do was recruit volunteers for teaching assistants from other faculties of the University. He would get a French professor to come in and be a teaching assistant for the laboratory. There were not enough graduate students. Any graduate student in physics would be used by the laboratory to help with the calutron experiments.

It was interesting; some of the students, of course, were terribly concerned about the relation of the course to something to do with war.

Hale: You're talking about the younger students?

Helmholz: Yes. They would be the ones sent there by the army or the navy. They were particularly concerned because they knew that they were going to be in the army when they got out of this undergraduate program. I don't remember any problems that that introduced. There were some good students and some poor students, just as there always are.

Hale: Everybody was subject to the draft?

Helmholz: There were some students who hadn't been drafted and who were just in a normal college course. I know there were some groups of students in special programs. I think that occasionally they had a special course for them. For example, there was a meteorology program of some sort on the campus, and they were given special courses because they were a separate group. I don't think I ever gave one of those courses. I taught the standard undergraduate courses.

Hale: Apart from the war work and the teaching, did you manage to do any pure research?

Helmholz: No, I didn't do any at all. That had to wait until after.

Hale: I imagine that if you did somebody would say, "What are you doing with this; you could spend that time on the war."

Helmholz: Yes.

Other War Research

Hale: Were you aware of other areas of war research that the physics department was involved in?

Helmholz: Not a great deal. We heard of what the people who had gone to the Rad Lab at MIT were doing on radar. Then, during the war, we would occasionally see people from Los Alamos and hear a little bit about what was going on, although I'll say they were quite closed-mouth about that. I remember one time that Segrè and a spectroscopist named Julian Mack came up to Berkeley. I saw them measuring the hyperfine structure of uranium 235. To

measure the spin of uranium 235, they used the spectrograph downstairs. No, I wasn't terribly involved, and I wasn't terribly aware of the whole variety of the kinds of experiments that were going on. I'm sure I heard about the time McMillan was at San Diego doing some underwater research. But I never knew about these things in detail.

I'm a little mistaken when I said that Massey and Burrup took over when I left the 37-inch magnet. There was a period in there when a fellow from Westinghouse named Slepian worked with the 37-inch magnet, so it probably was in between my leaving and Massey and Burrup starting their work.

Hale: That was on another electromagnetic method. Was that the magnetron method that he was working on?

Helmholz: It was sort of a magnetron method, in a way.

Hale: Ion centrifuge, they called it.

Helmholz: Yes, I think that was the name they used. I can't remember the name of the other physicist with him whose name I knew very well at the time. Slepian wasn't here all the time, but this other fellow was sort of in charge of the group. When that was shut down, Massey and Burrup started their work.

Hale: Why did you think you'd have nothing to say about the war years?

Helmholz: It was sort of pretty much the same day to day, not that one did the same thing every day but the experiments moved as fast as we could make them move. What I was doing was pretty much the same from month to month. I think this was different for the people who were in Los Alamos. They first had a lot of experiments to do, cross sections and things like that. Then they had worked on the explosion. The people who went to Oak Ridge had a lot of different problems of getting the apparatus running. We had a system, an experimental setup that worked, and we just went through the different experiments, trying to improve first this and that.

Hale: You don't see your contribution to that sort of effort as a small contribution?

Helmholz: I don't think it was as much as from people like Thornton by any measure, or Brobeck or even Richardson. There is no question but what teaching did take up an appreciable amount of my time, and I therefore didn't get into some of the things as deeply as some of these other people did. I think that is probably one of the reasons that I never went to Oak Ridge; teaching was twelve

months a year. I never got in a position where Lawrence or somebody said, "Well, you better find somebody else to teach, and you go to Oak Ridge; you would be more useful there."

Hale: You're demeaning your contribution because you weren't very dramatic.

Helmholz: I contributed some to the final development of the Alpha and the Beta processes. I wasn't as much involved as some of the other people like Thornton with the actual design and the Oak Ridge because the operation of Oak Ridge was an awful problem. I heard about it from people who would come back all the time and tell us about it.

Moral Questions Regarding the Bomb

Hale: What were your thoughts during the war and immediately after the war on the moral and ethical questions on what you were doing?

Helmholz: I don't think I thought a great deal about it until after the bomb was dropped. I suspected that something like the Alamogordo test was going to be carried out. I have a feeling that not a lot of the people at the laboratory knew that the Alamogordo test was going to be carried out. I have a feeling that not a lot of the people at the laboratory knew that the Alamogordo test had been carried out and was successful. I didn't know, so in a sense the news of the Hiroshima bomb was a surprise. I won't say not a complete surprise, but just because I didn't know of the Alamogordo test, it was a surprise in that sense.

Hale: What did it feel like to have been involved in the policy of that bomb and then suddenly to hear the bomb was dropped? And to realize the awful consequences?

Helmholz: It was a powerful experience just to hear about it. I don't know if I would say I was shocked. I guess I'd say I really was sort of awed by the story of what happened at Hiroshima.

Hale: Did you have moral regrets or remorse?

Helmholz: No, I never really did. I certainly appreciated that it was a question of whether one should have dropped the bomb or not. I felt that probably it was the right thing to do, and I felt that it was a troop decision. Had I to decide that, I would have decided to drop it.

Hale: Your overall philosophy of life doesn't conflict with that?

Helmholz: No. Afterwards, I was very much against the so-called May-Johnson Bill for the military control of atomic energy.

Hale: Were you active--

Helmholz: I took part in some of the discussions around here that the Federation of American Scientists sponsored.

Hale: Just attending their discussions?

Helmholz: I'm sure I have been a member. I don't really remember if I was regularly a member or whether I was a member for a few years and then dropped out. I gave a few talks to local groups about the bomb, semi-scientific talks, and tried to explain the physics of it. People would ask me, and I wouldn't really answer them as to whether it was an awful thing to have dropped the bomb or not.

Hale: You avoided that sort of question?

Helmholz: I avoided it. I probably said, "Well, the war was on, and this was the way to win the war." I was a member of the department, and I was an assistant professor then. People and friends of mine knew that I had been involved in the project and one or two of them asked me to talk.

Hale: This was like, in a sense, a speaker's bureau from the department.

Religious Views

Hale: I never asked you about your religious or philosophical feelings and how you guided your life.

Helmholz: I'm probably an agnostic as far as religion goes. When I was young, I went to Sunday school. When I was in prep school I went to an Episcopal school and used to go to chapel every evening. After that, I have been to church relatively seldom. I think I am sort of a practicing Christian but not an active churchgoer.

Hale: Although you characterize yourself as an agnostic.

Helmholz: I think so. I certainly don't believe in a personal God or anything like that. I suppose that my philosophy has a lot of the Community of Man, and the fact that man, wherever he is, is related to all other men on the earth. I feel that I have an obligation to aid and assist other people as much as I can. In a sense, this goes with being a teacher. I get a lot of satisfaction out of it and feel that by teaching one does help other people.

Hale: Were you ever a humanist, for example?

Helmholz: I don't know. I would almost have to ask you what you mean by a humanist.

Hale: In a formal sense.

Helmholz: Not in a formal sense, no. I remember once that I got interested in the Unitarian Church. We went to the Unitarian Church for a while. Maybe it was because of the person who was the minister at the time. I found that I wasn't really interested in it and so I didn't keep it on.

I guess I have always been sort of an internationalist. I have always felt that but never taken any active part in foreign affairs. Several friends of mine belong to the Foreign Affairs Council. I remember one year a friend of mine who was in my class in college came out and visited here and wanted to go to the meeting of the Foreign Affairs Council in San Francisco. I think at that time I had heard that such a thing existed, but I had never been interested in going. Even though I've had quite definite opinions about these things, I've done less than I probably should have about such things such as foreign affairs.

Hale: In an active way?

Helmholz: Yes. Those first few years after the war, I didn't take a very active part in any of the discussions that went on or went to meetings about the May-Johnson Bill. I'm sure I even wrote a letter to a congressman or senator against it, but I didn't take a public role or anything.

Hale: Do you ever regret that?

Helmholz: Yes, in some ways I do. I think that probably I could have done something--not to defeat the May-Johnson Bill but at least to get the bill through that finally got in. I sort of regret it. It is one of the things I might do if I had to live my life over. I don't have any gnawing regrets about it, but I think if

I had to do it over again, I would probably take a little more active part.

Hale: Your agnosticism dates from during the war?

Helmholz: No, I think it dates from college. I have always been an agnostic. My father went to the Unitarian Universalist Church in his older years. I think he was almost an agnostic, too. I suspect that part of my religious beliefs come from him.

IV POSTWAR YEARS AT THE RADIATION LAB

[Interview 6: November 4, 1975]##

Concern with National Security and the Loyalty Oath

Hale: Professor Helmholz, could you compare and contrast the organization, scope, size, and the philosophy of the Rad Lab before and after the war?

Helmholz: One can obviously say that the laboratory was much bigger--maybe not broader, but at least a bigger scope after the war than before. I'm sure that the experience of the war brought a little different way of looking at things, although I must say I don't remember it very distinctly. I'm sure that the kind of financial technical support that we had was much more extensive after the war than it was before the war. Probably this influenced the way we did things. There wasn't as much question of support after the war as before; we had to watch pennies and things like that. I think that the attitude of the scientists about the laboratory was not very much different. There were, of course, lots more people interested in physics, more graduate students, more of a public interest in what the laboratory was doing. But the attitudes of the scientists there was still pretty much the same. We were interested in physics research. We did it a little differently on a bigger scale.

Hale: What about the philosophy of the lab?

Helmholz: That's a little hard to say. I think we in scientific research had the same philosophy. I suspect that Lawrence's attitude was a little different; he was always concerned not only with the physics research at the lab, but he was also concerned with national security work of the type that eventually led to Livermore. I never took part in that kind of work, but I'm sure that the fact that the laboratory did have security regulations and so on arose because Lawrence was concerned with such things. That made a difference, I'm sure. I could see that it wasn't as

great as in the years following the war and so I wasn't particularly bothered by it.

I'm sure that anybody that had been in the laboratory before the war and then came back to the laboratory would have said, "This is much different because you have this tight security." Everybody had to have his PSQ, or whatever they call those statements that you had to make. There were a number of people who were concerned about that, particularly at the time of the loyalty oath dispute on the campus.[1] There were a number of people who felt very strongly that the laboratory was too much concerned with what is called national defense and secret things. Lawrence himself was, I think, probably ill-advised in taking a strong attitude about the oath. He felt that anybody that wasn't willing to sign the oath was either foolish or possibly didn't deserve to work at the laboratory. A number of people left the laboratory at that time. We certainly suffered from their leaving.

Hale: Who were they?

Helmholz: Gian Carlo Wick was one. He went to Columbia. [Pief] Panofsky was another. Panofsky didn't pick up and leave immediately--I have the feeling that he got an excellent opportunity at Stanford--but it was in good part because of the oath dispute and of Lawrence's attitude toward it that he left. The third person that I remember is Geoff Chew, who left and went to the University of Illinois. Of course, he did come back later. Richmond and Wilcox are two that I remember, so that it certainly did have an effect.

I felt strongly that the Regents' position was an unreasonable one. Naturally, that applied to Lawrence's insistence that people who worked at the laboratory be willing to sign the oath. I felt that that was wrong. On the other hand, I did have to admit that it was an awful series of blunders, one after the other. In other words, [President of the University Robert Gordon] Sproul himself had not realized the furor that it was going to cause. When the faculty appointed a committee to deal with the Regents, to try to work out some sort of a compromise, then the faculty wouldn't accept what their own committee had recommended. That was another

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[1]The Board of Regents adopted a special loyalty oath for University faculty and staff in March, 1949. Thirty-six faculty members refused to sign and were dismissed by the Regents. They were reinstated following a California Supreme Court decision in 1952. See David P. Gardner, The California Loyalty Oath Controversy (University of California Press, 1967).

blunder which just exacerbated all the hard feelings. It was certainly an unfortunate situation. It certainly was at least ten years before people got back to feeling a little more normal about that.

For example, there were five or six rather prominent faculty members of whom Edward Tolman was the leader; they were eventually fired. A lot of members of the faculty contributed to a fund to support those people while they looked around for other jobs. Finally, they took it to court and won. The Regents had to pay them the difference between whatever they did receive and what they would have received if they had been here. There were a lot of bitter feelings. I think that the laboratory had less of those feelings than we did on the campus.

Hale: Why was that?

Helmholz: Well, I think because most of the people in the laboratory didn't have to take the Regents oath. Just the faculty had to sign. The members of the laboratory had to sign a state oath that somebody named Levering in the state legislature had worked up. But the faculty was different from that, and the faculty had to sign an oath earlier than the Levering oath; I think maybe it was a year earlier. A lot of the people who objected to the Regents oath objected to it because they felt that they were being singled out for special punishment. That was the reason, I think, that the faculty felt very strongly about it, whereas the laboratory employees didn't feel so strongly. There were some of the laboratory employees who didn't like the Levering oath, but it seemed to me the argument was that it was for all state employees. You couldn't really strongly object to it.

Hale: Were you close to Lawrence at that time?

Helmholz: No.

Hale: You grew apart from him, or were you closer to him before the war?

Helmholz: Before the war but not after the war.

Hale: So you might not have been quite aware of the way he was thinking all the time?

Helmholz: I wasn't. I saw him often but didn't talk to him often during that period.

Hale: Did he seem to be remote?

Helmholz: I don't know. By the year 1950, he wasn't concerned with the individual experiments that were going on. I felt that his time was just taken up with running the laboratory and with his defense work.

Hale: From around 1950?

Helmholz: Yes. It's hard to pinpoint any particular date, but I would say perhaps through '47 and into '48, I used to see him at the laboratory when I was doing experiments before the electron synchrotron got going. But when the 184-inch was operating, he was around there. Obviously, he was particularly interested in the 184-inch. As the time went along, I think, I saw less of him since I wasn't concerned with the direction of the laboratory or the management of the laboratory. It was people like Alvarez and McMillan who knew more about what he was thinking and planning.

Postwar Transitions

Hale: At the end of the war, a number of people at the Rad Lab were actively cut back. A lot of them left, of course. Amongst those people who were going to lose jobs, or didn't have another good job to go to, did you sense any unrest or dissent?

Helmholz: I didn't, no. It seemed to me that the technicians or the machinists and the electronics people did pretty well about getting jobs. A fair number of them stayed at the laboratory because they were needed for the reconstruction of the 184-inch as a cyclotron, for building the electron synchrotron, and for building the proton linear accelerator. As far as the scientific personnel went, I really don't remember anybody who had a hard time getting a job. I'm sure there were some people, but it was a great time for physicists to get a job. There wasn't a lot of dissatisfaction or unrest.

Hale: Did you have to reconsider your responsibilities anew--I mean, in the Rad Lab and physics department?

Helmholz: I don't think I did. At least, let's say I didn't have any problem in thinking about it anew. I had always, even since I was a graduate student, been concerned both with the laboratory and the department, and I just kept on. In other words, I had both a responsibility to the laboratory and one to the department, and since I was mainly concerned with graduate students in the laboratory and with teaching in the department,

those flowed together very well. I didn't have the problems that some of the faculty had who were involved in purely Rad Lab activities; for example, those who were involved in defense and who got involved with the kind of work that was later done at Livermore. I was never involved in things like that, and consequently I didn't have that split in my activities. I'm sure those people did have a more difficult time in trying to allocate their time.

Hale: Was the transition to the postwar period, as far as you're concerned, pretty smooth?

Helmholz: Yes, I would say it was smooth. People just stopped. They wrote the last reports and tried to wind up their work on the isotope separation. There was a little bit of isotope separation that went on after the end of the war, and some of us did some experiments trying to use the separated isotopes. I remember Burt Moyer did some of those, and I did a few experiments.

I had been involved with some work with radioactive cadmium which had two radioactive isotopes. The radioactivity characteristics were almost identical in giving isomers of silver, and I think I used some separated isotopes to straighten that one out. The isotope separation sort of flowed into some work that went on, and it was mainly getting back to nuclear physics and then to higher energy physics that we were all interested in.

Building the Electron Synchrotron

Hale: As the completion of the 184-inch approached, did it occupy everyone in the lab?

Helmholz: At the end of the war when McMillan came back, he asked me and Powell to take part in the building of the electron synchrotron. I'm sure there were other people involved. I didn't really have much to do with the completion of the 184-inch cyclotron.

Everybody in the laboratory, of course, knew that the 184-inch would never be completed according to the original plan. It would use the synchrotron idea. There were first some experiments done on the old 37-inch. And then the design was changed so as to use the ideas of a variable frequency in the acceleration process.

Hale: But you weren't further involved?

Helmholz: I wasn't involved in that.

Hale: You were getting involved in the electron synchrotron; tell me about that.

Helmholz: We had sort of three special problems which needed to be worked on. One of them was the injection of the electrons, a second was a radio frequency system, and a third was the design of the magnet. McMillan really took part in all of these.

Hale: Wasn't it his baby to start with? Did he just come up with the idea to do this?

Helmholz: Well, you see, when he was still in Los Alamos, he had this idea, the synchrotron idea, and when he came back he wanted to see it applied to the acceleration of protons. But he had the idea for building an electron synchrotron for accelerating electrons to something of the order of 300 MeV. You can get this in a device which is of the order of a meter in radius and which will get a magnetic field to the order of 10,000 Gauss in an AC magnet. That gives you 300 MeV.

Hale: The most critical thing is to vary the frequency, right, because the electrons are so quick to become relativistic?

Helmholz: Well, you really don't have to vary it a tremendous amount. It is the alternating magnetic field--the idea of an alternating field--that is important in the electron synchrotron, whereas in the proton accelerator you had essentially a constant magnetic field and a variation in the radio frequency system.

Well, we had a small group of people including Jack Franck and Jack Peterson, who went on and got his Ph.D. in physics. Jack Franck was really more of an electrical engineer; he kept working at different things in the laboratory. The third younger person was Mitchell Daisy; I don't believe he was a graduate student in physics. I think he got a master's degree in electrical engineering or something like that. The three of them and I worked on the design for a radio frequency system. We originally had a proposal for a rather complicated system with a copper electrode which would fit in a larger vacuum system. Then, when there were problems with the whole idea of this bigger vacuum system, we went over to a quartz envelope in which the electrode system was replaced by the lining in the quartz donut of an elliptical cross section.

Then, we coated the inside of the quartz donut with silver paint and made it essentially a quarter wavelength in the magnet. The gap was the high voltage end and the other end was the grounded end, and then one had a developed high voltage end which would accelerate the electrons. That was the way in which it was finally worked out.

The problems in the design of the magnet were ones that Wilson Powell and McMillan himself worked on. It was complicated, because that sort of thing just hadn't been done before, or at least that size of magnet. See, when Kerst built the betatron, he essentially had an AC magnet, but he had not built them into the region where one had to make up for fairly sizable radiation losses. This was a bigger magnet than any that he had built. The biggest betatron that was ever built, I think, was 100 MeV, and that had fairly sizable problems. I think that one was built at General Electric, and this was three times the energy.

Hale: How long did it occupy you?

Helmholz: About three or four years immediately after the war.

Hale: You wrote a paper or UCRO report, and roughly the same thing came out in the Review of Scientific Instruments in 1949 and 1950. So that would have been after it was complete.

Helmholz: I suspect that the electron synchrotron started operating around '49.

Hale: What proportion of your time did you spend on that during that period?

Helmholz: I'd say in '46, '47 I probably spent half my time on that.

Hale: Half the total or half research time?

Helmholz: Half my research time. In the first months before it started operating, I guess I spent a lot of time up at the electron synchrotron. Whenever any machine starts operating, you have to spend a lot of time on it. McMillan, I'm sure, was the one who spent almost all his time on just getting the machine operating. I spent very little time after the electron synchrotron started to operate. I had other students working on things that were down on the campus; at that time, I had a number of students working in problems in radioactivity, and I guess I had one of them involved in building the beta-ray spectrograph. I think Powell spent a lot of time with the magnet because he had

designed the magnet. It was a bigger piece of apparatus, of course, than just the radio frequency circuitry.

Hale: What did you get out of that personally?

Helmholz: Oh, well, partly the use of the synchrotron. I guess the only paper that I published was this one they referred to on the operation of the radio frequency system.

Postwar Physics Research

Helmholz: Then a number of my students, including Carl Strok, did some interesting work on the degradation of high energy X rays as they pass through materials. That was an excellent paper, which he really did pretty much on his own. I think it was his idea to look into the way in which the gamma rays were degraded as they went through material. McMillan helped him with some, and I helped him with some, but he was perhaps the best student that I ever had, and he did a lot of that just on his own. Serber was the theorist who helped him.

I had another student named Bill Gilbert with whom we did an experiment on the photo-disintegration with deuterons. And that was a particularly interesting phenomenon because it looked as if there would be meson effects, pi meson effects, in the photo-disintegration process. There was some disagreement about his measured cross sections. There was a group at Cornell with Robert Wilson; one of the people who was working there was a fellow named Lichttower. Their measured cross section was different from ours. When third parties got to do it, the Cornell measurement was closer to the actual correct values than ours was.

Then there was another one of my students, Fred Coensgen was his name, whose thesis was on the Compton effect on protons. Exactly when he got his degree, I just would have to look up; it was somewhere around 1955. There were a number of other experiments that I was interested in because they were being done at the electron synchrotron, but they weren't by students for whom I had the primary responsibility.

Hale: Was this the major research going on in the synchrotron?

Helmholz: Well, the more interesting research at the synchrotron was, of course, the research having to do with pi mesons. Jack Peterson, who had been involved in this radio frequency system,

did some of the initial work with emulsions and got the production of, I guess, pi+ mesons or was it pi+ and pi-? Then there were other experiments on the lifetime of the pi meson. Clyde Wiegand, who was working with Segrè, took part in those. Then there must have been something that had to do with the neutral pi mesons.

Hale: Would you say that your interests were centered on the electron synchrotron?

Helmholz: Yes.

Hale: And you just had peripheral things on the 60-inch?

Helmholz: Just because I had been interested in the operation of the electron synchrotron, I think I had a greater interest in the experiments that were involved there. But, of course, I also had substantial interests in radioactivity.

Hale: You were still heavily involved with isomers?

Helmholz: Yes, that was my interest in the radioactive work: lifetimes and so on. That was the time that Segrè and I were writing that article for the Review of Modern Physics. Do you have the date in which that was published?

Hale: 1949.

Helmholz: That article was written just before the shell model. When the shell model was introduced, that made quite a change in the views of isomers. That review article was a good review of isomers up to that time. I was particularly interested in the explanation of isomerism by the shell model, but I was not involved in the development of the shell model myself. As the years went along, I became less interested in isomerism, I guess because I hadn't been involved in isomerism according to the shell model.

Hale: What would you say then became your major interest?

Helmholz: Well, I would say my major interests were some particular ideas with different aspects of radioactive decay. Then there were some ideas that had to do with the scattering of alpha particles, with the 60-inch cyclotron, and I felt I really should get out some of the theses of the students that I was working with. When the experiments with the electron synchrotron started to become less interesting in the late fifties, then I got somewhat involved with the experiments that were done with the bevatron. That was about the time that the

electron synchrotron stopped being so interesting and really stopped being used as much. Our group joined with the Moyer group in the late fifties, and that was a substantial change also.

Hale: What did you mean by "our" group, and when did a group originate?

Helmholz: In the years after the war, I had a group of my own graduate students which was made up of graduate students involved in the work with radioactivity and isomers and the work at the 60-inch cyclotron.

Hale: The form of it was one major professor and a group of students in the traditional manner?

Helmholz: Yes.

Hale: Was it the same for other groups? Was that the general setup for research?

Helmholz: Yes, and the work at the laboratory was supported by the laboratory. Some people were, I think, working together, like Chamberlain and Segrè; almost all of their work was together.

Hale: From when?

Helmholz: Oh, I'd say almost from the time that Chamberlain came back to Berkeley. You see, after the war, he came back to Chicago and finished his degree work there on slow neutrons with Fermi. But then, from the time that he came back to Berkeley, he and Segrè worked together with Clyde Wiegand.

Hale: You were still the professor with a bunch of students? Or had you got some junior faculty members with a Ph.D.?

Helmholz: I really didn't have any other faculty members with whom I worked.

Hale: Were Segrè and Chamberlain the only ones who worked that way?

Helmholz: Panofsky had worked with the proton linear accelerator and so had worked more with Alvarez.

Hale: Do you think you were really aware of what was going on?

Helmholz: I went off. I was involved with some experiments at the electron synchrotron and also with experiments in radioactivity and with experiments of the 60-inch cyclotron, so I was spread

over those different activities. Howell was involved with the experiments at the electron synchrotron and the 184-inch. McMillan was involved with the experiments at the electron synchrotron and some with experiments at the 184-inch. Panofsky and Alvarez were involved with experiments at the proton linear accelerator and then also with some of the experiments at the 184-inch. Wilcox was involved with some experiments at the 184-inch.

Formation of Research Groups at the Rad Lab

Hale: Would you say that groups were forming even then as you see it today at the Rad Lab?

Helmholz: Well, I think that they were probably forming in the early fifties. They got started through the different members of the faculty and their interests at the laboratory. My own group had some aspects of the work on the campus with the 60-inch cyclotron and radioactivity; it was different from the groups, such as Chamberlain and Segrè, working more with the 184-inch cyclotron and then the betatron.

[Interview 7: November 11, 1975]##

Hale: Why did you join with Burt Moyer? What things did you have in common? What did that mean in the change in style of research with you?

Helmholz: The joining, I think, was brought about in part because there was a tendency to avoid having small groups when there was a reasonable union of interests. At the time, Moyer was interested in particular experiments at the 184-inch cyclotron and the Bevatron--but also to some extent with experiments at the electron synchrotron. This led us to have some interests in common. For example, one of his students named Robert Cence, had done his thesis work on some experiment involving photo-production involving pi mesons at the electron synchrotron. At that time, Bob Kenny was working with me, and the students were doing some work at the electron synchrotron also. Consequently, we had this community of interests. Moyer was involved in health physics. Although I was interested in that subject, I never had anything to do with that. He had a wider range of interests than I did. It seemed logical [to merge] in order to be a little more efficient with the use of nonscientific personnel, from the point of view of space, and of just financial aspects in general. It really didn't make a great

difference in our modes of operation. I think we really operated compatibly and similarly.

The description I gave before was common to both our groups and, I think, continued to be so. A graduate student came to start his research work with an experiment that was already ongoing. After that was finished, he took some particular experiment on his own as his main project, and that turned into his thesis. We still maintained a delineation of whose student was whose. When somebody wanted to sign up for doing research, he would come either to me or to Moyer during his time as a research student, and he was nominally Moyer's or mine. This, of course, meant that we did some trading off with personnel--for example, in the time when someone was learning the ropes. There might be some experiment that Moyer's students was using as a thesis experiment with which one of my students would help and vice versa.

We used to try to have weekly or bi-weekly meetings at which students would talk about either their own work or some interesting other experiments. It made it a little easier to arrange such things when you have a bigger group than when you have a smaller group. We tended to have teas on Friday afternoon in which everybody would get together, and, after the tea, we would have one of the talks which would last half or three-quarters of an hour, usually by one of the students. That was useful first in getting everybody acquainted with what was going on. Then, I think, it was useful to the students in teaching them about giving a talk.

Hale: Did you share the administrative chores?

Helmholz: Yes. We had an excellent secretary, Mary Ann Macklus, who became very adept at making out necessary forms for personnel changes and so on. Moyer had more of the administrative chores because he was more familiar with them and partly because he was involved in the health physics aspects which required more administration. It was an ongoing laboratory activity involved with the operation of accelerators, and you constantly had to keep track of that. Most other groups that I knew very much about operated in somewhat the same way.

I think faculty members took part in the experiments themselves or in the building of equipment and so on. I think I probably a smaller part in the individual experiments than some other people did, partly because I was chairman during a fair amount of the time. Moyer, I know, took somewhat greater part. Having two faculty members did have an additional advantage; when one of them was away, the other one was almost always

there. When I went on sabbatical, Moyer took care of my duties, and when he went on sabbatical leave, I took care of his students. That happened in the years '62-'63 and, I think it was, '65-'66 or '64-'65.

Hale: When was the group formed?

Helmholz: My guess is about '59. We were for a number of years in building 64 at the lab, and we had just moved into building 58 about the time I went on sabbatical leave in '62.

Hale: That's where you are now?

Helmholz: Yes, that's right. We are still in the same place.

Hale: Were other groups joined at that time, or were they separate?

Helmholz: I think they had nucleated by that time. Powell had his own group beginning in about 1950, and then he was by himself for a good many years. Then [Robert W.] Birge joined with him; it was called the Powell-Birge group. When Powell retired, they had attracted Bob Ely, who is in the faculty, so it became the Ely-Birge group. Then, when Birge became the associate director, it's now the Ely group. Maybe when Birge leaves it will become the Ely-Birge group again. The Segrè-Chamberlain group has always been known by that name. For a number of years, it included Ed Salandas, who was a faculty member. When he left the laboratory, Herb Steiner, a laboratory staff member, took the place of Ed Salandas. He was an additional member. Then, when [William] came he joined with that group, although I think for the first year or two that [William] Chinowsky was at the laboratory, he was not associated with any one particular group.

The Alvarez group, of course, grew very substantially, particularly in the bubble chamber period. I don't know if I remember who was the first one joining with Alvarez. Perhaps Rosenfeld and then Stevenson and Crawford and Tripp. When Gerson Goldhaber came to the laboratory, I believe that he did some work with Segrè and Chamberlain, and then he got into the analysis of bubble chamber photographs and finally joined with Trilling, and so they are together. There has been some shift, although not spectacular. In other words, there was no time at which the laboratory management said, "You ought to be in this group, and you ought to be in that group." It was left to the individuals, really.

The Helmholz-Moyer Group

Hale: Would you say that the main reason that people go in together in a group was because of consonance of interest?

Helmholz: Yes, I would say certainly that was one reason. There is another reason: experiments just got so complicated and large so one person is really not able to stay by himself. That was the basic, although maybe not the strongest, reason that Moyer and I joined together. Just the size of the operation does tend to make it necessary for a couple of people, the senior staff, to get it together. Moyer's group, before we joined with them, had quite a large number of graduate students and could mount an experiment all by themselves. It just made it easier when we joined with them, and I think the same thing occurred with other groups.

Hale: Was anything much involved with personalities there?

Helmholz: You would like to talk to other people if you were a faculty member or a staff member. You wouldn't try to form a group with somebody with whom you didn't get along. We saw enough of other people in the laboratory to know with whom we'd get along well and with whom we wouldn't. I think Moyer and I had relatively easygoing personalities, and I suspect either of us would have got along quite well with a wide variety of people. On the other hand, there were other members of the staff who had more distinct, let's call it, personalities and wouldn't have gotten along with others. They would get along with some people but not with others.

Hale: Do you think what happened with the lab groups set a trend for other institutions?

Helmholz: I think so. Berkeley had the first big machine or the first couple. I think the 184-inch before it started was <u>the</u> big machine, and the Bevatron; I think the Berkeley model was followed by others. And, in some sense, it had to be followed just because of the size of things. You can't adjust the operation of a big machine the way you can adjust the operation of a small, one-room laboratory. In other words, if you are scheduled for a certain time, you've got to be ready to run. You can't move some other experiment in in a few minutes, and you can't let the machine be idle.

There was a change. I'm sure Lawrence realized it; whether he would say he'd instigated it or not, I don't know. It was having a technical establishment to operate the machine. When I

started as a graduate student, all of the graduate students operated the machine. Lawrence would come in every so often and operate it, and McMillan and Alvarez and Snell and Cooksey and so on would do it very well.

After the war, when the 184-inch got operating, the laboratory had a technical staff of people who were really not physicists but who were experts at running the machine. So the physicists spent all their time on the experiments; they would call up the control room and say, "We want the beam now." If it didn't come, you would say, "Work on the beam until you get it and let us know," and they would. The experimenters--the graduate students and the faculty members--just spent their time on the experiment.

There were some people who were interested in the operation of machines. Lofgren is an example; he was sort of the leader of the group and spent some of his time thinking about new kinds of accelerators and methods of deflecting beams and so on. He was relatively seldom down in the control room fiddling the knobs. He could do it, whereas many of the rest of us couldn't. If you put me in the control room, I wouldn't know where to start, but McMillan and Lofgren and a few others were interested in the new machines and new kinds of machines.

Hale: [Raymond] Birge made a comment that you seemed to be very overworked; I don't think there was anybody who had as many students as you had.

Helmholz: I think to some extent it must have been because I was easygoing and liked graduate students, partly because I think I relied quite consciously on graduate students to help each other. I was involved in a couple of different fields of research, both radioactivity work at the 60-inch cyclotron and then work at the electron synchrotron and even a little bit with the 184-inch cyclotron so that I, in a sense, catered to a wider variety of students. I think Moyer at his peak had as many as I did. He was another one who had a large number, for being by himself. I'm sure that the Alvarez group had as many students, but there were several faculty members involved, and the same way with the Segrè-Chamberlain group. They would have as many graduate students but with, for example, three faculty members, whereas Moyer had only one and I had only one.

Hale: Would some of their students have some other faculty member as their advisor, or would they all be advised by Alvarez?

Helmholz: No, they would be advised by one of the group. You would have to ask Alvarez and Rosenfeld whether when a graduate student

came to Alvarez he would say, "I just haven't got the time right now; I have three other students. Why don't you try working with Tripp or Rosenfeld?" There was, I'm sure, some of that going on. The students would see Alvarez and Rosenfeld as well as Tripp. When it came to the qualifying examinations or the thesis, Tripp or whoever it was would be the particular faculty member involved. That would lighten the load for any individual members.

Hale: The load for you would be rather high, in fact, if all the students were under you.

Helmholz: It was, of course. I did get Bob Kenny, I forgot now when it was that he came back to the laboratory. He had been a graduate student and then had been in Los Alamos for a while. Then he came back to help with all the work at the electron synchrotron, and that took a good deal of the load off of me.

The Department of Physics, the Rad Lab, and the Changing Fortunes of High Energy Physics

Hale: You were chairman of the Department of Physics from 1955 to 1962. During that time, what was your relationship with Lawrence and the Radiation Lab?

Helmholz: Well, we had a number of new appointments of high energy physicists. I would consult with Lawrence and McMillan, Alvarez, Segrè, and so on about those new appointments, but I really had relatively little to do with Lawrence. Don Glaser would obviously work at the Radiation Lab; I'm sure that I had talked to Lawrence about this at least once. Glaser may not have been appointed until after Lawrence died [in 1958], I'm not sure. [Glaser was appointed in 1960.] But, you see, Lawrence was the director for three years while I was the chairman, and I would say I don't think that I had any more contact with Lawrence than Birge did, perhaps even less. I knew a little bit about what went on at the laboratory. I knew more than Birge did, and I just didn't have the occasion.

Hale: So there was not much the lab and the physics department had in common?

Helmholz: We did have faculty members, and the lab point of view was always well represented. I think the most important aspect of it was in the new appointments. The way in which high energy physics has developed and prospered, and then not prospered, we

might have been better advised to go less heavily into high energy and a little more into some of the other branches of physics. But that's a mistake that we had good reason for making.

Hale: You had that degree of control during the time that you were chairman?

Helmholz: If I had been very strongly of the opinion, we might have made a few less appointments in high energy physics and a few more appointments elsewhere. I would have had to have had the support of the other members. Since I was department chairman, there is a requirement in the University that when you recommend an appointment, you indicate in the letter of recommendations how many members of the faculty voted in favor of it and how many against it. My recollection is that we very rarely had cases in which the vote was split. We have a vote of maybe nineteen to one or twenty-eight to two or something like that. But, in general, we just sat around and talked until everyone was convinced that that was the right thing or the wrong thing to do.

Hale: Do you have a feeling in retrospect then that maybe the Berkeley physicists were a little one-sided?

Helmholz: Well, yes, I think probably.

Hale: You mentioned before it was possible to become imbued in Berkeley physics and not look at the outside world.

Helmholz: Well, I think that there was a time when Berkeley was the world leader, and obviously that time has passed. When you're the leader, you don't have to look around so carefully at other places to see what is being done that's worthwhile. And I think the way high energy physics has gone, it probably isn't the most interesting field for young students to get into.

There was the time, for example, in the late sixties when all good students--the outstanding students from almost all other schools that applied for graduate school here--wanted to be in high energy theory. I used to ask the other theorists why this was, and they just said, "The most exciting things are happening in high energy theoretical physics." That time is passed, I think, and the same thing for high energy experiments.

I never really thought very hard about it, but I'm sure you can make a case that we did exactly the right thing. On the other hand, I think it is easier to make the case that we went a little too much into high energy, both in theory and in

experimental work. We'd be a little better balanced now as far as taking care of graduate students that are coming along if we had a few more people in solid state physics and a few less in high energy physics. And, as the time goes along, and as people retire, that will get taken care of.

Hale: That is because students are saying that they want to do this?

Helmholz: Yes, they are interested. It changed in '72 or thereabouts, as they began to realize that jobs were hard to get in high energy physics.

Hale: During the time you were chairman, did you publish?

Helmholz: Not much, if anything. My students published things, and we did have a number of theses. But I don't remember that I published anything.

Hale: Did you turn down students while you were department chairman?

Helmholz: A few. I don't think I can remember any names, but I'm sure that I would try to talk to a student about what he was interested in, and if I could see that he was really interested in what Stevenson or Crawford or Ypsilantos was doing, I would say, "Why don't you go work with him?" But I kept a fair number of students.

Hale: Is there anything during the time you were department chairman that you think we omitted?

Helmholz: No, I don't. The only way in which I could have anything additional would be to go back and look at the theses of my students.

Hale: When you retired from the chairmanship, you immediately had a sabbatical?

Helmholz: Yes, that's right. That is sort of the logical way. I had a sabbatical due me for quite a number of years, and I really wanted to get away from being department chairman. I applied for a sabbatical and also for a Guggenheim fellowship and was granted both, and so I went to CERN [European Center for Nuclear Research].

Hale: Did you have a specific problem that you knew that you wanted to deal with in advance?

Helmholz: No, I didn't. I was able to talk to a fair number of people here who had been to CERN, and their suggestion was to just look

around when you get there and then decide what group at CERN you want to work with. I must have talked to six or eight different groups and finally found one that had an experiment that they were working on; it was scheduled, and there seemed to be as good a chance that that experiment would be run while I was there as any other group. So I decided to work with that group: a kind of scattering of high mesons.

Hale: Did that influence your interests during the following years?

Helmholz: To some extent, yes. I think I got to know a little bit more about the techniques that were used. Although that was an experiment done with higher energy than we had here, I think it certainly had some influence. I knew by hearing occasionally from Moyer that there were some experiments going on here in that general field, that that would be a good thing.

Hale: Was it a difficult time for you, coming away from the chairmanship after having spent that long in administration and wanting to get back in research?

Helmholz: Yes, I think it was. Let me say it this way: it wasn't as easy as it probably should have been, because I hadn't kept up with the research quite as well.

V PREWAR AND WARTIME BERKELEY

[Regional Oral History Office Interview 1: October 3, 1989]##

Arrival

Lage: Today we are going to talk about some of the things that weren't covered in very much depth in your earlier interview for the History of Science and Technology project. I wanted to get a sense of what I've titled in the interview outline "Life in Berkeley"--coming to Berkeley in 1937, how you put down roots, and--

Helmholz: Yes. Well, as I perhaps mentioned in that other interview, I knew that Luis Alvarez was in Berkeley, partly because my family knew his family. So when I decided to come to Berkeley, I wrote to him, and he responded and told me a little bit about what was happening at the laboratory. He and his wife met me at the train when I came in and brought me to the laboratory. I immediately got to know Ernest Lawrence, I think mostly through Alvarez, although my father had once talked to Ernest Lawrence about my coming, and so Ernest Lawrence was prepared for me.

Lage: How did your father happen to meet Ernest Lawrence?

Helmholz: Lawrence was giving Sigma Xi lectures in Rochester, Minnesota, and it was after I had decided to come to Berkeley, and so my father spoke to him about it.

Lage: Were you coming as a student of Lawrence's?

Helmholz: Yes. Well, I was coming just as a graduate student in physics, but Lawrence was a member of the Department of Physics, and he had come here as a young associate professor in 1930. He had started the radiation laboratory up after the success of the early cyclotron. From then on, he sort of grew away from the physics department. He kept giving courses in the physics department until the fall semester of 1937, which was when I

came. He taught a course that semester, but I don't think he ever taught another course. He was too much involved in cyclotron affairs. Cyclotrons were beginning not only to be used in the production of radioactive materials for tracer work and medical work, but they were beginning to be important in the possible treatment of various kinds of medical problems.

John Lawrence and Medical Physics at Berkeley

Helmholz: John Lawrence, who was Ernest Lawrence's brother, came to Berkeley several times before I ever arrived and was interested in the medical uses of both radioactive materials and radiation. He came to Berkeley permanently, I think, in something like 1937. There was at that time somewhat of a conflict between the medical work that was being done in Berkeley and the University of California at San Francisco. They had a quite strong X ray therapy division over there. A man named Stone was the representative of the San Francisco Medical School [Robert S. Stone, professor of Radiology]. He got along well with Ernest Lawrence because he wanted to be in on these new developments like the use of neutrons. John Lawrence was a sort of an interloper, and Joe Hamilton, who was the other doctor who spent his time in Berkeley, I think, had originally come from San Francisco. He got interested in the use of these radioactive materials and the cyclotron in general.

Lage: So did he reaffiliate with Berkeley?

Helmholz: He reaffiliated with Berkeley, but there were just these two of them, and I think San Francisco felt that they ought to be really members of the San Francisco faculty. I think they didn't want to be, because they wanted to spend their full time over here.

Lage: What about John Lawrence?

Helmholz: John Lawrence was the other of the two. Stone was permanently in San Francisco, and he would come over here for an afternoon, maybe once a week or something like that, but that was all. John Lawrence and Joe Hamilton--they got along all right together, but I think they each looked at each other as a sort of an interloper on the other's projects. So there wasn't any great love lost between them, either.

Lage: Did you tell me you had roomed with John Lawrence for a while?

Helmholz: Yes. In the second semester that I was here, which was the spring semester, 1938, I discovered that the person I had been rooming with, Dean Cowie--he was a sort of a machine man; that is, he knew a lot about fixing the cyclotron and so on, but he never got very far in the academic study of physics. He and I lived together over on Haste Street for my first semester here, and then I discovered that he was leaving. He went to the Carnegie Institute [of Technology] because they were building a cyclotron at that time. In the process of getting that cyclotron going, he apparently had got too much radiation in his eyes. I think he lost his sight, oh, say, maybe ten or fifteen years later. I saw him about once or twice after about ten or fifteen years, but I haven't heard a word about him since them.

Lage: Were those kinds of accidents common? Do you know many physicists from those days who have had later problems?

Helmholz: No, only a very few, and nobody from Berkeley. I really don't know whether it was John Lawrence who convinced Ernest that there really was a danger to people's health, but Ernest was very particular about it. I mean, we later on wore film badges all the time. He was very particular about teaching each new person who came to the lab about the dangers of radiation. I don't think <u>he</u> did it, particularly. Don Cooksey probably did it and perhaps John Lawrence also. We had really no problems with that.

There was a man in Illinois who lost his sight from this same sort of a problem. It was really getting your eyes down at the level in which there was a lot of radiation, and trying to see something, and not being able to do it. I think that that was the generally accepted explanation.

While I was working at the 60-inch cyclotron, which started up in about 1938, one of the fellows whose name was Bob Cornog--it turned out he got his degree with Luis Alvarez--was in fixing the place where the beam hit. Somehow or other, and I never did hear a good explanation of how it happened, but somebody turned on the beam. He got quite a bit of radiation in one hand, but he never suffered permanently from that. I guess you could see the hand got red and so on, but he never lost any fingers or anything like that. The early people who worked with radioactivity in Madame Curie's time did. A number of them lost fingers and so on from handling radioactivity.

There really were not a lot of people injured. There were a couple of people who died at Los Alamos from having been irradiated. That was a sort of a scandal at Los Alamos. Knowing the way they protect people nowadays, you could see that

they should have protected them then, but they didn't, so there were two people, I think, who were irradiated with neutrons and passed away.

So John Lawrence and I--when I found that Dean Cowie was going, I heard that John Lawrence was looking for somebody to live with, and we lived over on LeConte Avenue for that semester. I was married that summer of 1938. I don't know what John did then. He may have lived at the Faculty Club or something like that.

Lage: Was John outstanding on his own, or did he kind of live in his brother's shadow?

Helmholz: I think he was an able doctor, but John Lawrence certainly was not of the same caliber that Ernest Lawrence was. You could be like Emilio Segrè and say that Ernest didn't understand physics the way Fermi did, which is undoubtedly true, but Ernest was a very dynamic and enthusiastic person who could get things done. His development of the cyclotron idea was a remarkable piece of what I think Emilio Segrè would call "engineering," but I think most people would say, "Well, that's pretty good physics, too." John Lawrence didn't have that kind of ability.

John Lawrence was interested in all kinds of medical problems. The whole question of the so-called medical physics is sort of an interesting one which kept on through the years. Before the war started, there was a building built which was called the Donner Laboratory. It was built as a result of a gift from a wealthy Philadelphia man named Donner.

Lage: And that was pre-World War II?

Helmholz: Yes, that was prewar. Actually, it was meant to be for the use of the people interested in the medical uses of radioactivity and radiation. But as soon as the war started or, I guess, perhaps even before that, Ernest Lawrence started using that for some of the Manhattan Project work. That was the beginning of a department which was part of physics, really, which was called medical physics. Birge, I think, suffered through that. He wasn't particularly anxious to take on medical physics as a part of physics, but Ernest Lawrence was very anxious to do that, I think, partly because of John Lawrence. And Ernest Lawrence could see that there was some good scientific work coming out of this new group.

Then, later on, after the war and particularly after Wendell Stanley came here, there were a group of people who were interested in that sort of thing--not particularly the use of

radioactivity but the ideas of molecular biology. So molecular biology started to come up.

Lage: Did that incorporate aspects of physics?

Helmholz: Oh, yes. It's sort of the union of physics and chemistry and biology. There was quite a to-do on the campus as to whether there should be a department of molecular biology or biophysics or whatever you wanted to call it. The people in medical physics were really not looked on with great favor by the other people on the campus who were interested in those things. Oh, for example, Melvin Calvin in chemistry, who won the Nobel Prize for work on photosynthesis, felt that the people in medical physics were not quite up to the standard that he wanted to see. After Donald Glaser won the Nobel Prize, he wanted to get into this biological work and, let's see, who else? [phone ringing]

So I don't know whether anybody has ever really tried to write a history. There were several people in LSB and that part of the campus who were pretty expert in the molecular biology fields, but they were all members of other departments like zoology or entomology or something like that.

Lage: Did they have background in physics?

Helmholz: Not particularly, no, but they had accumulated a good deal of information about physics. There still is a group over in Donner Laboratory, and there is a campuswide group which call themselves--it must be "molecular biology" or something like that. It's still not a single department, unfortunately. I think perhaps they might profit by being more or less all put in one group.

Lage: The new reorganization of the biological sciences didn't take care of them?

Helmholz: Possibly it did. Anyway, in those early days, there was a good deal of--I guess I'd call it--bickering back and forth as to what should happen to medical physics and whether medical physics should be removed from physics, because for a number of years, even when I was department chairman, the medical physics budget had to go through me and the new appointments and so on.

Lage: Did you take an active role in the affairs of the medical physics faculty, then?

Helmholz: There was somebody over there whom--I think I said, "Well, you look after the things that relate to this." That started out with John Lawrence. Then, as time went on, John Lawrence

retired. I don't know. He must have retired twenty years ago or something like that. I think he's well over eighty now. But in recent times, medical physics has been removed from physics, and they report directly to the dean of the College of Letters and Science.

Lage: I ran across, in the papers, a comment from one of the physics professors. I guess he had been on a number of doctoral exam committees or something. He used the term "biophysics"--and felt that students of biophysics didn't know physics. He was quite disturbed about that.

Helmholz: Yes. Whenever there's physics in it, why, you think they ought to know physics. We used to have the same problem with chemists. We would often have a chemist on a Ph.D. examining committee, and the chemist would ask some question having to do with thermodynamics, which physicists, at least in Berkeley, didn't use much--the chemists used it a lot--and he would say, "Oh, that fellow doesn't know anything about thermodynamics. How can he get a degree in physics?"

If I can find out something more about what molecular biology is now, I'll bring it in sometime later.

Lage: It's such an important field. It is interesting to see its roots.

Helmholz: Yes. Those were the roots in Berkeley anyway, and I suspect that the lack of any sort of unification of that has kind of held back molecular biology, or whatever you want to call it, on this campus. When Stanley came here, he brought a man who had been trained in physics, Robley Williams, who was an electron microscope expert. So there was some more physics introduced into that field, which for a while was going to be called virology, because Stanley had discovered viruses. John Northrop [professor of bacteriology] and Stanley had discovered viruses. That was another step toward uniting the molecular biology on the campus, but it still--I think Stanley had plenty to do with running the virus laboratory, and he didn't pay a lot of attention to the problems of differing participants in molecular biology.

Lage: It seems like Berkeley is so strongly ordered by departments that they have trouble with these fields that cross several departmental areas.

Helmholz: Sure, yes, they do. [In 1945 at the suggestion of Ernest Lawrence, Birge, and John Lawrence, medical physics was set up under the Department of Physics. Lawrence, Joseph Hamilton,

Paul Aebersold, and Cornelius Tobias were the first members. [The last year that Medical Physics was listed under Physics was 1965-66. The decision was made that the biological interests of the faculty were sufficiently different from those of the faculty in Physics, so that Medical Physics should be established as a separate department.--ACH]

Tennis with Ernest Lawrence and General Groves

Lage: We've gone off in different directions, but we were going to get more on your life in Berkeley in these early years.

Helmholz: Yes. Well, as I say, I was married in the summer of 1938. I was away for something like six weeks, and there were a number of visitors at the radiation lab whom I didn't see. One of the ones was a fellow named Sid Barnes from the University of Rochester, whom I got to know very well because he and his family lived here in Berkeley during the activity of the Manhattan Project. He came, and he wanted to use the equipment that I had started using in the few months previous to his coming. He just came for the summer. He designed a beta-ray spectrograph, which I then used for a good deal of my thesis work.

Lage: So he designed it while you were honeymooning?

Helmholz: I was away, yes. That's right. It was a much better piece of apparatus than I had inherited from my predecessor in that field of work.

Lage: Where did you live after you married?

Helmholz: After we came back to Berkeley, we moved to a small house up on Shasta Road. We had made arrangements with the man who built it to rent it to us as soon as we got back. We lived there for about a year and a half. We had several parties, with people from the lab coming up to the parties. It was a quite convenient place. I could, but I didn't often, walk down to the campus. It was a nice small house. Betty and I put in a patio out in the back, and we had quite nice neighbors, one of them, Chester Crawford, lived right in back of us. We got to know them quite well. We kept seeing people from the lab a good deal, and, as I think I mentioned in that earlier oral history, Ernest Lawrence used to have a boat, a motor cruiser that he would take up the Sacramento River or take back into the delta.

Betty and I went with them once, when we went all the way to Sacramento and slept up there and then came back the next day.

Lage: Were these pleasant occasions or occasions where you were accommodating your boss?

Helmholz: No, they were very pleasant occasions. I think Ernest Lawrence was a gregarious person and liked to have people come with him on trips. Whenever he took the boat out, he would like to have somebody go out with him. Molly Lawrence was a long-suffering wife who would arrange for food for everybody. [chuckles] They were very pleasant occasions. Ernest would take a couple of the students along whenever he went on such a trip.

Lage: And then you also knew him as a tennis player.

Helmholz: Yes, and then I used to play tennis with him. He got me started playing tennis, well, almost as soon as I came. We would play over at the Faculty Club court and, more often than not, play doubles. I have played singles with him a few times. Later on, during the war, we moved over to play at the Berkeley Tennis Club. Ernest really liked to play tennis a lot, and that was one form of exercise that he sort of specialized in. He knew from his brother John that he should get plenty of exercise, and so he used tennis as that form.

Lage: You said he was enthusiastic.

Helmholz: Yes. He didn't have particularly good strokes, but his enthusiasm made up for his lack of expert training in tennis. Arthur Compton, who was a well-known physicist and came out here fairly often in connection with the Manhattan Project, liked to play tennis. So Ernest and I and Arthur Compton and I-don't-know-who-else would play sometimes. General [Leslie R.] Groves, who was the army man in charge of the Manhattan Project, also liked to play tennis, so Ernest got him out to play. I played with him a couple of times.

Lage: Do you learn about people from the way they play tennis?

Helmholz: Oh, you learn something different. I didn't really know General Groves the way some of the people did who were more prominent in the Manhattan Project work. I didn't know him as well as they did, but I met him quite a number of times, played tennis with him, and talked a little bit about physics and the uranium separation project. He was sort of a typical army person in that he wanted things done and done this way and that way and so on. He did have a hard time understanding how physicists operated.

Physics Research versus the Military Mind##

Lage: You say General Groves had trouble understanding how physicists operate. Now, how do physicists operate?

Helmholz: You try one thing, and if it doesn't work, you give it up fairly rapidly. Then you try something else. You don't have more than one thing in mind when you start the first one, and General Groves, I think, thought, "Well, you ought to be able to put down one, two, three, four, five steps. You just try them one after another." Physicists don't think of that many related to the immediate problem.

Lage: They think of one.

Helmholz: Yes. They think of one.

Lage: Then do they learn from the first mistake?

Helmholz: Yes, they learn from the first mistake, and they can see what ought to be the next step and so on. That wasn't the military way of doing things. It's a different field, really.

Lage: Does physics still operate in that way? Is that a basic modus operandi?

Helmholz: Yes. You have in mind the sort of long-range general problem that you want to solve, but you don't have a detailed plan to do this this week, and the next thing the next week, and the next thing the next week, because what does or doesn't work the first week will tell you a little bit about what you ought to do the second week.

Lage: Would he and Lawrence get into this sort of discussion?

Helmholz: Well, I'm sure they did lots of times. The people at Los Alamos--a lot of them whom I've talked to since--saw General Groves more often than I ever did, and so they would say, "He doesn't know much physics, but he still kept his hands off enough so that we were able to do the work." [chuckles]

Lage: He didn't try to design the experiments.

Helmholz: No, that's right. I thought he was a pretty good person for the job. I can imagine that there were plenty of army officers who would have insisted that the project be run the way they had in

mind that it ought to be run. They just didn't understand what the problems were. I know that General Groves had his enemies among the physicists and the people at Los Alamos, but the differences occurred at Los Alamos because that was where they had the more important and more numerous contacts with Groves.

Electromagnetic Separation of Isotopes: Berkeley's Role in the Manhattan Project

Lage: We were talking about tennis. But it leads so many places.

Helmholz: That's right. Betty and I made very good friends during those years. The Alvarezes left fairly early. I think they left in 1940. We made a lot of friends with people who came to work in Berkeley on the uranium separation work. And then, of course, we saw the people who were here. Particularly the [Francis] Jenkinses we saw a fair amount of. Jenkins was given the job of trying to coordinate the scientific work at the 184-inch laboratory. The magnet and the building for the 184-inch cyclotron were being built. The magnet was put up first, because it's the biggest thing in the building. The building was sort of built around it. That was started in 1941, I think, and completed in 1942. So the work on the electromagnetic separation that really started at the 37-inch cyclotron here on the campus was transferred up there. That's mentioned in the other oral history.

Lage: Right. Is that what you were involved with, this separation?

Helmholz: I was involved in the separation, really throughout the whole war.

Lage: Could you just expound a little bit on what that actually was?

Helmholz: Yes. It was what's called an electromagnetic separation. During the middle of the project, they got some patent lawyers to patent everything that the laboratory would discover. I don't know whether that was--well, I suspect that Ernest Lawrence agreed to that, but it may have been General Groves' idea also to patent everything.

Lage: Was it patented by the federal government?

Helmholz: By the federal government, yes. Some patent lawyer had the great idea that one ought to patent the electromagnetic

separation of isotopes. This had been discovered in about, oh, it must have been about 1905 by J.J. Thomson. And so the rest of us just laughed at that. I guess they probably did patent a few things, but that wasn't the kind of process that one could patent.

The reason that Ernest Lawrence picked on that particular method--there were a couple of methods of separating the uranium isotopes, which are 238 and 235. Electromagnetic separation uses a lot of magnets. The cyclotron has magnets, and so he picked on that one and said, "I'll go ahead with that method of separating the uranium isotopes." Then there was a process of gaseous diffusion which was used and turned out to be a more practical method than the electromagnetic separation.

Lage: Was that carried on in Berkeley also?

Helmholz: No, no. That was carried on really mostly at Oak Ridge but was started at Columbia by Urey and Dunning in, I believe, 1942. The production plant at Oak Ridge was called K-25.

Lage: So the diffusion method was more efficient.

Helmholz: Yes. It was harder to get working, and so the first Hiroshima bomb was made of uranium 235 from the electromagnetic process.

Lage: From Berkeley?

Helmholz: No, from Oak Ridge.

Berkeley was just a pilot plan for finding out how the apparatus would work and designing apparatus that could be built at Oak Ridge, Tennessee. Then at Oak Ridge, Tennessee, there were these big groups of magnets that were operated with the equipment that had been designed here. Uranium 235 is present only in about one part in 139 in ordinary uranium. So it was quite a problem to separate it from uranium 238. But by bending the ions in circles and collecting them separately you <u>can</u> do it.

Unfortunately, it turned out that the material had to be run through two stages, which were called the alpha stage and the beta stage. Ernest Lawrence and all of us didn't realize that it would have to be done that way when we started, which was in the fall of 1941. It had to be, because you could not enrich the material enough in uranium 235 to make a bomb out of it. It wasn't, I think, generally known by the people who were working here that the bomb was the ultimate destination of this material.

Lage: You didn't have a sense of what it was all for?

Helmholz: Oh, we knew it was for energy of some sort. I think that the people at Los Alamos really almost all knew. The physicists at Los Alamos and chemists who were involved there knew it was a bomb, but I think a lot of us here in Berkeley didn't know for sure what kind of a source of energy it would be used for. I guess it was fairly easy to understand that if the physics of it could be made to work, a bomb could result. But it wasn't apparent, when the work was started or even while the work was continuing here in the separation of uranium 235 from 238, that a bomb could be made. I think that the people at Los Alamos were the ones who had to make all the measurements to show that a bomb could be made, and it was important that all those measurements were done, and done carefully and done accurately.

Lage: But the people like yourself who were working here--was it just not discussed what you were about?

Helmholz: That's right. It wasn't discussed. I'm sure that Ernest Lawrence discussed it with his confreres who were well up in the Manhattan Project, because he would go to Los Alamos himself fairly often--I would guess, once every few months--and see how they were getting along. But most of us who were just involved in the electromagnetic separation didn't really know how the work was going at Los Alamos. There wasn't much, if any, communication between Los Alamos and Berkeley.

Lage: People didn't drive back for the weekend.

Helmholz: No, that's right. And there were quite strict limits on the security.

Lage: Was that an unusual thing to occur, when you're used to sort of a free academic setting?

Helmholz: Oh, yes, in that a few months before Pearl Harbor there was, oh, it must have been a captain or something like that, who was in the intelligence division of the army. He came out, and he was introduced to all of us. I think that we knew that he was in the army, but he lived at the Faculty Club and would have lunch and dinner with members of the lab and so on. He sort of tried to talk to us about the project, and I think that we told him more than he thought we should. His name was Nichols, I think, and he came back afterwards--after we had got into the war. He gave us all lectures as to what we should and what we shouldn't say and how to keep security.

Then they had code words so that uranium was never mentioned. It was called tuballoy, the code name for uranium. Uranium 235 was called magnesium and U-238 was called aluminum.

It was quite different after we got into the war. Then there was a large group of people hired to run this pilot plan up at the 184-inch cyclotron. The 184-inch magnet was never completed as a cyclotron magnet. The coils were completed, but the gap in the magnet, which was meant to be something like three feet, was left at six feet. They built the vacuum tanks to put into it, to run for the separation of the isotopes. There was a group, of which Segrè was the sort of leader, which tested the early separations to find out how well the isotopes were being separated. Then, of course, all the people used to run the machines twenty-four hours a day. All the people who were trained to run the machines didn't know much physics, and they had never heard of the electromagnetic separation of isotopes, but they had to be taught.

Lage: They were hired specifically during the war.

Helmholz: Yes. They were hired during the war. There were some electrical engineers and mechanical engineers and then women who were hired just to run the calutron, as it was called. They were told, "Well, now, look at this meter. This has got to go up to a certain point, and you just keep it there once it's there." They would be around to do that kind of running.

Lage: Before that, the graduate students had kept the cyclotrons going?

Helmholz: The graduate students, yes, sure. When the physicists got to Oak Ridge, Tennessee, they had a terrible time, because the Oak Ridge, Tennessee, natives knew even less than the women that were hired here, so the physicists had a terrible time teaching the Oak Ridge natives. Fortunately, I never got to Oak Ridge, but I have heard the stories about the problems.

Lage: Was this a round-the-clock assignment for you? Was it a terribly busy time?

Helmholz: Yes, it was a very busy time. You see, in June 1940, when I got my degree, I was hired by Ernest Lawrence to help with the 60-inch cyclotron, which was being run at that time particularly to try to use neutrons in cancer therapy. So you needed people who knew about cyclotrons to do that. Well, shortly after I started working at the 60-inch cyclotron, this opportunity to teach came along when Professor Brode left. And so I started teaching.

Lage: As a lecturer, was this?

Helmholz: Yes, I think I must have started as a lecturer, and then I was appointed instructor. Then, in about 1942 or '43, I was appointed assistant professor. I remember that [Robert Gordon] Sproul used to live in the house that's now called the University House. He had a sort of an afternoon cocktail party for all the people who had got appointed--new appointees. I was invited one time, and when we were working at the lab, we always had to wear a name badge. So I went down there and didn't take off my name badge. When I came in, Sproul said, "Well, I can see who you are, but here--take a different name badge."

In 1941, after Pearl Harbor, we got a lot of students from the army and the navy and the marine corps who had to take special courses. Some of them took the regular courses which Berkeley had for undergraduates, but there were others that were in special courses. I never had anything to do with the special courses, but there were plenty of students in the regular courses. I had a sort of a double duty. I worked part time at the Crocker Laboratory on the 60-inch cyclotron, and then in 1941, as soon as the work with uranium started, Ernest did the electromagnetic separation on the 37-inch cyclotron. I was familiar with the 37-inch cyclotron and started work there.

Lage: Was this the separation also?

Helmholz: Yes, that was the separation. The work at the 60-inch was never separation. The other thing about bombs is that some of the bombs were made with plutonium, and that was a separate process, which really was started mostly in Chicago at the University of Chicago and involved using nuclear reactors to make neutrons to make plutonium from uranium 238. I worked at the 37-inch cyclotron. By that time, I think they had other people who were able to operate the 60-inch cyclotron satisfactorily. By that time, Ernest Lawrence had just really stopped all the research work in physics and started on this uranium separation business. So new people came in, and, of course, most of those new people went up to the 184-inch as soon as that was opened. That, I'm sure, is all listed in John Heilbron's account of the history of the Rad Lab.[1]

Lage: Did you go up to the 184-?

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[1]J. L. Heilbron and Robert W. Seidel, Lawrence and His Laboratory: A History of the Lawrence Berkeley Laboratory, Volume 1, (Berkeley: University of California Press, 1989).

Helmholz: I stayed down at the 37-inch because there were experiments on the uses of uranium. You see, in this electromagnetic separation, you have to have a discharge of some sort that makes ions out of the uranium atoms, knocks electrons off, and then you pull them out and let them go around in the magnetic field. So I stayed down at the 37-inch cyclotron and did some work with a few other people. I think there were, oh, three or four others. We didn't always work twenty-four hours a day. We would often work just sixteen hours a day.

Lage: Did you have people hired to run the cyclotron down here, too?

Helmholz: I think that most of the people that were at the 37-inch were essentially physicists, Ph.D.'s, and there were undergraduates and younger people hired to run the 60-inch cyclotron. So then, eventually, a group of English people came under a fellow named H. S. W. Massey, who was an expert--well, he was a very fine theoretical physicist, but he got a group of the British started in working to find out how these discharges in magnetic fields operated. At that time, I moved on up to the 184-inch and started to work up there. I think that must have been, probably, '43.

Lage: Did the British work on the 37-inch?

Helmholz: Yes. They did some interesting things. I was always interested in what they were finding out, but after the war I never went back and read their papers to find out exactly--but we used some of their ideas in designing new apparatus at the 184-inch. Massey had a group of about three or four who worked at the 37-inch cyclotron. There was also a group from Westinghouse, who worked under a man named Sleppian. That, I'm sure, is also in John Heilbron's account. They worked at the 37-inch cyclotron also. Also a group from General Electric under Kenneth Kingdon worked on the Manhattan Project in Berkeley.

Wartime Instruction and Campus Life

Helmholz: So I kind of had a double life there for a while, teaching and working at the cyclotrons.

Lage: You were one of the few people who did, weren't you?

Helmholz: Yes. There weren't too many people who were teaching. Ordinarily, physicists were supposed to teach part time and do research part time. My time for research was taken up with the

Manhattan Project. But there were several other people who were brought in to teach physics. Perry Byerly, who was a physicist by training but was the head of the seismographic station, came to teach one of the undergraduate junior-senior courses, and I was teaching another one. Then I taught a graduate course in physics, toward the end of the war, the one that Ernest Lawrence had taught before.

Lage: Was the student body small? I would think most of this age group would be off in the war.

Helmholz: Most of the age group was. There were still--I don't know how many. There must have been forty or fifty graduate students who either were graduate students--well, there were a few women. Some of them were conscientious objectors, and some of them were working toward a Ph.D. with the idea that they would just go into the army work after that. But there were a lot of undergraduates. There were army and navy classes that needed all of the undergraduate courses in physics.

Lage: They were taking the regular courses?

Helmholz: They were taking the regular physics major.

Lage: What was the army training them for?

Helmholz: They were training for scientific work of some sort, either in ballistics or in the navy for just regular navy duty and so on.

Lage: Was this a different kind of group to teach to? Were they as well-prepared as most of your students?

Helmholz: Well, yes. They had been carefully selected, I think, and in a way they were just as good as the regular undergraduates. But they tried to squeeze their program so that they would take three semesters in one year instead of having two semesters and a summer session or something like that. The semesters were shortened, as I remember, also.

Lage: So you worked fast.

Helmholz: Yes. You could go faster.

Lage: What was the campus in general like?

Helmholz: Well, the campus had a lot of people in uniform. It was, you might say, not completely different from the regular sessions except for the fact that there were a lot of uniforms around.

Lage: Were there more women in proportion?

Helmholz: In proportion to the non-army, navy, marine corps, there were more women, but we didn't get a lot of women in physics even in those times. I think when I was a graduate student, among the seventy-five or eighty graduate students, there must have been three or four women. Maybe that went up to ten or something during the war, but the rest of the graduate population went down also. I think a fair number of people that worked at the 184-inch on the Manhattan Project also tried to take some courses, because they wanted to get ahead in their academic work. So there were both kinds there. Ernest Lawrence used as many as he could possibly get hold of, of people who had some physics training and who could work at least part time.

I think the atmosphere was, of course, different, but the atmosphere started being different after Pearl Harbor. It was different from that time on. In some ways, we were more impressed with that because we knew that the Manhattan Project was trying to make something that would be useful in the war effort. But there were students from the marine corps who were taking special courses in engineering, and all sorts of war-related education was going on. Of course, that didn't start until well after Pearl Harbor, but once it started, the campus had lots of uniforms. Physics, chemistry, math, and engineering had lots of courses for the military.

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Helmholz: Particularly for their officer corps, the army and navy needed people with some knowledge of physics. That was where all these students that we had here were headed for--essentially, the officer corps. I guess it's hard for me to remember really how much different the campus was after the war started. It certainly was different. When I moved up to the 184-inch to do work, I was very much impressed with how much different it was there from the kind of classwork that was on the campus. I think the classwork was less different during the war than from the regular period than the laboratory was.

Lage: What impressed you when you worked up there?

Helmholz: Well, just that there were a lot more people working all on this one project. As I said, all the projects were working twenty-four hours a day.

Lage: It must have been like a beehive.

Helmholz: Yes. We had to make a schedule each week as to who would be on the swing shift and who would be on the owl shift. We fortunately had a few people, one in particular, whose name I've forgotten, who was willing to work on the owl shift. He would work on the owl shift all the time. For his days off, we would bring in somebody else from the swing shift or the day shift. I remember I would occasionally work the owl shift, although I didn't very often. It was, as you can expect, a full-time operation.

They had a large group of people, maybe a dozen or fifteen, whose job it would be just to service the apparatus between runs. You would be running one source and receiver in the magnetic field, and then you would be planning for the next run. Then you would have to consult with these people who were preparing the source and the receiver for the next run while you were running one. The runs might last thirty-six hours, and you would try to collect enough material so that Segrè's group could test it out and see what the result of that run was. When that run was over, the apparatus could be changed, and it would be serviced. The one that you had been preparing for that run would be moved into place and run again for thirty-six hours.

Lage: Sounds like there was a tremendous sense of urgency.

Helmholz: Oh, yes, there was. Let's see. You were also interested in, you might call it the social life.

Lage: Was there any? Was there time for any?

Helmholz: Yes, there was some. When a time like Thanksgiving came around, why, the lab would, I'm sure at least in the early part of the war, shut down for Thanksgiving. I can remember having, oh, I don't know, fifteen or eighteen people at our house. By that time, we were living out in north Berkeley and having them for Thanksgiving dinner. There wasn't a lot of other social life. You should get Betty to come in and give you an interview on the social side.

Lage: That might be a good idea.

Helmholz: That would be fine.

Lage: The activities that you were telling me about off the tape last time, having to do with the section clubs, were in the fifties and later?

Helmholz: Yes, the section clubs were later. I can't remember. I think some of the section clubs went through the war, but I'm not

sure. I'd have to--I'll get Betty to come in sometime and tell you about that. I used to have lunch at the Faculty Club and would try to see other people. The physics department was sort of decimated by war work. Harvey White went away, and Brode went away. Victor Lenzen stayed, and Birge stayed, but Victor Lenzen and Birge never ate at the Faculty Club anyway.

Let's see. Who else? Jenkins was gone toward the beginning of the war. Then, later on in the war, when Oak Ridge opened up, he went to Oak Ridge a number of times. But Betty and I would see his wife Henrietta and him perhaps several times a year. I'm sure sometimes we would see them twice in a month. We didn't see them a lot. Brode was gone, and Oppenheimer, of course, was gone. I think Oppenheimer left in--it must have been '40 or '41. '41, I think it was. Segrè was--as he points out in things that he's written--an enemy alien at the beginning of the war, because he was still an Italian citizen. He was soon brought into the work of the Manhattan Project and worked here in Berkeley for a while, I would guess probably through '41, and then, in '42, went to Los Alamos. He had a couple of graduate students working with him.

Lage: So you really didn't have your standard professors at all.

Helmholz: No. That's right. Birge had a hard time finding people to teach the courses because there were more courses than usual to give, because of the special requests of some of the armed services. But, as I said, Ernest Lawrence got a lot of these people to come here, people who were physicists at other universities and other small colleges, that came here and worked on the Manhattan Project but who could spend a few hours a week required for giving lectures and so on.

Postwar Readjustments and New Accelerators

Lage: What was it like after the war, when things kind of had to be reconstituted? Were you in on some of that planning?

Helmholz: Well, not really. I could see what was happening. After the war, Birge arranged that we would go back to the regular semesters and the more or less regular numbers of classes. That took about--well, let's see. The war was over in August. There was work done at the laboratory at the 184-inch cyclotron magnet, just sort of finishing up things. During that next academic year, the numbers of people who had been hired for the war work fell off quite rapidly. I would say almost all of them

were gone within six months. Then there were new people coming who had been away from Berkeley like Alvarez and McMillan and some of their cohorts, because they were starting up work in physics again. They were brought back to teaching so that by August of 1946, the regular classes were going, and there were a lot of graduate students who wanted to work in physics, because physics was obviously a very popular field at that time.

Ernest Lawrence realized that he couldn't build the 184-inch cyclotron in the same way that he had planned to build it. But McMillan, while he was still at Los Alamos, had an idea for accelerating ions to higher energy. At the same time, a Russian physicist named Vladimir Veksler had the same idea. That idea was used to rebuild the 184-inch cyclotron. The magnet was there, and, of course, as soon as the electromagnetic separation work was finished, the magnet was rebuilt with pole faces that were closer together, so that they could get the higher magnetic fields that were needed for the new synchrocyclotron. That's what those things were called--synchrocyclotrons.

There was some work done at the 37-inch cyclotron which had been used during the war mostly by Massey and his group of British people. There was some work done there to test out some of these ideas of the synchrocyclotron. Then, as soon as those worked out, the plans went ahead for the final design of the 184-inch cyclotron.

In the meanwhile, Alvarez came back from Los Alamos with the idea of building a linear accelerator, which is sort of interesting, because when Ernest Lawrence first came here in 1930, one of his first projects was to use a linear accelerator to accelerate ions, and a fellow named Dave Sloan helped him with that. Then he dropped that idea as soon as the cyclotron started developing. With a linear accelerator, you just start with an ion here, and then accelerate it in a straight line until it hits the other end. If you can give it enough acceleration, that's fine, but the techniques for developing the power that you needed to get it going fast enough were very primitive in '31. Alvarez had done a lot of work with high frequencies during the war, particularly when related to radar. So he had this idea for building a linear accelerator to accelerate protons. He got a fair-sized group of graduate students and some faculty members also to build the linear accelerator.

Lage: What about funding? Did he have to get special funding, or did the government fund it?

Helmholz: Ernest Lawrence had the Manhattan funding, and he got it continued. Let's see. The Manhattan Project turned into--well, after a while, it turned into the AEC, but there were a few years there where I think the army just continued funding it.

Lage: So funding wasn't a problem in those years.

Helmholz: It wasn't a problem.

Then McMillan came back with an idea for building an electron accelerator, which was called the electron synchrotron and used his same idea. That was where I worked after the war. So those projects were started, oh, within a year of the end of the war. They had to start from scratch, because there wasn't any apparatus to add onto, so that one had to start with building for McMillan a magnet that would run with alternating current and so on. Alvarez had to start with everything from scratch.

Lage: Did this give them little centers of power, perhaps, in opposition to Lawrence?

Helmholz: Yes. Lawrence was much more interested in the 184-inch cyclotron. That's the one that he had agreed to build from his Rockefeller Foundation grant. So that was his great interest, but he sort of kept touch, mostly through Alvarez and McMillan, with these other machines. Then there was a fair-sized group in chemistry that [Glenn T.] Seaborg was in charge of. Seaborg and [Isadore] Perlman were both interested in radioactivity. The 60-inch cyclotron was still operating full-time, and they could get radioactive materials there.

Chemists and Physicists

Lage: Was there good cooperation between the chemists and the physicists?

Helmholz: Yes. There was good cooperation.

Lage: I went to the public lecture that Leon Lederman [Nobel Prize-winning physicist] gave. He had a lot of funny gibes directed at chemists. [laughs] I wondered what the dynamics were.

Helmholz: When Birge came here in 1918, I guess it was, the chemists and physicists were just on opposite sides of a fence which was pretty high. But Birge always claimed that one of his great

achievements was getting the physicists to talk to chemists, and chemists to talk to physicists. When I was a graduate student, and I think in the years afterward, there was a feeling among physicists that chemists were not interested in the same sorts of things, even though they realized that Seaborg and Perlman and a few other chemists were interested in radioactivity.

Lage: But from a different point of view?

Helmholz: From sort of a different point of view. Actually, physics in Berkeley has sort of lost interest in radioactivity. I kept on my interest in radioactivity for a good many years after the war and had a number of graduate students working in it and so on. But physicists got too interested in so-called "high energy physics," and the chemists, even, have slightly invaded that field.

Lage: Did you have more relations than other physicists, then, with Seaborg and his group?

Helmholz: Yes, I think I did, although not particularly close relations. But they had up in chemistry, on the hill, equipment that physicists had never set up, equipment for counting radioactivity, that I used a good deal, and McMillan's and some of Alvarez's students used. It was a good relation. I think we got over the feeling that they were treading on our toes. I'm sure the chemists had a feeling at one time that: "What are those physicists doing over there?" and so on.

But the work in nuclear physics, which Ernest Lawrence had started back in the thirties, has sort of gone over to chemists. We've made several attempts in the physics department to get people to come here as assistant professors in nuclear physics and have never really been able to get anybody. I think that people outside of Berkeley think that: "All <u>that</u> work goes on in chemistry, so why should I go to Berkeley and be in the physics department when all the people I'd want to work with would be in chemistry?"

Lage: Is that true just of this campus, or does that happen--?

Helmholz: No, that hasn't happened so much elsewhere. There are plenty of places in the United States--universities--where a fair amount of nuclear physics is done in the physics department. So it has always been a problem for the department chairman here to try to get nuclear physics going.

Lage: People want to come to something that's already strong.

Helmholz: Yes, that's about right. The chemistry department will occasionally hire somebody who's got his Ph.D. in physics, somebody who's had some training in chemistry but may not have specialized in that.

Impact of War on Physics Research

Lage: Well, I'm wondering if we should break here. We've gone through the war and some of the changes it brought to the campus.

Helmholz: I'll try to think of some more things that I might say about the war. It was a big change when the war finished, because we all were sort of anxious to get back to the regular academic work and the regular research work.

Lage: Was the effort during the war in your research--was that kind of a break from your progress as a research physicist? Or did it add to it?

Helmholz: Well, I never felt that it added much. I just went back to the kinds of research I had been doing before the war. There were a few people who stayed on around the laboratory and tried to finish up some of the things. There were even a few theses written by people who had worked hard during the war and could make a thesis out of the kind of work they had done.

Lage: That wasn't the usual?

Helmholz: That wasn't the usual thing. I think almost everybody was anxious to get back to his previous kind of work, even--

Lage: So it was more of a diversion. I talked to Sandy Elberg about his work in microbiology, and the war was a tremendous boon to his research.[1] And, I guess, in general, it brought a lot of advances.

Helmholz: It was a boon to us because we had an awful lot of money after the war, but I don't think there were very many people in Berkeley who kept on with the kind of work that they had been doing during the war. Some of the people who had been at Los Alamos stayed there. Well, there were several, but I guess

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[1]Sanford S. Elberg, Graduate Education and Microbiology at the University of California, Berkeley, 1930-1989, Regional Oral History Office, University of California, Berkeley, 1990.

nobody from this department stayed there. People from other universities, I know, did stay at Los Alamos and kept working on the kinds of things that they had been working on during the war.

Lage: Did they stay for a long time?

Helmholz: Yes. Some of them just stayed permanently and so on. We've since sent a fair number of our graduate students to Los Alamos, because their work is really in pretty high-powered research problems--not all associated with bombs, but some of it associated with regular nuclear explosives, such as Edward Teller wanted to use to make harbors and so on. They do other kinds of work there also.

Lage: You sent them there as graduate students or after they had completed their degree?

Helmholz: After they got their degree. There have even been a few--they have a pretty good-sized accelerator now, and one of the people here, I think it's Ken Crowe, whose desk that is [gesturing], has sent one of his graduate students down there to do an experiment for a thesis. So it's a good-sized laboratory there and pretty well-rounded, too, in physics and, I guess, in chemistry also, and probably partly in biology.

Lage: Well, that's a topic we'll take up later--the relation between the University here and Los Alamos and Lawrence Livermore laboratories. You've been involved in that scene, on several committees.

Helmholz: Yes, I have been, in all sorts of ways.

Lage: We'll make that a separate topic one day. I think this is a good place to break, and then we can start up with postwar activities.

Helmholz: Yes, all right. Fine.

VI THE DEPARTMENT OF PHYSICS IN THE POSTWAR YEARS

[Interview 2: October 10, 1989]##

Increased Enrollment in Physics

Lage: We talked last time about the wartime, World War II, on campus. I thought we would start out with the changes after the war. I understand that you had a new type of student and a lot of activity.

Helmholz: Yes. We had an increasing flow of graduate students who had been attracted to physics by the war and by the atomic bomb development and so on.

Lage: Did any of the people who had been in the courses for the army and navy continue?

Helmholz: Yes, a few of them either came back or continued on, but there were a number of graduate students who had started their work before the war and then came back to finish up--some who had worked in Berkeley during the war, and they finished up. Some of them, I think, two or three of them, got their Ph.D. degrees with a thesis on classified work, so that was a considerable problem. I think Ernest Lawrence had to go to bat for them and say, "I've read this, and it's a good piece of work," and so on. Those things got declassified very rapidly. They really were in the process of being declassified when the war ended.

Lage: I saw on your bibliography list that there were seventeen classified research items.

Helmholz: Is that right? [chuckles] Well, I've forgotten about them, but we did write up things as the war went along. I don't think any of them were really worth publication in the physics journals today.

Lage: So they were probably never declassified?

Helmholz: I don't think--well, there was a good deal of work that was declassified, that became unclassified when the war finished. There have been some histories of the kind of work we did which were written up. The main work on the bomb was, of course, at Los Alamos. That was the most exciting, but there was the Smyth Report which appeared after the war--it must have been six months or so after the war--which detailed a good bit of the research and the making of the atomic bomb. That didn't have a lot about the separation of uranium isotopes, but it had enough so that people could understand it.

Lage: Okay. I got you off the track here.

Helmholz: Well, as I was saying, there was quite a substantial increase in the numbers of graduate students. I should look this up in Birge's history as to how the number increased. Actually, while I was chairman, I guess we got up to over 300 graduate students at one time in the department. That's a few too many, as we discovered, and so it's back now to something like 225 or so. But during Birge's years, I think, it certainly went up over 250.

Lage: What had it been before the war, in general?

Helmholz: Oh, just eighty or so. I think the biggest number before the war was something like eighty-five.

Lage: So you tripled your size.

Helmholz: Yes, almost triple the size. And, as I perhaps mentioned before, the numbers of people who were needed to staff the courses substantially increased. Those people had to be hired in part by new hires, assistant professors. There were some associate professors and full professors also. And, in part, by getting people from the Lawrence Berkeley Lab, which was called the Radiation Lab in those days and which had substantial numbers of people working on these new accelerators that I think I mentioned last time: the 184-inch cyclotron, the linear accelerator, the Alvarez 32-million volt proton accelerator, and the synchrotron, which McMillan took charge of building. So there were people around that Birge was able to use.

Lage: As instructors or lecturers?

Helmholz: As lecturers, yes. They were certainly competent people, and a number of those people later were added to the faculty. My successor as the chairman of the department was Burton Moyer, and he was not only a very good researcher but a very good

teacher also. He was hired in about 1949 or '50 to give courses and then was added to the faculty. As I think I mentioned, a number of people were added to the faculty part time so that they would be one-third time on the physics payroll and two-thirds time on the Radiation Lab payroll. That process has, I believe, ceased completely or almost completely now, so that the physics department has only one or two people who are less than full time.

Lage: I see. So you don't have that kind of shared appointment anymore?

Helmholz: No. The process at the time was judged to be no danger to the University, because the University set aside its annual fee for managing the Radiation Laboratory, which amounted, by the time that Ernest Lawrence died, to something like six or eight million dollars. They set aside this, and nominally, it was going to be for the keeping on of faculty if the Radiation Laboratory closed down, so they would have money to support the additional faculty in both physics and chemistry.

Lage: And keep the staff going.

Helmholz: Keep the staff going, keep the full professors, to whom they had granted tenure, going. I forgot now how many were in physics and how many in chemistry, but I think approximately equal numbers. This was of concern to the people who were granted tenure. They said, "What will happen to my half a position if I'm half time at the Radiation Laboratory?" They had to make some provision for that. I think the Regents now put aside that money and use it for other special purposes. They don't keep building up that money anymore.

A fair amount of that money was put into the Lawrence Hall of Science after Ernest Lawrence's death. When they decided to build the Lawrence Hall of Science, I think there was something like six million dollars or so that was available for building the Lawrence Hall of Science. Harvey White went out and raised a good deal more at the time.

New LeConte Hall

Helmholz: There was also some sort of, let's call it, crush on research facilities, because there wasn't space for all the people who wanted to do research here on the campus. But there was a good

deal of research space made available at the Radiation Laboratory.

Lage: For the graduate students?

Helmholz: Yes. A lot of those early graduate students did work in nuclear and high energy physics. Then, in about 1947 or '48, Birge got the money to build this building, which is called New LeConte Hall. Harvey White was the one who did a lot of the design of the building. I didn't have much to do with that, but I can remember it was sort of exciting to find out how he had planned it and what had been planned for it.

Lage: Was Harvey White a professor?

Helmholz: Yes, he was a professor of physics.

Lage: How did he happen to take on the planning?

Helmholz: Well, he had been a spectroscopist before the war, and he and Jenkins were the two people in spectroscopy. He became, I would say, somewhat more interested in teaching than in research spectroscopy. He did keep on with a few graduate students after the war, but I think his last graduate student got his degree in '52 or something like that. I think he probably just volunteered to take on this new building. We used to have another sort of a caretaker for all the equipment, who was a majordomo in ordering and taking care of equipment and so on, named Eugene Viez. He and Harvey worked very closely on the design of this new building. They did a nice job.

They decided at a fairly early time to have the high ceilings that we have, but years later, when Birge Hall was built, the high ceilings were given up so that they could have more floors. New LeConte has floors that lead directly into old LeConte, which was, of course, built in something like 1923. They also took care that there were new lecture halls. The library had to be, you might say, changed. We had a fairly substantial library after this new building was built, and it was joined onto one of the old lecture halls in the old building, so that that became a part of the library.

There was one large lecture hall in old LeConte. It seated over three hundred people--I think 310 or something like that. It was a very broad room and not very deep. It was a little bit difficult to lecture in, but I can remember that Harvey White introduced the idea of having examinations--oh, I don't know--I think in Physics 10, which was the sorority physics course. He had an examination almost every week, a short examination of ten

or fifteen minutes, and he would put alternate examinations in alternate seats, then collect them all at the end of the period. That room was eventually given up as a lecture hall, and the lecture halls in this building, in New LeConte, seat at most, I think, 210.

Lage: It just wasn't necessary to have such a large hall?

Helmholz: It wasn't necessary to have, or we would give, for many years in the 1950s, two sections of a course in one of those two lecture halls that seat two hundred in place of one section of the old course. Then there were two lecture halls also in this building, which would seat about a hundred. So there were two lecture halls that seat two hundred and two lecture halls that seat one hundred.

Lage: You had some large undergraduate courses.

Helmholz: Yes. We always had a course for physicists and engineers. That used to be 1A-B-C-D. Then we had the two-semester course for biology students and pre-medic students. That was 2A-B. Then, we had Physics 10 [for non-science majors], which was a course that satisfied the science breadth requirement. That's the one that was called sorority physics.

Lage: How did professors feel about teaching Physics 10?

Helmholz: Oh, some of us kind of enjoyed it. There were some people who would never give it. Birge had to make do with the rest of us who would give it. But it was kind of fun. You used every lecture demonstration in the book that you could find and made a great deal of the demonstrations. About 1950, Harvey White stopped giving that course. He had written a book about it, called Classical and Modern Physics.

Lage: About--

Helmholz: Sort of on the basis of having given the course a number of times. He had every lecture demonstration that he used with a picture of it and so on in the book. It was a very elementary course, and it was too big actually to cover in one semester. But you just picked things out of it to give.

Then, after the war, people began to think, "Well, there must be other ways of giving a course that might teach students a little bit more about some aspect of physics and less about others," so that the course got to be changed around. I can remember one of our faculty--I think it was Walter Knight--started to give a course which had more astronomy in it. At

that time, I think there was no Astronomy 10. Then, of course, years later, there was Physics for Poets.

Lage: [laughs] I didn't hear about that one.

Helmholz: Well, there is a book named Physics for Poets.

Then, the upper division courses remained pretty much the same, except that we began to give quantum mechanics, which had only been given as a sort of an optional undergraduate course. As a matter of fact, I'm not even sure that it was an optional undergraduate course. It had been given as a graduate course. Oppenheimer had a regular course in quantum mechanics, but then, after the war, everybody realized that it ought to be put in undergraduate education. So, I think, McMillan was the one who started giving that course as an undergraduate course. We had a course in sort of atomic physics, Physics 121, and then this course in quantum mechanics, which is 115. I think at the beginning, students were not required to take it, but soon after the war, that began to be a requirement for the undergraduate physics majors. We had fair numbers of majors in those days. I think there were something of the order of sixty to perhaps ninety majors--that is, in their last two years.

Lage: That's not nearly as many as you had graduate students.

Helmholz: No, not nearly. Well, we still don't have as many undergraduate majors as we have graduate students. I remember for a few years after the war, I was the major advisor. Leonard Loeb had been the major advisor for many years, and then I think he got tired of it, as I did too, and so I became the major advisor. I think there was one other advisor for undergraduate students, but it meant seeing all the students sometime during the fall semester of the year in which they were going to graduate, and signing their study lists each semester. Then, in the last semester, getting some idea from them as to what they wanted to do after they graduated--whether they wanted to apply to graduate school or not--and writing letters of recommendation for them if they did want to go.

Lage: Seems like a big order to handle so many students.

Helmholz: Well, it was, yes. There was one other advisor, and I've forgotten now who it was. I was the one who was assigned by the College of Letters and Science to sign the final list of those who had satisfied all the requirements and so on. But there were good students, and it was kind of fun talking to them.

Then, of course, I had graduate students working with me beginning in about 1947. They started working on problems in radioactivity and nuclear structure using the 60-inch cyclotron, which kept going then.

The 37-inch cyclotron, which was the machine that I had used when I was a graduate student, was torn down. After the war, it was used for a short while to test the ideas of the synchrocyclotron, which were then used in the 184-inch cyclotron. As soon as that was finished, it was torn down and sent to UCLA, where they used it as their cyclotron. Then, after UCLA finished with it --I guess it was Harvey White who said, "Well, why don't we bring it back to Berkeley and set it up in front of the Lawrence Hall of Science as a memento to cyclotrons and to Ernest Lawrence?" So that's where it sits right now. It was a venerable machine. [laughter]

Lage: It's nice to have it preserved and on display.

Adding New Faculty, Representing New Fields

Helmholz: The graduate students--as I say, a good many of them used the facilities at the Radiation Laboratory for their work. Then there were an increased number who went into theoretical physics.

Lage: Are these your graduate students or in general?

Helmholz: No, in general. After the war, Oppenheimer for a while thought he might come back to Berkeley. He did come back, but he was always getting called to Washington to advise on problems of the atomic bomb and so on. In 1947--I guess it was '47--he was asked to become the director of the Institute for Advanced Studies at Princeton. He decided then that he would leave Berkeley permanently.

Lage: Was there an effort made to keep him here?

Helmholz: Oh, yes, I think so. It was sort of logical for him to do that, because, even in those days when we had fair plane service, you can't go to Washington once a week. From Princeton, you can go to Washington once a week or even twice a week as you need to.

At that time, then, [Robert] Serber, who had been one of Oppenheimer's students--well, not really a student--he had worked as a postdoc in Berkeley before the war and had gone to

Illinois for a couple of years just before the war and then gone back to Los Alamos to work with Oppenheimer. He came here as a regular faculty member. Then, we got a fellow named Gian Carlo Wick, who was also a theoretical physicist. Harold Lewis was a young assistant professor and a very bright theoretical physicist. He had a number of graduate students. Then there were other people added to the faculty, both in nuclear physics.

In about 1949, the physics department realized they would have to get people from other fields other than the ones Berkeley had stressed before the war, which were essentially spectroscopy, discharges in gases, cosmic rays, nuclear physics, and then, after the war, high energy physics, as it was called, and theoretical physics.

Lage: What areas did you want to--?

Helmholz: Well, they wanted to get into solid state physics, which was an important area. I think it was about 1949. I wasn't in on these discussions, because I didn't become a full professor until '51 or '52.

Lage: Is it only the full professors that would get involved in this kind of planning?

Helmholz: Yes, they seemed to get together more. Birge didn't have many meetings at which he would invite the whole staff. Nowadays, you have to. For the discussion of new faculty members, you have to have the whole staff present. In those days, you didn't. So Birge would get the full professors together and discuss what ought to be done about new faculty. At that time, solid state physics was one of the chief areas in which Berkeley had nobody. So it was decided to get one or two, at least, members and to start up a group in solid state physics.

Lage: Does it make it hard to hire when you have nobody?

Helmholz: Oh, yes. Jenkins was one of the people who went east and looked for new faculty members. He knew a lot of people in physics, and so he inquired around, and Brode the same way, and McMillan went. There were a number of people who were members of the National Academy and would go to the National Academy meetings, which are always the end of April. In fact Birge was in charge of setting the calendar for the University, and it was often said that he would put the spring vacation at a time when all the physics meetings were, so that the physics faculty could go back east.

Now, the first person in solid state physics who was hired just as a visitor was Charles Kittel, in the fall semester of 1950.

Lage: Was that a standard way of bringing people over?

Helmholz: I think it was decided that we ought to get a senior person, and Kittel had a good reputation as a theoretical solid state physicist. So he was invited out for--let's see, where was he at the time? It may be that he was at Bell Labs at the time, but he had never really been in academic work much. He had a stammer which bothered him, and when he got to lecturing, it didn't really bother him as much as it would sometimes in private conversation. So he came out in the fall of 1950.

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Helmholz: He was here for a semester, and then everybody seemed to be quite pleased with the way he taught. He liked the idea, and so he was on hand as a full professor in 1951-52.

Lage: That would be a big change from straight research.

Helmholz: Yes, it was, but he liked students. He, I think, insisted, and the department agreed that he should have somebody in experimental work. So they got a fellow named Arthur Kip, who had been a graduate student of Leonard Loeb's before the war, had gone to MIT, and then during the war had worked on radar problems and the like, and had stayed at MIT after the war. He was invited to come out as assistant professor with Kittel in 1951-52 and start experimental work. He and Kittel worked very closely together for quite a number of years.

Theorists and Experimentalists

Lage: Is that a usual case, where you have the theorist and the experimentalist working closely?

Helmholz: Yes. I think that it was in part the fact that we wanted to attract graduate students who would be interested in both theoretical solid state physics and the experimental solid state physics. Oh, even before the war, Oppenheimer had worked fairly closely with Ernest Lawrence on some of the problems, where he would work out the theory and tell Ernest Lawrence, "Well, you ought to check that theory and see how it works." Ernest Lawrence would say, "Well, isn't this a little bit strange?" and

Oppenheimer would say, "Well, let's work on it." They would work on it and find a theory of why it would seem strange.

Lage: It seems like a very logical thing to happen, but---

Helmholz: Yes, it is a logical way. I think physics has sort of changed in a sense--well, particularly high energy physics. I guess this is probably true of solid state physics, too, that the theorists suggest more ideas for experiment, and the experimenters then do the experiment that tests out the theory. In the old days, it was less of that, and more of the experimenters would do what they thought was an interesting experiment. If they didn't get quite the answers they expected, they would go to the theorists and say, "Well, what's the explanation of this?"

Lage: So there's more leadership from the theorists today?

Helmholz: There is now, yes. I think most people would agree. In the beginning, as I think I've mentioned, Oppenheimer was the only theorist. There were six or eight experimentalists who would consult him if they needed advice on what the proper theory might be. Nowadays, the ratio is more like, oh, I would say, three or four to one, experimentalists to theorists.

Lage: Here at Berkeley?

Helmholz: Yes. That's pretty much true, oh, I think, throughout the world of physics. I'm not sure if in foreign countries that it's quite so much true, but I think, for example, in England, France, and Germany, it is.

Lage: Do you ever have people who cross that line and do both?

Helmholz: Oh, yes. Yes.

Lage: Or does it take certain different qualities of mind?

Helmholz: It does, I think, take different qualities of mind, but there are people who can cross the line pretty well. Before the war, one of the interesting experiments that was done at Berkeley was an experiment with slow neutrons and measuring the magnetic moment of the neutron. Luis Alvarez, who was always an experimental physicist, knew Felix Bloch, who was at Stanford, quite well. Felix Bloch was interested in this, and so they used for the neutrons, sort of the same idea that [Isidor Issac] Rabi at Columbia had used in measuring magnetic moments of atomic beams or nuclei. They measured the magnetic moment of the neutron.

Felix Bloch had been a theoretical physicist all his physics life, and I remember that when Rabi happened to come out one time that summer when they were doing the experiment, Rabi said to Felix, "Don't you find experimental physics wonderful?!" I think Bloch said, "Well, yes. It's sometimes difficult, too." But Rabi had been the same kind of a person. He was able to start out as a theoretical physicist and end up mostly in experimental physics. Actually, he had quite a number of outstanding students in experimental physics.

Another person named Willis Lamb, who was one of Oppenheimer's very good students, turned experimentalist--I'm not sure completely. He went to Columbia and worked at Columbia. He had an idea for an experiment, which he followed up and did a quite famous experiment measuring what is called the Lamb shift. So there are both kinds. There are more people, I think, who come from theoretical physics into experimental physics than vice-versa. But the other is not unknown.

More Additions to the Faculty under Birge

Lage: We were talking about Kittel and Kip in solid state physics.

Helmholz: Yes, solid state physics. Almost at the same time, Jenkins was able to find Walter Knight, who's now on the faculty, who came in the fall of 1950, who was working in magnetic resonance, which is akin to solid state physics. Then John Reynolds, who worked in mass spectroscopy--Walter Knight had been at Trinity College in Hartford. Jenkins had heard of him and gone to see him. John Reynolds was from the University of Chicago. So they both came in the fall of 1950, the same year, starting up research in their own fields.

Lage: I always think of magnetic resonance in connection with medicine today.

Helmholz: Yes, later on, when Erwin Hahn came, he sort of invented the magnetic resonance that's now found in medicine. He's been an outstanding worker in that field. Erwin Hahn, as you perhaps know, won the Wolf Prize, which is the prize given in Israel. It's almost as good as the Nobel Prize but not quite. It doesn't have as much money with it, but it still has quite a bit.

So we had those additions, and then, of course, we had additions to the faculty in nuclear physics as well as high energy physics.

Lage: So it seemed as though Birge or the full professors on the faculty were very aware of keeping Berkeley up with the changes in physics.

Helmholz: Yes, I think they were. That was one of the things that showed their foresight about physics. They realized that there ought to be some additional fields represented in Berkeley. So they started out and intentionally got additional people.

Lage: Now, these people wouldn't have been connected with the Rad Lab, then. They were centered right here in the department.

Helmholz: Yes. They were centered here in the department. As the Rad Lab grew, it began to support research in some of these other fields, so that--oh, there must be three or four of the present faculty in what I would call solid state physics who are associated with work at the Lawrence Berkeley Laboratory. That's a case of the laboratory growing into other fields of physics and chemistry rather than the new assistant or associate professors taking on nuclear physics as a field of research. In those days, it was still a pretty sharp line between the people who worked at the Radiation Laboratory and the people who did their research down here on the campus.

Lage: Did the members of the physics department who also worked up at the Rad Lab have as much time for department affairs or committees?

Helmholz: Well, they didn't take as much time. I think they tried to take as many graduate students, to direct the research of as many graduate students as the people down here, but they didn't spend as much time in the campus affairs. So there was always some difference in department affairs. They taught the same number of courses, but they would spend their time up at the Rad Lab when they had time of their own that they wanted to spend.

Lage: It seems like you got so involved in a lot of department administration. Was that the usual case?

Helmholz: It wasn't the usual. I think I spent more time than other people who worked at the lab, although after I became chairman we had people who essentially had come as lab researchers, who

were department chairmen for a number of years.[1] After I was chairman, Burton Moyer became the chairman. He was gone for a year. He took a position as a professor in an Institute of Technology in India for a year. Segrè was the chairman during that year. Segrè was spending almost all his time up at the lab. He had to change during that year, and so did Moyer--had to spend a good deal more time down here on the campus.

Lage: But even before you were chairman--I have a list here of all these department committees: committees on budget, requirements for a higher degree, research, building, summer session.

Helmholz: Yes. There were a number of departmental committees that I served on. There wasn't anything, you might say, unusual about that. Those were sort of standard committees that everybody got involved in.

Birge retired, then, in '55. He had planned to retire in '54, but he did retire in '55. I became the chairman in '55.

Lage: Let's leave the chairmanship for next time. Let's see if we have anything pre-chairmanship to take on.

Helmholz: Well, I don't know. I just taught more or less regular courses. As I think I told you last time, during the war I even taught graduate courses. After the war, I went back to undergraduate courses and taught both upper division and lower division courses. I was the advisor to the physics undergraduate major.

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[1]Physics Department chairs:

Burton Moyer	1962-1965
Emilio Segrè	1965-1966
Burton Moyer	1966-1967
George Trilling	1968-1972
Eugene Commins	1972-1974
Geoffrey Chew	1974-1978
J. O. Jackson	1978-1981
Leo Falicov	1981-December 1983
John Reynolds	January 1984-December 1986
P. Buford Price	1987-1991

Sigma Xi at Berkeley

Helmholz: I think at that time I also became--or it may have been at the end of the war. I was the secretary of Sigma Xi, which is a scientific honorary society, which is a national organization mostly interested in promoting research. Perry Byerly was the president of the local chapter of Sigma Xi at one time. He got me to become the secretary, and since I was in Berkeley all the time, I could do that. I think I was secretary for two years, and after I had retired from the secretaryship, I became the president.

Lage: What did that involve?

Helmholz: It really wasn't much of a job. It required that you arrange for a Sigma Xi speaker, who was a nationally chosen figure in science, to give a lecture on the Berkeley campus. You had to pick some time that was accommodating to him and to the campus and arrange to give him dinner or something like that beforehand. Then, the final meeting of the year, I had to give a speech myself. Then there was the election of new members. Each of the science departments recommended Ph.D.'s who were about to get their degree. As a matter of fact, in the early years--and I guess this must have been when Byerly was president, maybe when I was president also--you had to have a Ph.D. in order to get elected a full member. There were associate members. A lot of graduate students were elected associate members, but you didn't quite get all the privileges when you were an associate member. I think mostly you had to pay bigger dues when you were a full member.

Lage: Were you nominated by faculty?

Helmholz: Each of the science departments would recommend members of their own department who were not members for membership--that is, faculty members. Many faculty members came from schools that didn't have Sigma Xi chapters. Then, if they had a postdoctoral fellow whom they wanted to recommend, he would have a Ph.D. and a published paper and so on. Then, after a number of years, it got to be such that when a student was about to get his degree and perhaps had a published paper he could be recommended for full membership. Nowadays, a good many students, particularly in physics, can be elected a full member.

Lage: What percentage would be chosen do you think?

Helmholz: My guess would be that perhaps 50 percent of the people who get Ph.D.'s in physics become members. It may be somewhat less than

that. Some students who get their Ph.D. just don't want to become--they're members of enough things already. If you're going into academic work, it tends to be something that adds to your reputation and also may add to your service to whatever kind of a school you're going into, because Sigma Xi at some of the smaller schools is a fairly important scientific organization. At Berkeley, it really isn't. They have a couple of meetings a year, and speakers, and sometimes there's a visiting speaker. But it isn't a really very active organization here. I've been to other places. Oh, for example, Whitman College in Walla Walla, Washington, has a very active Sigma Xi chapter, and so on.

Lage: Where there's not so much else going on.

Helmholz: Yes, I think that's right.

The Loyalty Oath Controversy, 1949-1952

Lage: I want to ask you about the loyalty oath [see also pp. 95-97]. Was the physics department split, and did it have lasting effects on the department?

Helmholz: Yes, the department was really--well, I don't think it had quite as much lasting effect as it did in some departments, but there were several people who left Berkeley because of the oath dispute.

Lage: From physics.

Helmholz: From physics, yes. Gian Carlo Wick, who is an Italian by birth and training, left. Geoff Chew left. Wick went to the Carnegie Institute of Technology. Chew went to the University of Illinois. One of the fellows named Wilcox, I think, left mainly on that score, and Harold Lewis was fired.

Lage: Were these people who had been hired postwar?

Helmholz: Yes, all of these people had been hired postwar. As a matter of fact, none of the people who left had come from the period before the war or even during the war. Segrè was very much exercised about it, but he stayed on.

Lage: Did the department hold meetings and discuss this on a formal basis?

Helmholz: Not really. Birge discusses it in his history. He was very much against the oath and tried in the Academic Senate to stop it. I don't know how much you've read about the oath dispute, but what happened--if you know this already, stop me--the Academic Senate set up a committee to advise it on what to do or what the wording of a loyalty oath might be. The committee reported on an oath which would be satisfactory, and the Academic Senate turned it down. The state had--

Lage: Was Birge on that committee?

Helmholz: No, I don't think Birge was on that committee, but he was in favor of what they reported. For example, Ernest Lawrence just couldn't understand how anybody could fail to sign any oath whatsoever.

Lage: That's why I thought it might have been a divisive issue in the physics department.

Helmholz: Yes. There was a division in the department. I think that almost everybody--well, Ernest Lawrence was not really active in the department at the time. He didn't take any part in the discussions of it. Alvarez is someone who felt somewhat as Lawrence did, but Birge was against the oath. He signed and all the rest of us, except those four people that I mentioned, signed.

Lage: Did you discuss this with any of the people who left to get their viewpoints?

Helmholz: Oh, yes. I knew how they felt about it, but we didn't have any regular faculty meetings. It would come up at faculty meetings, but Birge didn't like to discuss things like that at faculty meetings. Then, as you know, [Professor Edward C.] Tolman brought suit against the loyalty oath and finally won the case. The Regents had to take back everybody who had left.

Harold Wilcox was not "fired." He had been appointed in 1948, but left in 1950 to accept a position as research physicist at the U.S. Naval Ordnance Test Station at China Lake, California. It was Chew and Wick and, I think, Harold Lewis, who had been fired, or resigned. They were all at other places. Wick responded [to his notice of rehire] and said, "When I went to Carnegie Tech, I went there of my own free volition. I went there because I was going to stay there." And so he said, "I won't take up your offer to have me back." The same way with Chew. Harold Lewis was "fired" by The Regents. After the Tolman vs. Regents decision a contract for the year 1953-54 was sent to him. He failed to answer so another was sent, which he

signed and returned. Within a week he sent a letter resigning from the University.

Obviously, I didn't feel as strongly as the people who left and not nearly as strongly as some of the people who even stayed. The interesting thing about the physics department involvement was that Victor Lenzen, who was a professor of physics and a very conservative fellow, was chairman of the Committee on Privilege and Tenure, which had to hear all the cases of people who wouldn't sign, because they were going to be fired.

Lage: And he was chairman of that committee?

Helmholz: He was chairman of that committee, but somebody convinced him that he ought to retire as the chairman before any of those cases came.

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Helmholz: Jenkins was the most active of the physics department in the oath dispute. Brode and Birge were active also.

Lage: Active in working with the Academic Senate?

Helmholz: Yes. We got through that fairly well, but of course, some hard feelings remained. For instance, Segrè was a very good friend of Wick's because they were both Italian. They had known each other in Italy before each of them came to the United States. It rankled in Segrè's mind for a good many years. I didn't have very close friends who left.

The other case that I should mention was [Wolfgang Pief] Panofsky. I'll try, before the next time, to read about why Panofsky left. Panofsky was one of Alvarez's group at the radiation lab. He went to Stanford just about that time, and I think the oath dispute was influential in making him go. Stanford offered him a very good position, and they were lucky that he took it, because he built up the linear accelerator work there. He's the one who's mainly responsible for that two-mile machine that they have down there now.

Lage: Did it seem that other universities sort of used this as an opportunity to raid the department?

Helmholz: Oh, yes, I think they took their chances. At least, in that case of Panofsky--well, I think the same is true of Wick, too, that Carnegie Tech was glad to be able to get Wick. They knew that Panofsky intended to leave. Panofsky was very active, and

the department really hated to lose him. I'm sure he had a very good offer there, but it's quite possible that he might have stayed without the oath dispute. He signed the oath in the summer of 1950, but told Birge in January 1951 that he was going to Stanford.

Lage: You said you yourself weren't as troubled by the oath. How did you look at it?

Helmholz: That's right. I looked at it as a very unfortunate kind of dispute. I was never in favor of oaths, and I had been anti-McCarthy all through that time when the communist scare arose. I didn't have any real hesitation about signing even the oath that we had to sign. I can't remember whether I gave any speeches at that time about it or not, but I think we all hoped that the Regents would go back to some oath like the state oath if they had to have an oath or that they would just give the whole thing up when they recovered their senses about them.

Lage: When you were working with the Regents, then, a few years later --when you worked on the retirement system governing board [see chapter VII]--was there a residue from those strong feelings about the oath?

Helmholz: No, I don't think so. I think the Regents always had the feeling that the faculty was on the left side of everything. I didn't have any feeling that they were holding that in any sense against us or that they felt much different because of that.

Lage: It was just their fiscal conservatism that came into play on the retirement system?

Helmholz: Yes.

The Oppenheimer Case, 1954

Helmholz: As far as the Oppenheimer case goes, it came up--I've forgotten what year that was.

Lage: How did the physics department react to the Oppenheimer case? I think 1954 was when he lost his security clearance.

Helmholz: There was some discussion around the department--well, more particularly at the lab--about the dangers of communism. That was about the time that Edward Teller appeared on the scene. Oppenheimer had, of course, left a good many years before, but

people kept in touch with him. There were a number of people--Lawrence and Alvarez, sometimes McMillan, would go to Washington to advise on atomic energy affairs and so on.

When Oppenheimer lost his clearance, there was a great hue and cry among people who had known Oppenheimer, and a lot of the younger people who had never even known Oppenheimer felt that it was very unjust that he should be deprived of his clearance. On the other hand, there were some members of the department, and I guess Alvarez was probably the leader of them, who didn't feel that Oppenheimer was a Communist but felt that he was enough of a Communist sympathizer so that depriving him of his clearance was a good thing. That split people down the line, although there were not nearly as many people who were anti-Oppenheimer as there were pro-Oppenheimer. I think even Lawrence himself--I think he refused to testify on that case.

Lage: That's what I had read. He also told Alvarez not to testify, but then Alvarez went ahead and did. Was there any continued resentment towards Alvarez over the years?

Helmholz: In the minds of some people there probably was.

Lage: I mean, did these political things tend to--

Helmholz: No, I don't think it lasted very long. [Wendell M.] Latimer was the one on the campus who was the chief anti-Oppenheimer person, as I remember it anyway. I never talked to Latimer. I didn't know Latimer very well, but I never talked to him about it, either. The sort of bitterness over that, I think, remained longer than the oath dispute, as far as I was concerned.

Lage: It was more personal.

Helmholz: Yes. It didn't affect department affairs very much. In those times Alvarez and McMillan and Lawrence, although they were regular members of the department and taught, didn't come to department meetings of the faculty very often. I think if Birge really wanted them there, he would just call them up and say, "Well, look. Here's a case in which I think we need you there," and they would come. But they were so busy up at the Radiation Laboratory that they just didn't come very often.

Let's see. I can't remember whether there was anybody else who was anti-Oppenheimer. I think almost everybody was pro-Oppenheimer.

Lage: Did the pro- people organize in any way or do any particular--?

Helmholz: No, I think they wrote letters to their congressmen and so on. Some of them who knew people in Washington, knew people in the Atomic Energy Commission, wrote more letters and talked to those people personally. But it was a bothersome time. It was a national affair rather than a Berkeley affair.

We knew several people who were involved. Harold Lewis, whom I mentioned before, was involved in some of those Communist scares. Martin Kamen, who had been a member of the Radiation Laboratory when I came here--he was from the University of Chicago. He was excellent--trained as a chemist but with a lot of background in nuclear physics. He was here through a good part of the war, and I think he had friends who were obviously Communists, members of the party. He would see those people, and finally Lawrence and Don Cooksey had to tell him to stop it or he would be let go from the laboratory. Kamen has written a history of that time. Where he went at the end of the war, I don't know, but he went to Washington University in St. Louis after the war. He got more into medical things as the years went along. He, with a fellow named Sam Rubin here, discovered carbon 14, which has been a great help in all sorts of biological experiments. He had to testify before McCarthy committees at one time or another, and something was written about him in the Chicago Tribune, so he sued the Chicago Tribune and won the suit against the Chicago Tribune. He's a very colorful character. He ended up down at the University of California at San Diego. He's an excellent scientist also. His career is an interesting one. There's a story, at least, that he wrote this book to explain why he never won the Nobel Prize. [laughter]

Lage: I wonder how many books like that could be written.

Helmholz: A lot, I'm sure.

VII CAMPUS AFFAIRS: FACULTY WELFARE AND SOCIAL LIFE

[Interview 3: October 17, 1989]##

All-University Conferences

Lage: Today is October 17, 1989, and we're continuing the interview with Professor Helmholz. Before we get into your chairmanship of the physics department, we want to go back and talk about how you began to develop relations with the broader University community. You mentioned the All-University conferences.

Helmholz: Yes. I think I must have always been interested in Academic Senate affairs and the broader campus affairs. I've perhaps not spent enough time on my research and too much on those other things. But I had always been interested in that sort of thing.

Lage: When you say "that sort of thing," are you thinking of governance--University governance?

Helmholz: Well, both governance and what the Academic Senate did to govern itself, and what influence it had on the campus, and things like that. So Professor Brode in this department, with whom I was very friendly because he'd been the one who had left in the middle of a fall semester, and they had asked me to take his classes at that time--

Lage: What was his field of physics?

Helmholz: He was working in cosmic ray physics, and his best student, whose Ph.D. thesis he directed was Bill Fretter, who later became the dean of the College of Letters and Science and then vice-president of the University. He was also the chairman of the statewide Academic Senate at one time.

Lage: A lot of continuity there.

Helmholz: Oh, yes.

Lage: And it's interesting that you do have a lot of physicists represented in the broader University governance.

Helmholz: Yes, it used to be that you had to be either a botanist or a physicist in order to be chosen dean of the College of Letters and Science, because we began way back with Alva Davis and then Lincoln Constance, both from botany, and then Bill Fretter. And after Bill Fretter, Walter Knight from physics, and then Rod Park from botany.

Lage: Is that, do you think, a result of the older members of the departments bringing along the younger members? Or how would you--

Helmholz: Well, I really think it's just happenstance that--it's true that Alva Davis, for example, knew Lincoln Constance very well, and Bill Fretter knew Walter Knight well and judged that he was the right kind of person. But I don't think it was particularly associated with those who were dean, who tried to get members of their own department into the deanship next. I think they just looked around for what they considered would be the best person to do it.

Lage: But I was wondering if the physics department has sort of a tradition of taking responsibility for a broader sphere of University affairs. Do you think that was a tradition that affected you as a young faculty member?

Helmholz: Oh, yes, I think that's true. I think Birge and Brode and Jenkins, who were the leading lights in the physics department, were very much interested in campus and Academic Senate affairs. And that certainly affected me. There were some members of the physics faculty who were not influenced that way, but you can't expect not to have differences among physicists.

Lage: I've taken you from your story.

Helmholz: As I think I perhaps mentioned before, Brode was in charge of what was called the Davis Conference in those days, which was a conference of several days, perhaps three, which was held up at the Davis campus during the spring vacation. In those days, Davis had their spring vacation at the same time that Berkeley did. And all the campuses, really. So it was a time at which Sproul collected a group of the faculty--I should look up the report which was written in those days and find out about how many faculty came, but let's say fifty or sixty members of the faculty from all campuses.

Lage: Was it a yearly event?

Helmholz: It was a yearly event then. It started in 1944 and was kept going into the 1970s, when Hitch was president. The location was changed to other campuses.

Anyway, Brode was in charge of this and he had picked a topic, "Problems and Opportunities of the Large University." He assigned different aspects of the topic to a number of people, perhaps two for each aspect of the topic. And they wrote a report and gave this report at the meeting. It was held, I think, in the library of the Davis campus, and we all had breakfast, lunch, and dinner together, and the enology department gave us a wine-tasting one of the evenings. So it was a very pleasant occasion, and I think some interesting things came out of it not only for Sproul in learning what the faculty was thinking, but for his greater knowledge of what the campus problems were. Of course, each different campus would get up and give its own gripes about what was the trouble with the statewide administration, and so on.

Lage: Were people pretty forthright about their problems?

Helmholz: Oh, yes, they were very forthright, and at the end of the conference, Sproul had a question time. Everybody who wanted to ask a question would hand in his question. And then Bob Brode and others would pick out the ones that they thought were appropriate, at least. I'm sure there were some that were inappropriate, like "Why don't they celebrate my birthday?" or something like that. But then Sproul would answer them. And there would be discussion of the answers, too. But it was a--

Lage: What was the spirit of the occasion?

Helmholz: Well, I think Sproul started it because he felt that he'd like to have a greater opportunity to talk in depth with faculty members, and that there were problems such as the whole question about graduate work--should there be more graduate work, less graduate work, and so on, and how should the graduate work be run, and should--and I think there was undoubtedly one time at which undergraduate education was the topic. Sproul, I think, not only learned to know more faculty members, but I suspect that he got a good deal out of it. And we got a good deal out of it because we talked with people on other campuses and learned some of their problems.

Lage: I've sensed in interviewing other people a considerable resentment from Davis towards Berkeley. Was that something you picked up?

Helmholz: No, I don't think so. I never really sensed a--well, I'm sure there were some people at Davis who resented the position of Berkeley, but I didn't feel that there was a strong resentment.

Lage: You didn't get a sense from the newer campuses that they felt Berkeley was trying to hold them back?

Helmholz: No, I don't think so. As I say, there must have been, or I'm sure there was some of that, but you have to expect that. I mean, in the early days I'm sure that UCLA objected to Berkeley, and Berkeley objected to UCLA. But it was, I think, to be expected. And I'm sure that Sproul knew there would be those feelings. And I'm sure that Clark Kerr knew there would be such feelings. But I never had really--I never had any experiences in which people on the smaller campuses demonstrated strongly to me that they really objected to Berkeley or to the way that Berkeley was run. Those conferences were, I think, very valuable.

Lage: There was one you mentioned where you were secretary to the conference? Was this 1950?

Helmholz: Yes. Brode asked me to be the secretary to that, and so what I did was to try to correspond with the people who were giving the papers and then write down any particular aspects of the discussion that seemed worth putting into the final report. There was a final report which included all the written papers and then something about the discussion. I think I've mentioned that that was my first meeting with Lincoln Constance, because he and Brode knew each other very well. They were both, I think, on the budget committee about that time, and Brode knew that Lincoln wrote well, and so he advised me to go down and talk to Lincoln about the writing-up of these things. Which I did, and I, of course, learned a good deal of his points of view about the Davis conferences. So that was perhaps my first experience in sort of a statewide business.

The University Welfare Committee and the Faculty Retirement System, 1950s##

Helmholz: Then the first Academic Senate Committee that I really got on, in which I felt that I spent a lot of time and did a lot of work--in that particular case, quite useful work--was the Committee on University Welfare. There was a fellow named Peter Odegard, who was a political scientist. I remember he had come

from Reed College and was the chairman of the Department of Political Science for a number of years. It must have been in the late forties and early fifties.

I was asked, I guess by the Committee on Committees, to serve on that committee [1949-50; chair, 1950-51]. I was an associate professor at the time. One of their problems was the problem of the retirement system. In those days, the retirement system was sort of in shambles, because the Regents had set up a retirement system back in the days following World War I, when the Carnegie Foundation had helped to set up retirement systems all over the country and had actually given special money to provide for the retirement of faculty members. The Regents had then started their own system to which the faculty member would contribute a certain amount and the Regents would contribute a like amount. They would invest that money and then, at the time of retirement, they would use that money to buy a retirement insurance policy so that the faculty member would have money for his retirement.

The required age of retirement was sixty-seven then, and it remained that way for many, many years until the federal law finally required--it must be five years ago now--that nobody be required to retire before age seventy.

Anyway, during the period from 1920 or '21 to 1950, there was a fair amount of inflation. The faculty members were left with amounts of money in their retirement fund, through which they had to buy retirement insurance, that didn't do anything to pay for the cost of living. Quite distinguished faculty were retiring on, say, two hundred dollars a month, which in those days was something but not much compared with their salaries.

There was a move on the Committee on University Welfare to do something about this. The only thing that seemed possible was to somehow or other go into the state retirement system. The state had a system which was, at that time, called SERS or PERS. I forget. Yes, I think it was SERS at that time, which was State Employee Retirement System. They had been pretty forward in working out a system by which you were paid a retirement that was based on three things: one, your years of service; second, your average highest five years' salary, which was almost, in all cases, the last five years; and then a percentage which you multiplied all these things together and got the retirement allowance. That seemed pretty good, because it took care of inflation through those last five years. Then it was changed to three years. That's what it is now.

So it was decided in that Committee on University Welfare that that would be a good system to push for. Berkeley was obviously the biggest campus in those days. This was about '51 or '52. At that time, Peter Odegard retired from the committee, and I was named the chairman. We decided to push for that, and we had a good deal of help from UCLA, which had a number of people that were concerned with the retirement system and wanted to push for getting the University to go into the state retirement system. There was a fellow named Paul Dodd, who was the Dean of the College of Letters and Science down there, who was the UCLA person who really was pushing for this also.

Lage: Was there resistance on the part of the Regents?

Helmholz: Well, I wouldn't say there was resistance, but, yes, the Regents didn't like to move in any particular direction--not very fast, anyway. So it took a couple of years of pointing out these cases of distinguished professors who were retiring on a very small pension before they began to consider it. Then they, of course--I'm sure they set up some sort of a committee to consider the matter. Dodd and I had to go and convince them that transfer to the state system was the thing to do.

Lage: But was this a case where the faculty went right to the Regents without going through the President or anything?

Helmholz: No, we went through the President, but the President was in the same spot. His retirement was going to be much less than his salary at the time. He arranged for us to get our word to the Regents. Finally, and I think it was in the fall of 1953, they decided that they should do something about it. At that time, either I was still on the Committee on University Welfare or I had been delegated a special representative.

Lage: What date did you say that was?

Helmholz: It was the fall of '53.

Lage: So it took two or three years.

Helmholz: It took a couple of years. Dodd and I kept pushing at this, and it seemed to be going ahead very smoothly at that time. The Regents talked about it at a couple of meetings, and they had made arrangements with the state to take on all the members. In other words, they would just close the University system completely and put everybody into the state system. At that time, all the regular non-academic employees were already in the state system. Even the academic employees who had started out, for example, in the agricultural research station or something

like that, were in the state system. But the regular employees who had been appointed assistant professor to begin with, or even instructor to begin with, were in the University system.

This was discussed, I'm sure, at the May meeting. They had made arrangements so that the change would occur on July first. At the June meeting of the Regents, which was held in San Francisco in the old Crocker Building there, somebody had uncovered the fact that Sproul and [James] Corley and, I think, [Robert] Underhill, who was the secretary to the Regents--Corley was the vice president in charge of finance relations--would get more than 100 percent of their salary by this formula, you see. If you had more than thirty-seven and a half years of service, I think, you would probably get a little more than a 100 percent. Sproul was going to get 102 1/2 percent, and Corley 103 1/4, and Underhill maybe 101 1/2, or something like that. The Regents just were flabbergasted by this. They said, "We've been giving him a good salary. Why should he get more than that after he retires?!"

There were, I think, thirteen or fifteen Berkeley faculty members about to retire that year. I had been telling them they were going to move into the state system, and they were going to get a pretty good allowance. Most of them had been here twenty-five or thirty years. They would get 60 or 70 percent, at least. At that last Regents' meeting, this matter was brought up to the whole Board of Regents, and I'm sure most of them had heard about it before. They voted it down. They voted the change down. I can remember that was a discouraging day in my life, because I had to come back to Berkeley and call up all these people, who had been told that they were going to get a good retirement allowance when they retired on July first.

That was '54. Birge was due to retire in that year, and I was going to become the department chairman in that year. The Regents at their meeting said, "Oh, well, we're going to set up a better system than the state system." They were confident that they could do that. That was the only thing that was said at the Regents' meeting. They didn't say anything about what to do about all these people that expected the higher pension. But Sproul got an agreement from the Regents that they would hire everybody who was about to retire for an extra year. So all the people who were sixty-seven went on to age sixty-eight. Birge went on as the chairman for another year, and he retired in '55.

Lage: And did they set up this system?

Helmholz: They set up a system, but it wasn't better, unfortunately. They learned, to their chagrin throughout the years, that it really

wasn't better. Their system was very much like the state system except that it limited you to 80 percent of your retiring salary, and they have only this year changed it to 100 percent. Now there's a federal law that says you can't get over 100 percent of your retiring salary. That satisfied the Regents who had objected originally. Otherwise, it was pretty much the same, and so the people who had planned to retire in '54 retired in '55. They got just about what they expected.

The First Governing Board of the Retirement System, 1954-1958##

Helmholz: The first governing board of the retirement system was made up of two Regents, Underhill, and Paul Dodd, and myself. We had to set in practice all the rules and regulations about it, so that it was quite a problem. Donald McLaughlin, who had been a faculty member and the Dean of the School of Engineering at one time, was one of the Regents. The other Regent was a Regent from down south, who was a judge down there. His name was [Victor R.] Hansen. I'm glad to say that I never had a case come before him. I don't know how the Regents happened to pick him, but he was the chairman of the board. He really hadn't had any experience in financial matters, so while I'm sure he had heard the words compound interest, I often wondered whether he did understand what compound interest was.

Lage: And here he was chairing the board.

Helmholz: Yes. He was chairing the board, and we would have terrible times trying to convince him that this ought to be done or that ought to be done. The Regents had some actuaries who studied the system and told us about what the rates of contribution should be.

Lage: Is this one of the things you were setting up?

Helmholz: Well, we were setting it up, but we had to rely on the actuaries. They had to figure what the life expectancy of the faculty was and how much ought to be contributed each year in order to make it actuarially sound.

Lage: Did they bring in the nonacademic staff at that time to the system?

Helmholz: No. They didn't bring in the staff until a good deal later [October, 1961]. It wasn't a big system, but the University, of course, was expanding at that time. There was Berkeley and UCLA

and Riverside and Davis. Riverside and Davis were fairly small, but they still had some faculty. So we would sit and argue about this point or that point and what should be done about sabbaticals. In the state system, there wasn't anything like sabbaticals and so on, and then we had to agree that a faculty member, since he was paid, say, two-thirds of his salary for a full-year sabbatical, then the question would be, if he paid his contribution for an extra third, would he get credit for the full year, or would it be only two-thirds of a year, and so on. Donald McLaughlin understood these things very well. Unfortunately, Hansen didn't. Donald McLaughlin, poor fellow, had to side with Dodd and me sometimes, and other times he would side with Underhill and Hansen.

Lage: Did Underhill tend to side with Hansen?

Helmholz: Yes. Underhill was very conservative in financial affairs. He knew what compound interest was very, very well. [chuckles] Anyway, that was the way it ran. We would have long and somewhat rancorous meetings, oh, once a month to begin with. We had the question of whether a faculty member, once he reached the number of years which would give him 80 percent, whether he had to continue to pay into the system. Well, they had the argument that his salary was higher then, so sure, he ought to pay in. Then the question was if he paid in for getting 90 percent, should he be getting something back? Well, the Regents didn't want to give him back anything, but I think it was finally agreed that he would get something back. Anyway, there was quite a long series of discussions.

The trouble with the Regents' system was that the faculty tended to stay on. In the state system, the state contributed so much, and the employee contributed so much. Then, if a person left the state employment, he could take his own contributions with him, or he could leave the money in the system and then, when he reached the age sixty-five or what was about sixty, actually, then he could start drawing up a pension. A lot of people who had been in the system only two or three or four years didn't want to leave that money in the system, so they would just take it. They got some interest on it. That meant that the state money that had gone in was left to accumulate for the others. Well, the Regents didn't realize this to begin with, but they finally did. Then they said, "From now on, all the employees on the campuses are going to belong to the University system."

Lage: I see. That's the reason they put the staff--

Helmholz: That's the reason that they put the staff in. I'm sure they didn't say so directly, but that was, I think, the real reason. Then, when Social Security started, there was the question of what should be done about Social Security. The state system went in with Social Security right from the beginning, and the Regents didn't until--I think it was something like 1974 that the Regents gave faculty and staff the right to buy into Social Security. In other words, some of the money that the Regents had in their own system, they had paid over to Social Security to buy in for those employees back to '71 or something like that.

Lage: Did that tend to be a better deal?

Helmholz: It tended to be better--yes, I think, in general. One of the reasons is that of course Social Security has a regular cost-of-living increase. In other words, if the cost of living goes up 3 percent, your Social Security check goes up 3 percent. The Regents have had a lot of trouble with that.

Lage: They don't have an increase?

Helmholz: They do have now, but they didn't have for a good many years. Their system is well-managed financially; they're the only system that I know of, really, in the country--I'm sure there are others--which is just about 100 percent funded. In other words, if the University were to close today, and the Regents had to pay out for the people who are working now and when they got to their retirement age, they would have about enough money.

Lage: They don't depend on current workers' contributions to make their retirement payments?

Helmholz: No. So the state system is, I think, something like 75 percent or 80 percent funded, but the Regents' is something like 99 percent funded.

Actually, I think I must have served on the governing board for three years or so [1954-1958]. Then, the rules of the board made it so that Regents could be replaced and that faculty members were elected, one from the north and one from the south. Dodd and I both went off. I've forgotten exactly whether he went off at the same time I did. Probably not. I think that it was better for one of us to go off at a time. I probably went off first. Anyway, he later became the president of San Francisco State University.

Those were interesting but troublesome times. The Regents finally agreed that whenever the cost of living went up more

than 2 percent, they would give 2 percent increase in the retirement allowances. Then, when we had that period when inflation was 8, 9, 10 percent--

Lage: In the seventies--

Helmholz: In the seventies, yes--they went to a maximum of 3-1/2 percent.

Lage: Very conservative.

Helmholz: Yes, very conservative. But then you can see, they haven't spent very much money, so they are 99 percent funded. The state legislature, when they saw this, said, "Well, let's just cut out their funding for the retirement system." Fortunately, the President has always been able to retain the funding for the retirement system.

Lage: So that's the other side of the coin. It's so well funded that the state doesn't want to continue to fund it.

Helmholz: Yes. That's right. They would rather fund the state retirement system, which is now called PERS. That's the Public Employee Retirement System.

There still are problems, but it's really a pretty good system.

Social Relationships: The Section Clubs##

Lage: Later after we finish with the physics department, we'll come back to Academic Senate and University governance. Now I just want to get some idea of how a young faculty member builds relationships in the University community. We talked earlier, again off the tape, about social relationships, the Faculty Club, the section clubs. Do you want to tell about that now?

Helmholz: Yes. I think you should get my wife to come in and talk about the section clubs.

Lage: I hope to have an interview session with her, too. [See appendix for a brief biography of Elizabeth Helmholz.]

Helmholz: The section clubs were a way to meet people in other departments because you could get into one of the sections which--oh, it could be the drama section, which is the one that we particularly enjoyed. Actually, it was the Jenkinses who got us into the drama section. There were lots of other people. About

the time that we joined the drama section, there must have been three or four couples from physics, but not more than that. But there were people from English and classics and dramatic arts, French and other languages. Perhaps there were two from chemistry. Chemistry faculty were not great joiners in those days, but I know that Melvin Calvin and his wife belonged to the drama section, and--

Lage: Now, what did the drama section--

Helmholz: The drama section gave a play every month. Somebody was assigned the job of picking out a play for a certain month. Then that person would choose a play and would get enough copies so that each character could have a copy to read from. And then there would be a house chosen for giving the play. The section clubs at the University, or the campus, really, had chairs that you could get for these meetings of the section, and they would be delivered at the host house on--well, usually on Monday morning, because Monday evening was the performance. On Sunday evening the person who was in charge of giving the play would assemble with the cast, and the cast would read their parts, which was sometimes kind of funny. If you were supposed to be doing something with both hands and you had to hold your book in one hand and read from it, why, you didn't get much chance to act. You could gesticulate with the other hand, but you couldn't do it with both hands.

Lage: They were all staged readings?

Helmholz: Yes, they are. Then the person who was in charge of this performance would give his ideas as to what should be done at this point or that point, or what to cut out or what not to. You always had to adjust the entrances and exits to match the home in which it was given. Then on Monday night, people would assemble. I think the plays would start at eight o'clock, and you'd run through the play, and afterwards there would be refreshments. And it was a very nice social occasion, and it still goes on.

One of the early founders of this was George Stewart, who was a well-known writer. I think he wrote a couple of plays for the drama section. I've never heard them given, but I've been told anyway that was--he was still somewhat active when we joined, which was the late forties. But there were quite a number. There was a woman named Kitty Emeneau, who was the wife of Murray Emeneau, a professor of classics. She didn't see very well, and so in place of having to get extra-powerful glasses or hold the book very close to her face, she actually learned her

parts, and sometimes they were very long. But she was a star in one way or another.

Lage: Did they tend to be serious drama, or did you do parodies or comedies?

Helmholz: No, they tended to be comedies. I don't think--I can't remember any really serious dramas. They once or twice gave old Greek dramas, but they tended to be the ones that had their aspects of humor in them. So that was one way in which we met people in other departments.

Lage: It sounds as if it also cut across age groups.

Helmholz: Yes, it did. Betty and I still go to the drama section. Well, we don't go every month. I think they must have seven performances during the academic year, sometimes only six, and the last one is always given at the Faculty Club and we have dinner at the Faculty Club.

Lage: Does a broader audience come to that one? Or it was still just the section?

Helmholz: No, it was just the section, but usually at a Monday night there will perhaps be twenty-five or thirty people, and I think there must be fifty or sixty couples in the whole section club. So when the performance is put on at the Faculty Club and dinner is there, why, almost everybody turns up.

Lage: And at the current time, do you have a lot of the younger members of the faculty, or does it tend to be the older?

Helmholz: Yes, they try to get younger people in. I think what happens is that as people get older and come less often, there's an attempt by the leaders, by the president of the drama section, to get in younger people, and he or she will just ask members, "Don't you know somebody younger who would like to do this?" We've introduced two or three people, mostly from physics, I think, into the drama section, the same way the Jenkinses got us into the section.

There are a great many sections doing everything from playing bridge to studying Spanish or Russian or whatever.

Lage: And the section clubs are activities of the faculty wives' organization?

Helmholz: Yes, it's faculty wife organized. When Sproul was the president, Ida Sproul, his wife, took a great interest in the

organization, in organizing it, and she always had the first meeting at the President's house. I'm sure they had tea and coffee and refreshments there. Each year there was somebody chosen to head the section clubs the next year, and so she would especially entertain them before and arrange that they would have a chance to sit down in one place or another and have a sign--I suppose they have a sign up or something like that--saying what section they are and so on.

Lage: So she fostered that kind of thing?

Helmholz: Yes, she fostered that, and since then, the wives of chancellors have done pretty well. I think if you ask somebody like my wife, she will say that Ida Sproul did the best job of it, but I think probably some of the chancellors' wives are not particularly interested in it. While they keep it going, they don't show the interest that Ida Sproul did.

Lage: Well, we'll get to Betty on all that.

The Faculty Club: Revived by Milton Chernin

Helmholz: I mentioned the Faculty Club. I used to, and I still do, go to the Faculty Club for lunch very often. I started out when I was a graduate student because I was working at the Radiation Laboratory, and a number of the postdoctoral people would--some of them lived at the Faculty Club but they would always have lunch there, and they would invite me to come over. So I started going to the Faculty Club for lunch very early, and as soon as I got appointed to the faculty I joined the Faculty Club as a regular member of it. As a matter of fact, I probably did even earlier. I think they had a way in which, as a senior graduate student, you could be an associate member or something like that.

Lage: Now, was that the common thing to do, for the young faculty or senior graduate students to get involved in the Faculty Club?

Helmholz: Yes, it was. In more recent times, since the faculty has grown so much larger, it isn't nearly as common so the Faculty Club has had a difficult time getting new faculty members. Milton Chernin did a good deal to help that process along. Did Milton Chernin ever give an oral history?

Lage: No, I don't believe that he did.

Helmholz: He should have. He really sort of rescued the Faculty Club. The Faculty Club for many years would elect a president and he wouldn't do anything except sign checks or something like that. But Milton Chernin decided after he retired as dean of the School of Social Welfare really to try to make the Faculty Club go and rescue it from the considerable deficits it had run up year after year. And he did that, and he did it very successfully.

Lage: And how did he go about it?

Helmholz: Well, he went about it by getting more people interested in doing something about improving the food.

Lage: That always helps.

Helmholz: Yes, we always claimed that the food was terrible, and we still do claim that--but it really isn't bad at all. They serve dinner there, I guess, five nights a week, and they had a terrible time with the campus food service, which supplied the food. Finally, they were able to get a chef who was interested and did provide reasonable food. And then, as I said, they would put on evenings at the Faculty Club at which dinner would be served and then there would be a program of some sort. Get one of the faculty to speak on the stock market or foreign affairs or something like that, or even the football season. [laughter]

Lage: That can be pretty depressing. [laughs]

Helmholz: Yes, that's right. So Milton Chernin did a remarkable job, and he got other people to be interested.

Lage: When did this revival of the Faculty Club take place?

Helmholz: I think that that would have started about, my guess would be, in the late sixties. Since Milton Chernin passed away, Sandy Elberg has taken on the presidency, and he's doing a pretty good job, too.

Lage: Now, that's funny. I just finished interviewing him, and he didn't even discuss the Faculty Club except just to mention that he was president of it. I'm sorry that we didn't get that, though.

Helmholz: He isn't around as much as Chernin was. Chernin used to be there at least at lunch time four days a week, and sometimes five. He would come by and sit at your table and ask you how the food was, or did you have any suggestions. I've forgotten

when the Faculty Club got its beer and wine license, and when they started serving liquor, but the bar has made quite a difference. I think that enabled them to get into the black.

Lage: [laughs] By making profits, or attracting people?

Helmholz: Both. There are a lot of people who go over to the Faculty Club to the bar at the end of the day, and some of them will stay on and have dinner. So it's a very pleasant place.

Lage: It's such a physically attractive place.

Helmholz: Yes. And they have, of course, increased the numbers of rooms there so they can handle more visitors. And it's a nice place for visitors to stay. If a visitor comes for a week or something like that, it's very convenient for him to stay there.

Lage: Have they remodelled the rooms?

Helmholz: Yes, they've done a pretty good job. I very rarely go into the rooms.

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Helmholz: They've added onto the numbers of rooms, and they've done them over occasionally. I think they probably have a policy of redecorating one room or two rooms a year every year. So it's really very well run now. It's important, of course, to have a person--Richard Wherry is the name of the present manager--and have somebody who is not only good, but interested in that.

Social Geography of the Faculty Club

Lage: What do you think is the importance of having a Faculty Club and having it be an active ongoing organization?

Helmholz: I think it's very important for the campus as a whole. I think every campus that I've heard of, among the newer or smaller campuses, is anxious to have a Faculty Club, and most of them, I think, have been able to get one. It gives you a place to meet and to talk to other people--people from other departments. I guess I have used it more than most people in physics throughout the years. There's actually a rather sizable group of people now who use the Faculty Club.

Lage: From physics?

Helmholz: From physics. But that's not always been true. At the Faculty Club there's a round table at the end, off in a corner, which used to have all retired people sitting there. So it got to be known as sort of reserved for the older members of the Faculty Club. Nowadays, it's just ceased to attract people. I guess a number of the people who used to come there often--

Lage: You mean to that one table?

Helmholz: Yes, to that one table. There's another table near it which was really instituted as an interesting table by a mathematician, a statistician named [Michael M.] Loève. He came to Berkeley in the sixties, I think, and used to sit at this table and attracted quite a number of people. He unfortunately passed away, I would guess about ten years ago, and somebody had the idea to put a picture of him up on the wall in back of it. It's a small picture with an inscription at the bottom, "He inspirited this place," so he in spirit is at this place.

Lage: Did his table have a particular theme?

Helmholz: No, everything--Well, the physicists who are there do talk about physics some, but there's a geographer there, and a couple of people from business administration, and some people from engineering, and then an astronomer.

Lage: Does the conversation tend to be on an intellectual level, or events of the day, or--

Helmholz: Everything. We have several people who have an endless supply of jokes. [laughing] If you want to learn a few new jokes, why, you go over there and listen carefully.

Lage: You should have brought Ronald Reagan there and maybe he could have developed a little relationship with them.

Helmholz: That would have been hard, I think. In fact I'm not even sure that some of the people would have objected if you brought Ronald Reagan there. But there is another table at which a member of the mathematics department named Joe Hodges sits, and that is a little more gossipy about campus affairs. We don't--at this Loève table, we don't talk a lot about campus affairs. But Joe Hodges's table is the place that you go if you want the latest gossip on this or that.

Lage: This is an interesting social map of the Faculty Club.

Helmholz: The chemists have a table at which a good many chemists often sit. And I think those are the four tables that I can think of right off that have fairly regular people that are members of the faculty who eat there.

Lage: You mentioned this retired table.

Helmholz: It sort of dropped out of existence. Oh, Lincoln Constance used to sit there; he comes rather seldom now.

Lage: Now he's way down at the Marchant Building on San Pablo [where many offices were moved during the remodeling of the Life Sciences Building].

Helmholz: Yes, that's right. It's too bad. And Woodbridge Bingham used to sit there; and the Evans who was in mathematics after whom Evans Hall is named would come there very often; and [Yakov] Malkiel, who is sort of a linguist, would come there very often. But I think in the last year it's just sort of dwindled. If there are too many people at this Loève table, some of us will sit over at that other table. But I guess I must be the oldest one who sits at that table.

Lage: At the Loève table?

Helmholz: At the Loève table. Well, there's a retired professor of geography who really all his academic career was either at Texas or Arizona. His name is Dan Stanislawski. I asked him once how I could remember what his name was, and he said, "Well, just remember the Stanislaus County, and then put a ski on the end of it." [laughter] That's the way to remember it.

So in the old days, they used to have a pool table at the Faculty Club. And then Gilbert Lewis, who was a great card player although apparently not a very good card player, liked to play bridge there, and he apparently had a certain amount of money that he was willing to lose every day or per week or something like that. He would play regularly, but that was way back in the forties.

And every once in a while at the Faculty Club they'll have a special birthday party, like when Joel Hildebrand [professor emeritus of chemistry] was one hundred, why, they had a special birthday party for him; they broke out champagne and had cake.

So those are things that Milton Chernin introduced.

Christmas Program

Lage: What about the Christmas program? Isn't there a special Christmas program?

Helmholz: Yes, there is a special program at Christmas time which again sort of--well, I won't say it fell on evil days, but I think it wasn't as popular. But when Milton Chernin became president and really worked on reviving the Faculty Club, he revived it and just got people to come. I mean, as I say, he would sit down at your table and then if it were close to Christmas, why, he'd say, "Are you coming to the Christmas party?" They had done very well at the entertainment; there was a fellow named Jim Hill in the law school who wrote skits for the Christmas party, and one time it would be about the President of the University, and then another time about the Chancellor, and then another time about the dean, or something like that. But they're always very, very humorous. Then there's a group of singers who were called the Monks Chorus. You perhaps have heard of them.

Lage: I have, but I'm not certain who's involved with that.

Helmholz: Well, there's a fellow named Williams who I think was on the faculty for a while but I don't believe is a member now. He's a black man, and has a very good voice and also knows how to lead a chorus, how to work with a chorus. So he still leads the chorus, and I think he just comes back for this particular event. I think he lives in Oakland somewhere now and works in Oakland. But a number of members of the physics department sing. Brode was one of the old members, and the Monks Chorus sang at the memorial service for him. Bill Fretter sang, and John Reynolds sings, and Dave Judd sings. I can't remember whether there are any others now who sing, but from physics there have been a number of--

Lage: Is it a pretty good-sized chorus?

Helmholz: It's about, I would say, between fifteen and twenty, and they sing every year at the Christmas dinner. Well, since Milton Chernin revived the Faculty Club, the Christmas dinners have become so popular that they have to give it three nights a week now. It used to be always on Thursday, I think, and then they did Thursday and Friday. And then finally now it's given on Wednesday, Thursday, and Friday. And it's jammed every night; I mean, everybody likes to bring his friends, and the wives all come.

Lage: So then the performances are repeated?

Helmholz: Yes, the performances are repeated.

Lage: And do you think it will continue at this level even though maybe Sandy Elberg hasn't put quite as much energy into it?

Helmholz: I think so, yes. I think it's--well, whether it'll continue for twenty years like that, I don't know, but at least for the present it's in good shape and everybody looks forward to it. The evenings are crowded and you come and have drinks at six o'clock, I think, and then sit down at seven o'clock or something like that. The Monks Chorus will parade around through the tables singing their songs, and then there usually is some music written into the skits that they put on, so that one or two of the characters that they pick out carefully can sing. Well, now that they have the fellow--you've heard about this fellow from engineering whose name is Goodman?

Lage: I don't think so.

Helmholz: Who runs the Berkeley Opera? They try to get him to come and sing every year. They make a special part for him so that he can sing some song that's part of the skit. Then they will have sometimes a violin player there; there are a number of pretty good violin players in the campus. So that's another thing that they have. Let's see, they have wine-tastings, they have special dinners, the Emeriti Association meets there.

But in recent years they've got so full of special social occasions that it's hard to find a time that you can have a big occasion. I know the Emeriti Association has lunches on Saturday, and they found that there was a period of two months, I guess, in which every Saturday was taken up in the Great Hall. In other words, people have weddings there, or they have special luncheons having to do with conferences of one sort or another. So it's very well used, and that's of course one of the things that keeps it in the black.

Lage: That's gratifying. Well, I think you've given us an interesting picture of the Faculty Club.

VIII CHAIRMANSHIP OF THE DEPARTMENT OF PHYSICS, 1955-1962

Department Chair Raymond Birge; Leonard Loeb

Helmholz: My predecessor as chairman, Raymond T. Birge,[1] was chairman for twenty-two years, and I think nobody ever suggested that he relinquish his position. He was a fixture. He'd been chosen as chairman because he was active on the faculty and had taken part in getting new faculty to come to Berkeley. His predecessor as chairman died in November of 1932, and he took over; he was the logical choice for the chairman.

I think that all the faculty had slightly different opinions of Birge. Everybody agreed that he had done a remarkable job of building up the department, and so on.

Lage: Did he keep up with the times as he got older?

Helmholz: Yes, he did pretty well. He knew something about the fields of research that people were in, but he didn't know any of the details, particularly in theoretical physics. He just didn't follow that at all. He'd been trained in a very different time, so that I doubt that he'd ever taken a course in quantum mechanics, and nobody expected him really to be conversant with that subject.

Lage: But did he appreciate the value of it all?

Helmholz: Yes, oh yes. He knew that that was the way theoretical physicists thought and the method that they used in working out their ideas of the theory.

There was Leonard Loeb, who had been here just about as long as Birge, and he had come from a fairly distinguished

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[1]Raymond Thayer Birge, Physicist, oral history interview, Regional Oral History Office, University of California, Berkeley, 1960.

family. His father was--his first name was Jacques, I think--had been a quite famous physiologist and had been at Berkeley at one time during his life. But Leonard Loeb worked in the field of discharges and gases and was a prominent faculty member for a good many years, particularly in the twenties and thirties. He was also an avid member of the Naval Reserve, and I think he got to be a captain in World War II, which in those days was a very high office. His field went into eclipse after World War II, and while he had a few students after that, he didn't have many.

Lage: Would he have been assigned to teach the undergraduate courses, then?

Helmholz: Yes, he usually taught undergraduate courses. He had to have the lecture hall free for the hour before his lecture because he wrote everything on the board, and he would start off on the left-hand side and say, "Well, here's the first part of the subject of whatever we're going to talk about today," and he'd just run down the board and move over to the next board and run down that and run down that. It was very systematic. I don't think the students really--I think they perhaps appreciated it after they'd got onto the next course.

He was sort of dry in his lectures, but he was a very vigorous person. When I came, he discovered that I had played squash, and so I used to play squash with him down at the Harmon Gym. He was a real competitor. I was somewhat better than he was, but I would have to let him win a game now and then. [laughter]

Lage: To keep good relationships in the department?

Helmholz: Yes, that's right. But I think that helped my favor in the department.

Lage: Was he a powerful figure in the department by the time that you knew him?

Helmholz: Yes, he was. As I say, his field of research had--Well, he'd always practiced this field of research sort of by himself. He had a few pretty good students, one or two of whom were instructors, but none of them ever stayed on the faculty. I have spoken about this Monday meeting that Lawrence had, the Journal Club, as he called it [see page 50]; anyway, Loeb would never come to that. I think people occasionally tried to get him to come and give some talk about what he was doing, and he never would do that. He was a little envious, I think, that Lawrence and Oppenheimer had stolen the show, as it were.

Lage: How did he get along with Birge?

Helmholz: He got along pretty well with Birge, although I think that he always felt, "Well, I could run the department better than Birge if I'd been given the chance." I think he really recognized that Birge had done a tremendous job. On the other hand, there was a feeling that, "Well, it's too bad because I could do a little better." He always had some things that he griped about.

Lage: Twenty-two years was a long time for Birge to be chairman.

Helmholz: Oh, yes.

Lage: Was that unusual in those days?

Helmholz: Yes, it was. There were a few departments around which had sort of permanent chairmen. I can't remember what departments; that's something that Lincoln Constance would know about.

Lage: Well, his department had a pretty long-standing chairman in William Setchell. He served thirty-nine years [1895-1934].

Helmholz: Nobody that I know of really suggested to Birge that he leave.

Lage: People weren't unhappy with him.

Helmholz: No, they weren't. Sproul relied on him for his relations with Lawrence and so on. I mean, eventually, I think when the Radiation Laboratory was set up, which was about 1933 or so, Sproul would deal directly with Lawrence, but before that I think he would go through Birge.

Lage: One of the oral histories I looked at referred to the fact that Birge's presentations, say, to the budget committee, were always so thorough and well done that he always got everything he asked for. So that's the mark of a good chairman.

Helmholz: Oh, yes. And if you read his book which I have been rereading to some extent,[1] you'll see that he quotes from his letters that went to the budget committee; he was very proud of his preciseness and he would never admit that he'd made a mistake anywhere. There's one case in which he said that he must have been misquoted at some time because what he said was secondhand and he was very careful to say that this is secondhand, and it wasn't quite right. And he learned afterwards that it wasn't

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[1]History of the Department of Physics by Raymond T. Birge (1968), in University of California Archives.

quite right, and so he had to apologize because he had made a slight slip-up.

Chosen as Birge's Successor

Helmholz: When it came close to the time for Birge to retire, what the dean of Letters and Science does is to write letters to all the associate and full professors and say, "Raymond T. Birge is due to retire next year. Will you tell me who you think would be a good chairman and why?"

Lage: So he polls all the faculty. All the tenured--

Helmholz: All the tenured faculty. So I think that while there were a number of people who would have been glad to be chairman, they were all somewhat older than I was, and they had had their own gripes about the other people on the faculty. I think that Brode and Jenkins and Loeb would all have been glad to be appointed chairman; on the other hand, each one of them would have said, "Well, the other one would have made a poor chairman." So they sort of balanced each other off. I can remember Loeb telling me one time when this matter came up, he said, "Well, now don't recommend anybody else." He said, "I know you're being recommended." And he said, "Don't recommend anybody else or the dean will have to go around and find out why that person would be good or not good, and so on, and that'll raise all sorts of false expectations, and so on." So that was the way that I was chosen, and I guess there was--I mean, the faculty would talk among themselves about whom they wanted.

Lage: Did people talk openly with you about it?

Helmholz: Not very much.

Lage: Were you asked if this was something that would interest you?

Helmholz: Yes, I was interested, and I had told Birge and Loeb.

Lage: And you'd been vice-chairman?

Helmholz: I was vice-chairman in the year 1954-55. That was the year that I was supposed to become chairman because Birge was going to retire. And then he was kept on for a year.

Lage: So you hadn't had the experience of being vice-chairman before you were chosen.

Helmholz: No, I hadn't had that experience, but I'd been doing things in connection with the department. I hadn't ever really written letters to the budget committee about new appointments and so on, but Birge had tremendous files, and I could just pull those out and look at them and see what had been written about so-and-so when he became assistant professor, and then I could look at some other ones and see what happened when he was promoted to associate professor, and so on.

Lage: You had a good model to follow.

Helmholz: Yes, and it was very easy in that respect.

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Helmholz: I think, but I'm not positive, that the department was quite unanimous in electing me, or indicating their choice that I would be a good chairman. And so because Birge had been kept on for that extra year, I of course had an extra chance to learn how he had done things and to find out what one really had to do.

Lage: Did you have any kind of charge from the dean of L&S or discussions with Kerr, or--?

Helmholz: Not really. I think they knew that Birge would have told me that he wanted to maintain a distinguished department with good teaching.

Lage: This wasn't a department that had to be turned around?

Helmholz: Turned around, no. It wasn't. So it was, as I say, quite easy to become the department chairman.

Lage: How was it to have the retiring chairman close at hand, and kind of a strong person, I would gather. Was that difficult?

Helmholz: No, it wasn't. I suspect that for some chairmen it might have been quite difficult. I'm fairly easy to get along with, and it takes a lot to ruffle me. [laughs] I obviously expected Birge to be around, and I didn't probably ask him as much as I might have about, well, what would he have done in this case or what would he have done in another case.

Faculty Hiring, Then and Now

Helmholz: The most important thing that a department chairman has to do is to get new faculty. And of course that was the time when Birge had been through the very substantial additions to the faculty. There weren't as many when I was chairman as he had been through, but that certainly is the most important thing that a department chairman has to do.

Lage: And is it the chairman more than anyone else who takes that on?

Helmholz: Well, yes, he has to take it on. The way it's being done now is that, if it's decided in the faculty meeting that there ought to be somebody added to the solid state theoretical faculty, why, one or two members who are in that faculty, and one other, perhaps from experimental solid state physics, will be appointed as a subcommittee to look around. And then, nowadays, whenever you add a faculty member, you have to advertise in the journals that there is such a position open, and you get lots and lots of letters. In our days that wasn't the case at all. We had to know people whom we trusted and who could suggest new faculty members.

Lage: People from other schools?

Helmholz: People from other schools, or sometimes we added people who got their Ph.D. here, perhaps that were on postdocs here. A number of people from the Lawrence Berkeley Lab were added, particularly because they had worked with somebody up there who was on the faculty, like Segrè or Alvarez. And so they would be recommended by Segrè or Alvarez or one of the others, and we'd discuss those in the faculty meeting and ask them.

Lage: Now, how about justifying adding a new faculty member? How did you do that?

Helmholz: You do that--I think Birge started--well, I'm sure it was done more or less the same way in other departments, too. If you have a certain teaching load, then you have to have faculty to teach those.

Lage: But did the University ever say, "Well, now that's enough graduate students. Quit adding graduate students." I know they did later on.

Helmholz: Yes, they did later on, and Birge had always insisted that the teaching load be very light for physics relative to other departments--to the humanities or languages or something like

that. And he did this on the basis that physics faculty had to look after graduate students and that they were all active in research, and very active in research. So that we had, and still do have, a lighter teaching load than many departments.

Lage: Now, is that partly because you have some Lawrence Berkeley Lab money? Or you actually have a different standard?

Helmholz: No, it's just that Birge got it recognized by the administration, by Sproul, that we should have a light teaching load.

Lage: Has that been eroded over the years?

Helmholz: Well, it hasn't really been eroded. Fortunately, the physics department chairmen have been strong enough to maintain that. I don't remember ever having had any real problem with that. I mean, it would be mentioned whenever you had to see the dean about a faculty member; he would of course ask, "Well, haven't you got enough faculty members already?" and I would say, "No, we need somebody. We have more graduate students." That would be the first point, and the second point would be, "We have more undergraduates and so there are more courses to be given." And of course we've always had to teach undergraduate courses for the engineers and chemists and pre-medics, Physics 10 for the science requirement that the University's always had. Physics 10 is a popular course--as I've said before, I'm sure, it was called sorority physics--so that people could take that course and satisfy, I think it was--I'm not sure what the requirements are now, but in those days it was half of the science requirement. I think you had to take a year in science of some sort. Science or mathematics. So I don't think we've really ever been, let's say, strongly attacked on the fact that we have a light teaching load.

Lage: So you would start working to add a faculty member based on your number of students. But also, would it be based on needing to cover a new area?

Helmholz: Yes, it could be. In those days Birge would just go to Sproul and say, "Well, we need somebody in that field."

Lage: So Birge had more of a direct line to the President, then?

Helmholz: Yes. I became chairman in '55, and Sproul retired in '58, so I had three years in which Sproul was really directly in charge, but--

Lage: But you had Kerr as chancellor.

Helmholz: We had Kerr as chancellor, so we really had to go to Kerr for this sort of thing.

Lage: Would you go to Kerr, or would you start with L&S and the budget committee?

Helmholz: I'd start with the dean in L&S, and I don't think I very often went to Kerr. As a matter of fact, I can't even remember any time that I went to Kerr. I knew Kerr, partly because I had run into him at the Faculty Club, or I think his children were just about the same age as our children, and maybe they'd been to nursery school together, or something like that. Then, in connection with the retirement system, I had run into him because as chancellor he would go to the Regents' meetings. I remember there was a meeting in Santa Barbara; I think once that I rode on a plane with him down to Santa Barbara, and we talked about a number of things, but mostly about the retirement system at that particular time.

Lage: Did you have a sense that he showed a strong support for the science and physics departments?

Helmholz: Yes, he obviously did, because he had been impressed with physics and chemistry, and knew that physics was one of the prominent departments on the campus, and he obviously wanted to keep it that way.

Lage: How about when Seaborg became chancellor? That was '58, right in the middle of your chairmanship.

Helmholz: '58, yes.

Lage: Did that make any kind of a change in--?

Helmholz: No, I don't remember any real change. There was the change that Kerr as president was very much concerned with expansion of the University and new campuses and so on. I think Sproul, although he realized that the University had to expand, he probably would have been pleased to have the University remain just on the Berkeley campus. Of course, he had been instrumental, I guess, in getting the UCLA campus. Well, it was really before his time, but he was in the administration at the time that the UCLA campus had been added.

Lage: And I think Santa Barbara, too, didn't he?

Helmholz: I think probably that's right. But I don't really remember a distinct change in the campus, in the transition from Kerr to Seaborg.

Lage: Or a change in the feeling toward the sciences, with Seaborg coming from chemistry?

Helmholz: No, Seaborg had done a lot of his research work in the early days of the Radiation Laboratory with physicists, so he knew the physicists very well. I think we knew Seaborg better than we knew almost any other member of the chemistry department, but as far as our relationships with the Chancellor's office goes, there wasn't that much change under Seaborg.

Lage: Sounds like you had pretty smooth going, as you describe it.

Helmholz: Oh, yes. Certainly physics did, and it was really because Birge had built up an outstanding department, and there wasn't too much trouble with the administration.

Lage: Do you want to talk more about how at that time you chose faculty, and maybe give a couple of examples?

Helmholz: Yes, well, we can tape a little bit on that. Really, I think we chose faculty from the people that we knew, and how they had done in research. Of course, they had to be interested in teaching and interested in directing the work of graduate students.

Lage: Did you have a way of evaluating that?

Helmholz: Well, not a specific way. We didn't have points 1, 2, 3, 4, 5, 6, and 7 that we would judge them on. But we had to do that in good part from the acquaintance of members of the tenured faculty. We always, of course, had to have three to five letters from people outside who would be asked to write in a recommendation. And you had to do that for assistant professors that were going to be promoted to associate professor, and associate professors who were going to be promoted to full professor. You had to get outside letters.

Lage: In the case of hiring an assistant professor, was it difficult to assess their research at that stage, or had they been active enough?

Helmholz: Usually they'd been active enough, and usually you got that information in part from the person who had directed their thesis.

Lage: Would you read the thesis?

Helmholz: Sometimes. Usually, by the time they came to be appointed as assistant professor, they would have published something. And so you'd read the published paper and their published work rather than the thesis. The thesis is usually too long [laughs] to read. Theses tend to be a hundred pages or more, and articles in the journals tend to be four or five pages at most.

Lage: It's summarized a great deal.

Helmholz: It's summarized, yes. And almost everything that you're concerned with is in that four or five pages. You have to rely on either the thesis director or somebody you know quite well to judge whether the person is going to continue to be active in research, whether he's interested, whether he has really broad interests beyond just the thesis topic that he obviously was the world's expert in [laughter] at the time he wrote his thesis. But you have to judge that, and you have to judge how he speaks at meetings, and also it's probably more difficult to make a guess at how he would be at teaching. We try--As you know, there is research and teaching and sort of public service on which the faculty are judged. Many of the young people, of course, have never done much teaching, if any.

Lage: Would they have been TAs [teaching assistants]?

Helmholz: Sometimes they've been TAs, but there are outstanding research scientists who had fellowships all their way through graduate school and never were TAs. We have discussions in the department every so often as to whether--because it's hard for the graduate students to take oral examinations, whether we ought to make all of them serve as TAs at some time or other. Well, arguments go both ways, and sometimes you get members of the faculty who feel that you shouldn't force everybody to be a TA because some of them are smart enough to pass their oral exam anyway, and they're never going to be teachers, and so why hold them up for a semester or whatever it happens to be?

Lage: Was a personal visit part of the hiring procedure?

Helmholz: Yes, we certainly didn't hire people who were completely unknown to the faculty. Sometimes there were only a couple of faculty members who knew the person, but it was always, I think, at least two. And of course, then you had all the letters, and you had letters from people that you knew.

Aside from the formal letter, would you also have more informal telephone conversations with the persons who wrote the letters of recommendation?

Helmholz: Sometimes. The telephone wasn't as much used in those days as it is now. I think I very rarely had telephone conversations about candidates.

So it would be the formal assessment.

Helmholz: Yes. It is, I think, probably the most important thing that a department chairman has to do, is to oversee the replacement of faculty and the hiring of new faculty members.

Lage: Were personal qualities important? Either in hiring or in promotion? Did somebody who got along, or whose wife fit in well--?

Helmholz: They were important. It's important, and it was important, and it still is important, that you judge how a faculty member, and his wife also, will fit into the department. You don't want to get an irascible character who is going to be mad at everybody all the time. And there have been a couple of cases that I can remember of really quite outstanding people who have been brought up as candidates and essentially turned down because of their personal characteristics.

Lage: Even though they may have been more brilliant, say, than other people?

Helmholz: Yes, well, it has turned out that they were more brilliant. It wasn't obvious at that time. So that you can't judge the future, unfortunately, but I think that that has turned out to be the case.

Lage: So it is counted?

Helmholz: Yes, it's counted. And you like to have some idea of the--as I mentioned before--of the breadth of interest of the faculty, of the prospective faculty member, to know whether--if their particular field, you might say, goes downhill--whether it will be easy for them to pick up in some other field and start research in that field. There've been a number of members of the department in the past who have essentially changed fields. Brode was one of them. He had started out working in electron scattering, and then he spent a sabbatical leave in London with a quite well-known cosmic ray physicist, and he just changed over to cosmic ray physics when he came back from that. And Ed

McMillan was another. He was a national research fellow in molecular beams and switched to nuclear physics.

Lage: That must not be too common.

Helmholz: No, it isn't, and it seems to be--I was going to say it seems to be less common now. I'm not positive that it is. There are people like Alvarez who have quite broad interests and who can sort of shift overnight.

Lage: And study the dinosaurs.

Helmholz: Yes, and go into some other quite different field. But one of the people who came to Berkeley in high energy physics fairly recently, I guess, must be six years ago or so, in high energy physics, has now changed over to sort of astrophysics. That's not as big a change as you might think from the names, but anyway, he has really made the change.

There is some difference in hiring theorists and experimental physicists, too, although you do have to rely for the young people on what is recommended and what you know yourself, or what members of your faculty know. As I think I've mentioned, in the late forties and early fifties, the faculty made an attempt to get people in different fields, and the faculty members--I was a little junior at the time, but I know that Brode, and Jenkins, and not so much Birge, because I think Birge felt that he really wasn't expert enough in any of these new fields--but Brode and Jenkins and McMillan and Alvarez, and I guess Serber was here then, did interview people when they went back east for meetings. Or occasionally, I guess, somebody would make a special trip back east just to meet people and try to decide whether that would be a good person for the faculty. And then, of course, the matter would be discussed in a regular faculty meeting.

Lage: And then how was the final decision made, during your chairmanship?

Helmholz: Well, generally by a vote of the faculty at a department meeting, and if the faculty meeting doesn't happen to be well attended, the chairman just has to go around and ask the people who weren't there whether they would agree with this.

Lage: This sounds very democratic. Was it similar to what Birge did? Did he run it that way?

Helmholz: Yes, I think Birge did that. He was a little bit more, well, autocratic, I guess is the word, than it is now or than I was.

He tended not to consult everybody, but to consult people that he had come to rely on.

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Helmholz: In the case of the addition of Reynolds and Knight, who were brought from Chicago and the East after interviews with McMillan, Jenkins, and maybe others, I heard about their appointment essentially after the fact, after the decision had been made. Birge had a tendency to consult only full professors and not associate professors as much. That gradually changed over the years so that nowadays you consult everybody--assistant, associate, and full professors--on new junior appointments and then the associate and full professors on appointments to associate professor level and above.

Lage: Now, you mentioned last time that you'd made a number of senior appointments when you were building up fields. But were most of the appointments at the junior level? The assistant level?

Helmholz: Yes. Before next time, I'll look up the new appointments that were made while I was chairman and in the couple of years before I became chairman. Whenever you wanted to start up a new field, you chose one senior person. He might be an associate professor, but chances are he'd be a full professor. Then what happened in several cases--and Charles Kittel was a good example in solid state theoretical physics--well, he didn't have another theoretical solid state physicist but he had an experimental physicist [Arthur Kip] in solid state whom he, I think, essentially insisted on bringing. That has happened a number of times, that when you bring somebody, if you bring a senior person, he'll say, "Well, I really need so-and-so whom I have worked with in the past, and you can make him a middle-level assistant professor, or maybe a beginning-level associate professor," and he'll insist on that. Of course, that needs to be considered, and you have to go usually to the dean to make sure that he'll accept two people rather than just one.

[While I was chairman, Chew was brought back from Illinois, Glaser was appointed professor, Watson was appointed professor, York left to go to Washington, Trilling was appointed associate professor, and others.--ACH]

Role of the Budget Committee in Faculty Appointments

Lage: What about the Budget Committee in these instances? Is it the dean that you clear it with first?

Helmholz: Well, yes, you clear it with the dean. You don't go to the Budget Committee to ask them whether it's all right. You write your letters to the Budget Committee and you don't really talk to the Budget Committee about such matters.

Lage: Does the Budget Committee review these new appointments as carefully as they review the promotions?

Helmholz: Yes, I think they do. I've never been on the Budget Committee, so I don't know.

Lage: Did you have any run-ins with them? Any difficulty in getting appointments accepted?

Helmholz: No. Well, we had a couple of appointments in which the faculty wasn't, you might say, 100 percent. It was 8 to 2 or something like that, which didn't work out, and the Budget Committee turned it down also. You're supposed to indicate the departmental balance, and I think that in that particular case, or one particular case I can remember of a--he's still now a senior lecturer so I won't mention his name, but the Budget Committee I think knew from people they talked to that there wasn't a unanimous approval of the recommendation. And so one of the things that they can do, of course, is when they appoint the ad hoc committee to review the matter, they can put a person on the ad hoc committee whom they suspect will be not completely in favor of it. I don't think they consciously put somebody who's against the appointment, but they will put somebody on whom they know by the grapevine isn't 100 percent in favor of it.

Lage: So this would be a case where you had decided on somebody but it wasn't unanimous. And the Budget Committee would turn you down on it.

Helmholz: Yes, that's right.

Lage: Would you fight that? Take it to the dean?

Helmholz: Let's see. I don't think--I don't remember that I ever did. [pause] In the case of appointments, there's only that one that I can remember. I think there was another one in which it was an assistant professor, a young beginning person who was turned down but he took a position at the Lawrence Berkeley Lab and then after he'd been there for six months or of the order of a year, we renewed the request and it was approved. So we just had more information and better letters. It was somebody who had got his degree back east and I guess probably the people

here didn't know him well enough, and maybe the Budget Committee picked that up in the letters.

Lage: It sounds very carefully looked over.

Helmholz: Oh, the Budget Committee is a very careful organization, and I think that the distinction of the Berkeley faculty is as much due to the Budget Committee's reviews of the new appointments and promotions as anything. I mean, after all, if we're in physics we're supposed to know physicists around the world and to be able to judge who were the good physicists and who were the "only fair" physicists. But the Budget Committee has to do this on the basis of very careful work.

Lage: I would think as department chair, you would feel that your department could pick your staff better than any other group. So was there tension there with the Budget Committee?

Helmholz: In our case, we really have not had a lot of, let's say, trouble with the Budget Committee. But I think that's partly due to the fact that we've been pretty careful with our choices.

Lage: So they may exercise more discretion in a weaker department?

Helmholz: Yes, I think that's right. The Budget Committee is the one who tells the dean that you ought to get after such-and-such department because it's not very good. And the dean, of course, knows some of those cases himself, but he can talk to the Budget Committee and learn about that.

In some ways, I've always wished that I'd been on the Budget Committee, but I think probably I'm not quite critical enough, and perhaps somebody on the Committee on Committees realized that. Because the Budget Committee is the one that the Committee on Committees spends the most time on, picking the members of the Budget Committee. So as I say, I think that's probably the most important committee on the campus.

Lage: Is it powerful?

Helmholz: Yes, it really does have considerable power. But they work just terribly hard. Boy, it's an awful job.

Lage: What about tenure decisions in the department? Was that a difficult time for you as chairman?

Helmholz: Not really difficult. We had several cases in which there was disagreement about whether a person should be promoted to associate professor, but what usually happened--well, I should

tell you that in physics people got tenure at rather earlier ages than they did in many of the humanities or languages or social science departments.

Lage: Now, why is that?

Helmholz: Well, I think it was just that physics became so important and people started their careers and did their excellent work at earlier ages, so that it wasn't unusual to consider a young person for a tenured position. So I wouldn't say that we had a difficulty. I think we had a few cases of promotion to tenured positions which were turned down one year and then a year later we made the same recommendation and it was accepted. The Budget Committee keeps all these records, so they know--and when you write the letter of recommendation the second time, why, you--

Lage: You strengthen it.

Helmholz: You strengthen it and you know what they objected to the first time, so that you know how to make the better case.

Lage: What about your own decisions within the department not to offer somebody tenure? Were there a lot of those, or was that unusual?

Helmholz: No, not very many. When I was the chairman, and again I'll have to check up on this, I can only remember two that we had. And it was sort of unanimous among the full and associate professors. They were actually people who had come from the University of Michigan, and one of them had been doing research work at the Lawrence Berkeley Lab, or the Radiation Laboratory, and he just stayed on there because they thought he was a good research worker, and so he stayed there. The other one--they were both theoretical physicists --the other one went back to Penn State University and has done reasonably well.

Lage: Now, I came across reference to one in your papers in the University Archives that was interesting to me. You said the reason for denying someone tenure was that the faculty felt he had a "Harvard attitude." I wondered if--do you recall that?

Helmholz: Gee, no.

Lage: What might that have meant? [laughs] Let me see if I have his name here so I can [searches for name]--Silsbee, Henry Silsbee.

Helmholz: Silsbee. Yes. Was that when I was chairman?

Lage: You had written a number of letters, and that's why it's in your papers. I think you must have been chairman, or at least you were in the position of explaining it.

Helmholz: Yes. Well, that may well be true. Silsbee came as assistant professor in 1951 and left about 1957. He was an able physicist, went to the University of Washington, and is now at Stony Brook.

Lage: But that doesn't ring a bell, what a Harvard attitude implies?

Helmholz: Yes, I think it sort of meant, well, he seemed to be a little bit above it all, and it's the natural meaning of the word "Harvard attitude." [laughter] But he didn't easily partake in department affairs, and his research was not quite up to the standard we felt it should be. It was more of a personal characteristic than anything else.

Lage: More a personal attitude than his research?

Helmholz: No, it wasn't that his research was in the wrong field or that he didn't do very much of it. Well, he didn't seem to be getting ahead in his research as fast as quite a number of other people felt he should.

Lage: You mentioned that he didn't write it up.

Helmholz: Yes.

Lage: Is there anything else you want to add to wind up the discussion of choosing faculty?

Helmholz: No. I did make one trip back east to look for a new young faculty member, and that happened to be the case of the person who didn't get appointed that year but came to the Lawrence Berkeley Lab and was appointed the year following. He's been a reasonably successful faculty member.

Berkeley's Concentration in High Energy Physics

Lage: One other thing that you mentioned in your letters, you had the feeling that Berkeley was concentrating too much on high energy physics. Was that--

Helmholz: Yes, that was partly the result of having distinguished people like Lawrence and McMillan and Alvarez, who were pushing that

field. And partly the fact that these other fields like solid state physics and magnetic resonance and atomic beams and so on had just not got started early enough so that they were well developed.

I think it was also helped along by the fact that the Bevatron was built and was in heavy use as a high energy accelerator during those years. Since the Bevatron is no longer in the forefront as a high energy accelerator, the number of people in high energy physics, relative to the rest of the department, has decreased. You could almost see, by the time I stopped being chairman, that Berkeley wasn't going to have the big accelerator. Yes, I think they tried to get the next accelerator which was built at Chicago, outside of Chicago, the Fermi Lab accelerator.

Lage: They tried to get that here?

Helmholz: They tried to get it here, but the site they had picked was up near Sacramento--you can't just build it anywhere near here because it's so big. So they had a place outside of Sacramento that I'd been by a number of times.

Lage: But the government didn't choose California?

Helmholz: The decision was made--there were, I don't know, say, five, finalists in that position. Did you read about Texas being chosen as the site for the superconducting supercollider [SSC]?

Lage: Yes.

Helmholz: Well, it was the same sort of thing for the Fermi Lab.

Lage: Now, what time period was that, when the site for the Fermi Lab was being chosen?

Helmholz: Fermi Lab started operation in about 1967. Building started some years earlier.

Lage: Was that kind of a crisis situation for the department?

Helmholz: Not for the department. For the Lawrence Berkeley Lab, it was, because one of the fellows up there, Ed Lofgren, who was always interested in building machines and so on, was very much concerned about that, and I think really he had hoped . --if they had decided on this Sacramento position--that he might well have been the director of the lab. And the other people at the lab like McMillan and Alvarez and so on were hopeful that it would be in Sacramento.

Lage: Because without that new state-of-the-art equipment you don't get the top students, is that correct? And the top faculty in that area?

Helmholz: Yes, that's right. But it turned out that people from all over the country, of course, go to the Fermi Lab, but it's outside of Chicago and some of the people in our group at the Lawrence Berkeley Lab have done their thesis work there. So it's become more common, but if the accelerator had been outside of Sacramento then it would have been a lot closer to Berkeley and we would have had all the visitors and so on; they would have come down here to Berkeley.

Lage: Or to Davis.

Helmholz: Yes. I think--actually, as it turns out, the University of Chicago was the closest place to Fermi Lab, and people who go to Fermi Lab just don't so much go to the University of Chicago anymore, so it's probably true that if it had been in Sacramento we wouldn't have benefited as much as we thought we would.

Follow-up on the Loyalty Oath##

[Interview 4: October 24, 1989]

Lage: You were going to start with some follow-up on our last session, about the loyalty oath.

Helmholz: All right. I find by looking at Birge's history--Volume V, covering the period 1942-1950--that physics lost six people as a result of the loyalty oath. They divide into three categories. Panofsky and Serber resigned as of June 30, 1951. That was the year following the firing of the nonsigners. Wick and Lewis were fired as non-signers, and that was in probably August or September of 1950; and Chew and Wilcox resigned as of June 30, 1950. So they left as soon as the oath dispute really came to a head. So they didn't get fired as non-signers because they had resigned and taken other positions.

Lage: I see. And the two who resigned later--

Helmholz: They resigned and they told Birge they had resigned because of the oath dispute. They signed for that year, but they felt that it was not--Wick and Lewis were the only two who then were

officially fired. And of course they were asked to come back when the Regents lost the court case. But neither of them did.

Lage: They both were established elsewhere.

Helmholz: Wick wrote Birge and thanked him for writing him about being reinstated but he felt that he had left to become a permanent member of the Carnegie Institute of Technology.

Harold Lewis was a different case. After he was reinstated by the Regents, apparently a letter from Underhill, who was the secretary to the Regents at that time, went to him and Birge never heard a thing. Birge felt that he wouldn't take up the job again, and Birge then inquired from Underhill whether he'd heard from him. And Underhill said no, they hadn't heard a word. So Underhill sent him another letter, and Harold Lewis sent him a letter back. I guess he telephoned him and said, "Well, I am responding to your letter." And he sent him a letter which is couched in legalese. Birge makes the comment, which is quite amusing, that it must have been written by a lawyer and just signed by Harold Lewis. [laughter] It expresses concern and then says, "While not accepting any of the conditions that you might put," and so on--all this legalese business. But he finally says, "I do not want to come back."

Lage: Did those six men go on to distinguish themselves?

Helmholz: Yes. Wick was at the Carnegie Institute of Technology for a number of years, and then went to Brookhaven on Long Island, and finally, I think, ended up at Columbia. And Serber went directly to Columbia. Chew went to the University of Illinois and then came back here. All three of them had quite distinguished careers.

Harold Lewis has had a, I would call it a mixed career. He finally, after being--oh, boy, I've forgotten where he was initially--somewhere back east, he came out to Santa Barbara, was even chairman of the department at Santa Barbara for a while.

Lage: So he came back to the University.

Helmholz: He came back to the University of California, yes. And then Wilcox, who was the other one, left here and went to naval work at China Lake in Southern California, and stayed there for quite a number of years, and I'm not sure what's happened to him recently.

Lage: Interesting he went to the Navy, which you'd think would be an even more restricted environment.

Helmholz: Well, I think that he probably had a good offer there, so took it.

Lage: And Panofsky? You said he went to Stanford?

Helmholz: Panofsky had a very distinguished career at Stanford. He's the one who really built the Stanford linear accelerator. And just retired, oh, I guess it must have been five years ago. But he did have a very distinguished career.

Lage: Okay, well that's a good footnote to our last interview.

Department Response to New Computer Technology

Lage: Last time we were talking about broadening the fields and covering the new bases, and I wondered about the beginnings of computer technology, how the department responded to that.

Helmholz: Well, we've always had a fair, you might say, representation in computers through the Lawrence Berkeley Lab, because the Lawrence Berkeley Lab had a strong influence in computing, both in chemistry and physics. Not quite as strong an interest as Livermore, but Livermore made their facilities available to the Lawrence Berkeley Lab.

Lage: When did computers become important in physics?

Helmholz: Oh, I would say in the sixties. We had several people who were interested in them. The theoretical physicists--Serber, before he left, and Howard Shugart, who is the present vice-chairman--got quite interested in computers. I think it wasn't until perhaps the seventies that he got concerned, and he got some computers for the undergraduate laboratory, Physics 111, and used them, and he used them in his own research.

Lage: I had noticed a letter, I guess, and I don't have the date of it, where Kittel advised you that perhaps the department should get a computer physicist.

Helmholz: Yes, I think that idea was really turned down by the faculty, but we have had a number of physicists who use computers regularly.

Lage: So you don't have somebody who's a specialist in computer physics?

Helmholz: That's right.

Lage: Is there such a thing at other places?

Helmholz: Oh, I think there are, yes, but in physics in the United States, I don't think it's really gone in that direction. But a number of the faculty have been interested in the use of computers, and I think there probably have been a few graduate students who have done their theses with a physics faculty member and an electrical engineering or computer science engineer jointly in charge of their work, who have done the thesis essentially on computer problems.

Lage: Now, when we say computer problems, are we saying using a computer to solve a physics problem, or actually bringing physics to bear on the solution of a computer problem?

Helmholz: Bringing physics to solve problems of perhaps building computers or improving computers.

Lage: There seem to be two areas there.

Helmholz: Yes, there really are. There's the sort of mechanical--the problems of the mechanics of computers--

Lage: More into engineering.

Helmholz: --which is more engineering, but has a lot of good physics in it. And the problem of using computers, and of course there are plenty of problems in engineering in which you use computers, but there are a lot of problems in physics also in which you use computers.

Lage: Is it more on the theoretical side that you use computers?

Helmholz: Yes, it's really more in the theoretical, although sometimes the experimentalists have to use computers to get the, you might call it, the background information for their own experimental work, or sometimes the background information for theoretical work. I know that we've had students in high energy physics who have had to run quite complicated programs on the computers up at the Lawrence Berkeley Lab in order to get information about the background in their own experiments where they just don't have time to run backgrounds for weeks and weeks and weeks, but since they know what the background is due to, they can computer-simulate that background.

Lage: Has it made a big difference?

Helmholz: Yes, it really has made a big difference. Then there are people in theoretical physics, of course, who are doing very complicated problems in which they'd have to do the calculations by hand if they didn't have the computers. So they've used the computers.

Lage: Would you say Berkeley has taken advantage of the computer technology as well as other universities?

Helmholz: Oh, yes. I think that probably Kittel's letter would have given a sort of different emphasis, perhaps because at the time it seemed as if there would soon develop a group of computer physicists and he didn't want Berkeley to be left out.

Lage: Yes, he thought it might be a new field. When would his concern have arisen, do you think?

Helmholz: Well, it probably would be the fifties. Late, maybe--well, possibly early fifties but more likely late fifties.

Nobel Laureates and Outstanding Teachers

Edward Teller and Physics 10

Lage: As a follow-up of that discussion we had on faculty and recruiting, I was curious about the effect on the department of having a number of real stars on the faculty, and the Nobel Prize winners in particular.

Helmholz: I never felt that it was in any sense a hindrance. I think that some people whom we might have asked to come here would have felt that they were going to be a small fish in a big pond, but I never had one of them say that to me.

Lage: You mean it might actually have a negative effect on recruiting new faculty members?

Helmholz: Yes, it might have had some negative effect. But I think in most cases that people that we ask, or at least we hope that the people we ask, are distinguished enough in their own right or will become distinguished enough in their own right so they don't worry about the fact that we had a number of Nobel Prize

winners. The Nobel Prize winners that we've had--I think this is almost true of all Nobel Prize winners here--they're really cooperative people who are glad to cooperate with others and to work with others and give their opinions about matters of faculty welfare or department education and things like that.

Lage: So you haven't had a problem of not just Nobel Laureates but other top people not wanting to participate in department affairs? Or not having time.

Helmholz: That's right. I think we've been pretty lucky in that respect. McMillan and Alvarez were always up at the Lawrence Berkeley Lab so much, and carried on the work they were doing through the Lawrence Berkeley Lab. McMillan, of course, was director, from 1958 until he retired so he didn't have any time for departmental affairs. Alvarez, I think, took relatively little time, but I'm sure if somebody asked him a question from the department office, that he was glad to respond. He just didn't have a strong enough interest to come to the somewhat long faculty meetings [laughter] that we sometimes had.

Lage: How about in teaching?

Helmholz: McMillan stopped teaching when he became director. I'm not sure when Alvarez stopped teaching. I guess it was in the 1960s.

Lage: In assigning teaching loads, did the people with the greater reputations teach just as much as the others, or how was that--

Helmholz: Yes, we would give them the same teaching load.

Lage: And they took their turn with the introductory classes?

Helmholz: Yes, and some of them--well, we had a somewhat famous experience with Edward Teller at one time, who came here after Serber and Wick left, and we essentially didn't have anybody in theoretical physics. No senior person, anyway. Teller gave a course in quantum mechanics, which is the standard course for graduate students. He gave that for a number of years, and then--I don't know who it was who got him interested in teaching Physics 10--he requested that he be given a chance to teach Physics 10, which we were glad to do because at least I was sure that he'd give an interesting course, anyway.

Well, the first thing that he announced at the opening lecture was that there would be no final examination. Of course, this set off the campus people who followed the rules because one of the requirements for giving an undergraduate course is that you have a final examination or a final paper or something like that. So it wasn't more than a week or two later

that Teller had to sort of recant, and I think that he and his teaching assistants gave people oral examinations at the end of the course, and some of them wrote short papers on things. But you can [laughing] get into trouble with the campus regulations.

That was the most famous case that we had of somebody who was interested in teaching, and I think maybe he gave that course twice. But we had scheduled it for one of the lecture halls here which hold two hundred, and finally the crowd jammed Wheeler Auditorium. So it was a very popular course. Of course, he had quite a reputation by that time.

Lage: I think it was on television also, if I remember.

Harvey White, TV Teaching and the Lawrence Hall of Science

Helmholz: Yes, I think maybe it was. Harvey White gave the physics courses on television. He gave those in--let's see, one of them he gave in '57-'58 and one of them in '58-'59, I think. He was away for the whole year because he spent the first year in Pittsburgh and made the series of television programs there, something like 160 television programs.

Lage: And then was that used here as a class?

Helmholz: No, it was really a high school program.

Lage: So he was really interested in teaching.

Helmholz: Oh, yes, he was. He felt that television was the greatest development in education that had occurred in many, many years. The first year that he went, he took one of the two people we have who set up lecture demonstrations to Pittsburgh with him. And then the second time when he went to New York and made the program there--that was called Continental Classroom, I think--he took the other one of the two, and that person was gone all year, really, from September to the beginning of June. He made good use of them, because one of the nice features about Harvey White's lectures was that the demonstrations were always perfectly prepared. They almost always worked, and they were well tuned to the points that he was trying to bring out in the lectures.

Lage: And he'd done that here, as well.

Helmholz: Oh, yes. He'd made a reputation for that.

Lage: Did he have an impact on the department in his interest in teaching, do you think?

Helmholz: Yes, I think so. I think a lot of people who didn't like the idea of giving Physics 10 were glad that he was around and he was interested in giving Physics 10. He also used many of the same experiments for his physics course for biology majors and pre-medics. He was particularly fond of that course, and he wrote a book with essentially all the experiments that he ever showed in those courses. Those books sold, I think somebody estimated, three million copies. So he became fairly well off on that account.

Lage: Was he the first director of the Lawrence Hall of Science?

Helmholz: Yes.

Lage: Was the hall basically his conception?

Helmholz: That's a little bit hard to say. I wrote Harvey White's In Memoriam for the Academic Senate, and I found that Glenn Seaborg, who was the chancellor after Clark Kerr and when Ernest Lawrence had died, said that he started meeting with several Regents on a committee to set up a memorial for Ernest Lawrence. They'd got fairly well along in the ideas of what the memorial should be, and he realized when Harvey White came back from this New York experience, that Harvey White would be a good director. And so he called Harvey White in, and that was in '59, and asked him whether he'd be willing to be the director, and I think Harvey jumped at the opportunity.

Harvey always indicated to me that it was pretty much his idea that this should be a teaching laboratory rather than a memorial. I'm sure Glenn Seaborg would want to take some of the credit for that also. But I'm sure a lot of the ideas and the working out of the building design and so on were done by Harvey. He had a small group, I guess there must have been three or four people, working to prepare experiments which would be set up and which could be operated by the visitors to the Lawrence Hall of Science, so that when the building was finished they had these things ready to go. And Harvey did a lot of money-raising for the building. The Regents had set aside a certain amount of money, but I think it was only something like half as much as was needed. Harvey did a lot of the work of raising that other half.

Nobel Laureates Segrè, Chamberlain, Glaser

Lage: When this recent [1989] Nobel Prize was announced, there was a lot of resentment expressed by people who had worked with the team that won, over at UCSF. It brought to mind the 1959 Nobel Prize for Segrè and Chamberlain. There were two others they worked with on their prize-winning research. Was that an issue?

Helmholz: I don't think it really was an issue. Tom Ypsilantis had been a graduate student; in fact, he was a graduate student when the experiment was done. I think he was just finishing up his work. So I think he would probably be the first one to say, "Oh, no, I wasn't really responsible for that experiment." He's gone on to some good work in physics, but not a great deal. He hasn't stayed in the academic field.

The other person was Clyde Wiegand. He was a very good--I think I'd almost call him, with apologies to him--technician. He could make almost any piece of apparatus work, and he did a lot of that for both Chamberlain and Segrè. He retired shortly after Segrè retired. And I think that he probably felt sort of left out by not being a part of the Nobel Prize winning group. Personally, I never heard him complain about it, and he isn't the complaining type of person, either.

Lage: I thought I had seen some reference to some complaints, but maybe I'm mistaken about that.

Helmholz: Well, I think everybody here knew, because the publication had come out over those four names, that Ypsilantis and Wiegand were part of the group. But I think that the Nobel Prize committee would prefer to have two names to four names, and properly so, I think. They try to find out who has been the brains behind, or the idea people, and who's carried the ball in running the experiment, and so on.

Lage: Do they make inquiries? I mean, do people around here know who's under consideration?

Helmholz: Yes. Often people from here will recommend somebody, so they know who has been recommended. I think that nowadays in high energy physics when the papers have fifty or seventy-five names on them, that you often wonder how they could ever pick out any one person. But a number of years ago, an Italian named Carlo Rubbia won the Nobel Prize for an experiment he was obviously the head of, there's no question about that. He's an outspoken person, so he let everybody know that he was the head of this experiment. But I'm sure that on the papers that came out, that

he was probably one of twenty names, or something like that. But I think all people in physics and the people who write in and recommend names know those things. I don't feel that there has been much arguing or much complaint with the way the Nobel Prizes in physics have been awarded.

Well, I shouldn't quite say that because there was a fellow, an Italian whose name was [Oreste] Piccioni, who objected and even sued Chamberlain and Segrè about the award of the Nobel Prize to them. He ended up, I think, at San Diego.

Lage: He must have hindered good feelings between the two schools.

Helmholz: I don't think that he's still active. He probably is retired by now.

Lage: What was his objection?

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Helmholz: Piccioni felt that it was his idea, or at least as much his as Segrè and Chamberlain's. And so that he should have been recognized. He had some legal case, which I think that Segrè and Chamberlain were able to show was just based on misapprehension or misinformation or whatever you want to call it. That suit never got anywhere. I think everybody in the high energy physics community would agree that he shouldn't have got the Nobel Prize. He's a sort of a--well, I won't say--well, I guess I would say--disagreeable person. I'm sure if you stay on the right side of him, why, he's all right.

Lage: Also while you were chairman, Donald Glaser was awarded the Nobel Prize?

Helmholz: Yes, I can remember at the time that he was awarded the prize, we wanted to get him to come here from the University of Michigan.

Lage: He wasn't here at the time?

Helmholz: He wasn't here at the time that he did the original work. Bill Fretter wrote me a letter to support his recommendation for appointment as a professor of physics. Bill Fretter wrote me a letter and said, "Here's what I recently wrote to the Nobel Prize committee in recommendation of Glaser," so that it was apparent that he was being considered for the prize. Bill Fretter had probably been asked by somebody to write a letter to the Nobel Prize committee.

Lage: But did you have Glaser before he won the prize?

Helmholz: Before he won the Nobel Prize, yes. So he was here. I think almost everybody expected that he would win it. When he would win it was another question.

Lage: When you hire somebody like that, do you have to give a lot of extra financial incentives?

Helmholz: We didn't because those were all taken over by the Lawrence Berkeley Lab. He did his work in high energy physics, for which the bubble chamber is very well suited. He did his work up there, but as soon as he won the Nobel Prize, he said, "I'm going to take sabbatical leave and study biophysics," or molecular biology, whatever you want to call it, "and I'm not going to do high energy physics anymore." So he's left high energy physics, and he doesn't give courses in the physics department anymore.

Lage: He's still attached in some way?

Helmholz: He's still attached. I think a couple of years ago he gave some sort of seminar through a semester or a quarter--I think it happened to be in the quarter system--which was nominally for people in physics interested in biological problems. But he hasn't taught regular courses in physics since that--

Lage: Is that pretty unusual, that someone would switch?

Helmholz: Yes, it is unusual. But I think, well, I think all the people that I talked to in physics felt that the awarding of the Nobel Prize to him was thoroughly justified. There are only a few people in physics who have sort of been left out, according to more or less general opinion, and those are two people, Uhlenbeck and Goudsmit, who were in Germany at the time, who never won the Nobel Prize. But I think most everybody in modern physics would agree that what they did in pointing out that the spin of the electron was a half a unit, and all the explanation of spectra and so on, through their hypothesis, was adequately verified--those were two people who probably should have got the prize.

Lage: Was there a political reason why they didn't get it?

Helmholz: I have no idea why. I guess it's possible. Segrè was an expert on that. [laughs]

Lage: On the politics of it?

Helmholz: Well, you see, he knew all those people from his years in becoming a physicist in Italy, and he traveled around northern Europe all the time and knew everybody, so his book on Fermi and then his book called From X Rays to Quarks is a very good history of the development of physics from essentially the discovery of X rays. He knew everybody in those books except, I guess, Einstein. I think he said he had never met Einstein. Although I'm sure he had opportunities to do that.

Lage: What was Segrè like as a person?

Helmholz: He was a very--I would call him an intense person, and also a very broadly and well-educated person. I've never known anybody in physics, I think, who could quote Fermi's latest paper and Dante's Inferno almost in the same breath. [laughter] He was extremely well educated. His mother and father were well-to-do Italians who were, I'm sure, very well educated themselves and had a broad cultural background. So he was a very interesting person to talk to, and I always found him very stimulating to engage in conversations with, whether it happened to be about physics or music or literature--almost anything. He used to go back to Italy at least once a year, and of course the Italians were very proud of him. They created a chair at the University of Rome, I think, which was specially for him. Whenever he came I'm sure he had a fancy office.

Lage: Would he teach an occasional class in Italy, then?

Helmholz: He would give lectures there. One of those books, I think maybe it's From X Rays to Quarks, was written originally as a book in Italian. It came from lectures that he'd given in Italy. But he was also--to us, anyway, to Betty and me--he was a very friendly person because he and his wife used to live quite close to us, and they both liked to swim, and we had a swimming pool, and so we would invite them down to swim whenever they wanted to come. We enjoyed the couple very much.

When his wife passed away, which was in 1970, he was really subject to, I think, a very deep depression. He came back to his house, and then since we lived close to him, why, I think Betty one time said, "Well, why don't you just come down and stay with us for a while?" Which he did. He also had a very good doctor, whose name was Goldberg, and I think he was in the Permanente group. Apparently he could see from talking to Emilio what he was going through and he gave him appropriate drugs. So that he came out of it in, oh, really, in a few months; or at least in another six months he was out of it.

Then he married a very charming, very much younger woman from Paraguay whom he and Elfriede had arranged to come to this country. They had sort of said, "We'll be responsible for her financially when she comes," and so she got in all right and got citizenship. Well, of course she would have had citizenship when she married him anyway. She [Rosa] was really very helpful to him because as he grew older he was able to do fewer things physically, but she took very excellent care of him. She's now got the job of his papers and his estate. I don't know how much of a fortune he had in Italy, but he had some fortune because of his father's business.

Lage: Did he have children?

Helmholz: Yes, he and Elfriede had three children, one of whom is at the University of Texas now. He's an historian and specializes in Italian history. One of them is a zoologist and married a zoologist and lives in Tel Aviv, Israel. And the other one, the youngest, is also a girl and she married a sort of a marine biologist who had been here as a postdoc. He's now at the University of Bristol in England. So Emilio and Rosa would often take a trip to Rome, and then they'd go to Tel Aviv, and then they would go to Bristol, and finally back through Austin, Texas, or something like that.

Lage: Did he have strong political views as a result of his experiences in fascist Italy?

Helmholz: Very strong political views about Hitler Germany, and he had quite strong views about Communist Russia. Although, I mean, obviously he'd lived through the Hitler Germany era so I think he didn't feel quite so strongly about Communist Russia. Edward Teller was one of the ones, and also Luis Alvarez and Ernest Lawrence talked very strongly about the danger of Communist Russia. I don't think Emilio really felt that strongly about it. He thought we'd get along, and he knew a number of the scientists in Russia, and I think respected them and felt that they were doing good work.

Teller's Impact on the University

Lage: Did Teller's feelings have an impact on the University? I know they had an impact on national defense policies.

Helmholz: I don't think it had so much of an impact on the University. It didn't have a very great impact on the physics department. I

think everybody felt that Edward Teller was a good scientist, but we didn't believe some of the things that he said about the use of atomic energy or the use of atomic bombs. I think most everybody admits that it was his push that gave us the hydrogen bomb. But I think that Teller didn't have a great political impact on the physics department. I suspect he perhaps had more impact on Berkeley than he did on the physics department.

Lage: But he was a University professor. Maybe that encouraged a different kind of relationship.

Helmholz: He spent all his time here and at Livermore. While he was here, he tried to promote a graduate school in engineering for Berkeley.

Lage: For Livermore?

Helmholz: To be held at Livermore and to be associated with Berkeley. The physics department was against it, and I think that the engineering department here was--oh, they thought there were some good things and there were some bad things about it. So when it came down to his really proposing it, I guess to the Regents or maybe he did it to the Chancellor, it was turned down.

I think Edward felt sort of--well, I won't say he was against Berkeley after that, but he didn't feel quite so kindly toward Berkeley after that. That was something that he didn't like, anyway. Eventually, as you perhaps know, the Davis campus agreed to it, so he got what he wanted, but it would have been a much greater triumph if he'd had it on the Berkeley campus rather than on the Davis campus.

Lage: Was he a person who saw things as personal triumphs?

Helmholz: Yes, I think in a way. Probably somewhat more than Segrè did. I think that Segrè once said that there wasn't anything that would cure Edward Teller better than to win a Nobel Prize. [laughter]

Lage: So he might have been one who was disappointed he didn't win the Nobel Prize?

Helmholz: I doubt that he thought that anything he'd done was really world-shaking enough to win a Nobel Prize. But if he had, well, I think before Alvarez won the Nobel Prize, Segrè probably said the same thing about Alvarez.

Lage: When you said Teller had more impact on Berkeley, what did you mean?

Helmholz: On the Berkeley campus, I think, more. Of course the Berkeley campus is--politically it's such a feeling that they would naturally be against anything that he said because he was sort of a hawk, and so the Berkeley campus would turn against him. But I think that when he made a public statement in physics, why, we [the physics department] would say, "Well, all right, Edward has made another public statement." But the campus would say, "Now we've got to get after that person!" and so on. [laughs]

Lage: Oh, I see. So reaction was a negative one.

Helmholz: The reaction was a negative one, yes.

Lage: Not that he was influential, necessarily?

Helmholz: No, I don't think he ever was.

Lage: With the Chancellor? But was he another person like Lawrence who had more of a direct line to the Regents, maybe, than most of our professors have?

Helmholz: Oh, yes, I think so. I don't think he ever quite had as much of a direct line to the Regents because the University was that much bigger when Teller came in the early fifties. I should look up Teller's dates, anyway, but he didn't come until the fifties. and of course a lot of Ernest Lawrence's influence with the Regents were from the days of the late thirties and the war and the late forties, which when the University was a good deal smaller. The Regents have just too many other things to worry about to be influenced by individual professors nowadays.

Charles Townes

Lage: We didn't mention Charles Townes, who was another Nobel Prize winner. Was he hired during your chairmanship?

Helmholz: No. We tried to get Townes in about, I think it must have been '48 or '49, when his work was well recognized by people who were working in spectroscopy and molecular physics and so on. He came out here one time; I think it was Jenkins who had talked to him when he'd been back in the East and invited him to come out. I remember his coming and Jenkins was concerned about where he

might live if he came out here. I guess he was interested. I think it was before--well, I know it was before he went to MIT; he was a sort of a vice-president at MIT for quite a number of years. We had a nice house out in north Berkeley, and I remember that Jenkins and his wife brought the Towneses out there. We had a drink or something like that, and showed them around.

But he turned us down at that time. Then I think probably if he'd been offered the presidency of MIT, he would have taken it. I've never asked him that or really heard it from--well, I'm sure I've heard it from other people. So after that, he was offered this position as University professor of physics in 1967, and of course he knew a fair number of the people here. So he was interested and the Regents made him a good enough offer to come here. I think he's been very happy here since that time and has done a lot of quite distinguished work.

Lage: When someone's offered a University professorship, does that offer originate with the physics department?

Helmholz: I'm not sure. I think so, probably, but I don't know. Moyer was chairman, and he probably went to the Chancellor and said, "Well, look here, here's an opportunity to get a really top-flight person. Do you think we can swing it on the Berkeley campus?" The Chancellor may well have said to him, "Well, why don't we try to get a University professorship for him?" And that's what they did.

So the chairman of the physics department undoubtedly would have collected letters, anyway. There's no problem in getting a letter to recommend Charlie Townes. But he would have written a letter to the Regents or maybe just to the Chancellor, and the Chancellor would hand it on to the President, saying, "Here's a real opportunity for us to get a top-flight physicist. We have interest in this kind of work, and there are facilities here for him to do this kind of work, so please appoint him." Yes, it undoubtedly started that way.

Lage: Would he also have to be teaching or appearing at the other campuses?

Helmholz: He's supposed to appear at other campuses, and I think he does visit the other campuses. There isn't anything in the appointment as a University professor which says you've got to spend at least a week on this campus and that campus and a week in each one of the campuses. They just do it as they see fit or as they're invited or so on. Townes does spend--well, he spends, I suppose, at least a month a year back in Washington

and places like that, conferring on the commissions that he's on, and so on. So he's not here all the time, and he--while he does give talks, he has another person, Ray Chiao is his name, who works with him on almost all the projects that he's working on. I don't believe Townes has probably ever given a course, a regular course.

Lage: He didn't offer to teach Physics 10?

Helmholz: No, I don't think he has. He's interested in teaching, and he gives department meeting talks and things like that, and I'm sure that if you ask him to come and talk to your class sometime, he'd say, "If I'm here I'll be glad to do it." But I asked him whether he'd be interested in giving one of these freshman seminars which are supposed to be given by retired people. He said, "Well, I just haven't got the time to spend on something like that. Not that I'm not interested, but some year in the future."

But he does spend most of his time here, and he comes to faculty meetings and has influential opinions on new candidates and so on.

Assigning and Evaluating Faculty

Lage: I want to discuss more about teaching and assigning courses and curriculum.

Helmholz: Yes. As the department chairman, you do have to assign the courses. Usually for new faculty members, you ask them what they might be interested in teaching and usually sort of note that down in their folder. Then the question of teaching elementary courses is really one in which you may get some idea from asking them, or you may just have to say, "Well, look, we're going to have two sections of Physics 4A next year; why don't you take one of them?" And tell them that somebody who's been here for a number of years and has given Physics 4A before will be able to help them along with any problems that they may have.

Lage: Is there any review, then, of how they've designed the course? I'm assuming that you have certain things you want covered.

Helmholz: Yes, we do have questionnaires that are handed out to students at the end of the semester, or quarter, as the case might be. The general opinion is given and some of the other opinions are

given, and so on. They question whether the course is too easy or too hard, or whether the problem sets are no good, and so on.

You usually will tell the person about the results of the survey. If the survey has been very bad, if the results are very uncomplimentary, then you have the somewhat difficult choice of, well, should you tell him and give the course to him again? Or should you move him to another course? It depends a little bit on whether the comments are really about his characteristics or with the way he gave the course--if he gave it too hard, or something like that. With his characteristics, then, it's probably better to change him to another course. If it was a large course, change him to a small course, or something like that. If it's just that the course was a lot too hard or he graded too hard or so on, then you explain to him what the general procedure is.

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Lage: In the Archives papers, I saw that at one time you had asked Brode to review a Physics 110 exam for difficulty and appropriateness of questions. Was that something unusual?

Helmholz: Well, yes, I'd say it's unusual because it doesn't very often occur that the person who's giving the course has his own ideas about what this course should look like and doesn't pay any attention to anything that's gone before. When we first had Eyvind Wichmann--who is one of our theoretical physicists here, and he came in 1957, I think--he would give C's to graduate students, and C is a failing grade for a graduate student. Well, we had to tell him about this very quickly.

He wasn't as bad as a professor in mathematics whose name was Pinney, who came from Oregon State University. We required in those days all physics graduate students to take Mathematics 220AB, which for years had been given by a delightful older man named Tom Buck, on differential equations. His lectures were always fun to listen to and quite instructive. But he retired, finally.

His replacement decided that that wasn't the way to give a course in differential equations. So he would give it in a completely different way, and he would grade on the curve. The grades would range from A to D and there would be plenty of C's. So here were all these physics graduate students who had to take the course and who were getting C's. We objected to that through the chairman of the Department of Mathematics, and things went along. I think he eased up, particularly on physics students. He could easily find out who were physics students

and who were not, and he could downgrade the mathematics students if he wanted, but he'd have to explain that to the mathematics department.

Anyway, Wichmann was a little bit like that but not extreme.

Lage: Was it because he felt these students were failing?

Helmholz: He really was grading them on--he wasn't grading them on, well, everybody's got to get either an A or a B. He was saying, "Well, C is a possible grade, also," and I think he didn't feel that it was a failing grade.

Lage: But it must be a possible grade because you must be able to fail graduate students.

Helmholz: Yes, you can fail students, so Wichmann didn't really suffer the way this mathematics professor suffered, but he gave quite difficult courses. I remember, I think one year--well, for several years, graduate students would say, "Well, I'd like to have some time because I want to audit Professor Wichmann's course. So I want to take a minimum load outside of that." We'd say, "Well, why don't you take Professor Wichmann's course?" "Oh, couldn't do that; I'd probably get a C." It was a very difficult course. But it attracted a lot of students. So it may well be that at some time or other we investigated the way he graded and the questions he asked on the examinations and so on.

Physics 110, which you referred to, is an undergraduate course, but it is often the most difficult of the undergraduate major courses. We've had instructors who might have taken a graduate course and said, "Well, in Berkeley they ought to be able to handle some of these subjects that I studied in graduate school, so let's put them in," and so on. So that happens. Usually the department chairman hears about these things fairly readily, and particularly now when we have a vice-chairman who has to look out a little bit more for those things. I didn't really have a vice-chairman on whom I relied for such things, but I probably should have.

But I heard about some of these cases, anyway. We have a physics department committee on courses. Sometimes it's quite active and sometimes it isn't, but anyway, I would bring it up to them and say, "Well, look, here's a case in which you've had a real complaint. Why don't you see what you can find out about it?" There's no problem about getting a final examination or

even the final examination papers and seeing how they've been graded and whether the questions are possibly too difficult.

Lage: Is that a touchy situation? I can imagine certain professors would be very unhappy to have to be told how to teach the course.

Helmholz: Yes, it is, and usually you try to handle it adroitly enough so that he isn't being called a fool for giving a course like that. But you explain to him that this subject has passed right over the heads of the students, who were good students, and that he just ought to bring it down closer to their level.

Lage: Did you run into professors who really just couldn't teach? Didn't communicate well in big or small classes?

Helmholz: No, we certainly had ran into professors who were poorer teachers, but in our experience, since we've made an effort to get faculty members who can teach and really who are at least somewhat interested in teaching, that to get them to change their bad habits is not so difficult. I'm sure that some of the department chairmen who have been department chairmen since I was would say, "Well, I had one," or "I had two," or so on. I remember this Wichmann case because that happened while I was chairman. There have been others. Oh, I think some faculty members have a tendency to mumble, and if you give them a big lecture course, why, the students in the back just don't hear. So you have to give them a smaller class.

Lage: You don't talk to them about getting some training on delivery?

Helmholz: Well, yes, you probably do, also, but I think you tend to give them a smaller class. The smaller classes are usually the upper-division classes, and students are a little bit more ready to complain about instructors in their upper-division classes than they are in their lower-division classes. The lower-division classes are so big--usually anywhere from fifty or seventy-five up to two hundred--that each one of those students feels, unless he's an unusual student, feels, "Well, nobody will ever hear me anyway, so why complain?"

Lage: You mentioned that you did ask students to comment. Was that during your chairmanship, too?

Helmholz: Yes, we had questionnaires. They weren't required in those days, but a number of the professors would hand out some sort of questionnaire to the class and say, "Well, what comments do you have?"

Graduate Teaching Assistants in the Department

Lage: How about the TAs? Did you have any procedures for training TAs to be better teachers?

Helmholz: We really didn't have any formal procedures. We probably should have, but you put somebody as head TA who had not only had experience but had made a good reputation for himself as a TA. The department chairman tries--or at least in the days when I was the department chairman, and now I guess it's the vice-chairman, probably--tries to get around to the head TAs in each course and say, "Well, how did so-and-so do?" Or "Can you give me a sort of a run-down of all the TAs that you had? Whom would you recommend for head TA, and whom would you say should never be a head TA?" People who were always late in getting their papers back and so on, you never recommend for a head TA.

So we did that in those days, and usually the results are pretty good. They realize who's effective in his work as a TA and will recommend somebody who's responsible for the head TA position, or they may say, "Well, look, so-and-so is very good but he's not responsible, and so leave him as a TA."

Lage: So would that head TA, then, have some responsibility for talking with the others about effective teaching? Or is just assumed that if you know your subject matter, you're--?

Helmholz: Yes, at the beginning of the course, he'd be instructed to tell all his TAs. You see, you very rarely run into the case where you haven't got anybody from the previous year to be a TA in a certain course. And if it's in a certain course like Physics 4A, you get somebody who's been in 4A before and who has at least had a reasonably good reputation.

Lage: How about the professor in the class? Do they take a role with their TAs?

Helmholz: Oh, yes, they're supposed to. The department chairman really only gets into it when there are complaints about it. And occasionally the professor will come in and say, "Look, this head TA is no good, so don't appoint him again. Next semester, get somebody else."

Lage: Do you think the system of using graduate teaching assistants works well?

Helmholz: Yes, I think it works pretty well. I think probably one could do more in training TAs. Give them a short course, for example, before classes begin. Now with so much television and so on, you can show them pictures of a good TA and a bad TA, and so on. So not having been department chairman for a long, long, time, why, I can make that excuse that we didn't have so much television in those days. [laughter]

Lage: Do you feel good about the quality of the undergraduate education?

Helmholz: Pretty good. I think that what the undergraduate education lacks is more contact between faculty and students. Back in the late fifties and early sixties when I would be asked the question, "Should my child go to Berkeley?" I would say, "Well, if he's willing to go out and sort of make his own efforts to get a good education, yes, he should come. But if he's the kind of a fellow who sits in the next to the back row all the time in lectures and never goes to professors' office hours, he'd better go to a small college where he's going to get the attention that will bring him out." But I think that there are a lot of people in Berkeley who come here just because Berkeley has a good reputation, and they don't realize that Berkeley provides a good education if you take advantage of it--if you will go and talk to the professor in his office hours, and so on.

Lage: Do you think the student that will do that gets a good reception from his professors?

Helmholz: Yes, I'm sure he does. I've sat in my office long hours without anybody coming in, and I would have been glad to have more people come in at that time. I'm sure there are exceptions to that, that you'll find a professor sometime who doesn't want to see a student in his office hour at all, but I think they're the exceptions rather than the rule.

More on Teller and Townes##

[Interview 5: October 31, 1989]

Lage: You looked up a few facts that came up last time.

Helmholz: Yes. We had a discussion about Teller and Townes. Teller came to Berkeley in 1953 and was a professor of physics in Berkeley, just a regular professor of physics from '53 to '60. According to his biography in Who's Who, from '60 to '70 he was a

professor at large. I'm not sure how that differs from a University professor, because he claims from '70 to '75 he was University professor.

Lage: It might just have been a new category or a change of title.

Helmholz: It probably was.

Lage: Do you recall anything about his being hired? Or were you not privy to that at the time?

Helmholz: No, I don't think I was privy to it. I haven't found out anything from Birge's history, although I tried to look it up but not very carefully. I will look back again and see. Then we also were discussing Charlie Townes, who was provost at MIT from '61 to '67. He came to Berkeley in 1967 as a University professor. So he's been here from 1967 on. I think I mentioned that there was a new president of MIT picked in, I guess, 1966. And Townes, I'm sure, was a candidate for that. Whether he wanted the job or not, I don't know.

Lage: That would have taken him away from his physics.

Helmholz: At least he wasn't named the president, so there was an opportunity to get him to come out to Berkeley, so that was done. He remained at MIT for the year '66-'67 and came in the fall of '67.

Lage: Has he taken up any administrative roles here?

Helmholz: No, he really hasn't. He's stuck pretty close to physics. Of course, he's on all sorts of national commissions. Oh, he's also a director of General Motors. He gets a new car from General Motors every year, and apparently one year his secretary said to him, "Well, why don't you get a--" oh, some fancy car like a Corvette. It wasn't a Corvette, but--. So he did, and he was the talk of the physics department for a couple of weeks after he got that. He said he really liked the car.

Lage: Ordinarily, what kind would he get?

Helmholz: Oh, I think he gets a different kind each time. You know, Oldsmobile one time, and a Chevrolet one time, and so on. I think that he varies. He's not a great car buff anyway, so he does what he feels will serve him well. Another bit of campus gossip which I guess you perhaps have heard, that some time ago--it must be more than five years ago, but it was instituted by Heyman after he became chancellor--he gave a parking space to each Nobel Prize winner.

Lage: I have seen those spaces. I didn't know that Heyman instituted that.

Helmholz: I'm pretty sure it was Heyman. When [Yuan T.] Lee in chemistry won the Nobel Prize, he said, "Oh, boy, there goes another parking space."

Lage: [laughs] It's probably one of their most prized benefits from the Nobel Prize.

Helmholz: Out in this parking lot near the Physical Sciences Lecture Hall, there are about four Nobel Prize spaces, one of which, of course, has stopped now because Segrè has passed away. But Segrè and Debreu and Calvin and I guess Seaborg or somebody else like that had four parking spaces right there, so you would drive in there and there would be all these vacant spaces early in the morning, and none that you could take. Townes has one outside of Birge Hall on the other side.

Lage: Well, that's something to work towards.

Helmholz: Yes, that's right.

IX THE UNDERGRADUATE CURRICULUM

The Berkeley Physics Course

Lage: We were going to start with curriculum today. I wanted to ask you about the Berkeley Physics Course.

Helmholz: The Berkeley Physics Course was started by a number of people in Berkeley, of whom the major one was Charles Kittel, not too long after the Soviets put up Sputnik. I was the chairman of the department, but I didn't take any active part in the development of the course.

Lage: Was it felt that the Berkeley undergraduate teaching needed to be revised, in kind of a thorough way?

Helmholz: Well, yes. We never have found a book for the beginning Berkeley course, for the course for engineers and chemists and physicists. There's always an argument about what book should be used, and each new professor--well, it's not quite true that each new professor who teaches it picks his own book, because often it's the same as one that's been picked before. But there's always dissatisfaction. I guess I can say that there's no course given in physics in Berkeley for which the professor is satisfied with the book unless he's written it himself. Even then, sometimes he gets dissatisfied with it.

Lage: Do you include yourself in that group?

Helmholz: Yes. As I'll say, Burt Moyer and I revised Volume I of the Berkeley Physics Course.

The Berkeley Physics Course was conceived, as I said, by Kittel, and [Malvin] Ruderman, Knight, [Frederick] Reif, Frank Crawford, Wichmann, and [Edward] Ed Purcell from Harvard, whom Kittel knew quite well and whom Kittel got interested in this. I think perhaps at the beginning there were a few others, but

they were the ones who wrote the texts for it. They decided to write a really modern series of books.

Exactly why they picked five as a number, I'm not sure, because there was no real reason except that the beginning course was given around that time in five parts, five quarters, because the course for engineers and physicists and chemists had originally been a four-semester course and when the change to the quarter system came, it was made five quarters. The engineers were anxious that the course be reduced in length for their students, since they had more engineering courses that they wanted to give. And when the physics department changed to the quarter system, they reduced their requirement for most of their students from five quarters to four.

Lage: Of the introductory--?

Helmholz: Yes, of the introductory course. There were different curricula in engineering, like the civil engineering course. I think maybe they reduced theirs to three quarters. On the other hand, there is an engineering physics major, and that required all five quarters. So that differed from particular curricula to particular curricula in engineering. The chemists, I think, but I'm not positive, they're stuck with the five quarters, the last quarter of which was a course in atomic and nuclear physics.

Actually, the courses were written up in sort of mimeographed form before the books were actually published, and were used for teaching because the persons who gave the courses were able to get copies, essentially mimeographed copies, of them. So that even a year before--I think it was about a year before the books came out in hardback form, they were used.

Lage: And then were changes made, based on the experience of teaching with the books?

Helmholz: Some changes were made in the final edition. But it did become apparent that the books not only had too much material, but the material was a little too advanced for that beginning course. Purcell's book, to my mind, which is Volume II, is really the best of the five volumes (with apologies to the people who wrote the other volumes). But Purcell taught out of that book at Harvard, and Harvard has a little bit higher class and a smaller group of major students than we do, and he found it too advanced for them, even. So that he realized as well as everybody else that it was too advanced a book to be used in that course. One can always leave out things, but those books were written with an assumption at the beginning that the students knew probably a

little more mathematics than they actually did, counting Berkeley students as the kind of student who would be using it.

Lage: And after all, the Berkeley student was a cut above a lot of other students.

Helmholz: A lot of other students, yes. We've found since then--well, let me go back to say that in about 1963, when I came back from sabbatical leave, I think Kittel and the others were getting a little bit tired of working on the book. They had not all been published then. Crawford's book was the last to be published. Wichmann's was the next to last, and Reif's was the third from the last, and Purcell's was the second. Kittel, Knight, and Ruderman was the first book to be published. Kittel was anxious to [laughs] get away from it, I think, so he asked me whether I would take on the management of what was required. They had made an arrangement with an organization in Boston called Educational Development Center. So that whenever some question came up as to what was expected of the author in dealing with the publisher, which was McGraw-Hill, we'd go through that Educational Development Center. So I took that on probably in '63 or '64, I'm not sure which--and dealt with the authors, who had not yet published their books.

Lage: So you had to prod them along.

Revisions to the Book and the Course

Helmholz: And then I sort of had to prod them along, yes. Then Burt Moyer and I, who had each taught out of Volume I and had our research groups together at the Lawrence Berkeley Lab, decided that it would be a good idea to try to revise Volume I. So we did that; we revised Volume I.

Lage: With an eye to what?

Helmholz: With the idea of making it a little bit, let's call it, more straightforward and more adaptable to the Berkeley students. We dealt with the McGraw-Hill editor who was over in Novato at the time. In Novato, McGraw-Hill has a big book depository. And behind that building, they had an editorial office. At the time, Moyer and I had a very good secretary at the Lawrence Berkeley Lab, a woman named Miriam Machlis, whose brother was a member of the botany department. She was a great help in developing the material that we wanted for the book, because she

would type out what she gathered we wanted [laughter] from what we told her. And most often she was right in what she gathered.

Lage: Are you talking about wording?

Helmholz: Almost everything. Where we needed pictures, and what the illustrations--graphs and so on--would look like, and so on. She would go over with us to Novato and talk with the editors over there about the book and what needed to be done for changes and so on. Some of the illustrations we just took directly out of the other book. It was an interesting experience, and I think Moyer and I each used the book once or twice, and other people used the book once or twice also. But it again was not quite at the right level.

Lage: It was still too difficult?

Helmholz: It was still too difficult, yes. And a little bit long to cover in the one quarter. The special feature of that book as an introductory textbook was that the original authors had decided to put special relativity--Einstein's theory of special relativity, which he came out with in 1905--to put that in the first volume, at the end of the first volume. And then Purcell used that in his Volume II to introduce magnetism. It's an idea which is a very neat idea but is a little too much for the beginning student. The engineers, for example, could care less whether they think of magnetic poles of which nobody has ever found one, or whether they consider that everything is explained by electricity plus the Lorenz transformation which occurs in Einstein's special theory of relativity.

Lage: I would think writing it to be used for both engineers and physics students would present a problem. Your civil engineers aren't going to want the same kind of physics, will they?

Helmholz: No, that's right. Well, I guess we had the idea that they ought to have something which we might call cultural. They ought to know something about special relativity.

Anyway, we revised the book and sent it off for publication, and we didn't have a bad time with McGraw-Hill. About the time the book came out, the editorial office in Novato was closed. So we after that had to deal--if we had to deal at all with them--with the New York office of McGraw-Hill. Since then I have remained sort of in charge of the Berkeley Physics Course. There isn't much to do. Occasionally we get letters of good or bad comments, and occasionally the McGraw-Hill salesman will come around and say, "Well, when are you going to revise Volumes III, IV, and V?"

The original method of paying the authors was that the National Science Foundation made a grant to the Berkeley Physics Course. It actually made it through this Educational Development Center, but each one of the authors, I think, got some payment from this grant--something like, say, $10,000. So he didn't get any royalties. The royalties all went back to the National Science Foundation, I think. So then when it came time to think about revisions, the details of the revisions can earn royalties. When Moyer and I made the revision, we worked out with Kittel, Knight, and Ruderman a division of the royalties such that I think Kittel and Knight and Ruderman got half of the royalties because they had contributed so much to the first edition, and Moyer and I split the other half. And then Kittel, Knight, and Ruderman, I think, made an arrangement so that if the sales were great enough so that the sales of the revised edition exceeded the sales of the first edition, that any of their share of the royalties would go the University of California. I think that the sales have never got that far. [laughter]

Lage: So it's not widely used?

Helmholz: It's not widely used. The interesting thing about the sales have been that the sales to foreign countries are greater than they are in the United States. In the United States, the first few years the sales were fairly good. Not as much as the first edition, but even though the second edition was published in 1973 the sales still amount to a few hundred copies every year.

Lage: You'd think so much would have happened between now and 1973.

Helmholz: That's right. But the sales are, I think, predominantly foreign sales. I think there are as many sold in Canada now as there are in the United States.

Lage: Now, what do you attribute that to?

Helmholz: Well, to take England as an example, the students specialize earlier in physics so they're ready for a book like this even before they leave their prep school. The same, I think, is true of other countries like West Germany and France and Japan, and so on. The book has been translated into a good many different languages.

Lage: So they have more math and more introductory physics.

Helmholz: Yes. So they're better able to use it, and they use it at different levels, also. They may take a rather traditional

introductory physics course, which would be more like our course for biologists and pre-medics and so on, and be taking some mathematics at the same time, and then when they start their second course in physics, they're ready for the Berkeley Physics Course. So it fits into their schedules somewhat better than it would into the Berkeley students' schedules.

The one book for which a request for a revision has occurred, is Wichmann's book, Volume IV, atomic and nuclear physics. And Wichmann says unless somebody's willing to offer him a good deal of money to spend the time to rewrite that book he's not willing to do it.

Lage: It's a big commitment.

Helmholz: Yes, it is.

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Helmholz: Wichmann spent the most time on his original volume, and remembering that, he doesn't want to go back and spend a comparable amount of time unless somebody can make it worth his while.

Lage: How are physicists in general as writers?

Helmholz: Some of them are fairly good, and some of them are not so good. Kittel is the one who has had quite extensive experience in writing, because he wrote a book on solid state physics when solid state physics was, you might say, just developing into a common course in the undergraduate curriculum. He wrote what is apparently a very good book. Not being a solid state physicist myself, I've never done more than just look up things. But it's a book that has been used very extensively in the United States and I guess abroad, even in Russia; it's been translated into Russian.

Lage: Is there a lot of editorial help in books like this from the publisher or from a technical writer?

Helmholz: Yes, usually the publishers have technical writers who do help. The writer of the book, the professor, will write what he thinks ought to be in a certain chapter; I mean, he'll make an outline with so many chapters, and then he'll write the chapter and send it back to the editor, who is either technically oriented himself or will have a technically oriented person to advise him on it. And he'll come back with changes that he think ought to be made. So that then the professor has to look at it again. I

think the books that have been popular are really pretty well written, and they're mostly written by professors.

Kittel also wrote a book on, let's call it, statistical physics, which has been used occasionally in an undergraduate course that we have here in thermodynamics and statistical physics and has occasionally been used in the graduate course along this line. I think in the graduate course it would be sort of combined with other books. And that book has been fairly popular, also. Kittel has written one or two other books; I don't remember now exactly what they are, but he's done well as a writer.

Lage: How did he like your revisions to his book? Was that a difficult situation?

Helmholz: No, not at all. I think by the time we got to revising it, they agreed to the direction and we would send them a chapter at a time, and they'd say, "Oh, sure, this looks fine to me," and so on.

Lage: It wasn't a sensitive issue.

Helmholz: Yes, it wasn't at all a sensitive issue. And the book came out with all five names on the book.

Lage: Does Berkeley still use this basic set-up of the five quarters, covering the same materials?

Helmholz: Up until we went back to the semester system, we used the five quarters in more or less the same order. I think the atomic and nuclear physics was given in the fifth quarter rather than in the fourth quarter as the book does, but that didn't make a difference. The books pretty well stand alone. But now we're back to a three-semester course.

The other physics course, the course for biologists and pre-medics and so on, has pretty well stayed the same because it was just a one-year course--two semesters and then three quarters and then back to two semesters.

Lage: Now, does this get into biophysics, or is it just teaching traditional physics?

Helmholz: Well, it's mostly traditional physics, although one tries to give enough of what we call modern physics to introduce the subject. In other words, a little bit about the mathematical theory of atoms is given. That can lead into some of the biophysics aspects. Each instructor tries to pick out for

himself a couple of lectures that he'll have on biophysical applications of the ideas of modern physics.

Lage: But students aren't expected to have the level of mathematics that the course for physicists, chemists, and engineers presupposes?

Helmholz: No, they don't have the level of mathematics.

Lage: If they wanted to go on in biophysics or medical physics, would they need to get their mathematics?

Helmholz: Yes, they would have to, and this provides them with a difficulty because they would probably have to go back and take some of the course for engineers, chemists, and physicists. That has been worked out so that they can take a couple of semesters of the three-semester course without repeating the whole thing, and they have to pick up the mathematics also. There is a little bit of mathematics required for the course for biologists. We started to put a little bit of calculus into that course quite a number of years ago; I think it was probably in the sixties, in the late sixties. So there's been since that time some calculus required, which makes it a little easier to discuss some of the topics, you might say, quantitatively rather than just go over the qualitative aspects.

Lage: Had you noticed the difference in the preparation of students that come to you over the years?

Helmholz: Oh, yes, I think the students are better prepared. Students come now with, you might say, advanced placement in physics or mathematics. If they come with advanced placement in mathematics, they can start in on this engineering physics course right in their first semester.

With the three-semester course, we recommend to any student who came from high school without any calculus, that he take calculus the first semester, and then we use calculus right from the start in the physics course that starts in the second semester. So that is the normal procedure for the three-semester course.

Lynn Stevenson started a four-semester course, Physics 1A, 1B, 1C, 1D, but it ran only through one sequence and has been dropped. It was three units per semester for four semesters rather than four units per semester for three semesters.

Lage: Physics apparently has large numbers of students and works with large lecture halls. For instance, you mentioned that the

Physical Sciences Lecture Hall holds five hundred students. Do you see a difference in quality between a lecture for five hundred and for two hundred?

Helmholz: No, I don't believe there has been any. There are some members of the faculty who just don't like to lecture in the Physical Science Lecture Hall. It's supposed to be designed so as to make it easy to show demonstrations to 525 just as well as to two hundred. Two hundred is about the size of the lecture halls in this building, in Le Conte Hall. And there are methods of showing small demonstrations by putting them on the TV screens over there. The member of our department who really pushed that building and did a lot to design it was Harvey White, who specialized in that course. And he also specialized in Physics 10, which was the "sorority physics" course.

Lage: Do those introductory courses weed a lot of students out who thought they wanted to be physics majors?

Helmholz: I think there was a period in the fifties and, well, in the late fifties and early sixties when it did weed out some physics majors. There was that period after Sputnik when I remember all the parents of high school students would say, "Well, don't you think our son should major in physics?" [laughter] I would just as often say no as yes. But they felt that sciences like physics were the coming thing.

Lage: Was your reasoning that you didn't think there would be that many jobs to pursue, or that not everybody can do it?

Helmholz: I think both. Not everybody could do it. There were, of course, plenty of jobs available in those times, but the demand for physics majors has oscillated; it's gone up and it's gone down.

Lage: And how about the current job market?

Helmholz: The current scene, I think, is pretty good. It's not oscillating as much now as it did back in the fifties and sixties. In the seventies it tended to go down more, but now it's closer to steady.

Upper Division Courses

Helmholz: But the other thing I might talk a little bit about is the upper division courses, because that aspect of the physics major has

shifted not a tremendous amount but to some extent. For example, when I started teaching upper division physics, there was required of all physics majors a two-semester course in mechanics and a two-semester course in electricity and magnetism, a one-semester course in optics, a one-semester course in thermodynamics, and a one-semester course in atomic physics. Well, now there is still the two-semester course in electricity and magnetism, but there's only one semester in mechanics. That's sort of Newtonian mechanics with a little introduction to special relativity.

Then the course which started out as a course in atomic physics, which didn't require quantum mechanics at all--in other words, there was not quantum mechanics required for the major--now has turned into a two-semester course of which only the first semester is required of all physics majors. But most of them, I think, do take the two semesters. And it starts right in with quantum mechanics almost from the beginning, so that the atomic physics that used to be present in that other course is given in conjunction with quantum mechanics so that they can understand it from the more modern point of view.

Lage: Now, were these changes just gradual evolutions?

Helmholz: Yes, they were gradual evolutions. I think there were a few of the older faculty--. I forget whether Professor Loeb was still active when we made this change from, it used to be, Physics 121. Then there was a course in quantum mechanics, 115. Those courses were kept on for a while so that students from other departments could take them. A student who didn't have the mathematics necessary for our major course, which is called 137A-137B, could take the 121 and get along all right; the mathematics wouldn't be over his head. And then--

Lage: Would that be engineering or pre-med students?

Helmholz: It turned out to be, in the later years, a fair number of people from biology who didn't have a lot of time to go into the biophysics but who did want to get some atomic physics for their applications to their own problems in biology; they would take Physics 121. I think 121 has now ceased.

There was a course in nuclear physics, also, which we gave, and that was changed or it was kept on as a course for--not for major students, but for the non-majors. And there were a fair number of engineering students who took it, who thought about nuclear engineering, and it helped them a lot.

Lage: Would you work with the School of Engineering and the biology departments in designing these courses? Or how did that come about?

Helmholz: Well, yes, we would let them know that we were thinking of changing the requirements, the sort of prerequisites for the course so as to, for example, have less mathematics. We would ask them whether that was appropriate for them. They would answer; sometimes they'd just say, "Well, whatever you think is good is all right with us," and other times they would come in with some helpful comments. I don't think we ever went directly to them and said, "Is there any physics course in upper-division that you think you need?" Occasionally they would tell us of a course that they thought would be a good idea to give, and we would try to respond and perhaps modify one of our courses to make it better fit their needs. The course in nuclear physics was an example; I taught that a number of times, I remember.

If it isn't a required part of the major, the number of students tends to go down very substantially. Students in other colleges--the College of Chemistry or the College of Engineering--will tend to take a major course. They'll take the first part of a two-semester major course, and they find that better for them. It may be because their students are good enough so that if they're going to take any physics course in upper division, they will be able to take the major course just as well as our majors.

So that course, 124, was originally the only course in nuclear physics in the upper division. Then we started 129A-129B, which still exists, and 124 is just--well, I won't say fallen by the wayside--it's still given, I think, every other semester. But it doesn't have a very big enrollment. They have an enrollment of fifteen or twenty, and so on.

Lage: So it may be on its way out.

Helmholz: Yes, I'm sure it is. We have done away with the old course in optics which was developed into a course, really, in wave motion and optics, and sort of modernized, and it's still given, although originally there was a course, Physics 108A-108B, and the A was a part for the optometrists in geometric optics, and the B was the course for physics majors and other students who wanted to know more about physical optics. We now have just Physics 108. I think the optometrists give a course in geometric optics themselves, and there's a slight amount of geometric optics that is given in Physics 108, but very little.

Physics 112, which is the course in thermodynamics and statistical mechanics, is still given. There was a time when that wasn't even required in the major.

Lage: But is it again?

Helmholz: But it is now, yes. That can be done in part because the course in mechanics, 105A-105B, used to be required and now it's just 105. There's only a one-semester course. There still is one one-semester course outside the courses mentioned above plus five units of Physics 111, the modern physics laboratory. That additional course is one which students can choose as they want. There is, for example, a course in solid state physics which we never used to have, even, for undergraduates. And that can be taken, and students can take either a course in nuclear physics or a course in solid state physics. The course in nuclear physics is a two-semester course. The course in solid state physics I think may be a two-semester course also. There's a course in plasma physics which we never had before.

So the number of upper division courses given in physics has increased, and I'll have to try to find out how much it's increased. We, of course, have a bigger faculty, and we can staff those greater number of courses.

Opportunities for the B.A. Graduate in Physics

Lage: What do you expect that graduates at the B.A. level from Berkeley will do?

Helmholz: We expect that a good many of them, I would say probably more than half, will go on to graduate work. We don't take very many of those graduates into our own graduate program, but we recommend other graduate programs.

Lage: Is there a reason that you don't take many of them into your own?

Helmholz: Well, yes, in a way we feel if they've been at Berkeley for four years, well, they ought to see some other part of the world and part of the United States, and I think we all agree that exposure to two different physics faculties is better than exposure to just one. So we have graduates that go to Harvard and MIT and Caltech and so on.

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Helmholz: Then another group of them will go out and get jobs in physics, particularly now that Silicon Valley is taking some physics majors. That's an opportunity. Oh, General Electric, Westinghouse, Hughes Aircraft, and so on, will take students with a major in physics. There are some students who I think really have not quite decided that they want to go on to a Ph.D., who will go out into a job like I just mentioned, and then decide and come back to graduate school if they feel that they really do want to go on and do a Ph.D. We have a few students that will go into teaching, high school teaching.

Lage: Are there many of those? It seems like the pay differential is so great.

Helmholz: Yes. There are not very many now. During the, I think it must have been the sixties, when there was quite a trend toward more and better high school teachers, there were a fair number. They tended not to take a major in physics, but to take a fair number of physics courses. There is a major which was started, must have been way back in the fifties, which is called the physical science major. That requires a certain number of physics courses, a number of chemistry courses, some biology, some mathematics, and there are about, my guess would be about somewhere between five and ten students in that major each year. So that altogether there would be, let's see, twenty or twenty-five altogether. They're the type that would more naturally go into high school teaching. They can stay in Berkeley if they want to and do their fifth year here in Berkeley in education. We urge them to take some more physics while they're in that fifth year, but then they can get a teaching credential.

Lage: It doesn't appear to be the top of your class going into high school teaching?

Helmholz: No.

Lage: Do you think that's too bad for the high school student, or are they sufficiently prepared?

Helmholz: It's too bad for the high school student because if you had Ph.D.'s teaching high school physics, why, the high school students would get a better physics course. Even in Germany, I think it was particularly between World War I and World War II, there were Ph.D.'s who taught essentially high school physics. Their system was somewhat different from ours; in other words, the high school physics course went into a good deal more depth than our high school physics courses do. But anyway, there were Ph.D.'s who taught, and of course, those students came out with a better knowledge of physics. It's important that we get

increased knowledge of physics to the American public, and as far as I can see, it has to be done through the improvement of high school teaching.

Lage: I know you've done a lot of work with training high school teachers.

Helmholz: And the Lawrence Hall of Science and the Lawrence Berkeley Lab are particularly concerned with that. I think the physics department here is not really so--. I think they're concerned, but I think they're not really doing a great deal in that direction, which is too bad, but they've got plenty of other things to do. It isn't as if they were unable to find something to do and just twiddling their thumbs.

Summer Institutes for High School Teachers

Lage: Didn't you for several summers teach summer institutes for high school teachers?

Helmholz: Yes.

Lage: How did you find that experience? What observations might you have?

Helmholz: It was very interesting. I had NSF [National Science Foundation] grants to teach high school teachers for a number of years. Oh, it must have started about, oh, something like '57 or so on, and gone on when I was on sabbatical leave in '62, '63; I think I got Professor Reynolds to do it for me. And I kept going even into the seventies, I think.

I found it very interesting. The course actually changed a good deal. At the beginning it was a six-weeks' course with two weeks in physics, two weeks in chemistry, and two weeks in biology. And that lasted for several years. We would get people from biology to teach the biology, and I guess the students would have to go down to LSB [Life Sciences Building] for their laboratory work, and get somebody from chemistry in the same way. We tried to give them a course which would cover physics in general but would tend to emphasize the modern physics aspects--atomic and nuclear physics. For example, in the lectures, if there were ten lectures in the two weeks, we'd spend just one lecture going over mechanics and one lecture going over electricity and magnetism, and one lecture

thermodynamics and another one kinetic theory and another one something sort of classical, and then spend the whole second week on atomic and nuclear physics.

Lage: Were these the areas where they were least prepared?

Helmholz: Yes. A lot of them were high school teachers who had never had any atomic and nuclear physics in their college work.

Lage: That must have been hard to do in one week.

Helmholz: Well, yes. You had to pick out certain topics and try to give them enough books to read so that they could extend their knowledge. It was quite interesting and quite rewarding. Students would come back in later years if they happened to be able to get to Berkeley. We had them from all over the country, more of them from California to begin with. Then as the years went along, the courses that covered all three fields were dropped and the number of students decreased because we went into a course just for physics teachers. Sometimes we would have a few astronomy and sometimes a few chemistry teachers in with the physics teachers, but in those cases, then, we would again, if we had six weeks or even four weeks, we'd try to spend a little bit more time on atomic and nuclear physics and less on the classical physics. This went on for a good many years, I would say.

Some of those were given up at the Lawrence Hall of Science. I had our main contact when we first started the program with a fellow named Bob Rice, who was a physics teacher at the Berkeley High School. He kept up with that kind of a program for the summer throughout all his time at the Berkeley High School, and has since been doing something similar up at the Lawrence Hall of Science.

Lage: He helped teach the course, you mean?

Helmholz: Well, yes, he would take laboratory sections himself and would try to tell me what he felt would be appropriate the next time we gave the course, and what didn't work out quite as well. Being a high school teacher himself, he was able to communicate with them better than I was with regard to the difficulties they found and the things that they were anxious to avoid and anxious to learn.

Lage: Did you get any other kinds of feedback? To give a sense of what they were faced with from their own students?

Helmholz: Well, yes, I tried to spend, oh, something like an hour each day outside of the lecture talking to them about their individual problems and so on. I didn't have much to suggest in the way of how to make their classes better managed, but I could answer their questions about, well, how do you explain this and how do you explain that.

Eventually, the NSF stopped funding many of those programs. The last program I had to do with was in 1978, and it was called Energy and Environment. It was run through the Lawrence Hall of Science. Originally the NSF programs were run on the campus. That was before the Lawrence Hall of Science was built.

Lage: Lawrence Hall has quite a strong program, it seems to me, of teaching teachers.

Helmholz: Yes, they do, and that's one of the areas in which they have contributed a lot. The NSF programs started out with about two hundred at the very beginning when we had these three subject areas. And then as the years went along, that number got cut down when we went into just physics. So we would have, oh, perhaps, sixty or so in physics when it was reduced to cover just the one subject.

Lage: Were they primarily lectures? Or did--

Helmholz: Oh, no, we would have a lecture each day, but we tried to have lectures covering a good many different subjects in modern physics. I think the ones for physics, for just physics students, lasted for four weeks. We would have, oh, perhaps two weeks on kind of straight classical and straight atomic physics, and then in the other two weeks we would have special lectures by people in high energy physics or solid state physics or plasma physics and things like that in which I was able to get members of the department, or even occasionally you'd get a graduate student who was just finishing his thesis to give a general lecture on his own particular field.

Those worked out pretty well. I think at least it gave the teachers enough idea of what was involved so that they could relate to students; they could tell the student, "Well, if you want to go and study plasma physics," for example, "Berkeley would be a good place." I used to get occasional letters from those teachers saying, "I've got a student who wants to study computers. What would you recommend him doing in his college career?" And I would try to answer him with three or four suggestions. When you advise high school students, you have to know something about their intellectual ability. You can't suggest to each one that he come to Berkeley, because some of

them will never get into Berkeley. But there are other places that don't have quite as high standards as Berkeley, but still have good programs.

So that was very interesting, and I think perhaps something like that ought to be continued, ought to be started up again. I have not paid a great deal of attention to what the Lawrence Berkeley Lab and the Lawrence Hall of Science are doing now in the way of their teacher programs. I guess I could find out because this fellow Bob Rice is still up at the Lawrence Hall of Science. The other person in the program that was very helpful to me was Henry Nelson, who was also a teacher at the Berkeley High School, who must have just retired within the last couple of years. He was a very good teacher, and he helped me with the NSF program for quite a number of years. He still takes some part with the Lawrence Hall of Science. Another is Ted Beck, who is retired from El Cerrito High School.

Lage: After the lectures, would there be sections? Small groups?

Helmholz: Yes. We'd have a regular lecture, and the advantage of the lecture was that you could show demonstrations. Then we'd go into a general free-for-all discussion. Sometimes we'd just sit in the lecture hall and talk about different problems or what might be a way of showing some phenomena to the students. Then we would have some laboratory work which the teachers would have to do. Then some years we had them devise their own experiments or even build their own experiments that they could take back with them to their high schools. I'd give out examinations, and we would discuss, well, what was good about this examination and what was not good, what was too hard, what was too easy, and what kinds of questions they could use for their own classes when they had them.

Lage: How did this program get initiated? Was this your idea, and you applied for a grant, or--? Do you remember how it started?

Helmholz: I'm sure it wasn't particularly my idea; this was the Sputnik era, and there was the general feeling throughout the country that something ought to be done to improve the science education. And being one step above the high school education, we thought, well, maybe we could help with the high school education.

Then I think the Lawrence Berkeley Lab helped with the first program, or really suggested starting some of these programs, but then the NSF stepped in and was very instrumental. In the initiation of the programs, you had to apply for a grant, but in the beginning, I remember, you didn't have to even write

very explicitly what you planned to do. They were very generous in handing out the grants, so that was a fairly straightforward procedure to get a grant. The American Association of Physics Teachers would often have, at their annual meetings, a discussion of some of these NSF programs. And I remember I once went, must have been in 1960 or '61, back to New York to a meeting, and talked about our experience, and got comments on it, and heard talks on other experiences with summer programs.

Lage: So this was part of a general national trend.

Helmholz: Yes, it was. And I think there was some good that came from all those. I think that the supply of teachers and the improvement of the high school teaching was really evident. Probably somebody should have taken college surveys to prove it, but I'm sure that there was an improvement.

And then things just leveled off. I don't think, although I guess some people feel, that the quality of physics teaching in high schools has gone down in the last, let's say the last ten years. I'm not really well enough aware to judge that, but I'd be quite surprised if there had been a substantial decline in the quality of high school physics teaching. It depends so much on the teachers themselves. I think it's harder now to get good high school teachers than it was fifteen years ago.

Lage: Because of the lack of monetary rewards?

Helmholz: Yes, I think that fifteen years ago or twenty years ago there were a lot of college graduates who felt that teaching physics or high school teaching in general was very much a worthwhile field to go into. They were willing to devote their careers to that. I guess the lack of monetary rewards probably didn't seem so important at that time, and now, I guess, it does. So that's unfortunate, but that's the way it is.

Honors Course in Physics

Lage: Are there any other curriculum things we should talk about? I know you taught the honors course. What was that?

Helmholz: In connection with the introductory course in physics--the course that I called the engineering course or the biology course, and I think partly sparked by the Berkeley Physics Course book development, we've had honors sections of those courses.

Lage: Would they have the same lectures and then have separate sections with teaching assistants?

Helmholz: No, they wouldn't even have the same lecture. That was another thing that we could do with a good-sized faculty. We could have three different people giving Physics 4A--one of them giving the honors section and two of them giving regular sections of Physics 4A. The honors section would be chosen from the students who showed by their high school records that they could do an honors section, and then sometimes by an examination which would be given to the students either just before classes started or at the first lecture. Then students would be picked out who seemed to have the knowledge that we considered prerequisite to the honors section. The honors section would then go through the standard material a fair amount faster than the regular section, and then go on to more advanced aspects of, for example, mechanics, if it happened to be 4A, which is mostly mechanics. Or electricity or magnetism if it happens to be the 4B, which is mostly electricity and magnetism.

Lage: Was that fun to teach to students?

Helmholz: Oh, yes. It was more of a challenge, I think, than the regular section. It was more of an intellectual challenge. There was always a question with the honors sections as to what do you do when a person has finished the whole sequence in honors sections? Do you then let him skip some upper-division course? And there've been attempts to do that.

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Lage: It seems like it would be hard to sort out what course might be eliminated.

Helmholz: Yes, it is a hard thing to do. For physics majors, you can always make special rules. You can say, "Well, look, you did so well in that honors course that you don't need to take the upper division course. You can go on and take some other upper division courses. That's been done, and of course some of the students in the honors courses would be from other departments, so they would not necessarily go on to upper division courses anyway. I think chemistry provided a fair number of good students in the honors courses, and engineering some.

Lage: Do the physics majors take a good deal of chemistry, the way the chemistry majors do physics?

Helmholz: No, they don't really. It's always been sort of a bone of contention between the chemistry and physics. I think the chemistry department feels that physicists ought to take more chemistry. Then we always say, "Well, you don't have any courses that our students would fit into." I think it's somewhat better now than it was when I was chairman and for a number of years afterwards. We used to require a beginning course in chemistry of all our students. Now I think we only urge students who have not had much chemistry to take Chem 1A or 1A and 1B, or 4A and 4B. Chem 1A-1B is a very demanding course. Chem 4A-4B is ten units, two five-unit courses. Chem 1A-1B is two four-unit courses. When you consider the number of units you need to graduate, why, that's a pretty good chunk, so students wouldn't have really the time to take much more chemistry. In addition, I think the chemists themselves gave their upper division course more strictly than we gave our upper division courses for their own majors. We had that course like the atomic physics course--Physics 121, it used to be--and we gave that specifically for students from a lot of different departments. Sometimes we would have two or three sections of that in one semester. The chemists didn't have courses like that.

On the other hand, I guess there's the usual tendency to stay in your own department that both the chemists and physicists had. Chemists had a course in nuclear chemistry which most of their students took and relatively few physics students took, and we had, of course, nuclear physics, and mostly physics students took that.

Lage: Was there a lot of overlap in those two courses? Was there an area where they kind of come together?

Helmholz: Yes, there's an area, and I think that we probably at one time talked about it and just gave up the idea of coalescing the two into one. The chemists felt they'd have to give more chemistry in the course, and we felt there ought to be more physics in the course, and so we just decided we'd keep our course in physics and they'd keep their course in chemistry. So, let's see, we got started on this--

Lage: The honors course, we were talking about.

Helmholz: --through the honors courses. There was a good deal of discussion in the physics department faculty about what one should do with the students who had taken the honors course, and how could we make the honors courses mean more to students. For example, we had to grade them differently, because essentially all the students in the honors course would be A or B category

students, and in the regular course you give a fair number of C's. I think, oh, perhaps half the grades would be C's, 20 percent A's, and 30 percent B's, or something like that. And then the ones that you really didn't think should go on or ought to repeat the course would get D's. So that was a problem with the honors course.

Some students, for example, would want to get into the honors course after they'd had the regular course for the first semester. Then the question is, well, how should you manage that? I think that some of them did; if they got an A and a recommendation from the instructor in the regular course, they could try a semester in the honors course, and so on. And some students would drop out of the honors course and go into the regular.

Lage: In teaching these honors courses, did you get a sense of what qualities made these students more receptive or able to move faster?

Helmholz: Yes, I think you did. I'd never particularly thought of it from that point of view. Obviously intellectual ability is one aspect that's important. But the honors students really should have, you might say, a fundamental interest in the subject. It wasn't just that they needed it for their college requirements or something like that. We found--or at least I found--when I was advising undergraduate students, that the question of where they should go to graduate school, if they wanted to go to graduate school, was always an important one in which they would rely a good deal on the major advisors for suggestions. Then if a student has taken an honors course, you have an additional sort of handle on his abilities and his interests. So that you can perhaps advise him a little better as to what he should do, and use your experience with the honors course to write letters of recommendation for the student.

Lage: So it was valuable for somebody who wanted to go on to graduate school.

Helmholz: Yes.

Change to the Quarter System, and Back

Lage: Let's talk about the change to the quarter system and then back to the semester system. Was that disruptive? Did the

department like the idea of going to the quarter system initially?

Helmholz: No, I don't think anybody liked it. Well, there were some members of the faculty who had, for example, been to Stanford or been to Chicago, which I think, had a quarter system, who said, "Oh, it's all right, it won't make very much difference." But the faculty members who had not had that experience felt, oh, this was going to be the end of the world.

Obviously it wasn't the end of the world, and we got along all right. I didn't feel myself--I guess it's partly because I'm easygoing--that it would be too difficult. It did require a fair amount of planning, of work and planning. When you had a two-semester course, that wasn't bad because you could just spread it out over three quarters rather than two semesters. But the single-semester course, what do you do when you have to go to a quarter system? Do you make it two quarters and add a little bit on to the course, or do you try and reduce it to one quarter?

Lage: Did the change to the quarter system stimulate a lot of curriculum revision?

Helmholz: Yes, I think there was some good, but it was a lot of work. I think that the people who have new ideas about courses will in general come forth and express those ideas without necessarily having the change from the semester system to the quarter system to foment the new idea.

Lage: This general faculty resentment of the quarter system is brought up when people talk about FSM [the Free Speech Movement]. Some say it contributed to sort of an undercurrent of resentment against the University administration.

Helmholz: I think that the faculty had preferred the semester system, but one other thing that caused a problem was that the Berkeley faculty, particularly the older members, were used to starting the fall semester in August and having the full semester ending in December, before Christmas, and then not having to go back in January and review a little bit before the final examinations. That was the old Berkeley calendar. That's Professor Birge's.

Lage: He set the calendar, I gather?

Helmholz: Yes, that's right.

Lage: So you taught under that calendar?

Helmholz: Oh, yes, I taught in that for a good many years, and then we went to the calendar in which the final examinations were in January. The quarter system does have, you might say, sort of an advantage in that respect, because the quarter ends before Christmas vacation. But I think it's true of almost any faculty; they don't like to change their calendar.

Lage: Did you sense that the department felt this was something imposed from above?

Helmholz: Yes, I think so. Clark Kerr had the idea, and I think it was imposed from him. He was careful to try to convince everybody that this would be a good thing. And he had one thing in his favor, and that is that he wanted the University to be used essentially continuously throughout the year. And so we'd divide the year into four quarters, and students could go four quarters if they wanted to, or they could go three quarters if they wanted to and take another quarter off, or whatever--

Lage: When you made a change to the quarter system, though, did you offer the subjects in the summer?

Helmholz: Oh, yes, we did. Unfortunately for Clark Kerr's idea, students just didn't come in the summer as much. Well, they obviously wanted to take the summer off. And the faculty didn't seem to be as enthusiastic as I was about teaching in the summer and taking one of the other quarters off.

Lage: You thought that was a good idea?

Helmholz: I thought that was a good idea. We continued on that system for, it couldn't have been much more than three years. I know in the year '65-'66, I taught in the summer quarter and took one of the other quarters off. Betty and I had a son who was in India, so we decided to take the winter quarter off. We went all the way around the world and visited him in India and other friends in other places, and had a great time.

Lage: It's a nice time to travel.

Helmholz: Yes. And teaching in the summer here is not bad at all. It's very pleasant, actually. Whereas teaching in the Midwest, particularly in the northern part of the Midwest or even the East, it gets terribly hot so it's not as pleasant.

So the Berkeley faculty, I think, grumbled a fair amount about changing to the quarter system. But they did it. Then I think everybody was glad to go back to the semester system a few

years later. That's not an ironclad statement, but most people were, at least.

Lage: All the other campuses stayed on the quarter system.

Helmholz: Yes, that's right. Clark Kerr was no longer president when Berkeley changed back. Of course, since he'd started the quarter system, I think he would have been hard pressed if he'd maintained his presidency for long enough to have to consider going back to the semester system for Berkeley. It was President Saxon who finally said, "Well, if Berkeley wants to go back to the semester system, all right, let them go." So that's the way that came about.

Lage: Did you find teaching more difficult under the quarter system, having to cover the material faster? Or give the exams faster?

Helmholz: You had to give more exams. I didn't feel a great difference. I mean, except for the fact that you had to give one more final examination each year, it was a little more--

Lage: It must have involved more administrative effort--more assigning the classrooms, putting the grades in.

Helmholz: Sure. But I didn't find that bothered me. Well, of course, I was not department chairman under the quarter system.

[Tape interruption]

[Throughout the duration of Tape 10, Side B, music in the background came from the carillon in the Campanile.]

Campanile Suicide

Helmholz: One time when I was chairman, I had the office on the corner of Le Conte Hall, which looked right out at the Campanile, and I had a call from somebody back East. I was talking to them on the telephone, and I think I was standing and looking out toward the Campanile, and all of a sudden I saw a hat floating down from the Campanile. And I obviously hiccuped or something, because the person at the other end of the line said, "Well, what happened?" I said, "Well, somebody just jumped off the Campanile."

Lage: You knew immediately what it was.

Helmholz: Yes, I could look down and see. So that story got told around among physicists for quite a number of years following that happening. I don't know when that would have been. Actually, it was the father of a graduate student in physics who did it.

Lage: Oh, it was not a student?

Helmholz: It wasn't a student. It used to be much easier to get up in the Campanile, and there wasn't glass around the top.

Lage: Yes, they put the glass up not after that incident, but I think there was another after that.

X THE GRADUATE CURRICULUM

[Interview 6: November 7, 1989]##

Admission Criteria

Lage: I thought maybe you could talk about the graduate curriculum today.

Helmholz: All right. We've always had at least as many graduate students as we had undergraduate majors, and in recent years, I guess really ever since about 1946, we've had more graduate students than undergraduate majors. So the graduate curriculum is an important one.

Graduate courses are taught by all members of the staff. Particularly the theoretically inclined members of the staff tend to give more graduate courses, and the experimentalists the undergraduate courses.

Lage: Why is that?

Helmholz: Although that isn't necessarily true. I think it's because the courses themselves are more theoretically inclined. In other words, we have a graduate course in mechanics that tends to deal with, let's call it, the more advanced aspects of dynamics, and the theoretical physicists are the ones that use those more advanced methods. And some, of course, even more advanced methods that they don't give in the courses, but they're better able, I think, to give those courses because they tend to use that kind of material in their own work. And the same thing is true of electricity and magnetism and quantum mechanics.

This is not a hard-and-fast rule; for example, Eugene Commins has often given the graduate course for beginning graduate students--that's Physics 221A-221B. He's very well founded in quantum mechanics and uses it some in his research, but he's an experimental physicist. Nevertheless, he gives

that course with great success. So that's the tendency but not the rule.

We give quite a number of graduate courses, and each one of the courses is given essentially every semester. When we were on the quarter system, we would sometimes miss a quarter, but graduate students don't all start in with a given curriculum and follow it for every semester. They have some choice, and in addition, some graduate students need to go back and take an undergraduate course before they start the graduate courses. And we realize that when we accept the graduate students. I would say that it's a little bit less pressing nowadays than it was, say, twenty or twenty-five years ago. But there are some small colleges that don't give the same kinds of undergraduate courses that we do, and so occasionally a graduate student will start in and have to go back and take two, or sometimes as many as three, undergraduate courses.

Lage: In admitting your students, do you look at--

Helmholz: Oh, yes, we look at all the aspects, and we do--

Lage: Are they penalized for going to one of these smaller colleges?

Helmholz: No, not really. I mean, if we know the small college -- particularly if we know some member of the faculty at the small college and he really recommends the student--then we would usually accept him. But we have nowadays and we have had, oh, I think since the late forties or early fifties, many times more the number of applicants for graduate school than we can accept. I think nowadays, oh, it's at least four to one; we have four times as many applicants as we can accept.

Lage: What criteria are--

Helmholz: Well, the criteria are, one, the undergraduate record. Two, we require them to take a graduate record examination in physics, and we look at the score on that examination. And third, I should say the preparation for graduate work in physics. If a student applies who's majored in economics and taken only the introductory physics course, we will almost certainly reject him.

After the war we had a number of such students who applied, and we had a special category of student at that time. They could be registered as undergraduates in the College of Letters and Science with a second major. That occurred for people who majored in economics or political science or music or something like that. But they could then take courses to make up that

undergraduate major, and then we would decide, mostly on the basis of how they did in our undergraduate courses, whether we wanted to admit them. We don't have that happen very often any more. I think it occurred after World War II, when a lot of students wanted to get into physics who had for various reasons had a completely different major in their undergraduate work.

The other thing we look at, of course, are letters of recommendation. With those criteria, there's a committee of faculty members--I think there are three or four on the committee--they work hard during the months of November, December, and January to decide on which students should be admitted. We actually admit somewhat more than we feel we have room for, because a lot of students will apply to, oh, let's say, five or six top-flight graduate schools--Harvard and Caltech and MIT and, oh, Michigan, Berkeley, and Stanford.

Lage: Are those the schools that you're competitive with?

Helmholz: Yes, those are the ones that we really compete with. I should add Princeton and Cornell in addition, so that many students will apply to, let's say, all six or seven of them, however many I mentioned. Then they will pick the one that they one want to go to, partly on the basis of the financial support that they can get, and partly then on the basis of what appeals to that student the most.

Financial Support for Graduate Students

Helmholz: We have teaching assistantships which we can offer to new students, and then some fellowships. In the last couple of years, the School of Education has arranged with the federal government some fellowships in physics for graduate students, and we have to match some of the funds, so that is an additional method of supporting graduate students. We really do have, I think, pretty good support for graduate students. Most of the students who come in fresh either have fellowships, national fellowships such as NSF, or they may be able to get a University fellowship, but that's somewhat more uncertain because the University fellowships tend to be awarded to graduate students who are already here. So it's NSF fellowships, mainly, that take care of that.

And then the other way that we take care of new students is with teaching assistantships. Teaching assistantships are supposed to be a half-time job; well, it's somewhat less than

that because the student is really dealing with the same subject that he's doing the graduate work in. It's very helpful to the department because we do have a big undergraduate load to carry, with students from engineering and the biological sciences and so on. All of those courses do require graduate students to help with the laboratory work and graduate students to help with the discussion sections.

Lage: Does this mean a lot of the TAs are first-year graduate students?

Helmholz: Yes. I don't know what the numbers are right now, but I think it's almost always been true that the majority of the teaching assistants are first-year graduate students. We always have to keep a few of the second-year graduate students who know the ropes in the courses. So they tend to be students who are either going to do research work in some field where they can't get support from their research director, or they haven't reached the point where they want to start research yet.

So that's the way the graduate student, you might say, employment, goes. Nowadays we have a lot of students who are employed on a half-time basis by the research director, who has a government contract of some sort--for example, works at the Lawrence Berkeley Lab, which has assistantships for graduate students who want to do their research up there. So by the time the graduate student is in his third year, we have a policy that we should follow more closely than we do, that essentially all graduate students should have a research job rather than a teaching job.

Lage: And there aren't enough research jobs available?

Helmholz: Well, yes, but that is not absolutely true, because some of the faculty members will have times in their careers when they don't have research support. In theoretical physics, sometimes, a faculty member may not have any research support, and if he wants to take on a graduate student he'll just tell him, "Well, you'll have to be a teaching assistant while you're doing your research work." It's a lot easier, of course, if a graduate student can work on his research at the same time that he's earning some money. That is pretty much the situation at present. And I think it is all through the country in graduate schools in physics, that research is supported by federal grants, occasionally by state grants.

Lage: Has that gone up and down over the years, through your chairmanship and since?

Helmholz: Yes, it's gone up and down, but I think it's gone mostly up, which is fortunate for physics research. The big laboratories like the Lawrence Berkeley Laboratory and now the Space Sciences Laboratory have research assistantships which they provide for graduate students. Our graduate students stand well enough up in their work so that they are very often given these assistantships. It's not exactly competitive in the sense that you have to take examinations or anything like that to get the research assistantship. But you have to have the faculty member who recommends you, and usually you don't need more than the one faculty member to recommend you.

Preliminary Examinations

Lage: Is the students' choice of their research project often determined on what grant they can get onto?

Helmholz: Well, I think, unfortunately, yes. Although perhaps I shouldn't say "often." Often students will come here--and this has been occurring for the last ten or fifteen years--a graduate student will come, and he decides before he comes that he wants to go into theoretical physics. Well, he'll take theoretical physics, the standard courses, in the first year, and then he has to pass the preliminary examination, which is both a written and an oral exam.

Lage: After the first year?

Helmholz: Yes, students are supposed to take it in their second semester. For fairly good reasons, some students put it off until their third semester. But then once they pass the examination, they can start in on research. If they don't pass the examination, they have to take it over again the next semester. Sometimes this is difficult for a student who comes without what we call the "usual" preparation for graduate work, because one of the examinations covers material that's given in the first graduate course. So if he hasn't had at some previous school something which is a little more advanced than the usual undergraduate course, he'll have to take that course the first semester and then go on to the examination the second semester. But if he is way behind, then he'll have to take at least two semesters before he's ready for the examination.

Lage: And do you have many students who simply don't pass the preliminary examination, even after the second go-round?

Helmholz: Yes, and we keep having arguments in the faculty as to what we should do about a student who fails the examination the second time. There is both the written examination and the oral examination. At the present time the examination is sort of divided into two parts: one you might call classical physics and the other, modern physics. I think one examination is given on a Friday afternoon in the written part, and one on Saturday morning.

A student at one time was required to take both together. Nowadays he can take one and not the other if he feels--and can get his advisor's consent--that he really isn't ready for one examination. He can take, for example, the classical examination and then take the classical part of the oral examination. In this way he can sort of spread out his exam-taking. But usually most students will take both written examinations and both oral examinations at the same time.

Lage: And have you resolved what to do with the ones that fail twice?

Helmholz: Not really.

Lage: There must be some mechanism for failing out of the program.

Helmholz: We do just tell them that we don't think they're the proper candidate for a Ph.D. and they can go somewhere else, which a fair number of them do. I think they'll take a smaller school, one that according to us doesn't have quite as high standards as we do.

Lage: Is there any master's degree?

Helmholz: Yes, they can take a master's degree, and all that's required for a master's degree is coursework (thirty-six semester units) plus an oral examination, which is not really very demanding, either. We don't have a master's degree requirement such as some engineering programs do in which the student has to write a thesis of some sort. All of our thesis work is done for the Ph.D. So we don't make much of a master's degree, and it's really sort of a booby prize for students who are going to fail out of the graduate program

Well, anyway, I started out with courses. These courses are given every semester. That is, the A part and the B part of a course will be given in the fall semester. And occasionally, if it seems that there are a lot of students, we may even have two sections of the A part and one section of the B part. And then the next semester, two sections of the B part and one section of the A part. So students have a good opportunity to

make their way through their required program with a good deal of dispatch. In other words, they don't have to wait for a semester in order to get some required course.

There are now two two-semester courses that students are required to take before they can get their Ph.D. degree, Physics 221A-221B and 210A-210B. That makes twenty units. Nineteen more units are required for the Ph.D.

Then they usually take some other courses in their field of specialization. There's a graduate course in high energy physics, and many students will start out with an undergraduate course in nuclear and high energy physics because they haven't had that at their undergraduate institution, and then go on to the graduate course.

Lage: But you expect them to have a broad background in all the fields?

Helmholz: Yes. There's a certain minimum standard which this preliminary examination is supposed to cover. In other words, that's mainly a breadth examination. There's no special knowledge required in any one of the fields that are covered by the preliminary examination.

Lage: Does a whole range of faculty members sit on these preliminary examination committees?

Helmholz: Yes, they do. There's a committee of, I think, three to five members who make up the written examination. And after the written examination they have to read all the papers and mark them and agree on some sort of a standard for passing the written examination. Then the oral examinations come about two or three weeks later. They occur on a Saturday morning. The written examinations are usually Friday afternoon and Saturday morning--sometimes Saturday morning and Saturday afternoon. But the oral examinations are given by the rest of the faculty. Everybody who's not on the written examination committee is essentially required, short of death in the family or something like that, to take part in the oral examinations. Usually it's a two-man committee which gives the oral examination.

The oral exams are scheduled for forty-five or fifty minutes, so that each one takes an hour total before the next examination. Then the whole faculty gets together and decides who passes and who doesn't. There are always some students that pass right off; I mean, there's no question about it. Then there are always some tricky cases in which one of the two examining in, let's say, classical physics, will say, "Well, I

thought he did well enough." Then if the other person said, "Well, I would flunk him," then the question always comes up, "Would you be willing to accept him as a graduate student in research?" That's always a crucial question.

Lage: Is it just between the two who were on the committee, in on the examination, or does this broaden out into a discussion among all the participating faculty members?

Helmholz: Well, it broadens out into a--. I mean, those two will report on the result, and then we have the written scores in addition. The written scores are not given out to the graduate students. So you have both. If a student has done marginally on the written scores, and the written committee, which will be present also, says, "Well, we've just barely let him pass, and if one oral scorer says, "Well, he shouldn't pass," and the other one says, "Well, he probably should pass," the chances are that he'll be failed. So that's the way it works out, and as I say, there are always some cases that are difficult to decide, and some graduate students are sent back for the second try.

Lage: Are there some professors, do you think, that the graduate students despair of getting on their committee?

Helmholz: Yes. That's not really a common complaint, though. I think that the common complaint is that the graduate student will say, "Well, I just am so scared of oral examinations that I can't think when I get up in front of two faculty members who are examining me." So we have a lot of problems, and we keep having problems. The students will say, "Why do I have to pass an oral examination? I'm not going to apply for a faculty position when I get my degree; I'm just going to do research," and so on. Well, we still think that even if he's just going to do research, he's got to communicate with the other physicists around the country. He can't live a life in, sort of, exile.

We've had that system for, I think, about forty years now.

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Helmholz: When I was a graduate student, we had just oral examinations, and all the students had to take those oral examinations. The committee was usually a three-man committee. There weren't as many graduate students then, so it wasn't a big affair. You took them first in mechanics, and then one in electricity and magnetism, and then one in optics, and then one in thermodynamics and kinetic theory. So there were four oral examinations. And everybody who advanced to a Ph.D. had to pass

all of them, although it wasn't required that you pass all of them before you started research.

Now it's pretty well true that students have to pass the examination before they start research. It isn't completely true because some students come in with having done some research at their undergraduate institution or even having done some graduate work before they come here. And they can be hired on by a faculty member because he realizes what they can do and they can help him with his research. Sometimes they're the worst problems, though, because they tend to their research and not to studying for this preliminary examination.

Lage: Is there a problem with foreign students and ability to communicate in English? Do you have a good number of foreign students?

Helmholz: Yes, we have a fair number of foreign students. I would judge that it isn't as much of a problem now as it was, oh, right after World War II. Students from other countries seem to learn English in their native country and don't come here if they're really deficient in English.

Concern about the Time to Complete the Ph.D.

Helmholz: We have a situation in which the time required to get a Ph.D., to finish a thesis, is obviously too long for the University in general.

Lage: How long?

Helmholz: I think our present time averages about six years.

Lage: Has that gone up over the years?

Helmholz: It really hasn't gone up much, but we think it ought to be closer to four.

Lage: You do get this preliminary exam out of the way with dispatch.

Helmholz: Yes, well, we try to get that out of the way. If a student has gone through two years and hasn't passed a preliminary examination, then we really require him to try at each semester. When we were on the quarter system, we had the preliminary examination given in the fall and again in the spring quarter.

So we just gave it twice a year then, and now we give it twice a year--once each semester.

But there's a good deal of concern. Physics is not in any sense the worst of the offenders as far as time required for getting the Ph.D. I don't mean to pick on the English department, but they have students there who will take courses for several years, and then they'll start on a thesis, and it takes them so long to write the thesis or to do the research for the thesis that they often will go off and get another job for a couple of years, and they'll come back and register as a student again. So it takes them, in many cases, even longer.

But we feel, on the physics faculty, that the students shouldn't take as long as they do. So every so often we bring the matter up--or the department chairman will bring the matter up and point out to the faculty that they ought to get the students that they advise to take the preliminary examination early.

The Qualifying Examination

Helmholz: After a student has done research for a while and the faculty member decides that he is making good progress, he has one more oral examination, called the qualifying examination. It's an examination which the student has to pass before he can get his degree. Well, students often like to finish their thesis before they take this examination. We've tried to get the students to take it somewhat before that.

Lage: Is this in their area of specialty?

Helmholz: Yes, It's an examination really in their area of specialty, but we have to have one person from another field on the examination committee, which usually consists of five faculty members. One from another department--electrical engineering, chemistry, biology, sometimes mathematics. Sometimes it's a little difficult to find the other member.

Lage: Is that up to the student? Or his advisor?

Helmholz: It's up to the advisor. The student can say, "Well, I know So-and-so in electrical engineering pretty well and I think he understands my thesis." The faculty member may say, "Well, all right, let's try and get him to be the other non-physics faculty member."

But this examination, again, is probably more terrifying to the student than it need be. Occasionally a student will come to the examination and fail it, and be given a second chance. I guess there probably have been a few cases in which the student has failed the second time and never gets his degree. I don't know of any. I've had one or two of my own students who didn't pass the first time and had to take it again.

Lage: Is this mainly because of communication problems, do you think? Or are they really not well enough prepared?

Helmholz: They're prepared on their thesis topic, but if it's, for example, high energy physics and they've been doing their experiment on scattering of pi mesons, they won't know anything else but the scattering of pi mesons. And they're supposed to have studied and read up on other kinds of high energy physics. So it's an overspecialization which usually is the reason for a student having to be failed on that first attempt. Then it's up to the faculty member to really sit down with him and say, "Well, now, you learn everything that's in this course and another course, and then I think you'll be prepared for the examination."

So that's the way that the graduate work goes. I haven't really mentioned much about the courses. The courses are fairly standard for the ones that are required for the Ph.D. degree. In other words, they have to get an A or a B in the courses, but those are standard. Then there are a good many other courses that are more specialized and are not required of everybody but are more or less required for a student doing work in that field. Solid state physics courses, or high energy physics courses, or courses in plasma physics, or courses in space physics, are specialized, and not many students will take one of these specialized courses in another field, but he may just go and listen to the lectures and get some of the material by osmosis from listening to the lectures and not doing the problem sets or not taking the final examination.

Lage: Is the Berkeley curriculum for graduate students standard for comparable schools? Or does it have a particular feature that is--

Helmholz: I don't know. I think we give more oral examinations than a good many other schools, but it really is not terribly different.

Lage: Is there a lot of communication between schools in setting up the curriculum and exams? Are there associations where you discuss matters like this?

Helmholz: No, there really aren't associations. I think faculty members always talk to comparable faculty members in other institutions and may say, "Well, why don't we do it more like Harvard is doing it now," or "Why don't we do it more like Princeton or Caltech," or something like that. But this seems like a pretty satisfactory solution to the problems of training Ph.D. students. We don't have any sort of standard examination which all Ph.D.'s throughout the country have to take or anything like that. It's more of an individual affair.

Research Directors' Role in Training and Placing their Ph.D.'s.

Helmholz: When the student is finished, the question of what kind of a job he can get is one that again has to be pretty much up to the research director. He will usually know faculty members at other institutions, and if he thinks the candidate is worthy of it, he'll recommend him for a job at another institution. Usually he'll know that there is going to be an opening in some faculty at another institution, and he'll just write to them and say, "Do you have such an opening? I've got a student whom I can recommend to you." We don't usually take our own Ph.D.'s to add to our faculty. We like to get students from other universities. We have taken a number who have gone away from Berkeley for a few years and maybe made a good reputation somewhere else, and we will ask them back. In physics it's true most often that a student who gets his Ph.D. will take what we call a postdoctoral position at some other institution.

Lage: Is it most common to take a postdoctoral position, rather than go right into a faculty position?

Helmholz: Yes. And usually that postdoctoral position will be, oh, perhaps, at a national laboratory or in some research group at some other institution. But he won't go directly into a teaching position. There are a few Ph.D.'s who just want to teach and they'll try to get into a teaching position immediately. And the people with a Ph.D. who go into teaching at some of the smaller colleges that don't have a big research program are apt to do that as soon as they get their Ph.D. But most of the ones who are very much interested in keeping up with research in a place like Berkeley, or one of the big state or private universities, will tend to go into a postdoctoral

position for one or two years and then try to get back into the academic work.

Lage: It sounds as if that relationship with the research director is pretty important for the student's future.

Helmholz: Yes, it is very important. There are not so very many cases that I know of in which the student starts out with one research director and decides after two or three semesters that it's a mistake for him to be with that research director and just changes over to somebody else. It does occur, but quite rarely. Usually when he has passed this preliminary examination, he'll go around and talk to a number of faculty members about the research opportunities. And sometimes a research director will say, "Well, look, I've got as many students as I can handle right now. Why don't you try Professor X or Y or Z?" So that's the way they can proceed. Usually they talk to enough faculty members so that they can, I think, make a pretty intelligent choice as to whom they go with.

Lage: Whom they're going to fit in with?

Helmholz: Yes. We have one thing that occurs in physics, and that is that a number of students who want to get into a particular field will find that the only person in the physics faculty is, you might say, filled up with students. But that physics faculty member will say, "Well, look, rather similar work is being done in electrical engineering by Professor Y over there, so why don't you go over and talk to him?" Well, sometimes it happens that the electrical engineering faculty member will say, "Yes, I see you're well prepared for this kind of research, and sure, I'd be glad to take you on in my laboratory." That has worked out in a fair number of cases.

And the physics faculty member to whom he went first will have to agree to this. He will say, "Well, yes, I'll be the physics representative in the qualifying examination." Or "I'll try to spend enough time to understand what he's doing and approve it."

Lage: Will he approach the problem or the work differently from the students in electrical engineering? Or is there enough overlap?

Helmholz: There is usually enough overlap. It's pretty much up to some member of the physics faculty to keep track of the student and to keep track of the work that he's doing, but as far as the research work goes, it is pretty much up to that person in electrical engineering or chemistry.

It sometimes happens that a student is interested in a particular kind of theoretical physics, and I know of several students for which the only research around the campus is being done by theoretical physicists at the Lawrence Berkeley Lab. Then that student has to make arrangements with somebody on the physics faculty to keep some supervision, but the main research director will be up at the Lawrence Berkeley Lab.

Lage: How about out at Livermore? Are there these kinds of arrangements with Livermore?

Helmholz: There have been a few cases like that, but there aren't very many. Certainly the number is not increasing; it's probably decreasing.

I had a student once who wanted to do a piece of research that she had had some experience with at MIT, I think, before she came out here. There was one fellow at the Lawrence Berkeley Lab, actually, in the chemistry division up there, who was interested in that kind of work and actually had done some of it. So I made arrangements with him so that she spent essentially all her time up there working with him. I tried to keep good track of what was happening, and when she came to write her thesis, I did have a terrible time because sometimes there would be cases in which I would have to call him up, up at the Lawrence Berkeley Lab, and say, "Look, what does this mean?" [laughter]

She finally got her degree, all right, but writing the thesis was a hard problem because he, of course, was willing to supervise the research work, but he wasn't willing to supervise the writing the thesis. So I was the one who had to do that. But it varies a good deal from case to case.

Lage: Usually if the professor is the research director, they supervise the dissertation and the research?

Helmholz: Yes.

Lage: Now, how much supervision does the professor put in? Or does this vary from person to person?

Helmholz: It varies a good deal from person to person and problem to problem, really. Sometimes the faculty member will be right in the laboratory with the student and doing the experiment so that he's, you might say, sort of full time on the same problem. On the other hand, sometimes if he has several other students, he may be involved more closely in some other experiment that another student is doing, so that he may not spend nearly as

much time. Depends a good deal on the problem and then also on the student. Some students seem to have the maturity to manage their own experiment without the faculty member having to spend a lot of time with them or with that particular experiment.

Lage: But it doesn't sound as if too many students are just turned loose on a problem. Which I've heard from other professors in other fields--that they didn't really supervise very much.

Helmholz: No, I would say that there are not so many cases like that. I guess in physics the faculty member spends more time, you might say, directing the research. He, of course, would like the student to develop the methods of going about the research topic as much by himself as possible, but it isn't a case in which the faculty member says, "Well, here's a problem and there's the lab. You go ahead and do it." He spends more time than that.

Lage: Is the problem usually one that a team of people are working on? Or does this vary also?

Helmholz: That varies a good deal. I think it's now true that more Ph.D. problems are team problems. And then, of course, you've got to decide, well, who should get the credit for the thesis? Maybe one person at Stanford will be doing work on a problem, and the student at Berkeley will also be doing work on the same problem. The theses can't be on exactly the same problem, but that's another difficulty with team research. You have to decide, well, who should get the thesis out of this? Some students, of course, will be in on several team researchs and get the Ph.D. on one particular one of those.

Lage: It is sort of divvied up?

Helmholz: Yes. So that's a case in which you don't have an easy time, or a faculty member doesn't have an easy time. So many of the research projects, particularly in high energy physics and in astrophysics, tend to be big team efforts. It's not quite so true in theoretical physics or in solid state physics, but in high energy physics the thesis problems may appear in the journals with sixty names. [laughter]

Lage: Must be hard to distinguish yourself in a crowd like that.

Helmholz: It is, and when you're trying to get a job in a school with research in that field, it's hard really to assess what the abilities of a candidate are. That's a case in which you can't point out just that somebody who's applying to Berkeley for a position has six papers, because all six of them may have forty authors. You've got to go to the person who's senior in the

list of names on the paper, and he will say, "Yes, this student really was on this problem from the beginning, and he knows everything about that problem, and he's done more work than the other thirty-nine authors whose names appear on the paper."

Lage: So recommendations would come from the senior author?

Helmholz: Yes. That becomes almost a personal matter between faculty members who have been concerned--or senior members. It needn't necessarily be a faculty member; they may be senior members from the laboratory at Batavia, Illinois, or the Brookhaven Laboratory. That same thing occurs in space physics, also.

Lage: Now, when you have graduate students, do you feel a responsibility to see that they get placed someplace?

Helmholz: Oh, yes. That's important. If you try to train them so that they can get a good job when they--

Lage: But what about using your contacts to find them positions?

Helmholz: Well, yes, you do. I mean, I've had a number of graduate students who I really couldn't recommend for faculty positions even at smaller institutions, but I felt that they might fit in well at Livermore or Los Alamos or maybe at Brookhaven.

Lage: Because of their ability to teach, or the quality of their research?

Helmholz: Both, really, because some students are just not very good teachers. They're fine in the research lab, but I remember that when Professor Moyer and I had our research groups together, there were several students who we just felt were not really top-flight for a faculty position but would be good at the Lawrence Berkeley Lab or Brookhaven or Los Alamos.

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Helmholz: And then there were some other students whom we felt were really not top-quality students but still were good enough to do respectable research. I remember one who went to Boeing, and he's done all right but he's been more in the space physics kind of research. But you do feel--and I think all faculty members feel--a strong responsibility for getting a student placed.

Lage: It must be difficult, though, to confront the student with this judgment. Or do they sort themselves out?

Helmholz: Well, they sort of sort themselves out. You don't say--well, in a way, you do say, "Well, you're not as good as So-and-so." They realize that when they're doing their research. Their theses are respectable but not in any sense outstanding. Not in any sense any better than average. I think the students realize that. You try to get the best job you can for them, but you can't always get a very good job for them. If they're better than their thesis indicates, they'll make their way wherever they're going.

Lage: So you're not making the final judgment.

Helmholz: No, that's right.

Let's see whether there's anything more I should say about the graduate work. Sometimes the grades in courses are important to the research adviser in judging whether to take a student on or not. Most often if a student has got all B's, for example, in his graduate courses, there are some kinds of research directors who will say, "Well, all right, I'll grudgingly take that student on because my particular field of research doesn't require the kind of knowledge that would get him an A in a course." But usually students will have at least some A's, and the research director may say, "That shows that he can do the kind of work that I expect from a good graduate student."

Lage: About how many Ph.D.'s are awarded each year?

Helmholz: In physics, I think they're of the order of forty to fifty. If you figure that we have, let's say, two hundred graduate students and forty Ph.D.'s per year, well, that ought to mean five years. Usually there are somewhat more than two hundred, I think, nowadays. Let's say 240 students and forty Ph.D.'s per year, that would be six years to complete the degree. The average time is getting pretty close to six now.

Relations with the Graduate Division

Lage: Isn't the University sort of putting pressure on, to move the students along?

Helmholz: Yes, it is. The Graduate Division does this.

Lage: By what means?

Helmholz: Well, they have the records over there for when students get their degrees, and they figure out how many years it's taken, and they just tell the department chairman, "Look, six years is too long. You ought to be able to cut it down."

Lage: Do they also put a ceiling on the number of students you have?

Helmholz: Yes. I don't think they ever say, "Look, you've got to cut down your number of students," but they say, "We can't afford to have 275 graduate students in physics. The University just hasn't got that kind of resources." It's a perennial problem. The dean of the Graduate Division has some leverage; I mean, he may just say, "Look, you took in too many graduate students this last year." That's always a difficult problem because you may admit seventy students with the idea that twenty of them are not going to come because they get better offers, or they think they get better offers in other places.

[tape interruption]

Lage: Okay, we're back on. We're talking about the Graduate Division and graduate admissions process.

Helmholz: I think the dean of the Graduate Division and the Graduate Council talk about all these problems of numbers of graduate students and how long graduate students should stay before they get their degrees, and so on. So they have a fair amount of influence.

The chemistry department is the one that is really most strict about the numbers of years that graduate students stay before they get their degree. They have, I think, the best record. We sort of feel that we want our graduate students to be better prepared in the whole field of physics so that we don't let our students start research quite as rapidly as the chemistry department does. I think they would answer that their students are better prepared in chemistry in general when they are accepted as graduate students. But it's true that their students don't spend as many years getting a Ph.D. as ours do.

Lage: What about University-wide requirements, like language requirements? Is that something the department finds easy to conform to?

Helmholz: Yes, I think so. We don't have any language requirements now. For years we had two languages, then one, now none.

Women and Minorities in the Physics Department

Lage: I finished interviewing Sandy Elberg [dean of the Graduate Division, 1961-1978] not long ago, so a number of these things came up in talking with him. One of the things that he talked about was the effort to get more minority and women students as graduates, and how he worked with the departments to do that. Is that something you might be familiar with as it pertains to the physics department?

Helmholz: Yes, we still have problems in getting new women members of the faculty.

Lage: What about as graduate students?

Helmholz: We still have plenty of applications from women. We don't really have a lot of women graduate students. I suppose that perhaps the reason is that we don't have a very good reputation for keeping women graduate students after they've got their Ph.D.'s or that perhaps we have a reputation for being inhospitable to them. We certainly don't have a lot of women graduate students.

Lage: You mean they don't come here, or you aren't able to admit them?

Helmholz: Presently about 10 percent of our graduate students are women. The number is increasing, but I'm sure not fast enough for some. This last year 10 percent of the applicants for admission were women. That was fifty out of 526. Only five were blacks, four Latins or Mexican-American.

Lage: And do you have any women faculty?

Helmholz: Yes, we have two women faculty members now. We have one black faculty member, and we have a fair number of Asian faculty members. So I don't think we're remarkable in the numbers of minorities that we have, but we have enough so that I think we don't have a poor record for minorities and women.

Lage: Do you know if the department has made any effort to recruit minority students?

Helmholz: We claim that we're always out for the best people. We don't make a special effort to get minorities or women faculty members. So that I think we can't say that we recruit. When we have a faculty position open--and we do have a number of faculty positions that have been granted to us by the administration--we don't say, "Look, this has got to be a minority." We try to get

the best person we can, and if there happens to be a choice between two candidates, and one of them is a minority, we probably would take the one who is a minority person or a woman. But usually we don't make a special effort to get a woman or a minority person.

Lage: What about for the graduate students? Has there been any effort in that?

Helmholz: For the graduate students? We don't make special efforts to get minority graduate students, either. We take what we consider to be the best candidate. Again, if we have a tie vote sometimes, why, we probably would take the minority or woman student.

Lage: Have you been under any pressure from the Graduate Division or the administration to do that?

Helmholz: No, I don't think we have been. Again, I'll try and check up on this with some of the--

Lage: This wouldn't have come up during your chairmanship, I'm sure.

Helmholz: It wasn't the case in my chairmanship.

XI MORE DEPARTMENTAL CONCERNS

[Interview 7: November 21, 1989]##

More Space for Research and Teaching: Birge Hall and Physical Sciences Lecture Hall

Lage: We were going to start out talking about the process of getting additional space for the department.

Helmholz: Well, you go mainly through the dean of the College of Letters and Sciences, who at that time I think was either Lincoln Constance or Bill Fretter. Because the physics department was expanding in the fields like solid state physics for which the work would be done on the campus rather than at the lab, the need for more space was particularly acute.

Lage: So you needed research space.

Helmholz: So we needed, really, research space. We didn't need classroom space. We had, I think it must have been in the late fifties, changed a big classroom which we had in old LeConte Hall. It was a classroom that would hold three hundred students. It was a poor classroom from the point of view that it was very wide and not very deep, but it had been used since 1923 when the building was opened. We had changed that by putting more offices and it must have been four laboratory spaces in there.

Lage: And cutting down the size of the lecture room?

Helmholz: And then the lectures were just moved to the classroom in this building, which held two hundred rather than three hundred. Eventually the Physical Science Lecture Hall was built, which must have been finished in 1964. So we did have a lecture hall over there that would hold 550-525, I've been told recently, since I've been writing it up in connection with Harvey White's memorial. He was the one who really pushed for this rotating stage idea. After he had been away two out of three years to

lecture in Continental Classroom over TV, he came back with this idea. He was put on a committee to advise about a new chemistry and physics lecture hall, and he was put on the committee and he apparently brought up this idea of a rotating stage so you could have one part of the stage facing the seats, and the other parts of the stage could be used for setting up experiments.

Lage: They were hidden from the--

Helmholz: Yes, they were hidden. Have you ever been inside it?

Lage: I've been in there. I hadn't noticed that aspect, though.

Helmholz: Well, it has three sections, each one 120 degrees in arc, so you can set up experiments on two different sections while a person is lecturing on the third one. And each one of them is completely equipped for a lecture demonstration.

Lage: And then you'd rotate the stage to bring the--

Helmholz: And then you just rotate it. It takes, oh, something like the order of three or four minutes to rotate, so in the ten minutes between classes, why, you can rotate it. It's a neat system.

Lage: Does it get used in that way?

Helmholz: Yes. People in physics have not been particularly keen to lecture there. I think they were when it was first brought into existence, but it is a big lecture hall and you're pretty well removed from the students. The lecture halls Rooms 1 and 4 in LeConte Hall hold about 200 students, and they're big enough, I think. But Harvey White had the idea that he would put television cameras hung from the ceiling, and thought that this would be used more than they actually are. He felt that television was a great advance in education. Well, I suppose he's right that it's more of an advance than anything else we've had, but--

Lage: Did he envision his lectures being taped for high schools?

Helmholz: No, it was for college classes. He taught in that lecture hall for several years until he retired in physics. He gave the course for biology majors, and I'm sure he did it very well, because he was sort of an expert in that kind of lecture.

Lage: He must not have felt that the large crowd was a detriment.

Helmholz: No, that's right. I mean, he wanted to make it available to large classes and to perform the demonstrations which he thought

were appropriate. He even had a television camera that you could focus on small experiments so that if the experiment was of such a type that it needed to be within a small area and students couldn't see it, then he would focus the TV camera on it and project it up on the TV screen.

Lage: I see. The screens would be placed so the students could see--

Helmholz: Yes, they were placed around the room. There must be six or eight of them over in PSL. He had even experimented with the screens in Rooms 1 and 4 here. I've forgotten now how many physics lectures are given over there, but I think there are three or four classes given over there each semester. And chemistry gives a good many. And then they use it for other lectures, also. Other large lectures.

In the late fifties physics really needed that extra laboratory and office space for the added faculty that we had. People were in LeConte Hall--both this building, which was called new LeConte, finished in 1950 and--

Lage: You were outgrowing new LeConte by the end of that decade.

Helmholz: We were outgrowing it by the end of that decade. The geology department was next door in an old red brick building that was called Bacon Hall. It had been the University library at one time. It was a round building so that the librarian could sit in the center of the building and look all the way around.

Well, the geology department had a terrible time trying to remodel it and revise it, and so on, so they were very anxious to get a new building, which they did. It's the one that's over on, well, I guess they call that Astronomy Hill [the Earth Sciences Building].

So that was a great help because we could say, "There's just the right space for us," and I guess the University tried to get us to remodel Bacon Hall, but we made strong arguments against that. The walls were, oh, I think, three or four bricks thick, so they were great walls, and when they started to tear it down I think I left for a sabbatical leave in the summer of '62. They were starting to tear the building down about that time.

Lage: Were there objections on the grounds that it was an historic building?

Helmholz: I think there were a few people who objected on that basis, that it would obviously cost a good deal to build a new building and

they thought we could remodel it at slight expense. It must have been a pretty good building for a library in the days that it was built, but it was a poor building for a physics laboratory.

Lage: Did you work through the administrative committees--the Committee on Building and Campus Development?

Helmholz: Yes, you would have to--. We just went through the regular channels. The dean recommended that we get a new building, and when the new geology building was being built, which was before their building was torn down, we got favorable recognition of our need. So a fellow named Bill Nierenberg who was in the department here was one of the chief persons in physics who helped design the building.

Lage: Did he have any special skills to do that? Did he work with the architect?

Helmholz: He worked with the architect. He'd been around a lot of physics laboratories, first in Columbia and then in the University of Michigan and then here. So he knew a good deal about physics needs, and he went around and talked to other people and carried their ideas back to the architect. It was quite a problem because new LeConte Hall had been built with the same high ceilings that old LeConte, which had been built in 1923, had. So the floors between here and old LeConte are just on the same level, with the same number of floors. I think by the time that the architects looked at the ceilings here, they'd said, "You can almost have two floors in place of one, and twice as much space."

So Birge Hall, which is the building that replaces the old Bacon Hall, has more floors. They have a sub-basement and a basement, a ground floor, and then five floors--eight floors, whereas there really are only four here. So in order to arrange for going from this building over to the other building--there are walkways that connect this new LeConte with Birge Hall and old LeConte. Those walkways in many cases end you up with a half a stair down or a half a stair up in order to get to the floors in that new building.

It's been a very satisfactory building and has provided us with a lot of space. So it's only really now that people are beginning to worry about space again.

Lage: When was Birge Hall completed? At least twenty years ago?

Helmholz: Yes, that was completed in 1964. While I was away the year '62-'63, it was being built. It was named after Professor Raymond T. Birge, who was my predecessor as chairman. It was a great pleasure for him because he always told us that no buildings were named after living professors, so when they decided to name it after him it was a very pleasant surprise. He was here for the dedication. It was dedicated, I think, at Christmastime; they had a meeting of the American Physical Society at which that building was dedicated to him.

Lage: Professor [Joel] Hildebrand also got a building named after him. He said they just couldn't wait for him to die. [laughter]

Helmholz: That's about right. Yes, that old saying that you couldn't name a building after anybody who was living has been proved false by the passage of time. So that really worked out very well. In fact, the rooms, the laboratories over there, are well suited to the kind of work that's being done in solid state physics. When Professor Townes came, which must have been in 1967, he took up a good deal of the space on the top floor. They call it the fifth floor; it's floor number 5 because there is a ground floor, and then the first floor is the one above the ground floor. So he has occupied a good deal of space up there, and he also does some work at the Space Sciences Laboratory.

Lage: When you attract a professor of his notoriety [laughs] do you have to make promises regarding space?

Helmholz: Oh, yes, you do, and you usually, as I think maybe I said once about Kittel, you agree to bring some younger faculty member along who can help him in his research. And, of course, Townes has attracted a good many students and postdoctoral people to work with him. Some of his work is done through the Space Sciences Laboratory, also. That was another advantage to us because during the late sixties we got a couple of new faculty members beside Professor Townes who were interested in space science work. They were able to use the laboratories up there as well as some of the laboratories down here. When Townes came, Ray Chiao was added.

Lage: Does the Space Sciences Lab have a tie to this department, similar to the Rad Lab's?

Helmholz: Well, it's a little bit like the Rad Lab although there are not as many physics faculty involved there. It isn't as big as the Rad Lab, but it's tied to physics, chemistry, and astronomy, and I guess to some extent, to some kinds of electrical engineering. But they have a little bit different arrangement for the directors up there. The directors usually--I think in every

case--have been faculty members who just take their time off from teaching when they become the director.

Lage: A rotating assignment?

Helmholz: Yes, and they stay for, oh, maybe three years or thereabouts. The first director was named Sam Silver, who was an electrical engineer trained in physics. Since then I think there have been a couple of people in physics. First Kinsey Anderson and then Chris McKee have been director.

Lage: And it's another place where graduate students would do research, then?

Helmholz: Yes. It, I'm sure, is just as crowded as anything on campus now. It's hard in some ways to realize that even though you build new buildings and provide more research space, they're always crowded. The spaces are always crowded, and I guess it's just because--well, partly because we have more graduate students doing research and partly, I think, because faculty members, when they set out a research program, want plenty of space in which to do it and they don't want to be crowded in space. I remember before this building (new LeConte) was built, I had an office up on the top floor of old LeConte, which had a sort of a sloping roof. Then there was a room that wasn't even as big as this, and I had a graduate student doing his research in the same office.

Lage: With equipment and all?

Helmholz: Yes, the equipment was small and it took no bigger size table than this table, so that was easy. But space is always a problem, and I'm sure that Buford Price, who's now the chairman, is beginning to ask for more space.

Lage: So that's one of the chairman's roles?

Helmholz: Yes, that's one of the chairman's jobs.

Concerns about Radiation Levels and Environmental Safety

Lage: I ran across a couple of references to environmental safety; in fact, I think you were on a committee having to do with safety in the physics department.

Helmholz: Yes, I guess I have been.

Lage: Has there been a particular problem in that area?

Helmholz: Not really. Before the war we used to take radioactive samples into old LeConte from the old radiation laboratory, which is sort of where Latimer Hall is now. We didn't pay too much attention, although Ernest Lawrence was pretty good about insisting that we take reasonable care about exposure to radiation.

But after the war, people began to worry still more about radiation, particularly in the fifties and sixties. People got very much concerned about radiation.

Lage: When you say "people," do you mean physicists or the general public?

Helmholz: Well, I think the general public. I mean, the general public heard these--well, they heard the stories about what happened to people at Hiroshima getting too much radiation, and they began to think, well, if there's always a source of radiation up at the Lawrence Berkeley Lab, maybe it affects the whole city of Berkeley. So the campus administration reacted to that, and they had an office over in Cowell Hospital which monitored radiation levels. They had meters and so on. They would go around to different laboratories on the campus and see whether the radiation level in that laboratory was too great or greater than was usual for that kind of a laboratory. So we had to be fairly careful.

I think there were some rooms, particularly in old LeConte, that had to be really cleaned up and they had to take the floor out. I can remember I used to use one of those when I was a graduate student, and it wasn't the radioactivity but a lot of mercury had been spilled in the room and it gets underneath the linoleum. So the linoleum eventually had to be taken up and all the mercury got pulled out because mercury vapor is not very good for you. Then some of the experiments that were done in this building, down in the basement, involved radioactive materials. There hasn't been much use of radioactive materials in Birge Hall, but these people from radiation safety had to come every couple of months, I suppose, and test the radioactivity in the different rooms.

Lage: Was there approval of experiments that involved radioactivity?

Helmholz: I don't think so. I think that everybody who suggested an experiment knows about it. Members of the faculty wouldn't come and ask me whether they could do an experiment with such-and-

such a radioactive substance. They knew just as well as I do what the requirements were for radiation safety, and they would fix it before they started it. They knew that these people would come in and investigate the radioactivity in their laboratories so they would try to be careful. I don't remember any particular case of having to stop an experiment or anything like that; I don't think that was done.

No. This radiation safety group that had their office in Cowell Hospital had been essentially trained by people up at the Lawrence Berkeley Lab, where that work really started. Burton Moyer, who was the chairman after I was, was an expert in that field. He had trained a group up there to do the necessary testing and cleaning up, if cleaning up was needed.

There's been sort of a fight between the people who think you should get less radiation and those people who say, "Well, it doesn't make any difference anyway," and so on. I think that their safety standards are pretty reasonable now. Of course, there are people who claim, "Well, you should have much lower standards," to whom we always say, "Look at the people who live in Denver; they get twice as much radiation as we do because they're up higher." There's no particular evidence that people in Denver die of cancer or anything like that more often than we do.

So that's something, of course, that still goes on, I mean, there still are some radiation checks all the time. The arguments against the Lawrence Berkeley Laboratory have never been, to my mind, serious. There are people who claim they have made serious arguments against the Lawrence Berkeley Laboratory--

Lage: Community people?

Helmholz: Yes, but those that we could pretty well pass off as just crackpot ideas because they didn't know in any detail what they were talking about.

Lage: I've heard more complaints about the nuclear reactor over on Hearst Avenue, which has now been dismantled. Was that connected to the physics department?

Helmholz: No, it was always engineering. We were told about it, and I guess probably some of our students in times past have used radioactive materials from there. But as far as I could see, there wasn't any radiation from that either.

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Helmholz: They really objected to that reactor more after the Three Mile Island, because they thought that something similar might happen there. And I guess it's not even possible that something like that could have happened. It isn't a big reactor; it was no problem to shut it down at any time. In fact, it was shut down lots of times, so that I don't think the worry was realistic. I guess it's just really sort of fallen out of use now.

Lage: I thought it had been dismantled. But more because there wasn't--

Helmholz: Yes, more because there wasn't much use for it than for anything else. One of our students here--he must now be a professor in nuclear engineering--I know that he used materials from that for some of his experiments, but I don't think it was ever really used twenty-four hours a day.

So, let's see, what else about radiation safety? I think we've had local groups against radiation, but we haven't had any vocal groups that have really had any, you might say, scientific background in their objections. Nobody, at Lawrence Berkeley Lab, even, has been injured by radiation. I think I'm right about that. I'm sure there are some people out now who have retired up there, who now say they are injured by radiation, but--

Lage: Who may have had gotten cancer in connection with exposure?

Helmholz: Yes, they would claim that. I told you earlier about the fellow who, before the war, got his hand in the alpha-particle beam at the 60-inch cyclotron. He just happened to be in next to the target fixing up something, and the operator by mistake turned the beam on. His hand, I guess, swelled up some and got red and so on, but he's still alive and reasonably well, and so I don't think--it wasn't a big exposure.

There were a number of people at other laboratories who worked with cyclotrons in the early days, before World War II, who sustained damage to their eyes. One of them was a fellow who had worked here and went to Carnegie Tech where they were building a cyclotron, and he, I think, lost his eyesight. He may still be alive; he's about my age. At least he had a lot of problems with his vision. It was just from when they would start up these cyclotrons and didn't have much beam, or at least didn't think they had much beam. They would get in close to the cyclotron and look more or less at the level at which the particles were going around, which would be the place where the strongest radiation came out through the vacuum chamber.

Evidently that happened both to him and to two other people whom I've heard about. One of them also had been here, and he was from the University of Illinois where they had a small cyclotron. So it's not unheard of. I think Ernest Lawrence was farsighted, particularly because his brother John was here helping him, advising him on radiation problems. So he was able to avoid it.

Leadership of the Lawrence Berkeley Laboratory

Helmholz: The real objection to the Lawrence Berkeley Lab was the same objection that now is brought against the Livermore Laboratory and the Los Alamos Laboratory--that essentially they're making weapons. There used to be some secret research up at the Lawrence Berkeley Lab, and that was done away with, I think it must have been in the early sixties.

Lage: I noticed that when you had your visitors from foreign countries, they had to be cleared by the AEC to do their research at Lawrence Berkeley Lab.

Helmholz: That must have been about the early sixties. You see, Ernest Lawrence died in '58, and he'd been the director of the laboratory since the beginning, really. The beginning is really in '33 or '34. He had a very able assistant named Don Cooksey, who was a physicist also. The two of them ran the laboratory very well, so when he passed away, a new director had to be appointed. There's still some argument as to--. Clark Kerr was the President of the University, and the laboratory is really under the President. In other words, the President had to appoint the new director. The laboratory has a close connection with the campus, and I think that if the chancellor of the Berkeley campus doesn't like something that's going on at the laboratory, he can object to it, but--

Lage: But it's not actually under the campus, is it?

Helmholz: It really isn't under the campus. So that I can remember Clark Kerr called me up and there were a couple of possible candidates, one of whom was Ed McMillan. Another candidate was Luis Alvarez. If you read Luis Alvarez's autobiography, it sounds as if Clark Kerr asked him to be the director and he said no. But I don't think that's true. I think he may have called Luis and said, "Do you think Ed McMillan would be a good director?" as he did when he called me and asked me about that. It was unfortunate, in a way, that they didn't get somebody who

had not worked in the laboratory before and who would be able to come in and have the respect of both McMillan and Alvarez--and Seaborg and the others.

Lage: Instead of elevating one among the group?

Helmholz: Yes. McMillan is not a born administrator in any way, and neither would Alvarez have been. Alvarez ran a fairly good-sized group at the laboratory and did it very well, but having charge of the whole laboratory, I think that he would have had too many whims about this or that. But in any way--

Lage: Did it cause bitterness to have McMillan--

Helmholz: Yes. Alvarez was the one who mainly didn't like the way that McMillan was running the laboratory. He would say so at regular meetings that the senior staff had. But McMillan got along all right; it didn't, I think, hurt the laboratory very much.

Lage: You don't make it sound as if it helped it any.

Helmholz: No, I don't think it helped it at all. There was always some sort of bickering back and forth, and I'm sure that bothered McMillan. I don't think it bothered Alvarez.

Lage: He kind of enjoyed it?

Helmholz: Oh, yes.

Lage: Did you get a sense of Kerr's thinking about why he did choose McMillan?

Helmholz: Well, I think he probably chose McMillan because he thought McMillan would be a more stable director than Alvarez would have been. I don't think he really thought of going outside. I understand Gardner now will go outside in order to--in fact, he did go outside to get the present laboratory director, whose name is Charles Shank. But in those days there were obviously --Alvarez had not won the Nobel Prize but McMillan had at that time, so he was a natural person to appoint.

Lage: Did you get a sense when Kerr called you he was soliciting your advice, or had he made up his mind?

Helmholz: I think he probably had made up his mind and just--. Yes, I'm sure he called me because if I'd had some sort of major objection, he might well have changed his mind. I suspect that he called the dean of the College of Chemistry also, at that

time. And maybe engineering, too. But it was the natural choice, I think.

Lage: Did he appoint an administrator for McMillan?

Helmholz: No, he just let him pick his own administrators.

Lage: And what happened to Don Cooksey?

Helmholz: Don Cooksey stayed on, and he was McMillan's sort of right-hand man. They were very good friends, and Cooksey retired from that position, after another, say, five or six years. I think he was a little bit older than Ernest Lawrence. Ernest Lawrence was fifty-seven, I guess, when he died in '58.

As far as the objection to the work of the laboratory, Alvarez himself was against having secret work at the Lawrence Berkeley Laboratory, so it went fairly smoothly getting the secret work moved out to Livermore.

Lage: Was McMillan also opposed to it?

Helmholz: Yes. So that was one case in which they agreed. There weren't any, you might say, major issues on which they differed. It was always, well, how much money should Alvarez's group get compared with some other group that Alvarez didn't think was very good.

Residue of Feelings from the Oppenheimer Hearings

Lage: Was Alvarez pretty outspoken?

Helmholz: Oh, yes, he was very outspoken. He would bring these matters up at the senior staff meetings.

Lage: In the physics department?

Helmholz: Well, really at the labs. No, those things didn't get into the physics department, but since I was on the senior staff I would go to lab meetings also. So the real objection to the work--well, I should perhaps go back and say that when the Oppenheimer hearings came along in 1954 there was quite strong feeling on both sides. Ernest Lawrence, I think, was of the feeling that perhaps Oppenheimer should not be the head of this advisory committee that he was [the General Advisory Committee of the Atomic Energy Commission]. That was the one that had first really made a decision against the hydrogen bomb. Ernest

Lawrence, as I understand it, would never agree to go back to the Oppenheimer hearings and testify. He just said, "I won't, period." But Alvarez did go, and Latimer. Latimer was the other member of the faculty--he was in chemistry--who was very outspoken about Oppenheimer.

Lage: Why was he so involved in this?

Helmholz: He'd been involved with the Manhattan Project throughout the war, and I didn't realize until after the war--well, I didn't realize it until the Oppenheimer hearings came along--that he had been so much involved. But he felt that Oppenheimer was a real danger to our nuclear efforts and so on. So he testified, and Alvarez testified, and it was a real shock when the results of the hearings were over and they voted 2 to 1 to remove Oppenheimer's clearance. I was in Europe that summer, and I can remember being in Copenhagen and meeting with--I think I met either Niels Bohr or his son. His son was a person that I knew moderately well; Niels Bohr I didn't know that well, but I just met him a couple of times. He said to me, "What in the world are you thinking of over there?" to remove Oppenheimer's clearance.

Lage: I read a very informative book by Herbert York [The Advisors: Oppenheimer, Teller, and the Superbomb (San Francisco, 1976)]. He implied that they really had nothing on Oppenheimer's loyalty; it was an objection to his opinion that we shouldn't pursue the development of the H-bomb.

Helmholz: I think they tried to make it on the basis of some of his associates. I can remember when I came here as a graduate student, it was well known in the physics department that Oppenheimer had leftist leanings and so did some of his graduate students.

Lage: How was it treated at that time?

Helmholz: There was nothing wrong with that, so people just said, "Everybody has the right to his own political opinions, so let's not worry." So it was only when the war started, when we got into the war, that these concerns really took hold. There were, of course, a number of sort of tragedies, like Frank Oppenheimer, who worked through the war on the Manhattan Project, and then took a job at the University of Minnesota, where he had to say that he was not a member of the Communist Party. They found some records that he had been at one time, so he got fired.

Lage: He never really got back in the field.

Helmholz: That's right. He did a great job with the Exploratorium in San Francisco, but he never got back into cosmic ray physics, which he had gone to Minnesota to pursue.

Lage: Was there a lot of resentment here of Alvarez for his testimony against Oppenheimer? And of Latimer, for that matter?

Helmholz: I don't know about Latimer because I saw Latimer very little and I guess I never talked to the chemists about that. I think that the chemistry department--the College of Chemistry--was a little removed from the Oppenheimer case because they had not known Oppenheimer. Oppenheimer hadn't been in chemistry, and so on.

There was some hard feeling against Alvarez at that time. I think that people were tolerant enough so that they didn't in any sense try to blackball Alvarez or anything like that. But I can remember when Edward Teller started up the--well, he didn't start it up, but he was in part responsible for starting up the Livermore Laboratory as essentially a weapons laboratory--that there was some feeling against that. Not nearly as much as has been generated by [physics professor] Charlie Schwartz in the last fifteen or twenty years.

Lage: You mean feeling within the department?

Helmholz: Yes. But Edward Teller, of course, was here on the faculty for a number of years, and he was accepted as a good physicist. I think people just sort of forgot or didn't think about the fact that he would go out to Livermore and advise them on the weapons.

Lage: We're going to get into that whole Livermore question. I think that's really important for University history.

Was McMillan on a different side from Alvarez on the Oppenheimer matter? Did that have anything to do with their animosity?

Helmholz: Yes. McMillan was very pro-Oppenheimer. I think he had known Oppenheimer better, and he knew one of Oppenheimer's chief assistants, Robert Serber. McMillan knew Serber very well. Alvarez knew them perfectly well, but not nearly so well as McMillan did. So McMillan, I'm sure, was very much pro-Oppenheimer because he was, you might say, sort of prominent in the laboratory. I don't think he came out in any strong public statements about it, but I think he was as shocked as everybody else at the decision.

Lage: Alvarez says in his autobiography that Lawrence told him not to testify. He was ordered not to testify.

Helmholz: Yes, I think that's right. Why he did go back and--. I never knew whether Alvarez and Latimer were strong friends or not. I just didn't know enough about them. I mean, I knew Latimer but I didn't realize until those hearings came along that he was so much anti-Oppenheimer.

Lage: Did Latimer have a certain political bent, do you know, that might have influenced his attitude toward Oppenheimer? Was he more right wing or conservative politically?

Helmholz: I don't really know. I'm sure he was right wing. Conservative --that's almost the definition for people who are conservative. I don't really believe that Lawrence was naturally a right wing, very conservative person. He just got into this as the war developed, and he got to work with people in the nuclear field. It always seemed to me that he accreted conservatism and a right-wing tendency.

Lage: It must have hit some chord within him, though.

Helmholz: Oh, yes. Sure, I think it did. Well, you can't be from South Dakota and not have a certain amount of conservatism.

Visiting Physicists: Wolfgang Pauli and Others

Lage: Should we talk about visitors? We said we were going to talk about visiting professors--the role they played in the department, and also remembrances you might have of some famous physicists who were here.

Helmholz: The visitors we had were partly people at the Lawrence Berkeley Laboratory and partly visitors here who weren't here for very long. We always had a Wednesday afternoon department meeting, and if we heard that somebody was going to come, why, we'd try and get him to come so that he could be here on a Wednesday and give us a talk.

Lage: What about your visiting professors, though? In the University Archives I noted that you had a budget for a theoretical physicist, and you used it to bring a succession of visiting professors.

Helmholz: Yes, I think that's right. I can't even remember who the succession consisted of, but when--let's see, Serber--after Oppenheimer left, then it was Serber and Wick who were the main theoretical physicists.

Lage: On the regular staff?

Helmholz: On the regular staff. And then when Serber and Wick left, we didn't have anybody for a while, and I think it was probably then that we brought in visiting professors. I'll just have to look that up and see whether I can find the names of any of those. [In 1952-1953 J. H. D. Jensen was a visiting professor; in '53-'54 Teller was appointed; in the fall of '54, Gregor Wentzel was visiting professor.--ACH]

Lage: Do you remember Pauli's visit? That's one that you had a lot of papers about. Coming shortly before his death, and--

Helmholz: Yes.

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Helmholz: I remember Pauli because he's a striking character. The first time I remember Pauli was when he was here about 1939, I think. It must have been before World War II started. Anyway, he came and he gave a department meeting talk and talked with Oppenheimer and Serber and so on. He stayed over at a place where my mother-in-law was staying because she was here during that time, and she told us one day, "There's the strangest person; he puts his hand behind his back and walks around going like this all the time." And that was Pauli. [laughter]

Lage: He had kind of a mad scientist air about him?

Helmholz: Yes, it wasn't mad; it was just strange, I think. But yes, he came again later. I can almost remember the house that he and his wife had out on San Luis Road. That must have been in the late fifties.

Lage: I think it was '58, shortly before he died.

Helmholz: Right. But I really don't remember other people whom we had. I'm sure many visitors must have come, but I don't remember them coming for long enough to take a regular course. I think maybe we just invited them to give a few lectures.

Lage: Did Pauli teach a regular course? Or was he--

Helmholz: No, I don't think so. I think he just gave seminars. He was a sort of a--how do I say it?--a strange person, but of course very brilliant.

Lage: Were his seminars well received? Was this something that graduate students appreciated?

Helmholz: Yes, I think graduate students did. My feeling is that his seminars would have been for the advanced graduate students. I don't know what he would have been like as a lecturer in a course that most of the graduate students took as a required course. I think I would have been worried about that, that he would go over their heads.

Teller gave that course, which is called Physics 221AB, when he first came, and Teller is a great lecturer. The only trouble is, as is true with a number of people, both students and other postdocs and so on, you get sort of spellbound by the lecturer and then the next hour when you try to remember what he said, you can't remember anything. [laughs] That isn't strictly true.

Lage: Because it was such a lively lecture?

Helmholz: Yes.

Lage: As department chair, were you called upon or did you feel it was your responsibility to host some of these visitors? Entertain and look after them?

Helmholz: Yes, I did. We would often have them for dinner, particularly after one of these Wednesday talks. Betty always remembers when Otto Hahn was here. He was a German chemist who discovered fission. Let's see, I think her brother-in-law was here at the same time. Somehow or other, she had miscounted the number of people who were going to be at dinner, so we walked in to dinner and there was one less place than there were guests. It was no problem because there were something like eighty people there, and to put on another place wasn't any real problem.

Lage: But she remembers it.

Helmholz: [laughing] She remembers it, better than the people who were there.

I can remember when, at the beginning of each academic year, there would be either new lecturers or new assistant professors and so on, and Betty and I would try to either have them to our house or have them to the Faculty Club for dinner at

some early time in the semester. So that way we would get to know them and they would feel a little more comfortable in the department. In those days, in the late fifties, we had a number of people who came from the Lawrence Berkeley Laboratory and who would start out as a lecturer for one semester. We would find out how they'd been in lecturing, and then their names would be proposed for an assistant professorship. We took a number of those people, and they're now full professors.

Lage: They were known quantities.

Helmholz: Yes, we were able to find out about them.

Lage: It seems like a quite a mix of hiring a few "stars" and then a lot of people just coming up from the ranks.

Helmholz: I think that's the way the administration would like to see the department run. They'd like to see everybody hired as an assistant professor and make his way up. Of course, particularly when you're trying to start a new field of research, you want to get some star who will attract both graduate students and postdocs here, plus get the research program off to a good start. That's the reason for getting some of the stars that we have sort of managed to do.

Building Up the Physics Department at UC San Diego

Helmholz: You asked a question about the effect of other campuses on Berkeley. I think that we in Berkeley were glad to have the other campuses started up because that meant there wasn't quite the push on Berkeley to accept more and more students. In fact, of course, I think everybody here was glad to have the Regents set a limit on Berkeley enrollment, which was supposed to be 27,500 but turns out to be closer to 30,000.

Lage: So you generally thought you were big enough? There wasn't the desire to keep growing?

Helmholz: Yes. I think that the people in physics did not object when it was pointed out along in the sixties that by the time the nineties came along, we'd have a lot of retirements and then at that time we'd probably have to cut back the faculty some. In other words, just not replace them. As a matter of fact, I think it was when Geoff Chew was the chairman, he got an agreement from the dean's office that we could go over--in hiring young people, we could increase the faculty to somewhat

more than their standard figures in the College of Letters and Science would allow. And then in the coming years, in the nineties, when a lot of these faculty members who were hired in the fifties would be retiring, we'd just cut back to the number that they thought was more appropriate.

Lage: Well, that's interesting. That's looking ahead, because now we're coming upon those years.

Helmholz: Oh, yes. So we have, over the last several years, been able to hire more people than have retired. But we are having a couple retire right now and we're just replacing them, and in the next five years I think there will probably be six or eight retiring, and we'll probably hire four or five new people to replace them.

Lage: In letters in the Archives, there did seem to be a little resentment, say, of [University of California] San Diego, where the physics department was hiring top people.

Helmholz: The story was that San Diego didn't grow very rapidly because they had to wait for a new scientist to be elected to the National Academy before they would hire anyone. Yes, some people felt quite strongly about that. I'll have to say that I didn't feel as strongly as they did. I was, of course, surprised when it was announced that they were hiring those people.

The one case that I did regret was that in about '61 or '62, Clark Kerr got Herb York to become the chancellor at San Diego. He had left here as an assistant professor but he'd been promoted in absentia. He first went to the Lawrence Livermore Laboratory and ran that for a while, and then he went to the Department of Defense. I can remember that he once came back here and I talked with him for some length of time, trying to convince him that it would be a good idea if he came back here and became a professor of physics. He had had a heart attack in Washington, so I felt that was an argument that I could use, and that he wouldn't be bothered with all these Pentagon businesses and so on. It must have been some sort of unusual disease they had in Washington at that time that gave people something like a heart attack and that passed off, because I don't think he had a recurrence again. But anyway, that didn't work out, so he left and we could no longer say that he was a professor in absentia.

Lage: He took over the chancellorship at San Diego?

Helmholz: San Diego. And he was chancellor there for, oh, I think six or eight years.

Lage: Did he undergo a change in his outlook towards defense matters? He was so involved in nuclear weapons research, and then the book that I read, talking about the Oppenheimer years, seemed to be backing away from the nuclear arms race.

Helmholz: Yes, I think that since he's got into arms negotiations and things like that, he's realized that there's a lot to be said against nuclear weapons. He gave the Segrè lectureship just last Wednesday, and he gave a very good talk about the Cold War. I think he's always been a very realistic person. In other words, he's not the kind of person, oh, like, I guess Latimer would have been. You just couldn't argue with him against more and more nuclear weapons. He'll listen and give you good arguments why he thinks maybe we should have some more nuclear weapons.

His claim that he made in this talk last Wednesday was that we haven't really built up the number of nuclear weapons in the last twenty years as much as of course we did in the fifties, but that we improved the weapons and we scrapped the old ones. So that the numbers that we have now are not much different than what we had twenty years ago. So he's, as I say, a very realistic person. I never saw him often enough during those years that he was in the Defense Department to know how he would have reacted to Charlie Schwartz, for example, about that. But he's certainly a person who has thought and done a lot in the field of disarmament and in the field of nuclear weapons also.

Lage: Did he have a lot to do with building up the San Diego campus?

Helmholz: Yes, I think he did. It depends on whom you ask about that. If you ask Roger Revelle, who was the person who did a lot of recruiting for San Diego, Roger Revelle would indicate that he had done almost all the building up of the San Diego campus, but I'm sure that York did a great deal.

Lage: Is San Diego pretty well regarded in the physical sciences?

Helmholz: Yes, it is. Very much so. They got a lot of stars at the beginning, and they've kept a very good reputation and added younger people. So that it's, I would say, in the UC system, I think, in physics, at least, they're the second-ranking school.

Lage: And I have to assume that Berkeley--

Helmholz: Berkeley is first, yes.

Lage: One of the things that came up in letters that were in your papers was the suggestion that although San Diego got all these

stars, they weren't much at teaching, that the education was lacking.

Helmholz: I really don't remember having indicated that. I can see sort of where it came from. I don't really think that's true. I think they've had good people, and good people recognize that they should do a good job of teaching. If we have students who are good undergraduates, we've often suggested that they go to UC San Diego for their graduate work.

Lage: So you'd suggest they go to a different campus?

Helmholz: Yes. We don't take more than--well, now when we're taking about fifty new graduate students each year, we don't usually take more than five or six of our own graduates. So there are always another ten or fifteen who are really quite good students whom we have to recommend to somewhere else. And UC San Diego is one of the strong places. The place that it's trouble usually is in recommending the students who were just sort of average undergraduates to go to some place. Oh, we have, in the past, I know, recommended they go to Davis or to UCLA. UCLA has some good people, but I don't think they're a top-flight organization. Or the University of Oregon, the University of Washington, or places like that. Some of the midwestern universities are very good. So it's a problem, often.

Lage: A little touchy.

Helmholz: Yes, it is, because you never can be sure whether they'll fit into a place like, for example, the University of Oregon.

Lage: I have heard that in the early sixties, around the time of the expansion of the University, that faculty members were in general resentful of Kerr because they felt that Berkeley had been robbed to build up these other campuses. Now, was there any of that in the physics department?

Helmholz: I suspect there were some members of the faculty who felt that way, but I don't think it was in any sense a strong--

Lage: You yourself didn't have that feeling?

Helmholz: I didn't feel that way. We've always had very good treatment as far as getting new faculty goes.

Lage: I guess once you're a strong department, it's a lot easier.

Helmholz: Yes, it's a lot easier. Often when we've asked for two positions, why, we got only one or something like that, but that didn't seem unreasonable. Somebody's got to say no.

Charles Schwartz: Politics and Physics

Lage: You brought up Charlie Schwartz several times but we didn't really pursue that. Is this a time to talk about him as a person with strong political views? You should probably give a little background.

Helmholz: Well, Charlie Schwartz came, I think, from Stanford. He was a person who did a kind of theoretical physics which involved, oh, particularly careful calculations of this or that. He came, I'm sure, as an assistant professor and did reasonable work in research and in teaching. It really wasn't until the Free Speech Movement that he got at all into political affairs. I think at one time there were even some of us who urged him to take a more active part in Academic Senate affairs. So when he did, everybody was sort of surprised and somewhat dismayed at the way he did it. But I think the rest of the department has always been very accepting that each person can have his own opinions even if you don't agree with it.

Lage: Does it get injected into the department affairs?

Helmholz: Yes. He, for example, is very concerned that this department is white male Caucasian, and he's always urging that in the case of a new position that we've got to fill, that we try to get somebody from a minority group, and a woman, or something like that. He's been quite active in trying to alert graduate students who are getting their Ph.D.'s to the problems that they might run into if they take a job in the weapons field. He started a course which has been given a number of times, and I think still is being given, on sort of the social problems related to physics.

Lage: And that's an approved credit course?

Helmholz: Yes. It's not in any sense a required course at all, but I think it's a two-unit course and it has two lectures a week. I think he even got me to talk at one of the lectures at one time. It's a fairly reasonable course, slanted in his direction that there's no real reason for the United States to keep building new weapons. But I think that the rest of the faculty just don't pay a lot of attention to Charlie. I think they know how

he feels, and they say, "All right, well, that's all right for you, but I don't feel that way."

Lage: He's taken an active role in University selection process for the new chancellor now.

Helmholz: Yes, he's interested, but I don't think he'll have much effect on the selection of the new chancellor. He's taken an active part in this program to put a statue up somewhere commemorating the Free Speech Movement, and I must say I have sort of gone along with him in the idea that a good statue would be fine. Willy Chinowsky, who's in this department, is also in that movement. But when somebody--and I suspect it was at Charlie Schwartz's urging--was going to make it a statue in memory of the Free Speech Movement and against the Vietnam War, I sort of drew the line and said, "It's all right--the Free Speech Movement had its good characteristics and its bad, but let's not take a political stand on the Vietnam War." I guess they probably dropped that.

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Lage: What about his efforts to increase minority hiring? Does he bring forth candidates so that you can really have somebody to talk about?

Helmholz: No, I mean, I think he realizes that in the field that he has worked in, which is off the beaten track in theoretical physics, that we're not naturally going to hire somebody. I suspect that he hears about minority candidates or women candidates and will go to the committee which is going to make a recommendation to the department, and will say, "Isn't So-and-so a good candidate?" We'll find out about it when the department chairman comes up with this person as one of the candidates, and Charlie will speak up for that person. I really don't know whether he's made recommendations. This has all come about since I've been removed from choosing the new candidates. Or I guess I've been on the committee for high energy physics once or twice, but we didn't have any particular candidates that Charlie was concerned with.

Lage: Is the department polarized at all, would you say, around political issues? Or around the issues like the Lawrence Livermore Lab?

Helmholz: No, I don't think so. I think that the department really just accepts the fact that different faculty members will have different opinions about it, and we're not trying to, in any sense, coerce anybody into feeling that the Livermore Laboratory

ought to be closed or anything like that. I've forgotten now whether I mentioned that when Edward Teller wanted to start up a graduate school of engineering at Livermore, that he tried to get the Berkeley campus to accept it as an adjunct to the Berkeley campus. He finally did get the Davis campus to accept it, which was his idea. I guess it's working out moderately well; Davis has a number of students up there who do their research there.

Lage: You said earlier that Berkeley didn't accept that program.

Helmholz: Yes.

Lage: Is that because it was felt that it wouldn't have been a good quality program, or because of the political implications?

Helmholz: I think more because of the political implications. I don't think it would have been as good a program as we're used to, although we've had students who have done research out at Livermore--not very many, but I would guess over the years we've perhaps had five students who got their Ph.D.'s with work that they had done out there. But the political, let's call it backlash, would have been unfortunate. I think we're known around the campus as Charlie Schwartz's home, but I don't think people--. Maybe the students feel like Charlie Schwartz is a shining light in that conservative group of professors in physics, but I don't really believe that there's any strong opinion on the campus one way or the other. Occasionally something will come up which sort of will polarize campus views, but usually they're not important enough to make that much difference.

Lage: I'm gathering from what you're saying that having views like he had, outside the mainstream, perhaps, doesn't affect his career. Is that true? Are there attempts to pressure the department to shut him up?

Helmholz: No. I don't know of any particular attempts. There may be alumni who don't like it, and you would have to ask some of the more recent department chairmen, but I don't think there's any strong opinion.

Lage: You don't think his idea to elect the new chancellor will catch on?

Helmholz: No, I think everybody just laughed at that particular idea. I'd say that's one of the least likely of his ideas to be adopted campuswide.

Lage: Might it have an effect to make the process a little more open?

Helmholz: Yes, it certainly would make the process more open, but--

Lage: I mean, what he suggested, I know it won't come to be, but might there be a broader representation on the committee, or something, as a result of his suggestion?

Helmholz: I doubt it. You can suggest anybody you want. I can suggest somebody, or Charlie Schwartz can suggest somebody, even the students can suggest somebody if they write it into the Regents' committee. So I think what he was sort of appealing to was the idea that probably students have, that they have no way of getting their voice heard. Well, they have a student Regent now, and I'm sure Charlie was very much in favor of that. I think I probably was against it when it first came into the public relations of the Regents. But I just feel that that would bring to the Regents committee for choosing this next chancellor, it would bring a lot of undue problems into the choice, and names as possibilities that really would have no possibility of being a good choice. I'm sure you can go through the list of Hispanic public figures, most of whom wouldn't even consider the position, and most of whom wouldn't be fitted for the position, either, just to bring them in as a group or as a couple of names that just diverts attention away from what that committee of the Regents ought to be doing. So I really can't see it. And of course the idea of having the chancellor elected by one student group is really crazy also, because--

Lage: One generation of students. A brief generation only here four years.

Helmholz: Yes, a brief generation. And what basis can they have in judging?

Lage: I think that is a good place to end for today.

XII THE UNIVERSITY AND THE LIVERMORE AND LOS ALAMOS LABS

[Interview 8: December 5, 1989]##

The Department of Physics and the Livermore Lab

Lage: We were going to talk today about the relationship between the University and the Livermore and Los Alamos Labs and the various committees you've been on that examined the relationship. Would you have anything to say about the founding of the Livermore Lab in '52 and how people in the department reacted to that?

Helmholz: Well, that was a time at which there was some divergence of opinion in the department, and even at the lab--at the Lawrence Berkeley Lab. Some people thought, well, we've got enough laboratories in the United States to do work on these new bomb projects, and those were the people who were sort of anti-Teller because they knew that Teller was a strong advocate of the Lawrence Livermore Laboratory. But really, even the people who didn't know that Teller was a strong advocate--and I'm not sure from the history whether it was an idea that Lawrence had, but undoubtedly he talked to Teller about it--but even among the people who didn't know about Teller's activities, there were some who were against it, who just felt that we had Los Alamos, why not concentrate the work there?

On the other hand, there were a good many others--oh, Louis Alvarez in particular I remember, just because he was a moderately close friend of mine who was very much in favor of it and urged members of the physics department to take some active part. So there was a division of opinion. Then when Teller proposed that there be a graduate school of essentially engineering physics out there, that solidified the anti-Livermore people against having Berkeley connected with the Livermore graduate school.

Lage: Were their feelings based on moral reasons, or educational?

Helmholz: I think mostly educational. It's hard to say because people--except people like Charlie Schwartz--don't vent their moral reasons. The other people just felt that it was too far away, that Berkeley wouldn't have much control over it, that we would find that the Livermore Laboratory wanted to appoint people that we wouldn't approve of for faculty positions, and so on. So Berkeley is, I think, in one sense, provincial in that respect. In another sense, it's that feeling that has kept Berkeley sort of on the top of things. I mean, they've been very careful about appointments to faculty positions and so on, and have not let the possibility of increased facilities or anything like that get away from them.

So although I never really made a survey of the rest of the campus, such as Engineering and Chemistry, about the formation of a graduate school out there, I think that they were concerned about it probably as much as we were.

Lage: Didn't you have a committee or something to look into it? In '61, I think.

Helmholz: Yes, I think it was something like that. Up until that time, there were a few faculty members who had taken part in work out there. Alvarez, whom I mentioned before, felt that the Livermore Laboratory was a good thing to establish but he didn't push trying to get faculty members to work out there after, oh, let's say 1955 or something like that.

Lage: So it became kind of a completely separate--

Helmholz: Yes, it was completely separate. There were a few people involved in the lab. Wulf Kunkel, who was interested in fusion processes, took some part in activities out there, and I guess maybe still does. There weren't very many. I think there were a few more in engineering. There were employees out there who were good Ph.D. graduate students--one of them, I directed his thesis, was Mike Kelly. But in any event, with this committee in '61--I'd forgotten that it was that late--it was turned down, and Teller turned to Davis at that time, as we discussed a couple of weeks ago.

Lage: He must have had quite a lot of force behind him.

Helmholz: Oh, yes, he did.

Lage: One of the reviews mentioned that Davis wasn't really happy at all about the idea.

Helmholz: I think that there was a good deal of division of opinion at Davis about that. Teller didn't have quite the influence to get Berkeley to go along with it, but I think he did--. You see, he was sort of at the height of his fame at that time, being the father of the hydrogen bomb and so on.

Examining the University's Oversight Role: The Zinner Committee, 1969-1970

Helmholz: Then in 1969, when they were having student uprisings on campus, the police tear-gassed the campus. At that time, the Academic Senate Council was meeting in the Alumni House. This affected a number of them so much that they said, well, one of the things that the University could do would be to look into the Los Alamos-Livermore laboratories.

Lage: So they were radicalized by the tear-gassing incident?

Helmholz: Yes, I think that really was it.

Lage: Were you there at that meeting?

Helmholz: I wasn't at that meeting. I think I was up here, in LeConte, and the tear-gassing didn't get up to this building. We knew it was happening but weren't affected. The Alumni House did get the tear gas.

Lage: Now, is the Academic Council statewide?

Helmholz: That's the statewide senate organization, and it has about forty members, a number from each campus, and then the statewide chairman, and there's a secretary who's either from the north or the south, and I guess there's probably a vice-chairman.

Lage: So that was quite a time for that tear-gassing incident to take place.

Helmholz: Yes. It just happened to be on the same day. Anyway, that committee was appointed, and--

Lage: Now, would that have been the Zinner committee?

Helmholz: Yes, that's the Zinner committee.

Lage: It came to be called the Zinner committee. How would that have been appointed? By the statewide committee?

Helmholz: It was by the statewide chairman. I should say that, in addition to the statewide chairman, who's alternately from the north and the south, there are the chairmen of each Academic Senate on the campuses. So there was somebody from Berkeley, and somebody from Davis, and Santa Cruz, and so on. All the campuses. So I think that group probably just got together and worked out who should be on it.

Lage: I would think that the choosing of a committee of that sort would be pretty important. Do they try to balance political views, or academic disciplines?

Helmholz: They knew that this committee would have to visit both Los Alamos and Livermore. So they tried to get people on it who had some familiarity with the science that was being done there, and I think wanted just a few other people. As I remember, there were seven of us. Is that right? Can you look at the--

Lage: Yes, here's their report to the Academic Senate [Report of the Special Committee on University Research at Livermore and Los Alamos, reprinted from Notice of Meeting, Assembly of the Academic Senate, May 11, 1970].

Helmholz: I can look at it. Paul Zinner was from political science and he was a real Russian expert. I remember he told us once he thought a Russian-Chinese war had a 50 percent chance of starting within the next few years. And Thomas Allen was from chemistry; I was from physics. Addison Mueller was in law at UCLA. Frank Sooy [UC San Francisco] didn't always come, although he was from medicine so he knew something about the medical applications. Randolph Wedding was from Riverside, biochemistry, and George Wetherill was in geophysics and geology at UCLA.

Lage: Were they men who had been active in the Academic Senate?

Helmholz: Yes, they'd all been active in Academic Senate work. It certainly tends toward the sciences, with only Mueller, in law, and Zinner, in political science, being outside that. There weren't any of those people who were very much anti-Los Alamos and Livermore. So it was, I thought, a pretty well-balanced committee. We visited both Los Alamos and Livermore, and Zinner, I thought, did a very good job of writing up our discussions and formulating the recommendations.

Lage: He basically prepared the draft of the report?

Helmholz: Yes, that's right.

Lage: Did you have any particular impressions from those visits? Had you visited Los Alamos before?

Helmholz: I'd heard a good deal about the labs. I had never visited Los Alamos before. I'd been to Livermore a number of times, but I didn't have any surprising impressions. They were certainly doing good scientific work there. We didn't try to pry into any of the classified work, although we essentially had to have a partial security clearance, in order to visit there. But it just seemed to us that the University administration could take a more active part in the management of the laboratories, in the designation of what went on, even in trying to influence the declassification of a lot of the work. There was work even then and I guess there probably still is work that could well be declassified. It's just that the laboratories are being funded by the DOE [Department of Energy], which keeps thinking, "We'd better keep a lot of stuff classified." The staffs are good, I think there's no question about that; they do good work.

The question of how much should be, let's call it, weapons work, and how much should be non-weapons work, was important then, and we had the feeling that maybe the University, by trying to influence the DOE, could get more unclassified non-weapons work in the laboratories. Which would have been good because it might have influenced more faculty members to send their graduate students there if they had a special expertise there which the faculty member was interested in.

Lage: So you wanted to see a broadening of their research?

Helmholz: Yes, a broadening. For example, there was a fellow there who had been at Berkeley who was--I think because they had tremendous facilities--essentially the world's expert on weather forecasting. I can remember a couple of times in the sixties and maybe early seventies, we had him in to give a department talk about where they were in the process. It's always sort of disappointing to most of us that they hadn't made more progress in weather forecasting.

Lage: Despite this big computer.

Helmholz: Despite the big computer. But some faculty members--none, I think, in this department, but certainly other departments, might be interested in using the big computer. Some of the Lawrence Berkeley Lab people were interested in using big computers, and they would just send their work out to Livermore to be done. Livermore was, I thought, moderately generous in making it available to the Lawrence Berkeley Lab.

There had been a time, probably in the early sixties, when all the classified work at the Lawrence Berkeley Lab was sent out to Livermore and no more classified work was allowed on the hill. I think Luis Alvarez, in spite of his early interest in Livermore, was in favor of that change. And I think McMillan was too, but I think McMillan was not quite as forthright in pushing that as Alvarez.

Lage: Was that more or less an internal thing within the lab itself?

Helmholz: Yes.

Lage: Not something the University imposed on it.

Helmholz: No, that's right. That's another case in which the University administration has never tried to influence the conduct of the work at the laboratory. I'm sure that the people at the lab who were in charge, which was in those days McMillan, had to get this approved by the Regents at the statewide administration. So that was one thing which, of course, pushed more classified work out to Livermore but it wasn't a tremendous amount anyway.

Lage: And then one of the recommendations of the Zinner committee was to break the tie between the Livermore and Berkeley labs.

Helmholz: Yes. The Lawrence Berkeley Laboratory got rid of all classified work sometime in the middle sixties. There was concern expressed in student movements at the time (and even by the public) that bombs were being built at the lab. They sent all the classified work and documents to Livermore.

Well, the Zinner committee report was approved. Wetherill, who was the geophysicist from UCLA, disapproved of it. He thought that the recommendations would never be acted on, be fulfilled.

Lage: Let's just talk a little more about the recommendations in general.

Helmholz: I think that certainly the main recommendation was to have the University administration take some active part by having both administration and faculty reports on the way the laboratory was running.

Lage: It seemed to bring it more into the model of a campus, with faculty review and academic plans.

Helmholz: That's right. Well, let's see if I can--. [looks over recommendations] Page Twenty-two: "The University should exercise leadership in the determination of technical policies." That's a great statement, which is hard to fulfill. But we did go on to, "We're not blind to the political realities of Washington," and so on. I think in the selection of laboratory directors, I don't think the University has taken as active a part as they could.

Lage: Even since this report?

Helmholz: Even since this.

Benefits of the University's Involvement

Helmholz: The Zinner report keeps coming up, essentially every five years because the contract gets renewed every five years, and every five years the anti-Livermore-Los Alamos people say, "Why didn't the University just get rid of it?" I must say that even with the failure of the administration to really take any active part in this, I've always been impressed with the fact that if the University said to the DOE, "We're not going to sign the contract next time it comes up; you'll just have to go and find somebody else to run the laboratories," I think that would have meant a real falling-out of the good work that was being done at the laboratories. Because, well, one of the suggestions was, "Why not have Westinghouse or General Electric run it?" Well, Westinghouse and General Electric just don't run good research laboratories. I think almost all the employees of the laboratories would have felt, "If Westinghouse is going to run this, I'm going to get out as soon as I can."

Lage: So it's primarily the name of the University that's important.

Helmholz: Yes, that's right. And then there was a suggestion that the University of New Mexico, maybe Arizona, and maybe Colorado, run the laboratory. Apparently that would have been possible. I think they would have been glad to take it over. Actually, they're closer, of course, to Los Alamos. But they don't have the prestige, either, that the University of California does. Now, it might well be that they could have attained some prestige. They would have got some of it by taking the laboratories over, but it didn't seem to me that the work would have been as good if they had done that.

Lage: And if the University didn't renew the contract, what would be the negative aspects of that for the University?

Helmholz: Just the fee, and there is the scientific collaboration with Los Alamos and Livermore, although it's not large, and it could have been continued, I think. Probably the people in the University who wanted to continue some collaboration there would have found it a little more difficult because they would have had to deal with the University of Colorado and New Mexico and Arizona, or whoever that would be, and it means almost like getting another contract to do it. For instance, the Los Alamos Laboratory built a proton accelerator for production of mesons--sort of a meson factory, as it was called in those days. I know people who have gone down there and done research work there because that happened to be a better source for the particular experiments they had in mind than the Lawrence Berkeley Laboratory had. And I think the same thing is true of the Livermore Laboratory; there still are both engineers and physicists, and I expect there are some chemists, also, who collaborate with work out there in Livermore.

Lage: Do you think the prestige, say, of our physics department or our engineering department, would suffer if we ended the tie with Livermore and Los Alamos?

Helmholz: Not really. I think the prestige that the departments here have is pretty much from their own standing, not because of Los Alamos or Livermore. It's true that the Berkeley physics department does stand to some extent and has prestige in part because of their collaboration in the Lawrence Berkeley Lab, but not because of Livermore, and so on. I doubt that there's very much in engineering and chemistry, either.

Let's see, let me look at a couple of these other recommendations. I really haven't followed them in detail. But every five years people say, "These recommendations have not been followed." I think that the various presidents of the University have tried to do a better job at following them but have been tied up with bureaucratic red tape or have just not insisted on it. I think [President David] Gardner is now perhaps doing a little better job; he's appointed, or is appointing, a sort of a liaison man with Livermore and one with Los Alamos, who will report directly to him.

Lage: But has the faculty senate become more involved in reviewing work or appointments?

Helmholz: Not that I know of. It may have.

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Helmholz: The Senate is taking up the struggle to try to get more formal campus activity in connection with the laboratories in the review process. But it certainly is true that it hasn't gone nearly as fast as the Zinner report would indicate it should have.

Moral, Political, Pragmatic Objections

Lage: I interrupted you when you were talking about Wetherill and his dissenting view.

Helmholz: Well, Wetherill just felt that from what he could learn of the activities at the laboratory, that they would never be able or be willing to follow these guidelines.

Lage: So he thought it was unrealistic?

Helmholz: Yes.

Lage: Maybe he was right, from what you have just said.

Helmholz: Yes, I think to some extent, he was right.

Lage: Did he look at it from more of a moral perspective?

Helmholz: No, I don't really think he did. I think he had never been closely involved with the laboratories. Not as much, for example, as Allen from Davis and I had been, so I think he knew somewhat less about it and perhaps thought less highly of the scientific work. But he was, I think, pragmatic--he was more pragmatic than we were about it.

Lage: Oh, more pragmatic? That's interesting.

Helmholz: Yes, because he just saw it as a nice bunch of recommendations, but ones that would never be carried out.

Lage: You seem to have had made an attempt to get people's view about the moral-political implications of it. Do you remember those?

Helmholz: Well, yes. I think we talked about the politics, and there is a statement in there that we're not unaware of the political implications of this. The moral ones, the Cold War was pretty hot in those days, so depending on whether you believed that the

United States was following the correct nuclear policy or whether you thought that much fewer nuclear bombs were necessary, you were either in favor of the work there at Los Alamos or Livermore, or against it. So it's hard to make--it was hard in those days, at least, to make a strong moral case against the laboratories. At least I think it seemed to all of us--perhaps not as much to Wetherill--that the government was going to continue the bomb work, no matter whether the University of California was involved in it or not. So if you're going to do something, you might as well do it well, right? That was sort of our opinion.

I think nowadays, of course, you can make a very strong case--a practical case, a pragmatic case, as it were, with the decrease of the Cold War--for cutting down the laboratory bomb work and adding other kinds of work. There's always the question, too, of the personnel of the laboratories, how they would feel about perhaps having the laboratory work cut down in total, so that they'd have to find other jobs.

Lage: Could someone who's been working on the thermonuclear bomb switch to an energy development?

Helmholz: Sure, he could switch to energy development. I think the people are pretty good, so I think the real question is whether they would like to look for another job. That would depend on the individuals, I think.

I don't know what's going to happen now. I think perhaps the work will go more toward energy--use of energy, energy development, and so on. I think probably the fusion work will stay at Livermore. I don't know how much fusion work there is at Los Alamos. I guess there is some, but perhaps not a lot. But there is a good deal of non-bomb work at Los Alamos, too. Since the facilities there are so extensive, I suspect that the DOE will want to keep the laboratories in existence. It would be too bad from the point of view of facilities to just close them down.

Academic Senate Stands on the Labs

Lage: So periodically, as you say, this issue comes up. And have there been changes in the senate's feelings over the years? Was that vote on the Zinner committee report an emotional one?

Helmholz: Yes, I think it was, and we've had a couple of votes since then that have been, well, let's see, perhaps 3 to 2. I think the Zinner committee one was perhaps a little bit more like 2 to 1, but--

Lage: So the faculty has always stood behind keeping the labs.[1]

Helmholz: Yes.

Lage: Is that something that you had a role in--the voting in the Academic Senate? Is there lobbying that goes on?

Helmholz: I think each time there has--well, certainly several times--there have been lobbies, perhaps more on the Berkeley campus than other campuses, against continuing the relationship, and each time I've sort of guardedly been in favor of continuing that relationship. I guess I've expressed myself publicly a few times in that argument. So maybe something different's going to happen now.

Lage: Have your views changed over the years?

Helmholz: Not really. I've been disappointed that the administration has not taken the more active part that we recommended.

Lage: Do you see that as inertia on the part of our administration, or as resistance on the part of the lab staff?

Helmholz: Well, I think it's probably both. I think that the administration started out with a completely hands-off policy, way back in the time of the Manhattan Project. Lawrence ran the Lawrence Berkeley Laboratory, and Oppenheimer ran the Los Alamos, and Sproul, who was the president at the time, I think cared about it, but he didn't want to take an active part because he felt that he was just really unaware of all the scientific problems and felt that Oppenheimer and Lawrence were doing good jobs. And certainly in the wartime, that was very much true.

Since the University administration had never taken an active part in it, when it was recommended that they should take an active part, in some ways they didn't know how to do it. In a way, they have inched into it, but, of course, the Zinner committee report said, well, you shouldn't inch into it, you ought to just take the full step into it. And they've never

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[1]Following this interview, the statewide Academic Senate voted in favor of discontinuing the University's management of the laboratories.

done that. I think even Gardner is now in a somewhat more difficult position because these recommendations have been on the books for so many years, and I think probably the general campus feelings will be more against the bomb work now than they were in the Cold War times. And, of course, Charlie Schwartz is still as active as ever. [laughs]

Lage: He seems like a lone figure, though.

Helmholz: Not exactly. This time he has a more active group of quite distinguished faculty working with him. I told you the other day that he's got another physics department member, Willy Chinowsky, on this artwork to finance a statue for the student uprisings in the Free Speech Movement. While I agree with him that the Free Speech Movement did have a big effect on the campus, it still has enough of a sort of anti-faculty ring to it so that I, well, I won't say I hate to, but I would be willing to go along with a statue for the Free Speech Movement in memory of the Free Speech Movement, but let's keep the Vietnam War out of it, and all the other things that some of these radical people want to put into it. I mean, they want to make it a memorial to every student activity protest in the last fifty years.

Lage: And we've had a lot of those.

Lawrence Livermore and Berkeley Labs: Direction and Diversification

Lage: Any comments on the lab directors that we haven't talked about? We've talked about York and Teller.

Helmholz: Yes. I used to know, in the years after the war, Roger Batzel, who was director of Livermore for quite a number of years. I knew him moderately well because he was doing some scientific work at the lab similar to what I was doing, and I never felt that he was an outstanding administrator. It may have been that after he went out to Livermore, he became one; I just have never asked around to find out.

The Lawrence Berkeley Lab has had a number of directors since McMillan. I think they've been pretty good. I think probably the one they have now, Charles Shank, is certainly an outstanding person. Whether he's going to be that good an administrator or not, I think, still remains to be seen.

Lage: How do you evaluate that?

Helmholz: Well, you just look at what happens to the laboratory and talk around to people who are in the various divisions. The laboratory has expanded a lot, and this started in McMillan's time and has gone through the time of [Andrew] Sessler and [David] Shirley. But, you see, it used to be just a center for high energy physics, and because Seaborg had started work in nuclear chemistry, it had some nuclear chemistry. But now it's got into solid state physics and fusion physics. In some of the engineering fields there's research work being done, and the health physics has sort of expanded. So it's a very much broader laboratory now than it was in the days that Lawrence was here. I think perhaps McMillan didn't realize that he ought to push those other developments, because after the Bevatron stopped doing high energy physics, the Berkeley presence in high energy physics decreased considerably. Everybody who wanted to keep doing high energy physics had to go either to Brookhaven or Stanford or the Fermi Lab in Chicago.

Lage: For the equipment?

Helmholz: Yes, for getting the high energy particles that they needed to run the experiments. I guess, in a way, it's been too bad for Berkeley, but it had to be. We don't have the, you might say, the geographical location here to get a high energy machine. In other words, if California had got this last high energy machine which is going to be built in Texas, it would have had to have been out in Stockton somewhere, towards Stockton. One of the people at the lab, [Edward] Ed Lofgren, who had a strong part in developing the plans for the Fermi Lab accelerator in Chicago, wanted to have the machine put up near Sacramento. I'm sure it was a reasonable site, and I suspect that the reason for not putting it there was just that Chicago is a little more accessible to physicists from all over the United States. Probably that may have been partly true about Texas this time, although airplane travel has gotten so easy that I suspect it's less true than it was in the case of the Fermi Lab.

Lage: Is there space at Livermore? Was that ever suggested? Is that not large enough?

Helmholz: I think that Livermore probably has a not very good reputation among high energy physicists in general, that they would just say, "Look, if you put the high energy machine at Livermore, it's going to have a hard struggle because we're going to have to get clearance to go in and it's going to be, at least at the beginning, sort of buried in the Livermore work--both the

weapons work and then the non-weapons Livermore work. So I doubt that Livermore would ever consider it as a possibility.

Lage: You say McMillan was a little slow to diversify after Berkeley lost its state-of-the-art accelerator?

Helmholz: Yes, I think he realized, but not strongly enough, that the laboratory was not going to remain a top high energy physics laboratory, but that some of these other fields should be developed, such as the new, what they call an advanced light source, which the old 184-inch cyclotron was being converted to. That will be a very useful tool in physics. It's not high energy physics, but it's still a very interesting part of physics.

Lage: Who has been responsible for diversifying?

Helmholz: I think that perhaps Shirley has been as strong as anybody. Sessler was the director, and Sessler was a physicist who did some diversifying. Shirley was a chemist, so he had the chemist's point of view and was able to push some other things without anybody being completely surprised or objecting to it.

And the medical work has gone along. It hasn't had a really strong leader, either, so that it's probable that if either the director of the lab or the president of the University had gone out and really tried to get somebody to push the medical work, more could have been done. They do have the human genome project, which takes place at the lab now, which is going to be a big project. It's one of these forty-year projects mapping the human genes, and so on. I think it was probably Shirley who got that going.

Lage: Are most of these funded by the government?

Helmholz: Yes, almost entirely. I think everything up at the lab is government funded. The physics department or engineering or chemistry can try to get grants from private corporations for funding some of the work, but the laboratory really, you might say, belongs to the government. The Regents own the land, but the government has built everything that's up there except the 184-inch cyclotron, and that's almost disappeared now. The Rockefeller Foundation funded the 184-inch cyclotron, but before it was finished, the Manhattan Project stepped in, so everything since then has been government funded.

Lage: So that means that the federal government continues to own the equipment?

Helmholz: Yes. Fortunately, there's never been an argument about this.

Gofman's Studies of Low-Level Radiation

Lage: Anything about John Gofman? He seems like an interesting figure in this Livermore connection.

Helmholz: Gofman was a member of the medical physics faculty at the time that I was the chairman. He had been a chemist working with Seaborg in some of the work on transuranic elements and radioactivity and so on, and then he decided that he was more interested in the medical sort of applications, so he went to medical school and got an M.D. degree and then came back. He was obviously a good candidate for a faculty position in medical physics.

At the time that Ernest Lawrence was still alive, he started work on trying to isolate the chemical compounds in blood that might have something to do with heart disease--with heart problems. Ernest Lawrence was very much interested in this kind of work, and Gofman didn't push cholesterol because he thought it was a different type of compound. It was so-called lipids, I think they call them now, and he felt that they had a very strong influence in the body on the formation of plaque and the narrowing of the arteries and so on. So exactly why--and I should look this up also--why Gofman left Berkeley, it's possible that people in that same general field felt that he was too one-sided in his approach to these problems of the harmful compounds in the blood.

But anyway, then he went to Livermore and took a position out there, and where he is now, I'm not just sure.

Lage: He at some point was studying the effects of radiation on--

Helmholz: Yes. When he got to Livermore, he must have felt that he'd gone about as far as he could with the lipids in the blood and so on.

Lage: So did he leave the Berkeley faculty? Or was he at Livermore as a Berkeley faculty member?

Helmholz: I think he left the Berkeley faculty. He's no longer in the list of the Berkeley faculty. So he then took up the effects of low-level radiation, and he felt that low-level radiation, particularly over long periods of time, could be very dangerous to human health. He didn't get very much support in this sort

of campaign. This was one that he took to the national hearings on radiation safety, and so on. So I don't know what his present situation is. But people felt, I think with some justification, that the levels of permissible radiation had been pushed about as far down as was desirable at the time that Gofman was urging that it be pushed down even further. There's always an argument that sort of appeals to me that you can't find a greater incidence of the kinds of cancer that radiation might produce in the inhabitants of Denver than the same kinds of people in the Bay Area. There's a factor of two in the cosmic radiation and natural radioactivity that you get in Denver compared to the radiation that you get in both cosmic radiation and radioactivity from natural sources at sea level.

So it's true that it's an interesting scientific study, but it's a long-time study which probably you should do for fifty years. There are not many of us that start in early enough to get fifty years of experimental information. And there are, of course, not really animals that you want to keep around for fifty years, either.

Lage: So I gather you don't think low-level radiation is a real problem at Livermore.

Helmholz: I don't think that the radiation problem is a problem for Livermore. I think Gofman wanted to study it, and certainly he has his rights to study it, and Livermore is not a bad place to study it, but I don't think that Gofman in the long run has had any great effect on Livermore.

XIII ACADEMIC SENATE ACTIVITIES

[Interview 9: December 12, 1989]##

Overview of System of Faculty Governance

Lage: Today we're going to turn to the Free Speech Movement [FSM] and your observations and your role. Do you want to start with some background on the Academic Senate in general?

Helmholz: All right, well, I came as a graduate student in 1937, and it wasn't too long before I heard a good deal about the Academic Senate. And, of course, when I joined the faculty in 1940, I learned a lot more. Birge, who was the chairman of the department then, had been at the University since 1918, and he went through the period under [Benjamin Ide] Wheeler when the Academic Senate obtained a good deal of the power and influence that they still have today. I'm not sure whether the history of that period has ever been really written up, but I gather that Wheeler sort of fell in disfavor in part because during World War I he indicated some propensities toward the German position, and I guess he never really recovered from that.

Lage: In the eyes of the faculty, or of the public?

Helmholz: Yes, in the eyes of the faculty. I suppose the students, also. But the faculty then had a number of influential members--Joel Hildebrand, for example, and some man in geology whose name was Andrew Lawson. [George P.] Adams in philosophy was also influential. So they maneuvered in such a way as to get the President to allocate to the senate responsibilities for, for example, the courses of instruction and the requirements for graduation and admission standards--all that sort of thing.

But perhaps most important for the general reputation of Berkeley was the power that they obtained for the appointment of new faculty and for the advancement of faculty who are already on the staff. That power was given essentially to the Budget

Committee [Committee on Budget and Interdepartmental Relations] on the campus. And of course, in those days, the Berkeley campus was the only campus. Davis existed, but it was just sort of the agricultural arm of the University. So the Budget Committee has always been the most important of the Berkeley committees, and through Berkeley's example it's become, I think, the most important of the committees on other campuses, because they make the recommendations for promotion and for appointment.

Lage: But they don't make the final decision.

Helmholz: They don't make the decision, but the custom has grown up so that, well, now the decision is made essentially by the Chancellor for appointments to tenure positions. The Regents have to approve them, but the Regents really act pretty much as a rubber stamp on these. And the Chancellor very seldom disagrees with the Budget Committee. When he does, there is often, I think--I've never been on the Budget Committee, but I understand that they sometimes do meet with the Chancellor, who will express his concerns about particular cases. But in almost every case, the matter just goes ahead as if the decision of the Budget Committee was a final decision. This has given the faculty a very substantial voice in the conduct of the University.

I think when Sproul came to be president--. Let's see. After Wheeler, there was [David] Barrows, and then [William Wallace] Campbell, who was an astronomer. Barrows was from political science. I don't know what the historians of the University would say about that, but I think Barrows sort of acted more like a caretaker. Campbell was active, but the University faculty really didn't start to distinguish itself so much until Sproul came along. Sproul was very much versed, of course, in the faculty influence--

Lage: Even though he himself was not a faculty member.

Helmholz: Yes, it's remarkable that he seemed to have such a very good association with the faculty and to appreciate their position and not to try to step in and overrule them at times. I'm sure there were times that he wished he could, but he felt that it was up to the faculty to run the things that they had achieved during those years before he came to the presidency.

The Work of the Committee on Committees

Helmholz: So the committee system then grew up very extensively on the Berkeley campus. As you know, everybody outside of Berkeley always laughs when you say we have a committee on committees. The Committee on Committees is essentially the only committee on the Berkeley campus that is elected. There are--at least when I was on it, there were two-year terms, and half would be elected each year. I suspect that's still true, but I'm not positive.

Lage: Did the older, more established faculty, tend to be the ones who were elected to that?

Helmholz: Yes, I think so. You had to have a name that would be recognized because the faculty, I think, took a pretty active role in voting for the committee members, and as a result, you had to have a name that at least people would recognize.

Lage: Were you nominated to be on it? To run?

Helmholz: You're nominated by--at least in those days, you had to have five signators on your nomination.

Lage: So did people tend to put forth their candidacy and get signatures?

Helmholz: Yes, they did to some extent. It was always a case of the members of the faculty, when they got their ballots, they would see the name of the candidate and then they would see the name of the nominators. So, I, at least, always looked to see whether they were all from one department--because their department affiliations would be given--whether they were all from one department or whether there was a good range of them. So I know in the times that I ran, I urged whoever had offered to nominate me to try to get members from other departments. Not just physics, but perhaps English or history or somebody in the biological sciences or engineering or chemistry or something like that. I think people did look at that in deciding whom to vote for.

Lage: Were they ever hotly contested contests?

Helmholz: I don't think so. There was no campaigning at all; nobody, at least to my recollection, ever campaigned. Nobody ever gave out written statements or anything like that to the Daily Californian to try to get elected.

Lage: So it wasn't highly politicized?

Helmholz: No, it wasn't. I'm sure that in some cases, members of the nominating group would go around to their friends and say, "Look, we think So-and-so would be a good member of the Committee on Committees; please vote for him." So that was done, but as I said, there was no politicizing of the campaigns. In other words, you couldn't say that there really were campaigns at all.

Lage: Did people have a certain point of view that would be put forth, like, "We need some new blood in the Academic Senate"?

Helmholz: No, I don't think so. I suppose individuals thought that, and perhaps they talked to their friends about that, but it was always my impression, mostly from physics, that people, when they got their ballots, would look at it and perhaps they'd make up their mind right away. Or if they had questions, they would ask other faculty members, even in the physics department or in other departments, as to what those people thought of the candidates, and then mark their ballots. To my recollection, although some historian of the campus probably would know better, there really has never been any objection to the election of the Committee on Committees. I mean to the election of specific committees on committees. What the Committee on Committees does is to first try to--. Well, in my day there were five members. I guess there are seven or eight members now.

Lage: I have you on the Committee on Committees in 1954, '58-'60, '71-'72 and '77-'78. And then also on the statewide Committee on Committees.

Helmholz: At the first meeting of the Committee on Committees, they distribute the various committees to different members. In other words, I'll take the Committee on Academic Freedom and the Committee on Research and the Committee on Faculty Welfare, or something like that, and somebody else will take three others, or four others, as the case might be. You see, the people who are elected to the Committee on Committees have generally served on a number of the committees, and consequently they know enough about those to take on the job of really reporting on what the committee has done, reporting that [laughing] they haven't done anything if they happen not to have done anything, and reporting on who new members should be.

Lage: So you review the work of the committee?

Helmholz: You really do review the work of the committee, because what each member will do, then, is to go and talk to the chairman who

has been serving for the last year, and ask him, "What have you done? What have been the problems? Who's been a good member? Who's failed to show up for meetings? Who's willing to express himself, and who's quiet all the time and doesn't contribute very much?" and so on.

So then at the different meetings, you will go over the committees one by one with that particular member reporting on, first, say, the Budget Committee. The Budget Committee's always the hardest one, so you'll talk about the Budget Committee almost every meeting. The other committees will take a good deal less time.

Lage: Now, what kinds of things would you be talking about when you're talking about the Budget Committee?

Helmholz: You'd be talking about, first, how much each member has contributed. Has each member of the Budget Committee done an appropriate amount of work? And second, what does the committee chairman think of the ability of that particular member of the Budget Committee to render fair judgments, and is he biased in any way? Does he stand up for his own opinions? Does he contribute to their discussions of new faculty members and promotions and appointments? It's the same sort of thing with all the other committees; you don't go into the activities of the committee in any great detail, but you have to get a feeling for what the committee has done and who on that committee has been good and who's been fair and who's been poor.

Lage: Let me just give you an example of something I'm thinking of to see if this would come up in the Committee on Committees. With all the concern about minority hiring and hiring of women, would one of the things to be discussed about the Budget Committee be, "Are these members following the University guidelines on affirmative action hiring?" Would that kind of issue come up?

Helmholz: Yes, I'm sure that would come up. You see, there is a committee on women and minorities, I guess they call it.

Lage: But if the Budget Committee actually makes these decisions about hiring, that's where the--

Helmholz: Yes, that's where things come, and you wouldn't put a person on the Budget Committee whom you knew to be very much biased in his views of minorities and women. There's always, then, the question of who will be the chairman, for example, of the Budget Committee. Usually I think there is somebody who has served on the Budget Committee who is a logical choice for the chairman for the next year. Sometimes I think that isn't so true. But

you certainly, in almost every committee, you want to take somebody who's been on the committee before, who's served on the committee before.

Sometimes when the Committee on Committees finds that one of the committees has been inactive, you want get somebody in who will reactivate it, and in this case, then you sometimes get out the list of people who've served in previous years but who weren't in the immediately previous year. And you find somebody among that group who you know to be active and a pusher to push for activities. Then, of course, you have to go out and talk to him, and ask him whether he'd be willing to do this.

Lage: And how does that aspect work? You have to talk people into taking assignments?

Helmholz: Yes, sometimes you do. The Budget Committee is, of course, the hardest one in that because they have the most to do. You sometimes have to talk awfully hard in order to get somebody to be willing.

Lage: But they do get a reduced teaching schedule, don't they?

Helmholz: Yes, they do now. In those early days, in the fifties and sixties, I think their own department chairman would give them a light teaching load, but there wasn't anything official in those days.

So the member of the Committee on Committees first reports to the whole committee his soundings on what the committee's been doing, how the chairman has acted, how he reports about the other members of the committee. And then you look at the list and see, well, if somebody's been on a committee for, let's say, three years, it's time, usually, that he move off again. Sometimes if a person has been inactive and doesn't come to meetings and so on, why, you say, "We'll just remove him next year and appoint somebody else."

In the early days there wasn't any form which faculty members filled out indicating their interest in committee service, but nowadays there's a form which perhaps you've seen.

Lage: Yes, it tells all the functions of the committees and then asks you to show your preferences. Is there a good return on these?

Helmholz: There is a fairly good return. At least there was when I was on the Committee on Committees. But in the early days you might sometimes just have to go around and talk to department chairmen, and I'm sure that was how I got on a committee first.

Someone on the Committee on Committees--maybe Birge himself was on the Committee on Committees. I do know that Brode was very active on it, and he knew that I was interested in this sort of thing, so he just brought my name up for membership on one of the committees. But you may just have to go out and find out who in the department chairman's view would be a good committee member, and then bring up his name. Usually when you do that, you have to get a little ammunition for that person if you think he would be a good member. Some members of the faculty, or some members who have been on the Committee on Committees, know a lot of people on the campus. I really never knew, well, a lot of people. I knew a lot of names, and I knew enough people so I felt that I had fairly good judgment on reporting about new members for the different committees and who would be good chairmen and who would not be. But you have to rely on the other members of the Committee on Committees also, to be sure that you get the right people.

Lage: What kind of things did you look for, for a good chairman?

Helmholz: Somebody who was interested in the work of the committee, and somebody who was, let's call it, diligent, in meeting with the committee as a whole, in order that they would get whatever they were supposed to do done. And finally somebody who had the kind of personality who could not only get the committee members together and sort of manage the meetings, but also was able to point out the different points of view and in some way make peace among the differing points of view and come up with a good recommendation. Occasionally, the Committee on Committees would even take the reports for the last couple of years and read them over and decide, "This committee hasn't been doing much; they didn't even make an annual report last year, so let's get somebody new in who will do something."

Lage: In general, did you have a practice of rotating people off after just a few years, or did any of these become kind of captured by the--

Helmholz: No, my recollection is that three years was about the right time to move them off. Oh, yes, there were some--. Birge, I think, was on the Committee on Research for about twenty years, but that sort of activity was rather unusual, and I think almost everybody on the Committee on Committees didn't feel that that was appropriate. Oh, I'm sure there were plenty of cases in which a committee member would be on for five years, but after that you found somebody new.

Lage: Did you find any difference between, say, the fifties and the seventies, in how difficult it was to get faculty to give their time to this kind of thing?

Helmholz: No, I don't think I really did. I'm sure there was some difference. It sometimes turned out that the people whom you really wanted to get on a committee would just say, "I'm too busy. I just can't do it." That was the case in both the fifties and the seventies, but I thought people were pretty interested in committee work. I'm sure that there would be other members of the faculty who would report differently about that. Other members of the faculty who had been members of the Committee on Committees who would say, "In the sixties everybody was interested because there was so much ferment on the campus, and then they just forgot about it by the seventies." But there always, to me at least, seems to be something up and doing.

Representing the Interests of Faculty and Passing on the Tradition of Faculty Governance

Lage: In general, what did you see as the most important function of the Academic Senate?

Helmholz: I think really to represent the interests of the faculty, but that's a fairly extensive interest. For example, there's a Committee on Admissions and Enrollment, and the administrative officer for admissions and enrollment has to take the views of the faculty committee very strongly into his administration of the project.

Lage: So the officer of admissions and enrollment is responsible to the Chancellor, but at the same time he has to take the views of the faculty committee into account?

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Helmholz: Yes, and if they think one thing and he thinks another, the chairman of the Committee on Admissions and Enrollment will just go to the chairman of the Academic Senate and say, "Look, we think this, and the administrator thinks the opposite, and we don't at all agree with him," and that person, the chairman, will go to the Chancellor and say, "Look, here we've got an impasse." So those things get worked out, but they are important, I think.

Lage: Because that's one of the areas of faculty responsibility, setting admissions standards.

Helmholz: Yes, setting admissions standards. I think the Chancellors on the campus have always been pretty aware of the influence of the Academic Senate, and they've certainly been thoroughly instructed as to what they'd better pay attention to--that they shouldn't, let's say, offend the Academic Senate.

Lage: When you get a new person in without a tradition at the University like Roger Heyns [Chancellor, 1965-1971], then--

Helmholz: He had to make his way carefully.

Lage: Is this a unique setup here at the University of California?

Helmholz: Pretty much so, I think. Different universities have different policies, but I think probably at the University of California there's more faculty governance of the academic activities than there is in probably any other single university. Now, there may well be in some of the smaller colleges, but any good-sized university, there's more here. I never really tried to find out about that, but you keep hearing stories: "So-and-so was made a professor by the President because he was a good friend of the President" at some other university, and so on. Or the President or somebody on the faculty might have wanted to give a special course, so he just gave it; I mean, just put it in the catalog and gave it. Well, that wouldn't be allowed here.

Lage: Is there some way that the older members of the faculty pass on to the younger ones this kind of lore about the role of the faculty?

Helmholz: I think department chairmen really have to do that to some extent, and then, of course, by getting the younger members of the faculty to take part in the committee work, you pass it on. I can remember a few times talking to younger members of the faculty about this, and it wouldn't be as extensive as even we're talking now. But one of them or several of them have come in and just asked me about it when I was department chairman, and I've told them what I thought. And I think that there was a period there where department chairmen were asked by the Committee on Committees to suggest new members. I think that was just before this form that you referred to started being distributed.

Lage: In order to bring more people in?

Helmholz: In order to bring more people in.

Lage: So that seems to be a conscious effort.

Helmholz: Yes, it is. You see, as the campus gets larger and larger, the number of committees doesn't increase that much, and the total membership on the committees doesn't increase that much, either. So in order to find out who would be interested in serving--there are obviously some faculty members who just couldn't care less and they won't serve even when you ask them. We've had some of those in physics, also.

Lage: Isn't that considered part of their responsibility? Teaching, research, and University--

Helmholz: Yes, teaching, research, and University service, but you forgive them some failures in University service, particularly if their teaching and research are good. So that it's not everybody who either likes or is willing to do committee service. So this method of getting the names of people who are interested in it has become more of a problem for the Committee on Committees as the years have gone on and the faculty has increased in size.

The Statewide Academic Senate

Helmholz: The statewide Academic Senate, since they sort of grew out of a model of the Berkeley Academic Senate, has always had their own committees, which are usually made up of the chairmen on each campus. They have, for example, a statewide Budget Committee. In general, unless some particular chairman on the Santa Cruz campus or the Berkeley campus says, "I'll be chairman of the Budget Committee on my campus but I won't take part in statewide activities," that person automatically is a member of the statewide committee.

Lage: What do the statewide committees do?

Helmholz: They try to go over divergences between different campuses, for one thing, and then try to work out those divergences and also to communicate with the statewide administration what their views are. Let's take Admissions and Enrollment as an example --if the statewide Committee on Admissions and Enrollment is examining the admissions procedure which was used for a good many years, where each applicant for admission to the university system was asked which was his first choice campus and which were the other campuses that he'd be willing to go to if he couldn't get into that one--well, that's a statewide matter.

They can discuss that, and if they feel that that's the wrong method, then they can tell the President this and he'll take some action.

Lage: It seems like some statewide committees would be more important.

Helmholz: Yes, they certainly are. I think you could find them by just reading their annual reports. They report to the statewide Academic Senate, which is made up of a few members from each campus, somewhat in proportion to the numbers of faculty on the campus. I think the President realizes that that's a good source of information. If he wants to know something about admissions and enrollment, he'll go to the statewide committee and find out about it. So that actually the statewide Committee on Committees is not a very onerous job. Because almost all the members of the statewide committees are chosen by their individual campuses. So the only problem which does come up is the choice of the new statewide chairman--the chairman of the statewide Academic Senate.

Lage: And how is that done?

Helmholz: Well, that's done by the statewide Committee on Committees in consultation with several of the recent statewide chairmen. It alternates back and forth between the north and the south.

Lage: Is there an attempt to pick up all the various campuses at one time or another?

Helmholz: Yes. Last year, the statewide chairman was from Davis. This year he's from San Diego. I think Randy Wedding from Riverside was statewide chairman.

Lage: I think so. In his oral history [former vice president for agriculture and natural resources] Jim Kendrick, who came out of Riverside, indicated that there was a time when Riverside dominated the Academic Council because they were the ones who were interested, even though it was a small campus.

Helmholz: That's right. It may be a somewhat difficult decision to make, but I think that usually once you've made the decision, it's pretty well thought out and the person is willing to do it, so it's accepted by everybody.

Lage: Is there a high degree of civility in your meetings?

Helmholz: Yes, I think so. There are disagreements about people, but that's when you have to be a good peacemaker, to get the agreement. It's kind of fun; I mean, the statewide Committee on

Committees just has about two meetings a year, and sometimes the hardest thing is to get a secretary for it, who will serve and write up the minutes of the meetings and so on. The statewide chairman of the Academic Senate becomes a full-time job. He has a secretary who really goes with the job, and she moves back and forth from north and south. Her present name is Pat Thomas.

Lage: She moves.

Helmholz: She moves, yes. She was in Davis last year, and she's moved to San Diego this year.

Lage: That must be kind of a strain for her.

Helmholz: Yes. I don't really know her; I've talked to her a few times on the telephone, but that's about all.

Choosing Academic Senate Officers

Lage: Does the Berkeley Committee on Committees pick the chairman of the Berkeley Academic Senate? Is that the way he's chosen?

Helmholz: Yes, that's done.

Lage: So he's not elected. Or she.

Helmholz: Yes, he or she is not elected.

Lage: Why is that?

Helmholz: I think probably it's partly to avoid politicizing the office. I think the chairman doesn't always get through his term without a lot of disagreements with the Berkeley Academic Senate. For example, I've heard people grumble about So-and-so is a lousy chairman of the Berkeley division. But I think nobody's quite willing to suggest changing the way the chairman is chosen. And when you're on the Committee on Committees, you consult with former chairmen of the division and with important people on, for example, the Budget Committee and the Committee on Courses and the Committee on Educational Policy, and so on. So that it isn't as if you were doing it all on your own. You get a fair idea from those other people as to who would be a good chairman and who would not be.

Lage: At times of campus uproar or unrest, like the events of the FSM or the Vietnam protests, is there ever a move to elect, say, maybe some of the younger members of the faculty who--

Helmholz: Yes, I'm sure there have been moves in that direction, but I don't remember it ever coming up so, let's call it violently, that the Academic Senate took a vote on it. Yes, I think that would be too politicizing. It may be that the Academic Senate will come to it eventually, but I sort of doubt it. As long as the present system works all right.

It works pretty well, and I'm glad that the Regents didn't decide to let the Berkeley campus grow ad infinitum, because everybody, all the students, would naturally tend to come to Berkeley, and then Berkeley would grow to 50,000 or something like that, and the amount of Academic Senate work would increase somewhat proportionally. I think that this idea of electing more officers might come up more violently than it did.

Lage: Were you involved at all in the decentralization of the Academic Senate? The period when in the late fifties, early sixties? There seemed to be a controversy.

Helmholz: Yes, not a great deal, but--. You mean setting up an Academic Senate on each campus?

Lage: Right. Instead of the northern and southern sections. And apparently some people saw it as a loss of control or power in Berkeley.

Helmholz: Yes. Well, there was a time when there was a northern section and a southern section, and exactly when it was decided to have a statewide Academic Senate, I'm not exactly sure.

Lage: Statewide, but at the same time giving each campus control over their own affairs. Because previously the Budget Committee of the northern section, dominated by Berkeley, would pass on promotions in Davis and promotions in San Francisco.

Helmholz: That's right. And I think that in a sense Berkeley felt they were giving up something when this came along, and I'm sure Los Angeles felt they were giving up something when they let Riverside have their own Budget Committee, and the same way for Santa Barbara, and so on. I don't remember any real furor over it, but it was just a case in which once the decision was made, people accepted it and were willing to try to make it work, which I think it has, pretty well.

XIV FACULTY GOVERNANCE DURING THE TURBULENT SIXTIES AND SEVENTIES

The Committee on Academic Freedom and the Katz Case

Lage: I think we've got a good background, an overview of how the system of faculty governance works. So shall we move then to those turbulent sixties?

Helmholz: Oh, yes.

Lage: I want to ask you about the Katz case [a conflict between the Academic Senate and Chancellor Edward Strong in 1964 over the appointment of Eli Katz as assistant professor of German]. Do you want to give some general remarks on that case now? I know you have said you don't remember specifics.

Helmholz: I'll try to. The chairman of the Committee on Academic Freedom at that time was a fellow named Joe Garbarino, who's in business administration. That was a turbulent time, obviously, and he did a remarkable job of managing that, I felt. The other members of the committee were strong minded. I think probably the other members of the committee were stronger minded than I was, but--

Lage: Should I mention them, or do you remember them?

Helmholz: I remember Jacobus ten Broek was one of them.

Lage: He was in speech?

Helmholz: Yes, he was in the Department of Speech, and he was a very strong-minded person. And well respected on the campus, although he was on the liberal side of almost all issues that he got into.

Lage: He was the blind professor.

Helmholz: Yes, he was the blind professor. He lived up on Shasta Road, and he almost always had some member of the faculty who would help him. I think in those days Joe Tussman, in philosophy, for several years helped ten Broek walk around the campus and things like that when he had special things that he needed to do. Let's see, there was Garbarino, and was [Everett] Dempster on the committee then?

Lage: Yes.

Helmholz: Dempster was on the committee. Let's see, myself, ten Broek--there must have been one more, I guess.

Lage: Kenneth Stampp. From history.

Helmholz: In history, yes. Stampp was more the right side, and I think Garbarino and I tended to be on the conservative side. Ten Broek and Dempster tended to be on the liberal side.

Lage: What about Dempster? What field was he?

Helmholz: He was in genetics, and I had known him. I had never really known Garbarino at all before service on this committee. Dempster I had known. Our children went to the same nursery school up in north Berkeley. Then Stampp, I really hadn't known. I mean, I knew who he was and that he was a prominent professor of history. I think it was American history.

Lage: Civil War and Reconstruction.

Helmholz: Yes, Civil War.

Lage: You had a busy time on the Committee on Academic Freedom.

Helmholz: Yes. We had a lot of contentious matters come before us, and I think that to get some sort of a consensus was very often difficult, and that's the reason for which I particularly highly regarded Garbarino--that he managed to get a consensus without offending the two or the three on the two sides.

Lage: Do you remember why the senate was so upset about the Katz case?

Helmholz: Yes, they felt that Katz had been dropped unfairly. Let's see. As I remember, and I think it's in that article you gave me, Katz had refused to sign the Levering oath. [Oath required of all state employees, affirming loyalty to state and nation and denying membership in organizations advocating the overthrow of the government by unlawful means.]

Lage: I think he'd signed it, but Chancellor Strong had been told that perhaps he shouldn't have signed it. And he'd been dismissed from another campus in the south.

Helmholz: That's right. I'll try to look up that a little more in the future. Garbarino probably will remember it. But anyway, that was an obvious case of academic freedom. The faculty in general felt that Strong had overstepped his proper power in dismissing him. Has Strong given an oral history?

Lage: He just completed one and I looked at what was said about this. It doesn't clear it up altogether. But I think Strong's opinion was that he wasn't judging the quality of his work. In fact, that was uncertain because his dissertation is in Yiddish and nobody on this campus had read it. So that issue was kind of put aside for later. But Strong felt that Katz didn't meet the requirements for employment. When he interviewed Katz, he didn't get a yes or no answer on whether he was in fact a Communist. So Strong felt that that it was his job as chancellor; if Katz didn't meet the basic requirements for employment, which included not being a member of the Communist Party, then it wasn't necessarily up to the faculty to judge. That's Strong's side, as presented in his oral history.[1]

Helmholz: As I say, I'll just have to find out. I suspect that there was some sort of a report written by our committee on that matter.

Academic Freedom and the Free Speech Movement

Helmholz: When the Free Speech Movement came up, of course, then that involved academic freedom without any question. So we had to take the various problems that arose and try to work through them and then make recommendations to the Academic Senate about them. It was a violent time, as you can get by reading that article. Who was it that wrote that article?

Lage: This was in the California Monthly, the alumni magazine, and the author was Andy Pierovich. It's called "The Season of Discontent," in the February 1965 issue.

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[1]Edward W. Strong, Philosopher, Professor, and Berkeley Chancellor, 1961-1965, Regional Oral History Office, University of California, Berkeley, 1992.

Helmholz: We had to interview all sorts of people, from right-wingers to left-wingers, and as I think I may have mentioned before, we interviewed Mario Savio once.

Lage: Do you recall how that went?

Helmholz: He was a very unimpressive person in the interview. He could, on a podium, I guess, express himself very well, but he didn't seem impressive to a group of four or five. I think probably what he said to his colleagues was, "Oh, well, I can't talk to faculty; they're not receptive," and so on. But I think, for example, ten Broek should have been able to bring him out. Ten Broek was that kind of a person, who could get at even the mildest of people. But Savio just didn't come through in a small group like that. I think I've been told that by other people, also. But he certainly was electrifying when he got up on the podium.

Lage: It seemed like your committee brought forth one of the key resolutions to the Academic Senate in December, 1964.

Helmholz: Yes.

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Lage: One of the crucial things in your resolution was the decision to give amnesty to the students for every act prior to the passing of the resolution. Now, do you remember if that was controversial within your committee?

Helmholz: Oh, yes, we had very different opinions about it, and I think eventually it was just the idea that some sort of leniency had to be made to get over the hump that was separating the FSM and the senate and the administration completely. There was no way to say, "We're going to punish Joe Smith, here, and not Joe Dokes, there," and so on. I really can't remember how the discussions went, but I know that we did work hard and long on that matter. And I think we tried to get opinions from other people, too.

Lage: And then, of course, the Regents didn't like that amnesty proposal.

Helmholz: No. And we knew they wouldn't. And of course it was particularly hard in that particular period because you had the Regents on one side and the faculty on another side, and the administration was sort of caught in between. So you had to try to work out something which you thought the Regents might be willing to accept and which would allow some sort of

pacification with the students. I think that there was enough education lost in that fall quarter to--well, I won't say ruin the education of people, but it certainly set back the education of a good many people from that time.

Lage: Was that a concern of the faculty on the Committee for Academic Freedom at the time?

Helmholz: Yes, I think so. I think we all felt that things could be worked out. You see, I think Mario Savio, at least by his public pronouncements, just felt that the administration would have to give up everything and let the students run everything. The faculty was a little bit in between in that. So yes, I think we all hoped that some, let's call it, some pacific agreement could be reached. We knew that there would be a lot of continuing discussions on these matters. Of course, that's the trouble; the faculty being here all the time are not worried about putting something off until next quarter or two quarters from now, whereas the students wanted everything done immediately.

Lage: Part of your resolution was the point that free speech would be regulated by time, place, and manner, but those things had to be worked out, what those regulations would be. And I guess the committee continued to work on it after this resolution was passed.

Helmholz: Oh, yes, but I've forgotten a great deal about that. That's another thing that I'll talk to Joe Garbarino about. There must have been reports to the Academic Senate about that afterwards, but those were just troublesome matters that you had to work out. And obviously you had to go, to some extent, to the students and just say to them, "Look, here's what we've thought of; first, do you have any specific objections to these, and second, can you think of other things that we haven't thought of that are going to have to be addressed because of what you might do?"

Lage: And did you find the students reasonable in that way?

Helmholz: Yes, I think so. I don't remember very many conversations with students, but we certainly tried to consider everything, and I think some faculty members were anxious to bring all sorts of possible student actions to us so that we didn't lack specific cases to think of.

Lage: The other part of that committee resolution, one of the key things, was that student discipline would be taken care of by

the Academic Senate, which was a departure from the recent years, at least.

Helmholz: Again, maybe I'd better check this up with Joe Garbarino also. That was an attempt to get a little better reaction from the students. I think that the faculty wasn't interested in being policemen. On the other hand, the situation had got so bad that to get enough trust from the students so that they would be willing to go on with education rather than just having rallies was, it seemed to us, the thing that should be done.

Lage: Now, was that view that you've expressed something that you had to be brought along to as part of a compromise? You described the committee as being three conservative members and two more liberal.

Helmholz: Yes, I'm sure that had to be compromised. Again, maybe--. Well, I should try to look those things up.

Lage: [laughs] They're going to be hard to dig out again.

Faculty Leadership during FSM

Lage: Do you remember the Committee of Department Chairmen?

Helmholz: Yes.

Lage: Something that occurred to me as I was reviewing all this was how so many ad hoc faculty committees sprung up. You had the Committee of 200. You had a Committee of Fifteen. And then you had the Committee of Department Chairmen. Now, they sprung up, and then you had all your regular Academic Senate committees. And then, of course, the administration. But why the ad hoc committees? Did they feel that the Academic Senate wasn't responding well enough?

Helmholz: I think they felt that matters were not being handled, and consequently that the Committee of Department Chairmen could do something.

Lage: Did you have any communication with them? With that committee?

Helmholz: Yes, I'm sure we did, but I don't remember any in particular.

Lage: Apparently they came up with an alternative to your plan which was more acceptable to the administration. But the Academic Senate accepted your resolution.

Helmholz: Yes. I don't remember. As I remember, the Committee of 200, that was a committee more of right-wingers.

Lage: No, it was described as an ad hoc committee sympathetic to student rebels in Verne Stadtman's history [The University of California, 1868-1968 (McGraw-Hill, 1970)].

Helmholz: Yes, I think different groups of people just thought that they could do something, so they went to people they knew and said, "Let's see whether we can work something out."

Lage: Were these things welcomed by the official Academic Senate structure? Or was it seen as sort of diversionary?

Helmholz: I think both. I mean, if you have a Committee of Department Chairmen, you can't develop something which the Academic Senate would unanimously declare was foolish. On the other hand, of course, the Academic Senate is more anxious to get some consensus which some members of the Academic Senate, particularly in those days when the liberals and the right wing were really at odds--. The liberals felt that the Committee of Department Chairmen was too much a committee of the power side of the issue and would be too close to the administration. But every--well, I'm sure not everybody had a chance to speak, but a lot of people had a chance to speak, and the meetings of the Academic Senate were crowded. People did get up and say their piece.

Lage: Do you recall any particularly lively members, or people you felt were leaders in one way or the other?

Helmholz: Let's see. Well, that was when [I. Michael] Mike Heyman got into the matters.

Lage: Did he make addresses on the floor?

Helmholz: Oh, yes, he spoke on the floor of the senate. Did I tell you the story which Rod Park told? Let's see, I think it was just recently at that meeting in honor of Bob Ogg's oral history. Rod Park said at one time he had a British visitor, and the British visitor expressed an interest in seeing how the Academic Senate worked. So he took him to the Academic Senate meeting. Mike Heyman had to get up and give some points with regard to one of these matters, and the British visitor said to him, "You better watch out for him; he's dangerous!"

Lage: [laughs] That's wonderful. Well, what was your impression of him at that time?

Helmholz: Oh, I thought he was able and good. I don't remember specifically what he was recommending at that time. It's probable that it had to do with the reinstatement of students.

Lage: I think that's what he got quite involved in.

Helmholz: And he obviously knew the legal aspects of it.

Lage: So is this a time, during these times of crisis, when faculty kind of make their mark? Clark Kerr had a role in the loyalty oath that must have gained him a lot of stature.

Helmholz: Yes, that's right. Well, let's see, as well as I can remember from those times--. Well, Martin Meyerson was named the acting chancellor, and I'm sure he was active in the Academic Senate discussions in the fall. I don't remember him as having particular points of view, and I think probably the reason that he was named the chancellor was that he had a somewhat moderate point of view.

Lage: He hadn't been on the campus too long.

Helmholz: No, he was from the School of Environmental Design or architecture.

Lage: Were there a lot of strong feelings that interfered with friendships or working relationships?

Helmholz: I imagine there were. I didn't have any particular ones. The physics department, at least, was not polarized the way they were about the loyalty oath.

Lage: Oh, they weren't.

Helmholz: No. I think a lot of the physics faculty didn't care to take much part in it, so they didn't. When it came to disruption of classes, why, I think they were concerned about this, but there wasn't a lot of that in physics. Even in the seventies there wasn't much in the way of disruption of classes.

Lage: What about faculty who sided with the students and didn't meet with their classes and whatnot? Was that something you were aware of?

Helmholz: That I don't remember. There certainly wasn't more than one or two of the members of the physics faculty who did that. I think that that wasn't any problem as far as physics was concerned.

Lage: In general, would that be something the Academic Senate would be concerned about?

Helmholz: Oh, yes. That would naturally lead to an argument between, first, the department chairman and the Chancellor, and then, of course, the Academic Senate would get into that. Those were Committee on Privilege and Tenure matters. Yes, the Academic Senate was very much concerned about that. But it didn't really impact physics. I think it was more the, oh, the humanities that had a harder time in that period.

Lage: And social sciences?

Helmholz: The social sciences, yes.

Faculty Members as Administrators

Lage: It's interesting; we keep talking about the administration, the faculty, but so many of the administration are faculty members. Prominent faculty members. Now, what happens when they step into the administration? What happens to their role as a faculty member?

Helmholz: They're, I think listened to, let's call it, respectfully, by the Academic Senate, but--

Lage: But are they seen as a different creature now?

Helmholz: Yes, they are. In a sense they are. They have their own worries to think about, and, well, they have to really be sure that the campus keeps running. The faculty doesn't like it if the campus doesn't keep running. On the other hand, they're not going to worry about it. They feel that's up to the Chancellor and the Chancellor's office, and so on. The faculty doesn't particularly worry about the relations of the University to the city of Berkeley. Of course, some faculty are so much interested in it that they'll go out of their way to try to influence either the city of Berkeley or the administration, but in general they could care less, unless it happens to impinge on their own home or their street in front of them or something like that. So, yes, it isn't, I think, a bone of contention

between the administration and the faculty, that administrators have been faculty members, but I think faculty will remember it.

Lage: I'm thinking of someone like Lincoln Constance, who says he never really considered himself not to be a faculty member when he was a vice-chancellor. It was always something temporary and he was going back to his faculty position. But he still had a very different role.

Helmholz: Well, I'm sure that the Chancellor is in a different position. I think, for example, [Vice Chancellor] Rod Park now is in an unenviable position in that he has to do some things that he wouldn't do if he were still just a member of the Department of Botany. There's a good reason, I think, for choosing a faculty member for the chancellor's position, but there's also a reason to try and get somebody from outside. You can say, well, the Chancellor will not have any past history that he has to live down. If a new Chancellor comes from outside, the faculty can't come to him and say, "Look, just a year ago you did this. Now why are you deciding against that?" or something like that.

Lage: What did you think in general of Strong's position and his actions during those few months?

Helmholz: I was quite a good friend of Strong's in that we used to play tennis together and so on. I felt it was just unfortunate that he's not a, well, let's call it a dynamic person, like--well, like Heyman would have been if he'd been there in that time. Consequently, it was just unfortunate to my mind that the FSM movement came up while he was chancellor.

Lage: Was it dynamism that was the main--

Helmholz: Yes, I think it's--. He obviously was not really prepared for anything like that, and consequently didn't have--. I don't know whether he had enough consultation with the other side to understand, really to understand, what was happening. But I think it was an awful shame. I felt that in some ways, Kerr's stepping into it didn't help it at all. That meeting that Kerr had at the Greek Theatre when Savio got up at the end, that was essentially a disaster as far as I was concerned. Of course, Strong was pretty much out by that time.

Lage: I think he was in the hospital. But I think Strong feels that Kerr interfered.

Helmholz: Yes. And he has fairly good reason for thinking that. And I can understand Kerr's point of view at that time, also, and he just felt, "Look, the Berkeley campus is going down the drain

and I'd better get in there and do something." And, of course, he's got the Regents to answer to also.

Lage: Maybe we should finish up here on this note. Then we can have a reprise at the beginning of our next session with additional thoughts you might have.

More on the Katz Case##

[Interview 10: January 9, 1990]

Lage: We were going to begin today by elaborating on your remarks from last time about the Katz case.

Helmholz: Yes, I got hold of Joe Garbarino, who was the chairman of the Committee on Academic Freedom, and he said he remembered the Katz case, although not very well. Apparently it was a case that had come from the previous Committee on Academic Freedom and concerned an appointment, a sort of permanent one, for Katz, who was apparently well known to be, let's call it, a Communist sympathizer. This case was well known not only just throughout the University but to the public as well.

The Committee on Academic Freedom finally decided that we should recommend to the administration that he be appointed, and Garbarino thinks he remembers that Katz was eventually appointed, but something like a year later.

Lage: My notes show that Katz was apparently hired as an acting assistant professor of German, pending a hearing by the Committee on Privilege and Tenure. Later, he became an assistant professor, but he did not stay long. He was listed in the 1968-69 directory, but was not here by 1971-72.

Helmholz: So Katz apparently at least did not stay at Berkeley.

Lage: It's interesting that something that caused so much ire and furor at the time has sort of faded from memory. Do you or did Garbarino remember the key point for the committee in making its decision?

Helmholz: Well, it certainly was the question of Communist leanings. By that time we certainly had the principle that if anybody would sign the Levering oath, that he should certainly be allowed to be a member of the Communist Party if he wanted to. If he were

a member of the Communist Party, he didn't have to advocate the overthrow of the government by force or anything like that.

Lage: So you felt if he'd signed the oath, that that was enough, and the additional questioning from Strong was unnecessary.

Helmholz: Yes. I think that was the case. I can't remember from those times whether--. I guess maybe membership in the Communist Party was not allowed.

Lage: I don't think it was allowed.

Helmholz: He probably was not a member of the Communist Party, but as long as he'd been associated with some Communists, why, there would be members of the Board of Regents who would think this was a mistake, and so on.

Lage: Was there anything else that Professor Garbarino recalled about the time, place, and manner resolution?

Helmholz: No, he said that he thought he didn't remember anything more than I remembered.

Reflections on the Free Speech Movement

Lage: In my note to you, I asked for some general reflections about FSM and its effects on the campus. We haven't interviewed a faculty member about FSM, and so perhaps you can give us a faculty viewpoint.

Helmholz: It was certainly, as far as college education goes, it was disruptive. In other words, people weren't able to think about much else. That is, people who were interested in the way the campus was running were not able to think about much else. I think as far as courses went and so on, we tried to go on giving the courses the way we would have otherwise, but there were all these marches and meetings on the campus, and sit-down strikes and so on. Those are bound to upset the whole campus.

Let's see. I had ceased to be the chairman of the physics department, but I remember that Moyer, who was my successor, was very much concerned about it and tried to join with other campus chairmen to suggest some sort of compromises that could be made. So I just regretted the whole thing. But I don't really have any strong feeling as to what could have been done to avoid the whole matter.

Lage: What about the Academic Senate's role? So often there is not much interest in the meetings, and things go along very quietly, and here was a case where the Academic Senate became a focal point. Did it perform as well as you would hope it would in this kind of crisis?

Helmholz: Well, I think so. I think the Academic Senate took enough of an intermediate position so that the matter could be settled. Now, it certainly wasn't settled the way the campus administration under Strong hoped it would be settled. But I think Meyerson, who took over from Strong as the acting chancellor, was able to proceed pretty well without going against the Academic Senate ideas. So, yes, I think the Academic Senate did pretty well. Of course, the Academic Senate didn't do anything about it until the matter arose. In the very early days, it didn't say to Strong, "Look out, you're going to run into a lot of trouble; you'd better soften your position," and so on.

Lage: It waited until the crisis happened.

Helmholz: Yes.

Lage: Did the chairman of the Academic Senate have a strong role there? [Richard W.] Jennings was the chairman, I think.

Helmholz: I suppose he did, but I really don't know.

Lage: It seems like various faculty committees like the Committee on 200 and the committee of department chairs sort of took over a lot of the leadership.

Helmholz: They were ones that made strong recommendations, and I think Jennings probably took a fairly active part in the negotiations. I don't remember anything, you might say, sort of particular about his activities. He relied on the committees of the Academic Senate to give him advice as to what would be the best thing to do.

Lage: Some things I read pointed out the fact that there were a lot of young faculty, recently hired faculty, on the campus at the time, and that this put a different twist on things. Did you see a division between the young and the older faculty?

Helmholz: Yes, but I didn't think it was terribly pivotal. The older faculty were obviously more conservative than the younger faculty, but I didn't think that if you'd asked the Academic Senate meeting, which was in Wheeler Hall, if all the older faculty would go on one side and all the younger faculty would

go on the other side, with some age distinction, that that would have represented the yeas versus the noes. There were plenty of younger faculty who sided with the older members, the more conservative members of the faculty, and vice versa to some extent.

Lage: So it's not as simple as all that.

Helmholz: No.

Lage: Were there divisions between the social sciences versus natural sciences?

Helmholz: Well, I think to some extent there were. Again, but not strikingly; I mean not absolute differences. I mean, the School of Engineering was more conservative than physics and chemistry, and physics and chemistry were more conservative than some of the social sciences, and so on.

Lage: But exceptions in every case, most likely.

Helmholz: Yes.

Lage: We talked about faculty leaders last time, and you talked about Heyman, but do you remember any others that stood out at those meetings?

Helmholz: No, I don't. Let's see, I can remember that when the insides of Wheeler Hall were burned out, Paul Seabury from Political Science called me at something like one o'clock in the morning and said, "Have you heard what happened in Wheeler Hall?" I said, "No, I haven't." And he told me, and he was really irate about that; he felt that it was the radical --.

Lage: That was the later controversy over ethnic studies.

Helmholz: Yes, that's right.

Lage: The FSM was just the beginning.

Helmholz: Yes. To me, at least, they were somewhat connected in the sense that the ethnic studies controversy wouldn't have been as, let's call it, violent, if the FSM controversy had not been somewhat violent. I mean, people could look back and hear about the FSM problems and say, "They got something; let's us try and get something this way."

Faculty Relations with the Board of Regents

Lage: How did the faculty react when Kerr was fired? That was in 1967.

Helmholz: Yes. I think the faculty was--. As a matter of fact, Betty and I took the winter quarter off to go around the world. We had a son who was in the Peace Corps in India, so we left after Christmas, I guess, and stopped in the Midwest and then flew to London. In London, we were staying with Robert Brode, who was heading up the University of California program in England. He was the one, then, because he was the director of the program, who heard about it first. He'd been a quite strong friend of Clark Kerr and was very sympathetic with Clark Kerr in that problem, and he was really irate. I imagine that most of the faculty was pretty irate.

Lage: Even though they may have been somewhat critical of Kerr during FSM.

Helmholz: Yes. They were critical of Kerr. I think that a majority of the faculty probably felt that Kerr had not been supportive enough of Strong, and while they agreed when Heyns came that Heyns was a better chancellor than Strong would have been if he had continued, they still felt somewhat anti-Kerr for that part. But the way that Kerr was fired by the Regents was just too much for them. He was a good faculty member as far as they were concerned, and if the Regents had come by and said to the President, "You've got to fire Professor X," they would have really risen up. So the feeling was somewhat the same with Kerr. And of course the faculty started, a couple of years later, I guess, they started the Clark Kerr Award and have given that every year since then. So it was really a bad time for the Regents as far as the faculty consideration of them went.

Lage: So this was a time not just of faculty-administration conflict but faculty-Regents. There must have been a lot of concern about Regents' power.

One of the things I want to talk about, if you recall it, was the Board of Educational Development program where Eldridge Cleaver [Black Panther Party leader] was appointed as a guest lecturer. Do you remember that? That issue came before the Committee on Educational Policy while you were chair [1968].

Helmholz: That's right. I think a fair number of the faculty felt that--but not a majority of the faculty--felt that Eldridge was not a very good lecturer and that there was some reason for refusing

to let him teach. But they felt that the Regents had gone much too far in their action in that case.

Lage: I should give some background. When Eldridge Cleaver was appointed guest lecturer, the Regents stepped in with their regulation that set the standard for how many guest lecturers a professor could invite or how many times someone could appear as guest lecturer in a course.

Helmholz: I can't remember whether it was the second time that Eldridge was going to give the course, or what.

Lage: I thought it was the first, but I could be wrong.

Helmholz: There was also the case of Angela Davis. She was a controversial case at UCLA.

Lage: Then there was something the Regents interfered with having to do with Herbert Marcuse at UCSD. I think it was about retired professors, whether they could come back and teach.

Helmholz: Yes, I guess that's right. I can't remember that at all.

The Committee on Educational Policy and the School of Criminology

Lage: Let's talk about the Committee on Educational Policy itself. You were on it, let's see, '66 to '69, and '74 to '75. Now, what in general does the committee do?

Helmholz: It originally used to deal with the problems of new curricula, and with whether some curricula should be dropped, or judge something about what the educational results of new experiments or even of old experiments. It also considers whether maybe some department should be closed down. The Committee on Educational Policy got a good deal of publicity when the School of Criminology was going to be disbanded in Berkeley.

Lage: Were you on it at that time?

Helmholz: I don't think I was. I think perhaps that came after '69 and before '75. I just can't remember.

Lage: I think you are correct on that; it was in the early seventies.

Helmholz: It was done in a reasonable way as far as the privilege and tenure questions went, because people were moved from criminology into other fields. Wilson, who was the head of the School of Criminology, went to Chicago, and he was the head of the police force in Chicago for quite a number of years after.

Lage: What was the complaint about criminology? Was it a politicized department also?

Helmholz: Yes, it was too much of a professional school, not associated with, let's call it, academic disciplines. Now, I guess you could make the same argument about engineering, but engineering was a well-established field and criminology was not that well-established a field. So I think that was the principal argument for disbanding it. There was also a question, of course, of whether Berkeley should have a discipline or a major or even a school that would train people for the police force. There was a good deal of argument about that.

Lage: In a political sort of way? Or an academic?

Helmholz: Yes, well, people tried to make the argument on academic grounds to some extent. For example, there was a quite well-known professor on the campus named Paul Kirk who was originally, I think, a biochemist by training. He became interested in--what do they call it?--forensics, I guess it is, and had built up quite a reputation for his work in that field. So it was pointed out by the anti-criminology people that if students wanted to get that kind of special work, they could by taking some of Kirk's courses, and so on.

Lage: It didn't have to be a special school of criminology.

Helmholz: Yes. The decision to drop criminology was, in part, I'm sure, politically concerned, or perhaps even politically inspired, but there was also a good deal of academic argument about whether criminology was appropriate for the Berkeley campus.

Lage: Sandy Elberg, in his oral history, talked about dropping criminology, from the Graduate Division's point of view. And he felt that the school had very poor standards, that the question of whether it was a real academic discipline wasn't as important as the fact that the standards of the school were not high. Was that anything you remember?

Helmholz: Well, that certainly must have been brought out, and that was one of the arguments used when we got into the controversy over establishing an ethnic studies program.

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Helmholz: Some faculty members felt that if we did establish a Department of Native American Studies that the standards would be very poor. Of course, the proponents of the measure would say, "They'll get better as we get good students and we get better faculty members." The opponents just said, "It'll continue to be a poor department." So criminolgy may have been a similar case of concern about academic standards. Elberg is probably a much better judge of why it was dropped than I am.

Lage: He seemed to have some pretty strong memories of it. Maybe he was more closely involved in it.

Helmholz: I guess he was much more involved in it than I was.

Lage: What I get a sense of from talking to any number of people is that all these decisions go through so many different committees and entities.

Helmholz: Yes, they're pretty well, you might say, hashed over by the time the decision gets made. Well, of course, somebody's got to make the decision, and it's the Chancellor who does that.

Lage: But would the faculty vote on something like doing away with the School of Criminology? Would that be a vote of the entire Academic Senate?

Helmholz: No, I don't think so, although it could have been. I don't remember whether the Committee on Educational Policy made a report about that to the Academic Senate as a whole. Usually committees like the Committee on Educational Policy will write a report for their work of the previous year, and during that year I think it must have been an important enough decision for them to say something about it. Now, if various other members of the faculty want to take the matter up, they can get up at the meeting that that report is made to and say, "Look, we don't agree with that at all. Let's have a vote on it," or something like that. But in general I'm pretty sure that didn't happen, and I think the faculty as a whole thought that the closing down of the School of Criminology was a good thing.

Controversy over the Ethnic Studies Program

Lage: Let's look at that ethnic studies department issue. I think you were chairman when it came by your committee (1969).

Helmholz: Yes, that's right. I think that there were some members of the committee--I forget how many of us there were, probably nine or something like that--who just felt that this was a phony subject. We had a fellow from--oh, boy, he must have been from History of Music or something like that, who felt that there was just no reason for ethnic studies anyway, and so on. But most of the committee--and we had a number of 6 to 3 votes--felt that if the campus was careful about, first, whom they appointed for positions in the ethnic studies courses, and if the Academic Senate kept its eyes on the departments to make sure that they were developing in such a way that their work was good, that they were leading departments in the United States in such fields, that then there probably was enough a reason for having the ethnic studies courses. This probably would not have been a good argument, certainly not thirty years before, not before the war. Even in the short period of time following the war for ten or fifteen years, I don't think you could have made such a good argument. But of course, particularly the black problem had come up with Martin Luther King and other people during the early sixties. So we decided that there should be a program of ethnic studies. I guess it was eventually put under the direction of the Chancellor. The Chancellor agreed with it and was willing to take it on.

Lage: I read that there was a precedent for that when regional planning was begun under the Chancellor's office, but I didn't quite see the logic of it. Do you remember?

Helmholz: I think the logic of it was only--I mean, the ethnic studies advocates would have liked to have a College of Ethnic Studies, and we didn't think that was appropriate at all. There weren't enough distinguished people even to have a dean of ethnic studies. So we sort of hunted around, and I guess as you say, there probably was a precedent for having it under the Chancellor. I think nobody really believed that if ethnic studies became a real field of teaching and research, that it would stay there. The College of Letters and Science was not willing to take this on, as my recollection goes, and the ethnic studies people didn't want to be in the College of Letters and Science either. They felt they would just be sort of shucked off and would never get much money for the development of their programs and so on. So the Chancellor was about the only one that was left.

Lage: Originally when the students proposed a college of ethnic studies, they seemed to want to have control over the admission process and have a lot of involvement with community people and more student control. Were those things--

Helmholz: Those things were considered and you might say turned down.

Lage: So you wanted to keep it pretty much under the direction of the Academic Senate?

Helmholz: We wanted to keep it a good academic field.

Lage: Did your committee meet with these student leaders or any community leaders, do you recall?

Helmholz: We certainly met with some student leaders. I don't really remember meeting with any community leaders. I think we heard about it a good deal.

Lage: Did Chancellor Heyns take an active role in it? Would he come to present any proposals?

Helmholz: He didn't take an active role in our discussions and considerations of this matter. I think before we agreed to recommend that it be under Heyns, he had to agree to it. But I don't think it was originally his idea that it be under him. I think he felt that it was at least a program that should be given an opportunity.

Lage: Of course there's an awful lot of pressure. The Third World liberation movement must have outshone FSM in creating disruption on the campus.

Helmholz: Yes, I guess so. It was a pretty turbulent time. It was another case that perhaps if we had been a little more forward looking, we could have turned it away so that it was less disruptive.

Lage: Heyns has been interviewed by our office, and I looked over his interview. He makes the point, without being critical of all the processes that the University has, that it took so long to make changes, like bringing in this new program--I assume because of all the committees it had to go through, and the faculty deliberation, and whatnot--that things got to a crisis point before they could put forth a program that might have satisfied the students. Is that something that you would agree with?

Helmholz: Yes, I think that's true, the Berkeley Academic Senate process is pretty slow. I do think that if the Committee on Educational Policy had the previous year been able to forecast that some crisis like this was going to develop, we might have been able to work on it a little faster.

Lage: It seemed there were a couple of other committees looking at it too. The Committee on Courses, and I've forgotten--

Helmholz: The Committee on Courses, particularly, was looking at it. So I think Heyns is certainly correct in that matter. The Academic Senate does move very slowly and I don't know of any faculty body that really moves very swiftly either. But we've felt that some attempt should be made to answer this problem. I don't know how many members of our committee were sort of driven into this, but--

Lage: Was there a particular person who took a leadership role in it, that you recall?

Helmholz: No, I don't think so. There was a fellow named [Gerald] Mendelsohn in psychology, who was quite influential. He'd been on the Committee on Courses, I think, and he knew some of the problems that would be run into by these new departments, so to speak. But it was a turbulent time, and I personally think that it really worked out pretty well. I think we had the idea that if some of these departments did develop well enough, that the College of Letters and Science might well take them on, which did happen with the Department of Black Studies. What do they call them now, anyway?

Lage: Afro-American studies.

Helmholz: Afro-American studies, yes, that's right.

Lage: That was during your second chairmanship, when the Department of Afro-American Studies became part of L&S (1974-1975). And that was controversial, too, it seems, because that left the ethnic studies--

Helmholz: --the other ethnic studies ones in a weaker position.

Lage: And they wanted to make a college, still. And without Afro-American Studies, it seemed. Do you recall the thinking behind that?

Helmholz: It's obvious that Hispanic and Native American and--what was the--

Lage: Asian.

Helmholz: --Asian American Studies would still want a college, I think.

Lage: But do you remember why Afro-American wanted to get into L&S?

Helmholz: I think they just felt that that would give them more prestige. I think if they'd been told that there was going to be a College of Ethnic Studies, then they might have reconsidered going into the College of Letters and Science. But I think they realized, as did most of the rest of the campus, that there really wasn't much chance of getting that College of Ethnic Studies. So I guess that that's why they did it.

Lage: It seems like there was a lot of infighting between the different divisions there, in the Department of Ethnic Studies.

Helmholz: I think at that time there was. So that's the way that went. I haven't really kept track at all of what's happened, whether those other departments have become stronger and perhaps strong enough to join the College of Letters and Science, I just don't know. Of course we've had a number of changes in colleges. The College of Natural Resources [created in 1974] was sort of a union of the School of Forestry and the agriculture college.

Lage: So there are realignments taking place.

Helmholz: Yes, there are realignments, and I guess there is a realignment of the biology departments going on now. But I think that even today, my guess would be if somebody got up and said, "Why don't we have a College of Ethnic Studies?" probably the faculty would turn it down. It just doesn't seem like a natural enough division of academic studies. In other words, there are plenty of ways in which ethnicity turns up in studies--studies of literature, studies of social problems and economics and so on. Why do you have to have a special college just for that?

Lage: In a way it's sort of a segregation.

Helmholz: Yes. Nobody's proposing that there be a College of European Studies or anything like that. That's already here, of course, because most of the studies are European, and I think the ethnic studies will hopefully eventually get to that state, where they have a proper place in studies of all the ethnic groups.

Lage: In all the different departments.

Pressures to Hire Minority and Women Faculty: Physics Department Procedures

Helmholz: As you know, on the campus there's been a great demand on departments to get minority faculty. It's still going on quite strongly now.

Lage: Is this an area where the Chancellor puts maybe more pressure than usual on the departments?

Helmholz: Yes, I think so. In the old days, the way a department got new faculty was that they would first get the dean to say, "Yes, you can have one more faculty member next year." Then members of the department faculty would write around to their friends and say, "Look, who's a good student that you think would be a good faculty member?" But nowadays, of course, you have to advertise in Physics Today and several other journals, and I guess in some ways it makes it more difficult for the faculty, because they'll get a hundred applications for a new faculty position in physics, and they've got to go through the decision-making; they have to cut it down to fifty to begin with, and then to ten, then to five.

Lage: Are there procedures like there are with hiring of staff, where you have to justify why you turned each applicant down? You have to give a reason, and I think that procedure is meant to further affirmative action goals.

Helmholz: Yes, I'm sure that is. If there is one position in physics, when you get down to about ten finalists, which you can usually do fairly--well, not readily, but you can do without a tremendous argument among the group that's doing it, then you sort of try to put a woman and a Hispanic and a black in that last ten. Then from there on you probably do have to justify the choice.

Lage: How about the applicant pool in physics? Are there very many women and minority graduate students coming in?

Helmholz: No. Well, there are more women now, and there are a lot of Asians, but not very many blacks. We have one black faculty member.

Lage: Is he a newer faculty member?

Helmholz: No, he's been here for ten or fifteen years, I think. We like him very well. He complains because he says, "I don't have any time to teach and do research; I've got to be on this committee

and that committee for the Afro-Americans and another committee for ethnic studies," and so on.

Lage: So he's really called on.

Helmholz: So he's really called on a tremendous amount. But we have a number of Asian descent people. When we had Luis Alvarez, everybody thought that he must be Hispanic, and he wasn't at all.

Lage: He must have had some Hispanic heritage.

Helmholz: Well, I don't know where that name came from, but his grandfather--

Lage: He wasn't of Spanish descent, or from Spain?

Helmholz: I think maybe he wasn't of Spanish descent. But we've got another Alvarez now who, I guess, is of more Spanish descent than Luis Alvarez. So for a while we had one retired Alvarez and one non-retired Alvarez.

In physics it probably is not as difficult to make the case for a Caucasian if a Caucasian turns out to be the top person that we want to invite, just because there are a lot of well-known people in the field who will support your choice. Usually when the chairman writes and says, "I need a letter of recommendation for So-and-so," he will note that we also considered two other people, and he could give their names--"Could you or would you comment on the relative abilities of those three people?"

Lage: I see. Is this after the decision is made?

Helmholz: Yes, after the decision is made, you usually get more letters. It takes more letters to get the appointment approved by the Budget Committee.

Lage: And you ask them to comment on the second or third choices. And who do you ask?

Helmholz: Oh, you ask well-known people in the particular field of research. Physics has fields of research, and we try in faculty meetings to decide what particular field should get a new appointment. If somebody has retired from solid state physics, since this is sort of a crowded field anyway and we don't have quite as many faculty members as we might, we'd replace him with a solid state physicist. But if somebody's going to retire in high energy physics, the argument can well be made that there

are enough high energy physics faculty now, so wouldn't it be a good idea to get somebody in astrophysics or something like that? So then the faculty will make a decision on this, and the notice that a position is available will go out saying that the University of California at Berkeley is accepting applications for a faculty position specializing in astrophysics. Now, sometimes the letter goes out saying just in physics. But other times it may go out giving the particular specialty. Then when you write letters for the faculty appointment to present to the Budget Committee, you write to the well-known people in that field.

Lage: And hopefully they will know of your candidate.

Helmholz: Yes, they'll know about the candidates.

Lage: Do you have any knowledge of how the recruitment or appointment of minority professors goes on in other departments?

Helmholz: No, I really don't. I think you'd have to ask department chairmen. I suppose it's a little bit the way we do it; I mean, we look for a black candidate, and this fellow whose name is Harry Morrison was out at Livermore. We did have two or three black candidates. At the time, we decided we probably should get a black faculty member. We now have two women faculty members. The first one came as a senior professor, and she had been in Europe and was quite well known in theoretical physics.

Lage: What's her name?

Helmholz: Mary Gaillard is her name.

Lage: She is European?

Helmholz: No, she's an American by birth, but she'd been in Europe at Cern in Geneva, Switzerland, and had married a Frenchman over there. They've been divorced since then, but--

Lage: Was that an effort to get a woman, or was she the most highly qualified?

Helmholz: Yes, I think that--. There were men candidates at the same time, and--

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Helmholz: If two candidates, one a man and one a woman, were equal, judged by everybody to be equal, I think the woman would be appointed. That rarely would occur, but it might, and so on. So we haven't

really considered the case of Hispanics, well, partly just because we have an Alvarez. We had two Alvarezes until Luis died. We thought we were proof against that. [laughs] But certainly when Asians have been appointed--or people of Asian descent--they're considered in the same light with Caucasians.

Lage: You don't give any special consideration?

Helmholz: We don't give any special, and of course we've got enough Asians as it is to avoid any criticism on that score.

Lage: So it sounds as if you had a lot of good candidates that are Asian.

Helmholz: Yes, we usually do have good candidates who are Asian. So it's really the blacks and the women whom you might say we've had a harder time getting.

Lage: Is it hard for women to come through here as graduate students? Is there sort of a male culture that's hard to buck?

Helmholz: I don't think so.

Lage: I should ask that of one of the women.

Helmholz: Yes, that's right. You should. We've always had a small percentage of graduate students that are women, and some of them have been pretty good. None of them until this one we have now, whose name is Marjorie Olmstead, have been really I think good enough to consider for a faculty position.

Lage: She was a Berkeley graduate student?

Helmholz: She was a Berkeley graduate student, got her Ph.D. here and then went to work for, I think it was the Xerox Company, for a number of years and did some very good work there. So it was decided to invite her back.

Lage: Do you know why she went to Xerox instead of to another university?

Helmholz: I don't have the vaguest idea. No. I think probably it was that the research at Xerox she knew about, and it was quite close to what she had done here for her thesis research. There was an excellent physicist there who was running the research division in Palo Alto, so if she'd gone to a university and he had been there, why, she might well have gone there. But I really don't know; I mean, I'm just suggesting that.

I suppose that when Harry Morrison retires, which I guess will be sometime in the next ten years, that the question will come up, "Are there any good black candidates?" I don't remember, but I don't go to faculty meetings very often anymore, so I don't know whether there have been any strong black candidates in the last, say, ten or fifteen years, for a faculty position.

Lage: It seems like a long time between candidates.

Helmholz: Yes, it is, and we think--and I know that the Chancellor would have to probably disagree with us to some extent--but we think that the quality of the candidate is, for physics, at least, the best means of judging. We try to make it so that both the research and the teaching are the important features. You can't judge the teaching as well as you can the research. In physics, at least, you rarely will hire a fresh Ph.D. Usually he's had a couple of years as a postdoc somewhere, and consequently he will have a fair number of publications from which you can determine his research abilities. Now, you say, "How do you judge the teaching?" Well, you judge the teaching partly by his talks--faculty members will hear him talk at meetings and so on. We often try to get a prominent candidate to come and give a Wednesday afternoon talk here.

Lage: Would this be a candidate for a assistant professorship as well as a more advanced position?

Helmholz: Yes. I think it's probably true that every candidate we've hired in the last five or ten years has given a talk to which the faculty members are invited to go and are even reminded that they ought to go by the department chairman. He will just say, "So-and-so is going to give a talk next week," and it may be the Wednesday afternoon talk, which a lot of the faculty go to, or it may a special seminar at which particularly the faculty in that field will go. From a talk like that, you can get a fair idea as to what kind of a teacher he would be.

Lage: Seems like a pretty good way to look someone over. It must be very nerve-wracking for the candidate.

Helmholz: Well, I think it is. Hopefully all the candidates we have, have a good enough reputation so that if they're interested in an academic job, they'll get a reasonably good one.

A Relaxation of Academic Standards?

Lage: Let's see if there are any other general things about this turbulent time period. One question that came up in the Elberg interview was whether these years saw a relaxation of academic standards. I wouldn't expect that in the physics department, but did you sense that on the campus as a whole in your role on the Committee on Educational Policy? Was that a problem?

Helmholz: I don't really know. I don't think we were aware of it. I'm sure the matter came up. For example, there were times in the ethnic studies debate, and so on, that students would ask a faculty member to call off classes for a week and just meet with the students and talk about ethnic studies rather than what he was supposed to be talking about.

Lage: Did the senate take a role in making judgments about this?

Helmholz: I think that when the cases came up of a faculty member who might agree to this, that somebody in the senate--probably department chairmen--would speak to the faculty member and say, "Look, your job is to give the course, not to educate the students in ethnic studies," and so on.

Lage: Were grades relaxed? The standards for giving grades, do you recall? Or was that a subject of discussion?

Helmholz: I don't think there was much discussion of that. I'm sure that the department chairmen had to look at the grades to see whether there apparently was some easing up on the standards. I think that it would have been the Committee on Courses which would have looked at the average grades during that period of time. And I expect they did, but I don't remember the result. But in physics, or in the sciences, anyway, I think it was easy enough to--if a student stood up in class and said, "We don't want to hear you lecture on physics; we want to talk about ethnic studies," you could just say to him, "Look, this is a class in physics or chemistry or mathematics. If you want to talk about ethnic studies, come to my office hour." I've done that a couple of times myself.

Lage: So that this has happened at some of your classes?

Helmholz: Yes, it's happened in a couple of my classes at that time. Fortunately for me, the rest of the class clapped when I said that. I'm sure that in some of the social sciences and so on, that it must have been quite different, and I'm sure the faculty had a difficult time in keeping the class on its subject.

A Medical School at Berkeley?

Lage: Are there any other issues? The program in health and medical sciences, was that something that you had a role in?

Helmholz: As you may know from, I suppose, Elberg and others, there was a time right after the war when the question of the medical school was prominent in University discussions, and whether to move the San Francisco medical school over here to this side or whether to essentially enable faculty members over here who wanted to teach in a medical school to go over there. That was a quite prominent discussion. I think the Berkeley campus was in favor of leaving the medical school over there. I'm sure there were plenty of campus faculty who individually would have liked to see the medical school over here.

Lage: But there were a lot who didn't want it?

Helmholz: But there were a lot who didn't want it, and I think that if you'd taken a vote in the Academic Senate, which I'm pretty sure was never taken, it would have been in favor of keeping the medical school in San Francisco and not having a medical school here. There was then, later on, when the shortage of doctors got to be a national problem, the suggestion that we do set up a medical school here.

Lage: In addition to San Francisco.

Helmholz: Yes, in addition to San Francisco. As a sort of a compromise, I guess, to try that idea out, this program in medical sciences was instituted. I've forgotten when it was [1970s] and when it was really dropped, but it was to enable students to take two years of work in Berkeley and I guess they then would have to go over to San Francisco or elsewhere, because Berkeley didn't have all the facilities for the clinical training that would be usually given in San Francisco. The idea was, among people who had thought about this--and I guess Elberg must have been one of them--that perhaps some sort of a general practitioner would be particularly developed in Berkeley.

Lage: Someone with a little more social consciousness was the idea, I gather.

Helmholz: Yes, and somebody who didn't specialize in neurosurgery or something like that. And those people, then, of course, one of

the other questions is, "Where are they going to go after those two years?" Well, I guess they had made arrangements with San Francisco to take those students if the students didn't want to go somewhere else or couldn't get in somewhere else. I think there were some people on the campus who said, "Oh, boy, now we're going to get a medical school here, and I don't like it."

Lage: What would be the objection to having a medical school?

Helmholz: Well, the trouble with the medical school is really that the size of a hospital, which you really ought to have quite close to the campus, is a problem both for the rest of the campus and for the kinds of social milieu that exist at the campus. I think most people on the campus felt that while the UCLA medical school, which is right on the campus, has been a pretty successful school, that the rest of the campus has suffered a little bit from having it. In other words, it just gets to be too big a campus. My own feeling was that it wasn't a good idea.

Lage: Wasn't there some feeling about faculty salaries in all that too?

Helmholz: Oh, yes. That's a problem which I'm sure UCLA has, and that is the question of how you put some sort of regulation on the salaries of the M.D.'s who are part-time faculty and part-time in private practice. The Regents have some rules about that now. I know that this is a problem that always comes up, but exactly what their present regulations are, I'm not just sure.

Lage: It doesn't look like it's in the future of this campus, though?

Helmholz: No, I don't think so. I did hear Clark Kerr say once--. I had a friend from Scotland who was the vice-chancellor of the University of Glasgow, Scotland, and he came over here one time, and since I had known him well, he said, "I'd like to talk to Clark Kerr." So we went out to Clark Kerr's house, and Clark said, "If you don't have a medical school, don't ever get one." [laughter] This was the time that the University had just agreed to have medical schools at Davis, Irvine, and San Diego in addition to UCLA and San Francisco. I think Kerr's point was that it's just too darn expensive. Just don't do it.

Carl and Betty Helmholz, August 1989.

XV RETIREMENT ACTIVITIES

[Interview 11: January 16, 1990]##

The Berkeley Emeriti Association and Emeriti Concerns

Lage: We're going to start today with emeriti affairs. You served on the statewide faculty welfare committee as an emeriti representative.

Helmholz: Yes. But let me start out with the Berkeley Emeriti Association. When I retired in 1980, there had been a fair amount of talk on the campus about forming an emeriti association. There was a fellow in public relations, Don Foley, who was interested in this, and when a number of us who'd been concerned with emeriti affairs suggested that he become the president at the first meeting, which was, I think, in May of 1981, he sort of agreed. Then his son got sick, and they convinced me that I ought to become the president. So it was agreed by all those present at the first meeting that we should have an association. I think Dick Jennings in Law drew up a constitution and by-laws which were approved.

Lage: Did it have a specific purpose in the beginning?

Helmholz: Just to associate the emeriti, to bring together emeriti for their own benefit and for the benefit of the campus. So I was the president, and I actually stayed the president for two years. What we did was to have luncheon meetings on Saturdays at the Faculty Club with speakers, either from the faculty or from outside the faculty, on topics that we thought might be of general interest to the emeriti.

Lage: Now, what kinds of topics would they have been?

Helmholz: We had some by retired members of the faculty on political science or economics--I forget whether we had Luis Alvarez talk about dinosaurs or not.

Lage: But general topics of intellectual interest, not topics of retirement?

Helmholz: That's right. I remember we had one fellow who'd been in the President's office twenty years before, who talked about business and stocks and bonds and so on. So those were quite successful, I think. We had about a hundred members at the beginning, active members, that is, who paid dues. Dues were $5 a year and they still are, as a matter of fact. We would get, oh, seventy-five to a hundred at these Saturday meetings, and then every May we had a dinner meeting. I think we probably had them on Friday or Saturday evenings, and we'd have somebody who was a little more special speak at that.

Lage: Would spouses be included?

Helmholz: Spouses were always included, and we invited the spouses of deceased faculty members also. I think we had Clark Kerr come; After I was president I remember Clark Kerr spoke at one of the dinner meetings. In general, they were quite interesting, and the members who came seemed to like them, because the association has kept on in more or less the same way.

Lage: In general, do most emeriti professors keep a strong link to the campus, would you say?

Helmholz: Yes, I think probably more than half of the retired professors do keep some kind of a link. The question of office space or research or lab space is a sort of a tricky one, because some departments just don't have any space, and even though a faculty member might want to continue with his scholarly work, he doesn't have any place to do it.

Lage: That must be very difficult.

Helmholz: It's particularly true of smaller departments, because, say if a department has five members and one of them retires, they're going to get a fifth man or woman to replace him. Well, what do you do about offices? A five-man department doesn't have any more than five offices, usually, so there's no place for him to sit and do scholarly work.

Lage: Did the association deal with that kind of problem?

Helmholz: Well, we really didn't. One of the reasons that I got into this was that in the last couple of years before I retired I'd been a member of an Academic Senate committee called the Berkeley

Faculty Emeriti Relations Committee. I also kept being a member of that for several years after I retired.

We tried to take a survey to find out what different departments did about this problem. Some departments had office space, such as physics. I've always kept an office, or at least half an office, in the physics department. Others weren't able to provide office space. All of them said that in the late eighties and in the nineties it was going to be a terrible problem, which it's turned out to be. I think that there are just more and more faculty retiring, and more and more faculty to replace them. Since there are more of them, then that increases the numbers that are interested in keeping up some sort of scholarly work.

The administration has not really done very much. It has done something. In 1981, when we started the emeriti association, we asked the Chancellor to provide us with some sort of a room to which emeriti could go and meet if they wanted to. But since the room that we finally got was in one of these temporary buildings over here, emeriti association members didn't use it very much. But one of the members, Darrell Amyx, who had been ousted from his department and still wanted to keep up his scholarly work, used it as essentially just an office, so that he was the only one who really made use of that office. Well, we finally gave it up again. There was no place in the Faculty Club that we could call our own.

Lage: They don't have spare rooms in there that could be set aside?

Helmholz: No. They had the rooms where we met for luncheon and dinner meetings, but that was all. Well, just in the last year, after the formation of CUCEA, the Council of the University of California Emeriti Associations, we have got the Chancellor to provide some--I think it will be one or two--rooms in University Hall. When those will actually become more available, I don't know, because they're supposed to be getting some furniture for them now, and I understand there are earthquake or seismic activities that they have to take care of to strengthen the building. The President is supposed to have said that as long as he was there, they weren't going to do anything about seismic safety. He had a big desk on the top floor and he was just going to ride it down as it fell down through the building. [laughter]

Lage: But he moved before the earthquake.

Helmholz: He moved before the earthquake, yes. And apparently it came through the earthquake without much damage at all. So that was a step forward.

Lage: Will that building be used for the association, or for individual emeriti professors to work?

Helmholz: No, that's for the association. The association will have an office there, and they will share a half-time secretary with the retirees' association, the Staff Retiree Association. I think the Staff Retiree Association seems to have more interest in association activities. What they're going to be, I'm not exactly sure.

The Berkeley association has never been very active in trying to get better counseling for emeriti before they retire. Some faculty members have felt that their counseling before they retired was perfectly satisfactory. Others have felt it was terrible; they just didn't get any satisfaction from the retirement counseling in office in Personnel.

Lage: Would this be financial counseling?

Helmholz: Mostly, yes. Financial counseling, health insurance, whatever other kinds of things that people are concerned about when they retire. More attention has been paid to it on other campuses, I think. They probably have done a better job of getting that financial counseling. I was talking with somebody the other day who said that he had been on phased retirement, and evidently the office over there doesn't know what to do about advising people on phased retirement.

Lage: Is that when you cut back on hours?

Helmholz: Yes, you cut back on hours over several years, two or three years.

Statewide Emeriti Affairs--CUCEA, University Committee on Faculty Welfare

Helmholz: So then in, it was about '84 or '85, mostly through the advocacy of the UCLA people--. UCLA has always had an active emeriti group, and when [Franklin] Murphy was the chancellor down there, he put aside some space in one of the buildings for emeriti to have small meetings and also to have a half-time secretary. That's always been a woman there named Marian Broome [Director,

Emeriti Center, UCLA], I think, is her name. She's been a very great help to their emeriti association. Well, partly through their interest in getting other emeriti associations going and getting some statewide organization, and at the suggestion of, I think it was an Academic Senate committee made up of Claude Fawcett and a fellow from Berkeley, this same Darrell Amyx whom I mentioned, and a woman from Santa Cruz named [Peggy] Musgrave, they issued the so-called Musgrave Report, which sets out things that emeriti could do for campuses and what the campuses might do for the emeriti. That suggested a statewide Academic Senate committee made up just of emeriti.

This was not accepted--that recommendation was not accepted by the statewide Academic Senate. Instead, they said, "We'll appoint two emeriti, one from the north and one from the south, to the statewide University Committee on Faculty Welfare. When that was done, that sort of eased the mind of the statewide Academic Senate, and they did appoint them, and those members still exist. I was one of the members for a couple of years; it must have been something like '86-'87 and '87-'88. There was one from the south also. We went to the statewide faculty welfare meetings.

The statewide committee consists of the chairmen of the Faculty Welfare Committee on each of the campuses, and a chairman. The chairman, oh, for example, was--I've forgotten. The chairman is alternately from the north and the south, and the first year I was on it, Professor Barton, Babette Barton from law, was the chairman. The second year, it was a fellow from Riverside whose name is I. J. Thomason. He was an expert in nematodes. He retired just a year and a half ago, I guess. Yes, it was just about a year and a half ago, and they had a big conference at Riverside on nematology, as they call it.

Anyway, the emeriti affairs were paid attention to in this committee, and they made some recommendations to the administration about emeriti benefits.

Lage: In thinking of financial benefits, or things like offices?

Helmholz: No, I think they made comments about offices, but it was mostly changes in the retirement system which would benefit the people who were emeriti. For example--and I'm sure there's a lot about this in the early discussions that I gave on UCRS--they finally got the limit of 80 percent of your retiring salary or highest average three-year salary, finally got that done away with. The Regents finally agreed to accept 100 percent, and there's a law now which says that you can't get more than 100 percent. It's a federal law. So that was finally adopted, and one of the

present arguments is, should it be made retroactive to people who've retired before the passage? This was only last July 1, and the administration has turned this down. But some present members of the emeriti associations are going to keep pushing for this to be made retroactive. It wouldn't cost the Regents--the retirement system--very much, because people sort of retired when they'd reached the 80 percent limit anyway. But they claim they don't have the records. Well, I'm sure they do have the records if they'd just look for them. [laughs] But that's one of the things that went through the statewide committee on faculty welfare and was approved.

The question about voting rights of emeriti in department meetings has been a sort of a sticky issue, and there are several members on the Berkeley campus who were interested in this.

Lage: Do they not have voting rights now?

Helmholz: They don't have voting rights on personnel issues unless the department itself wants to give emeriti permission to vote on personnel issues. I think it was in the Regents' definition of emeriti that they continued to be members of the Academic Senate, and it was claimed by the emeriti that departments are just a part of the Academic Senate, are really committees or divisions of the Academic Senate. So if you're going to be a member of the Academic Senate, you're obviously a member of any committee of the Academic Senate that you belong to. So I guess that was finally agreed to by the administration.

But there is a specific Regents' rule that emeriti shall not be able to vote on academic personnel matters. So that's perfectly all right with most emeriti, I think, but the Regents' rule does say that if a department wants to have emeriti vote on personnel matters, that they can by, oh, a two-thirds majority, so decide. That has been accepted by emeriti as well. The question of whether emeriti want to come and vote on academic personnel matters is not, to me at least, a very important matter. I occasionally go to department meetings of faculty, but I don't care to vote.

Lage: Maybe in a smaller department someone might feel emeriti could really play a role.

Helmholz: Sure. Now that emeriti are becoming longer-lived, why, I guess other people worry about that more than I do. But that's another matter which has come up and which the statewide University Faculty Welfare Committee has taken up and got settled.

Lage: Does the Berkeley Emeriti Association deal with these issues? In the earlier interview, you said they were mainly social.

Helmholz: Yes. They have dealt with this particular matter, because there was one member of the association who was particularly concerned about it. The meetings, as I mentioned before, I guess, are really more social, but when something like this comes up, they will take it up. When CUCEA was formed, I was asked to become the Berkeley representative.

Lage: By whom?

Helmholz: By the members from other campuses who formed CUCEA. They were mainly Claude Fawcett from UCLA and a professor of philosophy from Santa Cruz named [Albert] Hofstadter. There were two or three other people. Their concerns are primarily the retirement system.

Morley Walker [director of emeriti and annuitant relations] in the statewide administration was anxious to get this going. He's been interested in both staff retirees and academic emeriti.

Lage: Is he helpful to emeriti?

Helmholz: Yes, he's been, and he's really sort of pushed matters so that CUCEA has relied quite heavily on getting him to have the first meeting of CUCEA and to be a bridge between the administration and CUCEA, and the emeriti.

So that was formed in, let's see. The first meeting was in Berkeley in the fall of '87. Then the next meeting was in the south, and--oh yes. The secretary was a fellow named Ralph Nair from Santa Barbara, so he arranged that Santa Barbara be the host for that meeting. He got the Chancellor at Santa Barbara to come to the meeting and speak to the meeting. So that was '87-'88, and then the meeting was at Davis in fall of '88, and in the spring of '89 the meeting was at San Diego. A fellow named Hugh Bradner who was active in this made that arrangement.

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Helmholz: And then for the year '88-'89, let's see, the chair was Claude Fawcett from UCLA, and then for the year '89-'90, it's John Adams who is from San Francisco. So the meeting in the fall of October of '89 was in San Francisco.

Lage: It sounds as if they make an effort to rotate the chairmen.

Helmholz: Yes, they make an effort to rotate the chairmanship and rotate the meeting places. The meeting in the fall is almost always in the north. It was in Berkeley in the fall of '87, and at that time a constitution was approved, and so on.

Lage: It seems to be a group that's taking hold.

Helmholz: Yes, I think it is. I think each campus has slightly different problems. For example, both at San Diego and Riverside there is an active staff retirees' association. The staff retirees' association has been formed sort of at the same time as the academic emeriti association, so the two have met together at the start. Actually, the Riverside Emeriti Association had been in existence for quite a number of years. CUCEA has tried to maintain the emeriti association separate from the staff retirees, even though for some cases, at San Diego, for example, they have found it convenient to start together but keep some separation. The next meeting in 1990 is going to be at Irvine, and I guess we'll have a meeting at UCLA soon.

Lage: Now, are you still the representative?

Helmholz: I started out being both the representative and what they call the information officer, which is supposed to provide information to all the other members. There's a representative and then there's an alternate, so I was the representative and also the information officer. In 1989, I brought up to the Berkeley Emeriti Association the question of joining this group, because some of the money for CUCEA is supposed to come from the individual associations. So finally, at the May, 1988, meeting of the Berkeley Emeriti Association, they agreed.

Lage: You mean all this time, the Berkeley Emeriti Association really hasn't been the center of CUCEA on this campus?

Helmholz: No, it wasn't even a member of CUCEA. I tried to keep the Berkeley association informed. When Adrian Kragen became the president of the Berkeley Emeriti Association, he sort of listened more than previous presidents and said, "I don't know; what do we get out of it?" I tried to point out that there were things which CUCEA could do sort of in a statewide way, and he was a little hesitant about it but he finally agreed to it at the Berkeley dinner meeting in May. They passed that, and they passed the approval of it. Since their treasury was in good shape, they agreed to give a dollar per active member to the statewide CUCEA. Adrian Kragen also was the one who got these rooms down in University Hall for the emeriti association. So

now there has been established, or there was established, an emeriti association on all the nine campuses.

Lage: So when you were on CUCEA, you really weren't representing a group here at Berkeley.

Helmholz: That's right. I was the Berkeley representative, but I didn't have an emeriti association behind me.

Lage: But now the representative would be actually representing an association.

Helmholz: Yes. Now the president of the Berkeley Emeriti Association, who's Sherry [Sheridan] Warrick, is a member of CUCEA. I am the alternate now, and I've also become the secretary for this year. They've got a new information officer.

So CUCEA is thriving, and I think the Berkeley Emeriti Association is thriving also. As I previously mentioned, I think that on a lot of the other campuses the emeriti association has been taking a more active part in pushing for better financial benefits and better voice in some of the administrative matters of the emeriti, but that's sort of the situation at present.

Changes in Mandatory Retirement Age Rulings

Lage: We talked, just as I was leaving last week, about recent rulings on mandatory retirement.

Helmholz: The federal law is that it's all right for universities to have mandatory retirement ages, and that mandatory retirement age was changed by federal law from sixty-seven to seventy. They're going to consider in 1992, I guess, whether a mandatory retirement age for faculty should continue to be seventy or whether it should be done away with completely.

Lage: If they did away with the mandatory retirement age, would they leave it up to a university to make a mandatory retirement age, or would it be illegal to have one?

Helmholz: It would be illegal to have one. The reason that the retirement of seventy was left is partly because the faculties have tenure, and I think this makes Congress think, "If they have tenure they shouldn't get a lot of other benefits." Maybe they'll decide in 1992 that the mandatory seventy age should be left. I think

that faculty should have a mandatory retirement age. I mean, after all--. It's true that some faculty are active enough so that they could be available if the university or college wants them. And everybody, of course, points to Joel Hildebrand having been active to age one hundred. That's fine--

Lage: Hildebrand and [Ewald] Grether are the two--

Helmholz: Yes, but there are a lot of other faculty who are not that active after age sixty-seven. I'd even be willing to go back to age sixty-seven.

Lage: Will doing away with mandatory retirement create problems for the University, do you think? With tenure protection, then is there any way of saying to a faculty member, "You're really not teaching up to the level that we'd like to see."

Helmholz: That's a reason for having a mandatory retirement age. I think that as long as the college or university is willing to keep one, I think that that's the proper thing for Congress to do, not to say, "You can't have a mandatory retirement age." I think that some businesses would probably just as soon have a mandatory retirement age; in fact, many of them have kept the age seventy mandatory retirement age. My father and my brother were in the Mayo Clinic in Rochester, Minnesota, and they've had a mandatory retirement age of sixty-five. Apparently I think they perhaps just say, whenever a person is hired to their staff, "You've got to agree to the mandatory retirement age of sixty-five." Some of them don't like it very well, but I think most of them agree that it's a good thing.

Lage: It's easier to agree to when you're coming on at age thirty.

Helmholz: Yes, sure. This was another cause of Senator Claude Pepper, who--I guess he's died now.

Lage: Yes, he has.

Helmholz: He was all in favor of no mandatory retirement age, and he felt that retirees were valuable people and good people no matter what their age. But I think that that's a matter on which the emeriti have never taken a very strong position. They're perfectly willing to have a mandatory retirement age of seventy. I think that the universities probably are in favor of that. Now, if Congress comes up and says, "There can't be any mandatory retirement age," I think that I'd be inclined to recommend to the University that they keep the mandatory age seventy, and I know that there'll be an argument between those people who feel that if there's a federal law saying you can't

have a mandatory retirement age, that that should require the University to allow people to go on beyond age seventy. I'd be inclined myself to recommend to the University that whenever a person is hired as a faculty member, that he be asked to sign something saying that he agrees to retire at age seventy, but the Academic Senate would have to agree to this, and whether they would feel that there shouldn't be any mandatory retirement age, I don't know.

Lage: It's really a sticky issue.

Helmholz: Yes, it might well become a sticky issue. I don't know; it seems to me that as long as the federal government is supporting social security, I think they ought to be willing to agree to some sort of a mandatory retirement age.

Lage: And if the emeriti can get the University to give more emeriti benefits, allowing them to still be active--office space and the like--probably most would rather retire. If they can keep a hand in.

Helmholz: Yes, I think so.

Lage: But if you're completely shut off and kicked out of your department, it's a different feeling.

Helmholz: It is. There still is, sort of, in University regulations something that Sproul put out back in the fifties saying that departments should make every effort to provide emeriti with some space and facility to continue on their active work. Sproul did a nice job of saying that <u>whenever possible</u>. And of course, departments that don't have any space just say, "It's impossible, so you have to retire gracefully," to the emeriti.

<u>Recent Court Rulings on Confidentiality of Academic Personnel Records</u>

Lage: Speaking of court rulings, what do you think of this recent ruling limiting confidentiality of academic personnel matters?

Helmholz: I don't like it very well.

Lage: What do you think the effect might be?

Helmholz: I think that if faculty members insist that they know who wrote the letters that were used to prevent them from getting tenure

--that they may get a pretty bad result from that just because it'll become known to everybody in the field in which that person wants to get tenure which distinguished scholar in his field said, "He doesn't deserve tenure at the University of California," whether it's Berkeley or some other place, or even at Podunk University, that the letter was written saying that that particular person doesn't deserve tenure. When that gets known throughout the country, that person is going to be sorry that he ever asked to have it known who wrote the letter.

Lage: Oh, I see. So it might backfire on the individual who's being judged.

Helmholz: Sure, it'll backfire on the individual.

Lage: I guess it came up primarily as an effort to open up cases where discrimination against women and minorities might be an issue.

Helmholz: Yes, and I think that's a case everybody agrees that there shouldn't be discrimination, but if there has been discrimination, then the letters of people who wrote--. I mean, no person is going to write a letter saying, "I don't think that this person"--let's make it a woman--"deserves tenure because she's a woman." Nobody's going to write a letter like that. What that woman will object to is having a letter saying that she's not as good as some man. Well, if a person who writes such a letter feels that, I don't know what you can do about that.

Lage: You can't investigate that individual's social attitudes too easily.

Helmholz: Because he's not going to say, "It's because she's a woman that she's not as good."

Lage: I think we discussed last time that there are letters on file comparing the final candidates for the job. You ask for people outside the University to compare two or three final candidates. Do they go into great depth, like a critique of the various people's work? Is the letter that extensive?

Helmholz: Sure. That's the reason that they would write such a letter.

Lage: So they don't just give an opinion; they evaluate their work.

Helmholz: Oh, yes. I'm sure that people, when they're asked for a letter, they're not going to be guilty of personal bias. They'll give a critique. And of course, that's why I think that people should

be or will be very careful about asking for the publication of those letters.

Lage: But perhaps what the woman, whom we'll take as an example, is trying to see is if the committee followed the advice of the letters. Say the letters do say this woman is as good, or better, but the committee chooses a man. Is that what they're trying to uncover?

Helmholz: That's what she--in this case the woman--thinks. That the letters all say she's just as good as the man, but they pick the man. I don't think that the letters will show that.

Lage: You don't think there's that kind of bias on the budget committees?

Helmholz: I don't think so. Because the budget committees or the people in other universities that are in the positions of making the decision, they know that they're going to be pilloried if they've made the decision on that basis. I suppose that eventually, after the Supreme Court decision particularly, that decisions will have to be made on the basis of opinions of people who are in a position to judge.

Lage: Well, they already are made on this basis, wouldn't you say? Or not?

Helmholz: I think they are, but there are a lot of people who don't agree and who feel that it's been made on some sort of a biased opinion. I don't know; I personally feel that the people who ask for the names of people and feel that those people are biased are going to regret that they did that. Sure, everybody has his prejudices, but how you prove that the prejudice is based on the sex of the person who is being considered? Those are hard things, particularly nowadays when people know that bias has got to be avoided. Well, I don't know. I don't like the idea that names of individuals have to be given, because that will prevent people from giving their own particular views.

Lage: Do you think it will affect the way the letters are written? People will be afraid to be as forthright?

Helmholz: Yes, I'm afraid that that's the case. But we'll just have to see, I guess.

Lage: Quite an interesting issue, really. Do you think we've covered the retirement affairs sufficiently?

Helmholz: I think so, yes.

Controversy over the Naming of Campus Buildings, 1989

Lage: Let's go to the Naming of Buildings Subcommittee. That's a hot issue that's fairly new. How long have you been on that subcommittee?

Helmholz: For at least five years, I think.

Lage: And how much authority does the committee have? How does it function in relation to the administration?

Helmholz: It doesn't have anything to do with the Academic Senate, so it's a subcommittee of a committee that's now called Space Assignment and Capital Improvement Committee. It's a Chancellor-appointed committee, and the Naming of Buildings is a subcommittee of that committee. The Naming of Buildings subcommittee makes their recommendations to this SACI committee, so the Naming of Buildings is essentially an advisory committee.

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Lage: Does it consist only of faculty, or do they have administrative representatives?

Helmholz: Well, it's always been just faculty. I suppose if the Chancellor wanted, he could recommend or appoint people from the administration. Usually the matters are not very cantankerous.

Lage: Take one that hasn't been cantankerous. How do you come up with a name for a building when there's no controversy?

Helmholz: Somebody recommends that the building be named such-and-such.

Lage: There was one recently. Was it the genetics or biochemistry building several years ago that was named after a professor?

Helmholz: There wasn't a specific name; it was the Biochemistry Building. I think the people in biochemistry decided they'd like to have it named for a distinguished professor whose name was [Horace A.] Barker. So they wrote a letter to the Chancellor saying that that's what they thought it should be. The Chancellor, of course, sent it down to this SACI committee and they sent it down to us. Then we considered the matter, and particularly since the biochemistry department had recommended it, why, we approved it.

Lage: There was no gift connected with that?

Helmholz: No, there wasn't any gift connected with that. Most of the things that our subcommittee has considered are not very much at issue in any way. I suppose once a week something comes up. It's a case of somebody having given some money, and he or she who has given the money feels that he or she would like a particular name appended to an auditorium or a courtyard or something like that. Or a class will give some money and they feel that they want their class of 1956 to be honored by something that they had given the money for. And of course we have to be consulted on that.

Lage: Is it usually a fairly routine process?

Helmholz: Usually it's fairly straightforward, but--

Lage: So the request usually comes to you, "Can we name this building, courtyard, or auditorium" a certain name?

Helmholz: Yes.

Lage: They don't come to you and say, "What should we call it?"

Helmholz: No, they don't. Now, in the cases of naming after individuals, sometimes the naming will be--oh, for example, the case in which we have given some objection would be the case, as occurred, where the people who gave the money for an auditorium wanted it to be named the Matthew Mendelsohn, M.D., Auditorium. Well, we didn't mind the Matthew Mendelsohn, but we didn't think that the M.D. should be put in. Or it might have been the Matthew Mendelsohn, and they wanted it to be M.D. and give the year of his degree.

Lage: Is this an actual example?

Helmholz: Yes, this is an actual case. So we just said, "Matthew Mendelsohn, that ought to be enough." It shouldn't be M.D. 1936, or something like that. So we've had that. Now, sometimes that might present a problem with the donors.

Lage: Does the Development Office already make some promises before it comes to you?

Helmholz: Well, they may have given tentative agreement. They might have given a promise, but we have had to say, "Your promise shouldn't have been made," and so on. There haven't been really cases in which things couldn't be worked out, and as you probably know,

we have had the recent case of the Bank of America. Even the Chancellor agreed, in this particular case, that they had really made a mistake. The Chancellor is supposed to consult us; that's always, apparently, been agreed to, that the Chancellor should consult with us about the naming before--

Lage: Was there a policy? Let's just get the background on this issue; they were going to name an auditorium and a patio after the Bank of America?

Helmholz: They were going to name a courtyard, because I think the Bank of America had given a million and a half dollars for a courtyard in the new School of Business Administration building.

Lage: Now, had there been a policy about naming after corporations before?

Helmholz: The policy had been that our committee would be consulted.

Lage: But the committee didn't have firm guidelines?

Helmholz: So when our committee was consulted, actually the Chancellor in this case had sent the recommendation to the Regents before we had been consulted. So we took exception, and when we were consulted, finally, we said, "No."

Lage: Did you say it as a committee?

Helmholz: Yes, we said it as a committee, and we sent our notice back to the Chancellor. Well, unfortunately in this case, the Chancellor had already sent it on to the Regents. So--

Lage: Do you think the Chancellor did that because he thought it would be controversial?

Helmholz: No. I think he just--the recommendation had come from the School of Business Administration, and he just forgot that we were supposed to be consulted. So when we objected, he said, "Oh, boy, we made a mistake." I think that, of course, if we had agreed to it, everything would have been all right. But he in this case, I think, felt that we had a proper objection, so he, I think, went to the Bank of America, and the Bank of America had no strong feeling about our objection. So he said, "We'll put up a plaque but we won't put it on the campus maps or we won't have it put in the building directory that this is the Bank of America courtyard." So I think in a sense we won the argument.

There are at present some other cases. For example, the Wells Fargo has given three quarters of a million dollars, and--

Lage: For the business school also?

Helmholz: For the business school also, and it was going to be a floor, or something. For instance, the top floor of Barrows Hall was named the Lipman Room, and I'm sure our committee approved it. I think it was before I was on the committee. I think the whole top floor of Barrows Hall is one big room, and it was called the Lipman Room. Now, whether the School of Business Administration wanted to call something the Wells Fargo Room, I think it was actually an auditorium [or conference center] which they wanted to call the Wells Fargo Auditorium.

There is also an auditorium for which the Arthur Anderson Company had given money. I think we've reached an agreement in that case to call it just the Arthur Anderson Auditorium. He was an individual, and a distinguished individual who founded the Arthur Anderson Company; just call it the Arthur Anderson Auditorium. Well, that's fine, but what do you do about the Wells Fargo? There was a Mr. Wells and there was a Mr. Fargo, but apparently they haven't agreed with the Wells Fargo Bank to call it the Wells Room or the Fargo Room or whatever they were going to call it. Our committee has agreed to this solution of putting up a plaque, and saying on the plaque, "This courtyard has been made possible by a generous gift from the Bank of America."

Lage: Every courtyard doesn't have to have a name.

Helmholz: Yes, and it doesn't have to be on the campus map or the building directory. So we have agreed with this.

Lage: Is that acceptable to the Chancellor and the Development Office?

Helmholz: Yes, that's acceptable to the Chancellor and the Development Office. We're still arguing with the SACI committee about the way to word all these guidelines, as we call them.

Lage: Who else is on the subcommittee?

Helmholz: On the subcommittee we now have Dick Eakin from zoology.

Lage: Is he emeritus also?

Helmholz: He's emeritus, yes. We're all emeritus. Karl Kasten from art. And Jim Hart, who's the director of The Bancroft Library. We

did have Joe Tussman, who is in philosophy, but he resigned when this Bank of America business came up.

Lage: If he hadn't resigned, would it have been resolved the same way, or did that push the committee to--

Helmholz: No, the rest of us agreed with the Chancellor's guidelines in which he said a gift from a corporation will be acknowledged, but by a plaque.

Lage: Does that still mean that even though it doesn't appear on the campus maps or directory, that it is the Bank of America Courtyard? Or is it just a courtyard that has a plaque saying--

Helmholz: It just is a courtyard that has a plaque in it.

Lage: But Tussman didn't like that?

Helmholz: Tussman felt that this was just the beginning of a move that would end with putting the Bank of America name on a building. So he felt that we were going the way of shopping malls.

Lage: Or UCLA with its Thrifty Tennis Center.

Helmholz: Yes. And even Harvard has an ARCO Auditorium, I think it is [Arco Forum at the Kennedy School of Government]. Apparently this just slipped by President Bok before he realized that it was being done. And as Vice-Chancellor Mac [Watson M.] Laetsch has pointed out to me, the University of Washington has the Nordstrom Pavilion.

Lage: That might be a family name, too, Nordstrom. Probably is a--

Helmholz: Then the question of foundations has come up. Well, the Ford Foundation, that's all right. Ford Foundation Building would be all right, but if McDonald's sets up a foundation and gives a lot of money, should you put McDonald's Foundation?

Lage: Once you put the Ford Foundation, how can you then say, "We don't like the sound of McDonald's Foundation?"

Helmholz: We claim that there is a fundamental difference, that the McDonald's Foundation building would be just advertising McDonald's.

Lage: But why not the Ford Foundation advertising Ford?

Helmholz: But the Ford Foundation is recognized by everybody--every thinking individual, anyway--as a private foundation which has

done a lot of good in their gifts and so on. It's a touchy point, and we haven't even got by SACI our right to disapprove of a foundation gift if it seems to be just an advertising gimmick.

Lage: So you're doing the guidelines. Is SACI feeling stronger than your committee about corporate names?

Helmholz: I don't think they really do, but they haven't agreed to everything that we have, as yet.

Lage: What about the issue of naming after individuals who may not be particularly distinguished in education? Let's say, just to take an example, the Valley Building. Valley was a real estate developer in southern Alameda County, as I understand it, and here's the Life Science Building named after him. Does that bring some problems with it?

Helmholz: I think in the perfect world it does. But we have to agree that Valley made a substantial gift of fifteen million dollars, so something's got to be named after him.

Lage: And I guess on our older buildings we don't even know who they were named after. They were people who gave gifts. Like the Doe Library, we don't know how distinguished Charles Doe was.

Helmholz: No, we don't.

Lage: We know something about Hearst, of course.

Helmholz: Yes, Hearst has had a lot of--. It was great that the Getty Foundation decided they wanted to honor Sherry Washburn [professor of anthropology] rather than the Getty Foundation.

Lage: Yes, that's very nice. Is there anything else about the sticky and interesting questions connected with the naming of buildings?

Helmholz: Yes, and it's still being discussed. I'm sure that when this Soda building [the computer sciences building, to be named Soda Hall, after Y. Chester Soda] is built--. Somebody asked, "Wouldn't it be nice--do you think we ought to have a Fruit Soda Building or a Soda Fruit Building?" [laughter] There'll be laboratories in that in which the IBM company will probably give quite a number of computers. Should that be called the IBM Laboratory? Well, we'd prefer not, but I don't know whether we're going to win in that one, because there'll be another laboratory next door which will be the Apple Laboratory.

Lage: Is all this coming up because of the sort of aggressive development program that Berkeley's undergoing?

Helmholz: Yes. But I'm sure other--I mean, UCLA, as you point out, has had the same problem, and Harvard, Yale. So it's just that they need a lot of money to build these buildings. There are all sorts of rooms in this new biology building in which money has been given by individuals, and we have even agreed that class years might be put down just to sort of identify the room. There are actually rooms in that building for which gifts of the amount of $50,000 had been given by individuals who wanted to memorialize another individual, an individual who's dead and who was an alumnus at the University of California. I think we agreed in that case that his name, which maybe was Joseph Johnson, could be put Joseph Johnson '35, something like that. So it's a kind of a tricky business.

Lage: Have you worked closely with the Chancellor on this?

Helmholz: Well, we did on the Bank of America courtyard because I think he didn't even realize that he'd sent this matter of a million and a half dollar gift to the Regents before we reviewed the name. The Regents have to approve any gift over a million dollars, and he'd sent off the recommendation. So when he heard that we'd turned it down, then, of course, we had to work with him. That was when he wrote this guideline saying that gifts from corporations--it was something like, when the corporation requires acknowledgement, the acknowledgement shall be a plaque mounted in an appropriate place with the name of the corporation on it and something like, "A generous gift of the Bank of America."

Lage: How was Chancellor Heyman to work with, on this issue?

Helmholz: He was very pleasant, I think. I say very pleasant because he agreed with us. [laughter] I brought up this name of the foundation as being advertising, and I brought up the case of the Clorox Foundation. So he had Mac Laetsch look up the Clorox Foundation. Well, it turns out that Gene Trefethen, who's a distinguished alumnus and has given a chair, I think, to the School of Business Administration, is a director of the Clorox Foundation, so I had to agree that the Clorox Foundation was maybe not so bad after all.

Lage: Is there going to be something named after the Clorox Foundation?

Helmholz: I don't know whether there will be or not.

Lage: Has there been an effort to get the development office not to make too many promises to the companies?

Helmholz: Oh, yes. I got a list of I think it must have been a hundred names from the School of Engineering who were graduates, because they might be asked for gifts, and we would not want names of Mafia members on rooms, etc.

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Helmholz: Of course, we're just a subcommittee, and the Chancellor can overrule us. But I think the Chancellor feels that he doesn't want to step on our toes, because we could make a fair amount of noise about it.

Lage: There's probably a lot of sympathy for your side of the issue.

Helmholz: Oh, yes, there is.

The University Research Expeditions Program

Lage: We want to talk a little bit about UREP, the University Research Expeditions Program. You've been on the advisory committee for that?

Helmholz: Yes, I've been on both the faculty advisory committee and the community advisory committee since about 1976. It's an interesting organization that provides the public some opportunity to go on these research expeditions when members of the public can contribute something to the expedition. It was started by a woman named Jean Colvin who I guess has a master's degree in one of the sciences and who felt that this was a way that you could get support for a research expedition in a variety of fields, going all the way from anthropology to chemistry; there was even an expedition that had physics in it. Members of the public apply to go on an expedition, under faculty sponsorship--but it could be an expedition led by a graduate student--where the leader has a research project in which he could use members of the public to help with the research. The cases of particular applicability of this concept are cases in which digging up old fossil remains would be the object of the research. Then members of the public with relatively little training can contribute.

Lage: But they pay some extra fee that helps finance the expedition?

Helmholz: Yes, they pay some extra fee which helps finance the research. In other words, what they put in becomes a tax-deductible contribution to the University research. And then they go along, and they get some training of their own in that kind of work.

Lage: Were you in on this from the beginning of it?

Helmholz: Not quite from the beginning, but I got into it partly--I guess I got into it through a fellow that we know, a lawyer who was with the Ford Foundation, Dyke Brown, who started the Athenian School out in Danville. And I forget who else was in it when I got in it. He's since dropped out quite a while ago, but I've kept with it all the time, and it's finally come to the point where they're going to get some space in University Hall. One of the problems that I've been involved in all through these years is space, because they've been shoved off from one place to another. For a while they had a little space in the bottom of Wheeler Hall, and then they went down to the Extension Building, down on Oxford Street. But finally Vice-Chancellor [Daniel] Boggan has finally agreed to give them some space in University Hall, whenever that gets made safe from seismic disturbances.

Lage: Is UREP an official University group, then?

Helmholz: Yes, it is an official group. It has a telephone number and is listed in the telephone book. It's sort of a self-supporting University group because except for the space, the University doesn't support it at all. But when I got in it, I had been in communication with the National Science Foundation in getting grants for some teacher education work that I had done mostly through the Lawrence Hall of Science. So I wrote a proposal which was funded. The National Science Foundation has been quite generous in funding teacher education projects. Teacher education research expeditions had been funded--the teacher participation in those. The teachers have had their part in the research expedition paid for by the National Science Foundation. So that's been quite helpful. And of course they have provided enough funds so that the office upkeep of UREP has been partly paid by the National Science Foundation.

Lage: Is that one of the roles of the advisory committee, to help with the funding?

Helmholz: Yes, to help with the funding. The community advisory committee has mainly been concerned with fund raising, and they put on every year a sort of a fund raising event which gives the public --not the public that participated in the expeditions, but the

general public in the area--a chance to come and see the results of these expeditions and even buy things from the results of those expeditions. They had these Saturday afternoon or Sunday afternoon affairs and raised $5,000 or $8,000 each year to help pay for the administrative costs of the program.

Lage: Have you gone on an expedition yourself?

Helmholz: No, I never have been on one. Neither Betty nor I have been much interested in going on an expedition, but that may be just our age.

Lage: I have friends who've gone on them. They've really been very enthused. One friend went to Central America and was tracking monkeys through the forest. Now he's going back this year on another similar kind of thing.

Helmholz: It must be ten or twelve years ago, there was one which got a good write-up, which was investigating leeches in South America or Central America. There was a big article in the Los Angeles Times, I think, about studying leeches, and evidently I guess the general view of them was you went into swamps and you'd come out and you'd have leeches sticking to you all around. You'd have to take them off and study them. [laughter]

Lage: Is it a universitywide program?

Helmholz: It is universitywide, but it's centered here. But any faculty member can apply for a grant, so they do have a good many from Davis, particularly some of the things that are more agriculturally inclined. But they go everywhere from Easter Island to Israel to study these things. There's a professor of anthropology, Jim Deetz, who is interested in the recent anthropological aspects of the activities of the early settlers in Virginia. And there was even one out here in Concord which had to do with the early miners in that area.

Lage: Archaeological.

Helmholz: Archaeological, yes.

Lawrence Hall of Science Advisory Committee

Lage: Let's look at the Lawrence Hall of Science Advisory Committee. What's the role of that committee?

Helmholz: That's just to try to promote the activities of the Lawrence Hall of Science.

Lage: Is it responsible for fund raising also?

Helmholz: The Lawrence Hall of Science Advisory Committee advises the director of the Lawrence Hall of Science on educational activities as well as on fund raising. The Lawrence Hall of Science is now trying to expand the building. Harvey White was the first director of the Hall. He was a physics professor, and he did a lot of fund raising himself back in the sixties for the present Lawrence Hall of Science building. The basic funds came from the Regents, but they wanted to build a bigger building than the Regents' contribution would provide, so he did a lot of fund raising.

Now they're trying to raise funds for an expansion of the building. So the Lawrence Hall of Science Advisory Committee is really sort of twofold in its function and is advisory to the director on some of the educational aspects, and is helpful in fund raising.

Lage: Will the directors bring questions to the committee?

Helmholz: Yes, they will bring questions.

Lage: Requests for guidance?

Helmholz: Yes, and in such matters as parking. [laughter] The campus parking committee wanted to charge the employees of the Lawrence Hall of Science a parking fee for parking up there. Everybody on the Lawrence Hall of Science Advisory Committee pointed out that they really had to have free parking up there because that's where they get all their customers for the Lawrence Hall of Science--from the visitors, who need space up there to park.

So I think that's been worked out now. It's an interesting committee. I think some members of the committee are more active than I've ever been. When the Hall first started up, I was active because I had the summer institutes for training physics teachers up there, and we used the facilities to conduct the summer workshops.

Lage: Do you see any changes in the direction that the Hall is going that you'd want to comment on?

Helmholz: Not really. I think that the present director who just retired, Director Marjorie Gardner, has been very active in the work of the Hall. She's been able to give it a direction which I think

the previous directors sort of--well, I won't say they fell down on it, but they weren't able to focus the direction of the Lawrence Hall of Science as well as she's been able to do it.

Lage: How about the new director? Does the committee get a say in--

Helmholz: I think they probably would if they had some suggestions. I haven't been able to think of anybody.

Lage: They just appointed the acting director, Marian Diamond.

Helmholz: Yes, Marian Diamond, and I'm sure she'll do a good job, but I don't think she wants to be permanent. [Since this interview, she has been appointed director.]

Lage: And then what is Glenn Seaborg's role in the Hall?

Helmholz: Yes, Seaborg is sort of the dean of the faculty members who's been concerned with the Lawrence Hall of Science. He was the chancellor when Harvey White was appointed as the director, so he's been very active, and don't spread this about, but I think that probably when the addition is finally funded it'll be named after him.

Lage: That seems to be a good choice.

Helmholz: Yes, he's a very strong advocate of the Lawrence Hall of Science and has been the faculty member who's done the most for it through the years. So that'll be an appropriate naming, but that still is not official.

The Kosmos Club

Lage: I wanted to ask you about the Kosmos Club.

Helmholz: The Kosmos Club is a small campus organization of faculty members who meet six or seven times a year on Monday evenings at the Faculty Club. They have sherry and wine and then dinner, and then a talk by one of the members of the club.

Lage: How many members are there?

Helmholz: I don't really know. I think there probably must be a hundred members.

Lage: Is it by invitation?

Helmholz: Yes, your name has to be brought up. You have to be invited to join. I think I became a member because of Bob Brode, who must have suggested me as a member. So I've been a member for a long, long time. There is a president who's chosen each year. Except for choosing the speakers for the next year, the secretary is the most active person because he has to send out the notices of the meeting and arrange for the Faculty Club to give us a place to meet. We usually meet now in the Heyns Room. Their talks are very interesting and informative, I think both because there's a mixture of all kinds of people from the campus--from the sciences through the humanities and the social sciences. So the discussions of the papers are not only interesting, but informative.

Lage: Are the papers usually very academic, or do they take a different tack?

Helmholz: They're essentially academic, but they're broadly academic. Well, I mean, there are people from business administration and economics that are quite practical in the pragmatic sense of the word.

Lage: Have you presented a paper there?

Helmholz: Yes.

Lage: Do you remember a topic where you would be able to relate to all these different--

Helmholz: The first paper I gave was about physics and X rays and so on. But there are sometimes papers that are way off the usual academic sense. I think that the--I guess maybe I've given only two. I gave one on tennis.

Lage: The physics of tennis? [laughs]

Helmholz: No, sort of the history of great tennis players and who they were, and about the Davis Cup Competition. I must have given that, oh, it must be at least fifteen years ago. And sometimes people do give talks on their sort of pet subjects that are not particularly academic. Although they tend to be on academic subjects.

Lage: Are the papers usually followed by a lot of discussion?

Helmholz: Oh, yes, there's always a good deal of discussion. Each member of the club can get up and give his own pet aspects of the subject. They have two secretaries. One secretary gives a

humorous recount of the previous meeting's discussion--the previous meeting's talk. I think the present secretary who does that is a fellow named Anderson, and he's an historian, in a sense, and he always looks up some abstruse historical details of the particular subject in a very humorous way. So his talk about the previous meeting is always very humorous and sort of abstruse.

Lage: Are there particular faculty members who stand out as being shining lights in that setting, that you remember over the years?

Helmholz: No particular ones. If I had a better memory, I could probably point out some. Lincoln Constance has contributed a lot to the meetings. I think Bob Connick has been the president. Jack Raleigh from English.

Lage: Do they have a range of ages, or do they tend to be the older faculty members?

Helmholz: They tend to be the older faculty, and that's always a problem --how to get good younger members into the club. It has turned out that a couple of times I've asked who the members from physics were, and it's been pointed out to me that we have a lot of members. There must be eight or ten people from physics. More or less six or seven of them just don't come very often, so when we've tried to get some new younger physics members, it's been pointed out that the ones we have elected don't come very often. Which is kind of too bad. When I mentioned that probably there were a hundred members, I think at any particular meeting there won't be more than forty or forty-five people who come.

Women in the Kosmos Club and the Bohemian Club

Helmholz: I think that the first time we took in a woman was only about twenty years ago, and we have several women.

Lage: That hasn't presented a problem?

Helmholz: It didn't present a problem, but I think there had never been a woman member before, so--

Lage: Was that sort of a conscious thing, when a woman was suggested for membership? Was there a discussion about it?

Helmholz: There was a business meeting when Ed Strong was president of the Kosmos Club. It may be the only such meeting ever held. So far as the secretary, Steve Diliberto, knows, there is no constitution or by-laws. Herma Kay, from the law school, was the first woman member. This was about 1970.

Lage: Do you recall if someone like Marian Diamond--is there any self-consciousness about her role as maybe the only woman at one time in that group?

Helmholz: Yes, I think that the first time that it was suggested that a woman be invited to become a member, I'm sure somebody brought up the point, well, we've never had a woman member before. There must be in the minutes of the meeting some discussion about that point. It's like the Bohemian Club.

Lage: Of course, the Bohemian Club has an absolute policy. Do you think the Kosmos Club had an actual policy against women members?

Helmholz: No. It was just custom.

There have been women members of the Bohemian Club way back in the 1880s or 1890s. There was a poet who was a member of the Bohemian Club, and she was one of the early members and was quite a well-known poet in those days. And then they just--I think it was probably before they had any by-laws. But there were a few--I think two--women members of the Bohemian Club in those days.

Lage: Then they did institute a written policy?

Helmholz: Yes.

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Lage: We were talking a little bit about the Bohemian Club, which we haven't mentioned as an outside organization that you're a member of.

Helmholz: With the present publicity on women in men's organizations, this has come up in the Bohemian Club, and I suspect that one of these years there'll have to be some sort of an admission of women to the Bohemian Club. How it will be done, nobody has any good idea. There now are--there always have been--women employees of the Bohemian Club in San Francisco, but there never have been women employees at the Bohemian Grove until, I think this last year, there were some women who were employed at the,

so to speak, at the gate. That is, at the entrance to the Grove.

Lage: Is that part of the pressure of discrimination laws in employment?

Helmholz: Yes, that certainly is the reason. And the Bohemian Club did point out to the California state agency that deals with discrimination that it was inappropriate to have women employed at the Bohemian Grove, just because there's no place for them in the proper social organization at the Grove. But I think that they were ordered by the state organization to take in some women employees, and they have done so at the edge of the Grove, where the women don't get into the interior of the Grove.

Lage: Is there a place in our society for these all-male groups? Will something be lost when you finally admit women?

Helmholz: Yes, I think so. I think that there is a place for women-only organizations as well as for men-only organizations. But apparently, the courts have decided that men-only groups that are bigger than something like eight hundred members should allow women in them. And I'm sure that the Bohemian Club is fighting a losing cause. [laughs]

Lage: There must be people with pretty strong opinions about it, to hang on this long.

Helmholz: But they're still--it may be smaller than eight hundred, but I think in the--

Lage: Does it have to do with tax deductibility at all, or is it--.

Helmholz: Oh, yes.

Lage: I mean, if you gave up your tax-free status, would that affect the court ruling, do you know?

Helmholz: Well, I think if the people who are anxious to push for the admission of women to the Bohemian Club, for example, when they --. They are probably going to win out, just because the Bohemian Club is a fairly large organization. But the business about taxes has been brought to the Bohemian Club's management, and they have had to be sure that the people who are members do not have their membership dues supported, or you might say paid, by their employers. So that Bohemian Club members cannot have their dues or even their expenses in any sense paid by others in the regular course of their expenses.

Lage: Have you been in any kind of governing body in the Bohemian Club?

Helmholz: No, I never have, so I don't know a lot about it. But I do know that we have to, you might say, guarantee to the Bohemian Club that the expenses that we pay are not reimbursed by any of our employers.

Lage: From your experience, is it used as a place where people make contacts that are valuable in business?

Helmholz: I'm sure that you do. I mean, you have to guarantee that your expenses are not paid because of business--are not valuable in your businesses. And I'm sure that to some extent they are valuable in business. There is one of the mottos of the Bohemian Club that implies that you don't get any good business dealings from your Bohemian Club activities. But you certainly can't guarantee that you don't talk about business deals when you're at the Bohemian Club. [The motto is, "Weaving spiders come not here." --ACH]

Lage: And then, of course, there are the contacts you make in the club.

Helmholz: Sure. But those are problems that will keep coming up all the time.

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APPENDIX

BRIEF BIOGRAPHY--Elizabeth L. Helmholz

Elizabeth Little Helmholz grew up in Winona, Minnesota, and attended the Baldwin School in Bryn Mawr, Pennsylvania, and Stanford University. Following her graduation from Stanford in 1938, she married Carl Helmholz, whom she had known from her youth in Minnesota. Four children followed, Charlotte, George, Frederic, and Edith, born in 1940, 1942, 1946, and 1948.

Despite the attention required for home and family, Betty Helmholz has epitomized many faculty wives of her generation in her ongoing contributions to campus life and community service organizations. In his introduction to Carl Helmholz's oral history, fellow physics professor Walter Knight refers to "Chez Helmholz," the site of innumerable "family-type gatherings. . . all of which added importantly to the enduring cohesiveness among the members of the department." In addition, there was the faculty wives' Section Clubs, where the Helmholzes have been active members of the Drama Section for nearly fifty years--another venue for cross-departmental associations to develop.

Betty Helmholz's concerns have always reached to the broader community as well. She was on the board of the Berkeley Day Nursery, a child daycare facility, for twelve years; served the University YWCA for twenty-five years as board member, and president for five; had an active role for six years on the International House board; and participated in many other community organizations.

As a board member and former president of the Laird, Norton Foundation, the philanthropic arm of her family company, Mrs. Helmholz has been active in charitable giving in the areas of forestry, conservation, and economic education. One program in particular that reflects the interests of the foundation and of Mrs. Helmholz is the Distinguished Visitor program in the University's College of Natural Resources. Laird, Norton Foundation established and for ten years funded this program to bring outstanding representatives of government, business, and conservation organizations to present to graduate students a well-rounded picture of issues and problems in the area of natural resources.

A listing of Betty Helmholz's major university and community activities follows. Dates are often approximate.

Elizabeth L. (Betty) Helmholz
Activities

University Section Club Drama Section I Founded University Art Museum Section	1943-1992	
College Teas	1943-1960	Treasurer
Berkeley Day Nursery	1948-1960	Treasurer
Town & Gown Club	1954-1992	Treasurer, Board member, Admissions Committee, etc.
University YWCA	1951-1971	Board member, president (five years)
Mills College Assoc. Council	1958-1992	
Town & Country	1958-1992	
Children's Hospital Walnut Creek Branch	1952-1960	
Alexander Lindsay Jr. Museum	1958-1987	One of founding members
University Art Museum	1977-1980	Board member
International House Board	1981-1988	Chair, House Committee, three years
ARCS (Achievement Awards for College Scientists)	1978-1992	
Member of: Book and Dooley Mendico Stanford Women's Club of the East Bay		
Laird, Norton Foundation Distinguished Visitor Program University of California, School of Forestry	1955-1992	Board member and President, 1975-1982

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ANN LAGE

B.A., University of California, Berkeley, with major in history, 1963

M.A., University of California, Berkeley, history, 1965

Post-graduate studies, University of California, Berkeley, 1965-66, American history and education; Junior College teaching credential, State of California

Chairman, Sierra Club History Committee, 1978-1986; oral history coordinator, 1974-present

Interviewer/Editor, Regional Oral History Office, in the fields of conservation and natural resources, land use, university history, California political history, 1976-present.

CURRICULUM VITAE

GRAHAM CHARLES HALE

Ph.D., University College, London University, England, 1967, in Atomic Physics.

B.Sc. (Special), University College, London University, England, 1963, in Physics.

Postdoctoral Fellow in Physics, University of Missouri-Rolla, 1968-1969.

Assistant Professor of Physics, University of New Haven, West Haven, Connecticut, 1970-1974.

Studies in history of science, Department of History of Science and Medicine, Yale University, 1974.

Assistant Coordinator, History of Science and Technology Project, The Bancroft Library, University of California, Berkeley, 1975-1976.

Assistant Professor of Physics, San Jose State University, 1977-1979.

Studies in poetry, 1977-, with Allen Ginsberg, Diane di Prima, Philip Whalen, John Oliver Simon, Clive Matson.

Poet-teacher, Oakland public Schools, 1980-1981.

Anti-nuclear researcher, 1981-